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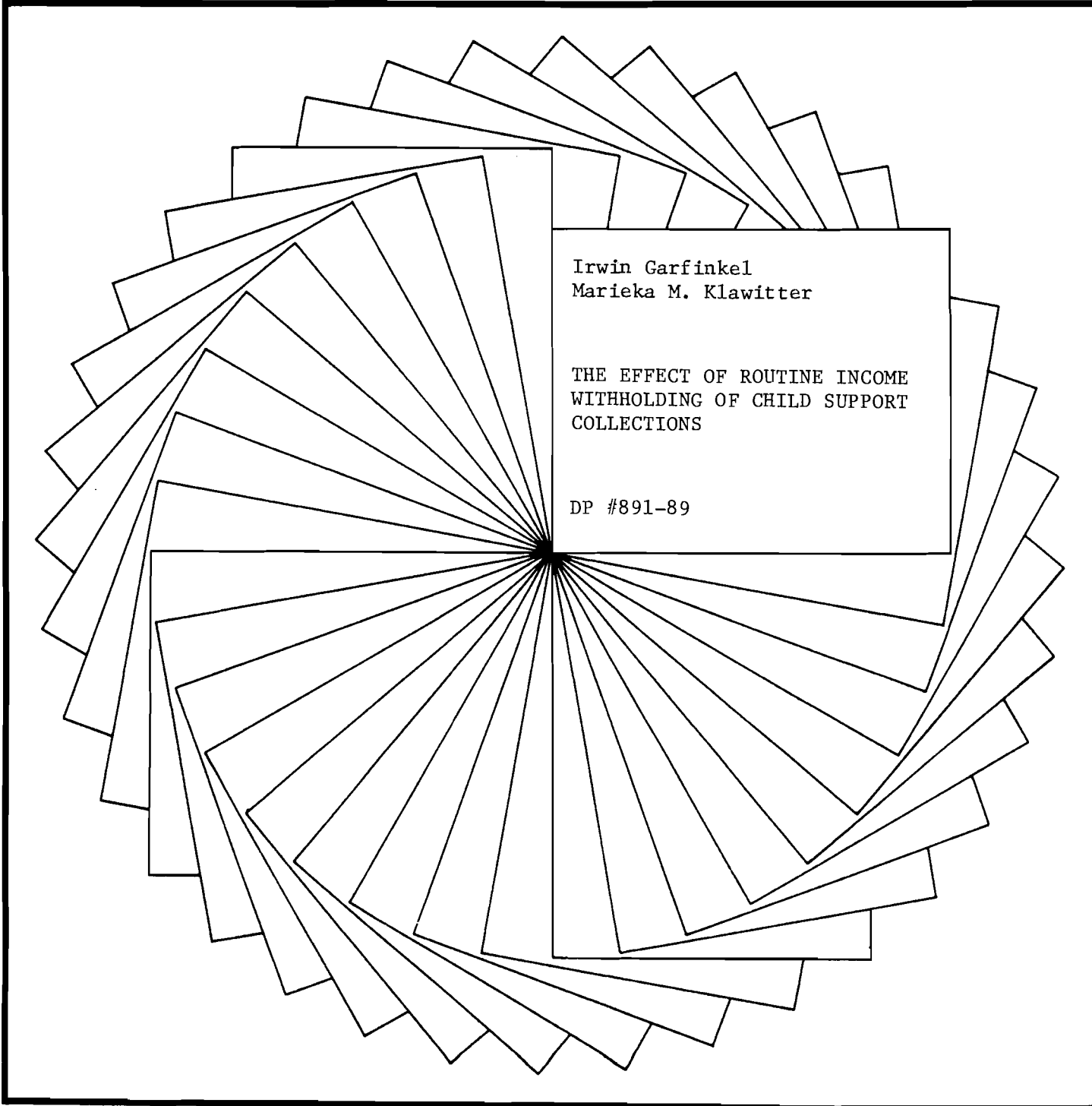
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THE EFFECT OF ROUTINE INCOME
WITHHOLDING OF CHILD SUPPORT
COLLECTIONS

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ON CHILD SUPPORT COLLECTIONS

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Abstract

Recent federal legislation requires states to make substantial
improvements in paternity establishment, enact numeric child support
standards for determining child support awards, update those awards at
least every three years, and adopt routine income withholding of all
child support obligations. Data gathered for the purpose of evaluating
the Wisconsin Child Support Assurance System make it possible to examine
the effects of routine income withholding on the size and regularity of
child support payments.

Data on child support obligations and payments and the use of income
withholding were collected from the court records of 6400 child support
cases which entered the court system between 1980 and 1986 in the 10
pilot and 10 matched control counties. Because the control counties
began to use routine income withholding in a large number of cases, the
cross-county, before-after comparison understates the true effect of
income withholding. On the other hand, because income withholding
cannot be implemented in some cases in which payment is unlikely, and
because we can control for such selectivity only imperfectly, a
comparison of child support payments of those with and without income
withholding orders is likely to overstate the true effect of routine
withholding. The former comparison suggests routine income withholding
increases child support payments by 11 percent whereas the latter
suggests an increase of 30 percent.

Relative to gains achieved by most program interventions, this one is
quite substantial. Relative to the difference between current child
support payments and estimated ability to pay child support--which

implies potential gains of close to 400 percent--the gain is trivial.
Attention should now be turned toward evaluating the independent and
interactive effects of other reforms such as increased paternity
establishment, numeric child support standards, and regular updating of
awards on child support payments.

THE EFFECT OF ROUTINE INCOME WITHHOLDING
ON CHILD SUPPORT COLLECTIONS

INTRODUCTION

This paper examines the effects on child support collections of one

major reform of the American private child support enforcement system--

routine income withholding. Private child support refers to payments

from a nonresident parent to the parent who resides with their children.

Routine income withholding is the requirement that the child support

obligations of nonresident parents routinely be withheld from their

paychecks.

The Family Support Act of 1988 requires all states to adopt routine

withholding for all child support cases by 1994. The state of Wisconsin

was the first in the nation to pilot and adopt a routine income

withholding system. The system was piloted in ten counties in 1984 and

adopted statewide in 1987. Data from Wisconsin's pilot counties and a

set of control counties are used to evaluate the effects of routine

income withholding on child support collections.

Reforms of our child support system are worth evaluating for two

reasons. First, in view of the fact that one-half of the next American

generation will live apart from one of their parents during childhood,

the quality of our child support institutions is important.[1] Second, it

is generally agreed that the system is inadequate.

Routine income withholding is but one of many recent, major reforms

of the American system of enforcing private child support. Although the

media have focused to a considerable extent on income withholding, there

is no logical or empirical basis for believing that this is the single

most important reform. Indeed, as explained below, there is good reason for believing that other reforms may be equally important. Our rationale for focusing on routine withholding is pragmatic: there are good data for evaluating its impact.

In the first section we briefly sketch the outlines of the American child support system, highlight its alleged weaknesses, summarize changes in federal and Wisconsin child support law that were designed to ameliorate these weaknesses, and review the literature on the expected effects of routine withholding. The second section describes the data. The third section presents data on the extent to which withholding was implemented in the pilot and control counties on a routine basis. The fourth section presents estimates of the effects on child support collections of routine income withholding. The paper ends with a brief summary and conclusion.

THE AMERICAN CHILD SUPPORT SYSTEM

Until recently child support was a state and local matter. All states required divorced and separated parents to support their children. Prior to a series of Supreme Court rulings beginning in 1968, children born out of wedlock were not so well protected.[2]

The establishment and enforcement of private child support was characterized by judicial discretion. How much child support a nonresident parent was required to pay was left up to local judges. If the nonresident parent failed to pay, the resident parent--usually the mother--could take the nonresident parent back to court. The remedies available to the courts ranged from chastisement to jailing.

