Reducing Child Support Debt and Its Consequences: Can Forgiveness Benefit All?

Carolyn J. Heinrich
University of Wisconsin–Madison

Brett C. Burkhardt
University of Wisconsin–Madison

Hilary M. Shager
University of Wisconsin–Madison

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ABSTRACT

As child support debt owed nationally persists at alarming levels, both noncustodial parents and the custodial families who are not receiving support suffer significant hardships, and states are forced to expend greater resources on collection and enforcement efforts. This paper presents findings from an evaluation of a demonstration program developed to help noncustodial parents with large child support debts reduce their debt while simultaneously increasing child support paid to families, through gradual forgiveness of arrears conditional on payment of current child support obligations. The evaluation employs a randomized experimental design, nonexperimental analyses using propensity score matching and multilevel modeling techniques, and focus groups and follow-up interviews. Results show a pattern of effects that clearly suggests individuals responded to the program as intended. State child support debt balances decreased for program participants, and participants paid more toward their child support obligations and made more frequent child support payments. The study findings suggest considerable promise for the effectiveness of this program in reducing child support debt burdens and in increasing families' receipt of child support, and they also point to ways in which the implementation of the program can be improved.

INTRODUCTION

Despite recent policy efforts by federal and state governments to address the problem of nonpayment of child support, the amount of child support debt owed nationally, more than $105.5 billion in Fiscal Year (FY) 2008 (USDHHS, ACF, OCSE, 2009), remains at an alarming level. The fact that low-income noncustodial parents (NCPs) owe a disproportionate share of this debt (in particular, public assistance debt owed to states) suggests that traditional collection and enforcement policies may not be adequate, and that much of the amount may ultimately prove "uncollectible" (Sorensen, 2004; Sorensen, Sousa & Schaner, 2007). At the same time, child support debt presents significant hardships for noncustodial parents and custodial families who are not receiving support, as well as for states that expend resources on collection and enforcement efforts and face federal performance standard consequences for non-collection.

We present findings from an experimental and nonexperimental evaluation of a demonstration program, Families Forward, developed to help NCPs with large child support debts to reduce their debt levels while simultaneously increasing child support paid to families. The outcomes evaluated include: payments made toward current support obligations or debt, the frequency of payments made, and changes in state-owed and household (custodial parent) owed debt balances. Following a review of the literature on how child support debt accumulates, its consequences, and policy and program strategies developed to address this problem, we describe the distinctive features of the Families Forward debt reduction program and the samples and data used in the evaluation. We next discuss the experimental and nonexperimental methods employed in the evaluation and present findings from the analyses. The experimental analyses comparing those intending to receive treatment with the control group suggest responses by NCPs in the expected direction, although statistically significant impacts are not observed. Alternatively, nonexperimental analyses comparing *actual participants* to nonparticipants

find statistically significant program effects in increasing NCPs' child support payments, increasing the frequency of payment, and reducing state-owed debt balances. We conclude with a discussion of changes in the program's implementation that could be made to improve program enrollment and effectiveness.

LITERATURE REVIEW

How Child Support Debt Accumulates

Child support debt owed to custodial families may be composed of unpaid current support, retroactive support, and interest. Arrears owed to the government derive from a mix of government policies that pass costs on to noncustodial parents, including reimbursement for public assistance benefits (paid to mothers), lying-in (medical) costs, interest charging and other fees associated with case processing or genetic testing (Bartfeld, 2003; Roberts, 2001). [1] NCPs may also enter the child support system with high debt due to retroactive support orders, in which NCPs are responsible for the lump sum accumulated between the birth of a child and establishment of a formal support order (USDHHS, OIG, 2000a). Research suggests a correlation between establishment debt and subsequent non-payment of child support obligations (Cancian, Heinrich & Chung, 2009; USDHHS, OIG, 2000a). Furthermore, federal regulations require payment of current support before debt, so that debt and interest may accumulate quickly even for NCPs who pay their current obligations (Hennessey & Venohr, 2000).

Child support order establishment guidelines may also impose support obligations or default orders that low-income parents cannot afford. Some states require monthly minimum support orders that may be high compared to actual income, particularly for NCPs who are incarcerated or unemployed (USDHHS, OIG, 2000a). For NCPs who fail to appear in court or provide current

[1] It is important to remember that many child support policies are determined at the state or county level; therefore, not all practices described exist in all jurisdictions.

earnings information, default orders based on income imputation may be established. These estimates often assume that the NCP is working full-time and impute wages based on a state's minimum wage or an average state or industry wage (Roberts, 2001). Research suggests this may later reduce payment compliance (Sorensen, 2004; USDHHS, OIG, 2000a). Even if established orders are within the means of NCPs, subsequent changes in employment or health status, or periods of incarceration may call for order modification, which rarely occurs automatically and may be difficult for low-income parents to secure due to lack of information or legal representation (Pearson, 2004; Roberts, 2001; Turetsky, 2007). Retroactive modification of past due child support payments is generally prohibited (Waller & Plotnick, 2001).

Other policy factors that may contribute to the accumulation of child support debt include the system's failure to recognize informal custody changes (Pate, 2002; Roberts, 2001) and its inability to manage the variety and fluidity of the complex families that require services (Meyer, Cancian & Cook, 2005). Double-digit interest rates and the charging of other operational fees by states (e.g., for income withholding) also contribute to growing financial burdens for low-income NCPs with debt (Roberts, 2001; USDHHS, OIG, 2000b). Parents' poor understanding of child support policies may further exacerbate their negative impacts (Collins & Mayer, 2005; Johnson, Levine & Doolittle, 1999; Meyer, Cancian & Nam, 2007; Pate, 2002).

Analyses of child support arrears suggest that much of the debt is held by poor NCPs. A recent national study found that of the 7 million nonresident fathers who did not pay child support, approximately 36 percent had incomes below the poverty line (Sorensen & Zibman, 2001). Another recent study in nine large states found that half of the obligors reported no annual income or earnings of less than $10,000 a year, and the debt owed by these obligors accounted for 70 percent of the total

arrears (Sorensen et al., 2007).[2] Other factors contributing to NCPs' inability to pay child support include poor job skills and employment opportunities, low levels of education, incarceration, unstable health, and a lack of assets to negotiate large, lump sum payments for conventional debt-forgiveness (Cancian & Meyer, 2004; Pate, 2002; Pukstas, Albrecht, Auten, Drew & Dabruzzi, 2004). Waller and Plotnick (2001) describe a "mismatch" between underlying assumptions of the child support system (e.g., that NCPs are divorced fathers working full-time) and the social and economic realities of many low-income NCPs (e.g., who often have never been married and face multiple barriers to stable employment) that diminishes these parents' willingness and ability to comply with existing policies.

Given the poor economic circumstances of many obligors, researchers have posited that it may be unrealistic to expect additional financial support from these parents (Cancian & Meyer, 2004). Simulations for seven states predicted that only 40 percent of the arrears currently owed would likely be collected within the next decade, and that total arrears would grow by at least 50 percent over the same time period (Sorensen et al., 2007).[3] These results reflect the large proportion of debt owed by poor obligors and low expected collection rates for this population. For example, the authors estimated that obligors with no reported income (owing 40 percent of the arrears), would pay only 16 percent of their debt over 10 years. A California analysis produced even more dire results. Assuming continuation of current trends and no change in policy, simulation models suggested that over 10 years, NCPs would be able to pay only 25 percent of the $14.4 billion in arrears accumulated as of March 2000, and arrears would continue to more than double over the same time period (Sorensen, 2004).

[2] The nine states included in this study were Arizona, Florida, Illinois, Michigan, New Jersey, New York, Ohio, Pennsylvania, and Texas. Collectively, these states accounted for 39 percent of the country's child support arrears in FY 2006. An analysis of California's child support debt yielded remarkably similar findings; 70 percent of the state's arrears were owed by noncustodial parents reporting annual incomes of less than $10,000, and most of these parents owed more than $20,000 (Sorensen, 2004).

[3] These states provided detailed administrative data on obligors and their arrears, which were matched by OCSE to six quarters of national quarterly wage and unemployment insurance data. Collection rates were estimated using current individual state child support policies.

