

Welfare and Child Support: Complements, Not Substitutes

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Abstract

In most states, child support paid on behalf of Temporary Assistance for Needy Families (TANF) participants is used to offset TANF and child support administrative expenditures; this policy primarily benefits taxpayers. In contrast, Wisconsin allowed most custodial parents to keep all support paid on their behalf. This policy, which treats welfare and child support as complements, was evaluated through an experimental design. This paper reports the key results of the experimental evaluation, using state administrative data to examine the effects on child support outcomes and governmental cost. We find that when custodial mothers keep all child support paid on their behalf, paternity establishment occurs more quickly, noncustodial fathers are more likely to pay support, and custodial families receive more support. These outcomes are achieved at no significant governmental cost. © 2008 by the Association for Public Policy Analysis and Management.

A central issue underlying recent social policy debates has been the extent to which providing for low-income children is the responsibility of parents or the public. Moreover, when children are in a low-income household because they are living with only one parent, the relative responsibilities of the custodial parent (usually the mother) and the noncustodial parent (usually the father) are also contested. One key point at which these competing claims must be resolved occurs when a noncustodial parent pays child support on behalf of children receiving Temporary Assistance for Needy Families (TANF). In most states, this payment is retained by the government to offset TANF and child support administrative expenditures rather than going to families. An alternative policy would allow the custodial-parent family to benefit financially when child support was paid. Thus the treatment of child support for TANF participants determines whether taxpayers or custodial families benefit from child support payments to families receiving public benefits. Stated differently, in this case should private support (the noncustodial parents' child support payments) and public support (TANF benefits) be substitutes, or complements?

The 2006 TANF reauthorization bill includes a provision that encourages states to allow children to receive both child support and TANF. What effects might be expected if states were to adopt this approach? Would the policy have high costs? How might the policy affect the children, custodial parents, and noncustodial parents? An examination of the experience in Wisconsin provides insights into these questions. In the late 1990s, Wisconsin implemented a statewide random assignment experimental evaluation of a policy in which some TANF participants received all child support paid on their behalf with no change in their TANF grant, whereas

others received only a portion of the child support paid on their behalf. In this paper we report the results of this experiment, with particular attention to the effect of the policy on fathers' child support payments, mothers' child support receipts, paternity establishment, and government costs.

WELFARE POLICY: THE CONTEXT

The Aid to Families with Dependent Children (AFDC) program was intended to provide for the needs of low-income children in single-parent families. As a result of this focus on *need* combined with a desire to limit costs, only families with very low incomes were eligible for benefits, and income from any other source generally lowered the amount of the benefit. Consistent with this perspective, families with child support income usually had their AFDC benefit reduced dollar for dollar. Thus in early welfare policy history, private child support and public support for children were clearly seen as substitutes.

The federal child support program, initiated in 1975, retained this perspective.¹ To participate in AFDC, single parents faced several requirements: If child support were already due to them but had been uncollected, they were required to sign over the right to this support to the government. If they did not have a child support order, they were required to cooperate with the child support office in its attempts to establish an order. Although states had some freedom in how they treated current child support, in general all collections on behalf of welfare participants were used to offset child support administrative costs and welfare costs rather than benefiting the family, so that the total family income was unchanged whether or not child support was paid (what we term a "zero disregard").²

This policy was thought to be cost-effective, as public support went only to those most in need. But the policy had some obvious disadvantages. Why should custodial parents cooperate with the child support system if they would be no better off financially when support was paid? Why should noncustodial parents pay formal child support if their children would be no better off? Policymakers believed that parents would be more cooperative with the child support system if they had a financial incentive to participate. Thus, legislation in 1984 formalized a requirement that the first \$50 per month of current child support collected would go to the family, with the remainder of child support paid retained by the government to offset expenses. The \$50 per month limit was a compromise designed to give parents

¹ There have been numerous changes in child support policy since 1975. For a recent review, see Pirog and Ziolo-Guest (2006). We focus here on policies regarding the treatment of child support within AFDC and TANF. There have also been changes in the composition of the child support caseload since 1975. Although most custodial parents served by the child support enforcement system have been married, the program increasingly serves single mothers who have had nonmarital births.

² The use of the term "disregard" has some ambiguity in the case of child support. Typically "disregard" is used to mean an amount (or proportion) of a source of income directly received by the family that is ignored in the calculation of the benefit level. Thus, disregards of a particular dollar amount by definition result in an increase in total income of that dollar amount. Consistent with that approach, we treat all child support paid on the participant's behalf as potential income and then define the disregard as the change in the total income package due to child support. Note, however, that child support is unusual in that welfare participants sign over their right to support to the government, and then child support is typically collected by a state collection agency rather than given to parents directly. As a result, a state can control the total income provided to a family by changing either the size of the welfare check, the amount of child support check that is received by welfare participants (typically called the "pass-through"), or both. For simplicity of exposition, we focus here on the amount disregarded, equivalent to the amount by which the family's total income is increased. We consider child support that is provided to the resident parent at a dollar-for-dollar cost in terms of public benefits to be a "pass-through" without a disregard. For a more detailed discussion of the relationship between disregard and pass-through policy, see Cancian, Meyer, and Roff (2007); see also Wheaton and Russell (2004).

some incentive to cooperate while maintaining provisions to use most support to offset government costs.³

The 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) replaced AFDC with TANF. TANF provisions increased the potential importance of child support as an income source for low-income, single-parent families in that TANF benefits were time-limited and cash assistance no longer an entitlement. These changes made nonwelfare sources of income more essential.

PRWORA also eliminated the requirement for a \$50 per month disregard of child support and allowed states substantial flexibility in determining how to handle child support paid on behalf of families receiving TANF. A majority of states now retain all child support paid on behalf of TANF families. Most of the remaining states continue to have a \$50 per month disregard (Justice, 2007).

In contrast, in Wisconsin implementation of the TANF program, Wisconsin Works (W-2), coincided with a 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 was distributed to custodial-parent families and did not affect the level of the TANF check they receive (a full disregard).⁴ Thus, total family income rose by the amount of child support paid. (As we describe here, some families in Wisconsin were randomly chosen to face a different policy in which they received only a portion of what was paid; in this section, we describe the policy faced by most TANF families in the state.) This treatment of child support is consistent with W-2's guiding philosophy that called for the TANF program to be modeled after the labor market: Because regular employees do not have their wages adjusted based on child support receipt, the W-2 program, which generally requires participants to engage in a work activity in exchange for benefits,⁵ should also ignore child support in the calculation of benefits.⁶

Under the initial TANF rules, states interested in a full disregard faced major financial disincentives. Child support paid on behalf of welfare participants was generally retained and then divided between the federal and state government in

³ A few states ("fill-the-gap" states) implemented a different type of policy. These states compensated for low AFDC benefits in part by being more generous with child support. In particular, although most states' AFDC benefit levels matched their established standard of need (the amount that was thought to be necessary to support a family of a given size), a few states adopted a maximum benefit amount that was less than the standard of need, but which might reflect the amount they thought the state could afford. In these states, if child support was paid, it first went to fill the "gap" between the standard of need and the benefit level. Until the gap was filled, a family's total income was not reduced for any child support collected. Thus, these states effectively had a disregard.