The system did not work very well. Whereas eight of ten divorced women had child support awards, only one of two separated women, and one of ten unmarried women had support awards. Altogether, 40 percent of children with living nonresident fathers still have no legal entitlement to child support.[3]

How much nonresident parents were required to pay in child support varied dramatically from case to case. Families in similar circumstances were treated quite differently.[4] On average, the awards were low. The mean award in 1985 was $2200.[5] It is not clear whether the problem was due to initial awards being low or to the failure to increase awards over time.[6] Only about half of resident parents with child support awards received the full amount of child support to which they were entitled, and about one-quarter received nothing.[7] Finally, about half of families headed by single mothers potentially eligible for child support are poor, and nearly the same proportion are dependent on welfare.[8]

Federal interest in child support grew as the caseload of the Aid to Families with Dependent Children (AFDC) program grew and shifted from orphans to children with living absent parents. Although the first federal legislation to enforce child support was enacted in 1950 and there were further bills in 1965 and 1967, the 1975 legislation was particularly significant because it (1) established the federal Office of Child Support Enforcement; (2) required all states to establish state offices of child support enforcement; and (3) provided federal reimbursement for about three-quarters of each state's enforcement costs. That is to say, the 1975 act created the public bureaucracy to enforce the private child support obligation.

Some states already had relatively strong public agencies charged with enforcing private child support. Michigan and Wisconsin, for example, along with five other states, already required that all private child support payments be paid to and thereby monitored by an administrative arm of the courts. The courts in Michigan also used a simple numerical standard for establishing child support obligations as a percentage of the nonresident parent's income.[9] By 1979 Wisconsin had enacted a law which required county courts to require employers to withhold child support obligations from wages and other sources of income in the event that the obligors became delinquent in payment of child support.

In 1980 the Wisconsin Department of Health and Social Services(DHSS) contracted with the University of Wisconsin's Institute for Research on Poverty to evaluate the existing child support system in Wisconsin and make recommendations for reforms. In 1982 the Institute published its recommendations, which called for building on existing strengths to construct a new child support assurance system (CSAS).[10]

The philosophy underlying CSAS is that parents are responsible for sharing income with their children, and the government is responsible for assuring that children receive the child support to which they are entitled. In addition to strengthening laws to establish paternity and child support awards, CSAS consists of three components. The proportion of their income that nonresident parents are required to share with their children is specified in law. The resulting child support obligation is withheld from wages and other sources of income in all cases just like income taxes and payroll taxes. The child is entitled to receive all that the nonresident parent pays, but no less than an

assured benefit. If the nonresident parent pays less than the assured benefit, the state pays the supplement.

Wisconsin began implementing CSAS in late 1983, when DHSS published a percentage-of-income standard that courts could use to establish child support awards. The standard established child support awards of 17 percent of the nonresident parent's income for one child, and 25 percent, 29 percent, 31 percent, and 34 percent for two, three, four, and five or more children respectively. Between January and June 1984 ten Wisconsin counties began to use routine income withholding. Legislation in 1985 made the percentage-of-income standard the presumptive child support obligation as of July 1987, allowed additional counties to adopt routine income withholding, and required all counties to do so as of July 1987. In addition, the legislation authorized DHSS to pilot the assured child support benefit in several counties. The assured child support benefit is now scheduled to be piloted in two counties in 1990.

After a series of minor reforms in the late 1970s and early 1980s, major federal child support legislation was enacted in 1984 and 1988. These reforms move the nation a considerable distance on the collection side toward a child support assurance system. They require states to adopt expedited procedures for establishing paternity and child support awards, provide for increased federal assistance for establishing paternity, mandate performance standards for state paternity establishment programs, and require states to obtain social security numbers from both parents when issuing birth certificates and to use these numbers in the establishment of paternity.

The states are required to adopt numeric child support guidelines that courts must use in determining child support obligations. While the 1984 legislation allowed the courts to ignore the guidelines, the 1988 legislation makes the guidelines the presumptive child support award. That is, judges may depart from the guidelines only if they construct a written justification which can be reviewed by a higher court. Furthermore, states are required to review child support awards at least every three years.

States are also required to routinely withhold child support obligations from paychecks. Whereas the 1984 legislation required withholding only in the event that payments were one month delinquent, the 1988 legislation requires withholding of the child support obligation from the outset for all IV-D cases (those being handled by the Office of Child Support Enforcement) as of 1990 and for all child support cases as of 1994. In order to do so, states will be forced to develop the capacity to routinely monitor payments in all cases. Only seven states, including Wisconsin, had this capability as of 1988.

Previous research suggests that child support payments could be increased substantially, but that routine income withholding by itself is likely to result in only modest increases. Whereas nonresident fathers in the United States now pay about $7 billion annually in child support, estimates of their income and family circumstances indicate that they can afford to pay between $24 and $28 billion.[11] Yet the failure to collect what is owed is only one part of the problem. McDonald, Moran, and Garfinkel estimated that if Wisconsin were to collect 100 percent of the amount of child support owed by fathers of children on AFDC, collections would increase by $19 million; if the

amount of the child support awards of those with awards were increased

to the level specified by the percentage-of-income standard, collections

would increase by $20 million; finally, if child support awards were

secured in 100 percent of the cases, with no increase in the amounts of

awards and with no improvement in the proportion of the award collected,

collections would increase by $26 million.[12] These figures clearly show

that obtaining more awards and raising the level of awards are at least

as important as collecting a greater proportion of existing awards.

Furthermore, the McDonald, Moran, Garfinkel study found that improving

all three aspects of the system simultaneously--obtaining awards in all

cases, setting award amounts according to the percentage-of-income

standard, and collecting all that is owed--would have increased

collections by $160 million. Because routine income withholding

directly affects only the proportion of child support paid to child

support owed, by itself it will lead to only modest increases in child

support collections.

DATA

Family court records involving divorce, separation, and paternity

cases in which there was at least one child under age 18 were sampled

from 20 Wisconsin counties. Ten of the counties had agreed to utilize

immediate income withholding on a routine basis in all cases in which it

was possible. Ten control counties were chosen based on county

population, geographic location, divorce rate, unemployment rate, and

average per capita income. Table 1 shows that on the whole the pilot

and control counties are fairly well matched. The control counties have

Table 1

Characteristics of Pilot and Control Counties

	1980 Population	1981 Divorced	1980 per Capita Income	1983 Unemployment Rate
Pilot Counties				
Clark	32,910	112	$7,125	12.6%
Dane	323,545	1,741	10,364	6.8
Dunn	34,314	118	6,875	7.7
Kewaunee	19,539	67	8,067	14.3
Monroe	35,074	191	7,995	10.1
Oneida	31,216	188	8,023	10.4
Ozaukee	66,981	303	12,245	9.4 .
Richland	17,476	97	7,346	11.8
Sheboygan	100,935	447	9,773	9.5
Winnebago	131,703	658	9,772	9.2
Unweighted mean	79,369	392	8,775	10.2
Control Counties				
Calumet	30,867	104	8,766	14.7
Dodge	75,064	456	8,882	10.7
Green	30,012	160	9,945	8.1
Jefferson	66,152	336	9,017	11.4
Juneau	21,039	93	7,395	11.4
Marathon	111,270	341	8,240	11.4
Price	15,788	79	7,225	11.7
Racine	173,132	1,083	10,229	13.0
St. Croix	43,262	213	8,087	8.7
Waukesha	280,326	1,481	11,819	10.0
Unweighted mean	84,691	435	8,961	11.1

slightly higher population, number of divorces, per capita income, and unemployment rates.

Predemonstration data were collected for three years. Cases for the baseline sample included only those which entered the court system for the first time during the period from July 1, 1980, through June 30, 1983.[13] Information on all these cases was collected until December 31, 1983.

The demonstration-period sample included cases which began one month after the implementation of routine withholding in each pilot county and at the same time in the "matching" control county. Case selection continued until May 31, 1986, and collection of all case activity continued until January 31, 1987. Table 2 shows the implementation dates for pilot counties as well as for the control counties, all of which implemented routine income withholding later.[14]

In order to facilitate early evaluation of the demonstration, the sampling method for the second and third years of the demonstration period was altered slightly. Cases were considered chronologically from the beginning of the case-selection period instead of being randomly selected over the entire selection period. Sampling continued until a predetermined sample size was reached or until the end of the case-selection period. The selection criteria were otherwise unchanged.

The entire sample is further divided into 6 cohorts by the year during which the case began. Table 3 shows the dates of case selection, data collection, the resulting average number of months of data for each cohort, and the number of cases in each cohort.

