

**W-2 CHILD SUPPORT
DEMONSTRATION EVALUATION
REPORT ON NONEXPERIMENTAL ANALYSES**

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The CSDE includes experimental and nonexperimental components. A previous report presented results from the experimental evaluation for the first cohort of cases. This three-volume report includes results from the nonexperimental analyses.

- Volume I summarizes and compares the experimental evaluation and three quantitative nonexperimental studies.
- Volume II reports on fathers of children in W-2 families, and features findings from an ethnographic study.
- This volume includes the full quantitative nonexperimental studies summarized in Volume I.

Chapter 1

Child Support Disregard Policies and Program Outcomes: An Analysis of Data from the OCSE

Judith Cassetty, Maria Cancian, and Daniel Meyer¹

From its inception in 1974, a key function of the federal-state child support enforcement program, termed the IV-D program,² has been to increase child support payments so as to decrease welfare use. Declines in welfare receipt would reduce government welfare costs, particularly those of the Aid to Families with Dependent Children (AFDC) program, the main cash assistance program for single parents. Throughout the IV-D program's twenty-five-year history, gaining the cooperation of welfare applicants and recipients has challenged federal and state officials. Over the years, attempts to increase cooperation have included both "sticks" and "carrots": most practices sought to impose sanctions on those not deemed cooperative, usually in the form of denial of AFDC benefits, but a few provided inducements for cooperation with efforts to secure financial assistance from the fathers of welfare-dependent children.

The most widespread and well-known inducement for cooperation has been the child support "disregard," whereby all or a portion of a child support payment is disregarded for the purpose of determining eligibility for, or setting the level of, an AFDC payment in any given month. (Cash assistance under AFDC later became known as TANF, Temporary Assistance for Needy Families.) The remaining amount in excess of the disregarded portion of any given month's child support collection is retained by state and federal governments to offset the costs associated with the cash benefits that have been provided.)³ Because a portion of child support collected by the IV-D agency is disregarded by the welfare (IV-A) agency, the effect of this policy is to increase a recipient's income during periods in which child support is collected. This potential increase in income is believed by many to encourage cooperation with the IV-D agency's paternity determination and child support enforcement efforts. Thus, states with more generous disregard policies may see greater cooperation, which could lead to increases in paternities established and child support collections. Similarly, within a state, variations in disregard practices over different time periods may have produced variations in outcomes for the state's IV-D program, all else being equal. During periods when a state had relatively high disregards, for instance, cooperation with paternity determination and enforcement efforts might have been greater, and outcomes better, than during times when there were no disregards applied to child support collections.

Despite the near-continuous representation of a variety of disregard policies in the history of state welfare programs, little empirical examination has been given to the effects of these policies on IV-D

¹The authors gratefully acknowledge the assistance of Royce Hutson in gathering, compiling, and analyzing the data utilized in this report. Jan Blakeslee, Elizabeth Evanson, and Dawn Duren provided valuable editorial and manuscript preparation services, for which the authors are also grateful.

²The Child Support Enforcement and Paternity Establishment program resulted from 1974 congressional legislation that included a part "D" to amendments to title IV of the Social Security Act, creating the federal Office of Child Support Enforcement and authorizing federal funds to be used for state child support enforcement efforts.

³Amounts of child support collected in excess of the disregard are divided between the state and the federal government in proportion to the share of costs borne by each in providing assistance to the family through the AFDC/TANF and Medicaid programs.

program performance. The state of Wisconsin is currently conducting an experimental evaluation of the effects of a particular type of disregard policy that is unlike those used by any other state previously. This study complements that evaluation by exploring the effects of various levels of disregards on IV-D paternity establishment and child support collections across all states, over a fifteen-year period. We begin the description of our study with background on the policy environment, followed by a review of some previous, related research in Sections I and II. Data and methods are presented in Section III, results in Section IV. We close in Section V with a discussion of the policy implications of this research.

I. Policy Background

Child support disregards have enjoyed nearly continuous representation in federal policy since the beginning of the IV-D program in 1974. Throughout, three core rationales supporting disregards have been advanced, endorsed by many members of Congress and state officials:

- Welfare recipients (increasingly unmarried women) would be more inclined to cooperate with publicly supported efforts to identify and pursue support from the fathers of their children if there were some economic incentive for them to do so.
- The fathers of children in public assistance households would be more inclined to pay child support if there were actually an economic advantage to their children in so doing.
- Disregards are seen as the right thing to do. That is, all children have a *right* to benefit from their nonresident parents' contributions, irrespective of welfare status, and nonresident parents have a right to see that the economic well-being of their children is actually enhanced as a consequence of their support payments.

Compelled largely by the intrinsic intuitive logic of these arguments, but in the absence of any empirical evidence to support it, federal regulations in 1976 required all states to disregard up to \$50 of every month's child support collection in determining the amount of that month's AFDC grant.⁴ Federal legislation in 1984 clarified policies and accounting practices related to the application of the \$50 disregard in each state. This feature of the interface between the welfare and child support programs continued until the welfare reform legislation of 1996, the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), which replaced AFDC with the Temporary Assistance for Needy Families (TANF) block grants and allowed states to establish their own policies regarding child support disregards. Thus, in all states, from the beginning of the federal child support enforcement program up to 1996, all resident-parent AFDC families for whom child support was collected had higher net income than they would otherwise have had. As a consequence of the federal mandate that each state disregard \$50 of each month's child support collection, an AFDC filing unit could expect up to that amount to be either passed through to them as a separate check from the IV-D agency or added to the maximum amount of AFDC to which they were otherwise entitled. Similarly, a nonresident parent who made child

⁴The \$50 disregard was incorporated in federal regulations beginning in October 1976 (42 USCA s.657). However, implementation across the states was gradual, as each state's IV-D program was developed and accounting practices were coordinated with those of the IV-A (AFDC) program, which was responsible for administering the disregard. The \$50 disregard replaced an even more limited disregard: the original federal legislation establishing the IV-D program required that state AFDC programs allow for a one-time-only disregard with a ceiling set at one-half of the first \$40 collected for any child in an AFDC family unit.

support payments saw the cash available to his⁵ child's family unit increase by an amount up to the level of the disregard. While the explicit cap at \$50 per month may have limited its power to induce cooperation with paternity and enforcement requirements, the disregard still provided a potential positive incentive for *both* parents to cooperate with paternity determination and child support collection efforts.

Disregard policies have changed over time and have varied from state to state. When states were encouraged to seek federal waivers to experiment with alternative welfare policies in the early 1990s, several states included disregards in their requests for waivers of existing IV-A requirements (Horvath and Peters, 1999). While most of these waiver requests involved approval to eliminate the mandated \$50 disregard, a few involved demonstration projects that were to increase the size of state disregards as they were applied to child support collections. However, many of the requests to eliminate \$50 disregards never took effect in the states that had requested them under federal waivers, having been made moot by federal action in 1996 that rescinded the disregard requirement. Following the 1996 legislation, states could and did choose to increase, decrease, or even abolish child support disregards altogether, in part reflecting a range of untested beliefs regarding the contribution of disregards to the overall effectiveness and efficiency of child support enforcement programs. Approximately two-thirds of the states chose to drop the fixed \$50 disregard, while the remainder retained a disregard. Of those that retained fixed disregard practices, all but two kept the ceiling at \$50: one state decreased it to \$40 and one increased it to \$75.

In addition to the historic policies regarding fixed child support disregards, some states have had histories of welfare practices that have included *variable* disregard policies, referred to as "fill-the-gap" budgeting. Since inception of the IV-D program, up to eight states have had IV-A policies in place that allowed them to disregard child support collections in excess of the \$50 per month fixed disregard. These states were covered under federal laws that applied to selected states whose welfare benefits did not reach their official Needs Standards as established by each state's legislature. In addition to the \$50 fixed disregard, these states disregarded all additional child support collected, up to the difference between the maximum grant for which a family was eligible and the Needs Standard that applied to any given family unit. A recipient in these states who received any unearned income, including child support, would realize an increase in total income up to the point where the unearned income, plus the welfare grant, plus the \$50 fixed disregard reached the level of the Needs Standard.⁶ Five of the eight states that have had effective fill-the-gap practices since 1974 have continued them up to 1998, the last year included in our study.⁷ It is important to note, however, that having "fill-the-gap" policies in place in a state does not necessarily imply that the state has an effective "gap" to be filled. The gap only exists in those states with the appropriate policies in place and in those years when the AFDC/TANF benefit schedules called for

⁵The gender pronouns used in this paper are chosen to reflect the more common experience in which resident parents, especially those that head public assistance households, are the mothers of the children in these households and the nonresident parents are the fathers.

⁶One of the earliest published references (Krause, 1983) to these variable-disregard practices included a recommendation that all states that failed to pay AFDC benefits equal to the state Needs Standard adopt the practice of disregarding all of the child support collected for families up to their specific Needs Standards. Krause argued for fill-the-gap practices on grounds of economic equity for children and fairer treatment of the men who were paying support for their children. Such practices may also be viewed as being responsive to the economic conditions in a particular state, since the disregard increases when the size of welfare payments fails to keep pace with official calculations of what income is required to meet the most basic of economic needs.

⁷Tennessee, Georgia, Maine, Virginia, and South Carolina have retained their fill-the-gap policies since the beginning of the IV-D Program. Wyoming and Mississippi have recently discontinued their variable-disregard practices.

welfare payments that were less than the states' official Needs Standards.⁸ Among the five states that had effective variable disregard practices at the time that PRWORA was enacted in 1996, only one state, Maine, continued both its fixed (\$50) and variable disregards. The remaining four states with effective fill-the-gap practices abolished their fixed \$50 disregards when given the option of doing so, reducing the potential net benefit resulting from a child support collection by \$50 per month for a family that receives TANF benefits.⁹

Policies related to both “pass-through” and “disregard” practices can be confusing, as well as carry different theoretical underpinnings. Unfortunately, state IV-D agency practices of *passing through*, as a separate check, the amount that federal law required that the state IV-A agencies *disregard* when establishing a recipient's monthly grant amount has led to some confusion between the two concepts. The original “pass-through” language may have had its roots in the early practices of county-run child support collection agencies that served county domestic relations courts. These agencies “passed along” or “passed through” the original instruments—checks and money orders—to all child support recipients, irrespective of welfare status, often reporting to state IV-A agencies the amount of these collections for AFDC budgeting purposes. When some states began adding the *disregard* amount to recipients' AFDC checks, they continued to call it a “pass-through” or “pass-along,” even though it was no longer a separate instrument representing a father's full payment that was passed directly to the welfare recipient. Some states, however, continued to “pass through” all or a portion of the child support collection, as federal child support law evolved, substituting state checks for fathers' checks and money orders, deposited in the state treasuries. These states maintained a clearer distinction between their IV-D program “pass-through” practices and the IV-A program “disregard” practices, which related directly to AFDC budgeting policy. The amount of the “pass-through” that was disregarded was accounted for in monthly welfare calculations. In the context of this report, to avoid confusion, we will use the two terms to refer to two distinct and different practices. It is helpful to remember, however, that *pass-through* policies are more apt to pertain to IV-D agency practice related to how fathers' checks are handled, whereas *disregard* policies are applied by IV-A agencies in connection with AFDC/TANF budgeting practices.

The distinction between pass-through and disregard practices would amount to no more than a semantic one were it not for the possibility that the two administrative practices may produce very different behavioral responses in individuals: Economic theory suggests that child support that is disregarded, thereby increasing the amount of income received from the IV-A agency above that which would otherwise have been received, will serve to encourage cooperative behaviors; pass-throughs alone, however, do nothing to increase income, thereby offering no *economic* incentive for welfare recipients to cooperate.

It may be the case, however, that the practice of “passing through” some portion of the child support collection to the welfare recipient as a separate check will have its own effects, independent of the economic incentives of disregards: Pass-through practices may serve as a reminder that child support is being paid by fathers, a fact that may be lost when a single check is issued to welfare recipients. If this is the case, it would follow that welfare recipients may gain confidence in the regularity and reliability of child support payments through the mechanism of pass-throughs which could, in turn, contribute to their

⁸Virginia, for example, maintains its “fill-the-gap” policies, but in 1998 maximum TANF benefits reached the levels of the state's Needs Standard, leaving it with no *effective* variable disregard that year. Similarly, Delaware, a “fill-the-gap” state in the early years of the IV-D Program, has paid 100 percent of its Needs Standard since 1977 and has thus has no effective “gap” to be filled by unearned income, including child support collections.

⁹Delaware also retained the \$50 disregard.

motivation to support themselves, independent of welfare. Pass-throughs, when combined with disregards, could serve to reinforce the economic incentive effects of disregards by making it easier for welfare recipients to recognize that a portion of their child support collection is being disregarded.¹⁰

Now that states have more options for setting pass-through and disregard policies without federal waivers, the variation in state practices is somewhat greater than before 1996. While state IV-D programs such as Texas have passed through the entire child support collection for nearly two decades (though disregarding only \$50 until recently, when it dropped the formerly mandated disregard), Wisconsin has become the first state to disregard the entire child support collection each month. No other state has adopted a full disregard. We illustrate the state policy options in Table 1.

The rows demonstrate the policy options available to a state. The columns show the amount of the TANF payment, the amount of child support received by the family as a separate check, and the total income of the family under each policy option. For illustration, we consider a case in which the TANF maximum grant is \$350 per month, \$150 of child support is collected by the IV-D agency, and the family has no other income.

Row One. In the first row, in a state that has a disregard of \$50 but no pass-through, the family would receive a single check for \$400 (which consists of the TANF maximum of \$350 and \$50 extra, the fixed amount disregarded).

Row Two. In the second row, a state that has a fixed disregard *and* a pass-through, two checks would be received. This could happen in one of two ways: states could either send a child support check of \$50 (the amount disregarded) and an (unadjusted) TANF check of \$350. Alternatively, the state could send a child support check of \$150 (the full amount collected) and a TANF check of \$250 (the maximum grant of \$350 minus the child support received in excess of the disregard). Both families have a total income of \$400.

Row Three. In the third row, when no disregard is in effect but there is a pass-through, the family again would receive a public assistance check and a child support check, but, since none of the child support would have been disregarded, the assistance amount would be reduced by the amount of the child support (checks of \$200 and \$150).

Row Four. The final row illustrates the case of neither a disregard nor a pass-through; in this case the family receives a single check of \$350.¹¹

¹⁰While the authors acknowledge the very important contribution that pass-throughs may make to welfare recipients planning and exercising self-support strategies, the focus of this research is upon cooperation incentives that are predicted to follow directly from the economic incentives associated with disregards. Thus, the research reported here will not include estimates of pass-through effects that may influence behavior, but will instead maintain a focus on child support outcomes that may result from greater incentives to cooperate with IV-D program requirements. An important area of future inquiry is the effects of pass-throughs.

¹¹There is one more possibility with reference to disregard policy that might be expected to produce some similar kinds of “cooperation incentives” as those suggested by the illustration above, though they may not be as easily observed or measured, and thus will not be studied here. Some states may choose to disregard the one-time payment of arrears collected, for instance, for the purpose of determining initial eligibility for assistance. Such a policy might be adopted with or without a disregard associated with ongoing welfare payments and child support collections.

TABLE 1
Results of Alternative Pass-Through and Disregard Policies

Example: Mother-only family with two children living in state with a TANF maximum grant of \$350/month and a child support collection of \$150/month (not a “fill-the-gap” state).*

Pass-Through and Disregard Options	TANF Payment	Child Support Received by Family	Total Income Available to Family
1. No pass-through, \$50 disregard	\$400	0	\$400
2. Pass-through, \$50 disregard	\$350	\$50	\$400
	\$250	\$150	\$400
3. Pass-through, no disregard	\$200	\$150	\$350
4. No pass-through, no disregard	\$350	0	\$350

*This table is derived from the common experience prior to 1998 and does not illustrate Wisconsin’s new policy of both fully disregarding all child support collected AND passing it through to the family as a check that is separate from the TANF check.

The example demonstrates that whether or not a state disregards a portion of a given month's child support collection can be critical to the economic well-being of the family. On the other hand, without a disregard in place, a pass-through does not change income, but is simply a tool that may help the AFDC/TANF adult recipient to track and take conscious account of the frequency and magnitude of the child support payments.¹² Because our primary interest in this research is the economic well-being of families, we focus on the effects of actual disregard practices, ignoring differences in pass-through policies. In the new political environment in which states can set their own disregard policies and have increasing concerns with financing their IV-D programs, understanding the effects of these policies has become more critical (Wheaton and Sorensen, 1998).

II. Previous Research on Child Support Outcomes

The overall objective for this section of the report is to identify variables other than state disregard levels that may influence various child support outcomes and to move toward isolating the independent effects of disregards on these outcomes. Our initial approach to this task involves an examination of several relatively recent key studies which have identified other potentially important policy variables that influence the same types of child support outcomes that we will be estimating later.

Factors Appearing to Influence Changes in Child Support Program Outcomes Over Time.

Since the earliest years of the federal Child Support Enforcement Program, substantial variation has marked Title IV-D program outcomes across states. As a simple illustration, Table 2 shows wide variance among the states in the ratio of collections to costs in each year. Moreover, the difference between the higher-performing states and the lower-performing states has narrowed only slightly over time.

Researchers and policymakers have explored whether differences in outcomes, such as cost-effectiveness, can be explained by variations in state IV-A and IV-D program policies. Many important child support enforcement policy changes were initiated by the states themselves, and thus may help explain differential IV-D program outcomes. Many of the more successful state innovations in child support law and administrative practice have been adopted by the federal government and implemented nationally. Variance in the timing of the implementation of these innovations across states may explain part of the variance in outcomes. The process of increasing federal influence over state child support laws and practices has led gradually to standardization of most substantive features of child support enforcement. More than twenty-five years following enactment of the federal IV-D program, it might be said that there are more similarities than differences in state IV-D programs and in the state laws which undergird paternity determination, location, enforcement, guidelines, and collection components of the

¹²Moreover, a disregard has a direct effect on the amount of child support the government retains to offset the welfare benefits it pays out and thus has a direct effect on costs. A pass-through in and of itself may affect administrative costs, but does not have the same direct effect on governmental outlays as does a disregard.