⁴ We consider Wisconsin to have had a full disregard because total income increased by one dollar for every dollar of current support paid. Note, however, that amounts paid for past-due support and amounts collected through intercepting tax refunds followed different distribution procedures. Moreover, prior to March 1, 2000, child support was counted as income in determining eligibility for W-2 and the level of the child care co-payment, but did not count in terms of the level of cash received. Beginning March 1, 2000, child support no longer was counted in determining eligibility for W-2 or child care.

⁵ W-2 is structured with a series of "tiers," or types of placements, with the expectation that participants would work their way up the tiers. The lowest tier, W-2 Transitions, is for those who are least work-ready; in exchange for their W-2 check, they are required to participate in some type of work-related activity. Community Service Jobs require work, primarily in government or not-for-profit organizations, in exchange for a W-2 check. Trial Jobs, a third category, was conceived as a private-sector placement in which the employer was paid a subsidy to provide employment; this category has been used very infrequently. In the top tier, participants receive work supports while they work in the private sector. Placement to tiers is designed to be based on individuals' work backgrounds and skills.

⁶ Policy in Minnesota provides an interesting contrast. Minnesota has a full pass-through of child support—that is, all current child support collected on behalf of TANF families is forwarded to them. However, Minnesota does not disregard any support—that is, the TANF check is decreased by a dollar for every dollar of child support. Thus custodial families receive all child support paid on their behalf, but they are no better off financially when it is paid.

proportion to the federal and state contributions to welfare expenditures.⁷ Thus, states that wanted to give the entire child support amount to families would face a double cost under PRWORA: Not only would they have to forego their own share of the child support collected, but they would also have to reimburse the federal government for the federal share that was now going to families. Wisconsin was spared repaying the federal cost through a waiver, in part because a random-assignment evaluation of the policy was conducted, as we describe later.

The reauthorization of TANF in 2006 changed policy again. Beginning in 2008, states that increase their disregard up to \$100 per month for one child and \$200 per month for families with two or more children will no longer have to reimburse the federal government for its share of these child support collections. How should states respond? What effects might an increased disregard have?

LITERATURE REVIEW: POTENTIAL EFFECTS OF POLICY

At least in the short run, child support disregard policy is essentially a choice between support directly benefiting families receiving welfare benefits or instead benefiting taxpayers through lower governmental expenditures. We have outlined three different approaches: treating child support and welfare as substitutes (the government retaining all child support, a zero disregard), treating them as complements (allowing custodial parents to keep both private and public support, a full disregard), and a hybrid approach (dividing child support between parents and the government, a partial disregard). The clearest effect of a full disregard (compared to a partial disregard or no disregard) should be that custodial families receive more child support and taxpayers (the government) retain less and thus have higher costs. But the policy could affect noncustodial parents as well as custodial families and taxpayers. Prior research provides insights into potential effects of the disregard on these three groups: noncustodial parents, custodial families, and taxpayers. In this review, we focus on effects on child support payments and receipts, paternity establishment, and governmental costs, rather than the range of possible effects (see Wheaton & Russell, 2004, for a discussion of selected other effects of child support distribution policies, and Pirog & Ziolk-Guest, 2006, or Garfinkel, McLanahan, Meyer, & Seltzer, 1998, for a broader discussion of the effects of child support enforcement).

The level of support the government retains could affect the way noncustodial parents interact with the child support system. Previous child support research has used a model in which the amount of child support noncustodial parents pay is related to factors including their ability to pay support, their willingness to pay support, and the policy environment (for example, Bartfeld & Meyer, 2003; Beller & Graham, 1993; see also Weiss & Willis, 1985). This model predicts that noncustodial parents 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 (for example, Waller & Plotnick, 2001) has suggested that noncustodial fathers of children receiving welfare realize that any child support paid through the formal system does not fully benefit their children, and are reluctant to cooperate given this policy.

If a full disregard has an effect on payments, this could occur either through more noncustodial parents paying formal support or through increases in the average amount paid among those who pay. Prior research suggests that the larger effect

⁷ PRWORA replaced the AFDC program, co-financed by state and federal resources, with the TANF block grant. However, Medicaid remains a jointly financed program and child support retained is split according to the Medicaid match rate.

would be on the percentage paying rather than the amount paid: To the extent that noncustodial parents 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 (Bartfeld & Meyer, 2003). The primary avenue available to a parent who seeks to avoid support payments is to work “under the table.” Thus, a disregard would be more likely to affect whether noncustodial parents are in formal employment and whether they pay any formal support than to affect the amount paid.⁸

Disregard policy can also affect custodial families. By definition, a full disregard will directly result in higher total incomes for those with child support receipts. But there may also be other effects. Research has indicated that in the prior \$50 partial-disregard system, some parents strategically collaborated (Edin, 1995). That is, in exchange for the custodial parent not providing information on the noncustodial parent, the noncustodial parent agreed to pay child support informally, which allowed the custodial parent to keep all child support paid (Edin, 1995; Waller & Plotnick, 2001). Thus, in the absence of a disregard, there is a greater incentive for parents to collaborate to avoid formal paternity establishment, and an order for child support. One effect of a full disregard may be that custodial parents cooperate more with the child support enforcement system, and this could lead to increases in paternity establishment as well as increases in child support orders and payments.

If total child support payments increase, there may be several secondary effects on custodial parents. For example, an increase in child support received might also be expected to reduce the need for custodial families to participate in the Food Stamp and Medicaid programs (Barnow, Dall, Nowak, & Dannhausen, 2000; Wheaton & Sorensen, 1998; 2005). The effect on TANF participation is ambiguous: A full disregard might be associated with an increase in TANF participation, because those who receive more child support are now allowed to continue receiving benefits. Alternatively, it may be associated with a decline to the extent that increased child support payments may facilitate a transition off benefits (Huang, Garfinkel, & Waldfogel, 2004; Miller, Farrell, Cancian, & Meyer, 2005).⁹

These changes in benefit receipt then affect government costs (the costs to taxpayers). The ultimate costs of the policy are difficult to predict based on prior research. Obviously, foregoing child support retentions is a cost. If child support changes the custodial parent’s participation in benefit programs, this affects governmental costs (Barnow et al., 2000; Wheaton & Sorensen, 1998, 2005). In addition, a simpler system in which all payments go directly to the custodial parent, rather than being divided at the end of each month, could lead to lower administrative costs in the child support system.¹⁰ The ultimate fiscal implications of a policy to disregard all child support will depend on the extent to which increases in cooperation with the child support enforcement system and changes in program participation compensate for the loss in revenue previously collected from child support payments to families receiving public support.

