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Abstract

In this paper I use individual level data from the 2001 panel of the Survey of Income and Program Participation (SIPP) to examine whether the strength of state child support enforcement efforts affects nonmarital birth or marriage rates. Evidence is mixed, but in the preferred specifications increased efforts at enforcement lead to a decrease in likelihood of marriage among never married childless women and a decrease in the annual likelihood of both a nonmarital birth and marriage among never married women with one child.

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INTRODUCTION

Recent research suggests that decisions affecting nonmarital fertility and marriage may be responsive to financial incentives.[1] This research and increased efforts at child support enforcement (CSE) during the 1990s raise the question of whether decision affecting fertility and marriage are responsive to strength of support enforcement efforts.

From the point of view of economic theory answers to these questions are not clear. For women, the higher expected collections resulting for increased efforts at CSE may provide incentives for (or facilitate) nonmarital childbearing and non-marriage. Conversely, increased efforts at CSE may lead men to take measures that reduce the likelihood of fathering children out of wedlock in an effort to avoid formal or informal support obligations. Strengthened efforts at CSE may also change the incentives facing men who are already, or are expecting to be, fathers in such a way as to reduce nonmarital births and increase marriage (and marital births) among women in absence of counter forces.[2] Because CSE efforts have apposing implications on the fertility and marriage incentives of women and men, and because fertility and/or marriage outcomes are the product of a joint decision making process, their net effects on fertility and marriage measures are ambiguous.

The weight of the evidence from the few studies that have examined the effects of CSE on nonmarital fertility and/or marriage directly suggests that increased efforts at CSE reduce nonmarital births and increase marital births (Acs and Nelson 2004, Case 1998, Garfinkel et al. 2003, Huang 2002). There are important policy implications of these findings, particularly when interpreted in the broader context of research examining the effects of policy on nonmarital childbearing and marriage more generally. Much of this related research has been focused on the effects of incentives provided by the AFDC/TANF (Aid to Families with Dependent Children or Temporary Assistance to Needy Families) program on nonmarital

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[1]See, for example Acs and Nelson (2004), Garfinkel et al. (2003), Horvath and Peters (1999), Huang (2002), Jagannathan and Cammaso (2003), and Jagannathan et al. (2004).

[2] Men who already have, or who are expecting, a child and are facing a support obligation may find it economically convenient to marry the mother of their child, depending on a variety of factors (economies of scale in family size, the size, nature and status of any current support orders and obligations, their incomes and those off their potential spouses, and geographic considerations.

births and marriage. Although it's difficult to characterize the results from such a large body of literature it is quite common for studies to report no statistically significant effects of AFDC/TANF benefits on measures of nonmarital childbearing, female headship, and marriage.[3] Taken together the research on the effects of CSE and AFDC/TANF benefits on nonmarital fertility suggests that altering the incentives toward nonmarital childbearing for women may not be as affective as providing strong deterrents for men to father children out of wedlock.

Although the available evidence hints that policies aimed at reducing nonmarital births would be more productive if directed at males, there have only been a handful of studies that have addressed the relationship between the strength of CSE efforts and nonmarital fertility. Furthermore, the data sources used in these studies are potentially problematic in that they cover a limited cross section of states over a limited time period (Acs and Nelson 2004), rely on aggregate state-level panel data (Garfinkel et al. 2003, Case 1998), or are based on a single cohort of women followed over time (Huang 2002).[4] Additionally, existing studies of the effect of CSE on fertility do not distinguish between higher order and first births (Acs and Nelson 2004; Garfinkel et al. 2003; Case 1998), or estimate the effect of CSE on first birth only (Huang 2002), despite the fact that unmarried women with children (and the fathers of their children) have a greater level of exposure to, and experience with, issues surrounding the sharing of parental responsibilities (financial or otherwise).

In this project I use individual-level data from the 2001 panel of the Survey of Income and Program Participation (SIPP) along with state-level measures of the strength of CSE, welfare rules, and economic conditions to assess whether CSE efforts have an effect on fertility and marriage among never married women. Using retrospective information available in the 2001 SIPP I construct fertility and marriage histories for two groups of women. The first group consists of never married women who turned 16 after 1988 and the second consists of never married women who gave birth to their first child after March 1988

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[3] See Moffitt (1998) for an extensive review of this literature.

[4] In studies that track women of the similar ages over the same time period it can be difficult to separate the affects of age from state-level variables with a strong trend (CSE measures for instance).

(and thus were at risk for a second birth after 1989). Both groups of women are tracked from the time they are at risk for a first or second birth until they have a nonmarital birth, they marry, reach the age of 45, or the end of the sample period (December 1999) is reached.