TABLE 2
State IV-D Program Cost Effectiveness, 1985–1998

Year	Ratio of Collections to Costs		
	Lowest State Ratio	Highest State Ratio	Range
1985	1.06	7.62	6.56
1986	0.92	8.33	7.41
1987	0.97	9.52	8.55
1988	1.21	10.83	9.62
1989	1.33	8.97	7.64
1990	1.49	8.71	7.22
1991	1.54	8.07	6.54
1992	1.57	9.27	7.69
1993	1.79	9.09	7.30
1994	1.78	8.58	6.80
1995	1.48	8.20	6.72
1996	1.43	7.74	6.31
1997	1.45	7.42	5.97
1998	1.59	7.18	5.59

Note: Data provided by the Federal Office of Child Support Enforcement.

states' programs.¹³ Nonetheless, significant differences in policies remain, and the effects of some of these policies (such as the level of child support disregards) have not been extensively studied.

In the years immediately following creation of the federal Office of Child Support Enforcement, researchers sought relatively simple explanations for variations in measures of state program success (Cassetty, 1978). In addition to considering the effectiveness of alternative laws and policies, empirical analyses of child support enforcement outcomes have often included measures of the demographic characteristics of the caseload, including factors that may directly influence outcomes of interest. Demographic measures, while far less amenable to direct policy manipulation, may offer insight into the influence that the policy environment has on program outcomes. (More recent studies showing strong effects of demographic variables on child support outcomes include Beller and Graham, 1993; Garfinkel and Robins, 1994; Meyer and Bartfeld, 1997; Freeman and Waldfogel, 1998; Meyer and Hernandez, 1999; Sorensen and Halpern, 1999.)

In addition to changes in state law and the policy environment, researchers in recent years have looked closely at administrative and implementation issues.¹⁴ Given the trend toward standardization in the features of state IV-D programs, variation in the timing of adoption and implementation of new enforcement tools and other mandated practices has presented researchers with both an opportunity and an obstacle to better understanding of their effects. Most laws and administrative practices related to the state IV-D programs were adopted and implemented within a very few years of being mandated by federal law. This narrow range in adoption dates may make it difficult to accurately estimate the effect of a given innovation. On the other hand, the political and administrative readiness of states to implement mandates, generally difficult to measure, may contribute to the relative success in different states following implementation. Researchers have used a variety of approaches to capture the potential delayed impact of policy changes that cannot be expected to have measurable effects immediately following adoption.

Key Studies Informing Our Present Efforts.

A number of previous studies of the influence of demographic, administrative, and legal factors on child support outcomes inform our analysis of the effects of state disregard practices. We review four recent examples of this type of analysis: Beller and Graham (1993), Garfinkel and Robins (1994), Freeman and Waldfogel (1998), and Sorensen and Halpern (1999). Specifically, our discussion focuses on the data, measures of child support enforcement "tools" used, and estimation approach.

All of the research summarized here has relied upon multivariate analysis, usually ordinary least squares (OLS) estimation techniques, utilizing merged data sets (compiled from two or more sources) to

¹³In 1974, for example, when the IV-D program was enacted by Congress, state statutes of limitations on paternity lawsuits, restricting them to only a few months after the birth of a child, were quite common, the rationale being that "witnesses" to an intimate association between the putative father and mother would lose credibility and evidence would grow "stale" with the passage of time. Over time, states gradually extended these time limits as large and rapid strides were made in the science and legal application of genetic testing techniques. However, by 1984, when states were required by federal law to adopt legislation allowing for paternity actions up to at least eighteen years after the birth of a child, more than half of the states still required legislation to bring them into compliance with the eighteen-year limit (Beller and Graham, 1993).

¹⁴The Office of Child Support Enforcement has recently commissioned research into the relationship between various administrative variables and child support outcomes. Preliminary findings from a single year are available in Fishman, Dybdal, Tapogna, and Laud (2000).

explore influences upon several child support outcomes. Of the four relatively recent studies considered here, all relied upon individual-level data from the Current Population Survey (CPS) to measure child support receipt by unmarried and divorced women who headed households that included their own minor children. The limitations of these data, including the difficulty of identifying the population of interest, have been documented in numerous studies, including those of Robins (1987, 1992), and are discussed in detail in Chapter 2.

Garfinkel and Robins and Beller and Graham use the March/April CPS, which includes the Child Support Supplement (CSS). In contrast, Sorensen and Halpern use 21 years of annual data from the March CPS. Although their choice did not allow an examination of the amount of child support owed to sample participants, the child support survey items in the CSS (which include order amounts) were thought to lack inter-year comparability, which these researchers found to be particularly restrictive, given their desired focus. Freeman and Waldfogel, also attempting to address the limitations inherent in the March/April CPS/CSS, pooled household-level data from the March CPS with selected items from the Survey of Income and Program Participation, resulting in a database with annual measures covering nine years, 1980–1988.

All of the studies summarized below used IV-D administrative cost data provided by the states in annual reports to the federal Office of Child Support Enforcement (OCSE) and measures of various state statutory tools and the dates of their adoption, data provided principally by OCSE and the National Conference of State Legislatures. Dependent variables used in all of these key studies were derived from mothers' reports of child support receipt.

The studies noted here employed a variety of strategies to accommodate the time between adoption or implementation of laws and the time by which one could reasonably expect the effects to be observed. Garfinkel and Robins used both continuous measures of the number of years since implementation of a law and a single indicator of whether a state had a law in place. Freeman and Waldfogel constructed an index of state laws that allowed for implementation delays by using a two-year lag. Beller and Graham and Sorensen and Halpern also utilized the lagging strategy to accommodate the time between adoption and implementation of legislation and the appearance of the posited effects. Beller and Graham lagged their enforcement laws' enactment date by two and four years; Sorensen and Halpern chose to lag only three of their six similar measures of the adoption of legal tools.

One difference among these studies involves the handling of effects that may be attributable to state (or regional) characteristics, which may not be easily captured. These characteristics might include a political disposition to enforce the law or to support those agencies that do, or something as concrete as the nature of the administrative infrastructure for child support enforcement. Freeman and Waldfogel and Sorensen and Halpern utilized state measures to control for this kind of "fixed effect" in their models that explored the impact of selected state IV-A and IV-D administrative and legal characteristics on child support outcomes. Beller and Graham chose instead to include regional, urban/rural, and Metropolitan Statistical Area effects upon order status and levels of receipt in models that focused on individual characteristics of survey participants.¹⁵ Garfinkel and Robins also chose controls for regional effects in their exploration of the effect of a number of child support administrative and legal characteristics on

¹⁵In another analysis, also published in the 1993 volume, Beller and Graham examined other questions, such as AFDC reciprocity and child support, in which regional controls were employed. They did not, however, employ either state or regional fixed effects controls in their examination of the impact of state enforcement tools on child support outcomes.

selected child support outcomes. In their analyses of the impact of legal tools available to the states on child support outcomes, all four of the research teams chose to control as well for temporal effects.

The studies reviewed here differed in their measures of the legal tools adopted and implemented by the states to strengthen the child support system. Some of the differences reflect limits imposed by the time periods covered by the studies. In some cases, passage and implementation of laws mandated by federal law had not been completed across states by the end of the period under study. In others, the range of time over which states adopted particular laws was quite narrow, limiting the amount of variance in the measure and a full assessment of its impact on specific child support outcomes. Each of these four studies, however, included at least one independent variable that measured a key administrative feature of the federal child support enforcement program: each state's total expenditures for operating the IV-D program. This measure understates the total public costs of child support enforcement, because it captures only state expenditures that are eligible for federal Title IV-D matching funds.¹⁶ The findings related to the impact of IV-D expenditures on the child support outcomes used in these four studies varied, depending on the structure of the analysis designed by the specific research team, as summarized below.

The studies reviewed here suggest that many of the states' enabling child support enforcement laws and administrative expenditures and practices have made significant contributions to receipts reported by single and divorced mothers. Beller and Graham's earlier study found automatic income withholding to have been an effective means for increasing women's child support receipts, as did Garfinkel and Robins's research. The latter study found women's reports of child support orders and receipts during an early period in the federal program's history to have been significantly and positively affected by their measure of immediate wage withholding. Noteworthy is the fact that by the end of their study years (1978–85 for Beller and Graham, and 1978–87 for Garfinkel and Robins), nearly a third of the states had not yet implemented this legislation. Sorensen and Halpern, whose span of years studied was more extensive at both ends of the range, found immediate withholding to have been a significant factor in women's reports of receiving child support, but only for a subpopulation of AFDC recipients who had been previously married. The methodology employed by Freeman and Waldfogel did not allow for disaggregating the effects of specific laws, including immediate wage withholding, on child support outcomes.

Beller and Graham found little support for the influence of IV-D expenditures in increasing child support receipts, although they did find that *efficiency* measures increased child support receipt rates by small but significant amounts. It is important to note, as do the authors, that these findings relate to the child support receipt experience of a sample of *all women*, not just those who were clients of the IV-D agencies in each state. In some years included in their study, it is unlikely that most women who were potentially eligible for child support had ever been clients of the IV-D agency. Sorensen and Halpern, using data for a comparable CPS subpopulation across a much longer time period, 1976–97, tested separate models for AFDC and non-AFDC and ever-married and never-married women. Their reasoning was that women who received AFDC would be more likely to have received child support enforcement services provided by the state IV-D agency than women in the general population. It was not surprising, then, that Sorensen and Halpern found that IV-D administrative variables and legal tools required by

¹⁶It does not capture any of the private costs of enforcement, including those associated with private litigation and collection. During the early years of the federal IV-D child support enforcement program, as some county- and city-operated enforcement and collection services were being gradually subsumed under the IV-D administrative umbrella, the public costs of enforcement lying outside the IV-D program were considerably higher than in the later years of the program. However, during the later years, the private costs of child support enforcement have been fueled by the appearance of for-profit companies that are especially aggressive in pursuit of arrears.

federal law showed far greater significance in predicting child support receipts for AFDC recipients than for their non-AFDC counterparts.

In addition to an array of demographic control variables that sought to capture some of the same factors explored in the earlier studies, the models developed by Sorensen and Halpern also included measures of the economic environment in the state where the CPS respondents lived. To control for the availability of resources from which the fathers of children could potentially draw to pay child support, single men's mean earnings and the overall employment rate were included. Also included in their models were six measures of statutory enforcement tools, including immediate wage withholding. For the purposes of our own study, it is especially important to note that the Sorensen and Halpern study was the only one of those summarized here that included a measure of each state's \$50 disregard status.

The principal findings of the Sorensen and Halpern study offer further evidence of the positive results of expanding the scope of child support enforcement programs through such tools as presumptive guidelines and state income tax intercepts. The researchers found that these two factors and the \$50 disregard had significant, positive effects on the child support receipt rates of both never-married and previously married mothers. The researchers further found that in-hospital paternity establishment programs and immediate wage withholding increased the likelihood of receiving support among some groups of mothers.

Sorensen and Halpern found suggestive evidence that child support receipts for AFDC recipients were more likely during periods in which the \$50 disregard was required, an effect not found in the non-AFDC portion of their study population. Further, they found that the IV-D and IV-A administrative variables, including the \$50 disregard, explained over 80 per cent of the earlier increases in child support receipt rates for previously married mothers who received AFDC.

The studies reviewed above, as well as other research, indicate the potential importance of administrative and statutory tools designed to strengthen child support enforcement. Two contributions in the Sorensen and Halpern study are particularly noteworthy. First, they included a measure of the disregard, a potentially important policy for AFDC/TANF recipients. Second, they found that the factors associated with child support receipt vary substantially by the AFDC/TANF status of those potentially eligible for child support.

We extend and improve upon previous research with our new measure of the disregard policy in effect in each state during each period and by explicit exploration of two additional measures of state IV-D administrative characteristics not previously studied: "new-hire" reporting and full federal certification status of states' automated systems, described in more detail below. We also use an alternative data source, administrative data on child support outcomes. Previous research has used mothers' reports of child support received and mothers' reports of their AFDC/TANF status, a limitation in that AFDC/TANF status is significantly underreported in the CPS. Moreover, the amount of child support that AFDC/TANF recipients report may reflect the amount of child support passed through to them, rather than the amount collected on their behalf. By using IV-A administrative data on child support outcomes related *directly* to AFDC/TANF recipients in IV-D caseloads, we are able to explore more accurately whether the disregard has had an effect on those outcomes, especially on collections on behalf of these clients.

III. Data and Methods

As detailed above, previous research has generally relied on micro data from the March and April CPS. The findings reported here are based on annual state-level data from state IV-D and IV-A administrative sources related to the characteristics and performance of these programs. These

administrative data allow us to focus more specifically on the population that is most likely to have been referred to and to have used IV-D program services in each of the states.

Data from other sources have been merged with these administrative data in order to estimate the effects of environmental factors, including states' relative economic health. The years covered by our analyses are, unless otherwise indicated, 1985 through 1998.¹⁷ We do not use the early years following establishment of the IV-D program, a period when anecdotal accounts suggest that federal requirements establishing a child support disregard for AFDC recipients may not have been fully implemented across all states. We consider observations through 1998 in order to include as many years as possible following the 1996 federal reform act, which abolished the federal disregard mandate and allowed them at state option. Our unit of analysis is each state (including the District of Columbia) in each year, from 1985 to 1998.

We used three primary measures of state IV-D program outcomes:

- Ratio of AFDC/TANF paternities established for the IV-D caseload to the size of the IV-A (AFDC/TANF) portion of the IV-D caseload;¹⁸
- Ratio of AFDC/TANF cases with collections to the number of IV-D AFDC/TANF cases;
- Average child support collection for AFDC/TANF cases that had any collection.

All other things being equal, we expect that a more generous disregard will have a positive effect on each of these indicators of IV-D program performance for the IV-A (current and former AFDC/TANF recipients) portion of the state IV-D caseloads.

The explanatory variables in our models include some of the administrative measures that previous research had found to have an impact on child support outcomes, as well as original variables that had not previously been tested. These variables fall into two general categories: those that measure characteristics of a state's enforcement effort, including enforcement laws or characteristics of enforcement systems; and measures of the overall state effort on behalf of its dependent children, including the financial parameters of both the IV-A and IV-D programs, such as the state's official Needs Standard, welfare benefit schedule, and, of course, the subject of our investigation, the size of any child support disregard. Among these two types of variables, we chose the following:

- Total disregard, measured as the sum of the federally required fixed disregard (\$50 after 1975) and the variable disregard that occurs in some states that pay welfare benefits that are less than that state's official Needs Standard.

¹⁷F.F.Y. 1999 data were occasionally used for some limited analyses reported here, and are so noted. However, major changes in OCSE reporting requirements in 1999 made state data for that and subsequent years insufficiently comparable for use in our key analyses. The authors are especially grateful to Gaile Maller and her staff with OCSE, who developed special data extract files for our use in this study.

¹⁸We also estimated models using an alternative measure, the ratio of the number of IV-D paternities established to the number of nonmarital births in the state. The results were similar.

- Full federal certification of the state’s automated system (dummy variable=1, beginning the year that a state’s automated information and case management system was fully certified by federal authorities).¹⁹
- New-hire reporting (dummy variable=1, beginning the year that a state implemented legislation requiring employers to report all “new hires” to the state’s designated agency for use by the IV-D agency).
- Immediate wage withholding (dummy variable =1, if enabling legislation was implemented by the state at least one year prior).
- Total costs of the IV-D child support enforcement program in each year/state, 1999 dollars.
- Maximum AFDC/TANF benefit size for family of three in each year/state, 1999 dollars.
- Female unemployment rate, measured in each year/state.
- Household median income, measured in each year/state.²⁰

All dollar-value variables were adjusted to 1999 dollars.²¹ Nominal-type, dichotomous variables were assigned the value of “1” for each year-forward from the time of passage or implementation, depending on whether or not the legislative adoption date was used for a particular measure. Thus, variables such as “new-hire reporting” would have been represented in the data code as “0” in each year prior to adoption and “1” for the adoption year and each year thereafter.

¹⁹We are grateful for the collaborative effort between the federal OCSE and the National Conference of State Legislatures that resulted in these data. Our understanding is that the dates of federal certification were obtained in a retrospective fashion and based upon federal certification standards that were in place at the time one of three certification statuses of each state system was awarded. “0” was the value assigned to the category consisting of both the “partially certified” and “not certified” status.

²⁰Data that were used to construct the amount of the disregard were obtained from documents relating to the recent status of states’ \$50-disregard practices published by the Center for Law and Social Policy (CLASP). Additional data pertaining to historic disregard practices, particularly those referred to as “fill-the-gap” and those connected with demonstration projects under federal waivers, were obtained directly from federal and state IV-A and IV-D program administrators. We are grateful for the cooperation of countless officials of the federal Office of Child Support Enforcement and the Administration for Children and Families in both federal and regional offices and state IV-A and IV-D administrative officials for their assistance in helping us construct a comprehensive historical record of these practices.

The immediate wage withholding variable through 1987 was provided by Irwin Garfinkel and Philip Robins, and from 1987 through 1999 by the National Conference of State Legislatures. Dates of system certification and new-hire reporting were also provided by the National Conference of State Legislatures. IV-D program performance data were provided by OCSE. Administrative data pertaining to states’ IV-A programs were obtained from the *Green Book* of the Committee on Ways and Means of the U.S. House of Representatives and from Vee Burke, at the Congressional Research Service. Female unemployment rates were obtained from official documents of the U.S. Bureau of Labor Statistics, Department of Labor. State median income data were obtained from the U.S. Census Bureau’s Current Population Survey, but because of concerns about the accuracy of single-year estimates, we include a rolling two-year average. We are grateful to all those who helped provide data.

²¹All conversions to 1999 dollar amounts were made using the CPI-U.

Our key independent variable, total disregard amount, is a continuous measure of the combined total fixed and variable disregards in each state and year, beginning in 1985 and continuing through 1998. As described earlier in this report, in most states/years, the fixed disregard component of the total disregard variable is the \$50 amount required by federal law. This portion of the total disregard measure was reduced to “0” in many states after 1996. However, a few states have had, at various points over the range covered by the analysis, “variable” disregards. This variable disregard component of the total disregard measure used in the analyses was measured as the “gap” amount for a family of three in each state that had such variable disregard practices. Also included, primarily in the latter years covered by the study, were some fixed disregard measures for a handful of states that had federal waivers to experiment with different levels of fixed disregards, such as Connecticut, or those such as Nevada and Kansas that changed their fixed disregards after PRWORA.²²

We expect that the size of the total disregard, which varied considerably over this range of years and states, will have a significant and positive effect on our measures of IV-D program outcomes. While we expect that paternities established and cases with collections will be positively influenced by the magnitude of disregards, there is less reason to expect that average collections for cases with any collections would be significantly affected by the size of a state’s IV-A disregard. Once a woman has initially “cooperated” with paternity determination, location, and other processes leading to an order for support, it may be that the *size* of the amount collected, once any collection is made, is less responsive to the mother’s efforts. On the other hand, to the extent that our model captures incentives for fathers to *pay support*, higher disregards may lead to higher payments by fathers, reflecting payments in amounts that more closely approximate their support orders.

