W-2 CHILD SUPPORT DEMONSTRATION EVALUATION PHASE 1: FINAL REPORT

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Volume I: Effects of the Experiment

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Table of Contents

List of Tables

Appendix Tables

List of Figures

Executive Summary

With the introduction of Wisconsin Works (W-2) in 1997, Wisconsin initiated a radically new approach to public assistance for low-income families. W-2 replaced Aid to Families with Dependent Children (AFDC), the program that previously offered cash to low-income, primarily single-parent families. W-2 participants are placed into one of four tiers of a "self-sufficiency ladder." The two upper tiers, Unsubsidized and Trial (subsidized) Jobs, provide case management and associated programs, but no cash payment. The two lower tiers, Community Service Jobs and W-2 Transitions, provide a cash payment in return for participation in work-like activities. W-2 also contains a unique child support component that is the subject of an experimental evaluation. This report presents the results of the first phase of the W-2 Child Support Demonstration Evaluation (CSDE). It includes results for cases that entered during the first three calendar quarters of the experiment. Future reports will present results of nonexperimental analyses and of experimental results for later-entering cases.

Recent welfare reforms have increased the potential importance of child support as an income source for low-income single-parent families. Time limits, work requirements, and the lack of an entitlement to cash assistance have made nonwelfare sources of income essential. In Wisconsin, relatively stringent work requirements have been combined with a uniquely generous approach to child support. Among most mothers participating in W-2, any child support paid on behalf of their children is passed through to them and is disregarded in the calculation of their W-2 cash payments. In contrast, in most other states child support paid on behalf of children receiving cash assistance is kept by the government to offset welfare costs, and the family receives no additional income.

To evaluate the impact of the full pass-through, the W-2 child support policy was implemented as a random-assignment experiment. Most W-2 participants received a full pass-through of child support, but a randomly selected control group received a reduced amount. Because assignment to the experimental (full pass-through) and control (partial pass-through) groups was random, any differences in outcomes between the two groups can be attributed to the difference in the treatment of child support. The CSDE was designed to evaluate a variety of impacts of this new approach to child support, beginning with the direct effects of the new policy on child support paid and received. We have also tried to measure a wide range of potential secondary effects—on mothers' and fathers' employment and earnings, on parents' interactions, and on the well-being of their children. To evaluate these effects we use the state's administrative records and a survey of W-2 families.

As shown in Chapter I.4, we find substantial evidence of the expected direct effects. In 1998, mothers eligible for the full pass-through received about $150 dollars more in child support than did those in the control group. Among those initially in a lower tier (and thus subject to a reduced pass-through if they were in the control group), the difference was about $200. Differences were somewhat smaller, but remained significant, in 1999. Although these differences in amounts of child support received by mothers are due in large part to the mechanical effect of the full pass-through, we also find significant increases in the percentage of nonresident fathers paying child support. These differences are statistically significant, but fairly small, in the full sample: 52 percent of fathers of children in the experimental group and 50 percent of fathers of children in the control group paid child support in 1998. However, among those more likely to be new to the child support and welfare systems, the differences were more substantial: among those cases in which the mother had not received AFDC in the two years prior to entering W-2, 58 percent of fathers with children in the experimental group, compared to only 48 percent of fathers with children in the control group, paid child support in 1998. The differences remained significant and in many cases increased in 1999. Finally, we also find significantly higher rates of paternity establishment for those in the experimental group in 1998, although the difference declined and was not statistically significant for most groups in 1999.

As expected, we find less consistent evidence of secondary effects, although in selected areas there is substantial evidence that the experiment had the expected impact. We hypothesized that an increase in child support received would reduce the need for cash payments. We find evidence of this

effect in 1998, with significant and larger differences among those mothers who received a W-2 cash payment and among mothers with a history of higher child support amounts. We also find some evidence of the expected effects on nonresident fathers' informal employment: fathers with children in the experimental group appeared to be substantially less likely to have informal earnings.

In other areas we find little consistent evidence of an experimental impact. There were few significant impacts on mothers' employment or earnings, perhaps because increases in child support receipt were not sufficiently large to have such secondary effects, or perhaps because the increase in child support simultaneously helped facilitate employment and reduced the incentive to work. We find few consistent impacts on child well-being—although there was some evidence of fewer health limitations and improved educational outcomes for children in the experimental group. Most measures of nonresident fathers' relationships with the mother and child revealed few differences among the two groups. However, we find some evidence of higher informal transfers made by fathers in the experimental group, suggesting that formal and informal transfers are complements.

Finally, while we find significant differences in some of the components of total government costs, we find no difference in overall government costs. Although more child support is passed through to those in the experimental group, not all of this is at the expense of the government, since some consists of additional support that would not have been paid in the absence of the full pass-through. More important, the reform also generated cost savings in other areas, especially W-2 cash payments.

We believe that the effects of the CSDE reported here are likely to understate the expected effects of the policy change in Wisconsin. First, our analysis shows larger effects among cases new to the welfare system. We expect that the effects of the experiment might be greater among those who have not already established behavioral patterns in response to the old system—a growing proportion of all cases over time. Second, W-2 involved dramatic changes in the administration and structure of welfare programs and payments. Especially in Milwaukee, where most participants reside, it appears that many caseworkers did not initially understand the CSDE or explain the implications of their experimental status to clients. In preliminary analysis of cases assigned as part of a later cohort of participants, after W-2 was more fully implemented and staff received additional training, we find evidence of greater effects.

For a number of reasons the effects of the CSDE may understate the effects of a full pass-through were it to be implemented in other states. First, the difference in the pass-through to those in the experimental and control groups in Wisconsin was more modest than the likely difference in other states. Even those in the control group of the CSDE received the greater of up to $50 per month or 41 percent of child support paid. Under Temporary Assistance for Needy Families (TANF), most states are neither passing through nor disregarding any child support. Second, to the extent that participants in other states might receive cash payments for a longer period, the effects of the policy change might also be greater. Third, because Wisconsin's caseload decline has been so steep, the state's current TANF recipients may be more disadvantaged than those in other states. This may mean that the amount of child support that nonresident parents could potentially pay may be lower, and thus the effects of a full pass-through may be lower in Wisconsin than elsewhere. Some of the factors that suggest greater potential impacts in other states could also lead to higher costs than found here.

The results of the CSDE presented here demonstrate that Wisconsin's full pass-through has been able to increase child support amounts received among an economically vulnerable population, to increase child support collections, and to have a variety of other positive effects. These benefits have come at little cost to government. While some factors might lead CSDE estimates to overstate potential effects, we expect that the effects in another state would be larger than those reported here. Indeed, in many ways it is striking that we find evidence of any substantial effects, given the implementation problems, the lack of a large difference in the policies faced by the experimental and control groups, the speed with which mothers are moving off W-2, and the relative socioeconomic disadvantage of W-2 participants.

In most states TANF participants do not receive any of the child support paid on behalf of their children. This no-pass-through policy generates revenue to offset public assistance and child support enforcement costs in the short run. Our results suggest, however, that this policy has potentially detrimental effects on developing child support as a long-run income source for single mothers and their children. Given the time-limited nature of cash assistance, the benefits to government of retaining child support are also quite limited. In contrast, the benefits to children of establishing paternity and setting a pattern of child support payments are potentially more enduring. Especially for this reason, a full pass-through seems to be a policy worthy of serious consideration by other states.

Chapter 1
Introduction

With the introduction of Wisconsin Works (W-2) in 1997, Wisconsin initiated a radically new approach to public assistance for low-income families. W-2 replaced Aid to Families with Dependent Children (AFDC), the program that previously offered cash to low-income, primarily single-parent families. The child support policy component of W-2 required a federal waiver, which was granted with the condition that the state conduct an evaluation of this part of the program. The W-2 Child Support Demonstration Evaluation (CSDE) effort has provided a context to collect and analyze a variety of data on W-2 outcomes, with particular focus on child support. In this report we provide information on the lives of W-2 recipients in the first years following this historic policy shift. Much of our focus is on outcomes associated with changes in child support policy. However, we review more general W-2 outcomes as well, both because the new child support policy has potential impacts on a broad array of outcomes and because the child support policy reform can best be understood as one piece of an overall welfare reform strategy. This report on the first phase of the experiment includes results for cases that entered during the first three calendar quarters of the experiment. Future reports will include results of nonexperimental analyses and of experimental analyses of later-entering cases.

This volume of the report presents the context and results of the experimental evaluation of Wisconsin's innovative approach to child support. We first explain W-2 child support policy. We then discuss the context of the reform—outlining the logic and evolution of welfare reform and child support policy. Section 2 discusses the implementation of the child support demonstration. Section 3 describes the data and methods used for the analysis, including information on the samples of participants analyzed, data sources, and general evaluation issues. Section 4 summarizes the results of the experimental evaluation. Section 5 provides an overall summary and a discussion of the ongoing nonexperimental evaluation efforts. It also offers our assessment of the lessons that those in other states can learn from this evaluation.

The principle behind W-2 child support policy is as simple as it is unique. When child support is paid on behalf of a W-2 mother and her children, she is allowed to keep that support without her W-2 cash payment being reduced. That is, 100 percent of child support is passed through to the mother and disregarded in the calculation of cash payments. For example, if a mother is participating in W-2 and receiving a payment of about $650 per month, a child support payment of $200 will raise her income from cash payments and child support to about $850 per month. In contrast, most other states retain any child support paid to participants in order to offset state and federal public assistance costs, so the income of mothers in most states would be only $650.

To evaluate the impact of the full pass-through, the W-2 child support policy was implemented as a random-assignment experiment. Most W-2 participants receive a full pass-through and disregard of child support, but a randomly selected control group receives a reduced amount. Because assignment to the experimental (full pass-

- This volume presents the formal results of the CSDE. It includes analyses of the effects of the child support reform on a broad range of outcomes for mothers participating in W-2, their children, and the fathers of their children.
- In Volume II we present a more general analysis of outcomes for W-2 participants. Using data collected for the CSDE, we analyze mothers' employment, earnings, use of government programs, and income and poverty status; fathers' child support payments, employment, earnings, and relationships with their children; and child well-being.
- Volume III consists of a series of technical reports that provide detailed information on the implementation of the CSDE, as well as on the details of data, methods, and analytic strategy.

through) and control (partial pass-through) groups is random, any differences in outcomes between the two groups can be attributed to the difference in the treatment of child support.[1] We use this strategy to evaluate the impact of the full pass-through on a wide range of outcomes. We evaluate the impact on formal child support payments and receipts, paternity establishment, and child support orders. We consider the impacts on mothers' program participation and employment, and the implications for family incomes and government costs. We also consider the impacts on fathers' employment and income, fathers' interactions with their children, and, finally, the impacts on child well-being.

Although the basic logic of both the W-2 child support policy and the experimental design are straightforward, the ultimate implementation of the policy and evaluation effort proved to be more complex. We next explain the context of the child support reform related to both welfare reform and child support policy. We then discuss in more detail the structure of W-2, the child support policy, and the experimental evaluation design.

I.1.1 Welfare Reform and W-2

Critics charged that the AFDC program was expensive, potentially discouraged work and marriage, and was ineffective at reducing high levels of poverty among children living in single-parent families. In 1996, dramatic changes in the public welfare system took place at the federal level with passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA). The act replaced AFDC with a block grant, Temporary Assistance for Needy Families (TANF), which gives the states considerable freedom in designing their own systems of assistance to low-income families. Wisconsin had earlier begun planning for an overhaul of its AFDC program. PRWORA enabled the state to complete planning and to implement the new W-2 program.

In contrast to AFDC, which provided an entitlement to cash assistance with limited work requirements, TANF-funded assistance is generally limited to 5 years, with recipients required to work within 2 years. Wisconsin has adopted a work-first model; the philosophy and structure of W-2 emphasize immediate employment. Under W-2, almost all participants are placed in one of four tiers of employment or employment experience. W-2 tiers and payments are summarized in Table I.1.1. The most job-ready applicants are provided case management services to help them find an Unsubsidized Job on the open market or improve their current job status. Trial Jobs provide work experience in jobs for which the state provides a partial subsidy to the employer. Participants in these two upper tiers receive no cash payments from the state (but may receive a variety of ancillary services). Community Service Jobs are public service jobs for which participants receive a monthly W-2 payment of $673. W-2 Transition is for those least able to work, either because of their own disability or because of the need to care for a child with a disability. W-2 Transition participants receive a monthly W-2 payment of $628. In addition to these four tiers, the Caretaker of Newborn tier provides, for parents caring for a child younger than 13 weeks, a monthly payment of $673 and exemption from work requirements. Those in the lower tiers receive the full amount only if they meet the time requirement; otherwise they lose $5.15 per hour of nonparticipation. Consistent with an approach that tries to replicate the "real world of work," W-2 is available to all low-income families with children, not merely single-parent families.

Other programs also provide assistance to low-income families. The federal Food Stamp program provides vouchers for food purchases and Medicaid (referred to as Medical Assistance in Wisconsin) provides health coverage. In addition to these federal programs, a new state program providing child care subsidies became available to low-income families at the same time W-2 was being implemented.

[1]We discuss the advantages and limitations of this experimental design below.

Table I.1.1

The Four Tiers of Wisconsin Works

Tier	Income/Payments	Time Requirement	Program Time Limit
Unsubsidized Job	Market wage	None	None
Trial Job (W-2 pays maximum of $300 per month to the employer)	At least minimum wage	40 hours per week	3 months per placement with an option for one 3-month extension; total of 24 months over all Trial Job placements
Community Service Job	$673 per month	30 hours per week, plus up to 10 hours per week in education and training	6 months per placement with an option for one 3-month extension; total of 24 months over all Community Service Job placements; extensions permitted on case-by-case basis
W-2 Transition	$628 per month	28 hours per week of work activities, plus up to 12 hours per week in education and training	24 months; extensions permitted on case-by-case basis

Note: A final category, Caretaker of Newborn, provides $673 per month for parents caring for a child younger than 13 weeks.

Moreover, in July 1999, BadgerCare began, providing health coverage to a broader range of low-income families with children than does Medicaid. All these programs have been "delinked" from the W-2 program so that low-income families can receive services regardless of whether they are participating in one of the tiers of W-2.[2]

I.1.2 Child Support and Welfare

Recent welfare reforms have increased the potential importance of child support as an income source for low-income, single-parent families. Time limits, work requirements, and the lack of an entitlement to cash assistance have made nonwelfare sources of income increasingly essential. In Wisconsin, the relatively stringent work requirements of W-2 have been combined with a uniquely generous approach to child support. The significance of this approach is best understood in contrast to policies from earlier periods and other states.

Under AFDC, all current child support paid on behalf of welfare recipients in excess of $50 per month was retained by the government to offset welfare expenses; the money was split between federal and state governments based on the formula for splitting Medicaid costs. TANF allows states substantial flexibility regarding the handling of child support paid on behalf of families receiving assistance. Most

[2]These related programs have higher income limits than W-2. Food stamps are available to those with gross income less than 130 percent of the federal poverty line. Medicaid has different eligibility requirements based on the age of the child. Child care subsidies are available to families with incomes up to 185 percent of the federal poverty line at the time of application. Beginning July 1, 1999, all members of families with children who have incomes below 185 percent of the poverty line and who do not have health insurance became eligible for BadgerCare, the new CHIP program in Wisconsin. Eligibility for BadgerCare continues until income reaches 200 percent of the poverty line.

states now follow one of two approaches, either retaining all child support paid on behalf of TANF families or continuing to have a $50 per month pass-through (Cassetty et al., 2000). In contrast, in Wisconsin implementation of the W-2 program coincides with a dramatic shift in the interface between the private child support system and the provision of public assistance. Under the new policy *the full amount of child support paid is distributed to resident-parent families and does not affect the level of the TANF check they receive*.[3]

These three potential approaches to the interface between the private child support system and welfare policy—retaining all child support paid, dividing child support into a portion that is retained and a portion distributed to the family, and distributing the full amount to the family—have different rationales and different potential effects. Retaining all support paid on behalf of public assistance recipients should enable the state to recoup a portion of its public costs. This is the policy most states have chosen since they were given the freedom to select this option in 1996. This policy may, however, discourage nonresident parents from paying private support, because none of their payments go directly to their children. Moreover, resident parents and nonresident parents have an incentive to cooperate with each other and *not* to cooperate with the formal system, because if any support received from a nonresident parent is hidden from the child support system, all support would benefit the children of a nonresident parent, and resident-parent families could keep both public and private support.

These negative consequences might be alleviated by the second approach, passing through a portion of the private support to resident parents. At least since 1984, and until the PRWORA changes in 1996, federal policy called for $50 per month of child support to be passed through and disregarded in the calculation of welfare payments for the resident parent. This policy removes some of the disincentive for nonresident parents to pay and increases the incentive for resident and nonresident parents to cooperate in compliance, and thus could increase formal payments. Although little quantitative research has been conducted on the effects of the pass-through, recent ethnographic research suggests that $50 per month may not be a large enough incentive to encourage cooperation (Waller and Plotnick, 2001). Research has indicated that some parents strategically collaborate; in exchange for the resident parent not providing information on the nonresident parent, the nonresident parent agrees to pay child support informally, which allows the resident parent to keep all child support paid (Edin, 1995; Waller and Plotnick, 2001).

The third possible policy, ensuring that all resident parents who receive public cash payments receive all of the child support paid on their behalf, should remove most of the disincentives for nonresident parents to pay through the formal system, thus increasing formal payments. The increase in formal payments may eventually lead to increased total payments if the formal system can ensure payments will continue when informal payments would have stopped. The policy may also increase the proportion of children who have paternity formally established because of reduced incentives to avoid the formal child support system.

As discussed further below, the third option might also be expected to reduce the need for resident parents to participate in the Food Stamp and Medicaid programs, to promote earnings among nonresident parents, to increase contact between nonresident parents and their children, and eventually to improve other aspects of children's well-being. Moreover, this policy would be consistent with the way child support is treated among those not receiving cash assistance (where all support is passed through to the family), making the income support system more consistent with the way the working world operates. Another benefit derives from lower administrative costs in the child support system that result from a simpler system. However, the policy may have unintended negative consequences. For example, if it increases contact between discordant parents, children could be exposed to greater conflict. The ultimate fiscal implications of a policy to pass through all child support will depend on the extent to which the

[3]Prior to March 1, 2000, child support counted as income in determining eligibility for W-2, but did not count in terms of the level of cash received. It also counted in terms of the level of child care copayment required. Beginning March 1, child support no longer counted in determining eligibility for W-2 or child care.

beneficial effects compensate for the loss in revenue previously collected from child support payments to families receiving public support. The cost will also depend on the extent to which low-income parents choose to receive (or to continue to receive) TANF payments if they can also retain child support.

The dramatic policy change in PRWORA allows states to set their own pass-through policies. States may keep all child support paid on behalf of children receiving TANF cash assistance, or they may pass through up to $50. Under current law, the third option, passing all child support through to the resident parent, has significant financial implications for a state—in addition to forfeiting its own share of the collected child support, it must reimburse the federal government's share of the collection. In the future, this option may be more attractive, as pending federal legislation would permit states to implement a full pass-through for most TANF recipients without requiring them to reimburse the federal government for its share of the collections.

I.1.3 W-2 Child Support Policy

Wisconsin is unique in selecting a full pass-through. Wisconsin was granted a federal waiver allowing it to use savings generated by previous reforms to pay the federal share of child support collected. An experimental evaluation of the effects of a full pass-through was required as part of the federal waiver. On August 31, 1997, the automated management information system of the Wisconsin Department of Workforce Development (DWD) randomly assigned all existing AFDC cases to an experimental group (who received the full pass-through) or to a control group (who received only part of the child support paid on their behalf, with the remainder retained by the government).[4] When new families requested assistance from W-2, they were also randomly assigned to one of these two groups.

Random assignment of new entrants continued through July 9, 1998, when a code error in the administrative data system discontinued the assignment of any incoming W-2 cases in Milwaukee to the control group. This error meant that cases entering during a period when the W-2 program was working more smoothly could not be included in our analysis. Because of this, the decision was made to restart random assignment in Milwaukee on January 1, 1999, continuing through June 30, 1999. However, only cases that entered before July 9, 1998, are considered part of the "original cohort" of the CSDE and are included in the analysis reported here.[5]

Under Wisconsin's AFDC system, resident parents received a partial pass-through equal to the first $50 per month of any child support paid. Under the CSDE, the control group receives a partial pass-through of the amount paid up to the first $50 per month, or 41 percent of the amount paid, whichever is larger.[6] The different treatment of child support for those in the experimental and control group is

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[4]Some participants were assigned to a "nonexperimental" group. They received the identical treatment as those assigned to the original "experimental" group (full pass-through) but were not originally to be included in our evaluation analysis. Our references to and analysis of the "experimental" cases include those in the original "nonexperimental" group.

[5]Cases that entered outside Milwaukee between July 9, 1998, and December 31, 1998, are referred to as the "second cohort," and cases entering statewide between January 1, 1999, and June 30, 1999, are referred to as the "third cohort." The Wisconsin DWD was recently awarded funding to extend the CSDE to allow for the analysis of the second and third cohorts. As described below, the Survey of Wisconsin Works Families is drawn from cases in the original cohort.

[6]Distributing the first $50 per month to control group families allowed the state to guarantee that no one was worse off than they would have been under the prior (AFDC) policy. We noted above that the amount of child support retained is split between the state and federal government; in Wisconsin the split is 41 percent for the state and 59 percent for the federal government. Distributing 41 percent of what was paid to control group participants enabled the state to say that it was giving away all of its share.

The experimental-control group status is relevant only to amounts of child support collected for *current*

illustrated in Figure I.1.1. The formula for the control group results in three ranges of child support pass-through rates. In the first range, when payments are below $50 per month, the experimental and control groups receive the same amount. The second range is between $50 and $122 per month. In this range the experimental group receives the full amount paid and the control group receives $50 (because $50 is more than 41 percent of the amount paid). In the third range, above $122 per month, the experimental group receives the full amount and the control group 41 percent.

To estimate how many W-2 mothers are in each range, we examine the amount of child support paid on behalf of mothers and the amount of child support orders in the month of W-2 entry. In the entry month, no child support is paid on behalf of 82 percent of the mothers. Among those receiving support, in only about 10 percent of the cases is less than $50 paid. Almost a third of cases receiving support are in the second range, with payments between $50 and $122. Over half of all cases receiving support are in the third range when they enter W-2; however, this is less than 10 percent of all mothers because child support receipt is fairly uncommon. If we consider the amount *owed* rather than the amount paid, more mothers are in the third range. Of those with orders, very few (4 percent) are owed less than $50/month, only 14 percent are owed in the second range (between $50 and $122/month), and 82 percent are owed more than $122.[7] As discussed further in Section I.4.1, the percentage receiving any support, and the mean amount of support received, increases substantially in the 2 years after entry, so more and more mothers would be in the third range over time, the range in which the difference in treatment between the experimental and control groups is greatest.

Control group members receive the partial pass-through only when they are receiving a payment from W-2 (in a lower tier); control group cases in a higher tier (Unsubsidized Job or Trial Job) receive the full amount paid because they are treated as non-TANF recipients (i.e., they receive no state or federal funds that could be offset by child support collections). As discussed further in Section I.4.3, many W-2 participants never are in a lower tier, and most mothers who participate in a lower tier leave it fairly quickly. This means that for many mothers, over time there is no difference in how the experimental and control groups are treated.

In summary, the actual intervention, that is, the difference in treatment between the experimental and control groups, is fairly limited for many women. Experimental group mothers for whom no (or very low) child support amounts are being collected are treated no differently from the control group (although the promise of different treatment should formal child support be collected may affect behavior). Moreover, experimental group mothers who leave a lower tier of W-2 also are treated no differently from those in the control group (although the promise of different treatment should they return may affect behavior).

Even though the actual intervention is limited, the random assignment model provides a powerful tool to estimate the effects of the intervention. Because random assignment should make the experimental and control groups comparable in all ways except for the treatment of child support, the effects of a full pass-through (as compared to a partial pass-through) can be seen by simple comparisons of outcomes between the experimental and control groups.

Before discussing the evaluation approach in more depth, we begin with a description of the way the child support reform was implemented.

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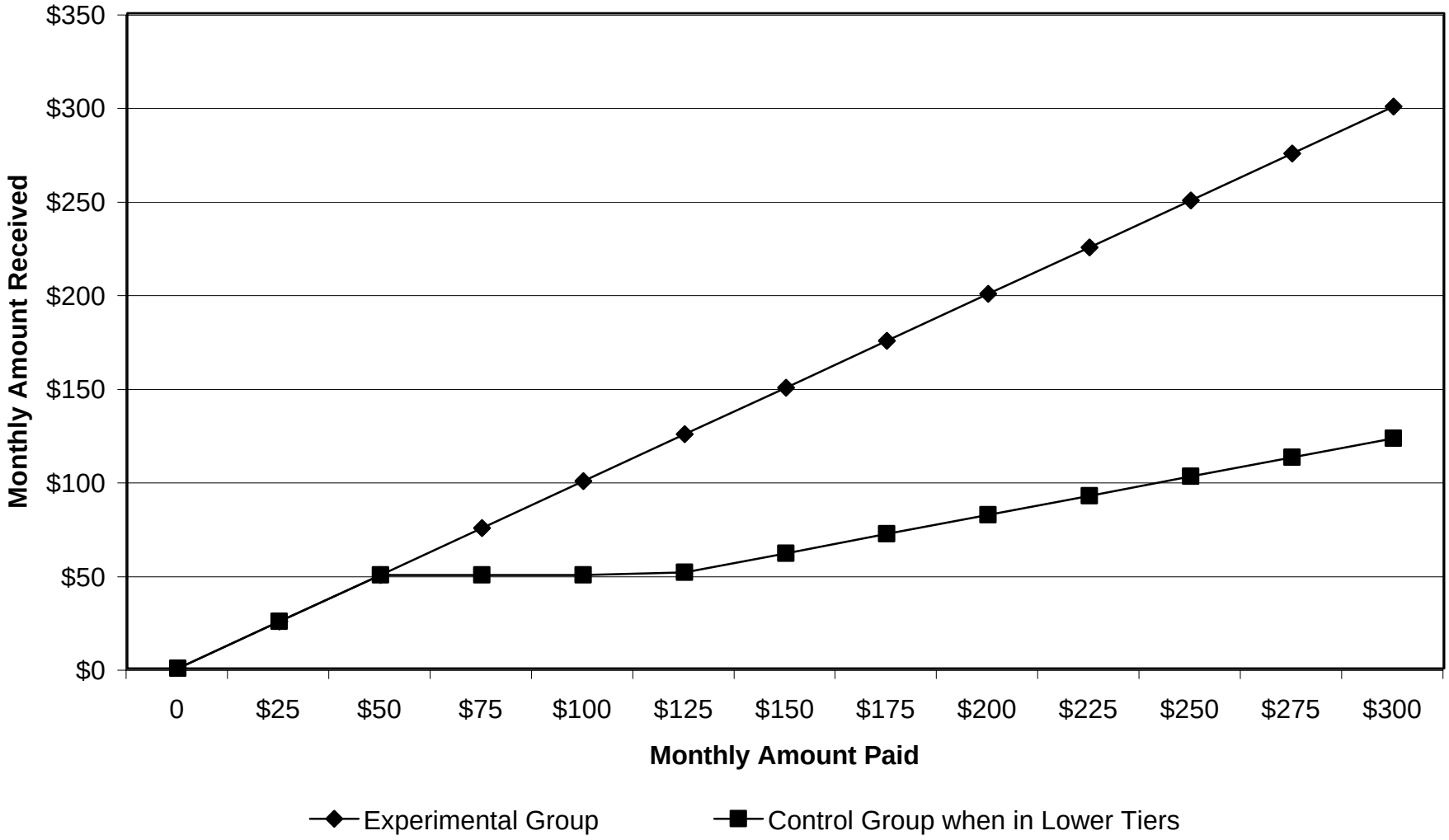
support. Amounts for past-due support and amounts collected through intercepting federal tax refunds both follow different distribution rules, primarily going to benefit the government first.

[7]These figures are based on 5,237 mothers who had fixed-dollar orders in the month they entered W-2.

Figure I.1.1
Amount of Child Support Received: Experimental vs. Control Group



Chapter 2
Implementation of the W-2 Child Support Reform[8]

Wisconsin's child support experiment occurred as a part of large changes in the state's overall cash assistance system. This chapter describes the experimental context and the key challenges encountered in implementing the experiment. Additional details are provided in Technical Report 2 in Volume III.

This report relies on information gathered through field observations and interviews with managers of child support and W-2 agencies, child support specialists, financial and employment planners (FEPs), and resource specialists. Interviews in the first year of the project (1998) were conducted in Dane, Douglas, Juneau, La Crosse, Milwaukee, and Racine counties. In 1999 and 2000 the interviews were conducted in Milwaukee County and included sessions with four private W-2 agencies in the county: Employment Solutions, Maximus, UMOS (United Migrant Opportunity Services), and YW-Works. The chapter also relies on two surveys of FEPs in W-2 agencies. The first was a statewide mail survey conducted in March and early April 1999. We received 287 responses, an estimated response rate of 61 percent. The second survey, in July 2000, focused on Milwaukee County and was directly administered by IRP staff in each of the five Milwaukee County W-2 agencies. We received 91 responses, an estimated response rate of 73 percent. Both surveys were voluntary.

I.2.1 The Context

The Child Support Demonstration Evaluation (CSDE) was initiated as a part of Wisconsin's W-2 program, which was put into operation over a seven-month period, from September 1997 through March 1998. Implementation of W-2 presented severe challenges. Among many other tasks, the automated family assistance management information system (Client Assistance for Re-employment and Economic Support, CARES) of the Wisconsin Department of Workforce Development (DWD) had to be reworked to track people through the tiers of W-2, new contracts with W-2 agencies had to be written, and W-2 agency staff had to be trained in new state policies. Perhaps most significantly, five private agencies had to be established or adapted to operate W-2 in Milwaukee County, the source of about 80 percent of the state's W-2 caseload. The five agencies, each handling cases residing in particular geographic districts of the county, had to hire entirely new staff and train them in W-2, community resources, agency policies, and use of the complex CARES system.[9] W-2 agency staff also had to be trained in the CSDE, including how to find in CARES whether a participant was a full or partial pass-through case, the implications of pass-through status, and how to assure that participants understood their pass-through status and its implications.

Enrollment in W-2 was unexpectedly low in the first years of the program. Because the state's initial contract with W-2 agencies, which covered the period from September 1997 through December 1999, had anticipated more participants, W-2 agencies could provide a high level of service to each case without worrying about cost overruns. Those who did enroll and remained in W-2, however, were probably on average more disadvantaged than were TANF participants in states, such as California and Minnesota, that incorporate a larger share of the working poor in their TANF cash assistance programs.

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[8]This chapter is primarily based on the work of Thomas Kaplan and Thomas Corbett, with the assistance of Victoria Mayer. Further information on the implementation of CSDE can be found in Volume III, Technical Report 2. Further information on the implementation of W-2 in general can be found in Volume II, Chapter 1.

[9]Some of these staff, however, had worked on programs that provided experience relevant to W-2, such as the Job Opportunity and Basic Skills (JOBS) component of Aid to Families with Dependent Children.

Although W-2 was the subject of much public attention in its early development, the CSDE component generally operated outside of public view. The only significant media coverage appeared in a report from a Milwaukee TV station soon after the demonstration began. The report described difficulties experienced by those in the reduced pass-through (control) group who did not immediately receive all their child support when they moved out of a tier in which they received a W-2 payment, a change in circumstances which should have made them eligible for a full pass-through. For the first year of the program, linkages between CARES and the child support data system did not make these adjustments automatically.

I.2.2 Administering the CSDE

Because the CSDE was both a welfare reform policy, potentially affecting the total income of recipients of cash assistance, and a change in child support policy, managers in the Wisconsin Department of Workforce Development could have chosen to place primary responsibility for implementing the CSDE in either the Bureau of Welfare Initiatives, which had operational responsibility for W-2, or the Bureau of Child Support. Managers chose the Bureau of Child Support, in part because they judged the other bureau to be overextended by competing demands associated with the implementation of W-2. This turned out to be a significant choice. As the CSDE developed, county child support agencies, with whom the Bureau of Child Support has routine formal and informal contact, played little role in CSDE implementation. W-2 agencies instead became primarily responsible for informing parents of their pass-through status and its implications, a task that ideally would have involved efforts by W-2 agencies to emphasize and reemphasize child support pass-through issues to W-2 participants.[10] For this emphasis to be realized in the complex environment of the early phases of W-2, the state would have had to repeatedly train W-2 agencies on the CSDE and repeatedly stress the potential of the experiment to influence future state and national policy. Staff in the Bureau of Child Support tried to accomplish this, setting up training sessions for W-2 agencies which, so far as we could judge, were of high quality. Bureau of Child Support staff were probably not, however, well positioned to make the CSDE a major part of the overall responsibilities of W-2 agencies during the early development of W-2.

I.2.3 Informing Resident and Nonresident Parents of Their Pass-Through Status

The most basic implementation activity—necessary for an actual experiment to have occurred—is assignment of resident parents to treatment and control groups and the provision of information to resident and nonresident parents concerning that assignment. The CARES computer system randomly generated these assignments. W-2 agencies were to inform resident parents of their experimental group assignment, the meaning of that assignment, and the fact that the experiment existed, when they applied for W-2. The state prepared a separate brochure for those subject and not subject to the partial pass-through, explaining and providing an example of how child support would be handled, according to their assignment. In addition, cases transitioning from AFDC to W-2 were sent a letter that notified them of the change and explained how child support would be handled, according to their assignment.

From the beginning of W-2, the state required W-2 agencies to present to participants a standard "Notice of Assignment of Child Support" form and to obtain participant signatures concerning the assignment of child support. However, the first draft of the form that the state distributed to W-2 agencies made no distinction between full or partial pass-through status. Although by the start of W-2 the state had developed a revised form that asked participants to identify their pass-through assignment and indicate with their signature whether their assignment had been explained to them, some Milwaukee agencies

[10]In staff surveys, 76 percent of child support workers said they never discussed pass-through status with resident parents, and about 80 percent of W-2 case managers (FEPs) agreed that providing information about pass-through status was a W-2 agency (rather than a child support agency) responsibility.

apparently continued to use the old draft forms for many months, and staff in one Milwaukee agency did not appear to know that the form in either version existed even in January 1999. Once W-2 participants were in the program, the CSDE was often not a central part of routine discussions between FEPs and program participants in Milwaukee, but it was discussed more commonly in the rest of the state.

I.2.4 Knowledge and Attitude of W-2 Staff Concerning the CSDE

Throughout much of the experiment, staff in most of the W-2 agencies outside of Milwaukee exhibited greater understanding of the CSDE than did those in Milwaukee. Some of the evidence for this derives from interviews with W-2 case managers. From the beginning of the experiment, staff in those W-2 agencies that had previously administered AFDC (which was the case in most counties except Milwaukee) exhibited an understanding of the pass-through demonstration. Almost all of the experienced FEPs we talked to in these agencies could describe the program accurately, and all said they mentioned it to resident parents starting on W-2.

The level of staff understanding was quite different in the Milwaukee W-2 agencies, which, again, served about 80 percent of the state's W-2 participants. For these agencies, everything—including the CARES system—was new, and the child support demonstration was not immediately understood, nor were explanations of it made a part of routine agency operations. Understanding of the pass-through policy grew incrementally, and in early 1999 the state reemphasized training in the pass-through demonstration. We observed two Milwaukee training sessions, which seemed to be effective in describing the policies for the full and partial pass-throughs and the relevant CARES screens, using a series of very clear examples. The training sessions also seemed to convey effectively that the demonstration was a high priority of DWD and that W-2 agencies should make the explanation of the pass-through to participants a high priority of their own.