⁸ It is possible that a full disregard may not change a father’s total contributions but merely change the extent to which he pays formally rather than informally. In related work, considering survey reports of informal support paid, there is no statistically significant difference in the level of informal support between the experimental and control groups (Meyer & Cancian, 2001).

⁹ Mothers might change their labor supply in response to support (for example, Graham & Beller, 1989; Hu, 1999; see the review in Pirog & Ziolk-Guest, 2006), either decreasing it in response to increased non-labor income or increasing it if child support facilitates the transition off benefits and into the workplace.

¹⁰ Wheaton and Sorensen (2005) report that administrators in most states they contacted believed there would be little change in administrative costs associated with changes in the pass-through and disregard. However, the administrator in Wisconsin reported lower costs associated with customer service.

EVALUATING DISREGARD POLICY: DESIGN, METHODS AND DATA

Rationale and Structure of the Experimental Evaluation

A random-assignment experiment is a powerful approach to policy evaluation (Moffitt & Ver Ploeg, 2001). If the random assignment is conducted properly, the experimental and control groups should be equivalent on every dimension except the policy regime they face (and differences that occur by chance), and thus any difference in outcomes between the experimental and control groups can be attributed to the policy. The full disregard of child support in Wisconsin was implemented as a random-assignment experiment called the Child Support Demonstration Evaluation (CSDE). Most parents in the state were randomly assigned to receive the full amount of child support paid on their behalf with no change in their TANF check (the experimental group). A randomly selected group of parents (the control group) received only a portion of the child support paid when they received TANF, with the remainder being retained by the government to offset costs; these families received the first \$50 per month paid on their behalf, or 41 percent of the amount paid on their behalf, whichever was more.¹¹ On August 31, 1997, the automated management information system for AFDC and TANF randomly assigned all existing AFDC cases to either the experimental group (the full disregard) or to the control group (a partial disregard).¹² For most of the first year of the program, when new families requested assistance from W-2, they were also randomly assigned to one of these two groups.¹³

Under the CSDE, the formula for the control group results in three ranges of child support disregard 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. Which of these ranges would be most typical for a TANF participant? In the month that custodial mothers entered W-2, no child support was paid on behalf of 82 percent of the mothers. Among those receiving support, in only about 10 percent of the cases was less than \$50 paid; in almost a third of cases, payments were between \$50 and \$122 (the second range); and in over half of the cases, more than \$122 was paid (the third range). As discussed further below, the percentage receiving any support and the mean amount of support received increase substantially after entry for both the experimental and control groups. Thus, over time more and more mothers were in the third range—in which the difference in treatment between the experimental and control groups is greatest.

¹¹ 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* support; amounts for past-due support and amounts collected through intercepting federal tax refunds both follow different distribution rules, primarily going to the government.

¹² Participants were supposed to be told the rules that governed child support for them, but there were some difficulties in implementation (Meyer & Cancian, 2001); survey evidence suggests that some participants did not understand the rules (Meyer, Cancian, & Nam, 2007).

¹³ 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. Random assignment was restarted in Milwaukee on January 1, 1999, continuing through June 30, 1999. However, most cases entered before July 9, 1998, and only this “original cohort” of the CSDE is included in the primary experimental analysis reported here.

Experimental status affected the amount of child support received only in months in which a family was receiving TANF cash benefits. Some W-2 participants receive only noncash services for some or all of the time they are W-2 participants. In a month in which they received only services, these families received the full amount of child support regardless of experimental-group status. Once control-group members left W-2, they received the full amount of current support paid on their behalf.

Given patterns of child support and welfare participation, the experimental-control difference was somewhat limited for many families. For some custodial families, child support was extremely unlikely, so experimental status was irrelevant. In other families, the amount of support that could be paid was low, so differences in the amount received did not vary much by experimental status. Finally, some families received only services and were not affected, or left a cash-paying TANF placement quickly so there was only a limited period of time in which experimental-group status was relevant.

Even though the actual intervention was limited, the random assignment model provides a powerful tool to estimate the effects of the intervention. There may be many potential effects of the child support disregard; in this paper, we focus on the main effects: child support payments and receipts, paternity establishment, and government costs.

Data and Methods

Data

We rely on Wisconsin's administrative records of child support and TANF. The TANF database includes information on experimental group status, demographic characteristics, W-2 participation, and participation in related programs (food stamps, Medicaid, BadgerCare [Wisconsin's State Children's Health Insurance Program (SCHIP)], and child care subsidies).¹⁴ The child support database includes information on non-custodial parents, paternity establishment, child support orders, and payments.

Samples

Our primary analysis considers outcomes for a single cohort of custodial mothers who first participated in W-2 between September 1997 and July 1998, their children, and associated noncustodial fathers. We exclude custodial fathers and non-custodial mothers because there are very few and their patterns may differ. Our base sample of custodial mothers includes all those who had a random assignment code, had entered W-2 by July 8, 1998, were demographically eligible for child support (that is, the father was not known to be dead and was not married to and living with the mother), still had a child aged less than 18 at the end of our three-year observation period, and met other sample criteria primarily associated with timely progression in the intake process.¹⁵ Analyses showed no significant differences

¹⁴ We use the actual amount of W-2 payments, Food Stamp benefits, and child care subsidies. For Medicaid and BadgerCare, we are able to identify those who participate in the program. For costs per participant for both these programs, we use the average per-person HMO capitation rate within a health-cost area (as defined by the Wisconsin Department of Workforce Development).