We chose to include measures of federal certification of states’ automated systems in large part because certification may be a proxy for other administrative features that are difficult to measure directly. Full certification of a state’s electronic information processing and retrieval and accounts management system may represent a higher level of functional, administrative, and geographic coordination between the various components and myriad processes of the IV-D program.²³

Our next variable capturing a qualitative characteristic of a state’s child support enforcement system measures the effect of the state’s implementation of a law requiring employers to report all “new hires” on a monthly basis for access by IV-D personnel to locate the nonresident parent, to verify employment and salary information, and to collect child support. Because this tool serves more than one of the functions necessary for the successful enforcement of child support obligations, we expect to find that it adds significantly to the effectiveness of the child support enforcement system. Further, we believe that the independent effects of this enforcement tool will be observed immediately with implementation, and we do not lag this variable.

Our choice of immediate wage withholding as an explanatory variable is informed by previous research findings, including those summarized above. We chose to lag this variable, by one year past the

²²Wisconsin stands alone as having recently adopted a *full* “variable disregard” which was, for the purposes of this study, measured as the average amount of the child support collection for the first year of Wisconsin’s experimental study.

²³We chose a binomial measure of certification that reflects only full certification, with partially or provisionally certified and uncertified systems as the omitted category. Our rationale for this is that the breadth of the “middle,” or partially certified, category is likely very broad, and we have no information on the nature or severity of the deficiencies that have stalled full certification.

implementation date, as in some earlier research.²⁴ We expect that this administrative tool will be a significant factor in predicting collections, but not paternities established, as its relationship to this measure of program effort seems remote, at best.

In addition to these key explanatory variables, we include a number of control variables. The total cost of states' IV-D operations is a standard measure of the level of effort committed in pursuit of enforcement outcomes. Maximum AFDC/TANF benefit for a family of three was chosen as a control variable in our models because of the historic relationship between a state's welfare benefit structure and child support outcomes (Cassetty, 1983). The size of the welfare benefit can be expected to influence the relative value of child support receipts and, thus, cooperation with such child support enforcement activities as paternity determination and pursuit of orders for support. Including the size of the benefit also may control for differences in the types of cases receiving AFDC or TANF in different states at different times.

The female unemployment rate was chosen to represent the need for AFDC/TANF and the services of the child support enforcement agency. Assuming a high unemployment rate for women, one would expect that they would have greater economic need for these services and be more eager to cooperate fully with IV-D program efforts, leading to a positive impact on the performance of state IV-D operations, all other things being equal. High female unemployment rates may also alter the composition of the AFDC/TANF caseload, bringing in those with higher potential for child support payment (Cancian, Meyer, and White, 2000). On the other hand, one might envision the opposite effect: low unemployment among the female workforce may encourage a desire for self-sufficiency, which may include pursuit of child support. Moreover, low unemployment among the female workforce is closely linked to overall economic conditions, so may be associated with an increased ability for fathers to pay support.

We include median income primarily as a control variable, although we expect that to the extent median income reflects the income available to nonresident parents, higher median incomes will be associated with increased child support collections and increased willingness to take on the legal responsibilities of fatherhood through cooperation with paternity determination efforts.

Finally, our base results follow much of the recent literature and include both state and year fixed effects.²⁵ As such, we are estimating the effects of other variables—including the disregard—on variation not accounted for by the state or year of observation alone.

IV. Results

As our primary interest is the effect of state AFDC/TANF disregard practices, our principal analyses estimate outcomes for this share of the state IV-D caseloads. We analyze three outcomes: ratio

²⁴Because the state IV-D caseload data constitute a "mix" of cases in various stages of enforcement, it is not reasonable to assume that the effects of wage withholding will show up in study results for a period of time sufficient for appreciable numbers of new IV-D cases to be issued court orders for support. Unlike new-hire reporting, which involves routine, periodic "sweeps" and comparisons of caseload databases made up of enforcement cases of all status types against state new-hire files, immediate wage withholding requires a longer period of time for the results of court action on a cumulative portion of the caseload to be observable. Thus, a "build-up" of cases effected by immediate wage-withholding laws is expected to be necessary before analytic results can be observed.

²⁵1998 is the omitted year category and Wyoming is the omitted state category in our regression equations.

of paternities established to caseload size, ratio of cases with child support collections to caseload size, and collection levels among cases with collections. We estimate the relationship between these outcomes and several child support program features, particularly the disregard. As discussed above, our models also include measures of the economic environment—including maximum AFDC benefit levels, female unemployment, and median income—as well as state and year fixed effects.

Table 3 summarizes the results of our analysis. We find that the disregard has a positive and statistically significant effect on paternity establishment and the proportion of cases with collections. The coefficient estimates suggest that the effect is fairly small—a \$100 increase in the disregard is associated with almost a one-percentage-point increase in the ratio of paternities established to the size of the AFDC/TANF caseload ($.0085 \times 100 = 0.85$ percentage point) and a slightly larger increase in the proportion of AFDC/TANF cases with collections ($.0145 \times 100 = 1.5$ percentage points).²⁶ Although the estimated effects are small they suggest potentially important results. For example, given that there were almost 10 million welfare cases in the states' IV-D caseloads in 1996, our estimates suggest that a \$100 increase in the disregard would be associated with 150,000 additional cases with collections.²⁷ The findings for the new-hire reporting variable also note a significant impact on cases with collections, suggesting an improvement of approximately 2 percentage points in cases with collections for those states with a new-hire database over those without. If all states without a new-hire database in 1996 had adopted one, we estimate an increase of about 40,000 in the number of cases with collections nationally.

As shown in the third set of columns, the size of the disregard does not have a discernible effect on the average collection per case among cases with any collections, although the direction of the coefficient remains positive. This may be because higher disregards increase the proportion of AFDC/TANF cases with collections, increasing the representation of cases of all kinds, including low-paying cases. On the other hand, it may be that although disregards encourage resident parents' cooperation with IV-D program efforts, this works to increase paternity establishment and initiation of payments, but does not influence the amount paid by nonresident parents who are already paying something.

Full federal certification of a state's automated data system has no discernible impact on paternity establishment or the proportion of cases with collections, but has a significant relationship with average collections for cases with any collections. The estimates suggest that this status, conferred on a state by the federal Office of Child Support Enforcement after a lengthy and complex process of systems development and federal review, results in an increase of over \$790 per year per AFDC/TANF case with any collections. While these are cases that have already had at least one collection, the incremental increase in the size of the average collection that is associated with full certification is sizable. Do fully certified systems increase the effectiveness of child support enforcement in maintaining a steady stream of child support payments, once an obligation is established? Or is full certification capturing another unmeasured characteristic of the states that have fully certified systems? Given the strength of these results, this measure of child support policy warrants closer investigation.

²⁶For example: if a state has a ratio of paternities established to the size of the AFDC caseload of, say, 700 to 10,000 (700/10,000, or .07), a one-percentage-point increase in paternities established would raise the ratio to .08. This means that a \$100 increase in the disregard would increase paternities from 700 to 800 in an AFDC/TANF caseload of 10,000.

²⁷Given the coefficient estimates and standard errors, the 95 percent confidence interval suggests an increase in cases with collections of 37,000 to 253,000.

TABLE 3
IV-D Program Outcomes for IV-A (AFDC/TANF) Caseloads, 1985–1998

Independent Variable	Dependent Variables					
	Paternities Established/IV-A Caseload		IV-A Cases with Collections/IV-A Caseload		Mean IV-A Collections/IV-A Cases with Any Collections	
	Coefficient	p-value	Coefficient	p-value	Coefficient	p-value
Maximum effective disregard	0.0085***	0.0012	0.0145***	0.0086	0.4344	0.6844
Full automated system certification	-0.4096	0.4904	-0.8005	0.5203	790.9326***	0.0011
New-hire reporting required	0.6348	0.1174	2.0168**	0.0178	-78.1580	0.6354
Immediate withholding (lagged)	0.2916	0.5062	1.0238	0.2656	-478.9029***	0.0075
IV-D program expenditures	0.0153***	0.0013	0.0033	0.7391	-1.5920	0.4111
AFDC/TANF benefit maximum for family of three	0.0106***	0.0030	0.0209**	0.0051	1.8334	0.2040
Female unemployment rate	-0.4867***	< .0001	-0.8865***	< .0001	-35.5530	0.3942
Median income	-0.2266***	0.0004	-0.6405***	< .0001	104.1889***	< .0001
	N = 714		N = 714		N = 714	
	R ² = .4062		R ² = .5213		R ² = .6260	

Note: State and year fixed effects and an intercept are included in all models, but coefficient estimates are not shown.

Bold print indicates statistical significance:

* = p < .10

** = p < .05

*** = p < .01

Not surprisingly, new-hire reporting appears associated with a greater likelihood of making a collection. It does not, however, appear to be a significant factor in enhancing the average size of collections among cases with collections. It also does not have any discernible impact on paternity establishment. It may be the case that among nonresident parents who are trying to avoid their support obligations altogether, new-hire reporting systems reduce the success of those attempts. Among those who are paying all or a portion of their obligations already, new-hire reporting systems are not likely to increase the size of those collections.

The final measure of child support enforcement tools is the implementation of legislation to authorize immediate withholding of child support. Although we did not expect immediate withholding to have an impact on paternity establishment, we did expect a positive relationship between this measure and the proportion of cases with collections and the amount collected among cases with collections. Contrary to our expectations, the only discernible relationship is a significant *negative* relationship with average collections per case with any collections. The average collection for cases with any collections that is attributable to our measure of immediate wage withholding drops more than \$478 per case ($p < .01$). When the impact on average collections over all IV-D cases is estimated, the magnitude of this effect is not different from zero, suggesting that at least part of this unexpected result may be explained by this policy's contribution to increasing the representation of low-paying cases among those cases with some collections.²⁸

The next three rows of Table 3 show the relationships between the outcomes and three additional indicators of child support policy. Higher child support enforcement expenditures are associated with greater paternity establishment, but have no discernible impact on either measure of collections. Given the myriad processes involved in paternity determination, it is not surprising that paternity outcomes are closely associated with the level of program expenditures. Paternity determination is possibly the most complex and labor-intensive of the many tasks associated with the overall child support enforcement effort. What is surprising is that we do not find an association between expenditures and collections, all other factors equal.

The next row in Table 3 shows the relationship between welfare generosity, as measured by the size of the welfare grant, and our measures of child support outcomes. Higher maximum AFDC/TANF benefits are associated with higher rates of paternity determination and a higher proportion of cases with collections. The association with average collections per case with any collections is not statistically significant ($p = .20$). This may reflect variation in the composition of the IV-D caseload; higher AFDC/TANF benefits may mean that less disadvantaged families participate in the welfare system because welfare is more attractive. Higher welfare benefits may also lead to longer periods of benefit receipt, leading to a longer opportunity for paternities to be established and child support collections to begin.

²⁸In other words, if low-income fathers are disproportionately represented in the "pool" of fathers who pay support as a consequence of immediate wage withholding, the average amount collected for *all* paying cases will decline due to the smaller amount of support that is assumed to be ordered for such fathers. It would then appear that wage withholding "caused" average collections to decline among all cases with any collections when, in fact, the average collection declined as a result of a poorer pool of payors. An alternative explanation is offered by Meyer and Bartfeld (1992), who suggest that if a significant proportion of those subject to initial withholding changes jobs frequently thereafter, some doing so deliberately to avoid further support collections, the effectiveness of immediate wage withholding may decline over time.

The final two rows of Table 3 show the estimated relationship between child support outcomes and two control variables. Higher female unemployment rates are associated with lower paternity establishment and a lower proportion of cases with collections, perhaps reflecting different compositions of the caseload or reflecting a lowered ability of fathers to pay support. Higher median income is also associated with lower paternities established and lower cases with collections, but higher collections per case. Although the positive relationship between collections for cases with any collection and median income is intuitive, the negative findings for paternities determined and cases with collections are not. It is possibly the case that as state median income rises, so does the ability of putative fathers to defend themselves against allegations of paternity. But once paternity is successfully established, average collections are higher in response to higher median income.

The results summarized in Table 3 suggest that a higher disregard is significantly associated with higher rates of paternity establishment and a higher proportion of cases with collections, but has no discernible impact on average amounts collected for cases with collections. We conducted three sets of sensitivity analyses on this sample, with results shown in Table 4. First we eliminate state and year fixed effects, then we eliminate both. Our results regarding the disregard are generally robust to the alternative specifications shown. A higher disregard is consistently associated with higher paternity establishment and a higher percentage of cases with collections. In general, the disregard has no impact on average collections per case.²⁹

In our base results, the measure of the disregard was the *maximum* amount disregarded; our second sensitivity test considers alternative measures of the disregard, the increment to income for a family of three if \$100 per month, \$150 per month, or \$200 per month were paid in support. Changing the measure of the amount disregarded also shows that a higher disregard is associated with higher rates of paternity establishment, and continues to show that the disregard amount is not significantly related to collections per case with any collections. However, this measure of disregard size shows no discernible relationship with the percentage of cases with collections. This suggests that the relationship between disregard policy and cases with collections seen in the base model may be driven by the high maximum disregards available in some states. Future research should further explore this finding.

It is also important to note the need for future research aimed at an exploration of both the independent and interaction effects of pass-throughs and disregards. Because this research began with an assumption that pass-throughs were identical in size and application to disregards, given extensive confusion between the two policy terms in the academic literature, we gathered data related only to the level of disregards. As our understanding of various state practices related to pass-throughs grew, we began to view pass-throughs and disregards as distinct policies that may have different effects. We now believe that it will be useful to sort these effects out more carefully following a detailed historical review of state pass-through practices, a review that is bound to be time-consuming due to the absence of a central repository of highly variable historical data and the absence of uniform mandatory federal policies governing state pass-through practices.

The level of the disregard should affect outcomes only for families receiving AFDC/TANF. Our final sensitivity test examines whether the level of the disregard is associated with non-AFDC/TANF case paternities and/or non-AFDC/TANF case collections, using the same model. In contrast to our findings for AFDC cases, the final row of the table shows no discernible relationship with paternities or mean

²⁹An exception is the marginally significant negative relationship between mean collections per case in a model without state or year fixed effects.

TABLE 4
Sensitivity Test Results: IV-D Program Outcomes for IV-A (AFDC/TANF) Caseloads

Model Specification	Dependent Variables								
	Paternities Established/IV-A Caseload			IV-A Cases with Collections/IV-A Caseload			IV-A Mean Collections/IV-A Case with Any Collections		
	Disregard Coefficient	p-value	R-squared	Disregard Coefficient	p-value	R-squared	Disregard Coefficient	p-value	R-squared
Base model	0.0085***	0.0012	0.4062	0.0145**	0.0086	0.5213	0.4344	0.6844	0.6260
Without state fixed effects	0.0027*	0.0559	0.0724	0.0151***	< .0001	0.1281	-0.9386	0.1428	0.2934
Without year fixed effects	0.0084***	0.0011	0.3983	0.0146***	0.0074	0.5006	-0.4088	0.7102	0.5795
Without state and year fixed effects	0.0029**	0.0407	0.0604	0.0153***	< .0001	0.0915	-1.0574	0.1080	0.2293
Effective disregard if \$100 paid	0.0128	0.1038	0.3989	0.0065	0.6923	0.5162	-1.4565	0.6459	0.6260
Effective disregard if \$150 paid	0.0117**	0.0405	0.4003	0.0049	0.6818	0.5162	-0.0088	0.9969	0.6259
Effective disregard if \$200 paid	0.0105**	0.0152	0.4019	0.0060	0.5080	0.5164	0.3414	0.8453	0.6259
Base model for non-AFDC caseloads	-0.0023	0.1808	.4628	-0.0280*	0.0614	0.4408	-0.4080	0.9205	0.1215

Bold print indicates statistical significance:

* = p < .10

** = p < .05

*** = p < .01

collections and a marginally significant *negative* relationship between disregard levels and the proportion of non-AFDC/TANF cases with collections.³⁰

V. Summary and Policy Implications

Our analyses suggest that increases in the disregard applied to child support collections among AFDC/TANF recipients are associated with increased paternity establishment and a greater proportion of cases with collections. While many states have eliminated disregards under TANF, a focus on cost savings realized from discarding disregard practices may be short-sighted. Our findings offer evidence that disregards may play an important part in fostering cooperation with child support laws. Results also point to the potential importance of new-hire reporting and fully certified state automated systems, enforcement tools not previously studied in this context.

The analyses reported here rely exclusively on a measure of state disregards. The distinction between the structure and application of state *disregard* and *pass-through* practices is potentially important and bears further comment. The fiscal implications of changes in the pass-through and changes in the disregard are quite different—for both government and low-income families. Disregards increase the income available to families and are designed to foster cooperation with child support enforcement objectives by providing direct economic incentives to do so. A pass-through does not increase the economic resources at the disposal of a welfare recipient. However, a separate check (or electronic deposit) representing a pass-through of child support would provide TANF recipients with better information on the level and reliability of child support—particularly important information as they move off of cash assistance. The public policy debate and the research literature would benefit from clarifying the distinction between the pass-through and disregard of child support.

³⁰Appendix Table A shows the full results of the application of our basic models using administrative data that pertain to the non-AFDC/TANF portion of the IV-D caseload. Appendix Table B shows results when AFDC/TANF and non-AFDC/TANF administrative data are combined.