However, even after the intensive retraining that occurred in January 1999 in Milwaukee, surveys of FEPs confirmed the greater understanding of the CSDE outside of Milwaukee. Table I.2.1, based on a survey to which FEPs responded in spring 1999, summarizes answers to questions about knowledge of the CSDE.

Although it might have been desirable for FEPs to include the CSDE (and other child support topics) in their case management approaches to a greater extent than they did, implementation of the CSDE was not totally dependent on the FEPs. Initial sessions with new applicants to W-2 in Milwaukee County were handled by a different category of worker, called Resource Specialists, who were responsible for providing an initial explanation of the CSDE and informing applicants of their pass-through status. Our interviews with Resource Specialists indicated that they knew about the CSDE and could explain its implications to program participants (although they did not always know that they were supposed to have participants sign a Notice of Assignment form). In addition, one Milwaukee County child support specialist was stationed in each W-2 agency and available to answer questions on child support. We thus believe that most new applicants to W-2 received a quick explanation of the CSDE and their pass-through status by mid-1998, but that subsequent reinforcement from FEPs was probably provided infrequently in the Milwaukee W-2 agencies.

I.2.5 Resident and Nonresident Parent Knowledge of Child Support Pass-Through Policy

Given the difficulties that many staff members experienced in understanding the CSDE, it would be not be surprising if parents did not understand the way their child support would be treated. To assess the level of understanding, the Survey of Wisconsin Works Families, described below in Volume I, Chapter 3, asked resident mothers several questions about the way the child support system worked. We focus here on the responses to two questions that were asked in both 1999 and 2000. The first question

Table I.2.1
Percentages of FEPs Who Correctly Answered Questions Concerning Knowledge of the CSDE, March–April 1999

Response	Milwaukee County (N = 99)	Other Urban Counties (N = 85)	Rural Counties (N = 103)	All Respondents (N = 287)
Knew at least 1 CARES screen to check pass-through status	53.5%	84.7%	81.6%	72.8%
Knew the CARES code indicating partial pass-through status	51.5	77.7	86.4	71.8
Knew that those assigned to partial pass-through would have same assignment if they applied for W-2 again at a later time	73.1	82.1	85.0	80.1

Source: Survey of W-2 Staff, Institute for Research on Poverty, 1999.

Notes: "Other urban counties" are Brown, Calumet, Chippewa, Dane, Douglas, Eau Claire, Kenosha, La Crosse, Marathon, Outagamie, Ozaukee, Pierce, Racine, Rock, St. Croix, Sheboygan, Washington, Waukesha, and Winnebago counties. "Rural counties" are all counties in Wisconsin except for Milwaukee County and the other urban counties.

asked respondents whether they would receive all child support if they were receiving a check from W-2. The correct answer depends on experimental group status: for mothers in the experimental group, the correct answer is "I would receive all"; for those in the control group, the correct answer is "the state would keep some." The second question asked whether they would receive all child support if they were not receiving a check from W-2.[11] The correct answer for both groups is "I would receive all."

Few mothers answered these questions correctly, and many (about one-quarter to one-third) said they did not know what would happen. About half the mothers understood that they would receive all child support if they were not receiving W-2 payments, but only about one-quarter of those in the experimental group reported that they would receive all child support if they were in a W-2 payment tier. These responses are relatively consistent with the way the child support was treated under AFDC, when all mothers received only a portion of child support paid on their behalf when they were receiving payments, and all mothers received all child support if they were not receiving payments.

The survey also asked comparable questions of nonresident fathers. Knowledge among fathers was even lower than among mothers. Nearly half of fathers reported that they did not know what would happen if they paid support. As was true of mothers, more fathers knew what would happen if the mother was not receiving payments, and few experimental-group fathers correctly responded that the mother would receive all child support if she were receiving payments.

Combining the responses to these two questions, only 26 percent of mothers correctly answered both questions in 1999. Knowledge was even lower among fathers, with only 12 percent correctly answering both questions in 1999. Among both mothers and fathers, there was no increase in knowledge between 1999 and 2000.

In no subgroup that we examined did more than half respond correctly to both questions.[12] Mothers in the control group had slightly greater knowledge, as did those who entered in upper tiers and those with less recent AFDC history. In contrast to what our observations and surveys of program staff led us to expect, mothers in Milwaukee County had no less knowledge than mothers in the rest of the state in both 1999 and 2000. Mothers with a history of higher levels of child support paid on their behalf before entering W-2 were the most knowledgeable, perhaps because child support was most relevant to them, or perhaps because they continued to get payments under W-2 and had direct knowledge of how payments were being treated. Mothers who answered both questions correctly in 1999 were substantially more likely to answer both questions right in 2000; however, even among this group, fewer than half responded correctly in 2000. We did not find a discernible difference based on mother's educational level or the quarter in which she entered W-2.

There were fewer detectable differences for fathers, particularly in 2000. In 1999, in contrast to the findings for mothers, fathers of children in the experimental group had greater knowledge than fathers of children in the control group, and fathers in other urban counties had greater knowledge than those in Milwaukee. No other variables consistently predict knowledge levels. Similar to the findings for mothers, those who reported correct answers to both questions in 1999 were more likely to do so in 2000, but even this group had low overall knowledge. Finally, for couples in which both parents answered the survey, mothers' knowledge is not statistically related to fathers' knowledge.

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[11]Specifically, the first question was: "If you were in a W-2 assignment where you received a check from W-2, would you receive all of the *current* child support <child's name>'s father paid or would the state keep some of it?" The second question was "If you were *not* receiving a check from W-2, would you receive all of the *current* child support <child's name>'s father paid or would the state keep some of it?"

[12]We conducted a probit analysis on mothers' and fathers' knowledge to determine which of the bivariate relationships discussed here held in a descriptive multivariate context. The text discusses only those variables found to be statistically significant ($p < .10$).

In sum, we found quite low levels of parental knowledge of pass-through policy. Perhaps the questions were not understood correctly. However, these responses are consistent with the results of the survey of workers and our field observations, in the sense that all suggest that implementation difficulties may have limited the reform's impact.

Chapter 3
Data, Sample, and Methods for Evaluating the Child Support Reform

In this section we discuss the primary data sources used for the CSDE and describe the research population and the samples used in our analysis. We also outline our basic approach in evaluating the impacts of the CSDE. This section largely summarizes material presented in greater detail in the Technical Reports in Volume III of this report (see text box).

Contents of Volume III

- The CSDE Experimental Design
- Implementation of the CSDE
- Administrative Data Sources
- Samples and Weighting
- Design and Content of the Survey of Wisconsin Works Families
- Nonresponse in the Survey of Wisconsin Works Families

The CSDE experimental evaluation draws on two primary data sources, administrative records and a survey. Section 1 of this chapter discusses the administrative data, which were constructed by merging three different databases and include information on all W-2 families. The second primary source is the Survey of Wisconsin Works Families, which includes a random sample of mothers who were W-2 participants and the father of one of their children. We discuss the survey in Section 2 of the chapter.

I.3.1 Administrative Data and Samples

Administrative Data Sources

The main administrative database used for the CSDE is CARES (Client Assistance for Re-employment and Economic Support), which contains information on W-2 applicants and participants and the code for their random assignment to either the experimental group or the control group. It also includes information on other programs for low-income families, including Food Stamps, Medicaid, and child care subsidies. CARES data include not only whether participants received payments or services, but also such demographic information as birth dates, number of children, family composition, marital status, educational background, and residential location. We used CARES to identify a research sample, to monitor and measure the use of W-2 and other programs, and to provide background information. CARES contains updated information on those continuing to participate in W-2 and, in addition, those who receive other services even if they no longer receive W-2.[13]

The second administrative database is KIDS (Kids Information Data System), the administrative database for child support. KIDS contains information on child support orders, payments, past-due amounts (arrearages), the method of payment (wage withholding, tax intercepts), the distribution of the payment (resident parent, state), and demographic information about the parents and children in the case (birth dates, residential location of both parents). It can include information on dates of marriage and divorce and usually contains the date of paternity establishment for nonmarital children. All W-2 cases in

[13]Data are entered into CARES at application, and updated when eligibility is redetermined. This happens monthly for W-2, every 3 months for Food Stamps, and every 6 months for Medicaid. Under BadgerCare, eligibility will be redetermined every 12 months. Additionally, under all programs, participants are to report changes in income and family situation as they occur.

which there is child support potential (i.e., a living nonresident parent) are included in KIDS.[14] KIDS does include many cases without child support orders, but with child support potential: nonmarital cases in which the paternity adjudication process has begun, cohabiting nonmarital cases, and cases in which no child support order has been made owing to extenuating circumstances, such as problems in locating the nonresident parent, exemptions for good cause (e.g., domestic violence), the economic situation of the nonresident parent, or cases in which the parents agree that there will be no order.

Although KIDS has valuable information on child support, there are limitations. Some types of information are often missing in KIDS, particularly in older cases that were loaded onto the data system in 1996 when KIDS began. This includes information on income, information about the child's living arrangements (physical custody/placement awards), and dates of paternity establishment. In addition, some information of interest is not included in any administrative data, such as informal payments of child support (in cash or in kind), the actual residence of children, and contact between the nonresident parent and children.

The third administrative database we use is the Unemployment Insurance Wage Record Files. Unemployment Insurance (UI) wage file data provide quarterly earnings for individual covered workers, by employer. "Covered" workers include about 91 percent of Wisconsin workers. Not covered are the self-employed, federal employees, commission sales workers, farmers, church employees, and employees of not-for-profit organizations with fewer than four workers. There is a lag of about six months between the end of a quarter and the time at which the information is complete. The wage file contains information only on individuals working in Wisconsin. It does not contain information on occupations, the hourly wage, or the number of hours worked per quarter.

Records from these three data sources were linked to each other by use of Social Security numbers, or, lacking Social Security numbers in the data, by CARES case numbers and KIDS identification numbers. Data used in this report are based on CARES, KIDS, and UI data extracted in July and August 2000.

Administrative Data Sample

The basic research sample used in our analyses of administrative data includes cases that received a random assignment code, had entered W-2 by July 8, 1998, were demographically eligible for child support (there was a living nonresident parent),[15] met other sample criteria primarily associated with timely progression in the intake process,[16] and in which the mother was the resident parent. The sample includes cases that actually entered W-2 rather than all cases that received an assignment. In the state as a whole, about three-fifths of both the experimental group and the control group entered W-2. A multivariate analysis (detailed in Volume III, Technical Report 1) confirms that the rate of entry onto W-2 is the same for the experimental and control groups.[17]

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[14]In less than one percent of W-2 families, the CARES record shows them as referred to child support, but there is no record of the family in the KIDS data system. Our investigation into these cases showed that most are appropriately included in our sample as having no child support activity. Although a small number of these cases are errors and should be excluded from the sample, it is not always possible to distinguish these cases from those that really had no child support activity to date. Since most of the nonmatching cases should be included, we have included all of them.

[15]In other words, we exclude cases in which records indicate that the fathers of all children are dead and cases in which all children lived with both parents.

[16]See Volume III, Technical Report 3 for details.

[17]We limit our primary analysis to those who entered W-2 because the information available on those who did not enter is often very limited and because we want to be able to discuss the circumstances of W-2 participants.

The original design called for a sample of 8,000, half of them drawn from the stock of AFDC cases active in August 1997, the remainder from the flow of cases applying for assistance after the implementation of W-2 in September 1997. Because the rate of new entrants to W-2 was slower than anticipated, the assignment rates for new cases changed over time.[18] We report results weighted to account for these different assignment rates.

The final main research population includes 15,977 resident mothers, 73 percent of all W-2 cases headed by a single mother that had entered W-2 by July 8, 1998.[19] Outcomes for this sample are tracked for both those women who stay on W-2 and those who leave.[20] We include only cases with child support potential, because only those cases are potentially affected by the reform. Thus we exclude two-parent families, cases in which the nonresident parent is known to have died, and cases in which the mother has a "good cause" exemption from pursuing child support for any of her children (typically because of domestic violence). We have also excluded a limited number of cases that do not fit the typical pattern of program participation. These include cases in which the family was inadvertently assigned to AFDC after W-2 had begun, a few cases that had no minor children listed on the case, some types of cases that include children with a disability[21] and some cases in which there was an extended delay before the family entered W-2. Finally, our primary analyses include only cases in which the W-2 recipient (and thus the resident parent) is the mother. Cases in which the W-2 recipient is the father (and thus the mother is the nonresident parent) are relatively rare and are systematically different from mother-custody cases.[22] Volume III, Technical Report 1 discusses these exclusions and the characteristics of the included and excluded cases in detail.

Figure I.3.1 shows the relationships among the three main samples from the administrative data: (1) resident mothers, (2) nonresident fathers with legally established paternity when the mother entered W-2 ("legal fathers"), and (3) children—some with and some without legally established paternity at entry. The 15,977 mothers included in the first sample can be divided into those with only marital children when they entered W-2 (Box 1A, 7.3 percent of mothers), those with both marital and nonmarital children at entry (Box 1B, 7.6 percent of mothers), and those with only nonmarital children at entry (Box 1C, the vast majority of mothers, 84.1 percent). About 1 percent of mothers were pregnant when they entered W-2 and had no other children (Box 1D).

The derivation of the sample of legal nonresident fathers (and couples) can also be seen on the figure. Mothers with only marital children (Box 1A) are each associated with a nonresident father, and a few are associated with more than one. Mothers with both marital and nonmarital children (Box 1B) are each associated with at least one legal father (from the marital children); the nonmarital children may or may not have a legal father at the time of W-2 entry. Finally, mothers with only nonmarital children (Box 1C) may be associated with no legal father, one legal father, or more. The figure shows a total sample of fathers (and couples) of 14,343, primarily fathers of nonmarital children.

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[18]Among the initial AFDC cases in August 1997, and from September 1997 through March 16, 1998, 20 percent of cases were assigned to the control group, 20 percent to the experimental group, and the remainder received the experimental-group treatment but were not eligible for the survey (a "nonexperimental" group). From March 17 to May 8, 1998, 30 percent of new applicants were assigned to the experimental group, 30 percent to the control group, and 40 percent to the experimental-group treatment, but without survey eligibility. Beginning May 11, 1998, 50 percent of new applicants were assigned to the experimental group and 50 percent to the control group.

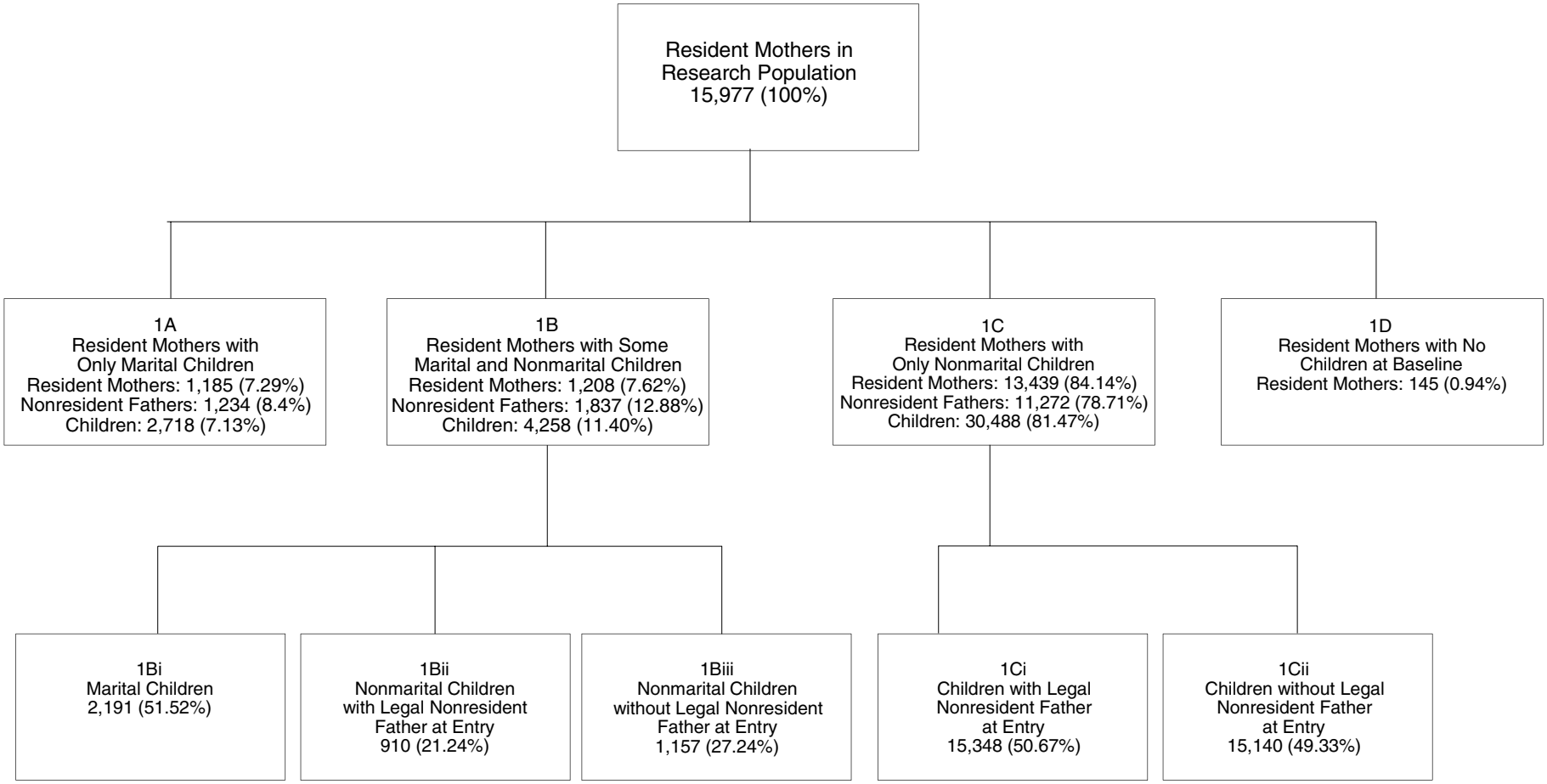
[19]The derivation of the research population is described in detail in Volume III, Technical Report 1.

[20]Since both leavers and stayers are analyzed, the results are not directly comparable with other studies of leavers (State of Wisconsin, DWD, 2000; Cancian et al., 1999; Loprest, 1999).

[21]Federal law does not allow the state to retain a portion of the child support paid on behalf of children receiving Supplemental Security Income (SSI) benefits, so these families were excluded from our research sample.

[22]We provide limited information on resident fathers for selected outcomes.

Figure I.3.1
Research Population, Phase 1



Note: Percentages are weighted to reflect differential assignment rates over time.

Finally, the sample of children needing paternity establishment comes from the mothers with nonmarital births (Box 1B and 1C). About half of the nonmarital children did not have legal fathers when their mothers entered W-2, for a total sample of 16,297 (1,157 from Box 1B and 15,140 from Box 1C).

Our analysis of outcomes for resident mothers and nonresident fathers includes all couples. Thus, when a mother is associated with more than one father, each father is included in our analysis of fathers' outcomes—so that a single mother may be implicitly counted more than once when we examine fathers (and couples). In the less common case, when a father is associated with more than one mother in our sample, we count a single father as many times as he is part of a couple.[23] About 30 percent of the 15,977 mothers in our sample have only nonmarital children who have not had paternity established, so no fathers associated with these mothers are in our sample. Of the remaining 11,179 mothers, 28 percent are associated with more than one legal father. Of the 13,339 fathers, 6.6 percent are associated with more than one mother in our sample, and thus are counted more than once, bringing the total to 14,343 couples.[24]

Table I.3.1 shows the initial characteristics of the resident mothers included in the research sample and some key subsamples. Column 1 shows that 70 percent of mothers transitioned to W-2 from AFDC, and the remaining 30 percent entered W-2 directly. In the full sample, 13 percent had no history of welfare receipt in Wisconsin in the two years prior to entry, and over half had received AFDC for at least 19 months in the previous two years. Most entered W-2 in a Community Service Job; about 10 percent each entered in W-2 Transitions or in the Caretaker of Newborns program. The remaining 31 percent entered in an upper tier, in which they received no cash payments. The remaining panels of the table show that most mothers entering W-2 resided in Milwaukee County (74 percent), were young (67 percent were 30 years old or younger), African-American (60 percent), had less than a high school degree (53 percent), had one or two children (62 percent), and had at least one child of preschool age or younger (76 percent).

Columns 2–5 in Table I.3.1 show the characteristics of four subgroups of mothers whom we expect to be particularly affected by the CSDE. We noted above that when control-group mothers participate in the lower tiers of W-2, they receive a partial pass-through, but when they participate in the upper tiers or are nonparticipants, they receive the full amount. If cases that enter W-2 in an upper tier are unlikely ever to participate in a lower tier, then experimental status is largely irrelevant to these women. On the other hand, if those initially assigned to the upper tiers later enter lower tiers and are subject to the reduced pass-through, or if experimental/control status affects initial assignment,[25] then all cases should be considered in the evaluation of experimental impacts. Given these competing concerns, our main research analysis sample includes W-2 entries in any tier (n=15,977). However, we also show separate results for the 9,634 mothers in our research sample who entered lower tiers, the group for whom the experiment is most relevant.

Column 2 in Table I.3.1 shows the characteristics of mothers who entered W-2 in a lower tier, in which they received cash assistance (W-2 Transitions or Community Service Jobs). Because the lower tiers of W-2 are generally reserved for those less able to move directly to work, we would expect this subsample to include women more likely to face barriers to employment. In most cases the differences in initial characteristics are consistent with this expectation, though in some cases they are modest. Lower-

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[23]Random assignment was based explicitly on resident parents (mothers in the samples we consider). Thus each mother is in only one group (experimental or control), but an individual father could be in more than one if he has children with more than one mother who enter W-2.

[24]See Volume III, Technical Report 1 for a discussion of the implications of this approach.

[25]Volume III, Technical Report 1 shows limited evidence that control-group members who received higher amounts of child support in the past are somewhat more likely to be placed in the upper tiers than comparable experimental-group members.

Table I.3.1
Initial Characteristics of Resident Mothers in Research Sample

	(1)		(2)		(3)		(4)		(5)	
	Total		Lower-Tier Mothers		Mothers with No Recent AFDC Experience		Mothers with Order at Entry		Mothers with Higher Child Support History	
Characteristics	N	%	N	%	N	%	N	%	N	%
Number of Mothers	15,977		9,634		2,005		8,924		2,744	
Case Type										
AFDC	11,333	70.0	7,170	73.5			6,899	76.5	1,950	69.9
W-2	4,644	30.0	2,464	26.5	2,005	100.0	2,025	23.5	794	30.1
AFDC Receipt before Entry										
None	2,005	12.8	897	9.6	2,005	100.0	406	4.7	227	8.8
1–18 months	5,332	33.3	3,124	32.3	76	8.2	2,545	28.0	783	28.4
19–24 months	8,640	53.9	5,613	58.1			5,973	67.3	1,734	62.8
Initial W-2 Assignment										
W-2 Transition	1,540	9.6	1,540	16.0	248	12.5	852	9.5	387	14.1
Community Service Job	8,094	50.6	8,094	84.0	649	32.7	4,596	51.5	1,186	41.5
Caretaker of Newborn	1,392	8.9			653	32.8	466	5.3	162	6.3
Upper tier	4,951	30.9			455	22.0	3,010	33.8	1,009	38.1
Quarter of Entry										
September–December 1997	8,754	54.7	4,913	51.0	596	29.0	5,040	56.6	1,582	57.8
January–March 1998	5,702	35.7	3,812	39.6	725	37.1	3,260	36.4	916	33.0
April–June 1998	1,521	9.6	909	9.4	684	34.0	624	7.0	246	9.3
Location of Resident Parent										
Milwaukee County	11,856	74.1	7,906	82.3	1,014	51.1	6,877	77.2	1,763	64.0
Other urban counties	2,746	17.2	1,149	11.9	564	27.5	1,367	15.0	583	21.3
Rural counties and tribes	1,375	8.7	579	5.8	427	21.5	680	7.8	398	14.8
Age of Resident Parent										
16–17	6	0.0	2	0.0	5	0.2	1	0.0		
18–25	7,507	47.4	4,304	45.1	1,246	62.2	3,832	43.3	744	27.3
26–30	3,276	20.6	1,915	19.9	275	13.2	2,156	24.5	742	28.0
31–40	4,225	26.0	2,690	27.5	391	19.7	2,508	27.6	1,059	38.0
41+	961	6.0	721	7.6	87	4.6	427	4.6	199	6.7
Missing	2	0.0	2	0.0	1	0.1				

Table I.3.1, continued

Characteristics	(1) Total		(2) Lower-Tier Mothers		(3) Mothers with No Recent AFDC Experience		(4) Mothers with Order at Entry		(5) Mothers with Higher Child Support History	
	N	%	N	%	N	%	N	%	N	%
Race of Resident Parent										
White	4,001	25.4	1,842	18.9	917	45.5	2,143	24.4	1,071	39.9
African American	9,640	60.0	6,425	66.9	835	41.8	5,774	64.5	1,361	49.0
Hispanic	1,200	7.4	694	7.0	98	5.0	519	5.6	166	6.0
Native American	365	2.3	190	2.0	40	1.7	212	2.4	75	2.8
Asian	274	1.6	183	1.9	16	1.0	54	0.6	23	0.8
Other	16	0.1	8	0.1	5	0.2	5	0.1		
Unknown	481	3.2	292	3.2	94	4.9	217	2.5	48	1.5
Education of Resident Parent										
Less than high school	8,605	53.4	5,748	59.2	928	45.1	4,702	52.5	1,227	43.6
High school diploma	5,829	36.9	3,097	32.5	813	41.7	3,331	37.5	1,150	42.7
Some beyond high school	1,543	9.7	789	8.3	264	13.2	891	10.0	367	13.6
Language of Resident Parent										
English	15,498	97.1	9,298	96.6	1,977	98.5	8,824	98.9	2,715	99.1
Non-English	479	2.9	336	3.4	28	1.5	100	1.1	29	0.9
Number of Children										
None	145	0.9	25	0.3	117	5.9	7	0.1	3	0.1
One	5,169	32.0	3,104	31.8	1,184	58.4	1,938	21.4	503	17.9
Two	4,677	29.6	2,793	29.2	416	20.5	2,917	33.0	853	32.4
Three or more	5,986	37.4	3,712	38.7	288	15.3	4,062	45.6	1,385	49.6
Age of Youngest Child										
Unborn Child at Entry	1,599	10.0	1,094	11.5	299	15.2	842	9.4	217	7.7
0–2	7,685	48.2	4,169	43.1	1,230	61.1	3,955	43.9	1,063	38.2
3–5	2,872	18.0	1,783	18.5	141	7.1	1,984	22.6	609	23.3
6–12	3,106	19.6	2,051	21.6	257	12.9	1,819	20.8	713	25.8
12–18	695	4.2	523	5.3	71	3.4	319	3.3	140	4.9
Missing birth date	20	0.1	14	0.2	7	0.3	5	0.1	2	0.1

Note: Percentages are weighted.

tier cases are somewhat more likely to have transitioned to W-2 from AFDC, to have more substantial AFDC histories prior to W-2 entry, to live in Milwaukee, to be African-American, and to have less than a high school education.

We expect those who had no recent AFDC receipt to be most responsive to the child support policy change. Long-term AFDC recipients are likely to have adapted to the old, partial-pass-through regime—for example, they may have long-standing patterns of informal child support arrangements with the fathers of their children. Moreover, they may not understand the implications of their experimental-group status. New W-2 recipients, in contrast, may focus more attention on the child support policies and may not have to change long-standing patterns. Column 3 shows the characteristics of the 2,005 new W-2 recipients. In general these mothers have fewer barriers to unemployment than those with longer AFDC histories.

The third group we expect to be more responsive to the policy change consists of those with child support orders when they entered W-2. These mothers are poised to press fathers to pay more support and so may quickly benefit from the policy change. In contrast, the policy change may have less effect on those without orders, because formal payments could not begin until the parents went through a potentially lengthy process of establishing an order (and maybe also of establishing paternity). Column 4 shows characteristics of the 8,924 mothers with orders. These mothers are generally similar to the whole research sample, but they have longer AFDC histories.

Finally, we expect larger effects among those with former partners who have a history of paying a substantial amount of child support. Many experimental-group mothers in this subgroup will see an immediate increase in the amount of child support they receive; this income could then have a variety of secondary effects that would be less likely among those who receive smaller amounts. Column 5 shows characteristics of the 2,744 mothers who received at least $1,000 of child support in the year prior to September 1997. These mothers are more likely to live in rural areas, to be older, to be white, and to have higher levels of education.

Random assignment to the experimental or control group is based on the resident parent, almost always the mother. However, resident mothers are not the only individuals whose behavior is expected to respond to the policy change. Supporters of a full pass-through have suggested, for example, that nonresident fathers may begin to pay child support if that support directly benefits their children. Thus our second main research sample drawn from the administrative data consists of couples—the parents of the children who are part of the mother's research sample. In these couples, we include only "legal" fathers (those with marital children or those who have had paternity legally established).[26] In addition, in our main analyses, we include only those who were legal fathers when the children entered W-2, showing separate analyses of couples in which the fathers were named as legal fathers after their children entered the program. This results in a sample of 14,343 nonresident couples, which we refer to as the "legal father" sample.

Most of the information provided in Table I.3.2 refers to the characteristics of the mothers associated with each father. These figures differ from the parallel panels in Table I.3.1 because mothers with no legal paternity established are not represented in the table. Moreover, as discussed above in reference to Figure I.3.1, a resident mother (or nonresident father) may be part of more than one couple. Not surprisingly, the characteristics of mothers in a legal couple are similar to the characteristics of mothers in the previous table. One difference is that the mothers associated with legal fathers are less likely to have entered in the Caretaker of Newborn tier; this is as expected, in that women in this tier are unlikely to have legal paternity established for their youngest child at the time that they enter this

[26]We could include "potential" fathers as well, because these are represented in the KIDS data; we do not because the child's father could be later found to be someone else, and, in fact, some cases have multiple potential fathers of the same child.

Table I.3.2
Initial Characteristics of Nonresident Legal Fathers and Their Associated Resident Mothers

	(1)		(2)		(3)		(4)		(5)	
	All		Associated with Lower-Tier Mothers		Associated with Mothers with No Recent AFDC Experience		With a Child Support Order		With Higher Child Support History	
Characteristics	N	%	N	%	N	%	N	%	N	%
All Nonresident Fathers	14,343		8,767		850		10,569		2,694	
Mother's Case Type										
AFDC	10,835	74.7	6,780	76.6			8,256	77.2	1,919	70.18
W-2	3,508	25.4	1,987	23.4	850	100.0	2,313	22.8	775	29.8
Mother's AFDC Receipt Before Entry										
None	850	6.0	465	5.6	850	100.0	402	3.9	211	8.2
1–18 months	4,065	28.0	2,281	25.7			2,779	25.8	763	28.1
19–24 months	9,428	65.9	6,021	68.8			7,388	70.3	1,720	63.7
Mother's Initial W-2 Assignment										
W-2 Transition	1,396	9.6	1,396	15.7	195	22.8	957	8.9	385	14.1
Community Service Job	7,371	51.6	7,371	84.3	270	33.8	5,468	51.8	1,148	40.8
Caretaker of Newborn	719	5.2			109	12.6	513	4.9	156	6.3
Upper tier	4,857	33.7			276	30.8	3,631	34.4	1,005	38.8
Mother's Quarter of Entry										
September–December 1997	7,922	55.2	4,431	50.3	263	31.4	5,920	56.1	1,571	58.3
January–March 1998	5,319	37.1	3,602	41.3	313	36.7	3,937	37.1	885	32.7
April–June 1998	1,102	7.7	734	8.4	274	32.0	712	6.8	238	9.0
Location of Resident Parent										
Milwaukee County	10,865	75.9	7,180	82.5	356	44.0	8,225	78.0	1,717	63.8
Other urban counties	2,238	15.4	1,033	11.4	241	26.5	1,570	14.6	580	21.5
Rural counties and tribes	1,240	8.7	554	6.1	253	29.5	774	7.4	397	14.7

Table I.3.2, continued

Characteristics	(1) All N	(1) All %	(2) Associated with Lower-Tier Mothers N	(2) Associated with Lower-Tier Mothers %	(3) Associated with Mothers with No Recent AFDC Experience N	(3) Associated with Mothers with No Recent AFDC Experience %	(4) With a Child Support Order N	(4) With a Child Support Order %	(5) With Higher Child Support History N	(5) With Higher Child Support History %
Age of Nonresident Parent										
16–17	1	0.0	1	0.0	1	0.1	1	0.0		
18–25	5,721	40.3	3,412	39.6	282	34.8	4,443	42.4	743	27.7
26–30	3,789	26.6	2,276	25.9	182	19.8	2,834	27.2	775	29.7
31–40	4,147	28.3	2,575	28.7	318	36.9	2,874	26.5	999	36.4
41+	685	4.8	503	5.8	67	8.5	417	3.9	177	6.1
Race of Nonresident Parent										
White	1,814	12.6	856	9.5	296	34.4	1,232	11.5	596	22.2
African American	5,911	41.7	3,838	44.1	180	21.6	4,500	42.9	646	24.3
Hispanic	600	4.0	319	3.3	27	3.0	393	3.5	106	3.9
Native American	234	1.7	122	1.5	16	1.7	161	1.6	32	1.1
Asian	85	0.6	60	0.7	2	0.4	34	0.4	13	0.5
Unknown	5,699	39.5	3,572	40.8	329	39.0	4,249	40.1	1,301	48.0
Education of Resident Parent										
Less than high school	7,632	53.1	5,128	58.1	282	33.7	5,618	52.9	1,193	43.6
High school diploma	5,320	37.1	2,917	33.4	398	45.5	3,930	37.4	1,148	43.2
Some beyond high school	1,391	9.8	722	8.5	170	20.8	1,021	9.7	353	13.2
Language of Resident Parent										
English	14,091	98.3	8,586	98.0	842	99.0	10,467	99.0	2,667	99.1
Non-English	252	1.7	181	2.0	8	1.0	102	1.0	27	0.9
Number of Children										
One	9,690	67.8	5,994	68.5	566	66.6	7,223	68.7	1,756	65.1
Two	3,044	21.1	1,819	20.7	210	23.0	2,237	21.0	588	22.6
Three or more	1,609	11.1	954	10.8	74	10.4	1,109	10.3	350	12.3

Table I.3.2, continued

Characteristics	(1) All N	(1) All %	(2) Associated with Lower-Tier Mothers N	(2) Associated with Lower-Tier Mothers %	(3) Associated with Mothers with No Recent AFDC Experience N	(3) Associated with Mothers with No Recent AFDC Experience %	(4) With a Child Support Order N	(4) With a Child Support Order %	(5) With Higher Child Support History N	(5) With Higher Child Support History %
Age of Youngest Child										
Unborn child at entry	193	1.3	144	1.6	19	2.3	115	1.0	14	0.4
0–2	3,592	24.9	2,053	23.1	251	29.9	2,438	22.9	467	17.7
3–5	4,028	28.2	2,424	27.8	171	21.2	3,272	31.3	770	29.6
6–12	5,382	37.8	3,358	38.6	314	36.2	3,991	38.1	1,166	42.7
12–18	1,103	7.6	757	8.6	94	10.4	720	6.5	270	9.5
Missing birth date	45	0.3	31	0.3	1	0.1	33	0.3	7	0.2
Pre-Entry Quarters of Employment of Nonresident Parent										
None	4,119	28.8	2,625	30.1	239	28.5	2,819	27.0	334	12.5
1–4 Quarters	3,414	24.0	2,118	24.7	121	14.8	2,568	24.6	196	7.1
5–7 Quarters	2,927	20.7	1,677	19.4	160	17.7	2,246	21.4	579	21.8
8 Quarters	3,213	21.9	1,898	20.9	281	33.1	2,574	23.6	1,560	57.8
Missing, no SSN	670	4.6	449	4.9	49	5.9	362	3.4	25	0.8
Pre-Entry Annualized Earnings of Nonresident Parent										
None	4,119	28.8	2,625	30.1	239	28.5	2,819	27.0	334	12.5
$1–$5,000	5,248	37.1	3,179	37.1	199	23.6	3,957	38.1	349	14.4
$5,000–$15,000	2,836	19.4	1,616	17.7	191	22.3	2,293	20.9	1,046	37.1
$15,000–$25,000	1,010	7.0	620	7.1	106	12.7	786	7.4	610	22.9
$25,000 or more	460	3.1	278	3.2	66	7.0	352	3.3	330	12.3
Missing, no SSN	670	4.6	449	4.9	49	5.9	362	3.4	25	0.8

Note: Percentages are weighted.

program. Table I.3.2 also includes information on some characteristics of the fathers themselves. These fathers have low levels of reported employment: only about 20 percent of nonresident fathers have some earnings in every quarter of the two years prior to entry, and 29 percent have no earnings in any quarter. Most fathers have very low annual earnings: of those with some earnings, most averaged less than $5,000 per year in the two years before entry, and only 3 percent averaged more than $25,000 per year. Considering basic demographic characteristics shown in the table, nonresident fathers are somewhat older than resident mothers, reflecting both the tendency for men to be older than their partners and the delay associated with paternity establishment. Their children also tend to be older (again in part reflecting the delay in paternity establishment).[27]

The remaining columns show the key subgroups for fathers. For reasons described above, we conduct separate analyses on the 8,767 fathers of children whose mothers entered in lower tiers, the 850 fathers of children whose mothers had no recent AFDC history, the 10,569 fathers who had been ordered to pay child support when their children entered W-2, and the 2,694 fathers who had paid over $1,000 in child support in the year prior to their children's W-2 entry. In most respects the patterns shown for these subsamples are consistent with those shown for resident mothers' subsamples in Table I.3.1. As expected, fathers paying substantial amounts of child support have more quarters of prior employment and higher previous earnings.