¹⁵ See Cancian Caspar, and Meyer (2001) for details. Of the 24,077 that entered W-2 during this period, about 1,100 did not receive a research group assignment due to a programming error; about 800 cases were not eligible for child support; about 3,400 cases had extensive post-assignment delays in entering W-2 (for example, left AFDC and did not enter W-2 within two months); 1,800 had a child receiving Supplemental Security Income (SSI) and thus faced different child support rules; 600 had children too old to be eligible for support throughout the observation period; and 400 were custodial fathers. Analyses show that experimental-group and control-group mothers had similar rates of entry onto W-2 from AFDC.

between the experimental and control groups in the rate of entry into W-2 (Cancian, Caspar, & Meyer, 2001). Our final sample includes 15,890 mothers. Note that we do include mothers who entered W-2 in a placement where they received services only (because placement could have been influenced by experimental-group status).¹⁶

We then select all children living with these mothers at the time of entry into W-2 who would have been less than age 18 at the end of our 36-month observation period, totaling 35,282. We examine the child support record to see whether they have “legal” fathers (that is, whether they were born to married parents or paternity had been legally established). About half of the W-2 children had a legal father established, leaving 14,887 children who did not have a legal father at the time of entry into W-2, the sample we examine in the analysis of paternity establishment.¹⁷

Among the 15,890 mothers, some were associated with multiple legal fathers, some with only one legal father, and some with no legal fathers. Our sample of non-custodial fathers is limited to legal fathers because we are unable to identify biological fathers who have not been legally determined. At the time of the mother’s entry into W-2, there were 13,616 couples including a legally established father, and we examine the fathers in these couples.¹⁸

Appendix Table 1¹⁹ shows characteristics of our samples of mothers, children without paternity established, and fathers. As might be expected in a TANF sample, mothers (and the fathers associated with them) generally have low levels of human capital. For example, about half the mothers had not completed high school, and about half were long-term AFDC recipients (having receiving AFDC in more than 18 out of the 24 months before entering W-2).

Subgroups

In addition to our analysis of the entire sample, we analyze two key subgroups: those with a child support order at the time of W-2 entry and those without recent AFDC history (the mother did not receive AFDC for any of the 24 months prior to W-2 entry). We hypothesize that the policy intervention will have larger effects for those with a child support order at entry, in part because the treatment of child support would be particularly salient to them. If those with exposure to the old AFDC system have a muted response to the new policy because of long-standing patterns, then the effects on new cases (our second key subgroup) will be a better estimate of long-term effects.²⁰

¹⁶ For example, a worker may have assigned a control-group member to a services-only placement so that she could keep all child support paid on her behalf. Analyses show that control-group members who had received higher amounts of child support in the past were somewhat more likely to be placed in non-cash placements than comparable experimental-group members. As a result, we include mothers in our analysis regardless of initial tier, and we control for whether this mother received higher child support in the past (see Meyer & Cancian, 2001). Alternative specifications, in which only mothers who enter into cash-paying tiers are included in the sample, lead to the same conclusions.

¹⁷ Of these children, 38 percent were the only child of the mother in the W-2 case, 34 percent were one of two children, and 29 percent were one of three or more. An analysis that allows for the possibility that these are not independent observations reaches similar conclusions to those reported below in the base results: Relationships that are reported to be significant at the $p < .05$ level generally remain significant at $p < .05$ when robust standard errors that allow for clustering are estimated.

¹⁸ Six percent of the individual fathers had children with more than one of the mothers in our sample. Because our analysis is of couples, these fathers enter our analysis more than once. An analysis that allows for the possibility that these are not independent observations reaches identical conclusions.

¹⁹ All appendices are available at the end of this article as it appears in JPAM online. Go to publisher’s Web site and use the search engine to locate article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

²⁰ On the other hand, those who enter W-2 during 1997–98 who did not have recent AFDC receipt may be a select group in that they entered TANF during a period of a strong labor market. As seen below, our results do not suggest that they are particularly disadvantaged, at least in terms of observable child support outcomes.

Time Frame

We examine three years following the mother's initial entry into W-2. We follow all mothers for the full 36 months, regardless of the month in which they entered the program or the time they remain in W-2.²¹ Some outcomes depend on processes that take time. For example, a mother may have been told about the full disregard when she entered W-2 and, as a result, decided to pursue paternity establishment aggressively. If a putative father contests paternity, a legal process would be needed and this would typically take some time,²² so the effects on paternity establishment may not be seen immediately. On the other hand, the disregard policy only directly affects those receiving TANF cash benefits; those no longer receiving these benefits receive the full amount of child support paid, whether they are in the experimental or control group. This would tend to limit the impact of the intervention as time passes if time receiving TANF is relatively short. Unfortunately, an implementation error occurred between September 2000 and February 2001, in which a computer error resulted in some of the control group members inadvertently receiving the full disregard. This affected the third year of treatment for some of the control group cases that continued to receive W-2, a small group. As a result, although we provide information over the three-year period for all cases, we pay most attention to the first two years. We include the full three-year period in order to observe medium-term effects.²³

Methods

Our strategy to estimate the impact of the full disregard is straightforward, presenting regression-adjusted comparisons between the experimental and control groups. These comparisons derive from a multivariate analysis in which we control for differences in the characteristics of sample members at W-2 entry (that presumably occurred by chance).²⁴ For dichotomous outcomes (for example, whether paternity was established) we estimate probit models; analyses of continuous outcomes

²¹ More precisely, we begin our analysis with the calendar quarter that follows entry (with the exception of a small number of cases that were the very first W-2 entrants in late September, 1997, for whom we begin the analysis in January 1998). Thus, first year outcomes correspond to the 12 months from January to December 1998 for the earliest entrants, and the 12 months from October 1998 to September 1999 for the latest entrants.

²² A brief analysis of court cases in selected Wisconsin counties shows that on average the time between a court petition and a paternity adjudication was about 90 days when paternity was admitted by the father, and around 200 days when the father denied paternity or admitted paternity contingent on blood tests.

²³ Interpreting longer term effects is complex. On June 30, 2002 (in the fourth year of participation, for most cases) all W-2 participants became eligible for a full disregard—regardless of experimental or control status. Because the control group and the experimental group were subject to the same policy in the fourth year, we do not compare outcomes for this period. For a discussion of longer-term effects and a summary analysis that is related to the analysis presented here, see Cook and Caspar (2006). Using the methodology and sample in this paper on outcomes in years four through six shows no statistically significant effects on the percentage of fathers paying support among the sample as a whole. Fathers whose partners had not received AFDC recently were more likely to pay in the fourth and fifth year ($p = .051$ and $.059$), but the effects on the log amount paid were not statistically significant at conventional levels. Consistent with the identical treatment of the experimental and control-group mothers, there was no statistically significant difference in child support receipts, with the exception of the fourth year for mothers with no recent AFDC history, when the experimental group received higher (log) amounts. There continue to be no statistically significant differences in net costs.