Consequences of the Build-up of Arrears

The accumulation of child support debt poses problems for custodial families, low-income NCPs, and states (Bartfeld, 2003; Hennessey & Venohr, 2000). In particular, the recent decline in welfare receipt may have increased the importance of child support payments for low income families (USDHHS, OIG, 2000b). In 2007, approximately 25 percent of custodial parents and children had incomes below the federal poverty level, and for poor custodial parents who received full payments, child support represented approximately 48 percent of their average income (Grall, 2009). On average, however, poor families received only 54 percent of the child support payments they were due, and approximately 31 percent of poverty-level families received no payments at all (Grall, 2009). In addition to the consequences triggered by reduced income, some hypothesize that non-payment of child support may also exacerbate conflict between custodial and noncustodial parents and reduce NCPs' contact with children (Bartfeld, 2003). For NCPs, debt may deter work, as the process of automatic wage withholding may encourage parents to go into the underground economy to avoid cooperating with the formal child support system (Waller & Plotnick, 2001).[4] NCPs with delinquent payments may also be subject to enforcement actions, including loss of driver's license, liens against vehicles or property, and incarceration that may depress their earning capacity and further exacerbate debt levels (Holzer, Offner & Sorensen, 2005; Pearson, 2004; Turetsky, 2007; Western 2002). Child support debt is also inherently problematic for states, requiring expenditures for increased collection and enforcement efforts, and lowering state scores on federal performance measures for the share of all arrears cases with payments collected and distributed (Bartfeld, 2003; USDHHS, OIG, 2007).

[4] In contrast, using data from the Fragile Families study, Rich, Garfinkel & Gao (2007), found that stricter child support enforcement was associated with fewer hours of underground employment.

Policy Responses to the Child Support Debt Problem: Debt Forgiveness Programs

Some of the policies and programs developed to reduce and manage debt and its negative consequences attempt to address root causes of debt accumulation in a preventative manner (Roberts & Sorensen, 2005; Sorensen et al., 2007). However, given the large amount of debt already on the books, debt forgiveness strategies (also known as "arrears/debt compromise, debt management, or debt leveraging") are increasingly employed by some counties and states (Bartfeld, 2003; Pearson & Griswold, 2001; Roberts & Sorensen, 2005; USDHHS, OIG, 2007). Federal law allows states to accept less than full payment of state-owed arrearages and also allows compromise of custodial-owed arrears if both parties agree (USDHHS, ACF, OCSE, 2000). With this flexibility, state or local courts and/or state child support enforcement (CSE) agencies can accept reduced debt payments or expunge debt altogether in exchange for other desired behaviors, such as on-time payment of current support obligations, or participation in employment or parenting programs. Such policies allow states to reduce the amount of "uncollectible" debt and are motivated by the expectation that low-income obligors will be more willing to participate in the formal child support system and better able to comply with current support orders if relieved of some of their financial burden.

The OCSE does not formally track or monitor debt compromise programs, or issue formal guidelines for them; however, it has included information about such programs in its annual Compendium of Best Practices (USDHHS, OIG, 2007). Previous reviews have identified small-scale or pilot debt-forgiveness programs operating in a number of states, primarily at the county level (Bartfeld, 2003; Hennessey & Venohr, 2000; Pearson & Griswold, 2001), and a recent report by the Office of Inspector General (OIG) describes child support debt compromise programs operating in at least 20 states.[5] To reduce or expunge debt, 19 states require NCPs to pay a lump sum toward the

[5] According to the OIG survey, 12 states have CSE agencies that operate fully implemented debt compromise programs, and 8 states have agencies that are currently running pilot programs. Another 23 states allow debt compromise on a case-

arrearage; 19 states require regular payments on arrearages and current support orders; 8 states require involvement with the child; 7 states require participation in fatherhood programs; 5 states require participation in a parenting program; and 4 states require maintenance of employment, or some combination of these. Eligibility requirements, the potential amount of debt eligible for compromise, and the rate of debt compromise also vary across the states (USDHHS, OIG, 2007).

Despite this increasing policy adoption, little empirical evidence of the effectiveness of child support debt forgiveness programs exists (Bartfeld, 2003; USDHHS, OIG, 2007). The OIG selected 5 of 12 states with fully implemented debt compromise programs and reviewed a random sample of 259 cases involved since the beginning of each program.[6] The average arrears per case was $22,029, and the average amount by which debt was reduced through compromise agreements was $9,383. Forty-one percent of cases (N=105) were closed (USDHHS, OIG, 2007).

Only three other pilot programs have been more formally evaluated (nonexperimentally). Colorado's Arrears Forgiveness Demonstration Program provided forgiveness of all state-owed child support debt in Larimer County or up to $5,000 of state-owed debt in Jefferson County for participants who paid their current support orders consistently and in full over a period of 10 months (Pearson & Davis, 2002). Minnesota's Debt Compromise Demonstration Program for low-income NCPs in Hennepin County also provided full forgiveness of state-owed public assistance arrears (either in two lump sums or more gradually) if participants paid existing child support orders on a regular basis over a period of 12 consecutive months (Pukstas et al., 2004).[7] Maryland's Arrears Leveraging Pilot Project (ALPP) (operating in Baltimore City) enrolled obligors who were currently working with community-

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by-case basis, with individually determined amounts, eligibility criteria, and conditions. Eight states (AK, ID, IN, MS, MO, NY, TN, VA) reported that they do not allow compromise of debt owed to the state, although all states plus the District of Columbia acknowledge the legality of compromise of unassigned debt owed to custodial parents, per request and agreement between the two parties (USDHHS, OIG, 2007).

[6] The states selected were California, Massachusetts, New Mexico, Texas, and Washington.

[7] Although originally designed as a randomized controlled trial, some noncustodial parents assigned to treatment groups chose not to enroll. Enrollees had significantly higher incomes and lower monthly child support obligations, factors found to be predictive of program success (Pukstas et al., 2004).

based organizations (CBOs) on employment issues and rewarded consistent current support payers by gradually reducing or eliminating their state-owed arrears (in five stages). Obligors had five years to achieve four periods of consecutive payment, which would result in the forgiveness of the full amount of their state-owed arrears (Ovwhigho, Saunders, & Born, 2007). Each of these programs reported some success in collecting more child support from participating NCPs; however, the Colorado and Minnesota programs experienced challenges with low and selective enrollment, and the Maryland program participants encountered substantial delays between eligibility and actual debt forgiveness. Additionally, all Maryland participants who completed the first program stage also completed a CBO employment program; therefore, as implemented, the evaluation could not distinguish between the impact of the CBO programs and the arrears leveraging component. In each of these cases, success in meeting program requirements was correlated with prior earnings and/or child support payment, which also contributed to difficulties in reaching definitive conclusions regarding the success of the debt reduction programs. Clearly, new, more rigorously designed research is needed to determine whether and how such programs work to reduce debt and increase formal child support payments.

THE FAMILIES FORWARD PILOT PROGRAM, EVALUATION SAMPLES, AND DATA

The State of Wisconsin Bureau of Child Support (BCS) and the Institute for Research on Poverty (IRP) at the University of Wisconsin-Madison began investigating policy options to address the problem of growing child support debt in Wisconsin in 2003. Following exploratory research on possible program features, the pilot program, Families Forward, was designed to reduce child support debt while increasing child support paid. Racine County (in southeastern Wisconsin) was chosen as the experimental site because of the substantial number of cases with high levels of debt and county demographics (e.g., race, income and poverty, employment rates, single parent families) that were comparable to other large urban U.S. counties (such as Milwaukee), where child support debt and

enforcement challenges are considerable. We now describe the pilot program features below and provide details on the sample and data sources used in the analyses of program impacts.

Program Features, Eligibility, and Enrollment

The Families Forward program includes several features that are relatively unique compared to other arrears forgiveness program models. Rather than forgiving child support debt in a lump sum after specific requirements are satisfied, Families Forward provides for gradual forgiveness of debt over time. For an NCP with state-owed debt, the state agrees to reduce the debt owed to the state by 50 cents for every dollar of current support the NCP pays. If the NCP has family-owed debt, the NCP may receive a debt reduction of 50 cents for every dollar of current support paid, although reduction of family-owed debt requires the consent of the custodial parent (CP).[8] NCPs may have both state-owed and family-owed debts, and in these cases, NCPs may receive reductions of state-owed debt, family-owed debt, or both. By design, Families Forward allows an NCP with both types of debt to receive reductions of state-owed debts regardless of whether the CP consents to reductions for family-owed debt. Additionally, NCPs with multiple child support cases may have different debt reduction arrangements across cases (e.g., debt reductions for state-owed debt for case A, state- and family-owed debt for case B, and neither in case C).[9] For all types of debt reduction, credit is applied quarterly, and is applied to an NCP's interest balance first and then to the principal of the debt. Interest charging on debt is also suspended during program participation. The pilot version of Families Forward allowed NCPs to continue in the program for up to two years, unless they failed to make a current support payment in two consecutive quarters.