The experiment could affect *children* as well as parents. The administrative data contain few outcomes for children; we focus on only one outcome—whether a nonmarital child has had paternity established.[28] Our third base sample from the administrative data consists of children who did not have paternity established when they entered W-2; we examine whether these children had paternity established at several points in time after they entered W-2.

The administrative data discussed here allow us to define the samples of interest, and provide substantial information on *all resident mothers*, and more limited information on *all legal nonresident fathers*. Although the administrative data provide these full samples, and the available information on them is quite complete, many areas of interest in the CSDE cannot be adequately addressed using these data alone. For this reason, the administrative data were used to define a target sample of cases for a companion survey, as discussed in the next section.

I.3.2 Survey Data and Samples

The *Survey of Wisconsin Works Families* is a panel study of mothers who participated in W-2 and of the legal fathers of a randomly selected focal child. We collected data in two waves; the first period of data collection measures families' experiences during 1998—the first year that the Wisconsin Works program was in place—and the second period focuses on 1999. We briefly describe the design and content of the survey along with completion rates and procedures for weighting the data. Volume III, Technical Reports 4 and 5 provide more detail on these topics.

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[27]Information on the number of each nonresident father's children should be interpreted with caution. It shows that over two-thirds of the legal fathers in our sample have only one child. Some, however, have other children with mothers not included in our sample. These children are not reflected here.

[28]It would be possible to analyze other outcomes from the perspective of children; for example, one could look at family income, counting each child once. Instead, we generally examine family-based outcomes from the perspective of the resident parent, counting each family once, instead of from the child's perspective, in which case we would count some families more times than others, based on the number of children.

Sample Design

The survey sample is a representative subset of the research population of resident mothers. A random subset of early W-2 entrants was first excluded from the survey population.[29] After this exclusion, we drew a probability sample of 3,000 cases. The sample was stratified by W-2 status ("transitioned from AFDC to W-2" and "new W-2") and by initial W-2 tier location (upper and lower). For each case, we randomly selected a focal child from among the children who were listed on the W-2 case at entry and who would be under age 18 on December 31, 1999. The designated focal child remained the same throughout the panel study.[30]

The legal fathers of the randomly selected focal child make up the survey sample of fathers.[31] Cases were excluded from the fathers' sample if paternity was not established by December 31, 1998, or if a "good cause" exemption from pursuing paternity or child support had been established or was pending against the father. These definitions generated an original sample of 2,028 fathers. At Time 2, we fielded samples of 2,950 mothers and 2,225 fathers. The mother and the father became ineligible if the focal child had died since Time 1 or when we identified errors or changes in the sample frame. Fathers became ineligible at Time 2 if a "good cause" exemption had been established since Time 1. Newly identified legal fathers for whom paternity was established between January 1, 1999, and December 31, 1999, were added at Time 2. If a father or mother had died since Time 1, the surviving parent remained eligible for a follow-up interview.[32]

Survey Content and Design

The content of the Survey of Wisconsin Works Families was guided by the objectives outlined in the evaluation plan, but with special emphasis on areas that were not represented in administrative databases or for which administrative records were incomplete. The Survey provides information on participants' experiences with and attitudes about W-2, their knowledge of W-2 rules and of child support policy, child well-being, and family relationships as well as employment, economic resources, and individual and household characteristics. Each sample member was asked about the demographic characteristics, employment, and earnings of the other parent to maximize the number of couples about which basic information was available even if both parents could not be interviewed. The Time 1 survey, which was fielded from February to July 1999, focused on events and experiences during 1998; the Time 2 survey was fielded from February to July 2000 and asked about events during 1999.

We conducted interviews by telephone and face to face, using computer-assisted instruments. Although both modes of data collection were employed simultaneously throughout the field periods, we attempted to interview as many respondents as possible by telephone.[33] We conducted telephone

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[29]This group was initially called the "nonexperimental" group. Because they received treatment identical to that of the experimental group, we include them with the experimental group in all administrative data analyses; the survey population, however, excludes these cases.

[30]We later identified five cases in which a different focal child was inadvertently selected and became the focus of the interview at Time 2. These cases are excluded from analysis.

[31]Detail of sample selection is included in Volume III, Technical Report 5.

[32]Survey sample design excludes cases in which father is the resident parent, but physical placement of the focal child may have changed since sample selection, or between Time 1 and Time 2. Analyses of survey data exclude mothers who were not the resident parent during the reference period, as well as fathers who were the resident parent.

[33]Telephone interviews were less expensive, expedited data processing, and could be more easily monitored and supervised to ensure data quality since they were conducted at a central facility. Nonetheless, we anticipated that a significant proportion of sample members could not be easily interviewed by telephone. Among a low-income population such as that represented by the survey sample, rates of households without telephones are higher,

interviews with persons regardless of their state or country of residence at the time of the survey.[34] However, efforts to interview respondents face to face were restricted to particular localities in Wisconsin because it was not cost efficient to pursue small numbers of cases in sparsely populated counties or in other states.[35]

All mothers in the sample were potentially eligible for in-person interviews, but only a random subsample of fathers was subject to this effort. Tracing efforts prior to the first wave of data collection indicated that fathers were much more difficult to locate than mothers. Location information in the administrative records (address, telephone number) was less often available for fathers and, when present, was more likely to be incorrect. Contact information gleaned from other sources more frequently yielded bad addresses and nonworking or nonexistent telephone numbers for fathers. Given the relatively large number of fathers who could not be reached by telephone and our goal of maximizing the response rate among a representative sample of fathers, we divided the fathers' sample into two groups. A random subsample of fathers (approximately one-third of the sample) was eligible for "full effort" and could be interviewed by telephone or in person; the remaining two-thirds could be interviewed only by telephone.

Response Rates and Weighting Procedures

The original survey sample was generally representative of the research population, as discussed in more detail in Technical Report 5 of Volume III (see especially Tables TR5.1–3). We completed interviews with 82 percent of mothers who were in the survey sample at Time 1 and 82 percent of mothers who were in the survey sample at Time 2. Among all fathers in the sample, we completed interviews with 33 percent at both Time 1 and Time 2. Completion rates for fathers in the random subsample eligible for telephone and in-person interviews were higher—43 and 46 percent at Time 1 and Time 2, respectively. Among pairs of eligible mothers and fathers, the survey data represent 30 and 29 percent at Time 1 and Time 2, respectively.[36]

Although the response rates for the fathers' surveys are lower than those generally reported by surveys of the general population, they compare favorably with other studies of separated families.[37]

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telephone service is interrupted more often, and residential mobility occurs more frequently. Therefore, we also deployed a staff of personal interviewers to assist in locating sample members and to conduct face-to-face interviews with persons for whom we could not obtain a telephone number or who could not be reached by telephone.

[34]By definition, mothers in the study, as participants in W-2, were residents of Wisconsin at some time during the period of the evaluation. The overwhelming majority were living in the state when interviewed, but some mothers had left the state and a slightly larger number of fathers who responded lived outside Wisconsin. At Time 1, less than 5 percent of mothers and 8 percent of fathers were living outside Wisconsin. Among nonrespondents it is likely that these percentages are higher.

[35]Specifically, we fielded personal interviewers in Wisconsin cities and metropolitan areas where there were at least 10 cases (mothers and fathers combined) that could not be reached by telephone. In practice, the application of this rule meant that in-person efforts were heavily concentrated in the central and southeastern corridors of the state, especially the Milwaukee metropolitan area (Milwaukee, Racine, and Kenosha counties), with another cluster of cases in and around Madison (Dane County). A few additional communities became eligible for in-person effort later in the field period after telephone contacts proved unsuccessful.

[36]Completion rates are computed as the number of completed interviews divided by the total number of eligible (in-scope) cases. Partial interviews are not included in the numerator and are not included in any data analysis in this report. The final number of in-scope cases was smaller than the original sample sizes because of errors or changes in the sample frame (mothers: 2,876 and 2,871 at T1 and T2, respectively; fathers: 1,926 and 2,123).

[37]The Child Development Supplement of the Panel Study of Income Dynamics completed interviews with 19.8 percent of the fathers who lived apart from the children in its sample. The Parents' Fair Share (PFS) Study achieved much higher response rates (ranging from 74 to 82 percent across different sites and intake periods) but the sample and study design differ significantly from the Survey of Wisconsin Works Families and contribute, in ways

Data from the survey are weighted to adjust for the stratification of the sample by W-2 status ("transitioned" and "new" cases) and by assignment to upper/lower W-2 tier. Weighting also adjusts for the differential rate of assignment to control and experimental status over the period during which the research population was developed (September 1, 1997, to July 8, 1998). The survey weights also include adjustments for nonresponse bias; respondents and nonrespondents differ systematically, such that analyses of respondents alone will produce biased parameter estimates. The high response rate among mothers presents less serious concern about nonresponse bias than exists for fathers, but the data underrepresent some subgroups of the mothers' population. The nonresponse analysis is discussed in more detail in Volume III, Technical Report 6. Weighting procedures are discussed in Volume III, Technical Reports 4 and 6.

I.3.3 Methods of Analysis

The random assignment of cases to an experimental and a control group provides a powerful tool to evaluate the effects of a policy. In theory, given random assignment, simple comparisons between the experimental and control groups should provide unbiased measures of the impact of the policy. This comparison is appropriate if the groups are comparable, differing only in the pass-through policy they face. The implementation analysis, summarized above in Section 2, found that the initial random assignment worked appropriately. The analysis of diversion—i.e. the tendency for cases that have been assigned not to actually enter the program—suggests there are no overall significant differences in the proportion of cases in the experimental and control groups that enter W-2 (and our research sample). As discussed in Volume III, Technical Report 1, an analysis of the

Regression Control Variables

For the regression analyses, the following standard ("basic") list of control variables was used (at minimum) in all regressions. All variables are defined at sample entry:

- Assignment rate
- Higher child support history
- Mother's age 31 or greater
- Mother is African-American

In addition to the basic list, the following control variables were used in some analyses (this is referred to as the "extended" list):

- Assignment rate
- Mother's child support history
- Mother's age
- Mother's race/ethnicity
- Mother's AFDC history
- Region
- Initial W-2 tier
- Child's age
- Mother's education
- Father's earning history
- Mother's employment history (not included in analyses of fathers' sample)
- Divorce or paternity case
- Number of legal fathers associated with mother
- Whether a child support order existed at entry
- Number of children

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that cannot easily be quantified, to their success (Abt Associates, 1997). For example, PFS focused its sample selection specifically on fathers—recruiting them after their participation in a court hearing about child support—and the program under evaluation involved services provided directly to fathers (e.g.,job search, skills training, peer support). These characteristics of the design and program intervention likely increased contact with fathers over the study period and made it easier to locate and interview sample members. In contrast, we sampled fathers indirectly through their attachment to a resident mother and focal child, and the pass-through policy itself does not directly benefit fathers.

initial characteristics of the experimental and control groups largely confirms our expectation that they are equivalent.

Although the experimental and control groups are not significantly different in most respects, we present regression-adjusted means, rather than simple means, in the analysis that follows. This procedure allows us to adjust for any observed differences in initial characteristics of the experimental and control groups that may exist. This approach has a number of advantages. First, even if random assignment worked perfectly, there will be some chance differences in the initial characteristics of the experimental and control groups. Regression-adjusted means adjust for chance variation in characteristics included in the regression. The regression-adjusted difference reflects the estimated effect of experimental status (i.e., the coefficient on the indicator for experimental or control status) after accounting for differences in baseline characteristics. This approach will also adjust for any nonrandom differential assignment based on observable characteristics included among the control variables. Finally, to the extent that control variables account for the variance in the outcome of interest, we are more likely to be able to discern the effect of the experiment.

The analyses of experimental effects in the next chapter generally use one of the two standard sets of control variables shown in the text box on page 29. The first set of control variables was generally used in the analysis of survey data because of the limited sample size. The more extensive list was used in most analyses of administrative data. The details of the procedure for estimating regression-adjusted means and differences are discussed in Volume III, Technical Report 1.

Chapter 4
Experimental Impacts

The full pass-through and disregard of child support will have a direct, mechanical, effect on the child support received by mothers. Given the scarce resources available to most families receiving cash payments, this effect, alone, may justify the policy change. However, both basic economic theory and ethnographic evidence suggest that a full pass-through may have additional effects. First, the policy change has a direct effect on the incentives for fathers to pay support, and for mothers to pursue support orders. Thus, if they respond to the policy change, fathers are expected to be more likely to pay child support, and to pay more support. Mothers are expected to be more motivated to establish paternity, and therefore to cooperate more fully with child support enforcement efforts in this regard.

The "direct" effect of the full pass-through is expected to have a set of secondary effects. Because these depend on behavioral responses to the changes in child support paid or received (which in turn is the expected response to the policy change), they are likely to be harder to detect, especially in the short term. We expect secondary effects on payment receipt, mothers' employment and earnings, fathers' employment and earnings, fathers' involvement, parental conflict, and child well-being. In some of these cases the potential impact of the policy change is fairly clear. For example, if fathers face fewer disincentives to pay formal child support, they are expected to work more in the formal sector and less in the informal sector. In other cases the effects are more ambiguous. For example, whereas greater child support received may increase a mother's ability to make investments that allow her to work, it is also possible that the increase in unearned income will reduce her need to work. In some cases we are unlikely to be able to measure secondary effects given the fairly modest direct effects and the short time period of the experiment. For example, if the full pass-through is to generate a measured effect on child well-being it must first either increase child support received or decrease parental conflict. The increased income or decreased conflict must then affect the context or content of child rearing, which must in turn be captured by our measures of child well-being.

Our discussion of the effects of the pass-through policy in this final report is organized within the following general categories:

- child support paid and received
- paternity establishment and new child support orders
- resident mothers' program participation
- resident mothers' employment and earnings
- resident mothers' total income and economic hardship
- government costs
- nonresident fathers' total income and economic hardship
- nonresident fathers' social and economic involvement with the child
- child well-being

In this section, we consider each of these sets of outcomes in turn. When they are sufficient, we use administrative data, which are available for the larger population. When we consider outcomes not adequately measured in the administrative data we rely on data from the Survey of Wisconsin Works Families. See the text box, "Notes on the Presentation of Data" (p. 32), for related details.

In each area we compare outcomes for the control and experimental groups, generally considering outcomes in 1998 and 1999. On the one hand, we expect larger effects in 1999 because implementation of the policy in 1998 was limited and because there may be some time lag between when individuals decide

to respond and when a response is measurable (it takes time to get paternity established, or an order changed, or to get a job in the formal employment sector). On the other hand, because the experimental and control groups are treated identically when mothers are not in the lower tiers of W-2, and because many mothers move out of the lower tiers of W-2 quickly, experimental effects in 1999 may be lower than they were in 1998.[38]

We measure effects over the research sample as a whole. We also show results for key subgroups: those who enter in the lower tiers (who, as recipients of cash assistance, are subject to the reduced pass-through if they are in the control group), those with no recent AFDC experience (who are less likely to have recent experience with the child support system under the previous policy, and who therefore may be more responsive to the reform), those with a child support order at entry, and those with a higher child support payment history. The text box on "Subgroups" (p. 33) provides further detail on the definitions of these subgroups. Results for the full sample and the key subgroups are summarized in tables for each outcome.

Our discussion in this section is limited to estimates of the effects of the experiment. More general discussions of the outcomes, and their implications beyond the CSDE, appear in Volume II, which includes a separate chapter for each of the main areas of discussion.

Notes on the Presentation of Data

Results are rounded. Dollar values are rounded to zero decimal places (except for wage rates, which are rounded to two decimal places), percentages to one place, and probability values to three places. Because of rounding, the difference between the experimental and control group means may not exactly equal the impact shown on the tables.

The probability values shown in the fifth and last columns indicate the probability that each reported impact might have occurred by chance if no difference existed between the two groups. The smaller the probability value, the more confidence can be placed in a conclusion that the difference was an effect of the experiment. Probability values of .05 or less are indicated in bold type in the tables. All tables show regression-adjusted values for outcome variables, which are generally not identical to the simple, unadjusted values found in the sample.

[38]Because the Survey of Wisconsin Works Families includes only resident mothers (and no resident fathers), mothers who have no resident children—either because they have lost custody of all their children or because all their children have died—are not included in the survey.

Subgroups

All outcomes were assessed for the four key subgroups described below. For some outcomes, other subgroups were also examined, as described in the text and tables.

Mother Entered in Lower Tier
Mother's first W-2 slot was either W-2 Transitions or Community Service Jobs.

No Recent AFDC History
During the 24-month period October 1995 through September 1997, mother did not receive AFDC.

Order at Entry
For mothers' sample: mother had at least one child support order in place at sample entry.
For fathers' sample: couple had a child support order in place at mother's sample entry.

Higher Child Support History
For mothers' sample: over the one-year period October 1996 through September 1997, mother had $1,000 or more in child support paid on her behalf. If there was more than one child support order for a case, payments were summed over all orders.

For fathers' sample: over the one-year period October 1996 through September 1997, father paid $1,000 or more in child support on behalf of the mother.

I.4.1 The Experimental Impact on Child Support Paid and Received[39]

Child Support Payments

A key outcome of interest is child support payments. Previous research has used a model in which the amount of child support paid was related to several factors, including nonresident parents' ability to pay support, their willingness to pay support, and the policy environment (e.g., Beller and Graham, 1993; Meyer and Bartfeld, 1996). This model predicts that nonresident fathers facing a policy in which their payments would not fully benefit their children would be less willing (and therefore less likely) to pay support. Ethnographic research (e.g., Waller and Plotnick, 2001) has suggested that fathers of children receiving welfare realize that any child support paid through the formal system would not fully benefit their children, and therefore they are reluctant to cooperate with this policy. Thus, one of the most critical tests of the full pass-through is whether fathers change their payment patterns when their children are able to keep the full amount paid.

We expect that fathers of children in the experimental group who want to support their children will pay more formal child support than fathers in the control group. This could occur because fathers will be more likely to pay formal child support or because fathers who pay will be more likely to pay higher amounts. To the extent that nonresident fathers are ordered to pay support through immediate income withholding when they work in the formal employment sector, they have limited discretion in how much support to pay. Thus, the primary avenue available to a parent who seeks to avoid support payments is to work "under the table." This suggests that the experiment would be more likely to affect whether support is paid than the amount paid.[40]

The main tables for this section show the effects on the 14,343 men who were legal fathers (those for whom paternity had been established or who had marital children) when their children entered W-2. We use administrative data on child support payments from the Kids Information Data System (KIDS). We examine two measures of fathers' formal child support payments: whether a father paid any support and the overall average amount of support paid. We show results for the standard subgroups described above; in addition, we show separate results for fathers who are divorced and for those in which the couple was not married, because prior research with Wisconsin data has shown substantial differences in child support between paternity and divorced fathers (Meyer and Bartfeld, 1998). All comparisons between the experimental and control groups control for differences in the characteristics of sample members at W-2 entry through multivariate analyses.[41]

Did Fathers Pay Support?

The first panel of Table I.4.1 shows that 52.0 percent of the fathers of children in the experimental group paid something in 1998; this percentage rose to 56.3 percent in 1999. These figures

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[39]This section is based on the work of Judi Bartfeld and Daniel R. Meyer, who thank Steven Cook for exceptional research assistance. Further information on child support payments and receipts can be found in Volume II, Chapter 2. A preliminary version of this section was presented at the CSDE National Advisory Board meeting, November 2000. The authors thank participants and especially our discussants, Andrea Beller and Vivian Gadsden, for helpful comments.

[40]The impact of the experiment on informal employment is discussed below, in Section I.4.7.

[41]Analyses of dichotomous outcomes (whether child support was paid, whether it was received) use probit analyses; analyses of continuous outcomes (average amount of child support paid, average amount received) use ordinary least squares regressions. We control for the extended list of control variables. See the box on p. 29 for the extended list of control variables.

Table I.4.1
Effects on Child Support Paid

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Percentage of Nonresident Fathers Paying Child Support										
All Nonresident Fathers	14,343	52.0%	50.0%	2.0%	0.083	14,343	56.3%	53.2	3.1%	**0.005**
Mother Entered in Lower Tier	8,767	48.4	46.8	1.7	0.249	8,767	53.4	50.7	2.7	0.057
Mother Has No Recent AFDC History	850	58.4	48.2	10.3	**0.022**	850	61.3	56.2	5.1	0.229
Couple Has Order at Entry	10,569	63.2	60.0	3.2	**0.012**	10,569	66.4	62.6	3.8	**0.001**
Higher Child Support History	2,694	94.3	92.4	1.9	0.111	2,694	91.0	86.2	4.9	**0.002**
Couple Is Divorced	2,359	55.3	52.2	3.1	0.293	2,359	58.8	53.3	5.5	**0.044**
Couple Was Not Married	11,941	51.5	49.8	1.8	0.155	11,941	55.8	53.3	2.5	**0.034**
(2) Average Annual Amount of Child Support Paid among All Nonresident Fathers										
All Nonresident Fathers	14,343	$798	$770	$28	0.228	14,343	$946	$891	$54	0.055
Mother Entered in Lower Tier	8,767	750	709	41	0.164	8,767	896	822	74	**0.038**
Mother Has No Recent AFDC History	850	1,273	969	305	**0.011**	850	1,580	1,348	232	0.133
Couple Has Order at Entry	10,569	971	937	34	0.238	10,569	1,109	1,032	77	**0.024**
Higher Child Support History	2,694	2,323	2,203	120	0.146	2,694	2,381	2,173	208	**0.030**
Couple Is Divorced	2,359	1,251	1,004	247	**0.001**	2,359	1,487	1,298	190	0.054
Couple Was Not Married	11,941	707	726	-19	0.409	11,941	835	813	22	0.422

Notes: All means are regression-adjusted, using the extended list of control variables. Probability values of 0.05 or less are shown in bold type. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33.

cannot be benchmarked against national data because no information exists on child support payments among the fathers of children receiving welfare.[42]

Consistent with our hypothesis, fathers of children in the experimental group were more likely to make payments in both 1998 and 1999 than fathers of children in the control group. The effects are fairly small and statistically significant only at the .08 level in 1998. In 1999, the effects are statistically significant ($p < .01$), and are larger. In 1999, 53.2 percent of fathers of children in the control group made payments, compared to 56.3 percent of fathers of children in the experimental group, a difference of 3.1 percentage points, or 6.0 percent. The larger effects in 1999 are consistent with the possibility that it takes time for fathers to understand the new policy and, once fathers understand, that it may take time for them to change their payment behavior.

The next rows in Table I.4.1 examine whether this effect is limited to certain key subgroups, as described earlier. In both years, the reform induced a statistically significant increase in the likelihood of payment among those who had a child support order when their partners entered W-2. This is perhaps not surprising: those who had an order when their partners entered W-2 could begin paying formal support fairly quickly; those without an order would generally not begin paying until an order was in place. (Section I.4.2 discusses the experimental effect on whether fathers were ordered to pay support.)

The largest impacts occurred among those whose partners did not have recent AFDC experience. Among these fathers, 48.2 percent of those in the control group paid something in 1998, compared to 58.4 percent of those in the experimental group. This is a difference of 10.2 percentage points, or 21.1 percent. Although fathers in the experimental group again appear to be more likely to pay in 1999, the effects were not statistically significant, partly because of the relatively small sample size. Among those whose partners entered the lower tiers and who had no recent AFDC history (not shown), the effects were even larger: 14.2 percentage points in 1998 and 7.1 percentage points in 1999 (not statistically significant). The reform significantly affected both paternity fathers and divorced fathers in 1999.

On Average, How Much Did Fathers Pay?

Considering all fathers—that is, those who paid and those who did not—the average amount paid in 1998 by fathers in the experimental group was $798; this increased to $946 in 1999, as shown in the second panel of Table I.4.1.

Overall, fathers of children in the experimental group paid higher amounts than fathers in the control group in 1999. The difference is $28 in 1998 and is not statistically significant, but in 1999 the difference increases to $54, a difference of 6.2 percent, and it is statistically significant at a level of .055.

The impact is larger among those whose partners entered the lower tiers, is large among those without recent AFDC history (the difference is $305 in 1998 and $232, which is not statistically significant, in 1999). Because experimental or control status results in a different amount of child support received only when resident parents are in lower tiers, the concentration of the effect among fathers whose partners entered in lower tiers is not surprising. The large impact found among those whose partners had no recent AFDC history is, however, potentially quite important: if behavioral effects are much larger among fathers who have not been exposed to the old policy, the long-run impact of this

[42]Accurate data on all nonresident fathers are quite difficult to obtain (see Garfinkel et al., 1998, or Sorensen, 1997, for a discussion of some of the difficulties involved), and data on the fathers of children receiving welfare even more difficult. Sorensen (1997) estimates that 46–54 percent of *all* nonresident fathers (not just the fathers of children receiving welfare) paid support in the late 1980s. Presumably fathers of children receiving welfare are less likely to pay; on the other hand, as the child support enforcement system has become more stringent, fathers may be more likely to pay now than they were in the late 1980s. National administrative data on child support for resident parents receiving welfare do exist and are reviewed below, but because a single resident parent may be linked with zero, one, or more than one legal nonresident parents, it is difficult to estimate payment patterns for nonresident parents from these data.

reform may be larger than seen here. The difference in amount paid was particularly large among those whose partners entered the lower tiers and had no recent AFDC history (not shown). Among this group, the impact was $440 in 1998 and $411 in 1999, differences of 56 and 35 percent, respectively. There were also significant effects in both years among fathers who had not yet accumulated a large debt to the state, also suggesting potentially larger long-run effects of this reform (not shown on table).

Our findings concerning overall payments are somewhat similar to our findings on the likelihood of paying: the reform had a larger effect among fathers already in the child support system. In 1999, the impact on payments was $77 among those with a child support order at entry and even larger for those who paid substantial support before their children entered W-2: an increase of $208 in 1999. There was a significant impact on divorced fathers in both years.

When Fathers Paid, How Much Did They Pay?

Our next analyses focus on differences in the amount paid among fathers who paid support. The amount fathers pay is important because previous policy limited the pass-through to $50/month; if few payments substantially exceed $50/month, then a full pass-through policy will be little different from the earlier pass-through policy. Although some fathers did pay fairly small amounts, the average amount paid (when something was paid) was substantially above $50/month ($600/year): those fathers who paid child support on average paid about $1,500 in 1998 and about $1,700 in 1999 (not shown).

Sensitivity Testing for Effects on Legal Nonresident Fathers

Our base results show that fathers whose partners were in the full pass-through group were more likely to pay support in both years and on average paid a higher amount in 1999. We conducted several sensitivity tests to determine the robustness of our results. First, we examined the extent to which fathers paid the full amount of their child support order, to see if this analysis yields similar findings to the analysis of the *amount* fathers paid. We defined "full" payment as paying at least 90 percent of what was due. (This outcome is measured only among fathers with fixed-dollar child support orders.)[43] Only about 20 percent of fathers with fixed orders paid the full amount due in 1998; the percentage rose to about 25 percent in 1999. There is no statistically significant impact on the likelihood of full payment in this sample.

Our second sensitivity test changed the timing used to examine impacts by looking at timing relative to the children's W-2 entry rather than looking at calendar years. Figure I. 4.1 shows the likelihood that fathers paid support, based on the time since W-2 entry. There was little difference in the early quarters, which is consistent with the calendar-year findings. A significant effect first appeared in the fourth quarter after entry. These findings generally confirm our base results.[44]

Our third test examined the subset of cases in which fathers seemed to understand the policy they faced. In the first two panels of Appendix I.4.1 we compare administrative data on child support

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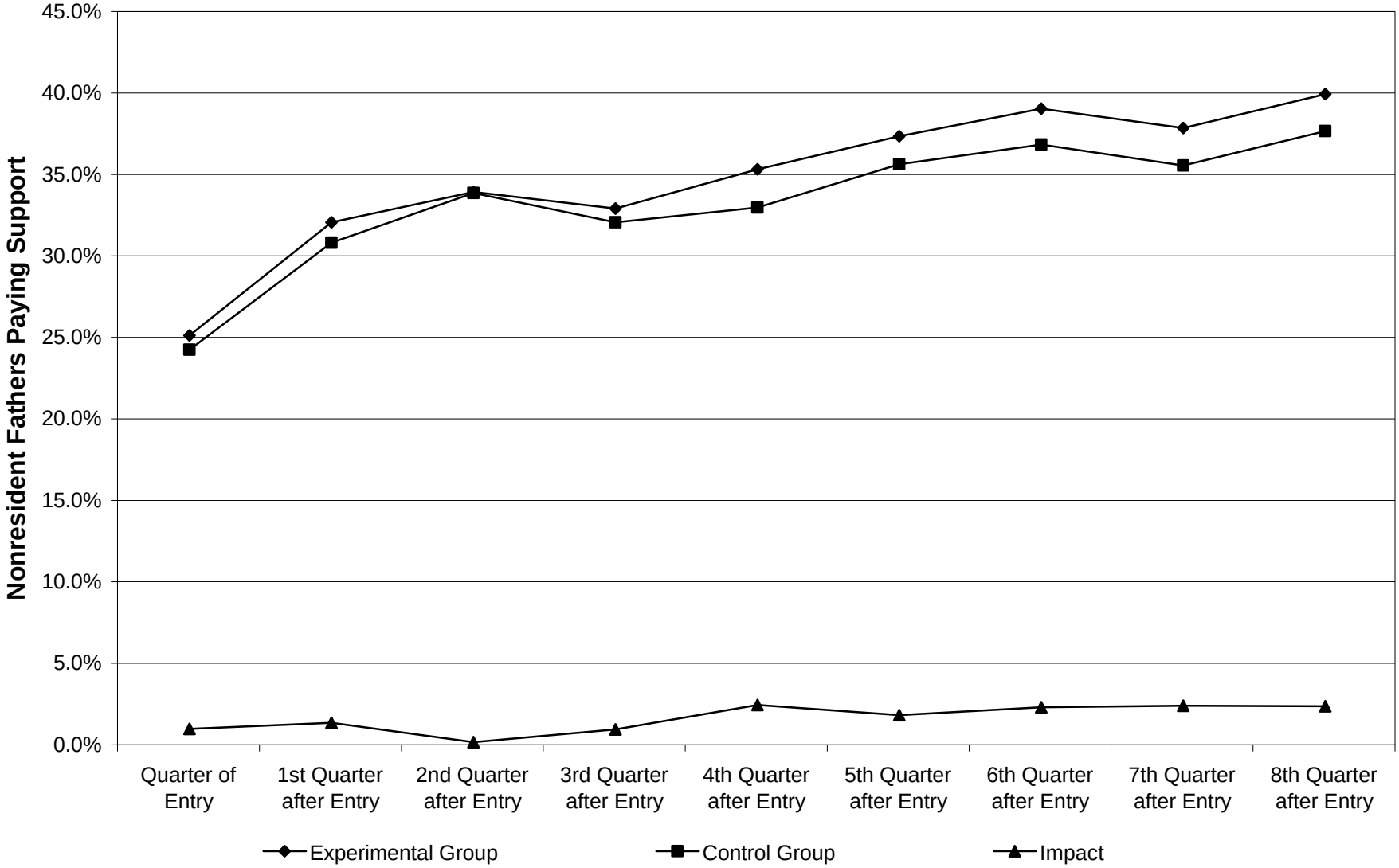
[43]There were 14,343 legal fathers at the time that children entered W-2. In 1998, 9,392 of these owed child support. Of this group, 1,197 owed amounts that varied based on the fathers' income, either percentage-expressed orders (e.g., "17 percent of the father's gross income") or mixed orders (e.g., "17 percent of the father's gross income or $200 per month, whichever is greater"). Eliminating these types of orders from our analysis left us with 8,195 fathers for the 1998 analysis. In 1999, 9,399 fathers owed support; of these, 1,239 owed amounts that varied based on their income, thereby leaving 8,160 fathers for the 1999 analysis.

[44]The impact on the likelihood that fathers will pay support is 1.0 percentage point (p=.37) in the first year, compared to 2.0 percentage points (p=.08) in 1998. The impact on the average amount paid is also smaller in the first year than in 1998, though neither is statistically significant. The effect in the second year is very similar to the effect in 1999 on the likelihood of payment: 3.0 percentage points (p=.007) in the second year, compared to 3.1 percentage points (p=.005) in 1999. The effect on the amount paid is also quite similar: $47(p=.09) in the second year, compared to $54 (p=.06) in 1999.

Figure I.4.1
Experimental Impact on Child Support Payments



outcomes for the administrative-data sample to administrative data on child support outcomes for survey respondents. We then look specifically at those survey respondents who correctly answered two questions about child support pass-through policy.[45] In general, the effects in the survey sample are similar to those in the administrative-data sample, but larger.[46] Only about one-tenth of the fathers answered the questions about policy correctly, meaning that only 65 respondents in the first wave and 69 in the second correctly understood the policy in which they were participating. Among these two small groups of fathers, there was a very large apparent difference in the amounts paid by the experimental and control groups, but in neither group is the difference statistically significant.

Our fourth test examined the experimental impact on payments in 1999 among *new* fathers, that is, men who had paternity established after the mother and children entered W-2 but before January 1, 1999. (Our base results for nonresident fathers examined only those who were legal fathers when their children entered W-2.) The likelihood of paying support was higher among the 1,817 new fathers than in the base sample, around 63 percent compared to 53–56 percent in the base sample. The overall mean amount paid was also somewhat larger, around $1,100 for the new fathers, compared to $891–$946 in the base sample. There is no statistically significant difference between the experimental and control groups of new fathers in either the likelihood of payment or the average amount paid. Perhaps the processes of establishing paternity, getting an order, and beginning to pay support take enough time that it would difficult to see an effect within the years examined here; longer-term follow-up may be revealing.