²⁴ In addition to controlling for differences that may have occurred by chance, we control for selected other variables that affect the outcomes of interest. To the extent that these control variables account for variance in our outcomes, we are more likely to be able to discern the effect of the experiment if we include them (see, e.g., Bloom, 2006). Variables in our analyses of mothers, children, and fathers are shown in Appendix Table 1. Appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

(amount of child support paid, amount received, and governmental costs) use ordinary least squares regressions. For the analyses of child support payments and receipts, we present separate analyses of whether something was paid or received, and the natural log of amount paid or received.²⁵ As discussed above, we expect that the determinants of any payment may differ from the determinants of level of payment.²⁶

RESULTS

For each outcome of interest, we first discuss levels and trends for control-group members. These measures indicate, for example, how much child support would have been paid or what proportion of paternities would have been established in the absence of the intervention. We then report experimental impacts.

Effects for Fathers

Among fathers in the control group, about half (51 percent) paid support in the first year, a number that increases to 55 percent by the third year. Fathers who had orders when their partners entered W-2 were somewhat more likely to pay, 60 percent in the first year and 65 percent in the third. In contrast, fathers whose partners were new to the welfare system were more likely to pay in the first year (54 percent), but the number paying declined over time, to 50 percent in the third year.²⁷ The increases in the likelihood of payments over time for most fathers may reflect the effectiveness of enforcement actions, improvements in the ability to pay support, or other factors (see, for example, Meyer & Bartfeld, 1998). Among all fathers in the control group, those who paid support paid an average of \$1,500 in the first year, increasing to \$1,700 and \$1,800 in the second and third years. Amounts for those with orders at W-2 entry are quite similar. Among fathers new to the welfare system who paid support, amounts were substantially higher (\$2,100 in the first year) and grew more rapidly (\$2,900 in the third).

We hypothesized that fathers in the experimental group would be more likely to pay some child support. Consistent with our hypothesis, we find higher proportions of experimental-group fathers paying support, as shown in Table 1. The differences are statistically significant among all fathers, and in the third year also attain significance at conventional levels for both the subgroups.

Evaluating the impact at the mean shows a difference between the experimental and control groups of 2.0–3.1 percentage points (4–6 percent). In the first year, the effects are larger for fathers who had an order at entry. This is not surprising: Those who had an order when their partners entered W-2 could begin paying formal

²⁵ We do not show a separate analysis of whether there are any costs for a mother because nearly every mother in the sample participated in some program with associated costs. We use a log-linear specification because we generally expect the effect of both experimental status and other control variables to be proportional, rather than absolute (that is, for the largest group of recipients, the amounts received by those in the control group should be 41 percent of those received by the experimental group). Tests of goodness of fit also suggest this is the preferred specification for the sample as a whole. We add \$1 to each amount before the log transformation to accommodate the zeros.

²⁶ As sensitivity tests, we also estimated tobit analyses of the log amount paid and the log amount received. These analyses lead to very similar conclusions.

²⁷ The payment patterns reported here reflect changes in payments among fathers with legally established paternity at the time the mother entered the sample. Below, we discuss child support receipt among mothers regardless of whether paternity has been established for their children. Thus, patterns of child support receipt reported for mothers reflect both changes in payments from fathers for whom paternity had been established prior to entry and payments from fathers for whom paternity was established after entry.

Table 1. Experimental effects for fathers.

Experimental	First Year			Second Year			Third Year		
	Experimental Effect	Std. Error	p-value	Experimental Effect	Std. Error	p-value	Effect	Std. Error	p-value
Percentage Paying Child Support									
All fathers	0.050	0.029	0.086	0.062	0.028	0.029	0.079	0.028	0.005
Couple has order at entry	0.095	0.033	0.004	0.071	0.033	0.030	0.090	0.032	0.006
Mother has no recent AFDC history	0.150	0.106	0.159	0.146	0.104	0.158	0.314	0.103	0.002
Log Amount of Child Support Paid									
All fathers	0.094	0.061	0.124	0.160	0.064	0.013	0.175	0.066	0.008
Couple has order at entry	0.165	0.071	0.021	0.172	0.075	0.021	0.213	0.076	0.005
Mother has no recent AFDC history	0.301	0.223	0.177	0.304	0.238	0.203	0.580	0.242	0.017

Note: Legal fathers are those whose paternity was legally established when the mother entered W-2. Sample sizes are 13,616 (all fathers), 10,261 (order at entry), and 966 (no recent AFDC history). Table shows experimental effects from regression models; co-variables reported in Appendix Table 1. \$1 was added to the amount paid before taking the natural log to accommodate the zeros. All appendices are available at the end of this article as it appears in JPAM online. Go to publisher's Web site and use the search engine to locate article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

support fairly quickly; those without an order would generally not begin paying until an order was in place. The largest impacts occurred among those whose partners did not have recent AFDC experience. Among these fathers in the first year, there was an experimental-control difference of 5.9 percentage points evaluated at the mean (not statistically significant), and the impact is 12.3 percentage points, or 25 percent (and statistically significant) in the third year.

Fathers of children in the experimental group paid higher amounts than fathers in the control group in all years, though the difference in the first year is not statistically significant. By the third year, those in the experimental group are predicted to pay 19 percent more than those in the control group.²⁸ We find consistent effects for the subgroups. The estimated effects are largest among fathers new to the welfare system, but for this subgroup are statistically significant at conventional levels only in the third year.²⁹

Effects for Mothers

The analysis of child support receipts among mothers is related to the analyses of payments among fathers but differs primarily because the analysis of custodial mothers includes some cases in which the fathers had not been legally established at the time of entry into W-2. As a result, the proportion of mothers who receive support is less than the proportion of (legally recognized) fathers who pay support. Moreover, our payment analysis treats 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 (at entry about one-quarter of the mothers who enter W-2 are associated with more than one father).³⁰

Among mothers in the control group, 37 percent received support in the first year. This is substantially higher than the national average, in which child support was collected for 24 percent of TANF cases in 1999 (U.S. Department of Health and Human Services, 2000). The higher figure largely reflects Wisconsin's continued effectiveness in collecting support (for example, Sorensen & Zibman, 2000). The proportion receiving support increases to 52 percent by the third year. Some of this increase reflects support from "new" fathers, that is, those who had not had paternity established prior to W-2 entry. Mothers who had child support orders when they entered W-2 were more likely to receive support (64 percent in the first year, growing to 70 percent in the third). The likelihood of receiving support is lower for those new to the welfare system (30 percent in the first year, rising to 45 percent in the third).

Among all mothers in the control group, those who received support received an average of \$1,400 in the first year, increasing to \$1,600 and \$1,800 in the second and third years. These figures are roughly comparable to the national average of collections among welfare cases in which a collection was made of \$1,545 (U.S. Department of Health and Human Services, 2000). In other states, however, very

²⁸ Estimates of proportional effects can be calculated by exponentiating the coefficient.