Within each county, from about 30 to 150 cases were chosen in each cohort. In some small counties, all eligible cases were used. In

Table 2

Implementation Dates of Routine Income Withholding

	Implementation Date
Pilot Counties	
Clark	1-15-84
Dane	5-01-84
Dunn	1-03-84
Kewaunee	1-01-84
Monroe	2-01-84
Oneida	6-15-85
Ozaukee	2-15-84
Richland	1-03-84
Sheboygan	3-15-84
Winnebago	4-02-84
Control Counties	
Calumet	5-19-87
Dodge	4-01-86
Green	3-17-87
Jefferson	4-10-87
Juneau	8-01-85
Marathon	6-15-86
Price	7-01-86
Racine	8-01-87
St. Croix	8-01-87
Waukesha	8-01-87

Table 3

Case Selection, Data Collection Periods, and
Average Number of Months of Data

Cohort	Case-Selection Period	Data Collection Ends	Average Number of Months of Data	Number of Cases
Predemonstration Period				
Cohort 1	July 1, 1980-June 30, 1981	December 31, 1983	36.6	1093
Cohort 2	July 1, 1981-June 30, 1982	December 31, 1983	24.5	1099
Cohort 3	July 1, 1982-June 30,1983	December 31, 1983	12.5	1083
Demonstration Period				
Cohort 4	Feb. 1, 1984-Sept. 30, 1984[a]	January 31, 1987	32.4	877
Cohort 5	October 1, 1984-May 31, 1985	January 31, 1987	26.5	1116
Cohort 6	October 1, 1985-May 31, 1986	January 31, 1987	14.8	1167

[a]The case-selection period for cohort 4 varied by county. Cases were collected beginning one month after the county implementation date for immediate withholding.

larger counties, a larger number of cases, but a smaller proportion of the caseload, was used. Weights were constructed to adjust for differences in the proportion of cases selected in each county and cohort by case type.

For each case, information was collected about every court action during the data-collection period. This included the dates and purposes of the actions, custody and visitation agreements, child support orders, and other types of monetary obligations. Payment data were also collected, including the amount and dates of all payments sent to the county clerk of courts. Wisconsin law requires nonresident parents to make child support payments through the county clerk of court, but payments are occasionally sent directly to the resident parent. Such payments are not included in these data. Although the court data indicate about 3 percent of cases had legal direct payment agreements, a separate survey estimated that some direct payment occurs in about 7 percent of child support cases.[15] To the extent that routine income withholding results in a substitution of payments through the court for direct private payments, our data will overestimate the true effect of routine income withholding on total child support payments.[16]

In addition to information on child support payments, the court record includes some demographic information such as the number and ages of children, and income and employment information for both parents. Unfortunately, much of the income and employment data is missing in the court record.

DEGREE OF IMPLEMENTATION OF ROUTINE INCOME WITHHOLDING IN PILOT AND CONTROL COUNTIES

The effects of any policy depend in large part on the extent to which the policy is implemented. Similarly, an evaluation of a policy that is based on comparing outcomes in pilot and control counties will be reliable to the extent that the policy was implemented in the former but not the latter. Thus, the extent to which routine income withholding was implemented in both pilot and control counties is of great interest.

Full implementation of routine income withholding would entail issuance of an income withholding order to the employer or other source of income of the obligor at the outset of the child support obligation in every case where it was possible to withhold income. Unfortunately, because some obligors are self-employed and others are unemployed, it is not possible to withhold income in all cases at the outset of the child support obligation.

Table 4 presents data on the potential for using withholding by county and by predemonstration and demonstration periods. In Table 4, as in all that follow, only sole (legal) custody divorce and paternity cases with support orders are included and cases with private pay agreements are excluded.[17] These and all other descriptive statistics in this report are weighted to account for the sampling scheme.

The assignable income sources identified in the court record are wages or salary paid by others and unemployment compensation. Any other source is assumed to be unassignable. Since the court record includes information on income source in only about three-quarters of the sample, the table gives the proportion of cases with missing information and two

Table 4

Percentage of Cases with Missing Income Source and Assignable
Income Source by County, before and during Demonstration

	Predemonstration Period			Demonstration Period		
		Assignable Income			Assignable Income	
	Missing Data	Lower Bound	Upper Bound	Missing Data	Lower Bound	Upper Bound
Pilot Counties						
Clark	18%	55%	67%	5%	67%	71%
Dane	21	67	86	13	73	84
Dunn	33	58	86	36	53	83
Kewaunee	18	65	80	0	90	90
Monroe	31	56	81	18	69	84
Oneida	35	50	77	29	55	77
Ozaukee	21	71	89	11	81	91
Richland	35	53	82	19	64	79
Sheboygan	36	61	95	15	83	97
Winnebago	15	66	78	11	71	80
All pilots	25	63	84	14	73	85
Control Counties						
Calumet	25	67	89	24	65	85
Dodge	29	60	84	19	71	88
Green	13	70	81	28	65	90
Jefferson	25	60	81	16	73	88
Juneau	35	56	86	46	40	73
Marathon	27	62	85	18	67	82
Price	18	69	84	35	49	76
Racine	23	68	87	12	72	81
St. Croix	29	65	92	23	60	77
Waukesha	23	72	93	20	71	89
All controls	24	67	88	18	69	85

projections of the potential proportion of cases with assignable income.[18]

The first column for both time periods gives the proportion of all cases with missing data for income source for the nonresident parent. It is clear that information on income source is collected when needed, since the proportion of cases with missing information on income source decreased dramatically in pilot counties during the demonstration period. Prior to the demonstration, pilot and control counties had about equal proportions of missing information, 25 percent and 24 percent, respectively. However, the proportion of missing information in pilot counties during the demonstration decreased to 14 percent while the proportion in control counties decreased to 18 percent.

The second column in Table 4 gives a conservative estimate of the proportion of cases with assignable income: the number of cases with a known assignable income source divided by the total number of cases (including cases without income information). This lower-bound estimate assumes that no cases with missing information on income source have an assignable income source.

The third column gives a more generous estimate of assignable income: the number of cases with a known assignable income source divided by the number of cases with information on income source. This higher estimate is made assuming that the proportion of cases with assignable income among the cases with missing income information is equal to the proportion for the cases with known income sources.

The estimates of assignable income for pilot counties during the demonstration are probably the most accurate assessment of the potential for using routine withholding. Those figures suggest that in about 73

to 85 percent (the lower and the higher estimates) of divorce and paternity cases, the nonresident parent has income which can be assigned.

Given the experience of pilot counties, the higher estimate of assignable income may be a fairly accurate estimate of the level of assignable income. For the pilot counties, the proportion of cases with missing income source decreased by 11 percentage points, increasing the pool of cases with income source information. At the same time, the percentage of cases with assignable income (based on the conservative estimate) increased by 1 percentage point. This implies that the "new" cases with income information had about the same proportion of assignable income as the "original" cases.

The actual use of withholding is shown in Table 5, by county and cohort. A case was defined as having a routine assignment if there was an income assignment issued within 60 days of the first court action with a child support order. Cases with an income assignment dated more than 60 days after the court action, or with no effective date in the court record, were not labeled as immediate assignments.[19]

Table 5 shows that the use of routine income assignments has increased in both pilot and control counties over the court-record period. In pilot counties, the proportion of cases with assignments has increased from 4, 5, and 6 percent in the first 3 cohorts, to 57, 56, and 65 percent in the demonstration-period cohorts. Although the increase in assignments in pilot counties during the demonstration period is dramatic, levels remained substantially below the potential levels reported in Table 4. Even by the third year, routine income withholding was not fully implemented in the pilot counties.

Table 5

Use of Immediate Income Assignment, by County and Cohort

	Predemonstration Period			Demonstration Period		
	1	2	3	4	5	6
Pilot Counties						
Clark	9%	17%	0%	50%	71%	56%
Dane	3	4	8	57	47	52
Dunn	0	0	0	76	48	88
Kewaunee	9	13	19	67	-[b]	83
Monroe	15	13	22	31	42	61
Oneida	0	5	0	44	26	64
Ozaukee	4	2	7	70	75	82
Richland	7	3	0	54	62	69
Sheboygan	4	2	2	73	63	80
Winnebago	0	6	6	50	67	75
All pilots	4	5	6	57	56	65
Control Counties						
Calumet	10	7	6	19	25	32
Dodge[a]	7	0	0	22	22	70
Green	2	6	5	36	32	51
Jefferson	3	7	13	18	30	41
Juneau[a]	8	4	0	19	5	93
Marathon	0	0	0	5	5	52
Price	0	0	0	0	7	60
Racine	40	19	22	33	27	57
St. Croix	0	0	0	4	5	31
Waukesha	2	7	3	14	35	48
All controls	15	9	10	20	25	53

[a]Became a pilot county in cohort 6.