Using these histories I estimate the length of nonmarital birth intervals where nonmarital birth interval endings are modeled as a competing risk. Nonmarital birth intervals can end because a women has a nonmarital birth (thus beginning a new interval), or because she marries (and thus is no longer at risk for a nonmarital birth). Individual annual probabilities of birth and marriage are specified as a function a set of control variables and several measures of the strength of CSE efforts. Evidence is mixed, but in the preferred specifications increased efforts at enforcement lead to a decrease in likelihood of marriage among never married childless women and a decrease in the annual likelihood of both a nonmarital birth and marriage among never married women with one child.

The current study contributes to the existing literature in a number of important ways. First, the SIPP data used in this analysis allows for the construction of accurate fertility and marital histories for women of a variety of ages (in 2001). The availability of fertility histories on multiple cohorts of women negates the potential problem of confusing the effects of tougher CSE with reduced fertility brought about by ageing of the sample. Second, sample sizes in the SIPP are large enough to allow births and marriages among childless women to be modeled and estimated separately from births and marriages among women with children. This distinction may be important because childless women and women with children have a different level of exposure to the CSE system and face different constraints. Lastly, this study contributes to the literature by examining a more recent time period (1989 -1999) than prior studies.

The remainder of this paper proceeds in four sections. The next section provides some background on the evolution of child support and CSE efforts and reviews the prior literature on the effects of CSE and other policies on birth and marriage. The third section describes the data used in this analysis. The results are presented in the fourth section and section five concludes the paper.

BACKGROUND AND PRIOR LITERATURE

In 1975 Congress enacted the CSE and Paternity Establishment Program which authorized federal matching funds that states could use to assist in establishing paternity and child support orders, and in collecting support from noncustodial parents to offset welfare payments or to increase the resources available to single-parent families. Since its inception the program has undergone a series of changes designed to aid the process of finding noncustodial parents, establishing paternit8 and support orders, and in collecting on such support orders.

Changes in the child support program in 1984 mandated that administrative systems be set up by states to expedite the process of obtaining and enforcing child support orders, and gave state CSE agencies access to IRS data for the purpose of locating and verifying the income of noncustodial parents. The Family Support Act of 1988 contained a mandate that states attempt to establish paternity for all children under 18. To meet this mandate states were encouraged to set up administrative procedures for establishing paternity by genetic testing in cases where paternity was contested. As part of the 1996 welfare reform legislation, states were required to establish a database of new employees, and employers were required to provide the names and Social Security numbers of all new employees to the states and, by proxy, to a national new employee database. This database can be used to locate noncustodial parents for the establishment of paternity or enforcement of a support order. When a noncustodial parent who is delinquent in child support payments is located using this system, employers are immediately instructed to begin withholding child support from the parent's wages.[5]

The effect of these and other changes in CSE practices can be seen in Figure 1, which plots three measures of the CSE over time. The first measure is the number of female headed families with child support collections in the March Current Population Survey (CPS), divided by the total number of female

[5]There are exceptions to this immediate withholding. If the noncustodial parent can show good cause, withholding may be delayed indefinitely. If the custodial and noncustodial parent can reach a suitable arrangement, immediate holding may also be delayed.

headed families in the CPS.[6] The second measure is the number of Office of Child Support Enforcement (OCSE) AFDC/TANF cases with collections in a year, divided by the average monthly AFDC/TANF caseload. The last measure is an index of CSE enforcement constructed by Huang, Garfinkel, and Waldfogel (2004).[7] This index is computed as the normalized sum of a legislative CSE index and four measures of CSE computed from the universe of never-married mothers in the CPS.[8] All of the CSE measures increase over time, but the AFDC/TANF collection rate increases more, particularly after 1998. From the early 1990s to 2000 the AFDC/TANF collection rate more than doubled. Although limited in coverage to AFDC/TANF child support cases, this rate is probably more accurate than alternative administrative collection rates, and is thought to be *broadly* consistent with overall collection rates. All three measures are comprehensive in that they reflect the evolution of combined efforts to locate non-custodial parents, establish paternity and support orders, and increase collections.

In response to increasing efforts at child support enforcement a literature dealing directly with the question of whether or not CSE has an effect on fertility and marriage was spawned. Studies addressing this issue vary in terms of the time periods covered as well as the types of data and measures of CSE utilized. Several studies make use of aggregate state-level panel data to address the question of whether CSE, as measured by collection rates, paternity establishment rates, and/or the existence of various CSE laws, have an impact on state nonmarital birth rates (Case 1998, Garfinkel et al. 2003). These studies control for state demographics, maximum AFDC/TANF benefits, and other aspects of the policy environment affecting decisions about childbearing. In addition to these controls, they typically include state fixed- and year-effects.

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[6] Freeman and Waldfolgel (2001) estimate that 27 percent of the increase in the fraction of never married CPS mothers with child support collects between 1981 and 19985 result from increased CSE expenditures and that expenditures were more effective in increasing collection rates when paired with CSE legislation.

[7] This particular the child support enforcement index is defined as index F in Huang, Garfinkel, and Waldfogel (2004). It is one of 7 indices that they compute, but seems to be a more appropriate for this application as the measures that go into the index are computed over the range of all women, as apposed to just welfare recipients.