[8] An NCP may pay more current support than he or she owes in a quarter. When such lump sum payments occur, the NCP receives debt reduction credit for the entire amount paid. For both state- and family-owed debt, NCPs do not receive credit for payments made via federal or state tax intercept.

[9] If an NCP makes payments that are divided among multiple child support cases, arrears are reduced only for the portion of the payment that is applied to the enrolled case(s).

To ensure that the size of the pilot program would be manageable, the eligible population was restricted to NCPs with total debt burdens of at least $2,000 owed to the state or the family on at least one child support case (excluding foster care, kinship, and interstate cases) and who had a recent history of non-payment, defined as: (a) no payment on current support in the past three months; (b) current support paid in less than 6 of last 12 months; or (c) paid less than one half of annual amount owed in current support over the past 12 months. In advance of the start of the pilot, the sample frame was identified by applying these criteria to NCPs attached to Racine County IV-D cases open at the end of December 2004 (using Bureau of Child Support (BCS) administrative data). All NCPs (and their IV-D cases) in the eligible sample frame (approximately 5,000 cases) were randomly assigned to the control or experimental group using the last two digits of NCPs' social security numbers; 7 of every 10 cases were assigned experimental status prior to the program roll-out. New IV-D cases were similarly randomly assigned to the control or experimental groups each quarter if they met eligibility requirements.

Eligible NCPs had to go through a number of steps before enrolling and receiving debt reduction (see Figure 1).[10] Because Families Forward was a new program, NCPs first had to learn of its existence. IRP mailed letters of invitation to eligible NCPs (both in the experimental and control group) and their CPs, and distributed posters and brochures at retail, government, and service provider locations in Racine County. In Steps 2 and 3, NCPs had to contact the child support agency to confirm their eligibility and express their willingness to participate; control cases were informed at Step 2 that they did not qualify for program participation at this time. An experimental NCP looking to receive reduction on state-owed debt then had to wait for the child support agency to generate and mail a stipulation agreement outlining the terms of the debt forgiveness. An NCP also looking to receive reduction on family-owed debt faced additional steps. First (Step 4A), the NCP had to contact the CP

[10] Enrollment of eligible NCPs in the Families Forward pilot began in May 2005 and ended in November of 2007.

and describe the program.[11] If the CP agreed to participate in the program, he or she had to communicate that willingness to the child support agency (Step 4B). Both parties then had to wait for the child support agency to generate and mail the stipulation agreements (Step 4C). Upon receiving the stipulation agreement(s), the NCP (and CP) had to sign and return the forms to the agency (Step 5). Only then did the agency formally enroll the NCP and enable him or her to receive debt reduction, which would typically begin in the next quarter (Step 6).

In practice, these steps to enrollment were sometimes difficult to navigate, and many eligible, experimental NCPs failed to complete the process. Figure 1 also illustrates the variety of "exit points" at which NCPs could fall out of the enrollment process. The end result was a low take-up rate, an issue we further discuss below.

Analytic Sample and Data

The sample used in the analyses of pilot program impacts consists of all NCPs attached to eligible IV-D cases (in the sample frame) who contacted the Racine County Child Support Agency and expressed a willingness to participate in the Families Forward program (Step 2 in Figure 1). Of the 559 NCPs (with 1,976 IV-D cases) who contacted Racine County to try to enroll in Families Forward, 531 were either "willing experimentals" or "willing controls." Within this analytic sample, 378 (71 percent) had been randomly assigned to the experimental group, and the other 153 (29 percent) were assigned to the control group.

Participation in the Families Forward program among the "willing experimentals" was low; only 120 (32 percent) of these 378 NCPs successfully enrolled. Of these 120 *participants*, nearly three-quarters (88) received forgiveness on state-owed arrears only, 25 received forgiveness on family-owed arrears only, and 7 received forgiveness on both state- and family-owed arrears. A follow-up

[11] The Racine County Child Support Agency did not provide NCPs with assistance in locating or contacting CPs. In order to gain CP consent, an NCP either had to personally persuade the CP to participate or the CP may have proactively contacted the child support agency to give consent (perhaps in response to an invitation letter from IRP).

survey of NCPs who expressed an interest in Families Forward (described in Appendix A) suggests at least two explanations for the low level of participation among willing experimentals. First, CP cooperation was a serious obstacle to enrollment for some NCPs with CP-owed debt (Exit Points 4A and 4B in Figure 1). Of 32 surveyed NCPs who reported contacting the CP about Families Forward, over 40 percent said that the CP was "not at all interested" in participating in the program.[12] Second, some NCPs may not have followed through with the entire enrollment process, mistakenly thinking that they had already been enrolled (Exit Point 5 in Figure 1). Among the survey respondents, 35 willing experimentals had failed to enroll by the time of their survey, and four mistakenly reported that they were enrolled.[13]

The Kids Information Data System (KIDS), used by the State of Wisconsin Bureau of Child Support for tracking child support orders and payments, provides the primary data for the impact analysis. The KIDS data contain information on child support orders, payments, receipts and arrearages; the method of payment (wage withholding, tax intercepts) and destination of the payment (custodial parent, state); and demographic information about the parents and children in the cases (including birth dates, residential location of both parents, and dates of marriage, divorce, and paternity establishment). These data were merged with Unemployment Insurance (UI) Wage Record Files, which track the wages of workers eligible for state unemployment insurance.[14] Measures were constructed with these data to control for the employment and earnings histories of parents, in addition

[12] According to the program design, experimental NCPs with state-owed arrears in addition to family-owed arrears were eligible to receive forgiveness on the state-owed arrears. Interviews with staff at the child support agency revealed that in practice, however, staff did not always enroll NCPs with eligible state-owed accounts when they were not simultaneously able to gain consent from the CP to reduce arrears on a family-owed account.

[13] At one point during active enrollment (in 2006), a Racine County Child Support Agency staffperson new to the program explained that they were not enrolling eligible willing experimentals because of their poor payment history (Exit Points 4/4C and 6 in Figure 1). Poor payment history, of course, was a criterion for eligibility for the Families Forward program.

[14] Generally, exceptions from UI wage reporting (about 9%) include the self-employed, federal employees, church employees, employees of small nonprofit organizations, and wages that are earned outside of Wisconsin.

to other characteristics of parents and children involved in the child support cases, in nonexperimentally estimating program impacts.

The primary outcome measures in this study, constructed from the KIDS data, include: (1) the change in the *average* monthly payments made by the NCP toward current support or arrears between 2004 (the pre-intervention year) and 2006 (the year after most of the NCPs had enrolled); 2) the change in the number of months that any payment was made between the pre-intervention year (2004) and 2006; 3) the total number of months that any payment was made over the period January 1, 2006-March 31, 2008 (after most NCPs were enrolled); 4) the change in the *state* child support debt balance from the month before the program start (March 2005) to the most recent month of data available (March 2008), and 5) the change in the *household* child support debt balance from the month before the program start (March 2005) to the most recent month of data available (March 2008). With the exception of the third measure (total number of months that any payment was made after the start of the program), program impacts are measured as "differences in differences," that is, comparisons of how child support payments and debt balances changed from the pre- to post-program period between the experimental/participating NCPs and the controls/nonparticipants. As the overarching goals of the Families Forward program are to reduce state and individual debt burdens while increasing NCP payments toward current support and/or arrears, these measures should adequately reflect progress toward these goals.

STUDY METHODS

We employ both experimental and nonexperimental methods in evaluating the Families Forward program impacts. The primary advantage of an experimental approach to evaluation that randomly assigns participation in the intervention is that it assures that participation in the intervention is the *only* factor that differs between those in the treatment group and those excluded from participating (the control group). Our analysis confirmed that the experimental NCPs who were invited to enroll and

indicated their willingness to participate were statistically equivalent to those randomly assigned to the control group who also indicated their willingness to participate. For this "intent to treat" sample, we use experimental methods to calculate the Families Forward program impacts.