APPENDIX TABLE A
IV-D Program Outcomes for Non-IV-A (Non-AFDC/TANF) Caseloads, 1985–1998

Independent Variable	Dependent Variables					
	Paternities Established/Non-IV-A Caseload		Non-IV-A Cases with Collections/Non-IV-A Caseload		Non-IV-A Mean Collections/Non-IV-A Cases with Any Collections	
	Coefficient	p-value	Coefficient	p-value	Coefficient	p-value
Maximum effective disregard	-0.0023	0.1808	-0.0280*	0.0614	-0.4080	0.9205
Full automated system certification	-0.5897	0.1360	-9.0210***	0.0059	-888.4618	0.3569
New-hire reporting required	0.2618	0.3316	-0.0858	0.9703	373.3896	0.5539
Immediate withholding (lagged)	0.0941	0.7472	-0.0800	0.9744	-16.0083	0.9813
IV-D program expenditures	0.0152***	< .0001	0.0749***	0.0059	5.8049	0.4335
AFDC/TANF maximum benefit for family of three	0.0006	0.8108	0.0406**	0.0443	-0.9381	0.8651
Female unemployment rate	-0.0025	0.9709	-0.4846	0.4060	135.4590	0.3963
Median income	-0.00801	0.8505	-0.5798	0.1105	112.3350	0.2587
	N = 714		N = 714		N = 714	
	R ² = .4628		R ² = .4408		R ² = .1215	

Note: State and year fixed effects and an intercept are included in all models, but coefficient estimates are not shown.

Bold print indicates statistical significance:

* = p < .10

** = p < .05

*** = p < .01

APPENDIX TABLE B
IV-D Program Outcomes for Total (IV-A plus Non-IV-A) Caseload, 1985–1998

Independent Variable	Dependent Variables					
	IV-D Paternities Established/Total IV-D Caseload		Total IV-D Cases with Collections/Total Caseload		IV-D Collections/All Cases with Any Collections	
	Coefficient	p-value	Coefficient	p-value	Coefficient	p-value
Maximum effective disregard	0.0051***	0.0008	0.0009	0.8463	0.1245	0.8928
Full automated system certification	-0.4193	0.2234	-0.2714	0.7900	-59.1111	0.7771
New-hire reporting required	0.4643**	0.0483	1.8498***	0.0080	30.5243	0.8303
Immediate withholding (lagged)	0.2193	0.3882	0.2059	0.7844	-199.0300	0.1971
IV-D program expenditures	0.0124***	< .0001	0.0070	0.3884	-0.1313	0.9375
AFDC/TANF benefit maximum for family of three	0.0036*	0.0800	0.0168***	0.0059	0.7617	0.5412
Female unemployment rate	-0.2172***	0.0003	-1.0424***	< .0001	41.4342	0.2506
Median income	-0.0974***	0.0087	-0.4789***	< .0001	45.3905**	0.0435
	N = 714		N = 714		N = 714	
	$R^2 = .4737$		$R^2 = .7064$		$R^2 = .5142$	

Note: State and year fixed effects and an intercept are included in all models, but coefficient estimates are not shown.

Bold print indicates statistical significance:

* = $p < .10$

** = $p < .05$

*** = $p < .01$

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Chapter 2

Child Support Disregard Policies and Program Outcomes: An Analysis of Microdata from the CPS

Judith Cassetty¹

I. Introduction

Disregards that are applied to child support collections by state agencies that administer federal Title IV-A programs (AFDC and TANF) have historically been the topic of substantial policy discussion, but have seldom been studied in an empirical context.² Advocates believe that these disregards provide a real and immediate economic incentive for AFDC/TANF recipients to cooperate with paternity determination and child support enforcement requirements. Others are skeptical that disregards have any influence on recipients, independent of stringent administrative requirements for cooperation and sanctions for failure to cooperate. This study uses data from the March Current Population Surveys, conducted annually by the U.S. Census Bureau, to test for cooperation-incentive effects associated with disregards. Studies of such effects are in general absent from the child support literature (exception noted later in context of review of Sorensen-Halpern study, 1999), at least in part because it is very labor-intensive to develop historical state-level data on disregard practices, including those associated with *variable* disregards (frequently referred to as “fill-the-gap” practices, discussed in Section III).

The act authorizing the establishment of the federal/state Child Support Enforcement (IV-D) Program in late 1974 was soon followed by federal rules mandating state disregard practices for welfare (IV-A) clients. Federal mandates for state disregard practices continued until 1996, when they were made voluntary under the welfare reform initiatives enacted as the Personal Responsibility and Work Opportunity Reconciliation Act. Nearly half the states then ceased all disregard practices for their welfare clients, despite the lack of evidence on the effects of disregards on paternity and child support enforcement outcomes.³

¹The author gratefully acknowledges the assistance of Royce Hutson in gathering, compiling, and analyzing the data utilized in this report. Maria Cancian provided valuable suggestions related to the substantive features of this report, Dan Meyer offered many useful suggestions related to sensitivity testing and other technical procedures, and Jan Blakeslee, Elizabeth Evanson, and Dawn Duren provided essential manuscript preparation assistance, for which the author is very grateful. Responsibility for the final report, including limitations and errors, rests entirely with the author, however.

²The term “disregard,” as used in this paper, refers to the portion of a child support collection that is excluded from official budget calculations in determining the size of a welfare recipient’s monthly AFDC/TANF cash assistance. In other words, the benefit reduction rate as applied to child support collected for AFDC/TANF families is zero. While “disregard” may apply to other forms of income and types of assistance (such as Food Stamps), the term in this report applies only to the treatment of child support by state/federal welfare programs. “Pass-through,” which is often confused with the term “disregard,” instead refers to the distribution of a check or direct deposit that is distinct from a welfare check and is clearly identified as child support from the nonresident parent.

³The \$50 disregard was incorporated in federal regulations beginning in October 1976 (42 USCA s.657). However, implementation across the states was gradual, as each state’s IV-D program was developed and accounting practices were coordinated with those of the IV-A (AFDC) program that was responsible for administering the disregard. The \$50 disregard replaced an even more limited disregard: the original federal legislation establishing the IV-D program required that the state AFDC programs allow for a one-time-only disregard with a ceiling set at one-half of the first \$40 collected for any child in an AFDC family unit.

Until quite recently, the apparent lack of interest in empirically exploring the effects of disregards has been of little fiscal consequence to states, as the option of providing for disregards has been unavailable to them. After PRWORA, states were no longer federally mandated to provide for a fixed disregard in the amount of \$50, as they had been for twenty years. But along with congressional action rescinding the mandate came greater state fiscal responsibility for the costs of providing any disregard amount that each state chose to allow its welfare recipients. While this shift in fiscal responsibility to states that chose to continue providing disregards to clients was later modified under federal law, questions about the advisability of assuming the burden of costs for this policy continue among the states. The underlying empirical policy question then became whether the state costs of providing a disregard could be offset, at least in part, by increases in the rate of paternities determined and in collections that might result from a disregard of any given size.

The first step in answering this question is to derive empirical evidence of the effects of disregard practices, if any, on key child support outcomes. Only then can the economic benefits of state disregard practices be estimated and weighed against the fiscal costs of providing for such disregards.

Prior research relevant to these issues falls into four general categories:

1. *Empirical research findings* that have helped to identify other state-level laws and policies that may have an impact on child support receipts. The findings suggest the need for policy *controls* for these factors when seeking to sort out the independent effects of disregards.

2. *Indirect evidence* obtained from empirical research that suggests that there may be a relationship between state disregard practices and reported child support receipt (Sorensen and Halpern, 1999).

3. Results of a single *controlled experimental* state-level study that has produced estimates of the effects of alternative disregard practices on various child support outcomes.

4. Results from state and national studies using court record and administrative data that provide some *direct evidence* of disregard effects on child support outcomes.

The experimental findings, which this study complements, are discussed in Volume I, Chapter 2 of this larger volume and will not be summarized here. Briefly discussed, however, are four studies that use sample data from the March and April Current Population Surveys to explore the effects of state laws and policies designed to enhance child support enforcement outcomes.

The summary of key findings from the recently completed study of state IV-D program outcomes that used administrative data from the Office of Child Support Enforcement (OCSE) and additional data pertaining to state historic disregard practices may be found in the previous chapter (Volume III, Chapter 1).

The next sections of this report discuss the benefits and limitations of the March CPS data as a source of evidence concerning the effects of state disregard practices on child support outcomes and describe the empirical model employed for estimating the size of these effects. The final sections summarize key findings from this study, discuss their implications for policy, and suggest additional research that may help to give further definition to policy options.

II. Factors That Influence Child Support Outcomes

Among the four recent studies that have immediate implications for the present study, three do not attempt to capture the effects of disregards but offer important evidence of the effects of specific laws and

policies. Only one of the four explores disregard effects: Sorensen and Halpern (1999) compared respondents' reports of child support receipt in the years before and after 1984, the year that represents the most likely timing of full implementation of the original federal mandate to state IV-A agencies to disregard the first \$50 per month of any child support collected for welfare families. They concluded that study subjects reported receiving more support following full implementation of the \$50 disregard across all states. The years that are covered by the Sorensen and Halpern study extend shortly beyond enactment of PRWORA, allowing the authors to capture some of the effects of rescinding the federally mandated \$50 disregard, as well. Child support receipts were higher during periods when the \$50 disregard was in effect.

Researchers who conducted the four studies discussed here used survey data from the March CPS and/or the April Special Child Support Supplement, along with state-specific data on statutes, policies, and economic features, to derive estimates of the effects of these variables on mothers' reports of child support receipts. These studies are of importance in selecting key policy, demographic, and state economic factors as control variables for the present study. Meaningful control variables will help to ensure that any observed effects associated with the measure of state disregard levels are not inadvertently capturing the effects of variables that are not otherwise included in the model. The four research articles are all national in scope (Beller and Graham, 1993; Freeman and Waldfogel, 1998; Garfinkel and Robins, 1994; and Sorensen and Halpern, 1999). Together they exploit a wide range of individual demographic data and state demographic and economic data, offering important clues as to which factors most consistently demonstrate significant influence on women's reports of child support receipts. Taken as a whole, they indicate the dynamic and complex process that leads to higher child support receipts by female heads of families with minor children. These studies include both poor and nonpoor respondents, and not all of their findings have immediate relevance for the research focus in this document. While each research team acknowledges the limitations of the CPS data for the purposes of these kinds of analyses, their measures, research methodology, and theoretical frameworks vary considerably from study to study, offering a range of insights into the dynamics that shape child support outcomes. (A fuller discussion of data limitations is in Section III.)

More detailed discussion of the methodology and findings from these four background studies is included in the previous chapter (Volume III, Chapter 1), hereafter referred to as the "OCSE study" because it relies largely upon administrative data from the federal OCSE, and is not reiterated here. Table 1 lists the variables that each research team found to be positive and statistically significant in their attempts to explain the observed variance in women's reports of child support enforcement outcomes. Although the direction and magnitude of the coefficients reported in these four studies were frequently ambiguous, varying from one study to another and between models explored by each team, a few consistencies among them may be observed. Most important among them appear to be the following: Mothers' demographic variables, when included in the models, more consistently demonstrate their importance in estimations of most child support outcomes than other categories of variables; direction and significance of most policy variables used in these researchers' models appear to be very sensitive to specification, with ambiguous findings not uncommon; and child support enforcement program efficiency measures are more often found to be positive and significant than are straightforward measures of expenditures for state IV-D programs. Some research teams that employed controls for state and regional effects indicated the importance of individual state or regional effects as well. In sum, the factors that influence child support outcomes among similar sample populations of female heads of families appear to be multi-faceted and a complex mix of state or regional features, including economic "health," mother's individual and household demographic characteristics, and, to a lesser and more ambiguous extent, "tools" designed to strengthen enforcement.

Only one of the above-noted studies (Sorensen and Halpern) sought to measure the effects of a state disregard, and it examined only the federally mandated \$50 disregard. But in most of the years covered by that study, states were obliged by law to disregard this \$50, reducing the variance on which estimates of the impact may be made. The study reported here builds on this initial effort. As discussed

TABLE 1
Significant, Positive Explanatory Variables Found in Earlier Child Support Outcomes Research

Significant Variables	Beller and Graham	Freeman and Waldfogel	Garfinkel and Robins	Sorensen and Halpern
	April CPS CS Supplement, 1979, 82, 84, and 86	SIPP 1987, 92, and 94; March CPS (pooled), 79,82, 84, 86, and 88	April CPS CS Supplement 1979, 82, 84, 86, and 88	March CPS (pooled), 1976-97
Policy (state legal “tools” and policies):				
Wage withholding	**		**	***
IV-D expenditures (total and ratios)			*	
IV-D program efficiency	***	***	**	***
Voluntary agreement (for support)			@	
C.S. guidelines				*
Paternity to age 18			**	
In-hospital paternity registration				**
\$50 disregard				**
Tax refund offset	*			***
IV-D agency-sole state collection and distribution entity			***	
Advertising state cs enforcement services			**	
Liens on property	***			
Criminal penalties	**			
Maximum IV-A benefit in state				***
Bonded or secured guarantee	*			
All legal and administrative “tools”		@@note below		
Economic (state and individual):				
State average earnings (male)	No coefficients provided			***
State unemployment rate or employment ratio (male)				***
Respondent IV-A status				***
Demographic (household and head):		Controls only		
Mothers’ demographic characteristics (including age and race)	***	No coefficients provided		***
Number and ages of children	***	No coefficients provided		***
Marital status	*	Never-married, only		***
Years since marital disruption	**			

Note: These variables were not necessarily measured in the same way nor employed in the same types of empirical models, so table entries of coefficients do not allow for meaningful comparisons. Further, some variables produced negative, significant or ambiguous results when employed in alternative variations of the models.

* = p < .10

** = p < .05

***= p < .01

@= p < .15 (“Significance” at this non-traditional level represents choice of researchers.)

@@ Researchers found their composite “index” of most state child support laws to be significant in the absence of state and year effects. However, with these controls added, statistical significance for both the index of state laws and IV-D expenditures/efficiency variables dropped markedly and lost robustness.

below, a more detailed measure of the disregard is used, over more years, and thus captures more cross-state and over-time variation.

At least two reasons are possible for the absence of a robust measure of total size of state disregards in the literature to date. First, data related to the size of each state's "variable" disregard did not exist in any central repository or database before it was collected for the earlier disregard study employing OCSE administrative data. Second, March and April CPS data do not allow for the level of measurement detail that is required to estimate a continuous relationship between the size of a state's total effective disregard for each individual case and the magnitude of the effects on various interval measures of child support.

A further problem is that when data from other research into state pass-through practices were examined, uncertainty arose as to what was being measured. It is often *assumed* that the research was actually measuring disregards (though the term "pass-through" was used), but some ambiguity remains.⁴ Without accurate and consistent terminology it is difficult to interpret results in studies that use measures called "pass-throughs," in which the context suggests that they pertain to *disregard* practices. Discussion of the measurement of total disregard and the very important real and theoretical distinctions between pass-through and disregard effects is in the next section. The limitations inherent in the choice of CPS data for the kinds of policy analyses described and conducted are also discussed there.

III. Data and Methods

The previous chapter, the OCSE study, stands apart from the four studies summarized in Table 1 in several key ways. First, state IV-D program data, rather than individual-level data from the CPS, were used in order to focus more clearly on child support outcomes for the AFDC/TANF population of IV-D clients,⁵ the only women who could reasonably be directly affected by the incentives associated with a child support disregard policy. The present study includes a sample of female heads of household who reported having received some state public assistance benefits during the preceding year. The surveys were administered in March of the years 1985 through 2000, reporting on experiences in the preceding year (1984–1999).

The chief reason for utilizing CPS data is that they offer a view of the individual characteristics of female-headed families, which is lacking in IV-A and IV-D administrative data. By choosing to use CPS data, however, other key variables were lost, variables that were included in the OCSE study. The CPS provides individual-level demographic data, but the trade-off is the loss of ability to measure total child support paid for each welfare case, as the CPS includes only how much they received. Given information only on child support received, there is uncertainty about how much was actually paid by the obligors, because of varying state disregard and pass-through practices.

The practice of disregarding varying amounts of monthly child support collected on behalf of AFDC/TANF recipients existed in some states before the inception in 1974 of the federal child support enforcement program. In that earlier period, several states had in place practices whereby unearned income

⁴For example, data from Lewin and Associates state survey, as cited in 1999 Report to Congress by the Administration for Children and Families and the Office of Child Support Enforcement. This report is based upon a study by BETAH Associates and is entitled *Analysis of the Impact on Recidivism of PRWORA Child Support Arrears Distribution Policy Changes, U.S. Department of Health and Human Services*. See pages 12 to 14.

⁵The data that we use to represent IV-A cases in IV-D caseloads includes both active and former AFDC/TANF clients.

of several types was disregarded in amounts that “filled the gap” between the state’s official Needs Standard, adopted for annual use in a state by its legislature, and the AFDC/TANF payment standard. In order to develop an historical record of the size of each state’s variable disregard as it was applied to child support, it was necessary to track annual changes in both the IV-A benefit schedule and the size of the official Needs Standard. The difference between them, for any family of a given size, was the maximum size of the disregard available to the family in a “fill-the-gap” state. As the number of persons in a family’s IV-A eligibility unit changed, sometimes frequently, their position with regard to the Needs Standard changed and so, therefore, did the potential maximum size of the disregard for which they were eligible. In addition to the welfare benefits schedule and Needs Standard, in order to measure the precise size of any given family’s annual *potential* disregard, one must know both the total size of the disregard (fixed plus variable types) to which the family was subject in all months of the year covered by the survey, and the size of the AFDC unit in each month that they received AFDC. This level of detail is not possible in the March CPS.⁶

To estimate from survey responses the actual total disregard to which a welfare recipient family unit was subject in any given year, one would further need to know a state’s pass-through policy, the monthly amount that was actually collected for the family by the state’s IV-D agency, other family income, and how that income was treated by welfare rules. Because the March CPS respondents are asked only how much child support they received in the prior year, the reported amount is likely to include the amount passed through, possibly including the amount subject to a disregard. If the amount reported is less than the full amount for which a respondent is potentially eligible according to these calculations, it may be assumed that the amount collected from the nonresident parent was less than the full amount that the family was eligible to receive.

Because no known studies have accounted for disregards and pass-throughs in such a detailed way, reliance upon the results of studies which use CPS respondents’ reports of child support received, as approximations of support actually collected by the IV-D agency, are not dependable. As noted above, in Texas, for instance, 100 percent of all amounts collected for AFDC/TANF recipients is passed through as a separate check to each client family. However, the maximum amount ever disregarded by the IV-A agency was \$50. Thus researchers might assume that the amount an AFDC/TANF recipient reported in the CPS survey represented 100 percent of what was collected in child support. Based upon the woman’s report, however, nothing can be assumed about the size of the disregard to which she was subject. As most of the literature which addresses pass-throughs and disregards tends to assume that they are one and the same, the important real and theoretical differences are easily lost in the methodological constructs. This discussion will be expanded in the section that follows. Suffice it to say that findings from studies of disregard effects using CPS data are probably only suggestive, given the substantial limitations in the data.