Summary

In sum, we find a higher likelihood of paying and higher child support payments among fathers in the experimental group in 1999. The effects are small in the overall sample, but larger in some subgroups, particularly fathers of those children without a recent history of AFDC receipt.

Child Support Receipts

Our next set of analyses focuses on differences in the amount of support received by mothers in the experimental and control groups. By definition, a full pass-through should result in higher child support receipts for the experimental group, since those in the control group will have a portion of the payments retained by the government when they are in the lower tiers of W-2. Thus, as a mechanical effect of the experiment, as long as mothers spend some time in the lower tiers and as long as the child support paid on a mother's behalf is at least $50/month, mothers in the experimental group should receive more child support than mothers in the control group. This effect should be largest among mothers for whom the most is paid, since in this group the differences between the experimental and control groups will be magnified. A finding that the experimental group received higher amounts of child support could therefore merely indicate that the experiment was administered properly, rather than revealing a behavioral effect.

The experimental impact is not, however, necessarily limited to this mechanical impact. On the one hand, to the extent that fathers in the experimental group are more likely to pay, and to pay more

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[45]The first question was: "Now, think about what happens if <focal child's> mother participates in W-2. In this situation, does she receive all of the current child support you or your employer pays, does the state keep some of the child support, or does the state keep all of the child support." The correct answer depends on experimental group status: for experimental-group fathers the correct answer is "She would receive all;" for control-group fathers the correct answer is "the state would keep some." The second question is "Next think about what happens if <focal child's> mother does not participate in W-2. In this situation, does she receive all of the current child support you or your employer pays, does the state keep some of the child support, or does the state keep all of the child support?" The correct answer for both groups is "She would receive all."

[46]Note also that the level of fathers paying support is substantially higher among the survey sample; this is because the fathers responding to our survey were those more likely to pay support and our weights do not completely correct for this lack of representativeness.

when they do pay, the effect on receipts would be even greater. On the other hand, during periods in which mothers are in higher tiers of W-2 or off W-2 altogether, the experimental and control groups are treated identically. Thus as time passes and more mothers leave the lower tiers of W-2, any experimental–control difference in receipts may shrink.

For the analyses of child support receipts, we examine the 15,977 mothers who entered W-2 during the demonstration and who were potentially eligible for child support. The analysis of receipts among mothers is related to the earlier analyses of payments among fathers, but differs in several ways:

- It reflects both the behavioral impact on fathers as well as the mechanical impact stemming from the nature of the pass-through policy.
- The amount paid does not necessarily equal the amount received, even under Wisconsin's reformed child support policy. For example, if a nonresident father pays more than the full amount currently due and an amount is owed to the state for past welfare payments or for hospital expenses associated with the birth, some of the amount paid will be kept by the state rather than received by the resident mother. In addition, if a nonresident father pays interest on state-owed arrears or other fees, this amount is not passed through to the mother. Finally, there are different rules for the distribution of support collected through interception of federal income tax refunds: amounts collected go first to the government, even among the experimental group.[47]
- About one-quarter of the mothers who enter W-2 are associated with more than one father, and a few with five or more. Our payment analyses treat the father in each couple as a separate unit of analysis, whereas the receipt analyses treat mothers as the unit of analysis, showing the total amount received from all associated fathers.[48]
- Nonmarital children may or may not have paternity established. The analysis of resident mothers includes some cases in which the father(s) had not been legally established, while the analysis of payments is limited to legal nonresident fathers.

Did Mothers Receive Support?

The first panel of Table I.4.2 shows that 38.0 percent of experimental-group mothers received support in 1998, and this percentage increased to 47.8 percent in 1999. These figures are substantially higher than the national figures, in which child support was collected for 24.0 percent of TANF cases in 1999 (U.S. Department of Health and Human Services, Administration for Children and Families, 2000); they reflect Wisconsin's continued effectiveness in collecting support among difficult cases.

The table shows that mothers in the experimental group were significantly more likely to receive child support than mothers in the control group.[49] This difference in the likelihood of receiving support is fairly small (3.1 percentage points in 1998 and 2.7 percentage points in 1999), but it is statistically significant and is related to differences in behavior, not only to the mechanical effect of the reform. The next rows show a large impact in 1998 among mothers with no recent AFDC history. There are statistically significant effects in several of the subgroups, including those with a child support order at entry and those who had a history of higher child support amounts (1999 only). There are particularly large effects in 1999 among those with marital children.

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[47]See Section I.4.7, which shows the mean amount retained for those in the experimental and control group.

[48]See Figure I.3.1 for an illustration of the relationship between the samples of mothers and legal fathers.

[49]There is no statistically significant difference in the simple comparison between the experimental and control group. Our analyses suggest that this is because the simple comparison includes a substantial portion of women who have not yet had paternity established, and thus do not receive child support. Once a regression model controls for whether a mother has paternity established, a statistically significant difference between the experimental and control groups can be seen.

Table I.4.2
Effects on Child Support Received

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Percentage of Resident Mothers Receiving Child Support										
All Resident Mothers	15,977	38.0%	35.0%	3.1%	**0.006**	15,977	47.8%	45.1%	2.7%	**0.014**
Entered in Lower Tier	9,634	33.4	31.2	2.2	0.117	9,634	43.9	41.2	2.7	0.057
No Recent AFDC History	2,005	27.6	19.9	7.7	**0.002**	2,005	40.2	36.0	4.3	0.123
Has Order at Entry	8,924	66.5	63.9	2.6	0.056	8,924	71.8	68.0	3.8	**0.003**
Higher Child Support History	2,744	92.0	90.5	1.4	0.305	2,744	90.5	87.0	3.5	**0.023**
Has Only Marital Children	1,183	55.6	48.9	6.6	0.128	1,183	61.9	50.3	11.6	**0.004**
Has Only Nonmarital Children	13,518	34.3	31.5	2.8	**0.016**	13,518	44.8	42.7	2.1	0.082
(2) Average Annual Amount of Child Support Received among All Resident Mothers										
All Resident Mothers	15,977	$641	$499	$142	**0.000**	15,977	$848	$725	$123	**0.000**
Entered in Lower Tier	9,634	588	390	197	**0.000**	9,634	759	604	155	**0.000**
No Recent AFDC History	2,005	642	448	194	**0.000**	2,005	992	841	151	**0.049**
Has Order at Entry	8,924	1,004	799	205	**0.000**	8,924	1,222	1,054	168	**0.000**
Higher Child Support History	2,744	2,220	1,738	482	**0.000**	2,744	2,402	2,102	300	**0.002**
Has Only Marital Children	1,183	1,361	822	540	**0.000**	1,183	1,684	1,265	419	**0.003**
Has Only Nonmarital Children	13,518	510	415	95	**0.000**	13,518	702	593	108	**0.000**

Notes: All means are regression-adjusted, using the extended list of control variables. Probability values of 0.05 or less are shown in bold type. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33.

On Average, How Much Did Mothers Receive?

The second panel of Table I.4.2 shows the mean amount of child support among all mothers. Mothers in the experimental group received an average of $641 in 1998, increasing to $848 in 1999. These figures are roughly comparable to the national average of *collections* among welfare recipients of $684 (U.S. Department of Health and Human Services, 2000). In all states except Wisconsin, however, the amounts collected for welfare recipients are not fully *received* by the families themselves.

The table shows that mothers in the experimental group received more in support in 1998 and 1999. The effect, though fairly small, is statistically significant: $142 in 1998 and $123 in 1999. Although small in dollar terms, it is equivalent to an increase of 28 percent (1998) and 17 percent (1999) in the average support received by mothers in the control group.

This impact reflects at least in part the mechanical effect of the pass-through policy, so it is not surprising that the effect can be seen in every subgroup in both years. That is, there are significant impacts among those who enter a lower tier, those who have no recent AFDC history, those with orders at entry, and those with a history of higher child support. Particularly large effects are found among those with only marital children—differences of $540 in 1998 and $419 in 1999, or 66 percent and 33 percent. There are also large effects for those without recent AFDC history who entered a lower tier—differences of $333 in 1998 and $300 in 1999, or 90 percent and 43 percent (not shown on table).

When Mothers Received Support, How Much Did They Receive?

The amount of child support received is important, because if few payments substantially exceed $50, then a full pass-through will be little different from the earlier pass-through policy (though it would differ in the timing of receipts, because a full pass-through enables resident parents to receive support paid on their behalf more quickly). The data on receipts show that the average amount received (if anything was received) is substantially larger than $50/month: mothers in the experimental group who received child support received an average of $1,583 in 1998 and $1,771 in 1999 (not shown). Mothers in the experimental group received higher amounts of support when something was received than did mothers in the control group. The difference in 1998 was $297, and in 1999 was $191. The amount was smaller in the second year because fewer mothers were still in lower tiers of W-2, and thus fewer mothers in the control group had child support retained.[50]

Sensitivity Testing and Additional Analyses

Our base results show that mothers who were in the full pass-through group were more likely to receive support in both years and on average received a higher amount in both years. We conducted several sensitivity tests to determine the robustness of our results.

Our first test involves changing the time period used to examine impacts by looking at timing relative to W-2 entry rather than looking at calendar years. Figure I.4.2 shows little difference in the probability of receiving support in any one quarter, particularly early in the period; only when longer periods are examined do we see statistically significant effects. In contrast, Figure I.4.3 shows sizable

[50]This difference, which comes from a regression-adjusted estimate, is not a true experimental effect because it is conditional upon the likelihood of receiving something, and we have shown that the reform does affect the likelihood of receiving something.

Figure I.4.2
Experimental Impact on Child Support Receipts

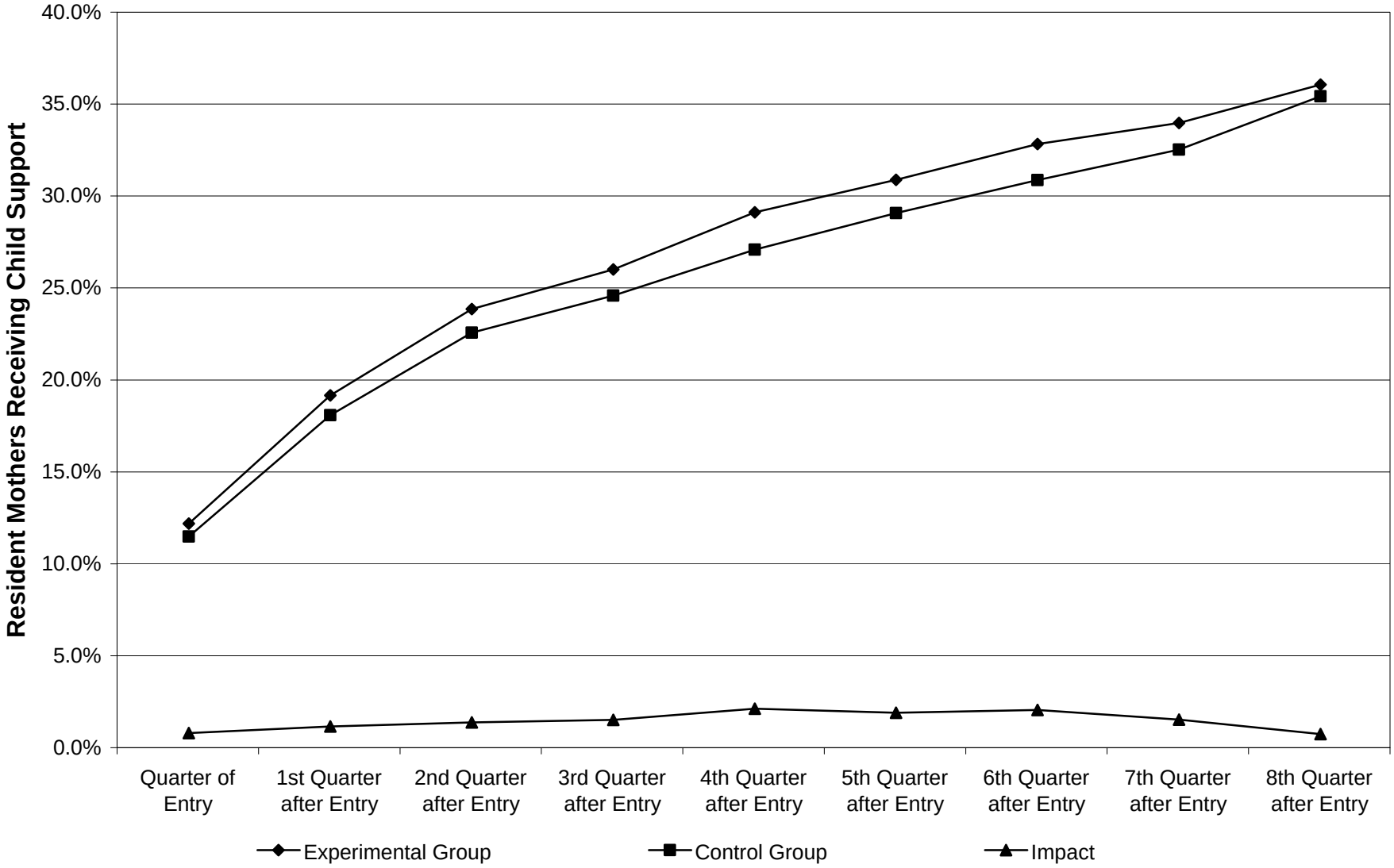
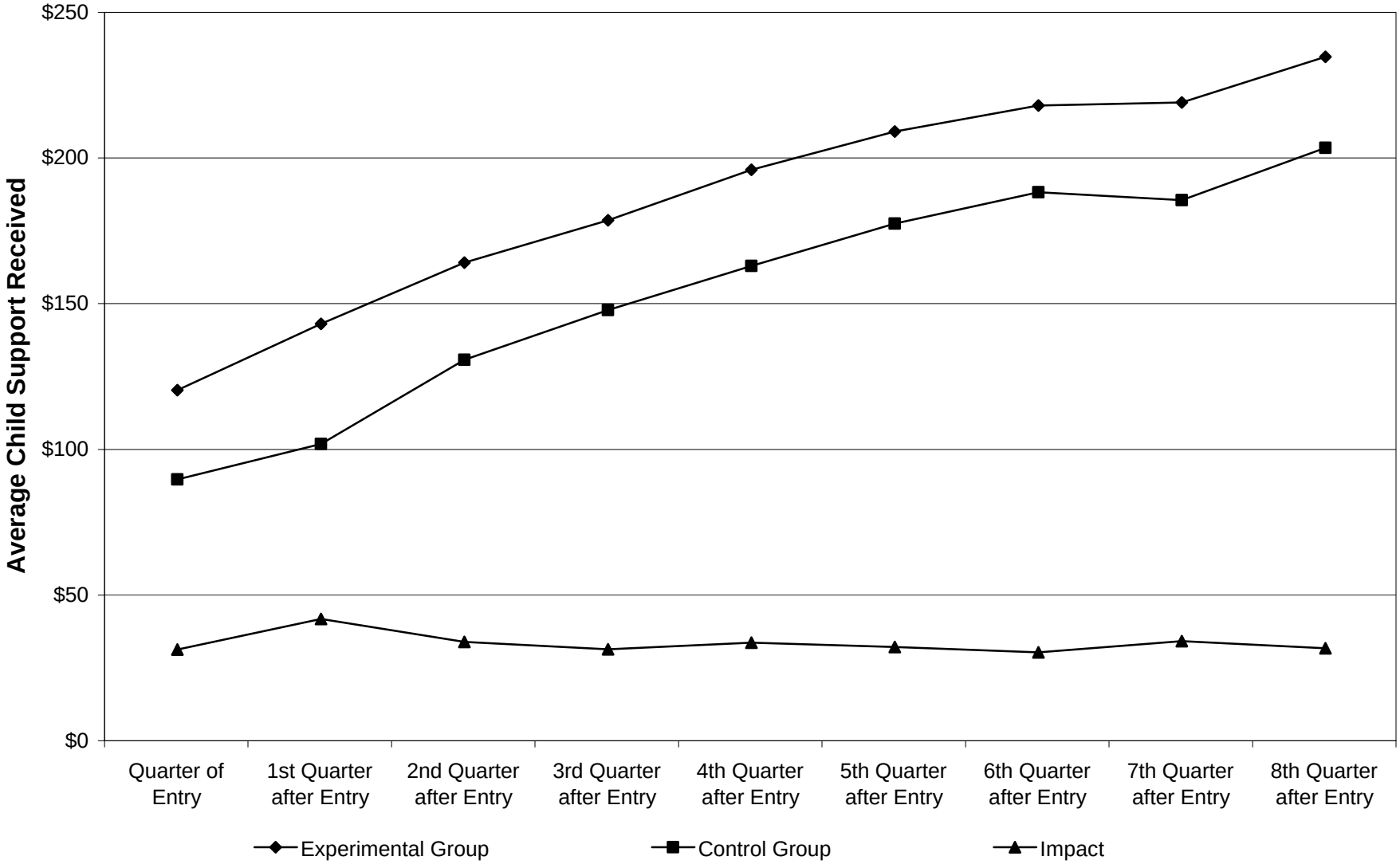


Figure I.4.3
Experimental Impact on Average Amount of Child Support Received



differences in the amount received from the first quarter on.[51] We view these tests as generally supporting our base results.

Our second test examines whether experimental impacts are larger among the subset of cases in which mothers understood the policy. The analysis follows the approach used for fathers, with full results in the third and fourth panels of Appendix I.4.1. We again look specifically at survey respondents who correctly answered two questions about child support pass-through policy.[52] Considering the summary variable for mothers (average amount received), there is no statistically significant effect among all survey mothers, but among those who knew the rules, the effect is $207 in 1998 (p=.02), and $191 in 1999 (not statistically significant).[53] Thus we find some evidence of larger experimental effects among those who understood the policy.

Our third test examines whether there is an effect on child support receipts among the unusual cases in which children live with their fathers; our base results examined the more typical case when they live with their mother. There are no significant impacts in child support received among the 380 cases in which children were living with resident fathers.[54]

Summary of Effects on Payments and Receipts

In summary, the main policy change in this experiment is that mothers will get the full amount of child support paid on their behalf; thus a full pass-through should mean that mothers in the experimental group receive more child support than those in the control group. Our results confirm this hypothesis: on average, experimental-group mothers received $142 more in 1998 and $123 more in 1999 than control-group mothers. Although this amount may seem relatively small, it represents increases of 28 percent and 17 percent. Among those who received any amount, mothers in the experimental group received $297 more in 1998 and $191 more in 1999. Because mothers not in the lower tiers of W-2 received the full amount of child support paid whether they were in the experimental or control group, and because most mothers were no longer in the lower tiers in 1999, the experimental effect was smaller in 1999 even though the amount received by the experimental group was larger in 1999 than 1998.

Because an increase in receipts is somewhat mechanical, a more important result is that fathers whose children received the full amount paid were more likely to pay, and, in general, pay higher amounts. The effect, though statistically significant, is less than 10 percent. Although the effect may seem relatively small, we believe it to be quite meaningful. As discussed in Volume I, Chapter 2, the experiment was implemented inconsistently, which likely dampened the reform's effect. The largest

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[51]The regression-adjusted estimate of the impact on the likelihood of receipt in the first year is 1.4 percentage points (p=.20), compared to 3.1 percentage points (p=.01) in 1998. In the second year, the impact is 2.9 percentage points (p=.01), compared to 2.7 percentage points (p=.01) in 1999. The impact on the average amount received in the first year is very close to the impact in 1998 ($136 compared to $142), and the impact in the second year is also very close to the impact in 1999 ($128 compared to $123).

[52]These questions are comparable to the questions for fathers. For example, the first question to mothers was: "If you were in a W-2 assignment where you received a check from W-2, would you receive all of the current child support <child's name>'s father paid or would the state keep some?"

[53]Although these results show that the experimental impact is concentrated among cases with greater policy knowledge, the direction of this relationship is uncertain, especially among mothers. It is possible that greater policy knowledge is a result—not a cause—of more favorable payment and receipt outcomes. That is, those in the experimental group who in fact receive support may be more likely to understand the policy than those who do not receive support.

[54]Levels of support received were much lower for resident fathers than resident mothers. Only about 12 percent of fathers received support, compared to 40–50 percent of mothers. When fathers did receive support, amounts were $900–$1,200 in 1998 and about $1,400 in 1999, compared to mothers' amounts of $1,300–$1,600 in 1998 and $1,600–$1,800 in 1999.

effects are found among cases that did not have recent welfare experience, which suggests that if this were to become policy, the eventual effects would be even larger. Finally, the experiment only shows the difference between a full pass-through policy and a generous partial pass-through policy. A full pass-through may have substantially larger effects when compared to no pass-through, the policy in most states.

I.4.2 Experimental Impacts on Paternity Establishment and Child Support Orders[55]

The W-2 child support reform increases the potential benefit of child support to resident mothers and children participating in W-2 by allowing these parents to combine child support with money from W-2 payments. Because of this additional benefit, we expect that fathers in the experimental group who want to support their children would be more likely to pay support than would fathers in the control group. We also expect that both mothers and fathers in the experimental group would be more likely to cooperate with the child support system than would parents in the control group. This should result in higher rates of paternity establishment, higher rates of support orders, and more frequent revision of orders to reflect changes in circumstances. We examine differences between the experimental and control groups in:

- the establishment of legal paternity,
- new child support orders for legal fathers at entry and those who became legal fathers during 1998, and
- changes in existing child support orders.

We examine experimental/control differences for the entire sample and for the key subgroups (lower-tier entries, those without recent AFDC experience, those with an order at entry, and those with a history of higher child support) in Table I.4.3. These comparisons are regression-adjusted, using variables that are listed for each analysis.

Effects on Paternity Establishment

One hope underlying the design of the experiment was that those who participated in the experiment would be more active in pursuing the establishment of paternity for their children than those who were in the control group. In the first panel of Table I.4.3, we examine 15,568 nonmarital children who lacked paternity establishment when they entered W-2. (About half of nonmarital children entering W-2 had had paternity established; the other half, who had not, are examined here.)[56] As the first panel of Table I.4.3 shows, just under 15 percent of those without paternity at entry had paternity established by the end of 1998 and roughly 25 percent had done so by the end of 1999. The differences in new paternity establishment between the experimental and control groups were in the expected direction, but were not significant.

In the first panel of Table I.4.3, it is noteworthy that in 1998 there was a significant difference in paternity establishment among the children of mothers entering W-2 with no recent AFDC experience (19.7 percent of the experimental group versus 14.5 percent of the control group). In 1999, however, there was no significant difference. This is one group among which one would expect an experimental effect

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[55]This section is based on the work of Judi Bartfeld and Gary Sandefur, who thank Hyunjoon Park for excellent research assistance and Steven Cook for assistance with programming. A preliminary version of this section was presented at the CSDE National Advisory Board meeting, November 2000. The authors thank participants and especially our discussants, Margo Melli and Wendell Primus, for helpful comments. Further information on paternity establishment and child support orders can be found in Volume II.

[56]The values are regression-adjusted, with AFDC history, residential location, and the child's age included as control variables in addition to the basic list of control variables; see the box on p. 29.

Table I.4.3
Effects on Paternity and Child Support Orders

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Paternity Established among Nonmarital Children without a Legal Father at W-2 Entry										
All Nonmarital Children	15,568	14.2%	12.9%	1.3%	0.056	15,568	24.7%	24.9%	-0.2%	0.840
Mother Entered in Lower Tier	9,649	11.8	10.9	1.0	0.227	9,649	21.3	20.9	0.5	0.650
No Recent AFDC History	1,794	19.7	14.5	5.1	**0.013**	1,794	32.9	29.3	3.7	0.165
Mother Has Order at Entry	6,135	16.5	13.6	2.8	**0.015**	6,135	29.1	27.0	2.1	0.151
Higher Child Support History	1,605	16.2	11.1	5.1	**0.027**	1,605	29.4	22.5	6.9	**0.021**
(2) Paternity Established among Mothers Who Had Not Had Paternity Established at W-2 Entry										
All Resident Mothers	4,798	21.2%	20.3%	0.9%	0.533	4,798	34.7%	35.4%	-0.6%	0.709
Entered in Lower Tier	2,785	16.6	16.2	0.4	0.825	2,785	29.4	28.0	1.4	0.519
No Recent AFDC History	1,232	29.1	24.0	5.1	0.088	1,232	44.7	42.5	2.1	0.521
(3) Order Established in 1998 or 1999, among Legal Fathers without a Child Support Order at W-2 Entry										
All Legal Fathers	3,493	14.5%	15.1%	-0.7%	0.642	3,493	21.8%	21.6%	0.3%	0.873
Mother Entered in Lower Tier	2,176	13.6	12.4	1.2	0.517	2,176	20.4	18.8	1.6	0.464
No Recent AFDC History	399	26.1	31.6	-5.5	0.332	399	38.8	39.0	-0.2	0.971
Mother Has Order at Entry	57	25.2	3.4	21.9	0.164	57	59.0	14.5	44.5	**0.041**
Higher Child Support History	161	4.9	6.8	-2.0	0.669	161	28.8	31.8	-3.0	0.777
(4) Orders Established in 1999 among Men Becoming Legal Fathers between W-2 Entry and the End of 1998										
All Legal Fathers	NA	NA	NA	NA	NA	2,139	77.5%	73.3%	4.1%	0.076
(5) Order Changes among Legal Fathers with Child Support Orders at W-2 Entry										
All Legal Fathers	10,812	6.5%	8.0%	-1.4%	**0.017**	10,812	14.3%	14.9%	-0.6%	0.451
Mother Entered in Lower Tier	6,576	5.9	7.9	-2.0	**0.010**	6,576	13.1	13.9	-0.8	0.468
No Recent AFDC History	444	10.8	15.3	-4.5	0.228	444	24.0	31.1	-7.2	0.160
Mother Has Order at Entry	10,485	6.4	8.0	-1.6	**0.010**	10,485	14.0	14.7	-0.7	0.432
Higher Child Support History	2,523	11.3	16.0	-4.6	**0.008**	2,523	23.2	25.1	-1.9	0.406

Notes: All means are regression-adjusted, using the basic list of control variables, plus other selected variables as described in the text. Probability values of 0.05 or less are shown in bold type. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33.

Sample Size: Panel 1: Children of nonmarital fathers without paternity at W-2 entry. Panel 2: Mothers who have not had paternity established at W-2 entry. Panel 3: Legal fathers with no child support order at W-2 entry. Panel 4: Men who became legal fathers between W-2 entry and the end of 1998. Panel 5: Legal fathers who had child support orders at W-2 entry.

to occur. Those who enter W-2 after participating in AFDC have already been encouraged to pursue paternity establishment as part of their involvement in AFDC and are likely to have been exposed to the child support system under the old rules. Resident mothers with no AFDC experience in the previous 24 months are more likely to be exposed for the first time to organized governmental efforts to encourage and facilitate the establishment of paternity. However, the effect for mothers with no recent AFDC experience declines by the end of 1999 and is no longer statistically significant, suggesting that although participation in the experiment accelerates paternity establishment among this subgroup, eventually the control group may catch up with the experimental group.

Table I.4.3 also shows experimental effects in 1998 for those with an order at entry and those with a history of higher child support. For the latter group, the difference between experimental and control groups remained significant at the end of 1999. This suggests that those mothers on whose behalf higher levels of support had been paid in the past may have expected greater gains from establishing paternity for another child, and thus were more affected by the experiment.

We did parallel analyses using the mother as the unit of analysis. These are shown in Panel 2 of Table I.4.3.[57] None of the experimental and control differences were significant for all mothers or for any of the sub-groups.[58] The experimental/control-group differences for mothers with no recent AFDC experience were in the predicted direction in 1998, but were not statistically significant. The sample sizes are much smaller when mothers are used as the unit of analysis, making it more difficult to find statistical significance for relatively modest effects.

On balance, the results provide some support for an experimental effect that increased rates of paternity establishment among two groups for which we would be most likely to observe it—children in families who were entering W-2 without a previous history of AFDC receipt and children of mothers who had comparatively high levels of child support paid on their behalf in the past.

Effects on Support Orders

If the experiment changes the incentives to cooperate with the formal child support system, those who are in the experimental group may be more cooperative in helping the child support agency pursue support orders than are those who are in the control group. The results for support orders at entry (not shown) indicate that among legally identified fathers of children in W-2, approximately 75 percent had an order at the time the mother entered W-2. In Panel 3 of Table I.4.3, we examine the 3,493 legal fathers who did not have an order at entry.[59]

Among those without an order, around 15 percent had an order at the end of 1998 and 22 percent had an order at the end of 1999. The experimental/control difference in child support orders is not statistically significant for the total sample, but is significant for one of the subgroups, women with an order at entry in 1999. We do not, however, place much confidence in this difference, since only 57 fathers are in this group.

Panel 4 contains results for men who became legal fathers during 1998. When we look at the establishment of an order among these men by the end of 1999, we find an effect in the expected direction, but it is not statistically significant.

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[57]The variables in the regression analysis included assignment, mother's AFDC history, residential location, W-2 tier, mother's age, mother's race, and mother's education.

[58]We could not do analyses of the subgroups of women who had a child support order at entry and who had a history of higher child support, since the women in Panel 2 had had no previous experience with the child support system.

[59]The results are regression-adjusted, and the control variables were assignment, mother's AFDC history, residential location, W-2 tier, age of the couple's children, mother's age, mother's race, mother's education, and father's earning history.

On balance, the results provide no support for an experimental effect in the predicted direction.

Changes in Orders during 1998 and 1999

The fifth panel in Table I.4.3 examines the 10,812 legal fathers who had an order established at W-2 entry, and considers an experimental effect on whether these orders had been modified by the end of 1998 and 1999. (An order modification requires a court action.) The variables that were used in the regression equation were assignment, mother's AFDC history, residential location, W-2 tier, age of the couple's children, mother's age, mother's race, and the father's earning history.

A very small percentage of orders (under 15 percent) changed during the two years of the experiment. It was hypothesized that the experiment might increase the percentage of orders that were changed, since those in the experimental group had more incentive to seek higher orders than did those in the control group. The evidence in the fifth panel of Table I.4.3 does not support such a view. The experiment had a significant impact in the unexpected direction in 1998. These effects were not present by the end of 1999. The significant effect was present for the total sample, among lower tier mothers, among those with an order at entry, and among those with a history of higher child support. This could reflect the different incentives faced by child support enforcement agencies in pursuing order changes for the control group.

Summary of Effects on Paternity Establishment and Orders

In this section we have reviewed the experimental evidence that the policy change in the child support pass-through had an effect on paternity establishment, new child support orders, and changes in existing orders. We found:

- At the end of 1998, paternity establishment rates were higher for the children of those mothers in the experimental group who were new to the welfare system, had an order at entry, or had high amounts of child support in the past. At the end of 1999, the rates were significantly higher only among the last group.
- For the full sample, there was no discernible impact on whether fathers without an order were ordered to pay support, and there were few consistent impacts for the expected subgroups.
- There was no evidence for the expected increase in the revision of existing orders, and some evidence for contrary results in 1998.

I.4.3 Experimental Impacts on Program Participation[60]

The heart of the child support reform is to increase the amount of child support received by mothers, an effect clearly seen in previous sections. This additional income could then affect participation in several government programs; these potential secondary effects are the topic of this section.

The reform could have offsetting effects on W-2 participation. On the one hand, mothers may be able to use additional child support to make it easier for them to enter the private labor market, so the experimental group could have lower W-2 payments. On the other hand, those in the control group may leave W-2 quickly so that they can begin to keep all child support paid on their behalf and thus they may have lower W-2 payments. Preliminary analysis of the W-2 program suggests that receiving child support is associated with an increased likelihood of moving to an upper tier or off the program by the end of the

[60]This section is based on the work of Maria Cancian and Daniel R. Meyer, who thank Chi-Fang Wu for exceptional research assistance. A preliminary version of this section was presented at the CSDE National Advisory Board meeting, November 2000. The authors thank participants and especially our discussant, Greg Duncan, for helpful comments. Further information on program participation can be found in Volume II.

first year (Cancian and Meyer, 1999). Thus to the extent that the reform increases support, it may also decrease W-2 program participation.

There is little prior research on the effect of child support on other government programs among low-income families. Because those who receive support have higher incomes, they may be less likely to receive governmental payments, either because their income makes them eligible for lower amounts of payments (or ineligible altogether), or because they do not feel the need for additional assistance and do not apply. Thus to the extent that the experimental group receives higher child support, they may be less likely to receive food stamps or Medicaid (called Medical Assistance in Wisconsin). The effects on child care subsidies are more ambiguous: those in the experimental group may be more likely to work, which would lead to higher child care subsidies, but they may also have higher income, which would lead to higher copayments and lower subsidies.

We examine all 15,977 resident mothers in our research population. We analyze participation by looking at average payments and services in four programs: W-2 (in which we consider only the cash payments of the lower tiers), food stamps (in which we consider the cash value of the stamps), health programs (in which we consider the average cost per participant of Medicaid and BadgerCare, Wisconsin's expanded health insurance program),[61] and child care subsidies (in which we consider the subsidy net of copayments). We take all information from the CARES system.[62] We compare average payments over all cases, presenting regression-adjusted means.[63]

W-2 Payments

The first panel of Table I.4.4 shows the average amount of W-2 payments. In 1998, the average amount for all resident mothers (including those who did not receive any cash payments) was $2,927 for those in the experimental group and $2,990 for those in the control group. The difference between the entire experimental and control groups in the average amount of W-2 payments is fairly small, but is marginally statistically significant in 1998. In 1999 the level of W-2 payments for both groups declined by more than half, and the difference in receipts between experimental and control groups is no longer statistically significant.

For both groups, these declines primarily reflect declining participation in the lower tiers of W-2, rather than a change in the payment amount among recipients. Because both the experimental and the control group receive a full pass-through during periods in which they are in an upper tier or off W-2, the direct effects of the reform are likely to be concentrated in the early period, before families move out of the lower tiers. Of course, those still in the lower tiers continue to receive different amounts, so direct effects continue for this group, and any indirect impacts generated during periods in which the family was in a lower tier may continue even when the family moves to an upper tier or off W-2 altogether.

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[61]We use 1999 average per-person HMO capitation rates within a health-cost area (as defined by the Wisconsin Department of Workforce Development) to estimate Medicaid costs per case. We assume per-person costs in all Medicaid subprograms are the same as per-person costs in the regular Medicaid program. For BadgerCare, we also use average per-person HMO capitation rates within a health-cost area. We do not offset BadgerCare costs with the premiums collected from participants because that information is not currently available to us. We believe this does not have a substantial impact since premiums are not required until net income (that is, gross income less up to $90/month of work-related expenses and up to $200/month of child care expenses) reaches 150 percent of the poverty line.

[62]Child care subsidies in Milwaukee County are taken from the SCRIPTS computer system, accessed through CARES.

[63]We use the extended list of control variables; see the box on p. 29.