However, contrary to the program designers' intent, all willing NCPs with experimental status did not complete all steps to enroll in the Families Forward program, and those who did enroll were not statistically equivalent to willing NCPs assigned to the control group. Thus, we are not able to address with experimental methods the policy question: What was the impact of the Families Forward program on those who received the treatment? That is, calculating average differences in outcomes between *actual participants* and control group members will not identify the impact of "the treatment on the treated."

Fortunately, the use of random assignment in the Families Forward program evaluation also facilitates the application of rigorous nonexperimental evaluation methods, where random assignment status is used as an instrument for participation in estimating program impacts. Assuming that randomization does not influence program outcomes, we use propensity score (differences-in-differences) matching and include random assignment to treatment as an exogenous predictor variable in the first-stage estimation of the probability of program participation. We also employ nearest-neighbor matching with bias adjustment as an alternative matching procedure. As a sensitivity test, we estimated a standard instrumental variables (IV) model of program impacts, and we also applied the Bloom (1984) correction for the proportion of experimental group members who enrolled in the program to the experimental and IV results. Finally, we take advantage of the longitudinal, hierarchical structure of our data (with observations over 51 months nested within individuals) to estimate growth curve models of changes over time in the outcome variables and their relationship to the timing of events (such as expression of interest in the program and the start and length of program

participation). Acknowledging their varying assumptions, we compare program impacts estimates across these different nonexperimental approaches (the details of which are described below).

Experimental Methods

Using standard terminology, we denote Y_1 as the outcome for an individual assigned to the experimental group and willing to participate in the program, Y_0 as the outcome for that individual over the same period in the absence of participation, and D=1 for those in the treatment group and D=0 for those in the control group. Although it is impossible to observe both Y_1 and Y_0 for a single individual, with random assignment assuring that treatment status is independent of Y_0 and Y_1 and factors influencing them (X), the control group can be used to approximate what would have happened in the absence of the intervention. The average difference in outcomes between treatment and control group members (Y_1 - Y_0) then identifies the program impact, assuming sample sizes are sufficiently large.

$$E(Y_1 - Y_0| \text{ D=1}) = E(Y_1| \text{ D=1}) - E(Y_0| \text{ D=0}) \qquad (1)$$

The first two columns of Table 1 show that with regard to individual characteristics and key measures of interest in this study—total household debt balances and state debt balances, and recent payment histories at the start of the program—the 378 willing experimental NCPs who contacted Racine County to sign up for the Families Forward program were statistically equivalent to the 153 control group members who also expressed an interest in participating but were not allowed to enroll due to their "control" status. In other words, the experimental design worked for the "intent to treat" group, and we calculate program impacts for the group of NCPs who expressed their interest in participating as the average difference in outcomes between the 378 experimental NCPs and the 153 control group members.

Nonexperimental Methods

The third column in Table 1 presents descriptive statistics on *actual participants* in the pilot program. Comparing actual participants with NCPs assigned to the control group, statistically significant differences are evident for a few demographic characteristics (percent white, never married) and for key characteristics concerning the program intervention (household and state debt balances, and payments in the quarter prior to program start). As random assignments to the treatment and control groups were made prior to the mailing of invitations to participate in Families Forward, we are not able to make the same assumption that *participation*[15] in the program (which we will call D') is independent of factors influencing Y_0.

In applying matching methods, we invoke the conditional independence assumption, which implies that after controlling for observable characteristics (X), a person's actual treatment status is not related to what his or her outcome would have been in the absence of treatment.

$$Y_0 \perp\!\!\!\perp D' \mid X \qquad (2)$$

The validity of this assumption depends largely on the set of variables (X) available for the estimation and how the comparison group is chosen. We expect that there may be some unmeasured factors that influence program *participation*; what is important is that participation not be predictive of the outcome that would have occurred without the program. That is, conditional on measured characteristics, there should be no unmeasured factors that affect *both* participation and relevant nonparticipant outcomes. If this assumption is valid, the effect of the program on participants conditional on X can be written as:

$$E(Y_1 - Y_0 \mid D'=1, X) = E(Y_1 \mid D'=1, X) - E(Y_0 \mid D'=0, X). \qquad (3)$$

[15] Participation in the Families Forward program is defined by the signing of a stipulation agreement that formally enrolls the noncustodial participant, stops interest charging on debt, and begins providing credit toward debt based on current support payments made.

All regression and matching adjustment methods make this same assumption, although they differ in the methods used to estimate E(Y_1| D'=1, X) and E(Y_0| D'=0, X).

Propensity score matching. The common application of matching we use, propensity score matching (PSM), is a two-step process in which we first estimate the probability of participation based on the conditioning variables. By generating predicted probabilities of participation (i.e., propensity scores), we reduce the matching process to a one-dimensional problem of comparing treated and untreated NCPs with similar propensity scores (rather than requiring matches on all of the X variables). In the combined sample of participants and comparison group members, let P(X) be the probability that an individual with characteristics X is a participant. If participants and comparison group members have the same P(X), the distribution of X across these groups will be the same (Rosenbaum & Rubin, 1983):

$$Y_0 \perp\!\!\!\perp D \mid \mathrm{X} \implies Y_0 \perp\!\!\!\perp D \mid P(X), \qquad (4)$$

and individual cases can be compared on the basis of their propensity scores alone. We estimate:

$$E(\Delta \mid D'=1) = E_{X \mid D'=1} \left(E(Y_1 \mid P(X), D'=1,) - E(Y_0 \mid P(X), D'=0\right), \qquad (5)$$

where $E_{X \mid D'=1}$ is the expectation across all values of X for participants. A probit function is used in estimating the first-stage model that produces the propensity scores $P(X)$.

In one PSM estimation strategy, we include all eligible NCPs who expressed an interest in participating in Families Forward, whether assigned to treatment or control status. As only one-third of eligible NCPs with experimental status participated, we use random assignment to treatment as an exogenous predictor variable in this model. As expected, random assignment to the experimental group is the most influential predictor of participation in Families Forward. Other statistically significant predictors of NCPs' participation included demographic characteristics (age, race), earnings in the year prior to program start, and state debt balance and child support payment frequency in the

month/quarter prior to program start (see Appendix B). As a sensitivity test, we also estimated the PSM models on the subsample that included only participants (n=120) and controls (n=153), that is, excluding the experimental NCPs who did not participate. Differences in the results (discussed below) were negligible.

The propensity score matching technique that we use to adjust for selective differences between the Families Forward participants and nonparticipants in the second stage model is among the more rigorous available. Because several of the outcome variables are defined as the difference between a pre-program and later period measure for each individual, we are able to use a panel form of the matching estimator ("difference-in-differences" matching) that allows for time-invariant, unobserved differences between participants and nonparticipants to be present without biasing estimates of program impacts. The particular matching technique we apply, radius matching, specifies a "caliper" or maximum propensity score distance by which a match can be made. It uses not only the nearest neighbor within each caliper, but all comparison cases within the caliper (based on the specified distance), and the common support condition is imposed to exclude poor matches from the analysis. Standard errors are calculated using bootstrapping procedures.

We also employ a nearest-neighbor matching procedure with bias adjustment, which was recently developed in response to concerns first articulated by Abadie and Imbens (2002), that even after removing the conditional bias, matching estimators with a fixed number of matches may not reach the semiparametric efficiency bound for average treatment effects. Their approach implements a bias-correction that removes the conditional bias asymptotically (Abadie et al., 2004). We assess the sensitivity of our results to the choice of matching estimator.

Instrumental variables regression and Bloom adjustment. Primarily as another sensitivity test, we also estimated instrumental variables (IV) models of program effects. We expect the random

assignment status variable to satisfy the two key properties of a good instrumental variable; it is highly correlated with participation in Families Forward (instrument relevance), but we also assume that it is not correlated with any *unobserved* factors that affect the outcome variables (instrument exogeneity). We employ a standard two-stage least squares IV approach in estimating each of the program outcomes described earlier.

The Bloom adjustment recognizes that the average "intent to treat" impact estimate is actually a mix of treatment effects for participants and null effects for nonparticipants. It employs a simple correction to experimental estimates—it allocates the difference between a given outcome for experimental and control group members to the fraction of experimental cases that received treatment (by dividing the experimental impact estimate by the fraction of participants). We apply this relatively rudimentary correction to the experimental and IV model estimates (again as a sensitivity test).