The initial objective was to focus the sample selection criteria in such a way as to maximize the likelihood of including female householders with children who had been IV-A (AFDC/TANF) recipients at some point during the year covered by the survey. Even though this sharper focus entailed the loss of some level of comparability with the other studies, it was necessary to reduce the sample in this way in order to help ensure that the impact of state IV-A disregard practices would not be diluted by including female family heads and householders not subject to IV-A disregard rules. The technical report included in the Appendix shows the criteria used to exclude from the sample persons with particular characteristics and the numbers of observations lost from the initial sample.

⁶The CPS Child Support Supplement, administered in April, comes closer to allowing for this level of measurement, but lacks the number of years required for meaningful results on a sizable number of variables because the survey population of female heads of households who received AFDC is so limited.

In the OCSE study, the principal finding was that the size of state disregards appears to have had positive and significant effects in paternity determination and on the rate of collections in AFDC/TANF caseloads, increasing the likelihood that new orders for support will be issued and support collected. Not found, however, was a positive and significant association between the size of the disregard and average collections for that portion of the AFDC/TANF caseload with any collections greater than zero.

From the inception of the federal IV-D program, federal law required that states disregard a portion of the monthly child support collected on behalf of a welfare recipient household.⁷ However, anecdotal evidence suggests that many states did not implement the early “one-half-of-the-first-\$40 collected” rule, or the \$50 disregard rule that replaced it. Because of the accounting and distribution complexities of implementing disregard policy in an essentially “manual world,” technological capability may have needed to progress beyond its status at the time of the original disregard requirements.

A 1984 amendment to the Social Security Act regarding state IV-A program operations also closed a loophole in disregards. The new amendments eliminated the practice of excluding a child from an AFDC filing unit in order to ignore that child’s father’s support contributions. All children of an AFDC recipient were required to be included in the AFDC filing unit, irrespective of how many fathers were associated with the unit. The new law required that all support contributed by all fathers of children in the filing unit be pooled for the purpose of adding to the fixed \$50 disregard limit (plus any variable disregard limit in effect in a few states). The 1984 filing unit rules made it far less likely that states could avoid implementation of federal disregard law.

A full discussion of the relative advantages and disadvantages of choosing data from either the March or April CPS for this study may be found in the Appendix to this chapter. In summary, the intention in choosing the March and not the April CPS and developing an exclusion strategy for sample respondents was to maximize the sample size while ensuring that respondents were those most likely to have been subject to disregard policies in the prior year. Each of the exclusion criteria undoubtedly cost some number of respondents who would have been good candidates, had there been a greater level of detail about their living arrangements and the welfare/child support enforcement benefits structure and eligibility to which they were subject. Neither March nor April CPS data allow for fully addressing the concerns, though the balance favored the choice of the March CPS data.

IV. The Estimation Model and Variables Included

Overview of the Model

The central prediction is that the size and direction of the beta value associated with the principal independent variable, *total disregard by state/year*, will demonstrate the effectiveness of the disregard in eliciting parents’ cooperation with IV-D program objectives, thereby increasing the likelihood of receiving a child support payment.

Control variables of three types were selected for inclusion in the models—two demographic categories, representing the state economic environment and the demographic characteristics of the survey respondent, and explanatory variables that represent the policy environment of state child support enforcement systems. Similar variables representing the policy environment were the focus of the four studies discussed earlier; the OCSE study included other key features of the IV-A and IV-D systems,

⁷The \$50 disregard was incorporated in federal law (42 USCA s. 657), to be implemented by the states in October 1976.

including the disregard that is the subject of the present research. These policy variables may be said to “mediate” between the need for child support and the willingness to provide support. Although alternative theories suggest that some of these variables may belong in more than one category, these categories were chosen because they best reflect previous overall findings (see Table 2), with one possible exception, noted below.

Independent Variables

Independent Variables Representing State Policies and Laws. The variable of keenest interest—the total disregard available to AFDC/TANF recipients under federal/state policy—is measured here as the sum of the federally required fixed disregard (\$50 after 1975) and the variable disregard that occurs in some states that pay welfare benefits that are less than that state’s official Needs Standard.

After 1996, many states abandoned the practice of disregarding \$50 of the child support collected every month and a few states abandoned their historic variable disregard practices. Some states increased their disregards and one state reduced the size of its disregard. These changes, along with differences in the size of variable disregards, were the source of considerable variance in disregards from 1985 to 1998. Thus, these data were employed as a continuous measure in the model, adjusted to 1999 dollars. Because child support disregards offer a real economic incentive for recipients to cooperate fully in paternity determination and enforcement efforts, it is anticipated that the effects of this variable will be found to be positive and at least marginally significant. However, because of deficiencies in the CPS data that relate to the particulars of welfare receipt and IV-D client status, it is possible that any relationship will be difficult to measure.

The second administrative variable of interest relates directly to the capacity of the IV-D system to affect child support collections. The previous study, relying largely on OCSE administrative data, suggested a strong relationship between the average amount of child support collected (among those with collections) and full compliance with all criteria necessary for *federal certification* of the state electronic system for the distribution of child support collections. It is quite probable that fully certified systems represent better state and interstate IV-D agency coordination, optimal communication with IV-A agencies, better and more responsive distribution of child support, and better response to client questions about such matters as the status of their child support receipts. This variable is expected to be positive and significant in the present model, to the extent that the study population focus strategy has been successful. In the OCSE study, full certification status for the state’s electronic IV-D management system was found to be positive and significant in the mean amount of child support collected, for the portion of the AFDC/TANF IV-D caseload for which there were *any* collections, (though not for the non-AFDC/TANF or combined segments of the caseloads.) This suggests that optimal system design and management are especially important to AFDC/TANF-related child support outcomes, in ways that increase the level of collections when some collections are already forthcoming.

The third of the six administrative variables chosen for the model is *implementation of immediate wage withholding requirements* by state/year, lagged by (i.e., given a value of “1”) one year following adoption in order to allow for a greater likelihood that the effectiveness of this new enforcement tool will show up in the results. Although the OCSE study failed to find any evidence of positive effects attributable to immediate wage withholding, the effects of this independent variable using CPS data will be explored in an attempt to add support to the findings of other researchers regarding wage withholding effects.

Maximum AFDC/TANF benefit level by state/year is very important, for three principal reasons. First, the size of a state’s maximum IV-A benefit contributes to the relative value that clients place on the size of the disregard and income alternatives, such as their own earnings from labor force participation, given the general economic climate and demand for their labor in the state. (As such, this variable might be

TABLE 2
Theoretical Classification of Independent Variables Used in Model

IV-A/IV-D System Administrative and Legal Variables (Policy)	Variables Representing Female Householder Economic Need	Household and Householder Demographic Variables
Total amount of state disregard	State female unemployment rate	Age of female householder (age and age-squared)
Full certification status of state electronic system	Unemployed	Number of children age 6 to 18
Immediate withholding (lagged)	Not in labor force	Number of children less than 6 years of age
Maximum state AFDC/TANF benefit level for family of three (one adult)	Median household income in state	Hispanic
New-hire reporting requirement implemented by state		Black
IV-D expenditures (adjusted)		Race other than Hispanic or black (including American Indian, Asian American)
		Elementary school highest attended
		High school attended, no diploma
		High school graduate
		Divorced
		Separated

viewed as reflecting the mother's demand for child support, placing it in the next category of variables.) Second, the maximum AFDC/TANF benefit available to a family of three in a state may be viewed as a measure of state fiscal capacity coupled with its political will to ensure a "livable" level of support for poor children. Third, higher benefits, other factors being equal, attract women with more and better alternatives to welfare, those that may be more likely to use the opportunity to secure child support that can, in turn, foster earlier escape from welfare. The classification of maximum welfare benefits for three persons as a policy variable is primarily because it is a structural component of the relationship between the disregard, child support collection, and welfare maximum, all of which are set by state policy. As such, this variable may be viewed as an essential control variable for inclusion in any multistate test of disregard effects.

New-hire reporting and immediate wage withholding are viewed as companion measures, because of the difficulty in separating their theoretical underpinnings. Even though the courts or administrative processes are the originators of the orders for immediate wage withholding, the effectiveness of the implementation of this policy tool rests upon the cooperation and capacity of employers and employees—as, indeed, does the effectiveness of new-hire reporting. Further, it is posited that this variable also captures the willful and nonwillful avoidance of the state's new-hire and wage withholding requirements by diverting parents' workforce participation to cash-wage types of employment and work within the "irregular" economy. But in the absence of direct measures of obligors' labor market activities, it is not possible to confirm or refute this speculation within the context of this model.

The last of the independent variables that reflect the policy environment is *the level of federal/state expenditures for the state IV-D program operations*. This is measured as expenditures that are included in the formula for federal matching. These annual figures, along with all other dollar measures used in the model, have been adjusted to 1999 dollar amounts in order to make them comparable in current dollar terms.

Independent Variables Representing the Economic Environment and Female Householder Economic Need. At least two of the variables that were chosen to represent the level of female householder need for child support may also be categorized as state demographic variables, reflecting the relative health of a state's economy. These two variables, *female unemployment rate* and *state median income*, were also used in the OCSE study.

Theory suggests that the second variable, *state median household income*, may reflect multiple influences on the child support outcome measure. It is anticipated that state median income could be significant, positive, or negative, insofar as it reflects the supply or demand side of the child support system: i.e., the ability of fathers to generate sufficient income to contribute to the support of their children or the need for mothers to pursue support as an alternative or supplement to welfare and own earnings. Unlike the female unemployment variable, median income is not disaggregated by sex. It most likely captures merely the relative health of the state economy and little else.

Other selected measures of demand for child support are two that were not included in the OCSE study, owing to the absence of individual-level data in the administrative database. Those chosen for this study include *unemployment status* of the female householder, in order to capture labor force effects; in the labor force but unemployed (looking for work), employed or out of the labor force (resulting in two dummy variables and one omitted category). This variable captures something of the motivation for self-support and the desire to improve one's economic well-being, with or without welfare assistance or child support. It is important to remember that the survey participant responds early in the year to questions about welfare participation and child support receipts in the *prior* year, but the timing and duration of each event is unknown. Thus, no conclusions can be drawn about the order of the events or about causation.

The results connected with these measures reflecting the demand or need for child support—or the failure to receive any at the prior time—should offer clues to the conflicting results of the earlier studies

using administrative data and other CPS databases and samples, both those of this author and those of the others noted in this study

Household and Householder Demographic Variables. The choice of these variables capitalizes on a range of individual and household demographic variables in the CPS database, beginning with the *age of the female householder*. Although others have found that age increases the likelihood of reported child support receipts (Garfinkel and Robins, 1994), it may be that this results from using subfamily household data and an older age limit for the female head. The “fuzziness” at the borders between multiple families in households may have led to an overestimation of the positive effects of older age of the householder.

To address this concern, the upper limit of the study population was set at 45 years of age and those who reported no welfare benefits in the year prior to the study were eliminated. Additionally, all subfamilies were eliminated from the sample in order to improve model specification. This process may help identify the net effect of age. Second, both age and age-squared will be regressed simultaneously to assess the linearity of the relationship versus its curvilinearity.⁸

The *age and number of children* are essential control variables to aid in the interpretation of the meaning of the age of female householder. Their inclusion in the model may offer further evidence as to the independent effect of the total number of children in given age ranges upon child support receipt. Other researchers have noted the importance of both number and age of children in predicting particular child support outcomes (Beller and Graham, 1993; Sorensen and Halpern, 1999). The two categories that were available are included in the model: number of children age 6 to 18 born to or adopted by the householder, and number of children younger than 6.

Though not ideal, these two interval measures allow for the estimation of the effects of the number of children in each of the two age ranges. For instance, a positive and significant beta value for the variable reflecting number of older children may suggest better child support outcomes associated with a longer history of welfare dependence, thus increasing the length of “exposure” to paternity determination and child support enforcement requirements. A positive and significant finding for the variable reflecting number of very young children in the household may indicate that efforts to seek paternity determination and an order for support for young children are especially fruitful, controlling for all other factors in the model. A significant and positive beta value for either variable may also reflect the possibility that the greater the number of children in either age range, the greater the likelihood that multiple fathers have been associated with these households. More fathers per household may increase the likelihood that *any* child support will be forthcoming.

The next series of independent variables is designed to capture the effects of a mother’s membership in a racial or ethnic minority group that has a lengthy history in this country of social and economic discrimination and segregation. These binomial variables reflect status in three categories: *Hispanic (ethnicity)*, *black*, and *any minority “race” or minority group other than Hispanic or black*. The omitted category is white. Positive and significant values for any of the three measures would indicate that membership in minority groups would be more likely to be associated with child support receipt at some time during the prior year. Negative and significant values for any of the three would, of course, suggest that being white would favor the probability of child support receipt; this would be consistent with

⁸If the age variable beta value is significant and positive and the age-squared coefficient is significant and negative, a curvilinear relationship is suggested: that is, the likelihood of child support receipt would increase, but at a declining rate. If the age-squared specification is insignificant but the linear age specification is significant and positive, these results will agree with others’ findings that age appears to enhance the likelihood of child support receipts.

previous findings (Cassettey, 1978). There is no readily apparent reason to believe that any of the minority status variables will be found to be associated with higher reports of child support receipts, given consistent findings of this sort by other researchers over a long period of time.

The next three binomial variables in the model are chosen to measure different levels of educational attainment: *elementary school; some high school, no diploma received; or high school graduate*. The omitted category is any postsecondary education. Positive values for any of the three would suggest that that level of educational attainment is associated with a higher frequency of reports of child support received in the prior year, relative to the omitted category. Given the consistent findings from other published research spanning many years, it is not likely that any of these three categories will be positive and significant, with the possible exception of twelfth-grade attainment.

The last two variables included in the model, representing marital status, are *divorced* and *separated*. The omitted category, never-married, includes women who presumably required paternity determination services as a threshold to child support orders and receipt. It is expected that, consistent with other findings (Sorensen and Halpern, 1999), the fact of being or having been married will have a positive and significant association with receipt of child support. This would suggest, then, that the omitted category, never having been married, is negatively related to child support received in the prior year, possibly because of the greater challenges entailed in first determining paternity, the prerequisite to obtaining an order for support and collecting on that order.

Also, as in the OCSE study, a series of fixed-effects measures were employed to isolate the independent influence that any given year or state may exert on the dependent variable, child support receipt in prior year. Although other researchers have controlled for various years, states, and/or regions in their models estimating a range of child support outcomes, none who have used these CPS data have simultaneously controlled for both state and year effects. This strategy allows for capturing the fixed influences of states and years that cannot be readily captured by more direct means. These may include such factors as population characteristics and state legislative “will” to enforce the parental child support obligation or to pursue this strategy for reducing the public cost of welfare.

Dependent Variable

The model that has been designed and utilized in this study involves only one dependent variable: receipt/nonreceipt of child support in the year preceding the CPS interview. Because of insufficient data pertaining to such factors as the exact number of persons in the AFDC filing unit and the period of time during the prior year over which the family received IV-A assistance, an interval-dependent variable would offer no meaningful results. The model, then, seeks to estimate the effects of a wide range of independent variables on a *binomial measure* of reports of child support receipt in the prior year. The model is estimated using logistic regression.

V. Principal Findings

Findings with regard to the independent variable of greatest interest in this study—size of the total disregard available through state welfare program policy—tend to support those found earlier using OCSE administrative data and those of Sorensen and Halpern (see Table 3). The estimates from the current study suggest a positive and significant ($p < .10$) relationship between receipt of child support and the level of the disregard.⁹ This is somewhat surprising given the substantial limitations in the March CPS data. The

⁹The coefficient suggests that a \$50 increase in the disregard would be associated with an estimated 7 percent increase in the odds of receiving support.

TABLE 3
Likelihood of Reporting Any Child Support Receipt, 1984–1999

Independent Variable	Beta	p-value
Policy Variables		
Variable + fixed disregard	0.00138	0.0841*
Full certification status	0.033	0.8513
Immediate withholding	0.0438	0.6596
Maximum AFDC benefit	0.000291	0.7165
New-hire legislation	0.0493	0.5796
IV-D program expenditures	-0.0009	0.2188
Economic Environment		
Female unemployment rate	0.0508	.0226**
Median income	-0.0084	0.4858
Unemployed	-0.2406	0.0008***
Not in labor force	-0.3054	<.0001***
Demographic Characteristics		
Age of recipient	0.0757	.0194**
Age squared of recipient	-0.00139	.0058***
Divorced	0.7899	<.0001***
Separated	0.3312	<.0001***
No. of children 6–18 years old	0.0688	.0020***
No. of children < 6 years old	0.0763	.0055***
Hispanic origin	-0.4208	<.0001***
Black	-0.7129	<.0001***
Other minority race/ethnicity	-0.4548	<.0001***
Elementary education	-0.673	<.0001***
H.S. education, no diploma	-0.3665	<.0001***
H.S. graduate	-0.1981	.0003***

Bold print indicates statistical significance:

* = p. < .10

** = p. < .05

*** = p. < .01

N = 17,829

Log Likelihood = -7584.471

greater attention given to limiting the subsample to those most likely to have been clients of state IV-D programs increases our level of confidence in these findings. However, the unavoidable inclusion of some women who were only “marginally” poor or had not spent very much time as AFDC/TANF recipients may have limited the level of significance found for the key measure.

Surprisingly, however, no discernible relationship between the likelihood of receiving child support and any of the other variables reflecting state IV-A and IV-D policy and program characteristics was found. It is also somewhat surprising that none of the enforcement tools that the OCSE study found to have significant and positive results for some measures of child support outcomes had any discernible effect on child support receipts in this CPS sample. This may suggest a need for further investigation, especially one that better captures the period of time that welfare recipients are “attached” to the IV-D system as a consequence of their welfare receipt.

The measure of state maximum AFDC/TANF benefit level for a family of three, included to control for the effects of the structure of state benefit schedules, was not statistically significant. In other words, the maximum size of a welfare benefit to which a person is potentially entitled has no discernible impact when the size of the disregard is included. The size of the potential disregard offered by a state, then, stands out as the only policy variable in the model that has an independent, positive, and significant impact upon mothers’ reports of child support receipts.