[b]No sole custody cases with awards for cohort 5.

Less than complete implementation may not be a serious problem for analysis as no policy is ever fully implemented. Is there any reason to expect that national implementation of routine withholding will be any better than the Wisconsin experience? The Wisconsin data do suggest that implementation improved over time. At the very least, therefore, a comparison of increases in child support payments in pilot and control counties in the first two years of the demonstration will underestimate the long-run effects of national implementation of routine income withholding.

A more serious problem is that use of routine withholding also increased in the control counties. Although control counties used routine withholding much less frequently than pilot counties in cohorts 4 and 5, the proportions (20 percent and 25 percent) were far in excess of zero and by the sixth cohort, control counties were using assignments in over half the cases. This widespread use of routine assignments in the control counties means that a simple comparison of collections in pilot and control counties would likely underestimate the effects of national implemention of routine income withholding.

ESTIMATING THE EFFECTS OF ROUTINE WITHHOLDING

Child support payments, the key outcome variable in the analysis, are measured by (1) the ratio of dollars of child support paid to dollars of child support due, and (2) the ratio of months in which a child support payment was made to months in which there was a child support obligation.[20] The former measures the collection rate of child support dollars, and the latter reflects the regularity and timeliness of

payments. Both of these were calculated for the time between the first month of child support owed and the last month of court record data collected. These measures, averaged over the data period, are used to study the effects of withholding over the first few years after a support order. Monthly measures, described below, are later used to study differences in the impact of withholding over time.

The impact of routine income withholding on child support payments is measured in three different ways. The first is simply the difference in increases in child support payments between cases in pilot and control counties. The second is derived from the relationship between the extent of utilization of immediate income assignments in counties and child support payments in those counties. The third measure is the difference in child support payments between cases with and without immediate income assignments.

As discussed above, the difference in child support payments between pilot and control counties is expected to be an underestimate of the long-run effect of national implementation of routine income withholding.

There are two reasons for believing that extrapolating from the relationship between county use of routine withholding and county payment rates may overestimate the effects of routine income withholding. First, differences in the utilization of routine withholding across counties may be attributable to county differences in the proportion of cases in which assignments are possible. Second, the differences may reflect administrative discretion on the part of the courts, which is correlated with other efforts the courts make to enforce payments. For both reasons, counties with higher levels of

routine withholding might have had higher payment rates even without the withholding.

The third measure may result in an overestimate of the impact because cases with routine income assignments may be better prospective payers. Courts cannot issue assignments to those with no income. On the other hand, courts may be reluctant to issue assignments to well-known, influential members of their community. In about 80 percent of the cases we have data on and therefore can control for whether or not the nonresident parent has income which can be withheld. But that still leaves about 20 percent of the cases in which we cannot control for the absence of assignable income. On balance, therefore, it is likely that the difference between payments in cases with and without immediate assignments will be an overestimate of the effects of implementing routine withholding.

In short, the first measure of the impact of routine income withholding is likely to be an underestimate, while the second and third measures are likely to be overestimates.

Table 6 presents the ratios of child support dollars paid to dollars owed and months paid to months owed by pilot county status and period, by county withholding level, and by case withholding status. All of the measures suggest that routine withholding has positive effects. The ratio of dollars paid to dollars owed increased by 7 percentage points in the pilot counties compared to only 4 percentage points in the control counties. The payment rates in counties which used routine assignments in less than 10 percent of their cases are nearly 9 percentage points less than the rates in counties that used assignments in over 60 percent of their cases. Also, payment rates for cases with

Table 6

Percentage of Child Support Dollars Paid-to-Owed and Months
Paid-to-Owed by Pilot County Status and Period, County
Withholding Level, and Individual Case Withholding

Experimental County Status	Dollars Paid-to-Owed	Months Paid-to-Owed
Pilot Counties		
Predemonstration period	55.16%	54.00%
Demonstration period	62.03	61.10
Control Counties		
Predemonstration period	55.12	56.94
Demonstration period	59.10	58.80
County Withholding Level		
0 to 10%	56.12	55.36
11 to 30%	56.57	58.11
31 to 45%	56.94	57.43
46 to 60%	61.15	60.70
61 and over	64.76	65.21
All	57.27	57.28
Individual Case Withholding		
Immediate withholding cases	70.34	70.78
Nonwithholding cases	53.17	53.04

routine withholding are 17 percentage points higher than for cases without withholding.

Because the policy variables may be correlated with other variables, we use multivariate regression analyses to further explore the relationship between the policy variables and child support payments. The values of the dependent variables in these regressions are constrained to be between 0 and 100, so we estimate tobit regressions.[21]

The first policy variable is a dummy variable equal to one for cases opened in a county after the county became a pilot county and equal to zero for all other cases. The second policy variable is equal to the percentage of cases in a county using immediate withholding during the period in which a case got an award. The third variable is a dummy variable equal to one if the case had immediate withholding and zero otherwise. Each of the policy variables is used alone in separate regressions.

In addition to the policy variable, each regression also contains the following independent variables: a dummy variable equal to one if the case was a paternity case, a dummy variable equal to one if the case was one in which the mother rather than the father was the payer, a dummy variable equal to one if the payer had a reported income of zero, a dummy variable equal to one if the payer had reported assignable income, a dummy variable equal to one if the payer had missing income amount, the dollar amount of the payer's income (set to the mean if missing), and a set of dummy variables for the cohort and county of the case. The coefficients of the other independent variables are not very sensitive to which of the policy variables is included in the regression. Thus

the full set of regression coefficients is presented in Appendix A and only the policy variable coefficients are reported in the tables.

Table 7 presents the coefficients of the three alternative specifications of the policy variable from separate tobit analyses. In addition to the coefficients of the policy variables, Table 7 also presents the percentage increases in child support payments that are implied by the coefficients. These impacts are calculated as the differences in the predicted means for the control and experimental statuses as a percentage of the control county mean in the demonstration period. For the second policy variable, the level of withholding, the experimental impact is evaluated as a change of 50 percentage points in the level of withholding.

All of the policy coefficients in Table 7 are positive and, as indicated by the t-ratios in parentheses, all but one are statistically significant at the 5 percent level or better. Not surprisingly, the percentage increases in payments implied by the coefficients are much larger for the second and third measures of the impact of withholding than for the first. Based on the previous argument that the first measure is an underestimate and the second and third are overestimates of the impact of implementing routine income withholding, the estimates in Table 7 suggest that routine income withholding will increase child support payments by more than 11 percent and less than 30 percent.[22]

Note also that the effects on the ratios of months-paid to months-owed are larger than those on the ratios of dollars-paid to dollars-owed. This makes sense insofar as payment irregularity is a problem over and above nonpayment. Routine income withholding should have an effect on the regularity of payments as well as on total payments.

Table 7

Effects of Experimental Variables on Summary Ratios
of Dollars and Months Paid-to-Owed from Tobits

| | Dollars Paid-to-Owed | | Months Paid-to-Owed | |
	Coefficient (t-ratio)	Impact	Coefficient (t-ratio)	Impact
Experimental county dummy	5.86 (1.74)	11.0%	7.38 (2.29)	14.4%
County withholding level	.23 (2.73)	22.0%	.25 (3.06)	24.5%
Individual case withholding	14.83 (5.92)	28.4%	14.94 (6.24)	29.6%

Note: Impacts are calculated as the difference in the predicted pay-to-owe ratio due to experimental status as a percentage of demonstration period control county mean. The impact for the county withholding level is evaluated for a 50 percentage point increase in withholding.

So far, we have examined the average effects of routine income withholding. Reports from state civil servants who monitored the implementation of routine income withholding, however, suggest that the process was relatively chaotic for the first five or six months of implementation. This suggests that the implementation of routine income withholding may actually have had a negative effect in the early months of implementation and an increasingly positive effect thereafter. Furthermore, as Table 5 indicates, the proportion of cases in pilot counties in which an income assignment was implemented increased notably in the third year, which suggests that the estimated effects of withholding should be larger in the sixth cohort.

To test for the possibility that the effects of income withholding may have varied over time, monthly child support payment data were used to construct a variable equal to the number of months between the first month in which support was owed and the first month with no payment. This variable was used to estimate a hazard rate, the probability of having no payment in a month, given there has been no previous month without payment. The model used for the hazard rate estimated here is a type of proportional hazard based on a Weibull distribution for the number of months until a nonpayment. This model allows the estimated hazard rate to either increase or decrease monotonically over time, but not to change directions. The independent variables are assumed to shift the hazard rate up or down proportionately over the entire spell length. The percentage change in the hazard for a unit change in the independent variable is $e^b - 1$, where b is the coefficient.