Table I.4.4
Effects on Program Participation

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Average W-2 Payments Received by Resident Mothers										
All Resident Mothers	15,977	$2,927	$2,990	-$63	0.095	15,977	$1,368	$1,377	-$9	0.819
Mother Entered in Lower Tier	9,634	3,968	4,067	-99	0.053	9,634	1,792	1,811	-18	0.744
No Recent AFDC History	2,005	2,359	2,461	-102	0.206	2,005	1,000	1,001	-2	0.985
Mother Has Order at Entry	8,924	2,925	2,952	-27	0.598	8,924	1,334	1,353	-19	0.712
Higher Child Support History	2,744	2,515	2,691	-176	0.062	2,744	1,050	1,191	-141	0.114
(2) Average Dollar Amount of Food Stamps										
All Resident Mothers	15,977	1,998	1,998	0	0.997	15,977	1,837	1,848	-11	0.687
Mother Entered in Lower Tier	9,634	2,076	2,100	-24	0.404	9,634	1,991	2,048	-57	0.117
No Recent AFDC History	2,005	1,009	1,013	-4	0.924	2,005	891	958	-68	0.200
Mother Has Order at Entry	8,924	2,239	2,260	-21	0.507	8,924	2,042	2,106	-64	0.092
Higher Child Support History	2,744	2,077	2,131	-54	0.368	2,744	1,730	1,787	-56	0.431
(3) Average Amount of Medicaid and BadgerCare Paid on Behalf of Mother's Family										
All Resident Mothers	15,977	4,134	4,123	11	0.722	15,977	3,952	3,906	46	0.250
Mother Entered in Lower Tier	9,634	4,317	4,342	-26	0.508	9,634	4,197	4,186	11	0.841
No Recent AFDC History	2,005	2,502	2,474	29	0.630	2,005	2,514	2,568	-54	0.522
Mother Has Order at Entry	8,924	4,543	4,563	-21	0.611	8,924	4,343	4,351	-8	0.890
Higher Child Support History	2,744	4,419	4,484	-65	0.417	2,744	4,077	4,006	71	0.498
(4) Average Amount of Child Care Subsidies Paid per Family										
All Resident Mothers	15,977	2,042	2,032	10	0.892	15,977	2,726	2,784	-58	0.517
Mother Entered in Lower Tier	9,634	1,842	1,767	75	0.377	9,634	2,583	2,488	95	0.404
No Recent AFDC History	2,005	1,130	1,229	-99	0.373	2,005	1,904	1,936	-33	0.842
Mother Has Order at Entry	8,924	2,465	2,488	-23	0.826	8,924	3,153	3,316	-163	0.220
Higher Child Support History	2,744	2,161	2,091	70	0.702	2,744	2,617	2,631	-14	0.950

Notes: All means are regression-adjusted, using the extended list of control variables. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33.

Food Stamps

The second panel of Table I.4.4 shows that average Food Stamp payments were about $2,000 in 1998 and slightly lower (about $1,850) in 1999. There is no significant overall experimental effect on average Food Stamp payments. There is no significant difference for any of the key subgroups in 1998. In 1999, payments for those with a child support order at entry were significantly lower, but only marginally so (p=.092); payments for those who entered in a lower tier also appear to have been lower, though the difference is not significant at conventional levels (p=.117).

Governmental Costs for Health Care (Medicaid and BadgerCare)

The third panel of Table I.4.4 examines governmental costs for health care. We include costs for Medicaid and costs for the BadgerCare program, which began in July 1999 and provides insurance coverage for lower-income families with incomes too high for Medicaid. Costs averaged about $4,100 in 1998 and were somewhat lower, about $3,900, in 1999. The table shows no significant overall effect on average Medicaid/BadgerCare costs.

Child Care Subsidies

The fourth panel of Table I.4.4 presents information on child care subsidies. In contrast to the other means-tested programs, costs for both experimental and control groups increased between 1998 and 1999, from about $2,000 to about $2,750. We noted above that the experiment could have contradictory effects on child care subsidies. We see in the table that the reform has had no overall effect on average child care subsidies. There are also no significant effects within the key subgroups.

Sensitivity Testing and Additional Information

This section shows the average payments received in various programs. We now turn to the probability of receiving payments from each of the four programs. We find:

- About 75 percent of mothers received a W-2 payment in 1998, and about 45 percent in 1999.
- Over 90 percent of mothers received food stamps in 1998, and about 80 percent in 1999.
- Virtually all mothers had someone in their family receiving Medicaid/BadgerCare in 1998, and about 90 percent were receiving some form of medical assistance in 1999.
- About 40 percent of mothers received a child care subsidy in both 1998 and 1999.

Examining the difference between the experimental and control groups in the likelihood of receiving payments, we see no overall effect in any of the programs considered.

Table I.4.4 shows results for calendar years. We also compared W-2 payments for the 24 months following entry to W-2 (a two-year period not generally coinciding with calendar years). The results were generally similar.[64]

Summary

As noted above, the reform potentially has counteracting effects on program participation. We find a marginally significant, small effect of the experiment on W-2 payments in 1998, but not in 1999, suggesting that the reform may have sped the process of leaving W-2 but has not had a demonstrated longer-term effect. We find no effects on participation in other programs.

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[64]We also examined participation in other programs as reported by mothers in the Survey of Wisconsin Works Families. We found that about 10 percent of mothers reported receiving unemployment insurance and very few mothers reported Workers' Compensation, Supplemental Security Income, or Social Security. About half the mothers received food vouchers from the Women, Infants, and Children food program. There are no important statistically significant differences in recipiency or average costs between the experimental and control groups.

I.4.4 Experimental Impacts on Resident Mothers' Work, Wages, and Earnings[65]

In this section, we present estimates of the impact of the child support demonstration on the work and earnings patterns of resident mothers. We first consider whether mothers worked in each of the 1998 and 1999 calendar years, and then move to various aspects of employment, including hours worked, wage rates, and total earnings. Where the data are sufficient, we use administrative data from the Unemployment Insurance (UI) system to track employment status, earnings, and number of employers for the almost 16,000 resident mothers in the research population. In assessing the impact of the experiment on outcomes measured only in the survey of resident mothers (including wage rates, hours worked, and months worked), we necessarily rely only on data for the 2,295 resident mothers responding in the first wave and 2,242 resident mothers responding in the second wave. In all cases, we present regression-adjusted estimates of mean levels for mothers in the experimental and control groups.[66]

Hypotheses

We discuss the effects of the full pass-through of child support on resident mothers' employment and earnings. The theory on which these hypotheses rest is somewhat at odds with standard economic theory; we speculate that an increase in exogenous income (child support) might positively affect labor supply, which is the opposite of the standard income effect. Our rationale is straightforward: Given the economic environment created by welfare reform in Wisconsin, resident mothers confront reduced opportunities for receipt of public income support; hence, additional income from child support (or other sources) may be used to expand their opportunities in the labor market. These efforts might involve increasing spending on child care, moving to take advantage of potential job openings, obtaining training, or purchasing an automobile to facilitate travel to work.

In particular, we test two hypotheses. Given that resident mothers in the experimental group on average received more child support income than those in the control group, we hypothesize that:

- Resident mothers in the experimental group will have higher rates of employment, higher wage rates, and higher earnings than those in the control group.
- Resident mothers in the experimental group will work more hours than those in the control group.

Prior Research

Previous research on the relationship between child support receipts and resident mothers' labor supply suggests quantitatively small effects on labor supply owing to increased child support receipts. An early study is that of Graham and Beller (1989), who analyzed the relationship between the receipt of child support and mothers' labor supply, using the Current Population Survey. The authors found that child support had small negative effects on mothers' labor supply, which they attribute in part to child support generally being a small and irregular source of income. More recently, Hu (1999) used longitudinal data on divorced mothers from the Michigan Panel Study of Income Dynamics (PSID) to study the effect of alternative child support policies on welfare and labor force participation. As did some previous studies of this issue (see Robins, 1986; Graham and Beller, 1989), Hu considered the possibility that child support income, labor supply, welfare participation, and remarriage are jointly determined. Hu found that policies that increased child support income to custodial mothers increased the labor force

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[65]This section is based on the work of Maria Cancian and Robert Haveman, who thank Sangeun Lee and David Reznichek for excellent research assistance. A preliminary version of this section was presented at the CSDE National Advisory Board meeting, November 2000. The authors thank participants and especially our discussants, Glen Cain and Rob Hollister, for helpful comments. Further information on the employment and earnings of mothers can be found in Volume II.

[66]We use the basic list of control variables; see the box on p. 29.

participation of mothers with recent marriage breakups, and decreased the welfare participation of those mothers who were working. However, Hu also found that this increase in child support income led to a decrease in the intensity of work among nonwelfare mothers.

Research Results from the Experimental Study

In the following, we review the impact of the experiment on employment, months and hours worked, wage rates, and earnings. Our analysis suggests that the experiment had no impact on the percentage of mothers employed, and had no consistent impact on months or hours worked. We find some evidence to support the hypothesis of an experimental effect on wages but no evidence of an effect on total earnings.

The results of our analysis are summarized in Table I.4.5. The first panel shows the percentage of mothers who worked at any time (i.e., who had any reported earnings in the year) in 1998 and 1999. Our measure of employment comes from the UI data. Overall, about 79 percent of resident mothers are estimated to be employed in both 1998 and 1999. The regression-adjusted results suggest no statistically significant difference between the control and experimental groups in employment rates for the entire sample of mothers or for any of the subgroups. Overall, we find no evidence suggesting mothers in the experimental group increased overall employment (as originally hypothesized) or decreased it (as economic theory would predict) in response to the experiment.

We now turn to a measure of intensity of employment, the number of months worked per year. This information is not available from administrative data, so we rely on information from the survey of resident mothers. The second panel of Table I.4.5 shows the average number of months worked by mothers in the experimental and control groups for both 1998 and 1999. In 1998, the mean number of months worked for all of the mothers is about 4.8, but this level of work intensity rose to nearly 6 months in 1999. We find no statistically significant difference between the experimental and the control groups in months worked in either 1998 or 1999, although, in 1998 only, there is a (marginally) statistically significant difference, in the opposite direction than we expected, for mothers with a history of higher child support.

For mothers who worked in 1998 or 1999, the survey obtained information on the average number of hours per week that they usually worked. This evidence is presented in the third panel of Table I.4.5 for the entire sample of nonresident mothers—including those who worked zero hours. For 1998, mothers worked, on average, 25 hours per week, with a small increase to about 28 hours in 1999, and there is no significant difference between the experimental and control groups. As we did for months worked, we find a significant difference in 1998, in the opposite direction than we expected, among women with a history of higher child support.

The fourth panel of Table I.4.5 compares the mean hourly wage rate for those mothers in the experimental and control groups who worked during the course of a year. For both groups, the hourly wage rate was about $7.25 per hour in 1998, increasing to about $8.00 per hour in 1999. In both years, resident mothers in the experimental group appear to have had slightly higher mean hourly wages than did those in the control group, although the difference for all mothers is only marginally statistically significant (p=.099). In both years, among mothers who had a child support order at entry, the experimental group had wages statistically significantly higher than the wages of control-group mothers.

The most comprehensive indicator of the extent of the overall labor market performance of workers is their annual earnings level, a measure that reflects both total hours worked and the hourly wage rate. Administrative data from the UI system are again used in evaluating the impact of the experiment on total annual earnings, because these data are available for a much larger sample (though the information available on each person may be less comprehensive). Average annual earnings for all mothers (including those with no earnings) are shown in the final panel of Table I.4.5. Overall, mothers in the experimental and control groups earned from about $4,300 to $4,400 in 1998. These figures rose to

Table I.4.5

Effects on Resident Mothers' Employment and Earnings

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Percentage of Mothers with Any Earnings (Administrative Data)										
All Resident Mothers[a]	15,976	78.6%	79.6%	-1.0%	0.214	15,976	79.0%	78.8%	0.2%	0.833
Entered in Lower Tier	9,633	70.5	72.0	-1.4	0.222	9,633	73.5	73.5	0.0	0.972
No Recent AFDC History	2,005	81.2	84.2	-3.0	0.117	2,005	80.9	81.5	-0.5	0.785
Has Order at Entry	8,924	80.2	81.4	-1.3	0.230	8,924	80.5	81.2	-0.7	0.472
Higher Child Support History	2,744	78.2	81.2	-3.0	0.132	2,744	77.6	78.9	-1.3	0.527
(2) Number of Months Worked among All Mothers (Survey)										
All Resident Mothers[b]	2,278	4.8	4.7	0.1	0.673	2,226	5.9	5.8	0.1	0.416
Entered in Lower Tier	1,206	3.4	3.5	0.0	0.857	1,178	5.0	4.8	0.2	0.393
No Recent AFDC History	474	5.0	5.1	-0.1	0.728	465	6.8	7.2	-0.4	0.229
Has Order at Entry	1,274	4.9	5.1	-0.2	0.404	1,261	6.0	5.9	0.1	0.537
Higher Child Support History	420	5.1	5.9	-0.8	0.053	413	6.1	6.7	-0.6	0.162
(3) Usual Hours Worked per Week among All Mothers (Survey)										
All Resident Mothers[c,d]	2,249	25.3	25.0	0.3	0.689	2,209	28.2	27.6	0.6	0.490
Entered in Lower Tier	1,186	21.8	20.9	0.8	0.470	1,169	25.8	24.5	1.3	0.261
No Recent AFDC History	469	27.2	28.7	-1.6	0.394	462	30.4	31.7	-1.4	0.418
Has Order at Entry	1,255	26.1	26.5	-0.3	0.765	1,255	27.9	28.3	-0.4	0.722
Higher Child Support History	416	25.9	30.3	-4.3	**0.019**	411	28.5	29.3	-0.8	0.688
(4) Average Hourly Wage Rate for Mothers Reporting Current or Last Job (Survey)										
All Resident Mothers[e]	1,586	$7.35	$7.18	$0.16	0.099	1,666	$8.16	$7.95	$0.21	0.099
Entered in Lower Tier	724	7.37	7.12	0.25	0.080	800	8.09	7.78	0.31	0.053
No Recent AFDC History	353	7.29	7.27	0.02	0.932	370	8.03	8.04	-0.01	0.958
Has Order at Entry	921	7.53	7.26	0.27	**0.034**	960	8.39	7.92	0.47	**0.006**
Higher Child Support History	315	7.43	7.53	-0.10	0.667	321	8.03	8.32	-0.28	0.453

Table I.4.5, continued

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(5) Average Earnings among All Mothers (Administrative Data)										
All Resident Mothers[a]	15,976	$4,380	$4,272	$108	0.273	15,976	$6,040	$5,885	$155	0.225
Entered in Lower Tier	9,633	2,946	2,923	23	0.821	9,633	4,784	4,727	57	0.704
No Recent AFDC History	2,005	4,353	4,364	-11	0.964	2,005	6,588	6,580	7	0.983
Has Order at Entry	8,924	4,651	4,605	46	0.735	8,924	6,298	6,174	124	0.477
Higher Child Support History	2,744	4,825	5,065	-240	0.354	2,744	6,412	6,656	-243	0.463

Note: All means are regression-adjusted, using the basic list of control variables. Probability values of 0.05 or less are shown in bold type. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33.

[a]Total sample was 15,977 cases. There was one missing case because there was no matching Social Security number.

[b]Total sample in 1998 was 2,295 cases. Cases are missing because they did not know if they did any work for pay in 1998 (4), refused to answer (4), or worked but did not specify months worked (9). Total sample in 1999 was 2,242 cases. Cases are missing because they did not know or refused to answer when they had last worked for pay (13), or worked for pay but did not specify months worked (3).

[c]Total sample was 2,295 in 1998. Missing cases include those with missing hours (23, most of whom indicated varying hours) or who did not know or refused to answer if they worked (23). Total sample was 2,242 in 1999. Missing cases include those with missing hours (20, most of whom indicated varying hours) or those who did not know or refused to answer if they worked (13).

[d]In 1998 424 mothers and in 1999 319 mothers had had a W-2 assignment in the past four weeks and were therefore not asked about current employment. These cases were assumed to be employed zero hours.

[e]Total sample in 1998 was 2,295 cases. Missing cases include those who had a recent W-2 assignment (424), those for whom wages were missing (100, most of whom refused to answer), who did not have work (162), or did not know or refused to answer if they worked (23). Total sample in 1999 was 2,242 cases. Missing cases include those who had a recent W-2 assignment (319), those for whom wages were missing (93, most of whom refused to answer), who did not have work (151), or did not know or refused to answer if they worked (13).

about $5,900 to $6,000 per year in 1999. Although the average earnings of all of the resident mothers in the experimental group appear to have exceeded those of mothers in the control group in both years, the difference in earnings is small and statistically insignificant.

Summary of Research Results

We hypothesized that the increased child support received by mothers in the experimental group would lead to higher levels of labor force participation, higher wages, and higher earnings. But we also recognized that the greater availability of nonlabor income might have the opposite effect, reducing mothers' work and earnings. Perhaps in part because of these countervailing forces, we find only limited evidence of effects in either direction. In particular, we find no impact on the percentage of mothers employed, no persistent impact on months or hours worked, and no impact on total earnings. Thus, we conclude that the experiment had at most a modest effect on wages overall, with more substantial impact on those mothers with an order at entry.

I.4.5 Experimental Impacts on Mothers' Income and Economic Hardship[67]

An ultimate goal of the full pass-through is that, with sufficient child support and earnings, mothers will be able to stop receiving W-2 (and food stamps) and maintain or increase their total income. Changes in economic status due to the pass-through might be captured by measures of hardship, as well as income. In this section, we begin by summarizing and combining the results of several previous sections, presenting information about the effect of the full pass-through on mothers' total personal income using administrative data. We then turn to survey information to examine an alternative measure of personal income, a measure of family income, and selected measures of economic hardship. These analyses allow us to consider the extent to which the increase in child support received is complemented or offset by changes in other income sources.

Our first measure of personal income is the sum of four components addressed above: child support receipts (section I.4.1), W-2 payments (section I.4.3), food stamp amounts (section I.4.3), and earnings (section I.4.4).[68] This is the most comprehensive measure available with administrative data. As a measure of personal income, it has limitations: we do not have information on other sources of income, including own earnings not covered by Unemployment Insurance (UI), the Earned Income Tax Credit (EITC), and other benefits, such as Social Security. Moreover, we do not have information on "disposable" income; for example, we do not know child care expenditures, taxes, or other nondiscretionary work expenses.[69] We show results for 15,976 mothers. We report regression-adjusted amounts that account for random differences between the experimental and control groups in initial characteristics.[70]

With this measure of income, we find fairly low average incomes, about $10,000 in each year. The average income in 1998 comprises about $600 in child support, about $4,300 in earnings,[71] about

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[67]This section is based on the work of Maria Cancian and Daniel R. Meyer, who thank Hwa-Ok Park for excellent research assistance. Further information on mothers' income can be found in Volume II.

[68]In 1998, we also include small amounts of AFDC payments received by mothers before AFDC was eliminated, including these figures with W-2 payments.

[69]Whether in-kind payments should be treated as income, and how they should be valued, is controversial. We include the cash value of food stamps but do not include a measure of Medicaid or BadgerCare in income.

[70]Because we use measures of income and hardship from both the administrative data and the survey, we use the basic list of control variables; see the box on p. 29.

[71]These estimates of earnings include all mothers regardless of whether earnings are received. Earnings among those with earnings are examined in Volume II, Chapter 5.

$3,200 in W-2 payments and $2,000 in food stamps. Between 1998 and 1999, child support increases to about $800 and earnings increase to about $6,000. Over the same period, W-2 payments fall by more than half (to about $1,400) and food stamp payments decline slightly (to about $1,800). Overall, the increases in child support and earnings are offset by the decline in payments, so that total income using these measures is approximately the same.

We now turn to a comparison of total income in the experimental and control groups. The first panel of Table I.4.6 shows that mothers in the experimental group had higher personal incomes than those in the control group. The difference, although statistically significant at the .10 level, is small—$160 in 1998 and $237 in 1999—and is due to the higher levels of child support and earnings of the experimental group, which are somewhat offset by lower payments (especially in 1998). The effects of the experiment on total income were no greater in the key subgroups.

We replicated our measure of personal income using data from the survey (but do not report the results in Table I.4.6). In particular, we considered the child support, W-2, food stamps, and earnings that mothers reported receiving in the survey, examining 2,213 mothers in 1998 and 2,152 in 1999.[72] The administrative record of formal child support, W-2, and food stamps is a very accurate reflection of formal child support and payments received in Wisconsin. Nevertheless, mother's reports of earnings may be more comprehensive than the administrative record (though for earnings in UI-covered employment, administrative records are again generally more accurate). As discussed in more detail in Volume II, reported earnings are substantially higher in the survey than in the administrative data, child support amounts are roughly comparable, and amounts of W-2 and food stamps are generally lower. The sum of these four sources is comparable to that found in the administrative data, about $10,000 in 1998; it is somewhat higher, about $11,000, in 1999. There is no significant difference between the experimental and control groups in this measure of personal income. The difference between this result and the administrative measure shown in Panel 1 of Table I.4.6 is primarily because the survey shows no significant difference in child support received, whereas in the administrative data the experimental group receives higher child support. Because we believe the administrative data reports of formal child support are more accurate, we place more weight on the administrative data results for personal income.

We now turn to a measure of *family* income, again using the survey. In addition to the sources above, our measure of family income also includes unemployment insurance, Workers' Compensation, Supplemental Security Income (SSI), Social Security benefits, alimony, the earnings of a spouse or partner, the other income of a spouse or partner, and any other source of income. (In 1999, we added two additional specific sources, SSI for a child and money from family or friends.) Although this measure is a fuller measure of income, it again does not account for nondiscretionary expenditures. We report results for 2,112 mothers in 1998 and 2,033 mothers in 1999.[73]

Most of the new sources individually provide little additional income in the aggregate, less than $250 per year. Although some sources are substantial for those who receive them (e.g., the median amount of SSI received by those who receive something is about $6,000), these sources are received by fewer than 10 percent of the mothers. The exception is a spouse or partner's earnings. Sixteen percent of mothers had a spouse or partner with earnings in 1998, and this rose to 19 percent in 1999. Among those who reported a spouse or partner with earnings, the mean amount was over $10,000. The overall mean amount of income from spouses' or partners' earnings (including the zeroes) was about $1,500 in 1998

[72]We made selected imputations. See Volume II, Chapter 6 for details. We report total income only for those in which amounts are present for all relevant sources after we have completed imputations. These rules result in our excluding 81 mothers in the experimental group and 89 mothers in the control group for whom we had incomplete income.

[73]Compared to the previous analysis of mothers' personal income, we exclude an additional 102 mothers in 1998 and 120 mothers in 1999 for whom we had incomplete income.

Table I.4.6

Effects on Income and Economic Hardship of Resident Mothers

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Average Total Personal Income of Resident Mothers (Administrative Data)										
All Resident Mothers[a]	15,976	$10,140	$9,979	$160	0.083	15,976	$10,088	$9,851	$237	0.054
Mother Entered in Lower Tier	9,633	9,835	9,765	71	0.510	9,633	9,322	9,210	112	0.449
No Recent AFDC History	2,005	8,344	8,286	58	0.814	2,005	9,458	9,373	86	0.803
Has Order at Entry	8,924	11,043	10,871	172	0.164	8,924	10,893	10,706	187	0.254
Higher Child Support History	2,744	11,859	11,664	195	0.424	2,744	11,610	11,657	-47	0.884
(2) Average Total Family Income of Resident Mothers (Survey)										
All Resident Mothers	2,112	$12,145	$12,575	-$431	0.234	2,033	$14,663	$14,511	$152	0.749
Mother Entered in Lower Tier	1,120	11,542	11,419	123	0.783	1,079	13,040	13,338	-298	0.610
No Recent AFDC History	443	11,948	13,768	-1,820	**0.048**	418	16,001	16,422	-421	0.696
Has Order at Entry	1,180	12,607	13,476	-870	0.084	1,160	15,343	15,309	34	0.958
Higher Child Support History	393	14,067	16,014	-1,947	**0.044**	383	16,823	19,055	-2,231	0.097
(3) Percentage of Mothers Reporting a Food, Shelter, or Telephone Hardship (Survey)										
All Resident Mothers	1,130	65.0%	68.3%	-3.3%	0.237	2,128	61.5%	60.5%	1.0%	0.633
Mother Entered in Lower Tier	599	65.9	69.9	-4.1	0.290	1,130	66.0	64.0	1.9	0.502
No Recent AFDC History	232	52.4	58.1	-5.7	0.392	443	50.9	53.0	-2.1	0.661
Has Order at Entry	646	64.7	67.9	-3.2	0.389	1,218	63.7	59.4	4.4	0.119
Higher Child Support History	216	68.2	66.6	1.6	0.807	396	61.6	52.5	9.2	0.068

Notes: All means are regression-adjusted, using the basic list of control variables. Probability values of 0.05 or less are shown in bold type. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33. Food hardship = often or sometimes not enough to eat; Housing hardship = gas/electricity turned off, doubled up, lived in shelter or homeless; Telephone hardship = no phone or phone disconnected. Sample for third panel includes only those subsampled for the economic hardship questions.

[a]Total sample was 15,977 cases. There was one missing case because there was no matching Social Security number.

and $2,400 in 1999. Total family income increased between 1998 and 1999 from about $12,500 to over $14,500.

We now turn to a comparison of total income in the experimental and control groups. The second panel of Table I.4.6 shows no significant difference between the groups in average family income. In 1998, there are statistically significant differences within two subgroups (and marginally significant differences in a third); in each case the experimental group had *lower* average income, contrary to expectations. Among those without recent AFDC history, those in the experimental group reported lower average earnings from a spouse or partner. Among those with a history of higher child support, those in the experimental group reported lower own earnings than the control group. In no case was the significant difference sustained in 1999.

In addition to measures of personal and total income, the survey included measures of economic hardship. We consider measures of food, shelter, and telephone-related hardship.[74] We consider mothers to have had a food hardship if they report that they "often" or "sometimes" did not have enough to eat in the previous year. With this definition, about 17 percent of mothers experienced a food hardship in 1998, a proportion declining slightly to 15 percent in 1999. The most common types of shelter problems were utility disconnections and moving in with others: in 1998, 22 percent of mothers had their gas or electricity turned off and 20 percent moved in with others because they could not pay the rent. These numbers declined slightly in 1999, to 20 percent and 16 percent, respectively. Less common was living in a shelter or being homeless: in 1998, 7 percent had lived in a shelter, and 3 percent had been homeless at some point. Numbers for 1999 were 5 percent and 3 percent. If we take these four measures of shelter hardship together, 38 percent of mothers experienced at least one shelter hardship in 1998 and 33 percent in 1999. A full description of those hardship questions is provided in Volume II, Chapter 6.

Finally, we consider that those who had spent time without a telephone or who had their telephone disconnected because they could not pay the bill experienced a "telephone hardship." This is the most common type of hardship, reported by 54 percent of the mothers in 1998 and 50 percent in 1999. As discussed in Volume II, these levels of hardship are somewhat higher than among a general low-income population but fairly comparable to a welfare population in other states.

The third panel of Table I.4.6 compares the experimental and control groups in terms of whether they reported any of the three types of hardship. Because we did not find differences in total income in the survey, it seems unlikely that there will be differences in the level of economic hardship. As anticipated, the experimental and control groups are similar in the percentage who report having an economic hardship, and in the percentages who report any of the individual hardship measures. None of the subgroups show statistically significant differences in both years.

In sum, levels of income are fairly low, and levels of hardship fairly high. The effects of the full pass-through on the measures in this section were somewhat inconsistent. The administrative data show that experimental-group mothers had somewhat higher personal incomes than control-group mothers, but these findings are not confirmed in the survey. The survey also shows no significant differences in overall family income or economic hardship.

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[74]The sample size for these questions is smaller than the overall survey sample. Because of constraints on the overall length of the survey, the 1998 survey asked questions about economic hardship of only half the sample. The first weeks of the 1999 survey followed a similar rule, but we quickly discovered we had more time than anticipated, and began to ask these questions of all mothers.

I.4.6 Experimental Impacts on Government Costs[75]

The previous sections considered mothers' income, including receipt of public assistance and child support. We now consider the effect of the experiment on government costs. Because the government is no longer retaining child support paid to those in the experimental group, governmental costs for this group increase.[76] However, the increased child support received by mothers in the experimental group consists of two components. First, as long as experimental-group mothers remain in a lower tier of W-2, they will (mechanically) receive more child support even if the same amount is paid, because child support previously retained by the government will now be paid to the mother. Second, we have seen above that mothers in the experimental group receive more child support in part because fathers of children in the experimental group are *paying* more. To the extent that these additional payments would not have been made had fathers expected the state to retain the support, this increase in child support does not come at the expense of government.

In addition to the direct effect of child support retained on government costs, the child support reform could have a variety of secondary effects that could also affect costs. The heart of the reform is to increase the amount of child support received by mothers, an effect clearly seen above. Also potentially important are changes in the receipt of publicly funded programs, including W-2 payments, food stamps, and Medicaid (Medical Assistance). Evidence presented in section I.4.3 suggests there were a few statistically significant declines in payment receipt. The extent to which reductions in assistance offset the state's loss of formerly retained child support is the focus of this section.

In this section we examine all 15,977 resident mothers in our research population. In our analysis of government costs, we first sum the costs of the four programs reviewed in Section I.4.3: W-2 cash payments, food stamps, health coverage (Medicaid and BadgerCare, Wisconsin's expanded CHIP program), and child care subsidies.[77] All this information is taken from the CARES system. We then subtract the amount of child support retained by the government and any amount collected from a nonresident father to offset Medicaid expenditures related to a child's birth, with information taken from the KIDS system. We report regression-adjusted means.[78]

Note that this is not a comprehensive list of government costs and savings: among other omissions, we are not able to show payments to employers through Trial Jobs, payments to clients for transportation, housing subsidies, income and payroll tax collections by the government, expenditures under the Earned Income Tax Credit, or administrative costs. (We believe the administrative expenditures in the child support system would be lower if all cases faced the same policy rules, that is, if all cases had a full pass-through. However, we are unable to measure this potential savings.) We calculate net governmental costs per case for the programs for which we have data, recognizing that our cost estimates are incomplete. Our measure of net costs includes both costs to the state and costs to the federal

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[75]This section is based on the work of Maria Cancian and Daniel R. Meyer, who thank Chi-Fang Wu for exceptional research assistance. A preliminary version of this section was presented at the CSDE National Advisory Board meeting, November 2000. The authors thank participants and especially our discussant, Greg Duncan, for helpful comments.

[76]The total amount of child support passed through may be used as state TANF maintenance of effort (MOE) as long as the state's determination of eligibility for cash assistance does not include child support payments as income.

[77]We also include a small amount of AFDC payments in early 1998 before the program was completely phased out.

[78]We use the extended list of control variables; see the box on p. 29.

government. We report regression-adjusted costs that account for random differences between the experimental and control groups in initial characteristics.[79]

Total Measured Net Governmental Costs

Costs associated with W-2 payments, food stamps, Medicaid/BadgerCare, and child care subsidies are summarized in the first panel of Table I.4.7. Average costs per case are substantial, about $11,300 in 1998, declining to $9,900–$10,000 in 1999. The table shows generally lower costs for those in the experimental group, though the effects are not statistically significant.

The second panel shows average child support retained. Members of the experimental group do have child support retained, though mean amounts are lower for experimental cases (averaging $126 in 1998 and $147 in 1999) than for controls (averaging $230 in 1998 and $221 in 1990). Even though current child support is passed through to participants in the experimental group, support is retained when collections are made through federal income tax intercepts, because by law these collections primarily go to reimburse the government. In addition, the full pass-through affects only the amount paid in current support; any amount collected that is greater than the amount currently owed will go toward back debts, and the government retains a portion of these extra payments.

The final panel of Table I.4.7 shows total government costs. These are the sum of total costs of program participation (Panel 1), less child support retained (Panel 2), less small amounts that the nonresident parent pays to reimburse the state for costs associated with a child's birth. Government costs per case are substantial, and, reflecting the pattern of program participation, are higher in 1998 (over $11,100) than 1999 (about $9,600). There is no significant difference between the experimental and control groups in average net governmental costs.[80] This lack of difference in overall costs is important because many expect that if the government were to give up its claim on a portion of child support receipts, this would have considerable costs. Instead, we find that the amount of child support that the state is retaining for the control group is quite small in terms of overall costs, so that small savings in the other programs lead to a cost-neutral reform. Moreover, there is no significant difference in costs in any of the subgroups we examine.[81]

I.4.7 Experimental Impacts on Nonresident Fathers' Employment and Earnings[82]

The previous sections have focused on the effect of the experiment on mothers' program participation, employment and income, and the implications for government costs. We now turn our attention to effects on fathers. In this section we report on the impact of the experiment on the employment, earnings, and program participation of fathers. In the following sections we consider fathers' income and hardship, and their social and economic involvement with their children. Our analysis employs the available administrative data to describe patterns of employment, earnings, and program

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[79]We use the extended list of control variables; see the box on p. 29.

[80]This table shows net costs in calendar years, with a difference between the experimental group and control group of $69 in 1998 and $46 in 1999. An analysis of costs in relative years (that is, the first and second years after entry) also shows no significant differences and similar net costs of $100 in the first year and $35 in the second.

[81]Our tables show results for resident mothers. We also compared total net costs for resident fathers. Although resident fathers had lower average costs than resident mothers, there was no significant difference in net costs for all resident fathers between the experimental and control groups.

[82]This section is based on the work of Maria Cancian and Robert Haveman, who thank Sangeun Lee for exceptional research assistance. A preliminary version of this section was presented at the CSDE National Advisory Board meeting, November 2000. The authors thank participants and particularly our discussants, Glen Cain and Rob Hollister, for helpful comments. Further information on fathers' employment and earnings can be found in Volume II.

Table I.4.7

Effects on Government Costs

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Average Total Costs from Program Participation[a]										
All Resident Mothers	15,977	$11,301	$11,343	-$42	0.684	15,977	$9,883	$9,915	-$32	0.813
Mother Entered in Lower Tier	9,634	12,467	12,533	-66	0.616	9,634	10,563	10,533	30	0.867
No Recent AFDC History	2,005	7,000	7,180	-179	0.325	2,005	6,309	6,464	-155	0.558
Mother Has Order at Entry	8,924	12,401	12,493	-91	0.536	8,924	10,873	11,126	-254	0.183
Higher Child Support History	2,744	11,363	11,579	-215	0.414	2,744	9,473	9,614	-141	0.675
(2) Average Amount of Child Support Retained by the State										
All Resident Mothers	15,977	$126	$230	-$104	**0.000**	15,977	$147	$221	-$74	**0.000**
Mother Entered in Lower Tier	9,634	127	262	-135	**0.000**	9,634	147	244	-98	**0.000**
No Recent AFDC History	2,005	16	59	-42	**0.000**	2,005	24	64	-40	**0.000**
Mother Has Order at Entry	8,924	210	382	-172	**0.000**	8,924	232	345	-113	**0.000**
Higher Child Support History	2,744	393	777	-384	**0.000**	2,744	381	624	-243	**0.000**
(3) Average Amount of Total Governmental Costs[b]										
All Resident Mothers	15,977	$11,124	$11,055	$69	0.505	15,977	$9,654	$9,608	$46	0.732
Mother Entered in Lower Tier	9,634	12,294	12,220	74	0.579	9,634	10,346	10,223	123	0.492
No Recent AFDC History	2,005	6,957	7,094	-137	0.452	2,005	6,191	6,309	-118	0.658
Mother Has Order at Entry	8,924	12,118	12,027	91	0.541	8,924	10,541	10,675	-134	0.486
Higher Child Support History	2,744	10,848	10,661	187	0.484	2,744	8,932	8,854	78	0.818

Notes: All means are regression-adjusted, using the extended list of control variables. Probability values of 0.05 or less are shown in bold type. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33.

[a]Total costs from program participation are summed across the following programs: W-2, Food Stamps, Medical Assistance, Child Care Subsidies, and AFDC.

[b]Total governmental costs are the sum of W-2; Food Stamps, Medical Assistance, Child Care Subsidies, and AFDC; minus child support retained and other reimbursements.

participation for all fathers for whom paternity was legally established at the time the mother entered W-2. In particular, we rely on data from the Unemployment Insurance (UI) system to track employment status, earnings, and number of employers for the 13,673 fathers for whom we have a Social Security number.[83] We use data from the survey of nonresident fathers (575 in 1998, and 608 in 1999) to describe the impact of the experiment on wage rates, hours worked, months worked, and informal employment. Finally, we use administrative data on W-2 participation and receipt of food stamps or Medicaid, in addition to survey information on a broader measure of program participation.[84] As discussed above, we are able to consider additional outcomes using survey data, but the extent to which it is possible to generalize from our conclusions is limited by low response rates for the surveys of nonresident fathers.