The findings for the three measures that reflect female householders’ need for child support present a mixed picture, given the hypotheses. Respondents who reported being unemployed or not in the labor force were significantly less likely to have received any child support in the prior year, compared to those who were employed. Being in the labor force seems, therefore, to have a highly significant, positive association with child support receipt at some point during the prior year. This *may* indicate that the impetus for self-support, through both labor market engagement and pursuit of child support, may spring from the same unmeasured characteristics of the mother. Whether there is a causal connection in either direction cannot be ascertained from the present methodology and data.

Results for the state female unemployment rate, on the other hand, were positive and significant, suggesting that more limited employment opportunities for women may increase their need and desire for child support, at least for a population of those who have some immediate history of welfare receipt.

The last set of variables, around which the rationale for using CPS data centers, includes most of the demographic variables that other researchers have used in CPS-based studies of policy and demographic influences on child support outcomes. Without exception, these variables produce beta values that fall well within the highly significant range. All variables relating to race and educational attainment are significant and bear negative signs. The results suggest that in every case, those with minority status have significantly less likelihood of receiving child support than their white counterparts. Similarly, the negative signs for all categories of educational attainment suggest that when compared to the omitted category, “more than a high school diploma,” each of these lesser levels of educational attainment is associated with significantly less likelihood of child support receipt.

The result for the age variable tends to support the findings of other researchers that mother’s age, independent of all other factors in the model, substantially affects the likelihood that child support will have been received at some time during the previous year. Both age specifications were found to be significant: the age variable parameter positive, and the age-squared specification coefficient negative. This finding supports the hypothesis that as a female head of a family with children ages, the likelihood of collecting child support improves at a declining rate, at least up to the cutoff point (age 45) for this study. Because these results, as others resulting from this study, are restricted to a subpopulation of CPS respondents who received state welfare benefits at least once in the prior year, it cannot be said whether

increasing age is associated with declining frequency of child support receipt for a broader population of female householders.

Variables reflecting the independent effects of numbers of children in each of the two age categories are found to be highly significant and positive. These results are not surprising, but also are not particularly easy to interpret, given the distortions introduced by such a broad categorical range for number of older children. It is possible that the positive, significant results for both of these variables to some extent reflect the increased likelihood that multiple fathers have been associated with householders' children as the number of children in each category rises.

Like the findings of other researchers, marital status is found to be positive and significant. For instance, Sorensen and Halpern (1999) found that CPS respondents were far more likely to report positive child support outcomes if they had been married. (Note that widows were excluded from the sample because information was lacking about the living or deceased status of each child's father.) The positive direction for each of the two measures of marital status, divorced and married but separated, suggests that having been married at least once, rather than never, is associated with a greater likelihood of child support receipt. This is probably in part due to the challenge of securing determination of paternity as a necessary condition to receiving an order for support. If paternity is already presumed by marriage, the legal timetable for securing an order for support may be considerably reduced.

VI. Conclusions, Policy Implications, and Recommendations

The more important conclusion here relates directly to the topic of greatest interest: the relative power of a state IV-A program disregard, as applied to child support collections, to affect the likelihood that child support receipts will be forthcoming. The results for the key variable, disregard amount, are positive and significant, especially in light of the influence of the variables that capture demographic features of the population of CPS respondents and the controls for fixed effects. Current results appear to support those of the OCSE study regarding the apparent influence of the amount of the total state disregard on child support outcomes, suggesting that increasing the size of a state disregard on child support collections for welfare recipients has a strong likelihood of increasing the receipt of child support payments.

In order to test for the sensitivity of our findings to alternative specifications of the models employed here, models were tested without state effects, without year effects, and with neither state nor year effects. The finding of a positive and statistically significant relationship between the size of the total disregard and whether child support was received was robust to the alternative specifications: the coefficient remained roughly the same. The model was also estimated on a sample of women who did *not* receive AFDC/TANF—for whom disregard policy should not have an effect on child support receipt. As expected, there was no significant relationship between state disregard practices and child support receipt for this sample. The results of the sensitivity tests, discussed in greater detail in the Appendix that follows the main body of this chapter, add confidence to our conclusions about the influence of state disregard policy.

Because of the limitations connected with the present methodology and data, a range of numbers of AFDC/TANF families, individuals, or actual child support cases that might be affected by increases in state disregards cannot be calculated. It is strongly recommended that future research into the scope of disregard effects use data that can provide demographic variables for fathers and those associated both with varying lengths of time spent on welfare and with state child support enforcement programs. Further, it is also strongly suggested that both pass-through and disregard effects be explored, as they theoretically influence welfare recipients' child support behaviors in different ways. With controls representing many of the other policy variables and the state economic environments, it may be possible to more precisely define

the relative importance of each of these types of variables for estimating child support outcomes. Future research should also include more detail related to welfare participation and its timing and other sources of income for a national sample of female-headed families.

A related recommendation for future research is that estimates of state pass-through amounts as an additional administrative measure be included in any further studies of the effectiveness of disregards, both as an independent variable and in interaction with state total disregard amounts. In the absence of empirical research which distinguishes between the independent effects of pass-throughs and disregards, the possibility that results indicating the positive influence of disregards on child support outcomes are “contaminated” by state practices related to pass-throughs must be borne in mind. It is possible, further, that accurate historical data related to state pass-through policy will demonstrate that there is sufficient overall variance in state pass-through practices, considered separately from disregard practices, to warrant attempts to sort out the independent effects of each.

Moreover, theory certainly supports isolating the possible independent effects of pass-throughs vs. disregards. To the extent, for instance, that the regularity and size of the child support payments convince AFDC/TANF recipients of their adequacy, they may pursue other means of self-support, leading to training in preparation for entry into the labor force or more effective job-seeking behavior. Disregards, unlike pass-throughs, add money to a welfare recipient’s budget and enhance the economic well-being of the family, independent of any pass-through. It is also suggested that interaction effects may be observed between the size of a state’s disregard and its pass-through, reflecting an independent, enhanced effect on child support outcomes when disregard practices are combined with pass-through practices, but the present model does not control for these effects.

In short, the current research appears to support many earlier findings pertaining to factors that influence child support outcomes, including child support disregards, such as the previous chapter and Meyer and Cancian (2001), and the larger report of which this study is a part. The present study results argue for additional work to refine estimates for disregard and pass-through effects.

An additional policy question remains unanswered and largely unaddressed: Why, despite state IV-D program efforts, do women who report minority status persistently report lower child support receipts than their white counterparts? Is it largely because (presumably) minority ex-partners are less able to generate earnings in excess of what they require for their own self-support? Or does much of the answer lie in the nature of institutional barriers that discourage minority women from learning about and responding to the economic incentives of disregard effects that are suggested by the findings reported here?

APPENDIX Technical Note

Royce Hutson

Sample Exclusions

The data from the early years of the Special Child Support Supplement to the April CPS were not used because they preceded the period in which there can be reasonable certainty of full implementation of federally mandated disregards. Also, choosing data from the Supplement would have severely limited the number of observations for the study, reducing the sample size to those included in the biennial or triennial surveys from 1986 forward. Prior to 1991, the April CPS excluded women who had a child before they were married and subsequently had a child after marriage to someone other than the first child's father. It is likely that this practice reduced the number of survey households potentially eligible for the sample, reducing the number and types of female heads in that sample.

Given that state disregard policies and practices fall within the administrative purview of state IV-A programs, there were challenges to selecting a sample from the March CPS of female heads of households who reported receipt of AFDC/TANF benefits at some time during the year preceding the survey. It was essential that the CPS sample be pared to those most likely to have been in a position to have directly benefited from state disregard policy, i.e., AFDC/TANF recipients.

As well, all *subfamilies* residing in households headed by others were eliminated from the sample, even if those subfamilies were headed by divorced, separated, or unmarried women with children of their own. The preliminary examination of the data for these women who headed subfamilies in households headed by others revealed a number of curious responses to interview questions, responses that led us to question whether or not these women could be said to have *themselves* been welfare recipients in the prior year or were simply included in the AFDC/TANF filing unit of the household head.¹⁰ In any case, many of these subfamilies would have been excluded on the basis of the age criterion for selection, i.e., excluded were all who fell outside the 19 to 45 age range. Preliminary analysis of the data indicated that most of these subfamilies would have been excluded on the basis of their youth, irrespective of their status as a subfamily of the householder.

The primary reason for restricting the age of sample members was the inability to determine that a significant portion of this relatively small population had a profile that was clearly consistent with that of the target population. Those that were less than 18 years of age were less likely to be receiving IV-A benefits in their own names and, therefore, were of questionable eligibility for mandatory child support enforcement services. Very few of these young women were identified as family heads or receiving child support, even if the data suggested they had children of their own. This led to doubt that they could be assumed to have AFDC/TANF reciprocity status in their own right. In short, the limited number of these subfamilies headed by women older than 18, along with the absence of other information about them that

¹⁰For example, these female subfamily heads would occasionally respond in the affirmative when asked if they received AFDC/TANF benefits at any point during the prior year, yet when asked if they had any of their own children residing with them would answer "no." We found no similar discrepancies in the responses of the female householders in whose homes the subfamilies lived. Similarly, inconsistencies in the information pertaining to the subfamilies in these households led to no clear profile for these families. We had many possible explanations for the discrepancies and inconsistencies in subfamily responses, but in the absence of any data that clearly pointed to one that justified the inclusion of these householders, we decided against including them. The presence of these subfamilies in the households headed by women who otherwise met our selection criteria did not preclude our selection of the latter into our sample, however.

presented a clearer and less unambiguous picture of the likelihood of their attachment to the welfare and child support systems, made omitting them from the sample the better choice. The sample was further limited to those women under the age of 46 because of concerns about data accuracy.

Marital status was another criterion for restricting the sample. The sample was limited to those female heads of households who were either separated, divorced, or had never been married. The rationale for this choice was that: (1) if the person were married, it would not be possible to determine that her husband was or was not the father of her child(ren); and (2) if the female were remarried, it would not be possible to ascertain whether the stepfather had adopted the child(ren) or possibly that the woman married the father of the child(ren). In either case, the person would not be eligible for AFDC-related mandatory IV-D services. Widowed female householders were also excluded, along with married householders with an absent spouse. These latter were most likely to have been wives of military men, men working abroad or offshore, and men in jail or prison. In any case, although they may have had children for whom they were potentially eligible for child support enforcement services or welfare, and many reported having received welfare at some point in the year prior to their interviews, they were unlikely to have been given high priority for services by the IV-D agency. Also, these women may have reported in their CPS interviews that they received “child support” when they were referring to a military dependents’ allotment or funds sent by their husbands in prison or working abroad. In any case, those who reported nonreceipt of child support would not necessarily have been potentially eligible for support, giving us another source of concern.

Appendix Table A describes the impact of these exclusions on the sample figures.

A Note on Use of the March CPS

To maximize the numbers of years of data for analyses, the losses connected with not using data from the Special Census on Child Support, the April Supplement, were weighed against the potential gains from using the March CPS. The Special Child Support Supplement data were not available on an annual basis, and it was determined to be of greater importance to favor the larger number of observation points, especially for the most critical independent variable for the purposes of the model, total disregard. In sum, the primary reason for choosing the annual March survey data, instead of those from the April survey, was the availability of a greater number of years of data over the span of years offering a much larger total number of survey respondents and data points for analysis. The main “cost” of this decision, however, was that the richer detail about child support orders and payments that is found in the Special Supplement had to be forgone. What level of detail may have been gained (a moderate amount, given the central purpose of the research) by using the Special Child Support Supplement data was far more than offset by the gains in the form of increased degrees of freedom for the analyses by using the March data. This adds, as well, to increased confidence in the results.

Further, the April supplement was last conducted in 1992. As implementation of several key policy variables had not been accomplished by a sizable number of states by then, it would have been of greater necessity to exclude one or two policy or demographic controls that previous research had shown to be significant in explaining child support receipt, just to preserve sufficient degrees of freedom for the analysis. This added to the weight of arguments favoring use of the March CPS data.

Sensitivity Analysis

A sensitivity analysis was performed to test the sensitivity of the key result of the model, as specified. Results from this analysis are displayed in Appendix Table B. Results from the full model

APPENDIX TABLE A
Sample Exclusions and Resulting Numbers of Observations

EXCLUSIONS	TOTAL N
All female heads of households receiving AFDC/TANF	22,973
Number of children in family > 1	21,101
Never married, divorced, separated	18,977
Age 19–45	17,829 = Final N

APPENDIX TABLE B
Sensitivity Analysis for Likelihood of Collecting Support

Variables	Full Model		State Effects Only		Year Effects Only		Neither State nor Year Effects	
	Beta	p-value	Beta	p-value	Beta	p-value	Beta	p-value
Policy Variables								
Variable + fixed disregard	0.0014	0.0841*	0.0015	0.0522*	0.0010	0.0024***	0.0011	0.0003***
Full certification status	0.0330	0.8513	0.0730	0.6682	-0.0872	0.5785	-0.0157	0.9180
Immediate withholding	0.0438	0.6596	0.3703	<.0001***	-0.1489	0.0825*	0.3092	<.0001***
Maximum AFDC benefit	0.0003	0.7165	0.0013	0.0724*	-0.0002	0.1859	-0.0004	0.0014***
New-hire legislation	0.0493	0.5796	0.0722	0.3187	-0.0882	0.2360	-0.0138	0.8127
IV-D program expenditures	-0.0009	0.2188	0.0008	0.2574	-0.0012	<.0001***	-0.0007	0.0074***
Economic Environment								
Female unemployment rate	0.0508	0.0226**	-0.0289	0.1127	-0.0135	0.3893	-0.0559	<.0001***
Median income	-0.0084	0.4858	0.0159	<.0001***	0.0050	0.3188	0.0132	<.0001***
Unemployed	-0.2406	0.0008***	-0.2240	-0.2240	-0.2182	0.0022***	-0.2128	0.0017***
Not in labor force	-0.3054	<.0001***	-0.2851	<.0001***	-0.3165	<.0001***	-0.2995	<.0001***
Demographic Characteristics								
Age of recipient	0.0757	0.0194**	0.0767	0.0173**	0.0803	0.0121**	0.0819	0.0101**
Age squared of recipient	-0.0014	0.0058***	-0.0014	0.0054***	-0.0015	0.0031***	-0.0015	0.0028***
No. of children 6–18 years old	0.0688	0.0020***	0.0693	0.0017***	0.0665	0.0024***	0.0644	0.0032***
No. of children < 6 years old	0.0763	0.0055***	0.0804	0.0033***	0.0643	0.0173**	0.0690	0.0104**
Hispanic origin	-0.4208	<.0001***	-0.4146	<.0001***	-0.5254	<.0001***	-0.5345	<.0001***
Black	-0.7129	<.0001***	-0.7187	<.0001***	-0.6306	<.0001***	-0.6349	<.0001***
Other race/ethnicity	-0.4548	<.0001***	-0.4336	0.0002***	-0.4495	<.0001***	-0.4311	<.0001***
Elementary education	-0.6730	<.0001***	-0.7204	<.0001***	-0.6586	<.0001***	-0.7312	<.0001***
H.S education, no diploma	-0.3665	<.0001***	-0.3869	<.0001***	-0.3537	<.0001***	-0.3875	<.0001***
H.S graduate	-0.1981	0.0003***	-0.2076	0.0001***	-0.1788	0.0009***	-0.1998	0.0002***
Divorced	0.7899	<.0001***	0.7690	<.0001***	0.7523	<.0001***	0.7251	<.0001***
Separated	0.3312	<.0001***	0.3173	<.0001***	0.3420	<.0001***	0.3243	<.0001***
Log Likelihood	-7584.5		-7635.5		-7721.5		-7780.5	

Bold print indicates statistical significance:

* = p. < .10

** = p. < .05

*** = p. < .01

N = 17,829

reported in the body of this paper are included in this table to allow for easy comparison with the alternative specifications. As one can see, for the independent variable of interest—the fixed plus variable disregard—the finding remains robust throughout all specifications and the magnitude of the coefficient remains roughly the same. However, the findings for other policy variables are not so consistent. The immediate withholding variable not only is found to become significant when we exclude any one or all of the fixed effects, but the sign changes from positive to negative when we exclude state effects only. As for AFDC benefit levels, when all of the effects or the year effects are excluded, the maximum AFDC benefit variable is found to become significant and the coefficient becomes negative. As one can see in the original specification, though not significant, the maximum benefit level coefficient was positive. Lastly, a significant and negative coefficient is observed for the IV-D program expenditures variable when state effects and both state and year effects are excluded. This was not consistent with the original findings of a nonsignificant and positive relationship.

For the economic variables, inconsistencies are found from specification to specification for female unemployment and the median income level for the state/year. However, for all specifications, being unemployed or not in the labor force remains significant and negatively associated with child support receipt. For the demographic variables, all of the variables are consistent and significantly associated with the likelihood of child support receipt.

The second sensitivity analysis conducted compared the results for the original model using an AFDC/TANF population to the results for the same model using a sample of female heads of families that did not report AFDC/TANF benefit receipts. It was anticipated that disregard findings would not be significant for this sample population, as states' disregard policies are appropriate only for AFDC recipients. If a significant relationship were to be found between the disregard policy and child support receipt for this population, it might well be suspected that some other factor that influences child support outcome measures was being captured.

“Non-AFDC-recipient status” was defined as any female householder with children who responds negatively to survey questions related to her receipt of public assistance or AFDC/TANF in the previous year. As in the original model, this sample was limited to female householders who were between the ages of 19 and 45 and whose children were less than 18 years of age. Additionally, as with the research model utilizing an AFDC/TANF sample, the analysis was limited to those women who were either separated, divorced, or never married. The results of these analyses are presented in Appendix Table C.

As one can see, none of the policy variables except the AFDC benefit level are significantly associated with child support receipt. As predicted, there was no significant relationship between the disregard policy and child support receipt. Because a significant, positive relationship between the AFDC benefit level and child support receipt is observed for a non-AFDC population, it can be concluded that this variable may, in fact, be capturing another feature of state policy. For instance, the higher state welfare benefits are, the larger the population of those who are income-eligible for benefits; thus, the higher the welfare participation rate, the more women will have been “exposed” to IV-D services, raising child support receipt rates in the general population.

Female unemployment is not significantly associated with child support receipt for the non-AFDC population. This is different from findings for the AFDC/TANF sample population. It may be that the AFDC population, often being those first hired, first fired, may be more sensitive to changing labor demand. In other words, the AFDC population may seek and participate more readily in the process of establishing child support from the nonresident father when job prospects are bleak. This finding is consistent with the original “demand-side” theory presented in the body of the paper.