An additional policy variable was constructed to assess changes in the effectiveness of withholding as the county gained administrative

experience. This variable, the county withholding experience, is equal to the number of months between the first month the county began routine withholding and the first month support was owed for the case. It is equal to zero for cases in the predemonstration period and for control county cases in the demonstration period.

Table 8 shows the coefficients for the experimental and county experience variables from the hazard functions (complete results are in Appendix B). Also shown are the estimated changes in the hazard rate of a nonpayment, calculated for a county with 12 months of withholding experience.

The coefficient estimated for the experimental county variable is positive but statistically insignificant. On the other hand, in the same regressions, the coefficient on the variable measuring county withholding experience is negative and statistically significant. Taken together, these coefficients suggest that nonresident parents who entered the court system in the pilot counties soon after the pilots commenced were somehat more likely to miss a payment than nonresident parents in control counties, but that as the pilot counties gained more experience with routine income withholding, income withholding became more effective.

The county withholding level and the individual case withholding coefficients are both negative, as are the accompanying pilot county experience coefficients. Note that the county experience coefficients in these regressions are half the size of the coefficient in the regression with the experimental county dummy. This makes sense in that presumably part of the effect of more experience is attributable to more

Table 8

Effects of Experimental Variables on Weibull Hazard Rate of Having a Month with No Child Support Paid

	Policy Variable Coefficient (t-ratio)	Coefficient on Months of County Withholding Experience (t-ratio)	Impact with 12 Months of County Experience
Experimental county	.11 (0.92)	-.02 (2.83)	-12.2%
County withholding level	-.004 (1.89)	-.01 (1.97)	-27.4%
Individual case withholding	-.34 (5.09)	-.01 (2.45)	-36.9%

Note: Proportional change in hazard is calculated as e^B for experimental county and individual case withholding. Change for county withholding level is $e^{B \times 50}$ (the impact of increasing withholding by 50 percent points).

widespread use of income withholding, which would be picked up by the county withholding level and individual case coefficients.

Survival rates, the proportions of cases expected to not yet have had a month with no payment, can be calculated from the estimated hazard rates. Figure 1 shows the survival rates over the months in a case for the hazard function associated with the experimental county dummy variable. The survival rates are calculated at the mean value for all control variables. Separate lines are graphed for cases in nonexperimental counties, and for cases in experimental counties at three levels of county withholding experience: 0 months, 12 months, and 24 months. For the nonexperimental counties, only about 20 percent of the cases would be expected to go 12 months without a nonpayment month, and only about 10 percent would last 24 months. Experimental county cases starting in the first month of county implementation (0 months of county experience) have slightly lower survival rates than the nonexperimental counties because of the negative coefficient on the experimental county dummy. However, for cases starting after 12 months of county withholding experience, the survival rates are slightly higher than those for the nonexperimental county cases. And for cases in counties with 24 months of experience, the rates are substantially higher: about 30 percent vs. 20 percent 12 months after the first month owed and 15 percent vs. 10 percent 24 months after the first month owed.

In short, the hazard models provide support for the hypothesis that routine income withholding became more effective over time.

In order to further explore this hypothesis and see if it holds up with respect to total child support payments as well as affecting the timing of the first delinquency, monthly data were also used to estimate
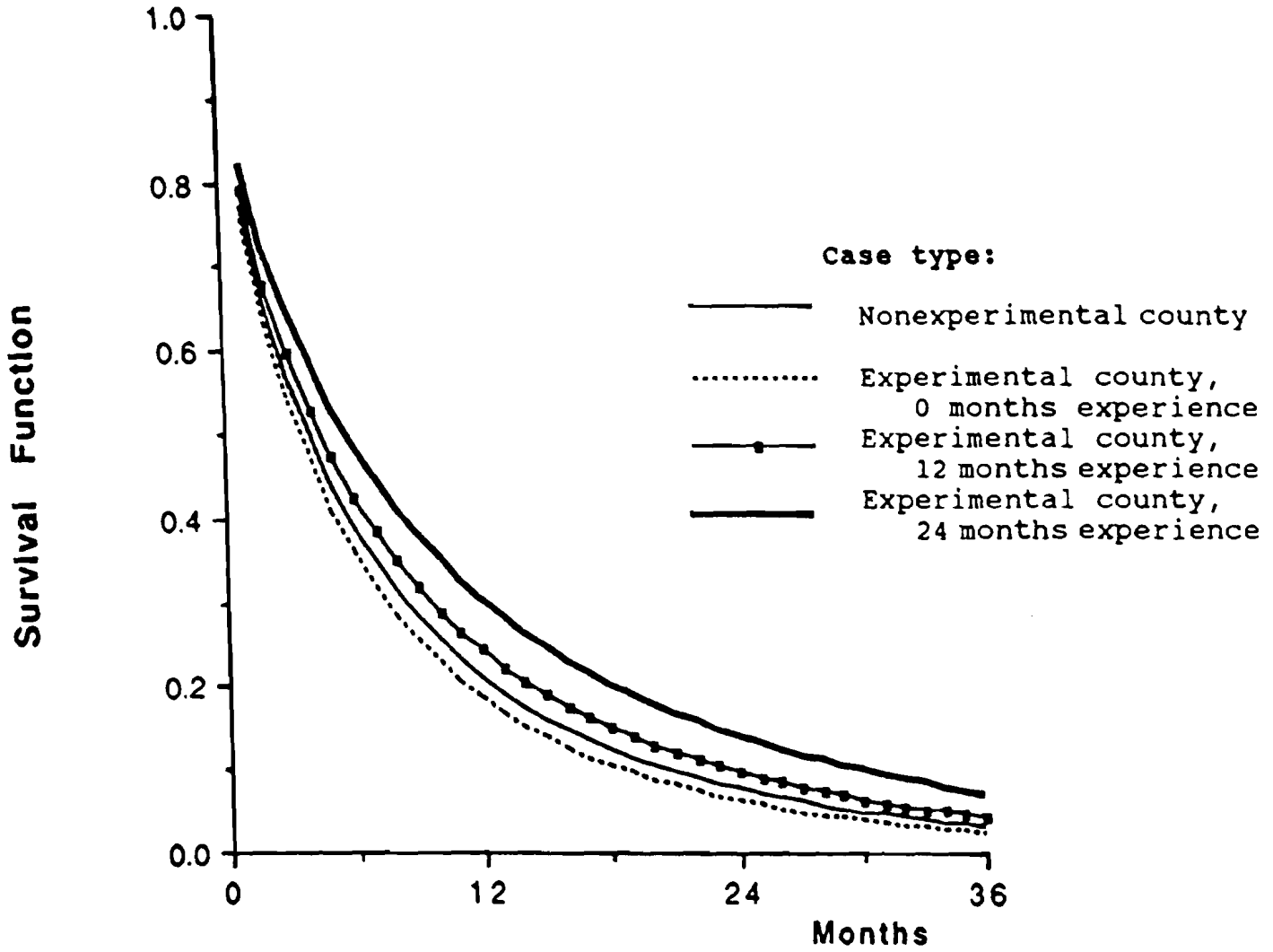
Figure 1: Expected Proportion of Cases without a Nonpayment Month by Months since Support Order

the effects of the experimental variables on payment in a given month
while also controlling for the number of months the case had had an
award. Both a dummy variable indicating any payment in a particular
month and the ratio of dollars paid-to-owed in that month were used as
dependent variables. The data for these analyses were created by
randomly selecting one month out of the case history for each case.
This was done to decrease the number of observed months and to eliminate
the possibility of correlation across errors for the months belonging to
the same case.

Table 9 presents the coefficients and t-statistics from probit
equations on the chance of having any payment in the random month.
While the county withholding level and individual case withholding
coefficients are positive and statistically significant, the
experimental county coefficient is negative, though not significantly
different from zero. Unlike the hazard model, none of the county
withholding experience variable coefficients is significantly different
from zero. Two even have the wrong sign. Table 10 presents the
coefficients and t-statistics from tobit analyses of the ratio of
dollars of support paid-to-owed in the random month. As in the probit
analyses, none of the county withholding experience variables are
significantly different from zero.