[8] The CPS measures used to construct the child support enforcement index are the percent off mothers in the CPS with child support paid, the ratio of child support collections to child support guidelines from the CPS, and the average child support payment divided by the maximum AFDC/TANF benefit.

One study that uses this approach was conducted by Case (1998). Using state-level panel data that spanned the period 1979 to 1991, Case finds that some state-level child support policies do appear to have an impact on nonmarital birth rates. For example, in her preferred specifications the presence of laws allowing for genetic testing to establish paternity, paternity establishment until the child is 18 years old, and presumptive guidelines all have negative and statistically significant effects on nonmarital birth rates.

One interesting feature of Case's study is that it takes very seriously the notion that CSE policy and AFDC benefit levels are determined endogenously with nonmarital birth rates. Case suggests that state policy makers affect nonmarital childbearing by choosing the level of AFDC benefit levels. If their adoption of CSE laws is dependent on the chosen of level AFDC benefits, CSE laws and nonmarital fertility will be correlated in a way not indicative of a causal relationship. Because of this potential endogeneity, she uses information on the size and sex composition of state legislatures to instrument specific CSE provisions in her preferred specifications. The coefficients on the instrumented CSE provisions tend to be negative, while the coefficients on the non-instrumented CSE provisions show no clear pattern.

In a similar study Garfinkel et al. (2003) use state-level panel data spanning the period 1980 to 1997 to examine the effect of CSE on nonmarital birth rates. This study differs from Case's in that it covers a longer, more recent, time period and uses a measure of CSE efficacy as opposed to individual legislative indicators. In particular, the authors' preferred measure of CSE is the natural log of the product of the paternity establishment rate and average child support collected per AFDC mother. Using this measure, they find that CSE efforts reduce the nonmarital birth rate. These results are robust to some iterations on the CSE measure, most of which involve the product of the paternity establishment rate and either the AFDC/TANF child support collection rate or the average payment per mother receiving AFDC/TANF.

One potentially shortcoming of both the Case and Garfinkel et al. studies is that they rely on aggregate state-level data and variation within states in CSE policies or measures to identify the effect of support enforcement. The difficulty with this type of data and approach is that there is an increased risk that the results are, at least in part, driven by spurious correlation between nonmarital birth rates and CSE measures or provisions. This concern is reduced in studies that use individual level data to examine whether increases CSE reduces nonmarital fertility, but to date there have only been several such studies.

One study that uses individual level data was conducted by Huang (2002). Huang uses individual level data from the 1979 through 1998 waves of the National Longitudinal Survey of Youth (NLSY79) to examine the link between CSE and both marital and nonmarital fertility. He starts with a sample of girls and women ranging in age from 14 to 22 in 1979 and follows them until they have a birth or all information on them is exhausted, either because of attrition or because the end of the sample period is reached. A multinomial logit model is used to estimate the likelihood of a marital birth or a nonmarital birth in each year in which the sample respondents are observed. The primary measures of CSE used in this analysis are a legislative index and CSE expenditures. Both of these measures enter as independent variables in the birth rate specifications. Additionally, their interaction is also included in the preferred specification.[9]

Huang finds the interaction between the legislative index and CSE expenditures has a statistically significant effect on the likelihood of a nonmarital birth (relative to no birth), the likelihood of a marital birth (relative to no birth), and the likelihood of a nonmarital birth (relative to a marital birth). The interaction of the legislative index and CSE expenditures decreases nonmarital births (relative to no births), increases marital births (relative to no births), and decreases the likelihood that a women will have a nonmarital birth (relative to a marital birth). In subgroup analysis, Huang finds that the legislative index - CSE expenditures interaction has differential effects by age and race. In particular, older women appear to be more affected by this CSE measure. Additionally, the primary effect of strengthening support enforcement on whites is to increase marital births relative to nonmarital births, whereas the primary effect on blacks is to decrease nonmarital births.

In another recent paper Acs and Nelson (2004) use individual level data from the 1997 and 1999 National Survey of America's Families (NSAF) and a difference-in-difference-in-difference estimation to

[9] Freeman and Waldfolgel (2001) provide evidence that there are complementarities between having CSE laws and in place and expenditures used to enforce those laws. In particular, they find that expenditures on CSE are more effective when CSE legislation is in place.

examine the effects of CSE (and other policy tools) on the living arrangements of low income families. Using medium income families as a comparison group, they find that higher child support collection rates are associated with a decreases in the relative incidence of single parent families and an increase in the relative incidence increases in two parent low-income families.

Taken together the studies by Case (1998), Garfinkel et al. (2003), Huang (2002), and Acs and Nelson (2004) suggest that tougher CSE leads to decreases in nonmarital births. Thus, the existing research supports the notion that tough CSE alters the sexual behavior of men so as to reduce the likelihood of nonmarital births, and that this “deterrent effect” dominates the increased incentives toward nonmarital childbearing provided to women under tougher enforcement regimes.