Multilevel longitudinal modeling. As briefly noted above, we employ multilevel growth curve models (Singer & Willett, 2003) that allow us to examine changes over time (51 months) in the outcome variables and their relationship to the timing of important events, such as the expression of interest and enrollment in the program, while controlling for other time-varying and stable characteristics. An advantage of this modeling approach is that both the number of measurement occasions and their timing may vary across individuals and need not be balanced in the sample. Individuals invited to participate in the Families Forward program, for example, responded to this opportunity over a period of 33 months, so that the timing of a key event (their first contact to express their willingness to participate) varied considerably across those included in this study. As the timing of this event determines the pre-intervention and post-intervention periods, it is important to accurately capture this in the modeling of individual outcomes over time. We also assume that NCPs' responses

to these events are on the same temporal cycle as everything else that might affect patterns in the program outcomes (and for which we do not have controls in the model).

Multilevel methods have no special capacity in solving the causal inference problem, and as there are numerous alternative approaches to model specification, it is important to have a strong conceptual justification for the models estimated (Steele, 2007). Our choice of model specification is informed largely by our contextual knowledge of child support and related programs, gained through the literature and our experience in the implementation of the Families Forward Program. For example, we know that with state interest charges on debt, the debt balances of most NCPs in our sample are growing over time, but as participation in Families Forward stops interest charging, we expect that there may be a change in this growth rate for participants. In addition, given the new incentives offered by the program to encourage NCPs to pay more support, we expect that there may be a noticeable increase in the amount that they are paying once enrolled (with possible corresponding declines in debt balances). As participating and nonparticipating NCPs are compared beginning at the point at which they first make contact to express an interest in enrolling, we also anticipate that there might be lags in the timing of the response to these new incentives, particularly if prospective participants hold off on making any (or bigger) payments until they are enrolled and getting extra credit toward their debt for each dollar paid. Finally, we anticipate possible drop-offs in payments over time, as participants exhaust financial reserves or reach the end of program participation (limited to two years).

In the multilevel models we estimate, Y_{ti} is the response at measurement occasion t ($t=1,\ldots,T_i$) for individual i ($i=1,\ldots,n$). At level one, we use time trend and indicator variables to model outcomes using a piecewise linear function (that is, consisting of two or more regression lines or "pieces" in which the slope of the function is not constant) to account for the effects of time in months (nested

within individual participants). We model the average slope in pre- and post-program (first contact) periods (π_{1i} and π_{2i}) with time trend variables, and we use several indicator variables to capture shifts (increments or decrements) in outcomes (π_{3i} to π_{5i}) at specific times after the first contact and the three-month periods (March-May) each year when tax returns are typically received (π_{6i} to π_{8i})[16]:

$$Y_{ti} = \pi_{0i} + \pi_{1i}\text{Time\_pre}_{ti} + \pi_{2i}\text{Time\_post}_{ti} + \pi_{3i}\text{1st3mos\_post}_{ti} + \pi_{4i}\text{2nd3mos\_post}_{ti} + \pi_{5i}\text{7plusmos\_post}_{ti} + \pi_{6i}\text{taxtime05}_{ti} + \pi_{7i}\text{taxtime06}_{ti} + \pi_{8i}\text{taxtime07}_{ti} + r_{ti.} \quad (6)$$

Note that the tax time indicators will be the same calendar time periods for all individuals in the sample, while calendar time of the other indicators will differ across individuals according to the timing of their first contact to express an interest in Families Forward.

In level two of the model, the intercept (π_{0i}, where time=0, i.e., initial status) from the level one model is specified as random and a function of individual characteristics (demographic, number of children, monthly earnings, whether a current support order is in effect, and their cohort as defined by timing of first contact) and treatment status (d_i=1 for participants):

$$\pi_{0i} = \beta_{00} + \beta_{01}d_i + \beta_{02}X_{i(1)} + ... + \beta_{0n}X_{i(n)} + u_{0i.} \quad (7)$$

The time trend (growth rate) variables and indicators for capturing post-contact shifts (π_{1i} to π_{5i}) are also specified as random and a function of treatment status, for example:

$$\pi_{1i} = \beta_{10} + \beta_{11}d_i + u_{1i}, \quad (8)$$

and correspondingly for π_{2i} to $\pi_{5i,}$, where the left subscript on the β coefficients indicates the level-one parameter, and the right subscript denotes the level-two parameter. The random effects equations (π_{1i} to π_{5i}) model the effects of the program on the outcome trajectories of participants (relative to those

[16] Our decision to include tax return time indicators is based on preliminary research conducted by Kathryn Edin and colleagues, "Investing in Enduring Resources with the Earned Income Tax Credit (EITC)," which investigates EITC allocations among low-income households in Boston and Central Illinois. They find significantly higher levels of consumption and debt reduction in low-income households during the three months in which tax returns/credits are typically received.

who were eligible and expressed an interest in the program but did not participate); π_{6i} to π_{8i} are modeled as fixed effects.

These level one and level two models are estimated simultaneously, with an autoregressive error structure (within-person error covariance) of lag one, AR(1). The choice of error structure imposed on the model residuals should correspond to the data, accounting for autocorrelation and heteroskedasticity as necessary. We tested a standard (variance components) specification, as well as six other error covariance structures that other researchers (Singer and Willett, 2003) have found most useful (unstructured, compound symmetric, heterogeneous compound symmetric, autoregressive, heterogeneous autoregressive and Toeplitz). Comparing goodness-of-fit statistics across models, we concluded that the AR(1) structure was the best fit.

ANALYSIS FINDINGS

Table 2 presents the findings of experimental, "intent to treat" calculations of program impacts (comparing interested NCPs assigned experimental status with those assigned to the control group); the simple difference in outcome calculations comparing *actual participants* with the control group; the IV regression results; the PSM estimation results (including the two specifications and sample sensitivity test), and Bloom-adjusted impact estimates (in parentheses below the experimental intent-to-treat estimates and the IV regression estimates). These results are shown for the five outcome measures described earlier: the change in the *average* monthly payments made in the year before the intervention (2004) compared to the year after the majority of NCPs had enrolled (2006); the change in the number of months in a year that any payment was made (again comparing 2004 and 2006); the total number of months that any payment was made over the period January 1, 2006-March 31, 2008; and changes in the state and household child support debt balances from the month before the program start (March 2005) to the most recent month of data available (March 2008).

Comparing the 378 experimental cases who expressed an interest in participating (the "intent to treat" group) with the controls (column 1), we see that there are no statistically significant differences in outcomes between these two groups. Thus, the strict experimental comparison—the difference in outcome values between *all* willing experimental NCPs and all willing controls—does not show any statistically significant impacts of the Families Forward Program. In general, though, the outcome patterns are in the expected direction, with willing experimental NCPs paying more monthly child support on average, making more frequent payments, and reducing their state debt balances. Comparing only the 32 percent of the experimental NCPs who were actually getting credit toward their debt through participation in Families Forward with the control group (and not adjusting for differences in their individual characteristics), this pattern of effects is more pronounced in terms of magnitude and/or statistical significance (see column 2 in Table 2).

The next four columns of results present the nonexperimental estimates of "treatment on the treated" program effects that adjust for pre-intervention differences between actual participants and comparison group members. Column 3 shows the estimated effects from propensity score matching; column 4 from nearest neighbor matching analyses, and column 5 using the subsample that includes only the 120 participants and 153 control group members (excluding willing experimental NCPs who did not enroll). After matching, the estimated impacts are considerably larger, and the differences between participants and comparison group members are nearly all statistically significant. Changes in average monthly payments by Families Forward participants between 2004 and 2006 were about $81-91 greater than nonparticipants' payments during that time. Participants paid 1.24-1.43 more months in 2006 than they did in 2004, paying a total of about 2.7 more months on average over the January 2006-March 2008 period relative to nonparticipants. Given that approximately 80 percent of participants were getting credit toward their state-owed arrears, the large, statistically significant

decline in state debt balances for participants—about $3,100-3,800 more than state debt balances of nonparticipants between March 2005 and March 2008—is not surprising. The changes (reductions) in household debt balances were also considerably larger for participants, although only one of these estimated differences was statistically significant. The results of balancing tests, available upon request, show that there were no statistically significant differences in characteristics between participants and nonparticipants after matching, with reductions in bias ranging from 55 to 95 percent.