On the other hand, in a probit and a tobit (not reported in Tables 9
and 10) which included all three experimental variables, as well as the
county experience variables, the county withholding experience variable
coefficient was positive, though only in the probit was it statistically
significant.

Table 9

Probit Analysis of Effects of Experimental Variables
on Having a Payment in a Random Month

Experimental county	-.13	(1.09)	--		--	
County withholding level	--		.006	(2.42)	--	
Individual case withholding	--		--		.35	(5.25)
County withholding experience	.007	(1.05)	-.003	(0.59)	-.0005	(0.11)
Case time	-.004	(1.32)	-.005	(1.44)	-.005	(1.60)
Paternity case	.09	(1.62)	-.09	(1.60)	-.10	(1.77)
Mother payer case	-.10	(0.48)	-.12	(0.54)	-.08	(0.40)
Nonresident parent's income variables						
Zero income dummy	-.32	(2.31)	-.33	(2.35)	-.29	(2.08)
Assignable income dummy	.70	(12.93)	.69	(12.83)	.64	(11.72)
Income amount	.0001	(6.83)	.0002	(6.87)	.0002	(7.00)
Missing income dummy	-.28	(4.68)	-.29	(4.72)	-.28	(4.66)
Cohort variables						
Cohort 2	.32	(4.32)	.32	(4.39)	.32	(4.36)
Cohort 3	.40	(4.89)	.40	(4.91)	.40	(4.87)
Cohort 4	.32	(3.48)	.13	(1.34)	.19	(2.27)
Cohort 5	.46	(3.34)	.30	(3.14)	.34	(4.07)
Cohort 6	.41	(4.00)	.18	(1.37)	.23	(2.17)
County variables						
Clark	.63	(4.39)	.57	(3.94)	.58	(4.05)
Dane	.58	(5.77)	.54	(5.50)	.55	(5.65)
Dunn	.63	(4.14)	.57	(3.77)	.57	(3.78)
Kewaunee	.76	(4.16)	.66	(3.57)	.72	(3.96)
Monroe	.53	(3.93)	.49	(3.62)	.49	(3.66)
Oneida	.49	(3.48)	.44	(3.21)	.47	(3.37)
Ozaukee	.56	(4.59)	.49	(3.94)	.52	(4.27)
Richland	.55	(4.16)	.49	(3.72)	.53	(3.99)
Sheboygan	.61	(5.40)	.52	(4.63)	.56	(5.06)
Winnebago	.35	(3.14)	.26	(2.38)	.29	(2.71)
Calumet	1.07	(7.65)	1.07	(7.66)	1.10	(7.83)
Dodge	.75	(5.84)	.72	(5.65)	.74	(5.81)
Green	.41	(2.95)	.38	(2.78)	.41	(2.96)
Jefferson	.75	(6.14)	.75	(6.15)	.77	(6.29)
Juneau	.65	(5.00)	.61	(4.68)	.62	(4.84)
Marathon	.63	(5.45)	.67	(5.77)	.67	(5.86)
Price	.87	(5.53)	.92	(5.76)	.91	(5.73)
Racine	.63	(5.76)	.55	(4.85)	.59	(5.40)
St. Croix	.25	(2.01)	.30	(2.35)	.31	(2.41)
Intercept	-1.13	(13.77)	-1.12	(13.73)	-1.10	(13.50)
Log-Likelihood	-2088.9		-2086.6		-2075.6	

Note: Absolute values of t-statistics in parentheses.

Unlike the hazard analysis, therefore, the probit and tobit analyses provide, at best, mixed evidence that routine withholding became more effective over time. It should be noted that the three analyses though related do not capture the same phenomenon. The hazard measures the length of time until the first month when there is no payment, whereas the probit measures the probability of a payment in any month and the tobit measures the proportion of the obligation that is paid in any month. Still, there is no obvious explanation for the seemingly inconsistent results. At this point, therefore, we conclude that there is not consistent support for the hypothesis that the effectiveness of routine withholding increases as counties gain implementation experience.

Moreover, note in Table 10 that neither the experimental county dummy nor the county withholding level coefficients are significantly different from zero, and the former has the wrong sign. Only the individual case withholding dummy is positive and significantly different from zero. Why the withholding coefficients in the random month pay-to-owe tobit regressions are weaker than those in the average pay-to-owe tobit regressions is not clear. One possibility is that routine withholding may have a smaller effect on one month ratios because of variation in pay periods or work availability for a payer, but may lead to a higher collection of delinquent payments when the payer is working. In any case, in view of the fact that the random-month regressions are based on much less information, more confidence should be placed in the average results reported above.

Table 10

Tobit Analysis of Effects of Experimental Variables
on Monthly Ratios of Dollars Paid-to-Owed

Experimental county	-.46	(1.55)	--		--	
County withholding level	--		.00002	(0.004)	--	
Individual case withholding	--		--		.49	(2.96)
County withholding experience	.007	(0.43)	-.009	(0.70)	-.01	(1.07)
Case time	-.001	(0.17)	-.002	(0.19)	-.002	(0.30)
Paternity case	-.02	(0.18)	-.02	(0.13)	-.03	(0.24)
Mother payer case	-.06	(0.13)	-.07	(0.15)	-.06	(0.13)
Nonresident parent's income variables						
Zero income dummy	-.46	(1.58)	-.46	(1.58)	-.41	(1.42)
Assignable income dummy	1.16	(8.60)	1.16	(8.62)	1.08	(7.94)
Income amount	.0001	(6.13)	.0001	(6.23)	.0001	(6.28)
Missing income dummy	-.38	(2.57)	-.38	(2.57)	-.37	(2.53)
Cohort variables						
Cohort 2	.43	(2.29)	.43	(2.31)	.43	(2.32)
Cohort 3	.73	(3.86)	.73	(3.87)	.73	(3.87)
Cohort 4	.63	(2.88)	.46	(2.00)	.33	(1.70)
Cohort 5	.88	(4.16)	.77	(3.24)	.63	(3.10)
Cohort 6	.80	(3.22)	.74	(2.29)	.51	(2.03)
County variables						
Clark	1.64	(6.49)	1.56	(6.30)	1.51	(6.19)
Dane	1.00	(3.82)	.90	(3.56)	.90	(3.57)
Dunn	1.42	(4.68)	1.34	(4.49)	1.31	(4.43)
Kewaunee	2.42	(6.24)	2.36	(5.94)	2.32	(5.97)
Monroe	.97	(2.86)	.89	(2.68)	.86	(2.55)
Oneida	.90	(2.56)	.81	(2.33)	.81	(2.34)
Ozaukee	.96	(3.14)	.89	(2.91)	.85	(2.82)
Richland	.96	(2.72)	.90	(2.54)	.88	(2.52)
Sheboygan	1.21	(5.09)	1.12	(4.74)	1.08	(4.69)
Winnebago	.74	(2.79)	.64	(2.43)	.60	(2.33)
Calumet	1.46	(4.34)	1.48	(4.39)	1.50	(4.47)
Dodge	1.06	(3.17)	1.00	(3.04)	1.01	(3.07)
Green	.56	(1.55)	.57	(1.55)	.58	(1.61)
Jefferson	1.09	(3.50)	1.11	(3.56)	1.13	(3.63)
Juneau	1.11	(3.54)	1.07	(3.38)	1.05	(3.36)
Marathon	.95	(3.30)	.97	(3.33)	1.03	(3.62)
Price	1.35	(3.80)	1.36	(3.80)	1.40	(3.92)
Racine	.82	(2.96)	.84	(2.89)	.77	(2.73)
St. Croix	.63	(2.22)	.65	(2.26)	.72	(2.55)
Intercept	-2.42	(10.78)	-2.38	(10.78)	-2.35	(10.63)
Log-Likelihood	-5101.7		-5103.8		-5095.5	

Note: Absolute values of t-statistics in parentheses.

SUMMARY AND CONCLUSION

The U.S. child support system is undergoing profound changes. Recent federal legislation requires states to make substantial improvements in paternity establishment, enact numeric child support standards for determining child support awards, update those awards at least every three years, and adopt routine income withholding of all child support obligations. This paper focuses on only the last of these many changes in child support enforcement practices.