Even though the existing studies in the literature are in more or less agreement about the effect of CSE on nonmarital fertility, none of them provide particularly compelling evidence on their own. For example, the Case’s study uses a law by law approach whereby each law impacts on nonmarital fertility individually without any consideration to the cumulative impact of multiple provisions. Not only does this approach negate the impact that having multiple provisions in place might have, but it also ignores states’ effectiveness in implementing and enforcing such provisions.

Later studies used improve measures of CSE, but there is no consistency of measures across studies. Additionally, the later studies that use individual level data are limited in scope. Huang’s study is limited in that it only examines first birth outcomes of a single cohort of childless women who were 14 to 22-years old in 1979. The potential problem with this approach is that CSE efforts were be strengthened at the same time the women in the sample were aging and thus at reduced likelihood of having a birth. It is quite possible the negative effects of enforcement on nonmarital births reported by Huang reflect aging of the sample rather than increased efforts at CSE. Lastly the results generated by Acs and Nelson are based on data from only 13 states over a two year period. Their results are also taken from a model that uses moderate income families as a control group. As moderate income families are potentially affected by more stringent CSE, their use as a control may not be appropriate.

Research on the effects of CSE efforts on nonmarital fertility fits into broader body of literature that examines the effects on policy on nonmarital childbearing, marriage, and/or female headship. Much of this research of this literature has been focused on the question of whether incentives associated with the AFDC program impact marital status, nonmarital fertility, female headship status, or abortion.[10,11] Other research focuses on the impact of welfare reform provisions (particularly the family cap) on fertility decisions (Camasso et al. 1998a and 1998b; Turturro et al. 1998; Horvath and Peters 1999; Jagannathan and Camasso 2003; Acs and Nelson 2004; Jagannathan et al. 2004; Joyce et al. 2004; Kearney 2004; Levine 2002). While it is difficult to summarize results from such a large number of studies, a careful reading of the available evidence suggests policy plays a limited role in decisions affecting nonmarital fertility and marriage. Studies of the effect of AFDC benefit levels on nonmarital fertility, marriage, and female headship generally find no significant effects or effects that are hard to reconcile using a strict rational choice framework[12] and a consensus has yet to emerge on the effects welfare reform on fertility.

The whole of the research on the effects of policy on nonmarital fertility and marriage suggest that policy tools that operate through the incentives of women do not have a strong effect on fertility and marriage, but that strengthened CSE efforts (which affect male and female decision making) may. In remainder of this paper I address the question of whether of tougher CSE efforts at the state level affect nonmarital fertility and marriage using event history data constructed from the 2001 SIPP. The next section describes the data.

DATA AND METHODS

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[10] Moffitt (1998) provides a thorough review of studies examining the impact of AFDC benefit levels on marriage, nonmarital fertility, and female headship.

[11] See Klerman (1995) for a thorough review of the literature on the effects of the AFDC program on abortion

[12] A common finding is that higher AFDC benefits are associated with increased marriage and reduced fertility among black women. On explanation for this finding is that the in states with high AFDC benefits there are cultural factors that lead to reduced nonmarital fertility and increased marriage among black women. In particular, it has been noted that high AFDC benefit states tend to have a low fraction of blacks in their population. If black women in these conform their fertility and marriage behavior to that of the majority group, it may look as though AFDC benefits reduce fertility and increase marriage, when fertility rates for blacks are higher than they would be in absence of lower AFDC benefits.

The SIPP are a series of overlapping longitudinal data sets published by the United States Census Bureau. Once every four months (wave) SIPP participants are asked about their income, earnings, and program participation over each of the previous four months. SIPP panels are designed to gather information at high frequency, but not over extended periods of time. In fact, the 2001 SIPP panel was only designed to gather monthly information on respondents' income, earnings, and program participation over a 36 month period beginning in late-2000 and early-2001. In addition to the standard questionnaire that is administered every 4 months, a separate topical questionnaire (module) is administered for each wave of every SIPP panel. These topical modules are focused on obtaining retrospective or more detailed information about particular areas of interest to researchers.

In this paper I make use the second wave topical module file (W2TMF) and the second wave core data file (W2CDF) of the 2001 panel of the SIPP. The W2CDF provides information on race and Hispanic origin, year of birth and month of birth, as well as residential location (state of residence and metro status) for each member of the sample. The second topical module file (W2TMF) contains information on marriage, fertility, migration, education and work disability histories, as well as information on family relationships.

Relative to other sources of individual level data that have been used to examine the impact of CSE enforcement on fertility, the SIPP data offers three distinct advantages. First, because the SIPP contains retrospective information, marriage and fertility histories can be constructed for women of a variety of ages (in 2001), thus avoiding the problem of confounding the effects age and other variables that are trending over time. Secondly, sample sizes in the SIPP are large enough to allow for models to be estimated separately by parity level. Lastly, the 2001 SIPP can be used to construct fertility and marriage histories that cover a more recent time period that alternative data sources.