The IV regression results, with and without the Bloom adjustment (column 6, Table 2), suggest similar patterns of program effects, although the comparability of their magnitude with other nonexperimental estimates varies, and the standard errors are considerably larger. For example, the *unadjusted* IV estimate for the change in the number of months NCPs made child support payments (1.47) is comparable to those of the matching estimation (but with a standard error twice the size of the others), while the Bloom *adjusted* IV estimate for changes in state debt balances (-$3238) is closer to the matching estimates for this outcome. The Bloom correction is a comparatively crude adjustment (relative to PSM) that will not affect the statistical significance of the impact estimates, and the IV and PSM approaches to adjusting for differences in the characteristics of actual participants and comparison NCPs also differ, so we do not expect exact correspondence in results across these methods.

These nonexperimental approaches to impact estimation may still be lacking, however, in that they do not distinguish effects for NCPs entering the program at different times and for varying lengths of participation. Because eligible NCPs expressed their interest in participating in the program over a period of 33 months, for a fraction of the NCPs in the matching estimation, some or all of the months in 2006 may actually still be pre-intervention months. We turn now to the multilevel longitudinal

model estimates, which better account for the timing of events and their relationship to changes in the outcomes over time (including lags in responses, changes in growth rates, shifts in levels, etc.).

The results of the multilevel longitudinal models for four outcomes—average monthly payment made, the probability of making a payment in a given month, state-owed debt balance and household debt balance—are presented in Table 3. The key parameters of interest in this table of estimates (shown in bold) are the indicator for the start of the period following an NCP's expression of interest in the program (identifying post-contact months) and the post-contact growth rate for participants, representing the slope differential between participants and nonparticipants after they expressed an interest in participating in Families Forward. Also of interest are the indicators that capture shifts in outcomes at specific times after the first contact for participants relative to nonparticipants. These include an indicator variable for the first three months after NCPs indicated a willingness to participate (and indicators for the second three months and six months post-contact), which are intended to model behavioral responses related to the administrative delays in the onset of participation following the first contact with the child support agency.

Table 3 also shows the coefficient estimates for stable and time-varying covariates in these models, as well as for other time trend and indicator variables. Included among these are indicators for "cohorts"—approximately six month periods during which first contacts were made by NCPs in the sample—although these variables interacted with participation are not statistically significant. Where statistically significant, the control variables predict outcomes in the expected direction: older NCPs, those with more children, and those with a current support order owe more debt but also pay more monthly; NCPs with higher prior earnings and higher monthly earnings pay more monthly, and the former also owe less to the household; white NCPs pay more monthly and owe less to the state; and all

NCPs pay significantly more ($56-78 per month) in the months of March, April and May each year when tax credits/returns typically arrive.

Looking at the key, statistically significant findings of interest for monthly payments made (column 1, Table 3), the coefficient on the post-contact period indicator for participants suggests that across all months following their expression of interest in the program, on average, participants pay $114 more per month toward their current support and/or debt owed than NCPs who are not participating in Families Forward.. The post-contact *growth rate* for participants is negative, however, with the coefficient suggesting that the additional amount they pay declines by approximately $4 per month in the months after first contact. Participants also pay an estimated $137 less on average in the first three months post-contact than nonparticipants, which is consistent with what we know from talking to program administrators and enrollees about how the NCPs wait until a stipulation is signed to begin making payments and getting credit toward their debt. A simpler specification fit better for the model estimating the probability that an NCP made a payment in a given month (see column 2, Table 3), and the findings show that on average, participants are 8.3 percent more likely than nonparticipants to make a payment each month in the post-contact period.

The third and fourth columns of Table 3 show the multilevel model results for changes in state and household debt balances over the period of study. The coefficients on the trend variables for pre- and post-contact growth rates show that for all NCPs, both state-owed and CP-owed debt are growing over time. However, for Families Forward participants, the post-contact trend in state-owed balances is negative and statistically significant, showing that the state-owed debt of program participants is declining by about $101 per month, on average, following the month in which they made contact to express their interest in the program. Participants' post-contact growth rate for CP-owed debt is in the

direction expected, but it is not statistically significant. This is not entirely surprising, as so few participating NCPs (32) were receiving any credit toward reducting their CP-owed debt.

In general, we view the nonexperimental findings from the alternative approaches to estimation as fairly comparable. The matching and multilevel models identified statistically significant impacts of the Families Forward program in increasing NCPs' payments toward current support and arrears, in increasing the frequency or probability of payment over the post-intervention period, and in reducing state-owed debt balances. More specifically, as the majority of program participants had at least 32 post-contact months that we were able to observe, the finding that state-owed debt balances are declining by an average of $101 is largely consistent with the matching estimates showing that participant state debt balances declined by $3,200-3,800 more than state debt balances of nonparticipants in the post-contact period. And as the decline in state balances is mechanically related to the payments made toward debt and current support, it is not surprising that the average increase in payments by participating NCPs ($114 per month) also approaches this total over 32 months, taking into account the initial decline in the three months after first contact and the negative post-contact growth rate, (where over time, the additional amount they pay decreases by about $4 per month). As described earlier, the moderation in increases in payments by participating NCPs over time is expected, as some will have completed their two years of time in the program, and others may deplete their financial reserves for making additional payments to get credit toward their debt.[17] Finally, the nonexperimental analyses show no statistically significant effects of program participation on changes in *household* debt balances, which we expected given that only about 25 percent of participants were able to secure stipulation agreements signed by the CP to allow forgiveness of CP-owed arrears.

[17] Survey results also suggest that some NCPs were disappointed in the results of the program after enrolling and may have subsequently moderated their payments. Among 30 surveyed NCPs who reported participating in the program, 17 said that Families Forward had helped them reduce their debt *less* than they had expected. While some of this disappointment may be attributable to unreasonably high expectations for the program's capacity to reduce debt, or to low levels of payment (and thus low levels of debt reduction), implementation shortcomings may also have played a role.

Overall, we argue that the rigor of the estimators (using a rich set of covariates and a randomization device to nonexperimentally adjust for differences in participants and nonparticipants), the balancing test results, and the relatively long period of observation (i.e., more than four years) call for considerable confidence in the nonexperimental findings.

We also conducted some simple, case-by-case analyses to examine how individual debt balances changed over the course of the program for participating NCPs. These calculations showed that approximately 22 percent of Families Forward NCPs eliminated their entire state-owed and/or household debt balances while participating in the program (compared to 16 percent of NCPs who did not participate). Some of the larger state debt balance reductions among participating NCPs included: $18,474 reduced to $554; $17,755 to $0; $14,410 to $0; $13,417 to $4,073; $11,114 to $0; and $10,266 to $1,141. The largest household debt balance reductions included one greater than $60,000 and another more than $40,000.[18] These substantial reductions in debt balances are likewise encouraging about the potential of Families Forward program to reduce individual and state child support debts.

CONCLUSION AND POLICY IMPLICATIONS

For the NCPs who participated in the Families Forward pilot program, results were promising. Comparisons of participating NCPs to non-participants using propensity score matching and multilevel longitudinal methods suggested important effects of the program: participants made larger child support payments, paid them more frequently, and reduced their state-owed debt balances. However, it is possible that these results, which statistically adjust for the selective nature of program take-up, overstate the potential to increase child support payments and reduce arrears across the population of NCPs with poor payment histories. The experimental estimates, which combine both treatment effects for participants and null effects for the 68 percent of experimental cases who did not participate,

[18] The NCP who eliminated $60,113 in household-owed balances plus $2,436 in state-owed balances during the program was excluded from the impact analysis as an outlier.

showed no statistically significant effects. If participation remains low in future implementations of the program, as has been the experience in other debt forgiveness programs, then the vast majority of NCPs would be unaffected by the program.

On the other hand, low take-up is a problem common to many important social program interventions. A MDRC survey (Wallace, 2002) reported that relatively small percentages of eligible, low-income families avail themselves to work supports such as education and training (23 percent) and food stamps (43 percent), and Bansak and Raphael (2006) estimated that the overall take-up rate among eligibles for the State Children's Health Insurance Program is just 10 percent. Studies of housing programs have shown that among the 77 classes of interventions, take-up rates never rise above 25 percent, and take-up rates are lowest among the poorest eligible households (Currie, 2006). Still, the Families Forward pilot program experience should encourage policymakers to address two questions before moving forward with similar programs (or an expansion in Wisconsin). First, what modifications might be made to increase participation (i.e., enrollment) among eligible parents? Second, if participation increases, will the new participants likewise pay more child support (and more often)?