Ten Wisconsin counties began piloting routine income withholding in 1984. As originally conceived, the evaluation design consisted of a cross-county, before-after study. Data on child support obligations and payments and the use of income withholding were collected from the court records of 6400 child support cases which entered the court system between 1980 and 1986 in the 10 pilot and 10 matched control counties. Because the control counties began to use routine income withholding in a large number of cases, the cross-county, before-after comparison understates the true effect of income withholding. On the other hand, because income withholding cannot be implemented in some cases in which payment is unlikely, and because we can control for such selectivity only imperfectly, a comparison of child support payments of those with and without income withholding orders is likely to overstate the true effect of routine withholding. The former comparison suggests routine income withholding increases child support payments by 11 percent whereas the latter suggests an increase of 30 percent.

Whether an increase of between 11 and 30 percent in child support payments is large or small depends upon the basis of comparison.

Relative to gains achieved by most program interventions, this one is quite substantial. Relative to the difference between current child support payments and estimated ability to pay child support--which implies potential gains of close to 400 percent--the gain is trivial. What this suggests is that while routine income withholding will increase child support payments by a modest amount, it is no panacea. Attention should now be turned toward evaluating the independent and interactive effects of other reforms such as increased paternity establishment, numeric child support standards, and regular updating of awards on child support payments.

Appendix A

Tobit Analysis of Effects of Experimental Variables on Child Support Paid-to-Owed

	Dollars Paid-to-Owed					
Experimental county	5.86	(1.74)	--		--	
County withholding level	--		.23	(2.73)	--	
Individual case withholding	--		--		14.83	(5.92)
Paternity case	-6.31	(3.09)	-6.29	(3.09)	-6.71	(3.30)
Mother payer case	-13.28	(1.83)	-14.33	(1.97)	-12.91	(1.75)
Nonresident parent's income variables						
Zero income dummy	-26.16	(5.28)	-26.09	(5.29)	-25.05	(5.11)
Assignable income dummy	19.67	(9.43)	19.50	(9.36)	16.96	(8.07)
Income amount	.003	(8.89)	.003	(8.95)	.003	(8.86)
Missing income dummy	-21.73	(9.70)	-21.90	(9.81)	-21.80	(9.83)
Cohort variables						
Cohort 2	.55	(0.19)	.71	(0.25)	.66	(0.23)
Cohort 3	1.27	(0.44)	1.60	(0.56)	1.42	(0.50)
Cohort 4	2.12	(0.59)	-1.47	(0.37)	.64	(0.19)
Cohort 5	9.43	(2.70)	5.88	(1.52)	7.68	(2.44)
Cohort 6	3.12	(0.89)	-3.99	(0.82)	-1.13	(0.36)
County variables						
Clark	14.69	(2.68)	14.26	(2.64)	14.91	(2.80)
Dane	3.74	(0.97)	4.73	(1.32)	5.25	(1.48)
Dunn	8.62	(1.46)	8.67	(1.50)	8.87	(1.59)
Kewaunee	7.78	(1.14)	5.85	(0.86)	7.31	(1.10)
Monroe	4.43	(0.87)	4.77	(0.95)	4.51	(0.91)
Oneida	-.39	(0.07)	.62	(0.12)	1.31	(0.26)
Ozaukee	6.96	(1.52)	6.01	(1.33)	7.30	(1.67)
Richland	-2.58	(0.51)	-2.86	(0.58)	-1.86	(0.38)
Sheboygan	11.14	(2.54)	10.33	(2.39)	11.43	(2.75)
Winnebago	-6.14	(1.43)	-6.80	(1.63)	-5.70	(1.43)
Calumet	27.47	(5.10)	27.57	(5.14)	27.59	(5.15)
Dodge	15.18	(3.18)	15.75	(3.32)	15.49	(3.25)
Green	-7.45	(1.34)	-7.98	(1.45)	-7.63	(1.41)
Jefferson	15.55	(3.21)	15.66	(3.24)	15.64	(3.25)
Juneau	7.28	(1.43)	7.30	(1.44)	7.36	(1.47)
Marathon	14.76	(3.24)	16.60	(3.63)	16.04	(3.54)
Price	19.29	(3.16)	21.16	(3.47)	20.43	(3.39)
Racine	2.18	(0.50)	-.76	(0.17)	-.05	(0.01)
St. Croix	-11.66	(2.62)	-10.03	(2.25)	-10.21	(2.32)
Intercept	42.20	(11.30)	41.14	(3.64)	42.64	(11.73)
Sigma	49.32	(61.47)	49.21	(61.60)	49.02	(61.47)
Log Likelihood	-14062		-14060		-14045	

Note: Absolute values of t-statistics in parentheses.

Appendix A (Continued)

Tobit Analysis of Effects of Experimental Variables on Child Support Paid-to-Owed

	Months Paid-to-Owed					
Experimental county	7.38	(2.29)	--		--	
County withholding level	--		.25	(3.06)	--	
Individual case withholding	--		--		14.94	(6.24)
Paternity case	-12.16	(6.00)	-12.26	(6.04)	-12.60	(6.26)
Mother payer case	-15.28	(2.26)	-17.35	(2.55)	-15.68	(2.29)
Nonresident parent's income variables						
Zero income dummy	-25.36	(5.08)	-25.27	(5.06)	-23.95	(4.86)
Assignable income dummy	21.72	(10.70)	21.53	(10.59)	19.09	(9.33)
Income amount	.003	(9.54)	.003	(9.52)	.003	(9.45)
Missing income dummy	-19.96	(9.17)	-20.08	(9.23)	-19.94	(9.26)
Cohort variables						
Cohort 2	4.43	(1.60)	4.55	(1.63)	4.49	(1.63)
Cohort 3	6.19	(2.24)	6.30	(2.28)	6.25	(2.28)
Cohort 4	2.60	(0.74)	-.76	(0.20)	1.56	(0.48)
Cohort 5	9.94	(2.97)	6.74	(1.81)	8.96	(2.92)
Cohort 6	6.36	(1.91)	-.88	(0.19)	2.79	(0.93)
County variables						
Clark	4.77	(0.84)	5.14	(0.91)	5.83	(1.06)
Dane	2.10	(0.57)	4.01	(1.16)	4.52	(1.34)
Dunn	2.68	(0.46)	3.58	(0.63)	4.05	(0.73)
Kewaunee	4.85	(0.72)	3.55	(0.52)	5.20	(0.79)
Monroe	-5.16	(0.99)	-4.03	(0.78)	-4.09	(0.80)
Oneida	-1.01	(0.20)	.85	(0.17)	1.76	(0.36)
Ozaukee	5.44	(1.24)	5.16	(1.19)	6.77	(1.62)
Richland	-5.90	(1.20)	-5.35	(1.10)	-4.11	(0.86)
Sheboygan	10.00	(2.42)	9.99	(2.44)	11.39	(2.90)
Winnebago	-6.59	(1.60)	-6.50	(1.61)	-5.22	(1.35)
Calumet	29.27	(5.83)	29.18	(5.82)	29.09	(5.85)
Dodge	13.78	(3.02)	14.60	(3.22)	14.51	(3.19)
Green	-2.41	(0.49)	-2.77	(0.56)	-2.22	(0.46)
Jefferson	19.34	(4.22)	19.58	(4.27)	19.57	(4.30)
Juneau	4.30	(0.90)	4.71	(0.99)	4.84	(1.03)
Marathon	15.94	(3.74)	18.30	(4.26)	17.64	(4.16)
Price	11.34	(1.82)	13.77	(2.21)	12.87	(2.09)
Racine	8.86	(2.22)	5.83	(1.42)	6.90	(1.71)
St. Croix	-19.82	(4.46)	-17.76	(3.98)	-17.98	(4.09)
Intercept	40.06	(11.19)	38.51	(10.97)	39.87	(11.41)
Sigma	47.35	(63.89)	47.36	(63.89)	47.07	(63.64)
Log Likelihood	-13814		-13812		-13796	

Note: Absolute values of t-statistics in parentheses.