Hypotheses

We address three key hypotheses regarding the effects of the experiment on fathers' employment and earnings:

- Nonresident fathers in the experimental group will show higher levels of formal employment, more stable formal sector employment, and higher formal sector earnings.
- Nonresident fathers in the experimental group will have lower levels of informal employment.
- Nonresident fathers in the experimental group will be more likely to access W-2 and related services.

These hypotheses rely on expected responses to the incentives present for fathers whose children are in the experimental group relative to those with children in the control group. For the control group (in which incentives reflect previous state policy), child support is at least partially retained by the state, reducing take-home earnings to the father but giving little direct benefit to him or his children. In this case, the child support payment is essentially a tax on earnings, and hence is expected to reduce employment. This is not so for the experimental group, and as a result nonresident fathers with children in the experimental group are expected to have higher and more stable employment and earnings.

Under the former policy there were also substantial incentives for fathers to avoid the formal child support enforcement system by frequently switching employers or working informally. Similarly, the desire to avoid the formal system may have discouraged participation in W-2 and related programs. The incentives in place under the former policy continue to affect the control group but are expected to be mitigated for the experimental group, since all current child support will benefit their children.

Prior Research

Recent research on the relationship between child support policy and nonresident fathers' employment offers little evidence of an effect on the labor supply because of increased child support payment responsibilities (Klawitter, 1994; Freeman and Waldfogel, 1998). Freeman and Waldfogel investigate this issue using data from the 1986 and 1991 Surveys of Income and Program Participation. The authors first demonstrate that stringent state enforcement did increase the probability that child support payments would actually be made, supporting the linkage between policies and the decreased take-home pay of the fathers. However, results offer little evidence of a labor supply response to policies requiring fathers to take greater responsibility for child support payments. A more recent study by Bitler (2000) uses state variation in the stringency of child support collection policies to measure the labor supply effects of the amount of child support payments, using data from the National Longitudinal Survey of Youth. The author finds that there was an increase in weeks worked and hours worked in the previous

[83]There were 14,343 legally established fathers at baseline, but we have no Social Security number for 670, leaving a final sample size of 13,673.

[84]In all cases we present regression-adjusted estimates of mean levels, using the basic set of control variables listed in the box on p. 29.

week, but no effect on usual hours worked. Overall, her estimates suggest modest effects on the labor supply of noncustodial fathers.

Research Results from the Experimental Study

In the following, we review the impact of the experiment on nonresident fathers' employment and number of employers, months worked, wage rates, formal and informal earnings, and program participation. Our analysis suggests that the experiment had a significant impact in the anticipated direction on informal earnings, reducing them for fathers in the experimental group. We do not find consistent overall impacts on formal employment and earnings, although there is some unexpected evidence that some subgroups of fathers in the experimental group had *lower* wage rates and worked fewer months. These results are summarized in Table I.4.8.

Formal Employment. The first panel of Table I.4.8 shows the percentage of nonresident fathers who worked at any time (i.e., for whom UI data reported any positive earnings) in 1998 and 1999. The first row of the table shows that in 1998, 62.1 percent of fathers in the experimental group and 61.4 percent of fathers in the control group engaged in some work. Employment levels declined modestly in 1999. The regression-adjusted results suggest that there is no statistically significant difference (at conventional levels) in employment rates for either the entire sample of fathers or for any of the subgroups.[85]

The second panel of Table I.4.8 shows the mean number of employers recorded in the UI data for each father in 1998 and 1999. Fathers in both groups had an average of 1.4 employers in 1998, and 1.3 employers in 1999. There is no evidence of any significant difference in stability of employment.

We now turn to a measure of intensity of employment, the number of months worked per year. This information is not available from administrative data, so we rely on information from the survey of nonresident fathers. Panel 3 in Table I.4.8 shows that in 1998, fathers of children whose mothers were in the experimental group worked an average of 6.4 months, whereas those in the control group worked an average of 6.5 months (both averages include fathers working zero months). In 1999, experimental- and control-group fathers worked 6.6 and 7.0 months, respectively. For the entire group of fathers, there is no significant difference between the control and experimental groups in this respect. In two subgroups (fathers with children living with mothers who were in the lower tiers of the W-2 program, and those with children living with mothers with a history of higher child support), the experimental group worked about 1.2 months less than those in the control group in 1999, and this difference is statistically significant at conventional levels. We conclude that there is at most some modest evidence that experimental-group fathers work *fewer* months.

The fourth panel of Table I.4.8 shows the mean hourly wage rate earned by employed fathers in their last jobs in 1998 and 1999.[86] In 1998, fathers in the experimental group had an average wage rate of about $9.00 per hour, control-group fathers about $10.00 per hour. The lower wage rate for experimentals is only marginally significant, but significant differences reflecting similar patterns are observed for those cases in which the mother entered in a lower tier. In 1999, estimated mean wages for the experimental group rose while those for control group fathers fell so that both groups reported average wages of about $9.60. Thus, overall, there is some evidence of higher wages for fathers in the control group in 1998, but these differences are not sustained in 1999.

Total annual earnings reflect both the amount worked and the wage rate earned in employment, and hence are the most comprehensive indicator of labor market performance. In evaluating the impact of the experiment on total annual earnings, we again use administrative data from the UI system, because they are available for a much larger and more complete sample (though the information available on each

[85]We use the basic list of control variables; see the box on p. 29.

[86]Last job in 1998 includes the current job at interview.

Table I.4.8

Effects on Earnings and Program Participation of Nonresident Fathers

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Percentage of Fathers with Any Earnings (Administrative Data)										
All Nonresident Fathers[a]	13,673	62.1%	61.4%	0.7%	0.467	13,673	59.9%	58.7%	1.2%	0.257
Mother Entered in Lower Tier	8,318	60.6	60.6	0.1	0.957	8,318	58.6	57.9	0.8	0.557
No Recent AFDC History	801	66.1	63.1	3.0	0.437	801	62.5	60.3	2.2	0.577
Couple Has Order at Entry	10,207	65.0	63.3	1.7	0.152	10,207	62.9	60.9	2.0	0.101
Higher Child Support History	2,669	82.9	81.8	1.1	0.545	2,669	80.9	78.1	2.8	0.161
(2) Number of Employers in a Year among All Fathers (Administrative Data)										
All Nonresident Fathers[a]	13,673	1.4	1.4	0.0	0.716	13,673	1.3	1.3	0.0	0.910
Mother Entered in Lower Tier	8,318	1.4	1.3	0.0	0.616	8,318	1.3	1.2	0.1	0.220
No Recent AFDC History	801	1.3	1.5	-0.1	0.309	801	1.3	1.2	0.1	0.323
Couple Has Order at Entry	10,207	1.5	1.4	0.0	0.259	10,207	1.4	1.4	0.0	0.671
Higher Child Support History	2,669	1.7	1.7	0.0	0.813	2,669	1.5	1.6	-0.1	0.274
(3) Number of Months Worked among All Fathers (Survey)										
All Nonresident Fathers[b]	564	6.4	6.5	-0.1	0.819	605	6.6	7.0	-0.4	0.221
Mother Entered in Lower Tier	271	5.5	6.2	-0.7	0.144	290	5.6	6.8	-1.2	**0.012**
No Recent AFDC History	117	7.2	8.0	-0.8	0.261	135	8.8	8.3	0.4	0.441
Couple Has Order at Entry	359	6.5	7.1	-0.6	0.170	382	6.5	7.0	-0.5	0.239
Higher Child Support History	143	8.9	8.6	0.3	0.561	141	8.2	9.4	-1.2	**0.021**
(4) Average Hourly Wage Rate for Fathers Reporting Current or Last Job (Survey)										
All Nonresident Fathers[c]	457	$8.96	$10.03	-$1.07	0.094	480	$9.56	$9.62	-$0.05	0.903
Mother Entered in Lower Tier	213	8.48	10.98	-2.50	**0.017**	224	9.50	9.43	0.06	0.930
No Recent AFDC History	106	10.76	10.18	0.57	0.654	114	10.03	10.97	-0.94	0.214
Couple Has Order at Entry	292	9.03	10.56	-1.53	0.077	301	8.91	9.75	-0.85	0.064
Higher Child Support History	115	10.96	11.67	-0.71	0.689	114	10.59	11.10	-0.52	0.558

Table I.4.8, continued

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(5) Average Earnings over All Fathers (Administrative Data)										
All Nonresident Fathers[a]	13,673	$7,127	$7,156	-$29	0.886	13,673	$7,544	$7,409	$135	0.538
Mother Entered in Lower Tier	8,318	6,845	7,072	-227	0.382	8,318	7,299	7,369	-70	0.802
No Recent AFDC History	801	10,328	9,947	381	0.705	801	10,460	10,928	-468	0.656
Couple Has Order at Entry	10,207	7,476	7,425	51	0.829	10,207	7,924	7,665	259	0.311
Higher Child Support History	2,669	15,885	15,931	-46	0.945	2,669	16,401	15,939	462	0.516
(6) Percentage with Any Informal Earnings (Survey)										
All Nonresident Fathers[d]	572	28.9%	32.0%	-3.1%	0.429	603	14.2%	21.1%	-6.9%	**0.028**
Mother Entered in Lower Tier	278	31.6	33.0	-1.4	0.811	290	11.1	20.6	-9.4	**0.031**
No Recent AFDC History	120	27.6	33.5	-5.9	0.499	134	12.0	26.4	-14.4	**0.037**
Couple Has Order at Entry	364	30.1	30.4	-0.3	0.949	381	15.4	23.4	-8.0	0.050
Higher Child Support History	145	26.2	23.8	2.4	0.760	141	12.5	13.2	-0.7	0.903
(7) Percentage of Fathers Who Participated in Any Program in 1998 (Survey)										
All Nonresident Fathers[e]	569	9.4%	7.4%	2.1%	0.367					
Mother Entered in Lower Tier	277	9.6	7.0	2.6	0.439					
No Recent AFDC History	121	0.0	0.0	0.0	0.980					
Couple Has Order at Entry	360	10.9	4.2	6.7	**0.016**					
Higher Child Support History	145	2.1	1.1	1.0	0.557					
(8) Percentage of Fathers Who Participated in Any Program (W2, FS, Medicaid) in 1998 (Administrative Data)										
All Nonresident Fathers[f]	13,989	4.3%	4.8%	-0.5%	0.222	13,989	3.6%	4.1%	-0.5%	0.162
Mother Entered in Lower Tier	8,534	4.0	4.2	-0.2	0.692	8,534	3.4	4.0	-0.6	0.187
No Recent AFDC History	820	8.2	7.5	0.7	0.744	820	6.6	5.0	1.5	0.409
Couple Has Order at Entry	10,452	2.3	3.0	-0.7	0.053	10,452	1.9	2.9	-1.0	**0.004**
Higher Child Support History	2,687	2.7	3.6	-0.9	0.270	2,687	2.4	3.9	-1.5	0.067

Notes: All means are regression-adjusted, using the basic list of control variables. Probability values of 0.05 or less are shown in bold type. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33.

[a]The total sample was 14,343 cases; 354 cases are missing because there was no matching Social Security number and 316 because there was no match with UI records.

Table I.4.8, continued

[b]In 1998, the total sample was 575 cases. Missing cases include one father who is a resident parent and fathers who did not know or refused to answer about work status (6) or months worked (4). In 1999 the total sample was 608 cases. Three are missing because the father did not know or refused to answer about work status.

[d]In 1998 the total sample was 575 cases. Three cases are missing because one father is a resident parent and 2 fathers did not answer the question. In 1999 the total sample was 608; 5 fathers did not answer the question.

[e]Total sample is 575. Cases are missing because father is resident parent (1) or did not answer the question (5).

[f] Of 14,343 cases, 354 are missing because there was no matching Social Security number.

person may be less comprehensive). The fifth panel of Table I.4.8 shows mean total earnings for all fathers (including those with no earnings) reported in the UI data. Overall, fathers in the experimental and control groups earned about $7,150 in 1998. In 1999 earnings rose to $7,544 for the experimental-group fathers, and $7,409 for the control-group fathers. The difference in earnings is inconsistent across the years and is not statistically significant for all fathers or for any of the subgroups in either year. We conclude that there is no evidence that the experiment had an effect on the level of nonresident fathers' mean earnings.

Informal Earnings. As we indicated above, the experiment is expected to reduce the informal earnings of nonresident fathers, to the extent that fathers pursue informal employment in order to avoid paying child support that does not go directly to their children. Because there are no administrative data on informal earnings, we again use information from the survey.

The sixth panel of Table I.4.8 shows that 29 percent of fathers in the experimental group and 32 percent of fathers in the control group reported informal earnings in 1998. This difference is in the expected direction, but it is not statistically significant. The reported rates of informal work fell substantially for both groups by 1999, when only 14 percent of experimental-group and 21 percent of control-group fathers reported informal earnings. This 7-point difference, again in the expected direction, is large and significant for all fathers, and for most subgroups. These results for 1999 provide substantial evidence in support of our hypothesis that the experiment would result in a decrease in informal work and earnings among nonresident fathers.[87]

Use of W-2 Services. We have limited information on fathers' use of W-2 and related services. In the first wave of the survey, nonresident fathers were asked whether they received any services from state or county assistance programs, including employment search or training programs. The seventh panel of Table I.4.8 shows the percentage of fathers who responded that they had received any services. About 9 percent of fathers in the experimental group and 7 percent of fathers in the control group reported receiving any services. The difference is significant only for the subgroup of fathers who had an order at entry. Administrative data provide us with information on a more limited set of programs, but for a much larger and more representative sample. The final panel of Table I.4.8 shows the percentage participating in W-2 or receiving food stamps or Medicaid in 1998 or 1999. As expected, given that we are measuring a more restricted set of programs, overall participation rates are lower– less than 5 percent in either year. In contrast to the survey results, which suggest higher participation rates for this group, administrative data suggest that the experimental group of fathers who had an order at entry had somewhat lower rates of program participation.

Summary of Research Results

We hypothesized that nonresident fathers in the experimental group would have higher levels of formal employment and earnings, lower levels of informal employment, and would be more likely to receive services available through the W-2 program. Overall, we find little support for the first hypothesis and mixed results for an effect on program participation. But, even though our measures are limited, we find substantial evidence in support of the second hypothesis: that nonresident fathers in the experimental group have lower levels of informal employment than fathers in the control group.

[87]In supplemental analyses, we found similar effects of the experiment when we consider the percentage of fathers with informal earnings over $200 per year.

I.4.8 Experimental Effects on Fathers' Income and Economic Hardship[88]

The goal of many child support policies is to increase child support paid by nonresident fathers, so as to increase the incomes of resident mothers or to decrease governmental welfare costs. The effects of these policies on the economic status of fathers have received less attention. In this section we consider the effect of the reform on fathers' economic well-being, considering economic status *after* child support has been paid. We consider the effect of the experiment on fathers' personal income less child support paid, as recorded in administrative data. We also evaluate the effect on total family income, less child support paid, as reported in the survey of nonresident fathers. Finally, we consider a summary measure of economic hardship, also drawn from the survey. In each case we use regression-adjusted estimates to account for differences between the groups when they entered W-2.[89]

There are a number of ways the reform could affect fathers' economic status. As noted in Section I.4.1, a full pass-through may mean that a father would be more likely to pay support if his children received all the support he paid. We found that fathers were somewhat more likely to pay support in 1999. Paying more support will lead to lower economic status for fathers unless support paid is offset by additional income. As noted in Section I.4.7, fathers of children in the experimental group may have higher earnings because all payments go to the children, giving them less incentive to try to avoid child support enforcement by avoiding the formal labor market. We do not find this effect consistently in our data: fathers in the experimental group do not have higher formal employment or earnings.[90]

Our first measure is fathers' personal net income. We used administrative data for both income and child support and included Wisconsin earnings, W-2 payments, and the cash value of food stamps. This is a very limited measure of personal income, in that it does not include self-employment, earnings not covered by the unemployment insurance system, or out-of-state income. Another limitation is that we subtract the amount of child support paid but not other nondiscretionary expenditures such as taxes. We examine 13,673 fathers for whom we have Social Security numbers and whom we can thus match across data sources.

Our data do not show any recorded income for nearly two-fifths of the fathers, even before child support is subtracted. In part as a result, average incomes are very low, about $6,500. This is substantially below previous estimates of the incomes of fathers of children on AFDC (Garfinkel et al., 1998). The low income levels may reflect the possibility that the declines in the Wisconsin welfare population over the past decade have left a disproportionately disadvantaged population of participants. In part, however, the limited measure of income may understate the true total resources available to many men.

For the purposes of this section, it is worth noting that we do not expect the limitations of our measure to differentially affect the incomes of fathers whose children are subject to the full or partial pass-through. With this in mind, we now turn to a comparison of net incomes among fathers in the experimental and control groups. Table I.4.9 shows no significant difference in net incomes between the experimental and control groups overall or within any of the key subgroups.

Our second measure of economic well-being is a measure of family income, less child support paid, using survey data. This includes a father's reported earnings, other income, and his spouse's or

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[88]This section is based on the work of Maria Cancian and Daniel R. Meyer, who thank Hwa-Ok Park for excellent research assistance.

[89]We use the basic list of control variables; see the box on p. 29.

[90]We also hypothesized that fathers in the experimental group might be more likely to access public programs, again because they have less reason to try to avoid the child support enforcement system. Increased program participation might be expected to increase income. However, measures of participation in W-2, food stamps, and Medicaid show no overall effect of the experiment.

Table I.4.9

Effects on the Income and Economic Hardship of Nonresident Fathers

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Average Total Personal Income of Nonresident Fathers, Net of Child Support Paid (Administrative Data)										
All Nonresident Fathers	13,673	$6,439	$6,501	-$62	0.749	13,673	$6,646	$6,573	$74	0.721
Mother Entered in Lower Tier	8,318	6,221	6,483	-262	0.286	8,318	6,452	6,600	-149	0.573
No Recent AFDC History	801	9,143	9,141	3	0.998	801	8,880	9,549	-669	0.503
Couple Has Order at Entry	10,207	6,552	6,543	9	0.968	10,207	6,834	6,671	163	0.495
Higher Child Support History	2,669	13,623	13,785	-162	0.797	2,669	14,069	13,796	273	0.687
(2) Average Total Family Income of Nonresident Fathers, Net of Child Support Paid (Survey)										
All Nonresident Fathers	431	$14,084	$15,406	-$1,322	0.354	480	$17,730	$20,637	-$2,906	0.115
Mother Entered in Lower Tier	199	10,906	14,827	-3,922	**0.050**	218	14,808	17,452	-2,643	0.210
No Recent AFDC History	93	25,810	22,758	3,052	0.474	114	26,105	25,034	1,071	0.755
Couple Has Order at Entry	274	13,984	15,689	-1,705	0.315	301	16,123	20,351	-4,228	0.092
Higher Child Support History	114	21,292	21,688	-396	0.895	117	24,815	22,648	2,167	0.508
(3) Percentage of Nonresident Fathers with a Food, Shelter, or Telephone Hardship (Survey)										
All Nonresident Fathers	569	46.7%	45.8%	0.9%	0.831	601	43.8%	43.8%	0.0%	0.993
Mother Entered in Lower Tier	275	47.5	47.3	0.2	0.980	287	50.4	42.5	7.9	0.192
No Recent AFDC History	121	30.0	43.3	-13.3	0.142	136	26.0	41.5	-15.6	0.058
Couple Has Order at Entry	361	51.2	44.4	6.8	0.211	378	43.0	41.5	1.5	0.770
Higher Child Support History	145	33.1	35.1	-2.1	0.804	142	41.0	38.3	2.8	0.746

Note: All means are regression-adjusted, using the basic list of control variables. Probability values of 0.05 or less are shown in bold type. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33.

partner's earnings and other income. We then subtract the amount of child support he reported paying. Note that this measure is limited in that we subtract the amount of child support paid but not other nondiscretionary expenditures such as taxes. We examine those fathers for whom we have full income information (431 in 1998 and 480 in 1999).[91] Although we use data weighted to account for nonresponse, we may still be missing fathers with the lowest incomes.

Fathers' earnings are substantially higher in the survey than in the administrative data, as are the other sources of income, all contributing to substantially higher overall income. For example, in the survey 25–30 percent of fathers have net family income under $5,000, compared to over 60 percent of fathers in the administrative data with net personal income under $5,000. Family incomes are nonetheless still quite low, with a median income of about $11,500 in 1998 and around $15,000 in 1999; means are around $15,000 in 1998 and $19,000 in 1999. The second panel of Table I.4.9 considers experimental-control group differences. There is no significant difference in net family income between the groups as a whole. No key subgroup shows a statistically significant effect in both years.

Finally, we examine economic hardship. As in our analysis for mothers, we consider three measures of hardship: food (often or sometimes not enough to eat), shelter (gas or electricity turned off, moved in with others, homeless, in a shelter), and telephone (no phone or phone disconnected), all based on the survey. We examine 569 fathers in 1998 and 601 in 1999.

The levels of economic hardship for fathers are somewhat lower than the levels for mothers: for example, 11 percent of fathers reported a food hardship in 1998 (compared to 18 percent of mothers) and 29 percent reported a shelter hardship in 1998 (compared to 38 percent of mothers). The third panel of Table I.4.9 compares the experimental and control groups in terms of whether they reported any of the three types of hardship. Because we did not find differences in total income, it seems unlikely that there will be differences in the level of economic hardship. As anticipated, the percentages of experimental and control groups that report having an economic hardship are similar. None of the other subgroups show statistically significant differences in both years.

I.4.9 Experimental Effects on the Nonresident Father's Social and Economic Involvement with the Child[92]

This reform is designed to increase nonresident parents' financial ties to their children by passing through the full amount of child support collected on behalf of the family. Both theory and most research suggest that when nonresident parents pay more child support they will also spend more time with their children (Furstenberg et al., 1983; Seltzer, Schaeffer, and Charng, 1989; Seltzer, 1991; McLanahan et al., 1994; but see Veum, 1993). Nonresident parents who know that their children receive all of the child support paid have more incentive to spend time with the children and to pay attention to how the resident parent spends the child support money (Weiss and Willis, 1985; Braver et al., 1993). When all child support paid on a family's behalf is passed through, nonresident parents may see themselves, and be seen by the resident parent, as better parents. As a result, nonresident parents may spend more time with their

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[91]We used several imputation rules when respondents refused to answer a question or stated they did not know. In general these rules are similar to those used for mothers, as detailed in Volume II, Chapter 6. Those who stated they paid child support but did not provide an amount were treated as missing. We report total income only for those in which amounts are present for all relevant sources after we have completed imputations. These rules result in our excluding 144 fathers in 1998 and 128 fathers in 1999 for whom information about income was incomplete.

[92]This section is based on the work of Judith A. Seltzer and Nora Cate Schaeffer. Further information on the nonresident father's involvement with his children can be found in Volume II, Chapter 8. A preliminary version of this section was presented at the CSDE National Advisory Board Meeting, November 2000. The authors thank participants and especially the discussants, Vivian Gadsden and Virginia Knox, for helpful comments. The authors also acknowledge research assistance from Sheri Meland, Shauna Morimoto, Christine Schwartz, and Sara Wakefield. Pat Brown, Steven Cook, and Margaret Krecker provided expert help with the data.

children and try to play a bigger role in making decisions about the children's lives. Resident parents who receive all of the child support paid for their children may see the nonresident parents' claims to access and influence as more legitimate and may therefore facilitate nonresident parents' greater involvement with their children.

Nonresident parents whose formal child support payments go, in part, to the state, as the payments did under AFDC rules and now do for the control group, may not feel as though they are supporting their children and, as a result, may not think that they are "good" parents (Johnson and Doolittle, 1998). Other anecdotal and ethnographic evidence suggests that parents may cooperate with each other to enable the nonresident parent to avoid the formal child support system (Waller and Plotnick, 1999). In such families, the nonresident parent makes informal contributions to support his children and the resident parent provides access to the children and helps the nonresident parent avoid formal child support enforcement.

Although the greater incentive to make formal payments under the experiment may increase informal economic contributions that occur when nonresident parents spend time with children, the likely effect of the demonstration on informal transfers is ambiguous. Nonresident parents may substitute formal for informal payments when formal payments are transferred in full to the resident parents and children. Given the limited economic resources of nonresident parents whose former partners are eligible to participate in W-2 Community Service Jobs and W-2 Transitions positions, the total resources available for child support may restrict nonresident parents' ability to both fulfill formal child support responsibilities and make informal cash transfers and in-kind contributions (e.g., providing clothing, diapers, or food) (but see Teachman (1991) who shows that nonresident parents who pay formal child support are more likely to make informal contributions).

Even if nonresident parents do not stop making informal payments when they increase formal payments, the financial value of informal transfers may decline under the experiment. The MDRC Parents' Fair Share (PFS) program evaluation found that stronger enforcement of formal child support obligations did not affect whether fathers provided informal transfers. However, the PFS did find a small reduction in the value of informal transfers (Knox and Redcross, 2000). This suggests some substitution of amounts of formal for informal child support.

Children's exposure to parental conflict may increase if parents who would otherwise avoid each other are drawn together by the W-2-child support reform. Parents who separate are likely to disagree about important issues, including how to raise their children. In addition, when nonresident parents seek greater access to their children, parents may disagree about scheduling visits and what nonresident parents and children do when they are together. Thus, any benefits to children of increased nonresident parents' involvement must be balanced against the potential costs of children's greater exposure to conflict. But if resident parents adopt a more positive image of nonresident parents, as a result of their greater involvement in child rearing, the reform may reduce or have no effect on parental conflict. The experiment may also reduce conflict between parents by removing an important topic of disagreement—the merits of being in the formal child support system.

We address three broad hypotheses about the effects of the experiment on nonresident fathers' involvement with children, based on previous research and, where possible, theory about nonresident fathers' participation in child rearing.

1. Compared to nonresident fathers in the control group, nonresident fathers in the experimental group will participate more in children's lives.

2. Nonresident and resident parents in the experimental group will experience more conflict about child rearing than parents in the control group.

3. Compared to nonresident fathers in the control group, those in the experimental group will make more types of informal financial or in-kind contributions to children. We describe differences

between the experimental and control groups in the value of these contributions but are agnostic about the direction of the experimental effect on the value of informal transfers.

We use data from the survey to evaluate these hypotheses. Data on these outcomes are not available through administrative records. There are no external criteria with which to evaluate the reports about fathers' social or economic involvement with children or reports on conflict between parents. Previous methodological studies show that nonresident fathers report higher levels of involvement—in particular, higher child support payments—than resident mothers report for them (Schaeffer, Seltzer, and Dykema, 1998; Schaeffer, Seltzer, and Klawitter, 1991; Seltzer and Brandreth, 1995). Findings reported elsewhere in this report are consistent with this (see Volume II, Chapter 8). We conduct the analysis in this chapter using mothers' reports because the coverage of the survey sample is much more complete than for fathers.[93] All questions on fathers' time with children ask about the randomly selected focal child. Questions on fathers' informal and in-kind transfers ask about contributions on behalf of the randomly selected focal child and that child's biological siblings. The analysis uses data from resident mothers interviewed in both waves of the survey.

The tables that follow show experimental- and control-group levels and differences adjusted for the basic list of control variables.[94] Parameters for dichotomous outcomes come from probit regressions. Parameters for count variables, such as the number of days of contact or types of informal transfers, come from ordinary least squares regressions. We use the criterion $p \leq .05$ to evaluate the statistical significance of adjusted differences between the experimental and control groups. Because of our small sample sizes we also discuss results significant at the .10 level if these are part of a consistent pattern. All estimates use weights to adjust for unequal probabilities of sample selection and differential nonresponse.

In addition to our discussion about overall comparisons of the experimental and control groups and comparisons in the key subgroups examined in all domains, we also examine whether the effects of the experiment differ for families with young children, because families with young children may have more potential to change their behavior. Parents with a newborn have had little chance to establish habits of interaction, and so may respond more readily to changes in fathers' incentive to pay formal support. Results from other program evaluations suggest greater program effects on paternal involvement when parents have young children (Knox and Redcross, 2000) and on visitation problems when parents are not yet entrenched in their positions (Pearson and Thoennes, 1998).[95]

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[93]Previous methodological work demonstrates that differences between estimates based on data from mothers, compared to fathers, are due to differences in rates of survey participation (fathers who are more likely to be involved with their children are much more likely to participate), as well as to reporting differences that reflect differences in parents' knowledge about the outcome and parents' desire to provide socially approved responses. (See, for example, Schaeffer, Seltzer, and Dykema, 1998; Schaeffer, Seltzer, and Klawitter, 1991).

Although fathers are uniquely positioned to know about *all* the money they spend to support their children, in both formal child support and other ways, many of those transfers are given to the mother to spend on the children or are provided to the children in the mother's presence or in ways that she can observe or routinely learn about. The transfers that mothers are least likely to know about are those that occur when the children spend time with their father, particularly when they are with their father for extended periods of time. We focus on transfers that mothers are likely to know about, such as gifts, medical expenses other than insurance, rent, and so forth. Although mothers may have less knowledge of some transfers than do fathers, studies of error in survey reports about formal child support payments support speculation that, for the transfers they do know about, mothers' reports will have less response error than fathers' reports. We expect that the superior coverage provided by the sample of mothers will reduce error in our estimates due to nonparticipation.

[94]See the box on p. 29.

[95]Previous research examines differences in family behavior by child's age or by time since parents separated, but few studies can examine both child's age and time since separation simultaneously. We examine experimental effects for families with young children instead of time since separation because we think that the demands of rearing a very young child provide an important setting in which to evaluate program effects. Our data

Children's Residence with Both Parents

Most children in separated families live with their mothers (Grall, 2000), but the father may also at times live with the family. To evaluate the effect of the experiment on father's coresidence with the mother and child, we examine two aspects of living arrangements: whether the father lived with the mother and child for the entire year and whether the father lived with the mother and child for at least seven months of the year. Table I.4.10, Panels 1 and 2 show that there are no differences overall or within most subgroups in the percentage of families in which the father lived with the mother and child for all or most of the year in 1998 or in 1999. In both years, between 5 and 7 percent of mothers reported that the child's father lived with them for seven or more months during the year (Table I.4.10, Panel 2).[96] Fathers in over two-thirds of these families lived with the mother and child for the full year (compare the first rows of Panels 1 and 2). The only subgroup difference in the first two panels that approaches statistical significance is that among mothers without a history of AFDC receipt in 1998. For this subgroup, about 11 percent of fathers in the control group coresided for at least seven months, compared to only 6 percent of those in the experimental group ($p \leq .10$).

Very frequent contact between nonresident fathers and children and coresidence may provide similar benefits to children. In both instances, fathers and children spend time together. Table I.4.10, Panel 3, shows that roughly one-third of fathers in this sample spend substantial time with their children. There are no differences between the experimental and control groups at either time for the full sample, but two of the subgroup differences approach statistical significance in 1998. Compared to those in the experimental group, slightly higher percentages of fathers in the control group spent substantial time with their children among the following subgroups: mothers in the lower tiers ($p \leq .10$) and those with a formal child support order at entry into W-2 ($p \leq .10$). These control-group advantages did not persist in 1999. In fact, in 1999, among those with a history of higher child support, those in the experimental group had higher levels of contact than those in the control group ($p \leq .10$).

Nonresident Fathers' Time with Children

We examine differences between the experimental and control groups conditional on living arrangements, looking only at those cases in which the focal child lived with the mother alone (i.e., without the father) for at least 6 months of the reference period.[97] Although, strictly speaking, the differences between the experimental and control groups in the next several tables do not show effects of the experiment because they are conditional on living arrangement, Table I.4.10 shows no consistent experimental impact on father's coresidence with the mother and child. In addition, the results reported below are consistent with those we obtain when we define the sample more broadly to include families in which the father lived with the mother and child most, but not all, of the year.[98]

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are more accurate and complete for children's ages than for time since separation, particularly for families who bore children outside of marriage, the majority of this population.

[96]As noted above, all percentages are adjusted for: rate of assignment to the experiment, mother's child support history at entry into W-2 (except for E-C differences within this subgroup), and mother's age and race (see box on p. 29). Volume II, Chapter 8 shows unadjusted levels of father's involvement and parents' interaction with each other.

[97]The survey design only obtains information on fathers' visits with children, conflict between parents, and informal transfers for families in which the mother is the resident parent and the father lived apart from the mother and child for at least part of the year.

[98]We report results using the slightly less inclusive sample because we believe that parents' reports about their children's contact with their fathers are more reliable if the parents were separated for most of the year than if the parents were together most of the year. We base this assessment on field reports from interviewers and on a preliminary examination of the data.

Table I.4.10
Effects on Living Arrangements

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Percentage of Families in Which Mother, Father and Focal Child Lived Together All Year										
All Resident Mothers	1,943	4.4%	3.7%	0.7%	0.405	1,935	5.5%	5.0%	0.5%	0.631
Mother Entered in Lower Tier	1,024	3.7	2.7	0.9	0.390	1,017	5.3	4.0	1.3	0.325
No Recent AFDC History	399	5.3	5.2	0.1	0.966	400	5.6	8.6	-3.0	0.228
Mother Has Order at Entry	1,121	2.4	2.7	-0.3	0.747	1,120	3.6	3.8	-0.2	0.868
Higher Child Support History	371	2.1	2.6	-0.5	0.734	370	2.2	2.5	-0.2	0.849
Focal Child Age 2 or Younger in 1998	547	9.0	8.2	0.8	0.753	541	9.5	12.3	-2.7	0.311
(2) Percentage of Families in Which Focal Child Lived with Both Parents for Seven or More Months During the Year										
All Resident Mothers	1,943	5.7%	5.3%	0.4%	0.729	1,935	7.0%	6.5%	0.4%	0.698
Mother Entered in Lower Tier	1,024	5.2	4.3	0.9	0.495	1,017	6.9	5.8	1.1	0.480
No Recent AFDC History	399	6.0	11.2	-5.2	0.064	400	8.7	11.7	-3.0	0.321
Mother Has Order at Entry	1,121	2.8	3.9	-1.1	0.295	1,120	4.7	5.2	-0.5	0.720
Higher Child Support History	371	2.1	3.1	-1.1	0.451	370	2.7	2.7	-0.1	0.962
Focal Child Age 2 or Younger in 1998	547	12.4	12.9	-0.5	0.852	541	12.3	15.8	-3.5	0.246
(3) Percentage of Families in Which Focal Child Lived with Both Parents for Any Length of Time or Had Frequent Contact[a] with Father										
All Resident Mothers	1,943	35.5%	37.6%	-2.2%	0.321	1,935	35.5%	35.1%	0.4%	0.853
Mother Entered in Lower Tier	1,024	33.3	38.8	-5.5	0.068	1,017	35.1	36.5	-1.4	0.652
No Recent AFDC History	399	44.7	50.1	-5.4	0.287	400	40.9	43.4	-2.5	0.615
Mother Has Order at Entry	1,121	34.4	39.6	-5.2	0.072	1,120	34.6	34.3	0.2	0.933
Higher Child Support History	371	41.2	40.6	0.6	0.913	370	43.3	34.1	9.2	0.072
Focal Child Age 2 or Younger in 1998	547	54.4	52.3	2.1	0.624	541	48.5	49.0	-0.4	0.920

Notes: Table is based on cases in which mother is the primary resident parent or mother and father together are primary resident parents for focal child. Table includes only mothers interviewed in both 1998 and 1999. At each time, table deletes cases for which mother reported that focal child or father had died. The panel for 1998 also excludes one case in which there was an instrument error. At each time, cases that are missing on the dependent variable are deleted from the analysis. All means are regression-adjusted using a probit model and the basic list of control variables. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33.

[a]Frequent contact is defined as 52 or more days during the time that focal child and father lived apart.