The pilot program experience suggests at least four approaches that could achieve higher rates of participation among those interested in the program. First, any future implementations should streamline the enrollment process for state-owed arrears. Because reduction of state-owed arrears does not require CP consent, a state need only determine an efficient administrative protocol for enrolling eligible and willing NCPs. For example, upon determining that an NCP is eligible and willing to participate, a stipulation agreement could be automatically generated and mailed to the NCP. If the NCP is also eligible for reduction in family-owed debt, enrollment for state-owed debt reduction should move forward regardless of whether the NCP has obtained the CP's consent to participate.

A second way to increase participation among eligible NCPs is to reach out to the CPs. The Families Forward pilot program was relatively unique in allowing reductions of both state- and family-owed debt. Yet, few of the participants received debt reduction on family-owed debt, and survey results indicated that many CPs had little interest in or understanding of the debt-reduction program. This presents a real obstacle to reducing family-owed arrears balances. Focus groups with CPs, conducted in the early stages of the pilot program design, suggested two means of increasing CP participation. First, outreach must clearly state what the CP might reasonably hope to gain from participating, (i.e., increased current support payments). One way of doing this would be to use testimonials from CPs who have benefited from the program. Second, contact with CPs should be made through a third party such as a child support agency. Both NCPs and CPs in focus groups described several reasons why this might be more effective than relying on the parents to contact each other: 1) they often did not have updated contact information for the other parent, 2) parents who had antagonistic relationships were concerned about heightening conflict, and 3) parents indicated that they had a hard time explaining the details of the program to the other parent. If CP participation can be increased, the incentives for NCPs to make payments will likewise increase (as they receive additional credits for payments) and more NCPs (with family-owed debt) will enroll in the program.

Another approach to increasing NCP participation is to improve communications with NCPs, both during and after the enrollment process. Survey results showed that participants had difficulty understanding if they were enrolled in Families Forward and whether it was working to reduce their arrears. These challenges likely contributed to some NCPs expressing disappointment in the program. For a conditional debt reduction program to maximize its impact, NCPs must be able to easily see how their payments link to reductions in their arrears. To this end, we recommend two features. First, future implementations should provide unambiguous notifications to NCPs that state when

participation begins (coupled with clear statements that enrollment does not begin until such notice is received). Second, monthly (or quarterly) child support statements should include an explicit statement of how much credit they received toward their arrears. The first recommendation aims to avoid situations in which NCPs prematurely assume that they are enrolled and fail to complete the full enrollment process. The second recommendation aims to clarify the monetary benefits that NCPs gain through their payments made while enrolled in the program, thereby making the incentives more salient to NCPs.

Finally, future programs should also strive for increased outreach among the target population and an expanded scale to involve greater numbers of indebted NCPs. The low response rate of eligible NCPs who were mailed a letter belied high levels of interest among those who heard of the program—over 80 percent of surveyed NCPs said they were immediately willing to sign up for Families Forward upon hearing about it. Although most important is proactive communication on the part of child support agency staff, beyond the child support agency, promotional materials should be distributed throughout the community (e.g., in libraries, unemployment offices, churches, etc.), including in non-English languages as locally appropriate. Focus group findings suggested the importance of using advertisements in media such as radio, newspapers, and internet, along with "testimonials" from parents who were in the program, to allay fears that it might be a sting operation.[19] Partnerships with other governmental and community organizations could also increase knowledge about the program. During the pilot program, prison social workers and men's and women's shelter staff, who often had direct contact with potential enrollees, inquired about Families Forward and were enthusiastic about

[19] Several Families Forward participants proactively volunteered to provide testimonials about the benefits offered by the program, with one NCP even offering to help lobby the State to extend the program beyond Racine County.

providing information about the program and encouraging clients to enroll.[20] Child support workers and focus group participants also recommended providing information at relevant court proceedings (e.g., paternity, child support order, or custody hearings).

Beyond increasing participation (i.e., enrollment) among eligible parents, we also have reason to believe that the additional participants attracted through improved outreach will pay more child support. In the Families Forward pilot study, the experimentals who enrolled in the program were *more* disadvantaged (and therefore less capable of paying) than the experimentals who did not enroll. Participants in Families Forward had lower earnings and were burdened by larger state debt balances (see Appendix B for predictors of participation). Results from previous evaluations of similar programs suggest that participants with higher earnings were more likely to become successful debt reducers; therefore, including this population could potentially increase the magnitude of the positive findings (Ovwhigho et al., 2007; Pearson & Davis, 2002; Pukstas et al., 2004).

Child support debt reduction programs like Families Forward have the potential to increase the amount of money received by custodial parents and children, reduce the burdens of debt experienced by noncustodial parents, and decrease the financial impact of child support arrears on states. Even if our expectation that aggregate participation levels could be increased and that newly added participants would behave like the participants in the Families Forward pilot program would not pan out, the low take-up rates of many other social program interventions suggest that this alone is not a reason to discard the program, particularly in this case, when it appears to be reaching those most in need of its benefits. The pilot study findings offer considerable guidance for achieving greater take-up among eligible populations in future implementations by improving the enrollment process for state-owed debt, increasing CP participation, and improving communications during and after the enrollment

[20] Coordination with prisons and jails could be especially valuable, as many NCPs in the target population are likely to have been incarcerated at some point, possibly for child support non-payment. In our survey sample, over 80 percent of NCP respondents said they had been incarcerated at least once since they were first ordered to pay child support.

process. Additionally, Racine County in Wisconsin represented a particularly challenging test case for this conditional debt reduction program, as staff persons were reluctant to implement the program as designed, and the county faced exceptionally challenging economic conditions (i.e., higher than average unemployment rates). In counties or states that are more receptive to the principle of forgiving arrears, implementation might proceed more smoothly, making it easier for NCPs to enroll in the program (Brodkin, 2007). For these reasons, we are confident that the non-experimental estimates of program effects in this study are valid for informing future policy decisions that could contribute importantly to reducing the persistent, high levels of child support debt in the United States.

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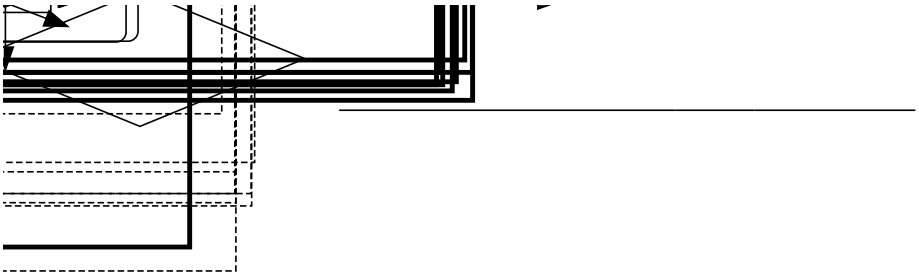

Table 1: Characteristics of Noncustodial Parents in Families Forward Program Evaluation

Noncustodial parent characteristics	Experimental (willing to participate) group (N=378)	Control (willing to participate) group (N=151)	Experimental group and participating (N=120)
Age in years	37.4	38.0	39.8
Percent female	5.3	5.2	2.5
Percent white	36.6	38.4	28.3*
Paternity case	70.0	68.6	73.1
Percent never married	13.1	11.3	7.5*
UI earnings in 2004	$4862 (8,757)	$4905 (11,178)	$4630 (8,644)
Number of children (across all cases)	3.0	3.0	3.3
State child support debt balance March 2005	$10,978 (17,875)	$10,173 (18,227)	$16,907* (17,924)
Household child support debt balance March 2005	$24,352 (23,341)	$21,645 (21,576)	$27,538* (24,979)
Percent w/no child support paid in 1st quarter 2005	25.8	30.5	16.7*
Number of months made payment in 2004	6.2	6.1	6.6
Amount of child support paid in 1st quarter 2005	$580 (987)	$588 (850)	$700 (1302)

** A t-test or chi-square test confirmed that the difference between this value for participating experimentals and that for those in the control group was statistically significant at p <0.05.*

Standard deviations are in parentheses below the measures of earnings, state and household debt balances, amount of child support paid in the 1st quarter of 2005.