²⁹ We estimated a series of alternative models to test the sensitivity of these results. The direction and statistical significance of effects is generally robust to alternative specifications. For example, a tobit model of log payments generally shows similar patterns of significant effects. However, point estimates vary substantially across some specifications.

³⁰ In addition, the amount paid does not necessarily equal the amount received, even under Wisconsin's reformed child support policy. For example, if a noncustodial 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 custodial mother. In addition, if a noncustodial 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.

little of the child support collected for welfare recipients is received by the families themselves, whereas in Wisconsin even control-group members received 41 percent (or \$50 per month) of what was paid. In the Wisconsin control group, receipt amounts for those with orders at W-2 entry are quite similar to the whole sample. Among those who received support and were new to the welfare system, amounts were substantially higher (\$2,100 in the first year) and also grew over time (\$2,500 in the third).

Table 2 shows that mothers in the experimental group were significantly more likely to receive child support than mothers in the control group in the first year. This difference in the likelihood of receiving support is fairly small (2.7 percentage points when evaluated at the mean), but it is statistically significant in the first year and is related to differences in behavior, not to the mechanical effect of the reform. The difference between the experimental and control group is not statistically significant by the third year. Examining the subgroups, there are statistically significant effects among those with a child support order at entry for all three years. Similar to the analysis for fathers, the effects were larger among mothers with no recent AFDC history, with effects of 5.9 percentage points in the third year when evaluated at the mean.

As discussed above, the full disregard should automatically result in higher average child support receipts for the experimental group, as those in the control group will have a portion of the payments retained by the government when they are in cash-paying placements within W-2. Thus, as long as mothers spend some time in this type of placement and as long as the child support paid on a mother's behalf is at least \$50 per month, mothers in the experimental group should receive more child support than mothers in the control group. The experimental impact is not, however, necessarily limited to this mechanical impact. If fathers pay more, this could increase receipts. Note, however, that during periods in which mothers are no longer receiving TANF payments, the experimental and control groups are treated identically. Thus as time passes and more mothers leave TANF, we anticipate that experimental-control differences in receipts may decline.

The table shows that mothers in the experimental group received more in (log) support in each year. The effect is statistically significant in each year: Experimental group mothers receive 23 percent more in the first year and 12 percent more in the third. Mothers with orders at entry and those with no recent AFDC history also show significant effects in each year. Although the point estimates suggest that the effect among the whole sample and among those with orders at entry declines over time, and the effect among those new to the welfare system is increasing over time, the differences in the estimated impacts in the first and third year are not statistically significant.

Effects for Nonmarital Children

Among nonmarital children in the control group who did not have paternity established when their mothers entered W-2, about 16 percent had paternity established by the end of the first year, and 39 percent had paternity established by the end of the third year. In part, this reflects that many children were very young when their mothers entered W-2, and initial attempts at paternity establishment were successful. Despite these improvements over time, less than half of the children without paternity established at entry had paternity established by the end of the third year, confirming other research showing that substantial gaps remain in the child support system (Bartfeld, 2003).

Given the expectation 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, we would expect to see higher rates of paternity establishment in the experimental group. Table 3 shows that in the first year, a higher percentage of

Table 2. Experimental effects for mothers.

	First Year			Second Year			Third Year		
	Experimental Effect	Std. Error	p-value	Experimental Effect	Std. Error	p-value	Experimental Effect	Std. Error	p-value
Percentage Receiving Child Support									
All mothers	0.070	0.029	0.017	0.052	0.028	0.058	0.030	0.027	0.262
Has order at entry	0.081	0.036	0.025	0.072	0.035	0.041	0.073	0.035	0.038
No recent AFDC history	0.141	0.076	0.064	0.131	0.072	0.067	0.149	0.070	0.034
Log Amount of Child Support Received									
All mothers	0.207	0.050	<.0001	0.182	0.055	0.001	0.116	0.058	0.047
Has order at entry	0.288	0.073	<.0001	0.264	0.077	0.001	0.217	0.079	0.006
No recent AFDC history	0.298	0.135	0.027	0.347	0.152	0.023	0.356	0.158	0.024

Note: Sample sizes are 15,890 (all mothers), 9,140 (order at entry), and 2,052 (no recent AFDC history). Table shows experimental effects from regression models; co-variables reported in Appendix Table 1. \$1 was added to the amount received before taking the natural log to accommodate the zeros. All appendices are available at the end of this article as it appears in JPAM online. Go to publisher's Web site and use the search engine to locate article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

Table 3. Experimental effects for children.

	First Year			Second Year			Third Year		
	Experimental Effect	Std. Error	p-value	Experimental Effect	Std. Error	p-value	Experimental Effect	Std. Error	p-value
Paternity Establishment among Children without Legal Fathers at Entry									
All children	0.061	0.031	0.053	0.017	0.028	0.555	-0.039	0.027	0.154
Mother has no recent AFDC history	0.216	0.085	0.011	0.215	0.084	0.011	0.131	0.086	0.127

Note: Sample sizes are 14,887 (all children) and 1,575 (no recent AFDC history). Table shows experimental effects from regression models; co-variables reported in Appendix Table 1. All appendices are available at the end of this article as it appears in JPAM online. Go to publisher's Web site and use the search engine to locate article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

those in the experimental group have paternity established ($p = .053$), but there is no statistically significant effect in the second or third year. Thus for the sample as a whole, our results suggest that children in the experimental group had paternity established faster, but with no difference by the end of the period. The impact among children of mothers without recent AFDC experience was larger and statistically significant at conventional levels in the first two years.

Effects for Government

We examine two types of governmental costs: the costs associated with foregoing a portion of child support receipts, and those associated with participation in several income-support and work-support programs—TANF (and, in the early part of the period, AFDC), Food Stamps, Medicaid, SCHIP, and child care subsidies. We do not estimate administrative costs or savings. In the first year, the average cost per control-group mother for program participation is about \$11,600 and declines to about \$9,600 by the third year. Costs are highest for the medical support programs, averaging over \$4,000 per mother in the first year compared to about \$3,000 for W-2, \$2,000 for Food Stamps, and \$2,000 for child care subsidies. Amounts are lowest for those with no recent AFDC history, as these mothers tend to move off benefits quickly, with total costs for participation averaging \$7,900 in the first year and \$6,800 in the third. Table 4 shows that although log program participation costs are lower for the experimental group, the differences are not statistically significant at conventional levels. Among those with an order at entry, costs for the experimental group are lower in the first year, but the differences do not continue to be statistically significant.