Table I.4.11, Panel 1, shows how much fathers saw the focal child when they were living in separate households. There are no differences between the experimental and control groups either for the full sample or for most of the subgroups in both 1998 and 1999. The adjusted number of contact days declines from 55–60 days in 1998 to about 49 days in 1999 for both experimental- and control-group families.[99] The only subgroup differences that approach statistical significance are in 1998 among those with a history of higher child support before the experiment and among families with a young focal child ($p \leq .10$); in both cases there is an experimental-group advantage. Among families with a history of higher child support, nonresident fathers in the experimental group spent 19 days more, nearly three weeks more per year, with the focal child than fathers in the control group. The difference is slightly smaller in 1999, but does not achieve statistical significance.

The data in this panel of the table suggest that passing through all of the formal child support that a nonresident father pays may increase contact when that support is sufficient to make a difference in the mother's budget. Mothers in the control group may know that the child's father is paying formal support, but most of his payments do not come to her or the child. When the father's contributions are passed on to the mother this may increase his motivation to spend time with the child and increase the mother's perception of the legitimacy of the father's claim to the child's time.

Compared to nonresident fathers in the control group, fathers in the experimental group were no more likely to look after children so the mother could work, go to school, or look for work. Table I.4.11, Panel 2, shows that roughly one-fifth to one-quarter of fathers in both groups in each year looked after their child so the mother could work, after adjusting statistically for pre-experiment characteristics. This outcome includes time that might be considered as providing child care, as when fathers spent time looking after very young children, as well as time that fathers spent supervising older children's activities so the mother could work. Not surprisingly, fathers were more likely to look after young children—between one-third and two-fifths of fathers of focal children aged two or younger provided this care, regardless of experimental- or control-group status.[100]

Quality of the Relationship between Parents, Mothers' Reports

We expected that mothers in families with the full pass-through would have more favorable attitudes toward the nonresident father than if his formal child support payments were largely withheld by the state. However, Table I.4.12, Panel 1, shows that there are no overall differences between the experimental and control groups in whether the mother thought the father did a good job as a parent. Fewer than one-third of mothers at either time reported that the father did a good job, after we adjusted for compositional differences between the experimental and control groups. As in the earlier analysis of the number of days of contact, the experimental effect in 1998 among families with a history of higher formal child support approaches statistical significance ($p \leq .10$). In this group, approximately 40 percent of mothers in the experimental group described the father as doing a good job, compared to only 30 percent of those in the control group. The difference between the experimental and control groups in 1999 is about the same magnitude as in 1998, but is significant at the .05 level. In 1999 there is also an experimental effect among those who had a formal child support order at entry, but there is no effect for this group in 1998. For families with a history of higher child support payments, passing through full instead of partial payments may make the most difference in the amount that the resident mother receives. If mothers believe that it is the father's job to provide financial support, they are likely to evaluate him more favorably when they receive his financial contributions in full. Lin and McLanahan (2000) show

[99]Appendix I.4.2 includes a systematic examination of whether or not the experiment affects short-term change in paternal involvement.

[100]When the sample is defined slightly more broadly to include families in which the father lived with the mother and child for most but not all of the reference year, the difference between the experimental and control groups is marginally significant in 1999 ($p \leq .10$) among mothers who entered in the lower tier for both panels of Table I.4.11. We do not interpret this effect because it does not appear to be robust.

Table I.4.11

Effects on Father's Contact with the Focal Child

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Mean Number of Days Father Saw Focal Child During the Time They Lived Apart										
All Resident Mothers	1,759	59.7	55.2	4.4	0.332	1,724	48.4	49.7	-1.3	0.766
Mother Entered in Lower Tier	933	55.6	59.3	-3.7	0.556	908	45.7	54.9	-9.2	0.129
No Recent AFDC History	345	77.0	76.2	0.8	0.946	339	51.9	47.1	4.7	0.602
Mother Has Order at Entry	1,047	62.8	58.6	4.2	0.479	1,033	50.4	49.8	0.7	0.900
Higher Child Support History	345	71.7	53.0	18.7	0.076	340	63.7	49.4	14.3	0.141
Focal Child Age 2 or Younger in 1998	462	93.7	76.2	17.4	0.097	450	63.8	60.1	3.7	0.694
(2) Percentage of Families in Which Father Looked after Focal Child So That Mother Could Work, Go to School, or Look for Work										
All Resident Mothers	1,792	26.9%	26.8%	0.2%	0.938	1,739	22.6%	22.9%	-0.3%	0.877
Mother Entered in Lower Tier	950	24.0	28.1	-4.1	0.154	914	20.3	24.1	-3.8	0.170
No Recent AFDC History	353	30.7	33.2	-2.4	0.632	342	28.4	22.2	6.3	0.192
Mother Has Order at Entry	1,067	28.3	27.6	0.7	0.792	1,044	22.7	22.5	0.3	0.923
Higher Child Support History	352	30.7	26.8	3.9	0.435	347	22.6	22.3	0.3	0.940
Focal Child Age 2 or Younger in 1998	468	42.0	41.1	0.9	0.845	451	36.1	36.4	-0.3	0.954

Notes: Table is based on cases in which mother is the primary resident parent for focal child. Table includes only mothers interviewed in both 1998 and 1999. At each time, table deletes cases for which mother reported that focal child or father had died. The panel for 1998 also excludes one case in which there was an instrument error. At each time, cases that are missing on the dependent variable are deleted from the analysis. All means are regression-adjusted using the basic list of control variables. In Panels 1 and 3, percentages are adjusted using a probit model. In Panel 2, means are adjusted using Ordinary Least Squares regression. Probability values of 0.05 or less are shown in bold type. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33.

Table I.4.12
Effects on Parental Interaction

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Percentage of Mothers Who Think That Focal Child's Father Does a Good Job as a Parent										
All Resident Mothers	1,818	30.6%	30.3%	0.4%	0.860	1,789	28.1%	25.4%	2.7%	0.204
Mother Entered in Lower Tier	964	29.4	32.0	-2.6	0.381	945	27.7	25.9	1.7	0.553
No Recent AFDC History	358	38.5	33.5	5.0	0.332	352	33.2	28.6	4.6	0.351
Mother Has Order at Entry	1,082	31.6	31.6	0.0	0.988	1,064	30.2	24.6	5.7	**0.040**
Higher Child Support History	357	40.4	30.4	9.9	0.053	352	35.5	25.6	9.8	**0.047**
Focal Child Age 2 or Younger in 1998	477	39.1	36.9	2.1	0.634	465	35.0	32.6	2.5	0.580
(2) Percentage of Mothers Who Report High Conflict on at Least One Child-Rearing Issue										
All Resident Mothers	1,818	38.3%	42.3%	-3.9%	0.090	1,789	36.8%	39.5%	-2.7%	0.242
Mother Entered in Lower Tier	964	36.6	40.7	-4.1	0.198	945	34.3	37.4	-3.1	0.319
No Recent AFDC History	358	44.8	45.1	-0.2	0.963	352	37.0	39.8	-2.8	0.587
Mother Has Order at Entry	1,082	38.7	46.9	-8.2	**0.007**	1,064	37.6	44.9	-7.3	**0.017**
Higher Child Support History	357	45.4	51.6	-6.2	0.246	352	46.8	53.7	-6.9	0.204
Focal Child Age 2 or Younger in 1998	477	47.3	41.4	5.9	0.202	465	41.5	35.6	5.8	0.200

Notes: Table is based on cases in which mother is the primary resident parent for focal child. Table includes only mothers interviewed in both 1998 and 1999. At each time, table deletes cases for which mother reported that focal child or father had died. The panel for 1998 also excludes one case in which there was an instrument error. At each time, cases that are missing on the dependent variable are deleted from the analysis. All means are regression-adjusted using a probit model and the basic list of control variables. Probability values of 0.05 or less are shown in bold type. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33.

that most unmarried parents believe that fathers should provide financial support even if the father and mother are not involved with each other.

If fathers become more involved in children's lives or invested in child-rearing decisions when their formal child support is all passed on to the children's mother, the child support demonstration may increase conflict between parents. Disagreements between parents may be good for children if the disagreements indicate parents' concern about children and demonstrate productive ways to deal with conflict. However, strong disagreements that involve children or are difficult to resolve are more harmful to children's adjustment. Table I.4.12, Panel 2, shows the percentage of mothers who reported that the parents had intense conflict.[101] Compared to those in the control group, mothers in the experimental group were slightly less likely to report high levels of conflict: 38.3 percent in the experimental group versus 42.3 percent in the control group, in 1998 (significant at the .10 level). The experimental effect is in the same direction but is smaller and not statistically significant in 1999. There are no subgroup differences, except among those with a formal child support order at entry into W-2; for this subgroup, the difference between experimental and control groups is more pronounced in 1998 and 1999 than for the full sample.[102] Compared to parents with a child support order, mothers and nonresident fathers without an order may disagree about whether to enter the formal child support system and about fathers' informal contributions to support the children. Once parents are in the formal system, the pass-through policy affects how much child support mothers receive. Those in the control group, who receive less formal support, on average (see Table I.4.2), may continue to disagree about how much nonresident fathers should contribute informally. Our evidence suggests that the full pass-through may reduce the likelihood of intense conflict between parents, especially among families who are already in the formal child support system.

Informal and In-Kind Transfers

We examine whether or not mothers reported receiving any in-kind contributions or cash, the number of types of informal transfers, and the dollar value of informal transfers. Table I.4.13, Panel 1, shows that almost half of resident mothers reported that they or the children received in-kind contributions or cash from the children's father in 1998, taking account of pre-experiment characteristics. Almost as high a percentage reported that they received informal transfers in 1999. There is no overall difference between the experimental and control groups in whether the family received informal transfers at either time, nor are there significant differences in the key subgroups, although there is a slight experimental-group advantage in 1999, marginally statistically significant ($p \leq .10$), among mothers without a history of AFDC receipt.

Fathers make few types of informal transfers, regardless of their experimental-group status. Table I.4.13, Panel 2, shows the mean number of types of transfers mothers received. The adjusted mean in 1998 is about 1.4, and it is slightly lower in 1999. There are no overall differences between the experimental and control groups in the number of types of transfers, and there is only one subgroup difference that approaches statistical significance ($p \leq .10$). In 1998, among mothers without a recent history of AFDC, fathers in the experimental group provide more types of transfers than those in the

[101]The response categories for these items differ at the two time periods. At time 1 high conflict is defined as "pretty much" or "a great deal" of conflict. At time 2 high conflict is "pretty much," "a great deal," or "a very great deal" of conflict. The highest response category at time 1 was "a great deal."

[102]When we examine the relationships in Table I.4.12, Panel 2 using our slightly larger sample, the results are consistent with those presented here, except that the experimental effect for families with a young focal child are marginally significant ($p \leq .10$) in 1999.

Table I.4.13

Effects on Informal Transfers

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Percentage of Families in Which There Were Any Informal Transfers[a]										
All Resident Mothers	1,808	47.7%	47.9%	-0.3%	0.912	1,787	43.8%	41.9%	1.9%	0.415
Mother Entered in Lower Tier	958	45.0	48.8	-3.7	0.254	944	41.7	43.1	-1.3	0.685
No Recent AFDC History	357	59.0	50.7	8.3	0.117	351	52.3	43.1	9.2	0.086
Mother Has Order at Entry	1,075	51.5	51.6	-0.1	0.964	1,063	46.2	44.8	1.3	0.672
Higher Child Support History	354	60.7	56.2	4.5	0.397	351	56.3	55.6	0.7	0.894
Focal Child Age 2 or Younger in 1998	477	61.7	55.7	6.0	0.186	464	50.9	46.6	4.3	0.362
(2) Mean Number of Different Types of Informal Transfers										
All Resident Mothers	1,808	1.4	1.4	0.0	0.804	1,787	1.3	1.1	0.1	0.134
Mother Entered in Lower Tier	958	1.3	1.5	-0.2	0.105	944	1.2	1.2	0.0	0.807
No Recent AFDC History	357	1.8	1.5	0.4	0.077	351	1.6	1.2	0.3	0.111
Mother Has Order at Entry	1,075	1.4	1.4	-0.1	0.610	1,063	1.2	1.1	0.1	0.219
Higher Child Support History	354	1.7	1.4	0.2	0.233	351	1.4	1.3	0.0	0.902
Focal Child Age 2 or Younger in 1998	477	2.0	1.8	0.2	0.364	464	1.7	1.5	0.2	0.214
(3) Percentage of Families for Which the Total Value of Informal Transfers Was $500 or Greater										
All Resident Mothers	1,808	10.9%	8.9%	2.0%	0.160	1,787	10.8%	7.0%	3.8%	**0.005**
Mother Entered in Lower Tier	958	11.4	9.5	1.9	0.348	944	12.1	7.0	5.1	**0.008**
No Recent AFDC History	357	12.1	9.8	2.3	0.492	351	13.3	12.3	1.0	0.775
Mother Has Order at Entry	1,075	11.5	9.1	2.4	0.197	1,063	10.3	5.7	4.6	**0.006**
Higher Child Support History	354	14.1	8.2	5.8	0.088	351	12.6	6.1	6.4	**0.042**
Focal Child Age 2 or Younger in 1998	477	14.7	13.6	1.1	0.733	464	12.9	12.2	0.8	0.807

Notes: There were seven types of informal transfers: diapers, clothes or shoes; birthday or other holiday gifts; food or household groceries; money for child care or school expenses; money for medical expenses; money for rent; other money to spend on child. Table is based on cases in which mother is the primary resident parent for focal child. Table includes only mothers interviewed in both 1998 and 1999. At each time, table deletes cases for which mother reported that focal child or father had died. The panel for 1998 also excludes one case in which there was an instrument error. At each time, cases that are missing on the dependent variable are deleted from the analysis. For Panels 1 and 3, percentages are adjusted using a probit model. All means are regression-adjusted, using the basic list of control variables. For Panel 2, means are adjusted using Ordinary Least Squares regression. Probability values of 0.05 or less are shown in bold type. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33.

[a] Mother reported at least one of the types of informal transfers defined in the table notes.

control group, 1.8 compared to 1.5, respectively. The difference is in the same direction, but somewhat smaller and not statistically significant in 1999.[103]

Informal transfers may be particularly important for children's material well-being when the transfers are economically valuable. In Table I.4.13, Panel 3, we examine whether mothers received transfers that they evaluated as worth more than $500. Families without any informal transfers are defined as having received transfers worth $0. Although there are no statistically significant differences overall between the experimental and control groups in whether or not mothers received informal transfers, this table shows that in 1999, mothers in the experimental group were more likely to receive transfers worth at least $500 than those in the control group, 10.8 percent compared to 7.0 percent, respectively. The difference between the experimental and control groups is not statistically significant in 1998, but it increases and becomes significant at a conventional level in 1999. The experimental effect suggests that fathers do not substitute formal for informal contributions. Our analysis does not identify the mechanisms that explain more valuable transfers among those in the experimental group. It is possible that resident mothers who receive the full pass-through learn about fathers' potential to pay support and, as a result, increase their requests for more valuable informal contributions as well. Alternatively, fathers whose formal child support payments are all passed through may be motivated to learn more about how the mother spends the support payments. If so, nonresident fathers in the experimental group may make more economically valuable in-kind contributions to help fulfill children's needs.

The table also shows several subgroup differences that are either statistically significant or approach significance in at least one of the years. The experimental impact is largest among those who have a history of higher child support, an experimental advantage of about 6 percentage points in both 1998 ($p \leq .10$) and 1999 ($p \leq .05$). In 1999 the experimental effect is also significant for mothers in the lower tiers and for those with a formal child support order at entry. Thus the pass-through has a large effect on the value of informal transfers among those with the most potential for an increase in payments as a result of the policy. This may be reassuring to those who fear that a full pass-through policy would hurt children's economic well-being by raising formal child support at the expense of informal or in-kind contributions.

Summary of Findings

Our findings show a general absence of any experimental effect on fathers' social involvement with children, where social involvement is measured by the amount and type of time fathers spend with children. There are also few consistent subgroup differences in fathers' time with children, including fathers' coresidence with the mother and child, frequent contact through coresidence or weekly visits when the father and child were living apart, or fathers' supervision of children while mothers went to work. However, among families in which fathers paid higher amounts of child support at entry, mothers are more likely to evaluate the father as being a good parent. The data also provide a hint that parents have less intense conflict under the full pass-through than in the control group, at least according to mothers' reports, but this experimental effect is small and only statistically significant at the more

[103]The magnitude and pattern of statistical significance for the experimental impact within some subgroups varies somewhat when the sample is expanded to include families in which the fathers lived most, but not all, of the year with the mother and child. Using this more inclusive sample the experimental advantage among those without an AFDC history is considerably smaller than that shown in Table I.4.13, Panel 2 and not statistically significant in 1998; but the experimental advantage is larger and approaches significance in 1999 (adjusted E-C difference = 0.4, $p \leq .10$). The data for the more inclusive sample also show a slight control-group advantage in 1998 among families in lower tiers. Mothers in the control group receive 1.5 types of transfers, compared to 1.3 types in the experimental group. This difference is significant at the $p \leq .10$ level, but in 1999 mothers in lower tiers received about the same number of types of transfers, about 1.2 for each group (not shown). Because these patterns are inconsistent we do not interpret them as experimental effects.

generous level of $p \leq .10$ in 1998. Families with a formal child support order at entry also report less conflict when they receive the full pass-through than when they are in the control group.

There is no evidence that mothers in the experimental group are more likely to receive informal transfers than those in the control group. There is also no consistent impact of the experiment on the number of different types of informal transfers (using mothers' reports). However, the financial value of informal transfers is somewhat higher in the experimental group than in the control group. Almost all of the subgroups in the analysis show an experimental effect on the value of transfers, although we have noted that the experimental effect is only statistically significant for some of the subgroups. In particular, the experimental impact on the financial value of transfers is consistently larger among families with a history of higher child support payments.

Although we expected the pass-through experiment to have a greater effect on families of very young children than it did on families with children of all ages, our results do not support this expectation. It is possible that families of very young children are not yet sufficiently involved in the formal child support system for the pass-through policy to affect them. Once legal paternity and a formal child support order are established, there is greater potential for the pass-through to affect the amount of formal child support resident mothers receive. Parents may change their behavior when they learn about the pass-through and observe its effects on formal child support receipts.

The subgroup analyses suggest that when there are subgroup differences, the experimental effect is concentrated in segments of the sample with closer ties to the formal child support system (those with child support orders at entry) and families in which fathers had somewhat more economic resources when the mother entered W-2, as indicated by a history of higher child support payments. This pattern is consistent with the findings described in Section I.4.1, which show that the experiment increased the receipt and amounts of formal payments.

Finally, our findings are generally robust across samples that vary in their inclusion of families in which the father lived with the mother and child for most, but not all, of the reference year. The analysis shows that there is no experimental effect on fathers' coresidence with the mother and child. Note, however, that this aspect of the evaluation looks only at the experiences of children who live with their mothers. Because this is, by far, the most common living arrangement for children eligible for child support, there would have been too few families in the survey sample to support an investigation of experimental effects among children who live with their father alone or in other types of households.

I.4.10 Evaluation of Experimental Impacts on Child Well-Being[104]

In this final section of our report on experimental impacts, we move from a discussion of activities and outcomes for mothers and fathers, to a focus on children themselves. As mentioned above, it is particularly difficult to detect impacts on child well-being, since this depends on the experiment having a direct effect on child support paid and received, which in turn must affect the context or content of parenting to an extent that it has a sufficiently large and immediate impact on child well-being to be captured by our measures.

In this section we use the survey data to explore the well-being of children. We examine the impact of the experiment on children's health and school performance, and on mothers' parenting practices. We hypothesize that the impact of the pass-through may be to increase parental involvement, thereby increasing time spent with children. This increase in parental time will be recorded as increased

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[104]This section is based on the work of Arthur Reynolds and Barbara Wolfe, who thank Scott Scrivner for excellent research assistance. A preliminary version of this section was presented at the CSDE National Advisory Board meeting, November 2000. The authors thank participants and especially our discussants, Greg Duncan and Nancy Mathiowetz, for helpful comments. Further information on child well-being can be found in Volume II.

time spent with children in activities such as reading to younger children, taking the children on outings, and an increased involvement with the child's school such as participating in the school's Parent-Teacher Association (PTA). Greater involvement in children's development in and out of school may promote better school performance for those in the child support experiment as measured by children's grades, school absences, and receipt of remedial services from the school. Our prediction is that children whose families are in the experimental group, and thus have greater resources than other families, may show better school performance than children whose families are in the control group and thus receive a smaller proportion of the child support amount. Our results are complicated somewhat by the fact that children of different ages will have very different experiences. For example, a toddler will not have a grade-point average reported by parents, or a report that her resident parent attends PTA meetings.

We also expect that the experiment will increase the probability of a child having health insurance coverage, especially private health insurance coverage through the nonresident parent, and that this coverage will have a small impact on increasing access to health care. Theoretically this increase in coverage should lead to an improvement in the child's health, but we are somewhat agnostic in our predictions of the impact of the experiment on measured health because, although improved access and income should lead to improved health, improved access will provide more information on actual health and this may lead to reported poorer health, or a higher probability of reporting health limitations and illnesses.

We begin with health, since the measures we report were collected over all of the dependent focal children regardless of age. Consistent with the estimates of other outcomes, the reported coefficients are regression-adjusted means using the basic list of control variables.[105]

Health

Our first measure of children's well-being is their health status. We use a measure popular in the literature: self-report or parental self-report of overall health status. We convert the 5-point scale of excellent, very good, good, fair, or poor to two groups: fair or poor, and all others. We find that about 10 percent of focal children of W-2 recipients have fair or poor health. Because the overall proportion of children 18 and under in the United States who had poor or fair health was 1.8 percent in 1998, this comparison suggests that the health status of W-2 children is far worse than the national average.[106] (Note, however, that low-income children generally have lower health status than the total population, so a finding that children receiving payments have lower health status is not surprising.) The specific hypothesis we address is:

1. Relative to the control group, preschool children from families in the experimental group will have improved health outcomes.

We find that there are no significant differences in the proportion of the experimentals or controls who report fair or poor health in 1998 or 1999 (Table I.4.14, Panel 1). The pattern of no group differences holds across the key subgroups and the additional subgroups, defined by child's age: preschool (0–5), school-aged (6–12), or adolescent (13 and older).[107]

Our second measure of health status is whether the child has any significant health limitations. The W-2 population appears far less healthy than children of similar age throughout the country; in the

[105]See the box on p. 29 for a list of control variables. We also estimated a set of regression adjustments using presence of a child under 6. The results were consistent with those reported here.

[106]This is based on U.S. Department of Health and Human Services, National Center for Health Statistics, 2000, Table 58. Among young children, the U.S. data show that 1.5 percent had reported poor or fair health as of 1998.

[107]Chi-square tests of the self-reported measure among all of these subgroups also indicate no difference between experimentals and controls.

Table I.4.14
Effects on Child Health

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Percentage of Mothers Reporting That the Focal Child was in Fair or Poor Health[a]										
All Resident Mothers	1,983	10.4%	11.6%	-1.1%	0.418	1,983	10.5%	11.3%	-0.7%	0.602
Entered in Lower Tier	1,050	10.3	12.6	-2.4	0.230	1,050	11.9	12.3	-0.4	0.841
No Recent AFDC History	407	9.6	10.8	-1.2	0.686	408	9.7	6.9	2.8	0.306
Has Order at Entry	1,134	10.1	12.5	-2.4	0.209	1,134	11.5	12.7	-1.2	0.534
Higher Child Support History	374	7.9	12.8	-4.9	0.124	374	7.9	9.6	-1.7	0.563
Child Age 0 to 5	1,068	11.9	12.1	-0.3	0.885	896	9.5	7.4	2.0	0.278
Child Age 6 to 12	687	6.7	10.4	-3.6	0.091	796	10.2	13.0	-2.7	0.232
Child Age 13 and Older	228	14.2	11.0	3.2	0.474	291	14.4	16.9	-2.5	0.554
(2) Percentage of Mothers Reporting That the Focal Child Had Limitations[b]										
All Resident Mothers	1,972	9.9%	14.7%	-4.9%	**0.001**	1,963	11.1%	14.0%	-2.8%	0.058
Entered in Lower Tier	1,039	10.7	16.4	-5.7	**0.008**	1,035	13.1	14.5	-1.5	0.491
No Recent AFDC History	405	6.2	10.6	-4.4	0.109	404	11.6	9.9	1.7	0.585
Has Order at Entry	1,128	10.7	17.1	-6.4	**0.002**	1,124	11.5	16.0	-4.5	**0.028**
Higher Child Support History	371	11.9	17.5	-5.7	0.126	371	13.0	15.7	-2.7	0.462
Child Age 0 to 5	1,060	5.9	11.4	-5.5	**0.002**	889	8.2	9.7	-1.5	0.435
Child Age 6 to 12	684	14.8	17.6	-2.8	0.331	787	15.3	16.6	-1.3	0.634
Child Age 13 and Older	228	12.9	19.2	-6.3	0.201	287	8.6	18.5	-9.9	**0.016**
(3) Percentage of Mothers Reporting That the Child's Health Improved in the Last Year[c]										
All Resident Mothers						1,981	21.4%	22.5%	-1.1%	0.539
Entered in Lower Tier						1,050	17.9	23.3	-5.4	**0.030**
No Recent AFDC History						408	25.2	24.7	0.4	0.918
Has Order at Entry						1,134	19.7	20.7	-1.0	0.677
Higher Child Support History						374	14.7	22.0	-7.3	0.070
Child Age 0 to 5						894	25.6	26.8	-1.1	0.704
Child Age 6 to 12						796	18.1	19.3	-1.2	0.668
Child Age 13 and Older						291	16.6	18.8	-2.1	0.635

Table I.4.14, continued

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(4) Percentage of Mothers Reporting That the Child Was Uninsured at Some Point in the Year[d]										
All Resident Mothers	1,972	15.6%	15.2%	0.4%	0.805	1,950	16.9%	16.6%	0.3%	0.854
Entered in Lower Tier	1,043	14.8	14.2	0.6	0.787	1,034	13.1	14.8	-1.7	0.443
No Recent AFDC History	405	20.1	22.1	-2.0	0.625	400	23.6	25.1	-1.4	0.740
Has Order at Entry	1,125	15.0	14.2	0.8	0.700	1,120	16.2	14.7	1.5	0.489
Higher Child Support History	371	16.5	17.3	-0.8	0.835	369	15.6	20.3	-4.7	0.246
Child Age 0 to 5	1,063	14.2	15.7	-1.5	0.489	883	15.7	19.6	-3.8	0.139
Child Age 6 to 12	684	14.9	13.2	1.7	0.524	783	17.0	14.5	2.5	0.340
Child Age 13 and Older	225	22.7	15.9	6.8	0.209	284	18.9	13.3	5.6	0.203

Notes: AFDC history is in the two years preceding W-2 implementation in October 1997. All means are regression-adjusted using the basic list of control variables. Probability values of 0.05 or less are shown in bold type. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33.

[a]In both 1998 and 1999, 2 cases of nonresponse were deleted for this outcome. In 1998, 14 cases of item nonresponse were deleted for the full-time-work subgroups, and 4 cases of item nonresponse were deleted for the same-father subgroups. In 1999, 19 cases of item nonresponse were deleted for the full-time-work subgroups, and 4 cases of item nonresponse were deleted for the same-father subgroups.

[b]In 1998, 13 cases of nonresponse were deleted for this outcome. In 1999, 22 cases of nonresponse were deleted for this outcome. In 1998, 13 cases of item nonresponse were deleted for the full-time-work subgroups, and 4 cases of item nonresponse were deleted for the same-father subgroups. In 1999, 19 cases of item nonresponse were deleted for the full-time-work subgroups, and 3 cases of item nonresponse were deleted for the same-father subgroups.

[c]In 1999, 22 cases of nonresponse were deleted for this outcome; 19 cases of item nonresponse were deleted for the full-time-work subgroups, and 4 cases of item nonresponse were deleted for the same-father subgroups.

[d]In 1998, 12 cases of nonresponse were deleted for this outcome. Cases that reported having no private insurance in 1998 and no Medicaid in 1998 and no period without health insurance in 1998 were deleted (n=1). In 1998, 12 cases of item nonresponse were deleted for the full-time-work subgroups, and 4 cases of item nonresponse were deleted for the same-father subgroups. In 1999, 19 cases of nonresponse were deleted for this outcome. Cases that reported having no private insurance in 1999 and no Medicaid in 1999 and no period without health insurance in 1999 were deleted (n=16). In 1999, 19 cases of item nonresponse were deleted for the full-time-work subgroups, and 4 cases of item nonresponse were deleted for the same-father subgroups.

United States as a whole, 6.6 percent of children 18 and younger are reported to have any activity limitation (U.S. Department of Health and Human Services, National Center for Health Statistics, 2000, Table 57), compared to 12 percent in our W-2 sample. The results, shown in Table I.4.14, Panel 2, suggest some overall differences in the proportion with significant health limitations by experimental status both in 1998 and 1999, but far more in 1998. The difference is statistically significant overall—that is, for all resident mothers—in 1998 and (marginally) in 1999. The difference is also significant for children whose mothers entered in a lower tier in 1998 and for children whose mothers had an order at entry in both years. For the oldest group of children, the difference is significant only in 1999. There are no other statistically significant effects in the other key subgroups in 1999. Most of the difference seems to have diminished over time, when we might expect the experiment to have its greatest impact. When we look at these results for preschool children alone, we find evidence of a significant effect only in 1998. Thus this indicator provides some, though limited, evidence that the experiment had a positive impact on the health of young children.

Our third indicator of health status is a measure of change. In the second wave of the survey, parents were asked whether their children's health had improved over the last year. The results provide only limited evidence that the health of children in experimental-group families had improved more than that of children in control families (Table I.4.14, Panel 3). There is no significant difference in the percentage with improved health overall, or in the subgroups, with the exception of those whose mothers entered in the lower tiers and those whose mothers had a history of higher child support (marginally significant). In these cases children in the experimental group show greater improvements in health.

Our last measure of health attempts to capture access to health care: whether the child was without health insurance at some point during the year. A comparison to national figures suggests that this population of children is similar to the national average. In the United States as a whole, 14 percent of all children 18 and younger were uninsured as of 1997, whereas among similarly aged children living below the poverty line, 15.4 percent were not covered (U.S. Department of Health and Human Services, National Center for Health Statistics, 2000, Table 128). This is consistent with the 15–17 percent of children in the study sample who were uninsured as of 1998, but the national data refer to the entire year whereas the survey refers to shorter periods of time, suggesting that these children are better off.[108] Again we find no statistically significant differences in percentage uninsured by experimental versus control status in either 1998 or 1999 (Table I.4.14, Panel 4). The high proportion uninsured is somewhat surprising, because most of these children would be eligible for Medicaid. The implementation of BadgerCare in July 1999 may lead to changes in these proportions in the future.

Parenting Practices

Our first measure of parenting practices is whether the mother had attended at least one meeting of the PTA for her school-aged child. We hypothesize that participating in the experiment might increase the amount of time that the resident parent spends with the child on a variety of activities both in and outside of school. More specifically, our hypothesis is:

2. Relative to the control group, children from families in the experimental group will experience greater investments from parents in their health and educational development and will have more nurturing relations with their parents.

[108]A small group of children was excluded from this analysis due to a lack of response by the parent to our questions on health insurance coverage for the focal child. We excluded those who reported no private insurance, no Medicaid, and no period without health insurance. Our measure of being without insurance is for some period of time during the year rather than the more stringent all-year criterion. The number of children uninsured for all of 1998 was 46 and for all of 1999 was 27.

Nationally, 76 percent of parents of children from preschool to grade 12 report that they attend a general school meeting during the year.[109] Our sample has a lower rate of attendance at PTA or PTO meetings: 36–41 percent attended (Table I.4.15, Panel 1). In 1998, some subgroups of resident parents in the experimental group were more likely than those in the control group to attend any PTA meetings, but the difference is no longer statistically significant in 1999. The subgroup difference in 1998 is especially large for those who had an order at entry, and for those with a history of higher child support. These group differences in PTA attendance narrowed substantially in 1999; the group differences for the total sample and the subgroups did not approach significance. Whereas the proportion of resident parents who did attend PTA meetings decreased slightly from 1998 to 1999 in the experimental group (42 percent to 36 percent), it stayed nearly constant at 37 percent among control-group parents.

The second measure of parenting practices is whether the resident parent reads to the child every day (Table I.4.15, Panel 2). Parent reading practices, especially during a child's preschool years, are a key predictor of early school achievement. In national surveys, 57 percent of parents report reading to their pre-school-age children every day (U.S. Department of Education, 1999, Table 143). The mothers in our sample approached (and in a few cases exceeded) this frequency of reading only in 1999. No significant differences were detected between experimental and control families in reading practices in 1998 and 1999 for the total sample and subgroups. Both experimental and control families reported increases in reading every day to their children from 1998 to 1999.

School Performance

This outcome assesses whether the expected increase in economic resources (time and money) of the child support pass-through promotes better school performance by children. School performance is measured by parent reports of children's grades (grade-point average), school absences (10 or more in the fall semester), and placement in special education. As common indicators of how well children perform in school, they provide relevant evidence about our hypothesis:

3. Relative to children in the control group, school-age children in the experimental group will enhance their school performance.

School grades were surveyed only in 1999, so change from time 1 to time 2 cannot be assessed. Nationally, children's mean grade-point average as reported by parents is approximately 3.1.[110] Our sample of children 10 and older has a somewhat lower mean grade-point average, 2.4–2.6. As shown in Table I.4.16, Panel 1, we find some weak evidence of a difference in mean grade-point average in favor of children living in experimental-group families. The pattern of findings quite consistently favors children in experimental-group families. The group difference is not significant for the total sample of children or for the key subgroups shown. However, we did find marginally significant differences for two subgroups not shown: those whose mothers worked full time, and girls living in Milwaukee.

Our second measure of school performance is school absences: whether the resident parent reported that the child missed 10 or more days of school during the fall semester;[111] this measure is examined in Table I.4.16, Panel 2. A higher reported proportion denotes more problematic attendance. Thus we predict that the experimental group should have lower scores than the control group, consistent

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[109]Reported for 1996 in U.S. Department of Education (1999),Table 25. Because of differences in item wording between the two surveys (PTA vs. general school meeting), this comparison should be interpreted cautiously.

[110]This value is estimated from U.S. Department of Education, 1999, Table 25, for 1996.

[111]National statistics on this particular coding are limited. Among 10th graders in 1992, 35 percent of youth reported missing 5 or more days of school during the first half of the year (U.S. Department of Education, 1999, Table 153). Among adolescents in our sample, 42 percent of resident parents reported in 1998 that youth missed 6 or more days in the fall semester.

Table I.4.15
Effects on Parenting Practices

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Percentage of Mothers Reporting That They Attended at Least One PTA or PTO Meeting in the Last Year[a]										
All Resident Mothers	904	42.0%	36.7%	5.3%	0.113	1,059	36.4%	36.5%	-0.1%	0.972
Entered in Lower Tier	527	43.8	41.4	2.4	0.582	615	38.2	39.5	-1.3	0.737
No Recent AFDC History	104	23.8	26.9	-3.1	0.734	117	21.6	31.8	-10.2	0.249
Has Order at Entry	620	42.5	33.6	8.9	**0.027**	739	36.9	36.6	0.3	0.928
Higher Child Support History	244	40.6	24.4	16.2	**0.009**	274	38.3	35.9	2.4	0.692
Child Age 6 to 12	686	42.5	36.2	6.3	0.097	792	37.4	36.2	1.2	0.738
Child Age 13 and Older	218	40.6	37.3	3.4	0.624	267	33.0	37.3	-4.4	0.460
(2) Percentage of Mothers Reporting That They Read to Pre-School-Age Child on a Daily Basis[b]										
All Resident Mothers	1,067	48.4%	51.1%	-2.6%	0.397	895	52.7%	58.2%	-5.5%	0.102
Mother Entered in Lower Tier	516	45.0	48.4	-3.4	0.448	417	53.4	55.5	-2.2	0.662
No Recent AFDC History	303	46.8	52.7	-5.9	0.315	289	57.4	64.7	-7.3	0.212
Mother Has Order at Entry	504	46.4	50.3	-3.9	0.380	379	52.6	52.1	0.5	0.921
Higher Child Support History	127	47.6	47.8	-0.2	0.980	96	63.8	56.1	7.8	0.447

Notes: AFDC history is in the 2 years preceding W-2 implementation in October 1997. All means are regression-adjusted using the basic list of control variables. Probability values of 0.05 or less are shown in bold type. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33.