To model the impact of CSE on nonmartial fertility I specifying individual birth and marriage rates as functions of individual-level demographic controls, state-level control variables, and state-level measures of CSE. The appropriate type of data for this empirical approach is a sample of women who start to be at risk for a nonmarital birth over a given interval of time, and are then tracked from the first period they are at risk for a nonmarital birth until they have a nonmarital birth, marry, or information on their birth and marital histories is exhausted..

The first step in constructing this data was identifying a set of women who were at risk for a nonmarital birth over the period of time for which adequate measures of CSE are available.[13] By assumption, a woman is at risk for a first birth beginning at age 16 and at risk for a second birth 9 months after her first. Relying on information the ages of each women's first and last born child and marital histories (from the W2TMF), I identified all black, white or Hispanic women who were risk for a first or second birth between January 1989 and December 1999,and who were also never married during first month they were at risk for a birth.[14] These women were then tracked from the first month they were at risk for a birth until they experienced a nonmarital birth, married, reached the end of the sample period (December 1999), or reached the age of 45.[15,16]

[13] Prior to the 1988 child support and alimony received were grouped together in the CPS income categorizations. For this reason it is impossible to separate child support payments for alimony in the CPS prior to 1987 (1987 because the CPS income questions refer to last year). Additionally, the CSE index is only available through 1999.

[14] The set of women at risk for a first or second nonmarital birth between January 1989 and December 1999 is composed of two overlapping groups of women. The first group is all black, white, and Hispanic women under the age of under the age of 29 (in December 2000) who were childless and never married at age 16. These women are at risk for a first nonmarital birth for at least some portion of the sample period and may be at risk for a second nonmarital birth, depending on their fertility and marriage histories. The second group is all white, black, and Hispanic women under the age of 46 (in December 2000) who had a first birth after March 1988 and before February 1998 and who remained never married for at least 9 months following their first birth. These women are at risk for a second birth for at least some portion of the sample period.

[15] Exact marriage dates cannot be determined from the W2TMF. To protect the privacy of survey respondents the public use version W2TMF only includes the year of first marriage. In constructing the data used in this analysis I assigned women who were married in a particular year the earliest possible marriage data. So, for example, if the year a respondent was first married is recorded as 1990, I assign her a marriage date of January 1990. The implication of this is that a small number of nonmarital birth intervals that end with a nonmarital birth will be coded as ending in marriage.

[16] In pinpointing the precise timing of births I rely on the information on each child's month and year of birth contained in the W2CDF. In cases where the W2TMF indicates that all children are living with their mother, the W2CDF should contain a record for each of the mother's children that contains the child's year and month of birth. The birth years and months obtained from the W2CDF were checked against the information on the birth year of first and last child contained in the W2TMF (where applicable) to eliminate cases where birth intervals started before 1988 or after 1999, cases with twins, and cases where there are apparent inconsistencies between the W2CDF and the W2TMF. Only a small number of inconsistencies where noted.

Including only women who meet the criteria outlined above leaves a sample of 4,962 women spanning over 250,000 person months. An additional 578 women were eliminated from the sample because they resided in states not well represented in the sample or because they moved to the current state of residence in the course of a birth interval.[17,18] Due to the nature of the sample design I do not observe first or second nonmarital inter-birth intervals for all 4,384 women in the sample.[19] Rather, I observe first birth intervals for 3,956 women who turned 16 between January 1988 and December 1999 and second birth intervals for 847 women who became eligible for a second birth during the same period. There are 419 women for whom I observe complete first nonmarital birth intervals and at least part of their second birth nonmarital birth interval.

Although the SIPP data provide enough information to construct monthly fertility histories, measures of CSE are only available at calendar year frequency.[20] For this reason, the monthly fertility and marriage histories coded by the process described above are aggregated to create annual data. Because women with censored inter-birth intervals shorter than 12 months are not at risk for a birth or marriage for an entire calendar year, they are dropped from the sample. After all exclusions I am left with a sample of 3,955 women; 3,546 of which have completed or right censored first birth intervals, 761 of which have completed or right censored second birth intervals, and 352 of which have both.

Failures, censorings, and Kaplan-Meier birth and marriage rates and are shown by parity level for the annualized data in Table 1. Upon inspection of Table 1 a number of features of the data are apparent. First, there are a large number of right censored first and second birth intervals. This characteristic of the

[17] Women from 36 states are represented in the sample.

[18] For women that moved to their current state of residence during the course of a birth interval it is impossible to precisely determine their state of residence prior to the move. Because their state of residence cannot be determined in each month of their birth interval, the value of variables measured at the state-level cannot be determined during the entire birth interval. For this reason respondents moved between states during a birth interval are dropped from the sample.