We next examine the amount of support retained by the state. Among all control-group mothers, about \$250 is retained in the first year, an amount that declines to about \$210 in the third. We expect the amount of child support retained to be less for those in the experimental group but note that even for these mothers, some child support is retained, primarily payments on back support, payments toward reimbursing Medicaid for the costs of a nonmarital birth, and payments that come through intercepting tax refunds, all of which were retained by the government even for the experimental group. Table 4 shows that the amount of (log) support retained by the government is lower among the experimental group, but the difference generally declines over time, in part because mothers do not stay on TANF very long. In fact, half the early W-2 participants left the program within the first six months, and about three-quarters within a year (Cancian, Meyer, & Wu, 2005).

Our measure of net costs is the amount spent on program participation minus the amount of child support retained. For control-group mothers, this averages \$11,300 in the first year, declining to \$9,300 in the third. The bottom panel of Table 4 shows that there were no statistically significant differences in log net costs to the government for the experimental and control groups. Thus, although the government gave up a portion of child support, the amounts given up are not large and are generally offset by small savings elsewhere in the system.

A cost/benefit analysis which included a more extensive list of government costs and benefits (Caspar & Cook, 2005) also found that, from a whole government perspective, the amount of child support foregone is largely offset by other savings. They found that the largest cost offset was the child care subsidy, with those in the experimental group receiving a lower average subsidy amount, by about \$70 per participant per year. Caspar and Cook were not able to identify why this occurred,³¹

³¹ Because child support was counted as income in determining eligibility for child care subsidies during the first two and a half years of the experiment, it could be the case that higher child support was causing fewer experimental cases to qualify for the subsidy. However, the experimental-control difference continues even after child support was excluded as income for determining subsidy eligibility.

Table 4. Experimental effects for the government.

	First Year			Second Year			Third Year		
	Experimental Effect	Std. Error	p-value	Experimental Effect	Std. Error	p-value	Experimental Effect	Std. Error	p-value
Log Cost from Mothers' Participation in W-2, Food Stamps, Medicaid, SCHIP, Child Care Subsidies, and AFDC									
All mothers	-0.030	0.020	0.140	-0.001	0.050	0.989	-0.015	0.060	0.810
Has order at entry	-0.056	0.025	0.023	-0.092	0.059	0.123	-0.122	0.076	0.107
Has no recent AFDC history	-0.016	0.062	0.794	0.162	0.155	0.297	0.002	0.176	0.993
Log Amount of Child Support Retained by the Government									
All mothers	-0.639	0.042	<.0001	-0.459	0.045	<.0001	-0.318	0.045	<.0001
Has order at entry	-0.971	0.067	<.0001	-0.705	0.070	<.0001	-0.415	0.069	<.0001
Has no recent AFDC history	-0.782	0.080	<.0001	-0.485	0.082	<.0001	-0.349	0.079	<.0001
Log Net Government Costs									
All Mothers	-0.021	0.023	0.367	0.050	0.053	0.338	-0.018	0.062	0.777
Has order at entry	-0.046	0.030	0.123	-0.020	0.064	0.753	-0.124	0.078	0.115
Has no recent AFDC history	0.027	0.069	0.691	0.255	0.161	0.114	-0.044	0.180	0.806

Note: Sample sizes are 15,890 (all mothers), 9,140 (order at entry), and 2,052 (no recent AFDC history). Table shows experimental effects from regression models; co-variables reported in Appendix Table 1. \$. \$1 was added to the participation costs, the amount of child support retained and the net costs before taking the natural log to accommodate the zeros. All appendices are available at the end of this article as it appears in JPAM online. Go to publisher's Web site and use the search engine to locate article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

thus it is uncertain whether other states should expect to experience similar savings. In addition, for a state that had a waiting list for child care subsidies, any reductions in costs related to a full disregard might be considered a way to serve additional families, rather than an actual cost savings.

DISCUSSION AND POLICY IMPLICATIONS

Supporting single-parent families through welfare has long been unpopular. Some policymakers have seen public financial support as an unfortunate substitute for private responsibility and have been particularly concerned that the public was providing support in the place of an “absent” father who was presumed to be shirking his duty. This view, which suggests that public support (welfare) and private support (formal child support) are substitutes, provides a justification for a policy of no child support disregard. However, the lack of a disregard may itself undermine private support if it leads to a lack of cooperation with the child support system, and lower payments and receipts.

The experimental component of the CSDE was designed to evaluate the effect of a novel approach to the treatment of child support for families receiving public assistance. The results reported here demonstrate that Wisconsin’s full disregard has been able not only to increase child support amounts received by an economically vulnerable population, but also to increase child support payments. We found the full disregard was associated with increases in the percentage of fathers paying support and the amount paid. The effects are generally small for the sample as a whole (increasing the likelihood of paying by 4–6 percent), but they are particularly large for those new to the welfare system (increasing the likelihood by 25 percent in the third year). Among mothers, there are effects on the proportion receiving support and the amount received, with effects that are often statistically significant but fairly small. Similar to the results for fathers, the effects are particularly large for those new to the welfare system. Paternity establishment occurred more quickly for children in the full disregard group.

Perhaps most surprisingly, the benefits of a full disregard have come at little cost to the government. The results reported here, combined with more formal cost/benefit analyses done in conjunction with the CSDE (Caspar & Cook, 2005), demonstrate that in Wisconsin the cost of passing through all child support to families is largely offset by reductions in benefit programs. Because most of the increased costs of the Wisconsin experiment are borne by the federal government, whereas most of the reductions in costs accrue to the state, there is a net cost to the federal government, while the state experienced a net savings, with the costs largely offsetting each other. However, as a savings in one particular program (child care subsidies) was the major component of the net state savings, it is unclear whether other states would experience similar savings. Moreover, Wisconsin did not count foregoing the state share of child support as a cost, as this occurred for both the experimental and control groups; in other states, this would be seen as a cost.

The experimental results reported above were for an initial cohort of cases entering W-2 during most of the first year of implementation. We also analyzed a much smaller cohort of cases entering W-2 beginning about a year after initial implementation. Unfortunately, the implementation error in which some control-group cases incorrectly received the full disregard occurred early in the experience of this later cohort (in the second year), limiting the value of comparisons between cohorts. The results for this later cohort were largely similar to those for the earlier entrants, but in most cases effects were stronger for the initial cohort. One exception

is that for the later cohort, the effects on paternity establishment persisted beyond the first year.³²

These results come from an experimental design. A key limitation of experimental designs is that they provide information only on the comparison of the policy regimes actually tested; they tell us little about the effects of other potential policies. (For discussions of the strengths of experimental and nonexperimental designs, see, for example, Garfinkel, Manski, and Michalopoulos, 1992; Greenberg and Michalopoulos, 2006; Moffitt and Ver Ploeg, 2001; Riccio and Bloom, 2001.) Currently, most states retain all child support, and in those that disregard some amount, the typical amount is \$50 per month. Thus the results of the Wisconsin experimental design, which rely on a comparison of a full disregard with a disregard of the greater of \$50 or 41 percent, cannot provide direct information on the effect of a full disregard compared to a zero disregard or compared to a straight \$50 per month disregard, nor can they provide direct information on likely effects of increasing the disregard to \$100 per month for one child or \$200 for two or more, as is now encouraged under TANF reauthorization.