APPENDIX TABLE C
Comparison of AFDC and Non-AFDC Populations

Variables	AFDC		Non-AFDC	
	Beta	p-value	Beta	p-value
Policy Variables				
Variable + fixed disregard	0.0014	0.0841*	-0.0001	0.7463
Full certification status	0.0330	0.8513	-0.0353	0.6833
Immediate withholding	0.0438	0.6596	-0.0609	0.2703
Maximum AFDC benefit	0.0003	0.7165	0.0009	0.0457**
New-hire legislation	0.0493	0.5796	-0.0498	0.3526
IV-D program expenditures	-0.0009	0.2188	0.0000	0.9055
Economic Environment				
Female unemployment rate	0.0508	0.0226**	-0.0014	0.9084
Median income	-0.0084	0.4858	0.0056	0.3296
Unemployed	-0.2406	0.0008***	-0.3147	<.0001***
Not in labor force	-0.3054	<.0001***	-0.5484	<.0001***
Demographic Characteristics				
Age of recipient	0.0757	0.0194**	0.1415	<.0001***
Age squared of recipient	-0.0014	0.0058***	-0.0021	<.0001***
No. of children 6–18 years old	0.0688	0.0020***	0.1884	<.0001***
No. of children < 6 years old	0.0763	0.0055***	0.0962	<.0001***
Hispanic origin	-0.4208	<.0001***	-0.6706	<.0001***
Black	-0.7129	<.0001***	-1.0681	<.0001***
Other race/ethnicity	-0.4548	<.0001***	-0.5876	<.0001***
Elementary education	-0.6730	<.0001***	-1.0839	<.0001***
H.S education, no diploma	-0.3665	<.0001***	-0.5401	<.0001***
H.S graduate	-0.1981	0.0003***	-0.2564	<.0001***
Divorced	0.7899	<.0001***	1.2579	<.0001***
Separated	0.3312	<.0001***	0.5249	<.0001***
Log Likelihood	-7584.5		-22444.2	
N =	17,829		39,906	

Bold print indicates statistical significance:

* = p. <.10

** = p. <.05

*** = p. <.01

For all demographic variables included in the model, the findings for the non-AFDC population are consistent with findings for the AFDC population.

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Chapter 3

Exploring Potential Effects of a Child Support Pass-Through and Disregard: Did Formal Child Support Payments Change When Mothers Went on and off AFDC?

Daniel R. Meyer and Maria Cancian

Supporting single-parent families through welfare has long been unpopular.¹ Some policymakers have been particularly concerned that the public was providing economic support in the place of an “absent” father who was presumed to be shirking his duty. Thus, much of the early impetus for child support reforms grew out of desires to encourage fathers to provide for their economically vulnerable children. Increased child support might enable single mothers to stay off welfare, or at least might offset some costs for those families that did receive public support.

This history helps explain the long-standing policy of retaining any child support paid on behalf of resident-parent families who received assistance from the cash program for poor single parents, Aid to Families with Dependent Children (AFDC). Any child support paid was used to defray governments’ costs associated with AFDC, rather than to increase the resources available to families. But it is clear that under this policy regime, nonresident parents had little incentive to pay support formally, since their children did not benefit. Similarly, resident mothers had little incentive to cooperate with the child support system, at least in the short term. If the mother cooperated, and the child support system established paternity, established a child support order, and collected support, she would be no better off financially unless she were able to leave welfare.

The disincentives to formal payment by the nonresident parent and to cooperation by the resident parent were recognized. As early as 1976, U.S. policy was to pass through to the family the first \$50 per month collected in child support and then to disregard this amount in the calculation of AFDC benefits.² Any amount paid over the \$50 was to be split between the state and federal governments. However, the provision was not universally implemented, and clarifications were made in 1984 requiring the \$50 per month pass-through in each state. Discussions with Wisconsin child support personnel and analyses of the Wisconsin AFDC data lead us to believe that mid-1985 was the first date on which the \$50 pass-through/disregard was in effect in Wisconsin. National policy changed again in 1996, when states were given the option to set their own level of pass-through and disregard.

Since 1996, states have set very different levels of the pass-through. Wisconsin has taken an unusual position, passing through all support to the resident-parent family and disregarding it completely in the calculation of W-2 benefits. The effects of Wisconsin’s full pass-through are being evaluated through an experimental design, a design that compares the effects of a full pass-through (received by the experimental group) to a pass-through of up to \$50 per month or 41 percent of what is paid, whichever is greater (received by the control group).

This paper presents an alternative, nonexperimental analysis of the effects of a pass-through. The approach in this report stems from the recognition that the disincentive to pay support that resulted from

¹The authors thank Royce Hutson for data analysis assistance and colleagues at the Institute for Research on Poverty for discussions about these issues.

²This is according to 42 USCA s.657.

a partial (or no) pass-through affected nonresident parents *only* when their children were receiving AFDC. During periods in which their children were not receiving AFDC, the nonresident parents did not face this disincentive, since during these periods all support that was paid formally would go to their children. (However, families would not necessarily receive any support paid on back debts; these amounts could be retained completely or partially by the government.) In this report, we consider the impact of the differences in pass-through policy faced by nonresident fathers as the mothers of their children made transitions off and onto AFDC.

In the time periods we examine, there were two pass-through policy regimes in Wisconsin. We understand that from 1980 to 1984, all child support paid on behalf of AFDC recipients was retained by the state; nothing was “passed through” to the resident parent nor “disregarded” in the calculation of benefits. From 1985 to 1997, the first \$50 per month paid on behalf of AFDC recipients was passed through to the resident parent and disregarded, and the remainder was retained by the state. Thus to the extent that state retention of child support was a disincentive to pay, the effect should have been greater before 1985 than after.

The basic idea behind this report is simple: if pass-through policy affects formal payments, we should see changes in the payments that nonresident fathers make when resident mothers move on or off AFDC. Consider first a nonresident father who is paying regular child support to a resident mother who is not receiving AFDC in the 1980–1984 policy regime. All the support he pays goes to the family. If the resident mother then begins receiving AFDC, the payment will be retained by the state, and nothing will go to the family. The nonresident father may learn that his payments no longer benefit his children. If so, he may stop making formal payments, or make fewer or smaller payments. This is one of the effects we will explore in this report: are fathers who were paying support more likely to stop paying *just after* their children begin receiving AFDC? Even in the 1984–1997 regime, when this same father made a payment after his ex-partner entered AFDC, a portion of that payment was retained by the state, and only \$50 per month would go to the resident-parent family. Again, if he learned that payments over \$50 per month would not benefit his children he might choose to make fewer or smaller payments.

In this report we also examine transitions to beginning to pay. Assume it is generally known that child support payments do not fully benefit welfare families. If his children are receiving welfare, a nonresident father might not pay formal child support, but instead provide diapers, food, clothing, etc. Perhaps he does this because he knows a formal cash payment would be either fully (1980–1984) or mostly (1984–1997) retained by the state and his children would not benefit much. Assume the mother then gains a low-paying but steady job, and she leaves AFDC. The nonresident father may then begin to pay cash child support formally, because his children will then receive the full benefit of his cash payments. This is the other effect we will explore in this report: are fathers who were not paying child support formally more likely to begin paying support *just after* their children leave welfare?

Unfortunately, an opposing factor may complicate both analyses. Child support agencies may focus more attention on collections among welfare cases than nonwelfare cases, since collections among welfare cases provide governmental receipts. If collection efforts are stronger among AFDC recipients, then fathers of children currently receiving AFDC may be more likely to begin paying support (and less likely to stop paying support) than fathers of children not receiving AFDC. In the analyses we conduct here we cannot identify the extent of agency effort for different types of cases, and thus cannot measure the potential countervailing impact of changes in enforcement. We also have no direct evidence of the extent to which fathers were aware of their children’s AFDC participation status or understood the implications for child support disbursement. We return to these issues in our discussion of the results of these analyses.

In both analyses we will explore whether there are different effects in the pre-1984 and the post-1984 periods. The report will thus provide some evidence on whether a full-retention policy regime and a \$50 pass-through policy regime were associated with lower formal child support payments. When combined with the experimental analyses of Wisconsin's full pass-through/disregard and other nonexperimental analyses, this will help policymakers understand the effects of various pass-through and disregard policy regimes.

I. Previous Literature

This paper is focused on the effects of resident mothers' AFDC status on whether nonresident fathers make transitions in their formal child support payments, either moving from not paying to beginning to pay, or moving from paying something to paying nothing. While no previous research that we are aware of has examined the effects of pass-through policy on child support payment transitions, there is some prior work describing factors related to child support payments in general and somewhat less research related to payment *transitions*.

Previous research on child support payments (or compliance with child support orders) has sometimes begun with a model in which child support is related to:

- the nonresident parent's ability to pay support,
- the stringency of the enforcement regime,
- the strength of the ties between the nonresident parent and the children and/or resident parent, and
- the need of the resident parent.

Previous work has generally found that the nonresident parent's ability to pay support and a stringent child support enforcement regime are both clearly associated with paying more support. Some work has found that those who have more contact with their children are more likely to pay support, but it is unclear whether: (a) those with strong relationships are more likely to pay; or (b) those who pay support are more likely to stay in touch with their children, or (c) whether both the relationship and payment are caused by a third factor, perhaps the nonresident parent's sense of responsibility. Some work has examined whether the resident parent's need is related to payments, but there are few conclusive findings. This body of research has not yet focused much attention on whether a mother's AFDC status (and thus the pass-through/disregard policy regime) affects payments.

An example of a research study examining factors related to compliance with child support orders can be found in Meyer (1999).³ He examines divorce and paternity cases in Wisconsin over the 1980–1993 period using the Wisconsin Court Record Data (described in the next section). The focus is on compliance with a child support order, that is, the amount paid divided by the amount due, all measured in the first calendar year following the order. He finds a strong relationship between ability to pay and compliance: fathers with higher overall income and those more able to afford their order (those whose orders are a lower percentage of their income) exhibit higher compliance. An examination of prototypical cases reveals that when fathers' annual incomes increase from less than \$10,000 to

³Previous studies of compliance in Wisconsin using earlier data are Bartfeld and Meyer (1994) and Meyer and Bartfeld (1996).

\$30,000–\$50,000, compliance is predicted to increase from 56 percent to 83 percent among paternity cases and from 61 percent to 85 percent among divorce cases. The enforcement system also has a large effect: cases with immediate withholding have higher compliance, and those with percentage-expressed orders (which are harder to monitor and enforce) have lower compliance. Because there is a general trend toward increased compliance over time, once other factors are controlled, this may also reflect effects of increasingly stringent enforcement. Meyer finds that none of the available measures of ties between nonresident fathers and the resident-parent family—divorce vs. paternity, number of children, marriage length, age of oldest child, the (re)marriage of either the father or mother, and whether the parents have joint legal custody—have a consistent discernible effect on compliance. He also finds no discernible difference in compliance rates between those whose ex-partners did and did not receive AFDC.

Sorensen and Halpern (1999) have recently completed a national study of resident mothers that explores factors related to whether child support, AFDC, neither, or both are received. Their model examines unmarried resident mothers between 1976 and 1997, with controls for state and year. They have no information on the nonresident father, so their measures of ability to pay are limited. Nonetheless, they find that mothers with higher education and those who are white are more likely to receive child support, and these variables may be associated with fathers who have more ability to pay. Moreover, average male earnings in the state during the time period are also associated with increased likelihood of receiving support. They explore the effects of living in a state that had a particular child support policy during the period examined, and find that several features of the enforcement system are important. For example, never-married mothers in states with in-hospital paternity establishment are more likely to receive child support. Living in a state with a state income tax intercept increases the likelihood of receiving child support, as does living in a state with presumptive guidelines. Previously married mothers in states with immediate wage withholding are more likely to receive support. They have no measures of the relationship between nonresident fathers and children, although having young children and having more children are generally associated with a lower likelihood of support.

They also examine the effect of living in a state that had a \$50 pass-through/disregard in place during the period examined. They find that this increases the likelihood of receiving child support for those on AFDC, decreases it for never-married mothers off AFDC, and increases it for previously married mothers on AFDC. It is unclear why pass-through/disregard policy should affect those off AFDC; the inconsistency of this finding therefore suggests that further research is needed.⁴

A few studies have focused on child support payment transitions. Meyer and Bartfeld (1997) present basic descriptive data from Wisconsin Court Record cases entering the court system between 1986 and 1988 who had fixed-dollar orders. Of those who did not pay in the first quarter of having an order, about 70 percent were still nonpayers in the next quarter, and 60 percent still had not paid by the end of the next quarter. The proportion beginning to pay fell fairly steeply over time, so that nonpayers who did not pay early were less likely to begin to pay as time continued. Among those who paid in full during the first quarter, about one-third ceased full payment by the end of the second quarter. Again the dropoff was steepest in the early quarters; cases that had consistent records of full payment were likely to continue to do so. They also reported on a preliminary analysis of factors associated with beginning to pay support, and found that those with a withholding order were more likely to begin to pay, but they did not find consistent effects of earnings. Another preliminary analysis of those who paid in full in the first

⁴During this period, some states had one schedule for AFDC benefits and a higher schedule for “needs.” In these states, all child support payments would go to the family until the level of payments reached the difference between the needs standard and the benefit level. The research ignores this complication to simplify the analysis; however, ignoring it means that states with different levels of pass-throughs/disregards are treated identically, complicating conclusions about the effects of these policies.

quarter found that earnings changes were strongly linked to ceasing full payment—those who dropped out of the earnings record were most likely to cease full payment.

In summary, previous work suggests several factors related to child support payments. However, this work has generally paid little attention to whether a mother's AFDC status or transitions affect child support payments, the focus of this paper.

II. Data and Approach

Our analyses are based on the Wisconsin Court Record Database (WCRD). The WCRD includes information on over 16,000 child support cases gathered from courthouses in 21 Wisconsin counties. The data were gathered in twelve cohorts, covering the period 1980–1993, and each cohort was followed for 2–7 years. The data include an administrative record of monthly child support payments and orders and a variety of demographic variables. IRP has merged these data with the administrative record of monthly AFDC amounts from January 1980 through December 1993. We examine only the 4,428 paternity cases in these analyses to limit our sample to cases that are at high risk of receiving welfare. To reach our final base sample of 3,058, we exclude several types of cases. The largest exclusion is 657 cases that do not have two years of pre-order welfare information, a critical variable for this analysis.⁵

In all multivariate analyses we use a discrete-time event-history model. The model is appropriate when the outcome of interest is a transition (in this case a transition between payment statuses). We conduct two separate analyses: (a) whether a case transitions from nonpayment to payment, and (b) whether a case transitions from payment to nonpayment. In the first analysis we are particularly interested in whether this transition occurs shortly after an exit from AFDC; in the second analysis we are particularly interested in whether this transition occurs shortly after an entrance onto AFDC. These are the transitions we would expect if individuals understand the pass-through policy and if a partial or no pass-through is a serious disincentive to pay in the formal child support system.

We examine only the first transition in payment status following the first child support order. Thus in the first analysis we examine cases with no payment during the first two months following paternity establishment (or, the first two months following their first child support order, if there is no order at the time of paternity establishment). We consider the likelihood that a case will begin paying, using the following equation:

$$P(\text{transition to payment}) = \beta_0 + \beta_1 * \text{Former Recipient (1980–1984)} + \beta_2 * \text{Nonrecipient (1980–1984)} + \beta_3 * \text{Former Recipient (1985–1994)} + \beta_4 * \text{Nonrecipient (1985–1994)} + \beta_5 * \text{Years} + \beta_6 * X + u$$

In this model, the unit of analysis is a person-month at risk of making a transition; cases contribute observations until they make a transition, the child support order stops, or the data end. We divide the welfare variables by the two policy regimes: the first indicator variables reflect the 1980–1984 regime and indicate whether an individual is a current welfare recipient (the omitted group), a former recipient (including separate indicator variables for those receiving 1–3 months ago, 4–6 months ago, 7–9 months ago, and 12–24 months ago, and those who were not a recipient in the prior 24 months). The

⁵The other exclusions are as follows: first, we eliminate 22 cases that ever had a private pay agreement, since for these cases the court record of payments could be inaccurate. Second, we eliminate 366 cases in which someone other than the father owed child support. Finally, we eliminate 325 cases that do not have a child support order during the period examined or do not have a social security number.

coefficient β_1 will be greater than zero if payments were more likely to begin in that period once an individual left welfare (and thus the state is no longer keeping all or a portion of the amount paid). The next variables reflect reciprocity status in the post-1984 regime (\$50 pass-through). Therefore, additional evidence on the effect of the pass-through can be seen by whether the coefficient β_3 is less than β_1 ; if it is, this suggests that relaxing the \$50 pass-through had a smaller effect than relaxing the no pass-through, as would be predicted by economic incentives. We include variables representing the period being considered; these variables capture changes in child support policies and other factors that change over time.

We include several additional control variables. Because previous research has tended to show that cases that have not been paying for a long time are less likely to begin paying than other cases (see, e.g., Meyer and Bartfeld, 1997 or Meyer and Hernandez, 1999), we include indicator variables that reflect the length of the current spell. To represent the enforcement system, we note whether a case had immediate withholding (in addition to our indicator variables for periods) and whether the order was ever expressed as a percentage of income (rather than a fixed-dollar amount). To represent variables associated with the father's ability to pay support, we include his earnings (when available, discussed below), age, race, and the unemployment rate in the county during the month examined. We have limited measures of the relationship between the father and children: we are able only to control for the number of children, the age of the youngest, and whether the father or mother was married at the time of paternity establishment. We include an indicator variable for whether the father has joint legal custody, which may be related to his relationship with the children. Finally, to control for other environmental factors, we include indicator variables for county (Milwaukee versus other urban versus rural).

We would like to include a measure of the nonresident father's earnings in this analysis, but these data are only available for a portion of the period of interest. Records of earnings reported to the Unemployment Insurance system are available from January 1988 forward; these data have been merged with the base WCRD by IRP. Thus we also conduct a variant of this analysis on later cases so that we can control for the nonresident parent's earnings. This variant is parallel to the model described earlier except that we have information only for the post-1984 period (the \$50 disregard). We report the results from an equation that includes the level of quarterly earnings (in 1999 dollars), although we also tested an equation that considered *changes* in earnings from the previous quarter to the current quarter.