[19]A birth interval is defined as the length of time starting when a woman becomes eligible for a birth (at age 16 in the case of a first birth interval, or 9 months after her first birth in the case of a second birth interval) and ending with her next birth. This is not the standard definition of birth intervals used by demographers. In the demography literature a birth interval is typically defined as the length of time between two births.

[20] Having some of the state level variables vary inter-month while the CSE variables only vary between calendar years may lead to bias coefficient estimates.

data is a direct result of the sample selection criteria outlined above. The longest that a sample member can be followed for is 11 years (1989 to 1999). Because individuals can be followed for a maximum of 11 years and birth and marriage rates are relatively low, many birth intervals are not observed as having endings. As these censored cases are not caused by attrition, or some other factor potentially correlated with the birth and marriage process, they should not lead to biased estimates.

A second feature of the birth interval data shown in Table 1 is that the processes determining first and second nonmarital births are very different. There are a number of dimensions to these differences. First nonmarital birth rates are much lower than second nonmarital birth rates corresponding to the same duration. In addition to the difference between first and second nonmarital birth rates, there are also notable differences in patterns of duration dependence across birth intervals. For example, marriage rates for never married childless women increase with birth interval duration, whereas marriage rates for never married women with children are constant or decreasing. Because the processes determining the length of first and second nonmarital birth intervals are different, they are treated separately in the empirical analysis that follows.

Sample means and standard deviations of the explanatory variables used in this analysis are shown in Table 1.[21] These variables can be divided into three groups: individual-level control variable, state-level control variables, and state-level CSE measures. Time varying variables are indicated in Table 1 with *t* subscripts and are computed as the average monthly value of the variable of interest over the 12 months covered by each person-year observation. All of the state-level time varying variables are lagged 10-months to account for the fact the results of decisions affecting fertility are observed with a lag.

Individual-level control variables

The individual-level control variables are designed to control for personal characteristics that affect birth and marriage probabilities among never married women so as to improve the efficiency of estimates of the

[21] The means and standard deviations shown were computed using person weights from the Wave 2 of the 20001 SIPP. These weights are designed to allow researchers to produce estimates that are unbiased for the 2001 US population.

effect of CSE on nonmarital fertility and marriage. The SIPP contains a basic set of background variables that describe each respondent's age, race-ethnicity, residential location, and educational attainment. These individual controls are each described in greater detail in the following list:

- *Age* – is the average age in years over the months corresponding to each year of data.
- *Metro resident* – a dummy variable indicating that the individual resided in a metropolitan area at the time Wave 2 of the 2001 SIPP was administered (late-2000 to early-2001).
- *White* – a dummy variable which is 1 if the respondent is white (non-Hispanic) and 0 otherwise.
- *Black* – a dummy variable which is 1 if the respondent is black (non-Hispanic) and 0 otherwise.
- *Hispanic* – a dummy variable which is 1 if the respondent is Hispanic and 0 otherwise.
- *Teen mom* – a dummy variable which is 1 if the respondent had a first birth prior to the age of 18 and 0 otherwise.[22]
- *Less than high school* – a dummy variable that is 1 if the respondent does not have a high school diploma or equivalent – Constructed from education histories in the W2TMF.
- *High school graduate* – a dummy variable that is 1 if the respondent has a high school diploma or equivalent and 0 otherwise – Constructed from education histories in the W2TMF.
- *Some college* – a dummy variable that is equal to 1 if the respondent's has some post high school training and 0 otherwise - Constructed from education histories in the W2TMF.

State level control variables

The state level control are included to provide an accounting for changes in the policy environment and labor market at the state level which may influence decisions affecting nonmarital fertility. Failure to include such variables in the model may lead to biased estimated of the CSE effects. Indicator variables for whether a state had a family cap or other major welfare reform waiver in place prior to implementing its

[22] As teen mother status is a transformation of the outcome of a first birth interval, it is excluded from first birth interval specifications.

Temporary Assistance to Needy Families (TANF) program make up the state level policy controls.[23] State level economic controls include the state unemployment rate and a wage measure. A more detailed description of each variable follows:

- *Family cap* – The annual average a monthly dummy variable that is equal to 1 if the state in which a respondent resides has a policy in place that limits the incremental increase AFDC/TANF benefits traditionally available when a welfare recipient has an additional child and 0 otherwise. [24]
- *Other Waiver* – The annual average of a monthly dummy variable that is equal to 1 if the state in which a respondent resides has a pre-PRWORA welfare reform waiver in place other than a family cap and 0 otherwise.
- *25th Percentile of hourly wages* - the 25th percentile of hourly wages for full-time, full-year workers ages 18 to 64 in 2000 dollars - Computed from the Current Population Survey Outgoing Rotation Group files.
- *Unemployment rate* – The annual average of the state monthly unemployment rate.