Appendix B

Results of Weibull Hazard Analysis of Time Until
First Month with No Child Support Payment

Experimental county	.11	(0.92)	--		--	
County withholding level	--		-.004	(1.89)	--	
Individual case withholding	--		--		-.34	(5.09)
County withholding experience	-.02	(2.83)	-.01	(1.97)	-.01	(2.45)
Paternity case	.38	(6.71)	.38	(6.70)	.39	(6.87)
Mother payer case	.18	(0.77)	.19	(0.78)	.16	(0.66)
Nonresident parent's income variables						
Zero income dummy	.53	(3.28)	.53	(3.32)	.52	(3.25)
Assignable income dummy	-.52	(9.07)	-.52	(9.04)	-.48	(7.99)
Income amount	-.03	(11.74)	-.03	(11.80)	-.03	(11.47)
Missing income dummy	.51	(8.35)	.51	(8.30)	.51	(8.36)
Cohort variables						
Cohort 2	.12	(1.78)	.12	(1.74)	.11	(1.59)
Cohort 3	.26	(3.16)	.26	(3.12)	.25	(3.03)
Cohort 4	-.44	(0.50)	.10	(1.07)	.08	(1.10)
Cohort 5	.72	(0.84)	.18	(2.00)	.18	(2.23)
Cohort 6	.41	(4.04)	.57	(4.49)	.56	(5.42)
County variables						
Clark	-.17	(1.15)	-.14	(0.93)	-.13	(0.90)
Dane	.52	(0.49)	.07	(0.73)	.06	(0.66)
Dunn	-.35	(2.22)	-.32	(2.01)	-.30	(1.94)
Kewaunee	.10	(0.56)	.16	(0.91)	.13	(0.76)
Monroe	.13	(1.06)	.17	(1.36)	.18	(1.46)
Oneida	-.10	(0.78)	-.07	(0.51)	-.05	(0.41)
Ozaukee	-.12	(0.88)	-.07	(0.54)	-.06	(0.48)
Richland	-.07	(0.51)	-.03	(0.27)	-.05	(0.36)
Sheboygan	-.50	(4.27)	-.45	(3.91)	-.44	(3.81)
Winnebago	.23	(1.98)	.28	(2.44)	.29	(2.57)
Calumet	-.75	(5.29)	-.77	(5.43)	-.78	(5.49)
Dodge	-.34	(2.66)	-.34	(2.67)	-.32	(2.55)
Green	-.09	(0.63)	-.08	(0.59)	-.10	(0.76)
Jefferson	-.53	(4.00)	-.54	(4.14)	-.53	(4.13)
Juneau	-.45	(3.31)	-.41	(3.06)	-.40	(3.04)
Marathon	-.56	(4.48)	-.60	(4.79)	-.61	(4.91)
Price	-.30	(1.90)	-.33	(2.11)	-.32	(2.03)
Racine	-.14	(1.27)	-.08	(0.75)	-.07	(0.60)
St. Croix	.14	(1.11)	.09	(0.73)	.08	(0.68)
Intercept	1.47	(13.67)	1.45	(0.00)	1.51	(14.36)
Sigma	1.38	(37.59)	1.40	(37.67)	1.38	(37.77)
Log-Likelihood	-6236.7		-6235.7		-6227.1	

Note: Absolute values of t-statistics in parentheses.

Notes

[1]L. Bumpass, "Children and Marital Disruption: A Replication and Update," Demography 21 (February 1984): 71-82.

[2]For a description of the legal system of child support in general and a detailed discussion of the laws governing child support in out-of-wedlock birth cases see Harry O. Krause, Child Support in America: The Legal Perspective (Charlottesville, Va.: Michie, 1981).

[3]U.S. Bureau of the Census, Current Population Reports, Series P-23, no. 152, Child Support and Alimony, 1985 (Washington, D.C.: U.S. GPO, 1987).

[4]Irwin Garfinkel and Marygold Melli, Child Support: Weaknesses of the Old and Features of a Proposed New System. Vol. 1. Institute for Research on Poverty Special Report 32A, University of Wisconsin, Madison, 1982; L. Yee, "What Happens in Child Support Cases," Denver Law Journal, 57 (1979): 21-68.

[5]U.S. Bureau of the Census, Current Population Reports, Series P-23, no. 152, Child Support and Alimony, 1985.

[6]In Wisconsin, initial child support awards during the period 1981-1983 averaged 18 percent of the nonresident parent's income for one child, and 24 percent, 27 percent, and 29 percent of the nonresident parent's income for two, three, and four children. These percentages are almost identical to those called for in the Wisconsin percentage-of-income standard adopted statewide in 1987. (See text below.) In view of the fact that the Wisconsin standard is considered to be relatively

stringent, this finding suggests that in Wisconsin at least, the problem of low awards results not from the initial award being too low, but rather from the failure to increase the awards over time in response to increases in nonresident parents' income.

On the other hand, preliminary, unpublished results using data from the Current Population Survey-Child Support Supplement suggest that the average initial award nationally is substantially below the percentages called for in the Wisconsin standard. We have yet to resolve whether Wisconsin differs from the rest of the nation or whether the Wisconsin results are due to missing data.

[7]U.S. Bureau of the Census, Current Population Reports, Series P-23, no. 152, Child Support and Alimony, 1985.

[8]Irwin Garfinkel and Sara McLanahan, Single Mothers and Their Children: A New American Dilemma (Washington, D.C.: Urban Institute Press, 1986).

[9]David Chambers, Making Fathers Pay: The Enforcement of Child Support (Chicago: University of Chicago Press, 1979).

[10]Garfinkel and Melli, Child Support: Weaknesses of the Old and Features of a Proposed New System, Vol. 1.

[11]Irwin Garfinkel and Donald Oellerich, "Non-Custodial Fathers' Ability to Pay Child Support," Demography, forthcoming. The upper and lower figures in the range are derived by applying respectively the Wisconsin percentage-of-income standard and the Colorado child support guidelines. These are two the most widely used standards in the country.

[12]Tom McDonald, James Moran, and Irwin Garfinkel, "Wisconsin Study of Absent Fathers' Ability to Pay More Child Support," IRP Special Report 34, University of Wisconsin-Madison, 1983.

[13]Cases just entering the court system were collected in order to evaluate the effects of routine withholding from the start of the payment history. Routine withholding could also be used in cases returning to court, but the effects in this type of case are not evaluated here.

[14]Two control counties, Dodge and Juneau, implemented routine withholding before the end of case selection for cohort 6. They are treated as pilot counties for cohort 6 cases.

[15]This information is from the Survey of Children, Income, and Program Participation, conducted by the Institute for Research on Poverty in 1985.

[16]The extent of substitution in the pilot counties is not likely to have been large because, as described in the text below, withholding was not implemented in a large proportion of cases. It is likely that private direct payment cases were excepted. A telephone survey of resident and nonresident parents currently being conducted will provide additional information on the extent of direct child support payments.

[17]About 3 percent of the cases had legal direct payment agreements, and about 20 percent had no child support order in the entire case history. In 30 percent of the cases there was a change in legal custody, joint custody, or custody granted to someone other than the

parents. These cases were excluded because of possible changes in the payer.

[18]The income source information (and later income level information) is taken from the data for the final judgment in cases in which one had been issued. A final judgment is issued in divorce cases at the time of property division. Although there is often a temporary child support order prior to the final judgment, income information is often available only at the time of the final judgment. For cases with no final judgment, the income information from the first court action was used.

[19]There are also cases in which assignments are made at a later action in response to deliquency. These are excluded by looking only at assignments made at the time of the first order. Also, it appears that in some cases assignments are issued at the time of the first order, but are not immediately sent to the employers and therefore have no effective date. These may be activated later in the event of a deliquency in payment, or if an unemployed nonresident parent later secures a job. These are not included in our definition of routine withholding.

[20]For awards expressed as percentages, the exact dollar amount owed is unknown to us. For these cases, the amount owed is estimated as the percentage ordered in the last court action multiplied by the income amount stated in that action. If no income information is available, the case is dropped from this analysis.

Not all support orders mandate monthly payments (e.g., some use

weekly or biweekly payments). Monthly equivalents were calculated for these cases.

[21]The ratio of dollars paid-to-owed could be greater than 1 if the nonresident parent pays more than the ordered amount. However, taken as an average over the entire case life, it appears to be rare for the payments to exceed the amount owed. Therefore, the cases with high pay-to-owe ratios are likely to be data errors and are treated as censored at 100 percent by using tobit models with an upper point of truncation at 100. The monthly data used later have many cases with payments of more than the monthly amount owed, so there a single (lower) point of truncation is used.

[22]The three experimental variables were also included in a single regression on the ratio of dollars paid-to-owed, and one on months paid-to-owed. In those regressions, only the individual case withholding dummy coefficients were significant. The impact of withholding calculated from those regressions was slightly larger than in the regressions reported in the text.