[a]In 1998, cases to which the question was not applicable were deleted, including cases with focal child schooled at home, focal child not attending school, focal child age less than 6 (n=1,081). In 1999, 926 not applicable cases were deleted. In 1998, 8 cases of item nonresponse were deleted for the full-time-work subgroups, and 3 cases of item nonresponse were deleted for the same-father subgroups. In 1999, 10 cases of item nonresponse were deleted for the full-time-work subgroups, and 2 cases of item nonresponse were deleted for the same-father subgroups.

[b]In 1998, cases to which the question was not applicable were deleted, including cases with focal child age older than 5 (n=914), and 4 cases of nonresponse were deleted for this outcome. In 1998, 5 cases of item nonresponse were deleted for the full-time-work subgroups, and 1 case of item nonresponse for the same-father subgroups. In 1999, cases to which the question was not applicable were deleted (n=1090); 9 cases of item nonresponse were deleted for the full-time-work subgroups, and 1 case of item nonresponse was deleted for the same-father subgroups.

Table I.4.16
Effects on School Performance

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Child's GPA (Children Aged 10 or More)[a]										
All Resident Mothers						532	2.58	2.44	0.14	0.116
Mother Entered in Lower Tier						306	2.48	2.46	0.02	0.877
No Recent AFDC History						71	2.79	2.59	0.19	0.349
Mother Has Order at Entry						349	2.55	2.55	0.00	0.978
Higher Child Support History						144	2.68	2.57	0.12	0.493
(2) Percentage of Mothers Reporting That the Child Missed 10 or More Days of School (Children Aged 6 or More)[b]										
All Resident Mothers	844	10.1%	17.0%	-6.9%	**0.004**	996	12.4%	15.2%	-2.8%	0.202
Mother Entered in Lower Tier	496	13.5	19.1	-5.6	0.095	579	15.2	18.2	-3.0	0.346
No Recent AFDC History	95	9.9	7.4	2.5	0.683	114	0.0	0.0	0.0	0.188
Mother Has Order at Entry	581	10.2	13.3	-3.1	0.249	692	14.0	12.8	1.2	0.652
Higher Child Support History	226	9.0	11.9	-2.9	0.474	261	13.2	11.5	1.8	0.668
Child Age 6 to 12	631	8.4	16.0	-7.6	**0.004**	730	10.0	11.4	-1.4	0.554
Child Age 13 and Older	213	15.4	19.3	-4.0	0.454	266	20.0	25.8	-5.8	0.270
(3) Percentage of Mothers Reporting That the Child Received Special Education[c]										
All Resident Mothers	902	21.0%	19.8%	1.2%	0.670	1,057	19.1%	21.5%	-2.4%	0.342
Mother Entered in Lower Tier	527	22.0	18.6	3.4	0.345	614	21.1	20.9	0.2	0.950
No Recent AFDC History	102	19.8	20.4	-0.6	0.942	117	17.4	29.5	-12.0	0.137
Mother Has Order at Entry	619	20.5	21.4	-1.0	0.770	738	20.5	24.1	-3.6	0.238
Higher Child Support History	243	22.1	21.3	0.9	0.870	274	23.6	23.3	0.3	0.959
Child Age 6 to 12	685	23.1	21.0	2.1	0.519	792	22.4	22.7	-0.2	0.938
Child Age 13 and Older	217	15.0	14.5	0.5	0.925	265	8.6	17.3	-8.7	**0.038**

Notes: AFDC history is in the 2 years preceding W-2 implementation in October 1997. All means are regression-adjusted, using the basic list of control variables. Probability values of 0.05 or less are shown in bold type. For additional notes on subgroups, control variables, and presentation of the data, see text boxes on pp. 29, 32, and 33.

Table I.4.16, continued

[a]In 1999, cases to which the question was not applicable were deleted, including cases with focal child schooled at home, focal child not attending school, and focal child age less than 10 (n=1,449), and 4 cases of nonresponse were deleted for this outcome; 5 cases of item nonresponse were deleted for the full-time-work subgroups, and 1 case of item nonresponse was deleted for the same-father subgroups.

[b]In 1998, cases to which the question was not applicable were deleted, including cases with focal child schooled at home, focal child not attending school, focal child attending preschool or kindergarten, and focal child age less than 6 (n=1,135), and 6 cases of nonresponse were deleted for this outcome. In 1998, 7 cases of item nonresponse were deleted for the full-time-work subgroups, and 3 cases of item nonresponse were deleted for the same-father subgroups. In 1999, cases to which the question was not applicable were deleted (n=982), and 7 cases of nonresponse were deleted for this outcome; 10 cases of item nonresponse were deleted for the full-time-work subgroups, and 2 cases of item nonresponse were deleted for the same-father subgroups.

[c]In 1998, cases to which the question was not applicable were deleted, including cases with focal child schooled at home, focal child not attending school, focal child age less than 6 (n=1,081), and 2 cases of nonresponse were deleted for this outcome. In 1998, 8 cases of item nonresponse were deleted for the full-time-work subgroups, and 3 cases of item nonresponse were deleted for the same-father subgroups. In 1999, cases to which the question was not applicable were deleted (n=926), and 2 cases of nonresponse were deleted for this outcome; 9 cases of item nonresponse were deleted for the full-time-work subgroups, and 2 cases of item nonresponse were deleted for the same-father subgroups.

with our hypothesis that school performance (including attendance) should improve through the child support pass-through. Children in experimental-group families had fewer absences in 1998 (10.1 percent vs. 17 percent for the control group); the difference in 1999 was not statistically significant. Similarly, those children aged 6–12 showed an effect in 1998, but not in 1999. Though not statistically significant in most cases, the pattern is consistent with better attendance for the experimental group.

The final measure of school performance is special education placement: the resident parent's report of whether the child received special education services (Table I.4.16, Panel 3). Lower rates of special education generally indicate that the child is meeting basic school requirements. Nationally, 13 percent of public school children up to age 21 received special education services in 1996–97 (U.S. Department of Education, 1999, Table 53); the children in our sample have higher rates. As with school absences, a minus sign in the difference column would be consistent with our hypothesis that those in the experimental group have lower rates than those in the control group. No overall effect on the receipt of special education services emerges. However, experimental-group children who were 13 or older were less likely to receive special education in 1999 (8.6 vs. 17.3 percent for the control group).

Thus, for the three school performance indicators there is very limited support for our hypothesis that, relative to the control group, school-age children from families in the experimental group will enhance their educational development.

In other estimates we explore a fuller array of measures of the differences between children in experimental and control families in the domains of child health, parenting practices, and children's school performance by age group. Among the indicators of child health are the number of visits to a physician because of illness or for routine care, and participation in private or public health insurance. Among the indicators of school performance are the number of school days missed, the number of schools attended, grade retention, parent's educational expectations for child, and, for adolescents, getting in trouble with the police. Additional indicators of parenting practices include the frequency of playing with child, frequency of helping with homework, use and quality of child care, and frequency of outings taken with children by both resident and nonresident parents.

In general, the unreported results do not provide strong evidence for systematic differences between the experimental and control groups. Exceptions to the overall lack of differences are as follows:

1. Families in the experimental group seem more satisfied with their child care arrangements. Relative to the control group, fewer resident parents in the experimental group would change to another arrangement if the cost were zero.

2. A lower proportion of experimental-group adolescent children had trouble with the police in 1998, and this difference was largely maintained in 1999 (although it was no longer significant).

Summary of Effects on Child Well-Being

Because the experimental group receives more child support, and thus may have higher income, this may increase children's well-being. (However, we do not expect to see large effects given that this is an indirect effect, occurring only if the experiment increases income or decreases conflict.) We examine health, parenting practices, and education.

- We find limited evidence of an effect on health. The indicator that suggests a difference is *health limitations,* for which we find a significant difference for the entire group in 1998 and 1999 (in the later year the difference is only marginally significant, $p = .058$). The only subgroup for which we find a significant difference in both years is children of mothers with an order at entry.
- We find little evidence of an effect on parenting practices. No indicators are significant for the entire sample or any subgroup in both years.

- We find little evidence of effects on educational performance. Experimental-group mothers reported that their children age 13 plus were less likely to receive special education in the second year, 1999.

Chapter 5
Summary and Conclusions

Recent welfare reforms have increased the potential importance of child support as an income source for low-income single-parent families. Time limits, work requirements, and the lack of an entitlement to cash assistance have made nonwelfare sources of income increasingly essential. In Wisconsin, relatively stringent work requirements have been combined with a uniquely generous approach to child support. Among most mothers participating in W-2, any child support received on behalf of their children is passed through to them and is disregarded in the calculation of their W-2 cash payments.

The Child Support Demonstration Evaluation (CSDE) was designed to evaluate the impact of this new approach to child support, which was adopted within the context of a new approach to welfare. The research aimed to evaluate the direct effects of the new policy on child support payments and receipts. In accordance with the initial evaluation plan, we have also tried to measure a wide range of potential secondary effects—on mothers' and fathers' employment and earnings, on parents' interactions, and on the well-being of their children.

I.5.1 Summary of Experimental Impacts

As shown in Chapter I.4, and as summarized in Table I.5.1, we find substantial evidence of the expected direct effects. In 1998, mothers subject to the full pass-through received about $150 dollars more in child support than did those in the control group. Among all those who entered in a lower tier of W-2 (and thus subject to a reduced pass-through if they were in the control group), the difference was about $200. Differences were somewhat smaller, but remained significant, in 1999. Although these differences in amounts of child support received by mothers are due in large part to the mechanical effect of the full pass-through, we also find significant increases in the percentage of nonresident fathers paying child support. These differences are statistically significant, but fairly small, in the full sample: 52 percent of fathers of children in the experimental group and 50 percent of fathers of children in the control group paid child support in 1998. However, among those more likely to be new to the child support and welfare systems, the differences were more substantial: among those cases in which the mother had not received AFDC in the two years prior to entering W-2, 58 percent of fathers with children in the experimental group, compared to only 48 percent of fathers with children in the control group, paid any child support in 1998. The differences remained significant and in many cases increased in 1999. Finally, we also find significantly higher rates of paternity establishment for those in the experimental group in 1998, although the difference declined and was not statistically significant for most groups in 1999.

As expected, we find less consistent indications of secondary effects, although in selected areas there is substantial evidence that the experiment had the expected impact. We hypothesized that an increase in child support receipts would reduce the need for cash payments. We find evidence of this effect in 1998, with significant and larger differences among mothers who entered in the lower tiers of W-2 and among mothers with a history of higher child support amounts. We also find some evidence of the expected effects on nonresident fathers' informal employment: fathers with children in the experimental group appeared to be substantially less likely to have informal earnings.

In other areas we find little consistent evidence of an experimental impact. There were few significant impacts on mothers' employment or earnings, perhaps because increases in child support receipt were not sufficiently large to have such secondary effects, or perhaps because the increase in child support simultaneously helped facilitate employment (as hypothesized) and reduced the incentive to work (as economic theory would suggest). We find few consistent impacts on child well-being—although there was some evidence of fewer health limitations and improved educational outcomes for children in the experimental group. Although most measures of nonresident fathers' relationships with the mother and

child revealed few differences among the two groups, we find some evidence of higher informal transfers made by fathers in the experimental group. In 1998, we also find marginally significant differences in coresidence among families that have no recent AFDC history; those in the experimental group are less likely to coreside. This finding is consistent with a view that increased child support receipt encourages mothers' independence and thus leads to lower rates of coresidence. Consequently, it may help to explain the unexpected finding that total family income in 1998 is lower among experimental group mothers with no recent AFDC history.

Finally, although we find significant differences in some of the components of total government costs, we find no difference in overall government costs. Although more child support is passed through to those in the experimental group, not all of this is at the expense of the government, since some consists of additional support that would not have been paid in the absence of the full pass-through. More important, the reform also generated cost savings in other areas, especially W-2 cash payments. We discuss the implications of these cost estimates in greater detail in Section I.5.3, below.

I.5.2 Generalizing from CSDE Impact Estimates

As summarized above, we find fairly consistent evidence in support of the hypothesized direct effects of the experiment. We find consistent evidence for selected secondary effects as well. To what extent should these estimated effects (and lack of effects) inform our expectations regarding the likely consequences of adopting a full pass-through in other states? In answering this question, we first consider the extent to which the results summarized here are indicative of the expected long-term results of a full pass-through in Wisconsin. We then consider limitations in our ability to generalize from Wisconsin to other states.

We believe that the effects of the CSDE reported here are likely to understate the potential effects of the policy change in Wisconsin. First, our analysis shows larger effects among cases new to the welfare system. We expect that the effects of the experiment might be greater among those who have not already established behavioral patterns in response to the old system—a growing proportion of all cases over time.[112] Second, W-2 involved dramatic changes in the administration and structure of welfare programs and payments. Especially in Milwaukee, where most participants reside, it appears that many caseworkers did not initially understand the CSDE or explain the implications of their experimental status to clients. In preliminary analysis of cases assigned as part of a later cohort of participants, after W-2 was more fully implemented and staff received additional training, we find evidence of greater effects.[113]

For a number of reasons *the effects of the CSDE may understate the effects that a full pass-through might have were it implemented in other states*. First, the difference in the pass-through to those in the experimental and control groups in Wisconsin was more modest than the likely difference in other states. Even those in the control group of the CSDE received the greater of up to $50 per month or 41 percent of child support paid. Under TANF, most states are not passing through any child support. A move to a full pass-through would therefore be a larger policy change in most other states, and would

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[112]In addition, some fathers of those in the experimental group have amounts owed to the state for payments they did not make when the mother was receiving AFDC. Because of this, any amount they pay that is in excess of what is currently due to the mother is distributed between the mother and the state. Thus, even among experimental-group cases, some payments may not be passed through to the mother. If the full pass-through were policy, in the long run there would be no past-due amounts of child support owed to the state, making the eventual effect of a full pass-through larger than what we have observed here.

[113]Because random assignment was mistakenly discontinued in Milwaukee on July 8, 1998, the original sample of CSDE cases, analyzed here, includes cases that entered relatively early in the implementation of W-2. With this in mind, random assignment was restarted statewide on January 1, 1999, and continued through June 30, 1999. Wisconsin was recently awarded federal funds to extend the CSDE analysis to include these later cases.

probably generate a greater response.[114] Second, to the extent that participants in other states might receive cash payments for a longer period, the effects of the policy change might also be greater. In Wisconsin, members of the control group were subject to the reduced pass-through only while participating in the lower tiers of W-2. Because of the structure of W-2 (including the absence of an earnings disregard, unique to Wisconsin), Wisconsin participants leave cash assistance more rapidly than do participants in most other states. Third, because Wisconsin's caseload decline has been so steep, the state's current TANF recipients may be more disadvantaged than those in other states. This may mean that the amount of child support that nonresident parents could potentially pay may be lower, and thus the effects of a full pass-through may be lower in Wisconsin than elsewhere.

Finally, many indicators show that Wisconsin is among the most effective states at collecting child support. This may mean that the child support system in Wisconsin is more automatic and less voluntary than elsewhere. If so, fewer fathers in Wisconsin may be participating in the informal sector, both because the probability of their being detected is higher and because the penalties that follow being detected are more stringent. If this is true, the effects of a full pass-through may be larger in a state in which more individuals are in a position to move into the formal child support system in response to incentives. On the other hand, if one effect of the reform is to bring new cases into the formal child support system, Wisconsin may be better able to ensure that child support payments continue, and thus the effects may be greater in Wisconsin than in a state in which ongoing enforcement is less effective.

I.5.3 Generalizing from CSDE Cost Estimates

One striking result of the CSDE is that government costs were not found to be significantly higher for those in the experimental group. In other words, the full pass-through and disregard increased the child support received by low-income women with children without significantly increasing government costs. In this section we consider the extent to which this cost-neutrality result is due to conditions specific to context of the CSDE, and we discuss the implications for expected costs if a similar reform were to be adopted by other states.

Two elements of the CSDE that may have mitigated the impact of the experiment could have particular consequences for cost estimates. First, not *all* of the child support payments end up benefitting the family, even among those in the experimental group. As we have noted above, in the experimental group some payments were retained by the government, primarily those that came from federal tax intercepts and any payments that were in excess of the amount currently due. If a full pass-through were completely implemented, eventually there would be very few cases with debts owed to the state.

How would costs differ if all child support—including tax intercepts and payments on arrearages—went to the custodial parent rather than to the government? Although it is impossible to precisely estimate the total impacts, the CSDE experience provides the basis for some general conclusions. We know that the per-case amount retained by the government for those in the experimental group was $129 in the first year after entry and $145 in the second year.[115] If these amounts were no longer retained, and there were no other behavioral effects, this would increase our estimate of net costs per case from $100 to $229 in the first year and from $35 to $180 in the second. Were this the only change, both cost differences would be statistically significant. However, this change would also increase the amount of support received by those in the experimental group, even in the absence of behavioral effects, and this would decrease Food Stamp costs (because the additional child support income results in

[114]In addition to the greater effects that might result from a shift to full pass-through, the contrast between a full and zero pass-through would likely be easier to communicate to workers and clients.

[115]These numbers are based on relative years. The numbers reported in Table I.4.7, based on calendar years, are quite similar, $126 in 1998 (compared to $129 in the first year after entry) and $147 in 1999 (compared to $145 in the second year after entry).

lower Food Stamp payments) and could decrease W-2 and Medicaid/BadgerCare participation (some mothers could become ineligible owing to the increased income). Moreover, if no child support was retained for those in the experimental group, there could be an even larger increase in the likelihood of paying support or in the amount paid, which would further decrease costs.

A second aspect of the CSDE may have reduced the impact of the policy on government costs. The control group in the CSDE is subject to a relatively generous policy, receiving up to $50 per month or 41 percent of what is paid, whichever is more. In contrast, most states do not pass through any child support, retaining the full amount paid. Thus, part of the reason for the lack of expense associated with the Wisconsin reform is that so much is passed through (and little is retained) for those in the control group.

How would costs differ if the comparison (control) group received no child support during periods in which the resident parent was receiving a payment from W-2? The amount of support received by the control group can be decomposed into two parts, an amount received during periods in which the family was in an upper tier or off W-2 (and thus would have been received whether there was a partial pass-through or no pass-through) and an amount received during periods in which the family was in a lower tier of W-2. The amount received during lower-tier periods was $112 in the first year and $79 in the second. If all else remained equal, but the full amount of child support was retained for those in the control group, this would add about $112 to the first year's net cost, increasing our per-case estimate of total government cost from $100 to $212, and adding $79 to the second year's cost, increasing our estimate from $35 to $114. But if all payments were retained for those in the control group, other effects would occur as well. Food Stamp costs for the control group would increase somewhat, because these families would have lower incomes. Perhaps more important, the amount of child support paid may be lower among control-group families; recall that we have shown that payments are linked to the generosity of the pass-through. The lower amounts paid would then mean lower amounts retained, increasing costs for the control group, and lower amounts paid may also mean that families in the control group remain in W-2 longer, further increasing costs for this group.

In summary, the experiment gives an accurate estimate of the difference in government costs of a full pass-through of child support as compared to a partial pass-through. We find that the differential costs of the full pass-through are not significantly different from zero. We conducted two simple simulations to provide upper-bound estimates of costs for more powerful interventions. First, if the experimental-group members really received the entire amount paid on their behalf (with nothing at all retained by the government), the mechanical effect would be to increase costs somewhat, by about $130–$150 per family. However, our other estimates suggest that there would be at least some offsetting receipts, so that this is an overestimate of the net increase in costs. Second, if control-group members were to receive nothing during periods in which they were in a lower tier, this would decrease costs among the control group by about $80–$110, thus increasing the costs of the reform. But again, other effects of this change would offset a portion of these increased costs, making the $80–$110 an overestimate of the cost differential.

I.5.4 Policy Implications

The results of the CSDE presented here demonstrate that Wisconsin's full pass-through has been able to increase child support amounts received among an economically vulnerable population, to increase child support collections, and to have a variety of other positive effects. These benefits have come at little additional cost to government. Although some factors might lead CSDE estimates to overstate potential policy effects, we expect that the effects of a full-pass-through policy in another state would be larger than those reported here. Indeed, in many ways it is striking that we do find evidence of substantial effects, given the implementation problems, the lack of a large difference in the policies faced by the experimental and control groups, the speed with which mothers are moving off W-2, and the relative socioeconomic disadvantage of W-2 participants.

In most states TANF participants do not receive any of the child support paid on behalf of their children. This no-pass-through policy generates revenue to offset public assistance and child support enforcement costs in the short run. Our results suggest, however, potentially detrimental effects of this policy on developing child support as a long-run income source for single mothers and their children. Given the time-limited nature of cash assistance, the benefits to government of retaining child support are also quite limited. In contrast, the benefits to children of establishing paternity and setting a pattern of child support payments are potentially more enduring. Especially for this reason, a full pass-through seems to be a policy worthy of serious consideration by other states.

Table I.5.1
Summary of Effects on Population and Key Subgroups

	All		Entered in Lower Tier		No Recent AFDC History		Order at Entry		Higher Child Support History	
	1998	1999	1998	1999	1998	1999	1998	1999	1998	1999
Effects on Child Support Paid and Received (Tables I.4.1 and I.4.2)										
Percentage of Nonresident Fathers Paying Child Support	+	+++		+	++		++	+++		+++
Average Annual Amount of Child Support Paid among Nonresident Fathers		+		++	++			++		+++
Percentage of Resident Mothers Receiving Child Support	+++	++		+	+++		+	+++		++
Average Annual Amount of Child Support Received among Resident Mothers	+++	+++	+++	+++	+++	++	+++	+++	+++	+++
Effects on Paternity and Child Support Orders (Table I.4.3)										
Paternity Established among Nonmarital Children without a Legal Father at Entry	+				++		++		++	++
Paternity Established among Mothers, Paternity Not Established at Entry					+		na	na	na	na
Order Established among Legal Fathers without an Order at Entry								++		
Order Established in 1999 among New Legal Fathers between Entry and End of 1998	na	+	na	na	na	na	na	na	na	na
Order Changes among Legal Fathers with Orders at W-2 Entry	– –		– – –				– – –		– – –	
Effects on Program Participation (Table I.4.4)										
Average W-2 Payments Received by Mothers	–		–						–	
Average Dollar Amount of Food Stamps								–		
Average Amount of Medicaid/BadgerCare Paid										
Average Amount of Child Care Subsidies Paid										
Effects on Resident Mothers' Employment and Earnings (Table I.4.5)										
Percentage with Earnings (Administrative Data)										
Number of Months Worked (Survey)									–	
Usual Hours Worked per Week (Survey)									– –	
Hourly Wage Rate for Current or Last Job (Survey)	+	+	+	+			++	+++		
Average Earnings (Administrative Data)										
Effects on Income and Economic Hardship of Resident Mothers (Table I.4.6)										
Average Total Personal Income (Administrative Data)	+	+								
Average Total Family Income (Survey)					– –		–		– –	–
Percentage Reporting a Food, Shelter, or Telephone Hardship (Survey)										+

Table I.5.1, continued

	All		Entered in Lower Tier		No Recent AFDC History		Order at Entry		Higher Child Support History	
	1998	1999	1998	1999	1998	1999	1998	1999	1998	1999
Effects on Government Costs (Table I.4.7)										
Average Total Costs from Program Participation										
Average Amount of Child Support Retained by the State	– – –	– – –	– – –	– – –	– – –	– – –	– – –	– – –	– – –	– – –
Average Amount of Total Government Costs										
Effects on Earnings and Program Participation of Nonresident Fathers (Table I.4.8)										
Percentage with Earnings (Administrative Data)										
Number of Employers in a Year (Administrative Data)										
Number of Months Worked (Survey)				– –						– –
Hourly Wage Rate for Current or Last Job (Survey)	–		– –				–	–		
Average Earnings (Administrative Data)										
Percentage with Informal Earnings (Survey)		– –		– –		– –		–		
Percentage Participating in Any Program in 1998 (Survey)		na		na		na	++	na		na
Percentage Participating in W-2, FS, or Medicaid (Administrative Data)							–	– – –		–
Effects on the Income and Economic Hardship of Nonresident Fathers (Table I.4.9)										
Average Total Personal Income, Net of Child Support Paid (Administrative Data)										
Average Total Family Income, Net of Child Support Paid (Survey)			– –					–		
Percentage Reporting a Food, Shelter, or Telephone Hardship (Survey)						–				
Effects on Nonresident Fathers' Social and Economic Involvement with Child (Tables I.4.10 - I.4.13)										
Percentage of Families in Which Mother, Father, and Child Lived Together All Year										
Percentage of Families in Which Child Lived with Both Parents 7 or More Mos.					–					
Percentage in Which Child Lived with Both Parents/Had Frequent Contact with Father			–				–			+
Mean No. Days Father Saw Child during the Time They Lived Apart									+	
Percentage of Fathers Cared for Child So Mother Could Work/Go to School/Seek Work										
Percentage of Mothers Who Think Child's Father Does Good Job as a Parent								++	+	++
Percentage of Mothers Reporting High Conflict on at Least 1 Child-Rearing Issue	–						– – –	– –		
Percentage of Families in Which There Were Any Informal Transfers						+				
Mean No. Different Types of Informal Transfers					+					
Percentage in Which the Total Value of Informal Transfer was $500 or Greater		+++		+++				+++	+	++

Table I.5.1, continued

	All		Entered in Lower Tier		No Recent AFDC History		Order at Entry		Higher Child Support History	
	1998	1999	1998	1999	1998	1999	1998	1999	1998	1999
Effects on Child Well-Being (Tables I.4.14 - I.4.16)										
Percentage of Mothers Reporting That Child Was in Fair or Poor Health										
Percentage of Mothers Reporting That Child Had Limitations	– – –	–	– – –				– – –	– –		
Percentage of Mothers Reporting Child's Health Improved in the Last Year	na		na	– –	na		na		na	–
Percentage of Mothers Reporting Child Uninsured at Some Point in Year										
Percentage of Mothers Reporting Attending at Least 1 PTA/PTO Meeting Last Year							++		+++	
Percentage of Mothers Reporting Reading to Pre-School-Age Child Daily										
Child's GPA (Children Aged 10 or More)	na		na		na		na		na	
Percentage of Mothers Reporting Child Missed 10 or More Days of School	– – –		–							
Percentage of Mothers Reporting Child Received Special Education										

Key:	Positive	Negative
Significant at the 1% level	+++	– – –
Significant at the 5% level	++	– –
Significant at the 10% level	+	–
Not applicable	na	na

Blanks indicate that the difference was not statistically significant.

Appendices

Appendix I.4.1

Experimental Impacts on Child Support Outcomes among Those with Knowledge of Child Support Policy

	In 1998					In 1999				
	N	Experimental Group	Control Group	Impact	P-value	N	Experimental Group	Control Group	Impact	P-value
(1) Percentage of Nonresident Fathers Paying Any Child Support										
All Nonresident Fathers	14,343	52%	50%	2%	0.083	14,343	56.3%	53.2%	3.1%	**0.005**
Survey Respondents	575	67	67	0	0.907	608	75.9	61.7	14.2	**0.000**
Knew Policy Rules in Time Period	65	78	78	1	0.957	69	96.6	74.6	22.0	**0.025**
(2) Annual Amount of Child Support Paid among All Nonresident Fathers										
All Nonresident Fathers	14,343	$798	$770	$28	0.228	14,343	$946	$891	$54	0.055
Survey Respondents	575	926	1,002	-75	0.460	608	1,438	1,179	259	**0.035**
Knew Policy Rules in Time Period	65	1,562	1,203	359	0.429	69	2,166	1,472	694	0.178
(3) Percentage of Resident Mothers Receiving Any Child Support										
All Resident Mothers	15,977	38%	35%	3%	**0.006**	15,977	47.8%	45.1%	2.7%	**0.014**
Survey Respondents	2,295	43	42	1	0.627	2,242	50.4	49.4	1.0	0.668
Knew Policy Rules in Time Period	549	55	51	4	0.434	543	58.3	57.0	1.3	0.768
(4) Annual Amount of Child Support Received among All Resident Mothers										
All Resident Mothers	15,977	$641	$499	$142	**0.000**	15,977	$848	$725	$123	**0.000**
Survey Respondents	2,295	614	547	67	0.098	2,242	866	786	80	0.160
Knew Policy Rules in Time Period	549	954	747	207	**0.023**	543	1,143	952	191	0.160

Note: First row in each panel shows outcomes regressed on extended list of control variables; second and third rows show basic list of control variables. Probability values of 0.05 or less are shown in bold type.

Appendix I.4.2

Effects of the Experiment on the Short-Term Stability of Paternal Involvement

This appendix examines the short-term change in nonresident fathers' involvement with children and the quality of parents' relationship with each other. The analysis reported in the main text examines differences at each time and is thus a cross-sectional analysis, even though the sample consists of mothers who were interviewed in both surveys. In this appendix we use data from both waves of the survey together to ask if the full pass-through increased fathers' participation in children's lives over the short term. The analysis examines experimental effects on the stability of fathers' involvement with children by looking at whether fathers changed their behavior between the two waves of the survey. Past research on separated families shows that nonresident fathers' participation in child rearing declines over time after the parents' relationship is dissolved (Seltzer, 1994). This analysis investigates whether the experiment slows down the deterioration of fathers' involvement with children.

We consider four outcomes: change between 1998 and 1999 in the number of days fathers spent time with children when they were living apart, whether families without intense conflict in 1998 experienced intense conflict in 1999, whether families without informal transfers in 1998 received transfers in 1999, and whether those whose transfers were worth less than $500 in 1998 (including those with no transfers) received transfers worth at least $500 in 1999. The analysis is structured as a difference-in-difference analysis and therefore uses only those cases in which the respondent reported any change in the outcome of interest. With that modification, the analysis uses the same sample as in the main text, that is, families in which the mother was the primary resident parent for at least half of the year. We continue to rely on resident mothers' reports because of the broader sample coverage and generally high quality of mothers' reports in the survey. Sample sizes for most analyses are quite small because of the exclusion of cases in which the dependent variable does not change. We examine experimental effects for all cases and for families in which the focal child was two years old or younger at the end of 1998. The table reports ordinary least squares-regression-adjusted or probit-regression-adjusted percentages as in the main text.

Appendix Table I.4.2, Panel 1, shows that contact declines for both experimental- and control-group families. The decline is larger for those in the experimental group, but the difference between experimental and control groups is not statistically significant. Among families with young children this difference is larger and is statistically significant. The experimental impact is in the opposite direction from that we expected. Additional analysis (not shown) suggests that the difference is robust across subsamples and some exclusion of outliers.

There is no difference between experimental and control groups in the development of intense conflict. Panel 2 of Appendix Table I.4.2 shows that among families whose level of conflict changed, 47 percent of mothers in the experimental group and 44 percent of those in the control group experienced an increase in the likelihood of intense disagreements between 1998 and 1999. However, the difference between the two groups is not statistically significant either for all cases included in the analysis or for those with young children.

The next two panels show differences between the experimental and control groups in changes in informal transfers between the two surveys. Appendix Table I.4.2, Panel 3, shows that, of those families whose transfer behavior changed between interviews, over a third who received no informal transfers in 1998 received something in 1999. There is no experimental impact shown in the row for all cases. Among families with young focal children, control-group families are somewhat more likely than experimental-group families to change in the direction of making informal transfers (32.9 percent compared to 12.3 percent). When we define the analysis sample slightly differently, to include families in which the father lived with the mother and child for most but not all of the year, the experimental effect is smaller and is only statistically significant using a more generous criterion ($p \leq .10$) (not shown).

Appendix Table I.4.2
Effects on Change in Family Relationships

	N	Experimental Group	Control Group	Impact	P-value
(1) Mean Difference in Days Father Saw Child When They Lived Apart (Time 2 – Time 1), among Families Who Changed					
All Cases	1,026	-14.1	-4.4	-9.8	0.130
Focal Child Age 2 or Younger in 1998	294	-54.8	-8.7	-46.1	**0.001**
(2) Percentage of Families Who Had High Conflict in 1999 and Did Not Have High Conflict in 1998, among Families Who Changed					
All Cases	552	47.4%	44.0%	3.4%	0.420
Focal Child Age 2 or Younger in 1998	133	41.9	33.9	8.0	0.355
(3) Percentage of Families Who Had Informal Transfers in 1999 and Who Had No Transfers in 1998, among Families Who Changed					
All Cases	350	38.8%	37.7%	1.1%	0.836
Focal Child Age 2 or Younger in 1998	99	12.3	32.9	-20.6	**0.022**
(4) Percentage of Families Who Had Informal Transfers Worth at Least $500 in 1999 and Transfers Worth Less than $500 in 1998, among Families Who Changed					
All Cases	163	49.2%	42.0%	7.2%	0.363
Focal Child Age 2 or Younger in 1998	62	23.8	44.1	-20.3	0.121

Notes: Table is based on cases in which mother is the primary resident parent for the focal child. Table includes only mothers interviewed in both 1998 and 1999. Table deletes cases for which mother ever reported that focal child or father had died. Table also excludes one case in which there was an instrument error in 1998. Cases that are missing on or for which there was no change in the dependent variable are deleted from the analysis. In Panel 1, means are adjusted using Ordinary Least Squares regression; in Panels 2–4, percentages are adjusted using a probit model. Probability values of 0.05 or less are shown in bold type.

Finally, Appendix Table I.4.2, Panel 4, shows that nearly half of families for whom the value of informal transfers changed between the two interviews received informal transfers worth less than $500 in 1998 but received transfers worth at least this much in the next year. This suggests that amounts of informal or in-kind transfers are fairly unstable. There are no experimental effects on the stability of amounts of informal transfers.

Taken together, our findings show no evidence for all cases included in the analysis that the experiment retards the deterioration of paternal involvement that characterizes most separated families. However, our results also suggest that the experiment may have a different effect on the stability of paternal involvement for families with young children. Among such families, fathers in the control group are more likely to maintain contact (less likely to reduce contact) with their children than fathers in the experimental group. Those in the control group may also be somewhat more likely to begin giving transfers than those in the experimental group. Fathers in the control group whose child support is partly withheld may prefer to provide for their children informally by giving children clothes and presents when they spend time together.

Parents of young children may show greater change as a result of the experiment than parents of older children, in part because fathers of many of these young children are very involved with the mother and their child. Parents share with each other information about the child support system and about the child's material needs. In addition, parents of young children are new to the system: they may not yet have established paternity for the child and acquired a formal child support order. Until they are part of the formal child support system, the pass-through policy is likely to have little effect on fathers' involvement with children. As children begin to grow up, the experiment seems to increase the likelihood of informal contributions to children from fathers in the control group compared to fathers in the experimental group, among families who changed. In evaluating the effects of a full pass-through policy, policy makers must balance the slightly greater likelihood of improvement in informal transfers for toddlers among those in the control group against the greater likelihood of formal child support for those in the experimental group.

For the sample including parents of both older and younger children, the lack of significant change at the individual level is generally consistent with the results presented in the main text. However, our findings in this appendix suggest that the largely null aggregate results presented in the main text may mask some changes at the individual level for families with young children—a segment of the population more likely to experience change than is the population as a whole.

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