Table 2: Families Forward Program Impact Estimates		Difference: experimentals (n=378) - controls (n=151)	Difference: participants (n=120) - controls (n=151)	Propensity score matching (120 participants vs. 409 not participating)	Nearest neighbor matching (120 participants vs. 409 not participating)	Propensity score matching (120 participants vs.151 controls)	IV regression (120 participants vs. 409 not participating)
Change in average monthly payment	2006 - 2004	$14.70	$50.05	**$85.14**	**$91.15**	**$80.68**	$17.34
				(22.75)	(34.86)	(29.28)	(76.24)
	Bloom adj.	*46.89*					*54.19*
Change in number of months made payment	2006-2004	0.3	**0.95**	**1.24**	**1.40**	**1.43**	1.47
				(0.63)	(0.66)	(0.40)	(1.17)
	Bloom adj.	*0.94*					*4.56*
Total months made payment	1/2006 - 3/2008	0.53	**2.45**	**2.68**	**2.71**	**2.70**	2.15
				(1.17)	(1.32)	(0.92)	(2.46)
	Bloom adj.	*1.66*					*6.72*
Change in state debt balance	3/2008 - 3/2005	-$583	**-$541.86**	**-$3,243.31**	**-$3,814.06**	**-$3,075.68**	-$1,036.21
				(1153.26)	(1116.68)	(798.94)	(2139.68)
	Bloom adj.	*-1821.88*					*-3238.16*
Change in CP-owed debt balance	3/2008 - 3/2005	$1,633	-$20.12	-$2,779.57	-$2,270.07	**-$3,174.43**	$1,993.53
				(1727.97)	(2153.41)	(1432.60)	(3702.93)
	Bloom adj.	*5103*					*4068*

Statistically significant differences at $p < 0.05$ are shown in bold. Standard errors are shown in parentheses and the Bloom-adjusted program impact estimates are presented in a separate row in italics.

Table 3: Multilevel Longitudinal Estimates of Program Impacts

	Monthly payment			**Made payment**			**State-owed arrears balance**			**CP-owed arrears balance**		
	Coef	Std error		Coef	Std error		Coef	Std error		Coef	Std error	
Intercept	-238.25	52.36	*	0.335	0.074	*	-19364.00	3945.19	*	-17314.00	4957.94	*
Pre-program growth rate	1.14	0.69		0.005	0.001	*	41.54	8.57	*	244.79	15.40	*
Post-program growth rate	1.72	0.71	*	-0.001	0.001		45.10	11.22	*	141.61	19.70	*
First 3 months post-contact	49.68	14.54	*				50.62	67.11		-52.18	88.85	
Second 3 months post-contact	4.56	15.20					63.51	93.60		40.44	127.65	
>6 months post-contact	1.80	15.02					37.91	114.79		129.62	158.93	
March-May 05	58.27	11.79	*	0.022	0.011		-28.04	43.16		-14.82	55.04	
March-May 06	56.01	11.65	*	0.036	0.011	*	-45.15	42.99		45.29	54.87	
March-May 07	77.65	11.58	*	0.015	0.011		-101.11	42.90	*	84.63	54.80	
Partcipant post-contact months	**114.31**	**33.12**	*	**0.083**	**0.031**	*	140.65	277.92		-96.69	387.22	
Participant post-contact growth rate	**-4.14**	**1.49**	*	-0.001	0.002		**-101.46**	**23.75**	*	-17.28	40.01	
Participant first 3 months post	**-136.82**	**35.49**	*				-119.10	203.31		329.30	274.76	
Participant second 3 months post	-23.42	32.98					-21.60	146.85		100.09	192.17	
White	55.56	16.33	*	0.005	0.023		-6120.06	1319.36	*	2868.52	1656.03	
Non-white	34.87	21.70		0.011	0.031		-3310.89	1755.83		-531.12	2200.97	
Male	49.38	31.28		0.051	0.045		4051.73	2535.69		9530.80	3183.54	*
Age in years	2.94	0.92	*	0.002	0.001		609.88	71.90	*	563.65	90.05	*
Never married	-25.50	21.54		-0.032	0.030		-2679.63	1737.32		3544.93	2187.92	
Log of 2004 earnings	5.77	1.72	*	-0.008	0.002	*	-205.00	133.14		-759.45	167.25	*
Number of children	15.19	3.72	*	0.010	0.005		2512.17	298.17	*	3727.15	372.32	*
Monthly earnings	22.48	1.17	*	0.038	0.001	*	2.45	6.32		5.31	8.19	
Current support order	89.84	13.28	*	0.030	0.017		303.11	91.01	*	1327.69	118.46	*
Cohort 2-first contact July-Dec 05	37.63	19.30		0.028	0.027		-2325.42	1528.23		4178.75	1944.20	*
Cohort 3-first contact Jan-July 06	61.61	18.83	*	0.078	0.027	*	270.84	1492.10		1222.95	1886.40	
Cohort 4-first contact July-Dec 06	96.87	22.24	*	0.143	0.032	*	-1134.81	1771.16		1913.78	2204.68	
Cohort 5-first contact in 07	78.81	36.47	*	0.102	0.058		-3621.64	2903.72		4107.05	3386.26	
Cohort 2 participants	-21.98	38.04		-0.040	0.050		-311.25	348.99		-0.42	467.65	
Cohort 3 participants	-46.99	35.61		0.050	0.047		-37.33	324.98		-75.47	434.40	
Cohort 4 participants	-49.95	45.58		-0.089	0.059		-153.42	408.93		-27.20	545.28	
Cohort 5 participants	8.28	89.54		0.079	0.106		-220.34	637.12		-54.73	847.00	

In this table, we highlight the key parameter estimates of interest in bold.*
Indicates the coefficient is statistically significant at $\alpha < 0.05$.

APPENDIX A*:* Survey Data Collection

Between March 2007 and April 2008, we conducted a follow-up telephone survey of NCPs to gather in-depth information about those eligible for Families Forward, enrollees' experiences with and attitudes about Families Forward, and potential effects of the program not identifiable in the administrative data. The sample frame for the survey included NCPs in the experimental group who had enrolled by February 2007 (n=79), NCPs assigned to the experimental group who did not enroll in Families Forward (n=192), and all willing NCPs in the control group (n=123). This resulted in a total raw sample frame of 394 unique NCPs who were attached to 604 IV-D child support cases.[21] From this original sample frame we excluded NCPs who had addresses listed as prisons or jails or no address listed in the BCS administrative database, resulting in an eligible sample frame of 327 NCPs: 68 participants, 151 willing but unenrolled experimentals, and 108 willing controls. Of these, 108 eligible NCPs responded to letters of invitation by calling to schedule a telephone survey, and 97 of these completed a survey.[22] Overall, 30 percent of the 327 NCPs in the eligible sample frame completed a survey.

We used administrative data from KIDS and UI wage data to compare the surveyed NCPs with the remaining NCPs in the analytic sample. T-tests of differences in means showed no systematic differences across the groups in terms of current support orders, family-owed debt balances, state-owed debt balances, amount of state- or family-owed debt reduction, total child support payments, or wages. There were also no differences across groups in terms of sex, race, or group assignment (treatment versus control). Survey respondents are therefore representative of the larger analytic sample on these important variables.

[21] The survey was designed to reference one specific IV-D child support case per NCP. Yet, 116 of the 394 NCPs had multiple cases. The rules used to select one focal case for each NCP with multiple cases prioritized cases in which a CP had enrolled with the NCP and cases with higher debt balances.

[22] We offered $25 for completing a survey during the first six months of the invitation process and increased the compensation to $35 after the first six months in an attempt increase the incentive to participate in the survey.

Appendix B: Propensity Score Matching First-Stage Model

(Probit regression, reporting marginal effects)

Predicting Participation in Families Forward			
Predictor variables	**Coefficient**	**Std. error**	
White	-0.069	0.032	*
Age	0.043	0.016	*
Age-squared	-0.0005	0.0002	*
Never married	-0.080	0.033	*
Female	-0.075	0.044	
Number of children	0.002	0.009	
Log of 2004 earnings	-0.009	0.004	*
Household debt balance Mar. 2005 (in 1000s)	-0.00046	0.00077	
State debt balance Mar. 2005 (in 1000s)	0.00231	0.00093	*
Number of months made payment in 2004	0.000	0.004	
Paid any support in 1st quarter of 2005	0.109	0.029	*
Assigned experimental status	0.274	0.026	*
Pseudo R-squared value=24.4%			

* Statistically significant at $\alpha < 0.05$.