In the main analysis in this section (covering the entire period), we begin with the 3,058 cases that meet our sample criteria. Of these cases, 1,398 had no payments in the first two months of the order, and thus are appropriate for the analysis of beginning to pay support. As noted above, we examine each month for these cases until they either begin paying, have their order stopped, or we run out of data. The total sample is 16,295 person-months in which the person could begin to pay support. The sample for the later period (in which we include earnings) totals 11,501 person-months.

The second model examines nonresident fathers who pay support initially, and considers factors related to whether they stop paying, with a particular interest in whether they are more likely to stop paying child support once the resident-parent family has entered AFDC. The model is generally a straightforward extension of the initial nonpayer model: we use the same general structure, the same independent variables, and run two equations, one on the whole period, and a second in which we include earnings data and examine only the later period.⁶

⁶Because there may be measurement error in the monthly amounts of child support (a payment made on the last day of one month may be credited in that month or the next), any single month of nonpayment between two months of payment is treated as containing a payment.

In the main analysis of the second model (covering the entire period), we again begin with the 3,058 cases that meet our base sample criteria. Of these cases, 821 had payments in both of the first two months of the order, and thus are appropriate for the analysis of stopping support payments.⁷ We examine each month for these cases until they either stop paying, have their order stopped, or we run out of data. The total sample is 12,842 person-months in which the person could begin to stop paying support. The sample for the later period (in which we include earnings) totals 5,791 person-months.

III. Results

Table 1 provides basic descriptive information on our sample. The first column shows the composition of the sample of those who did not pay in the first two months, for whom we analyze whether they then began to pay support. About one-fourth of initial nonpayers are in Milwaukee, about one-fourth in rural areas, and the remainder in other urban areas. The fathers are fairly young: 12 percent are 18 or less, and another 43 percent are 19 to 24. About 70 percent of the fathers were associated with women who were receiving AFDC at the beginning of the period examined. The second column shows those who did pay in both of the first two months, for whom we analyze whether they later stopped paying support. Because this sample includes only those paying in the initial months, the group appears to have somewhat higher ability to pay: there are fewer fathers less than age 19, fewer fathers of color, and fewer fathers in Milwaukee. The largest contrast is in the mother's AFDC receipt: only 4 percent of these fathers were associated with women who received AFDC at the start of the spell of payment.

Information on the relationship between stopping payment, beginning payment, and time is provided in Figure 1. The bottom line in Figure 1 examines the initial nonpayers and shows the proportion who are still nonpayers after each month. For example, at the beginning of the seventh month, 63 percent still had not paid. Because the line is steeper in the early months, the figure shows that those who did not pay at the beginning of the observation period become even less likely to pay over time. The top line examines the proportion of initial payers who continue to pay. By the beginning of the seventh month, 79 percent have not yet had a month of nonpayment. This line also shows somewhat steeper declines in the early period, showing that those who continue to pay are less and less likely to stop paying over time.

We now turn to the results from the first multivariate analysis, examining whether AFDC transitions are related to beginning to pay support. The first columns of Table 2 show the full period. The coefficients on AFDC history, our main variable of interest, show no consistent pattern in the early period (panel 1). In the later period (panel 2), there is still no discernible difference between those whose partners have recently left AFDC and those whose partners are current recipients. However, those whose partners never received AFDC are more likely than partners of current AFDC recipients to begin paying, perhaps because the model does not adequately control for ability to pay.

The last columns show the later period only, and this allows us to include earnings in the model. Again we find that fathers whose partners have just gone off welfare are no more likely to pay than those whose partners are still recipients. Even when we control for the father's earnings, those whose partners have never received AFDC are more likely to begin to pay than are the partners of current recipients or former recipients. When we control for earnings and examine the later period only, those whose partners last received AFDC more than 6 months ago are less likely to pay than are those whose partners are

⁷An additional 839 cases had payments in only one of the first two months, and thus are in neither of our analyses.

TABLE 1
Characteristics of Nonresident Fathers Who Are Nonpayers and Payers
within the First Two Months following Paternity Establishment

	Initial Nonpayers	Initial Payers
County		
Milwaukee	27.4%	13.3%
Other urban	45.1	53.5
Rural	27.5	33.2
Race		
White	34.6	42.5
Black	21.5	8.2
Other	3.8	2.7
Missing	40.1	46.7
Number of Children		
One	88.5	93.8
Two or more	11.5	6.2
Age of Youngest Child		
0-1	59.9	65.4
2-5	24.2	21.9
6-18	4.9	4.0
Missing	10.9	8.7
Age of Father		
15-18	11.7	7.3
19-24	43.3	45.6
25-30	25.9	25.6
31-40	12.6	15.2
41+	4.2	4.5
Missing	2.3	1.8
Immediate Withholding		
Yes	25.5	47.5
No	61.3	45.7
Uncertain	13.2	6.8
Year		
1982-1984	21.3	25.1
1985-1987	22.8	35.9
1988-1990	23.8	21.8
1991-1993	32.1	17.2

(table continues)

TABLE 1, continued

	Initial Nonpayers	Initial Payers
County Unemployment Rate		
<=5%	36.1	30.1
5.1–10%	53.9	57.4
10.1%+	9.9	12.6
AFDC History		
Current recipient	70.7	3.7
Not current, but record of prior receipt	15.5	77.0
Not current, no record of prior receipt	13.8	19.4

Sample: 1,398 initial nonpayers, 821 initial payers in the Wisconsin Court Record Database.

Note: All variables measured at start of spell.

FIGURE 1
Proportion Remaining in Initial Spell over Subsequent Months

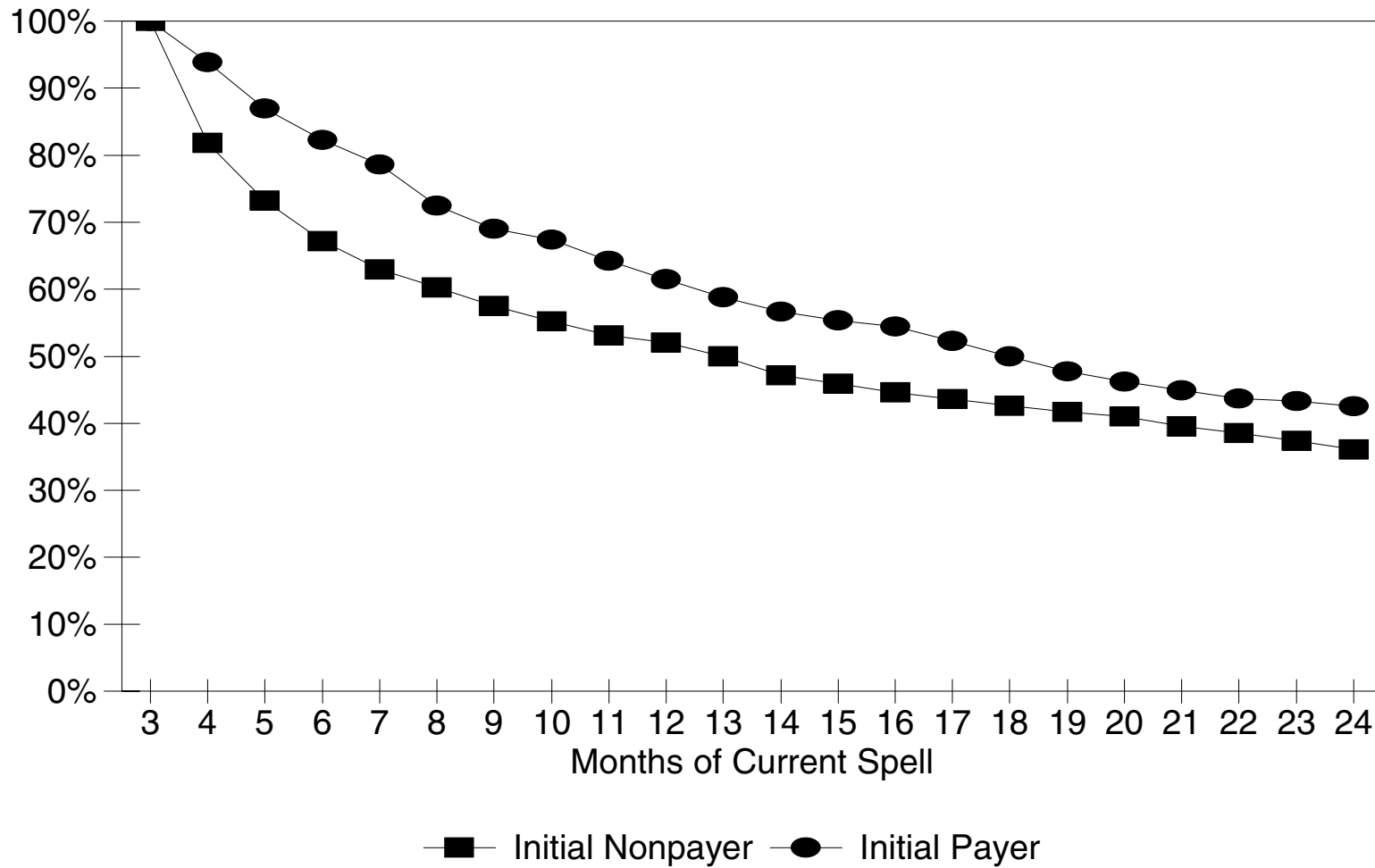


TABLE 2
Relationship of Mothers' AFDC Receipt and Fathers'
Beginning to Pay Child Support, among Initial Nonpayers

	Full Period ^a		Later Period ^b	
	Coeff.	Std. Err	Coeff.	Std. Err.
1980–1984 Cases, AFDC History (compared to current recipient)				
Last received 1–3 months ago	-.92	.52		
Last received 4–6 months ago	.51	.39		
Last received 7–12 months ago	-1.11	.73		
Last received 13+ months ago	.07	.74		
Never received	-.22	.29		
1985–1993 Cases, AFDC History (compared to current recipient)				
Last received 1–3 months ago	.21	.13	.19	.16
Last received 4–6 months ago	-.23	.24	-.43	.28
Last received 7–12 months ago	-.17	.22	-.64*	.28
Last received 13+ months ago	-.38	.23	-.71**	.26
Never received	.42**	.12	1.06**	.29
Months of Nonpayment (compared to 3–6)				
7–12	-1.11**	.10	-.88**	.13
13–24	-1.38**	.10	-.96**	.13
25–48	-1.99**	.18	-1.72**	.22
49+	-1.76**	.31	-1.44**	.32
Quarterly Earnings (in \$1000)			.22**	.02
County (compared to rural)				
Milwaukee	-.40*	.16	-1.11**	.23
Other urban	.10	.10	-.31*	.14
Immediate Withholding		.81**	.09	.79**
Never Had Percentage-Expressed Order		.17	.09	.23*
Unemployment Rate		-.01	.02	.03

(table continues)

TABLE 2, continued

	Full Period ^a		Later Period ^b	
	Coeff.	Std. Err	Coeff.	Std. Err.
Year (compared to 1993–94)				
1982	-.14	.28		
1983	-.33	.24		
1984	-.06	.30		
1985	-.27	.22		
1986	-.18	.19		
1987	-.07	.18		
1988	-.26	.19	-.23	.21
1989	-.43*	.20	-.44*	.21
1990	-.13	.18	-.14	.19
1991	-.16	.16	-.18	.17
1992	-.09	.15	-.10	.16
N		16,925		11,501
Log-likelihood		-2994		-1692

Note: Model also contains an intercept and indicator variables for father's age, race, marital status at paternity establishment, mother's marital status at paternity establishment, the number of children in a paternity action, the age of the youngest child, legal custody, and missing information on immediate withholding.

^aCovering the years 1980–1993.

^bCovering the years 1988–1993, when earnings data are available.

**p < .01.

*p < .05.

current recipients. This may reflect the effort that the child support agency is putting into collections, a lower need for support, or some other factors.

In both equations, the other coefficients are generally consistent with expectations. As we saw in the simple comparisons above, the more months a father remains a nonpayer, the less likely it is that he will begin to pay in the next month, even when we control for other variables. A father's earnings have a strong effect on beginning to pay, in the expected direction: fathers with higher earnings are more likely to pay. Fathers in Milwaukee are less likely to begin to pay than those in other urban or rural counties; in the later period those in other urban counties are less likely to begin to pay than those in rural counties. Fathers who had immediate withholding are more likely to begin paying, even though they had not paid in the first two months. In the later period, those who never had a percentage-expressed order are more likely to begin to pay. The unemployment rate has no discernible effect.

We now turn to our analysis of whether AFDC transitions are related to stopping payment of support among those who were paying in the first two months following order establishment. In these models we are particularly interested in whether those whose partners have just entered AFDC are more likely to stop paying formal support. The results are shown in Table 3. We find that having one's partner enter AFDC has no discernible effect on ceasing to pay. In the first model only (full period), those whose partners never received AFDC are less likely to cease paying than are partners of former recipients.

Few other variables are consistently related to stopping payment. The variables denoting the month of payment show that those who continue to pay for longer periods are less likely to stop paying than those in a short spell of payment, but this effect is not seen once we control for earnings. Earnings have a strong and expected effect; those with higher earnings are less likely to stop paying. The unemployment rate has a counterintuitive effect; those in counties with higher unemployment are less likely to stop paying.

We have explored several variations on both models (starting and stopping payment). For example, when we include prior earnings or earnings changes between this quarter and last, we find the expected effect that those who increase earnings are more likely to begin to pay. In none of the variants is there a consistent pattern between the AFDC transition variables and beginning to pay.⁸

IV. Summary and Policy Implications

In this paper we have examined whether nonresident fathers change their payment patterns in response to their ex-partner's AFDC participation status. If a father knows that his payments do not fully

⁸Four other variants were also tested. First, we included indicator variables for each month; fathers are most likely to begin to pay in January (though that coefficient is not significantly different from May, July, or August), but the coefficients on no months are significantly different from January in the equation for stopping payment. Second, we included indicator variables for each county. Again, fathers in some counties are more likely to start (or stop) payment than in other counties, but the basic conclusions remain the same. Third, we added an interaction term between region and the unemployment rate. In Milwaukee only, fathers are more likely to start paying as unemployment increases, a counterintuitive finding if we think only about fathers. An alternative way to think about this is that as unemployment increases, mothers are more likely to need support, and exert pressure on fathers to begin paying. Finally, we added an interaction term between Milwaukee and percentage-expressed orders. We find that those in Milwaukee without percentage-expressed orders are more likely to begin paying than those in the rest of the state, but there is no difference in the likelihood of stopping payment. Because these variants do not change the key result—that we cannot detect a change in payment behavior consistent with the incentives embedded in the pass-through and disregard—we show only the simple model in this paper.

TABLE 3
Relationship of Mothers' AFDC Receipt and Fathers'
Stopping Payment of Child Support, among Initial Payers

	Full Period ^a		Later Period ^b	
	Coeff.	Std. Err	Coeff.	Std. Err.
1980–1984 Cases, AFDC History (compared to former recipient)				
Began receiving this month or 1–3 months ago	-.84	.56		
Began receiving 4–6 months ago	.65	.38		
Began receiving 7–12 months ago	.13	.36		
Began receiving 13+ months ago	.20	.29		
Never received	.36	.32		
1985–1993 Cases, AFDC History (compared to former recipient)				
Began receiving this month or 1–3 months ago	-.07	.18	-.10	.27
Began receiving 4–6 months ago	-.07	.23	-.39	.39
Began receiving 7–12 months ago	.26	.18	.32	.28
Began receiving 13+ months ago	.03	.15	-.20	.28
Never received	-.35*	.17	-.25	.27
Months of Payment (compared to 3–6)				
7–12	-.13	.12	-.03	.24
13–24	-.59**	.13	-.05	.24
25–48	-.77**	.18	-.48	.31
49+	-1.19**	.39	-.80	.45
Quarterly Earnings (in \$1000)			-.24**	.03
County (compared to rural)				
Milwaukee	.05	.21	.15	.41
Other urban	.00	.12	.18	.26
Immediate Withholding	-.21	.11	.03	.21
Never Had Percentage-Expressed Order	.09	.14	.32	.23
Unemployment Rate	-.05	.03	-.17**	.07

(table continues)

TABLE 3, continued

	Full Period ^a		Later Period ^b	
	Coeff.	Std. Err.	Coeff.	Std. Err.
Year (compared to 1993–94)				
1982	.80	.43		
1983	.53	.39		
1984	-.08	.57		
1985	.25	.32		
1986	.51	.28		
1987	.24	.27		
1988	.35	.27	-.08	.33
1989	.15	.29	-.54	.37
1990	.11	.28	-.06	.40
1991	.04	.28	.09	.33
1992	.10	.27	-.05	.29
N		12,842		5,791
Log-likelihood		-1975		-655

Note: Model also contains an intercept and indicator variables for father's age, race, marital status at paternity establishment, mother's marital status at paternity establishment, the number of children in a paternity action, the age of the youngest child, legal custody, and missing information on immediate withholding.

^aCovering the years 1980–1993.

^bCovering the years 1988–1993, when earnings data are available.

**p < .01.

*p < .05.

benefit his children when they are receiving AFDC, he may be less likely to pay. All else equal, we would expect to see fathers begin to make formal payments soon after their children leave AFDC and to see fathers stop making formal payments soon after their children enter AFDC. This analysis finds no discernible evidence of these patterns.

There are several potential reasons why we do not find an effect. On the one hand it may be that fathers do not respond to the change in incentives because they do not understand the way the child support system works, or because they are unaware of changes in their children's AFDC status. Indeed, results from a survey of fathers of children receiving W-2 generally show that few fathers understand the pass-through and distribution policy (Meyer and Cancian, 2001). On the other hand, it may be that fathers respond to the change, but we are unable to detect the response because it is obscured by other coincident changes. In particular, if the child support system reduced enforcement activity for non-AFDC cases, this might counteract the positive impact of the increased incentive to pay. It may be that other data limitations have also confounded our analysis.

Because of this uncertainty, this analysis cannot provide definitive evidence to support or reject the hypothesis that pass-through and disregard policy affects child support payments. Implications for policy are also limited because, even in the absence of a disincentive effect of a partial (or no) pass-through/disregard, other criteria may justify a full pass-through. For example, full pass-throughs are one way to increase the income of economically vulnerable families, and they may have a number of other beneficial effects. The Wisconsin Child Support Demonstration Evaluation is examining several potential effects, evaluated with several different methods. That evaluation will provide policymakers with more information on the potential advantages and disadvantages of a full pass-through/disregard.

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