CSE measures

The CSE index, the AFDC/TANF collection rate, and the CPS collection rate are comprehensive measures of the strength of CSE efforts at the state level. To varying degrees they will reflect the CSE laws that are in place in each state as well as the strength of efforts to enforce these laws. They were described briefly, in the discussion of Figure 1.

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[23] Note that measures of welfare program generosity and a Temporary Assistance to Needy Families program implementation indicator are not included in the state level policy controls. These variables were excluded because they are highly collinear with other variables that are included in the model. In particular, the state and year effects (included in the model) account for over 95 percent of the sample variance in the maximum AFDC/TANF benefits for a three person family and the 1997, 1998, and 1999 year effects account for over nearly 85 percent of the sample variation in real maximum AFDC TANF benefits for a 3 person family. Additionally, because virtually all states implemented their TANF programs in 1997 or 1998, the year effects are also highly collinear the TANF indicator. Because of the high degree

[24]Information on welfare reform measures was pieced together from a variety of sources, most notably the Department of Health and Human Services Assistant Secretary for Planning and Evaluation website, the State Policy Demonstration website, and the Urban Institute's Welfare Rules Database.

The first measure of CSE is the CSE index. Recall that this index was constructed as by Huang, Garfinkel, and Waldfogel (2004) as the normalized sum of a legislative index and 4 CPS-based measures of CSE. The legislative index used in this measure records the number of 8 types of laws covering paternity establishment, the establishment of support orders, and collections on established support orders. The legislative index includes laws covering genetic testing, paternity establishment, numerical guidelines, presumptive guidelines, wage withholdings under delinquency, immediate wage withholdings for new cases, universal wage withholdings, and state income tax fund interception (Huang, Garfinkel, and Waldfolgel, 2004). The CPS measures that go into the CSE index are the percent of mothers with child support received, the ratio of child support collections to child support guidelines, and the average support payment divided by the maximum AFDC/TANF benefit.[25] So that the CSE index is scaled similarly to the other CSE measures, the index originally was multiplied by a factor of 100.

The AFDC/TANF collection rate is defined as the number of Office of Child Support Enforcment (OCSE) current assistance cases with collections divided by the number of families receiving AFDC/TANF. For this particular application a more appropriate measure would be the number of OCSE cases with collections divided by the OCSE caseload. Unfortunately, accurate and reliable information on the total number of OCSE cases with collections is not available over time as states were only required to keep accurate records AFDC/TANF child support cases.

Because the AFDC/TANF collection rate is constructed from administrative data it is not subject to sampling error. It is, however, subject to potential reporting error. The most serious concern with this measure of CSE is that because of rapid changes in AFDC/TANF caseloads in the 1990s there was a disjoint between what the OCSE considered AFDC/TANF cases and the actual caseload. In particular, state child support agencies may not have been able to keep up with rapid caseload declines of the late-1990s and some

[25] This child support enforcement index used in this paper was one of 7 that were constructed by Huang, Garginkel, and Waldfogel in their analysis of the effect of child support enforcement on welfare caseloads. It was selected for this analysis because the measures used to construct the index are obtained over samples of all non-married women as apposed to just welfare recipients. Relative to other measures of child support enforcement used by Huang, Garfinkel, and Waldfogel, the coefficients on the CSE indices were much more consistent across specifications.

OCSE cases which were no longer AFDC/TANF cases may have been included in the AFDC/TANF cases with collections count (Guyer, Miller, and Garfinkel 1996). If these former cases were disproportionately more likely to have child support collections, as might be expected if collections are associated with leaving welfare, the rise in collections in the late-1990s may be over-stated. This potential reporting error may be one reason why the *AFDC/TANF collection rate* increased more rapidly in the late-1990s than the other measures of CSE.[26]

The *CPS collection rate* is computed as a 3-year moving average the number female (primary) family heads (w/ children) with reported income from child support divided by the number of female headed families (w/children) in the CPS. This measure has the advantage of reflecting collections across all single mothers. The drawback to this measure is that it is an estimate, and thus subject to a degree of measurement error. The degree of measurement error in the CPS collection rate is substantial. In 1999, 95 percent confidence intervals for the true child support collection rates, based on the CPS estimate, ranged from ± 0.03 to ± 0.12 for the states included in the sample. Because of this high degree of measurement error, the CPS collection rate is not the preferred measure of CSE.

RESULTS

Relative odds of a birth and marriage (relative to no birth and no marriage) for never married black, white, and Hispanic women estimated using a multinomial logit model are shown in Table 3 (women with no children at baseline) and Table 4 (women with one child at baseline). For each parity level, the results of 3 separate models are shown. These models differ according to which CSE measure is used. The CSE index is used to measure CSE efforts at the state level in model (1). Model (2) uses the fraction of AFDC/TANF collection rate while model (3) substitutes the CPS collection rate. In addition to the variables shown in Tables 3 and 4, all specifications include age-duration, year, and state effects.