Two recent research papers (Cancian, Meyer, & Roff, 2007; Casetty & Hutson, 2005) have compared outcomes as disregard policies varied across states and over time. In theory, this approach should enable one to estimate the effects of the full range of disregard policies observed (no disregard, up to \$50 per month, other policies). The key limitation to this analysis is the possibility that even after controlling for other relevant factors, the groups facing different regimes are different in ways other than the disregard policy. Using each state's report of child support outcomes among the AFDC/TANF caseload over the late 1980s and 1990s, and controlling for states, years, and other characteristics of states, the most recent analysis (Cancian, Meyer, & Roff, 2007) has found that a larger disregard is associated with a statistically significant increase in the rate of paternity establishment and, in two of the four models estimated, a small statistically significant increase in the proportion of cases with collections. The size of the disregard does not have a statistically significant relationship with the average collection per case among cases with collections. Thus, although the experimental study and the nonexperimental analysis provide different information, the conclusions are similar: Increasing the disregard will not only increase the receipt of child support (a mechanical effect) but may also increase the probability of payment of child support (a behavioral effect).

In some ways it is striking that we do find evidence of substantial effects in our analysis of the CSDE experiment, given the lack of a large difference in the policies faced by experimental and control group members, the speed with which mothers have moved off W-2, and the relative socioeconomic disadvantage of W-2 participants. Moreover, recent research has documented that many W-2 participants did not understand the child support-welfare rules; that research estimates that effects would have been larger had there been better understanding of the policy (Meyer, Cancian, & Nam, 2007). If a typical state (that currently has a zero disregard) were to implement a full disregard, we would generally expect larger effects on payments and receipts than those reported here for Wisconsin, primarily because the change in the disregard would be greater. Moreover, if the new policy were implemented in such a way that participants understood the policy rules, we expect this would also lead to larger effects. Of course, because the change in the amount of child support

³² A pooled analysis of both cohorts leads to identical conclusions: Fathers in the experimental group are more likely to pay support and log payments are higher; mothers in the experimental group are more likely to receive support in the first year, and log receipts are higher. Paternity establishment occurs more quickly for those in the experimental group, but the effects in the sample as a whole do not persist over time. Finally, there are no statistically significant differences in net costs.

retained by the government would also be larger, we would expect greater costs to the government as well.

In most states, TANF participants receive none of the child support paid on behalf of their children. This zero-disregard policy generates revenue to offset public assistance and child support enforcement costs in the short run. Our results suggest, however, that this policy is likely to diminish 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 the government of retaining child support as a reimbursement for cash payments is also limited. In contrast, the benefits to children of establishing paternity and setting a pattern of child support payments are potentially much more enduring; even in this limited experiment, with problems in implementation, there were significant impacts on child support payments. In the current policy climate, it is increasingly important that the child support enforcement system evolve from a focus on government cost recovery to a focus on increasing family self-sufficiency.

The federal government allowed Wisconsin to implement the full disregard policy under a waiver. The reauthorization of PRWORA encourages other states to implement a policy of larger disregards. The results presented here suggest the wisdom of allowing children, rather than taxpayers, to directly benefit when child support is paid.

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Appendix

Table A1. Description of samples.

	Mothers	Nonmarital Children	Fathers
Mother's AFDC Receipt in the 24 Months before Entering W-2			
19–24 months	54.0%	58.3%	64.5%
1–18 months	32.9	30.9	28.4
None	13.1	10.8	7.1
Mother's Child Support in the 12 Months before Entering W-2			
\$1,000 or more	11.4%	6.9%	16.7%
\$1–\$999	14.3	10.5	19.7
None	74.3	82.6	63.6
Mother's Location at W-2 Entry			
Milwaukee County	73.8%	80.8%	74.6%
Other counties in an MSA	17.5	14.6	16.2
Rural counties	8.7	4.7	9.2
Mother's Initial W-2 Placement			
Caretaker of newborn	9.0%	11.4%	5.2%
Placement with a W-2 check	60.0	62.0	60.7
Support services only	31.0	26.6	34.0
Youngest Child's Age^a at Entry to W-2			
<3	58.9%	44.6%	27.7%
3–5	18.3	21.7	27.9
>5	22.8	33.8	44.4
Mother's Age at Entry to W-2			
<26	47.7%	48.1%	39.8%
26–30	20.7	24.3	27.2
>30	31.6	27.6	33.0
Mother's Race			
White	26.0%	16.5%	26.0%
Black	61.4	71.0	63.0
Other/missing	12.6	12.5	11.0
Mother's Years of Completed Education at Entry to W-2			
<12	51.8%	58.7%	51.4%
12	37.0	33.1	37.5
>12	11.2	8.2	11.0
Average Annual Earnings in the Two Years before Entering W-2 of the Highest-Earning Father Associated with this Mother			
\$15,000 or more	14.2%	12.0%	
Average Annual Earnings in the Two Years before the Mother Entered W-2			
\$15,000 or more			10.7%
Mother's Quarters of Employment in the 8 Quarters before Entering W-2			
0	19.1%	22.3%	
1–6	60.6	61.8	
7–8	20.3	15.9	

(Continued)

Table A1. *Continued.*

	Mothers	Nonmarital Children	Fathers
Children Marital or Nonmarital^b			
All marital	7.9%	NA	19.3%
Number of Legal Fathers Associated with Mother at Entry to W-2			
Zero	23.8%	40.6%	
One	57.4	48.5	55.1%
Two or more	18.8	10.9	44.9
Child Support Order at Entry to W-2 (b)			
Order at entry	57.9%	46.9%	75.6%
Number of Children (b)			
One (or pregnant with first child)	32.3%	17.2%	65.8%
Two	29.3	25.2	21.5
Three or more	38.4	57.6	12.7
<i>N</i>	15,890	14,887	13,616

^a For mothers and fathers, refers to her or his youngest child; for nonmarital children, refers to their own age.

^b For mothers and (if applicable) children, refers to the mother's situation; for fathers refers to his situation relative to the W-2 mother.