Examining Table 3 it is clear that the individual level control variables are important determinants of the first nonmarital births and marriage. It is also apparent that aside from the CSE measures there is very

[26] See Figure 1.

little difference in the magnitude or statistical significance of the effects reported across the models. In all the models being black (relative to white and Hispanic) substantially increases the relative likelihood of having a nonmarital birth. Both marriage and birth probabilities are significantly reduced for women who have at least a high school diploma, as compared to women who have not finished high school. Women who live in metro areas also have lower birth and marriage rates than otherwise observational equivalent women. Neither family cap implementation nor implementation of a pre-PRWORA waiver other than a family cap has a measurable impact on the relative odds of a birth or marriage among never married childless women. The odds ratios associated with the state level economic controls suggest that marriage rates among never married childless women are highly countercyclical, rising in times of high unemployment and low wages and falling in times of low unemployment and high wages.

Both the CSE index and the fraction of OCSE AFDC/TANF cases with collections have negative and statistically significant effects on the relative odds of marriage. There is no evidence the strength of state CSE affects the relative risk of a birth among never married child women. While both the AFDC/TANF collection rate and the CPS enforcement rate have a large estimated effect on the relative odds of a first bith, the standard error estimates are also very high. In the case of the CPS enforcement these high standard error estimates are surely attributable to measurement error.

The results with respect to the CSE variables are somewhat surprising. Never married childless women have no direct exposure to child support. What knowledge of the child support system these women have must come from friends, relatives, associates, or the popular media. It is a stretch to think that their limited knowledge concerning the strength of state CSE efforts would affect their relative odds of marriage, but not their relative odds of a nonmarital birth. It is also difficult to imagine that CSE efforts would dramatically reduce incentives for marriage among men.

One possible explanation for the strong negative effect of the CSE index and AFDC/TANF collection rate on relative risk of marriage is that effective CSE efforts reduce marriage among women who are already pregnant. Relative to couples that are not anticipating the birth of a child, it is more reasonable to think that expecting couples would be more in tune the state CSE efforts, and that the implications of these efforts would be considered in deciding whether to marriage. If this hypothesis is correct, we might expect that CSE efforts would also reduce the relative odds of marriage following a nonmarital first birth.

For a partial test of this hypothesis I turn next to the estimates of the determinants of birth and marriage among never married women with one child in Table 4.[27] The Table 4 specifications are equivalent to those in Table 3 with several exceptions. First, because age and birth interval duration are not equivalent for second birth intervals, the Table 4 specifications include age. Also included in the Table 4 specification is an indicator of whether a respondent had her first birth before the age of 18.

As with the results shown in Table 3, differences across models in the magnitude of effect of the control variables are small. In all of the specifications age has a negative and statistically significant on the relative risk of a nonmarital birth. Race and Hispanic origin do not have statistically significant effects relative odds of a nonmarital birth , but being black substantially reduces the relative odds of a marriage among never married women with one child. Women who have at least a high school diploma have substantial lower relative risk of a nonmarital birth as compared to women without a high school diploma. Having completed high school is associated with higher relative risk of marriage among never married women with one child, but these effects are not statistically significant. The relative odds of marriage are are highly countercyclical. The state-level policy controls (family cap and other waiver) do not have statistically significant impacts on relative odds of a birth or marriage.

The only statistically significant CSE effects in Table 4 are associated with the CSE index. The CSE index has negative and statistically significant effect on the relative odds of birth and marriage among never married women with one child. The magnitude of these effects is such that a one point increase in the

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[27] While estimates of the models over never married women with one child will provide some insights into the plausibility of this hypothesis, they will not paint a complete picture. These estimates will not paint a complete picture because women who marry in the first nine months following their first birth are not eligible for second a nonmarital birth, and thus do not appear in the set of women who have second non-marital birth intervals. Using the existing empirical framework there is no way to address the question of whether CSE efforts lead to marriages in the first 9 months following a nonmarital birth It is possible to code a dependent variable which is equal to 1 if the respondent married in the 9 months following their first nonmarital birth (0 otherwise) and to regress this dependent variable (via binomial logit or probit) on the individual controls, the state level controls and the CSE measures. Such model essentially static, and thus cannot contain state effects. Because state effects cannot be included in the model, the coefficients will be identified using different sources of variation than coefficients in the Table 3 and Table 4 and the results will not be comparable.

CSE index would decrease the odds of a birth (relative to no birh no marriage) by about 50 percent and odds of marriage (relative no birth no marriage) by about 35 percent. While none of the other CSE measures are statistically significant, the odd ratios associated with the AFDC/TANF collection rate are at least consistent with the Model I results in that they indicate a negative effect on the relative odds of marriage. Consistent with the Table 3 estimates, the odds ratios associated with the CPS collection rate are imprecisely estimated. Again this is almost certainly due to the substantial degree of measurement error associated with this CSE measure.

A number of alternative specifications (not shown) were estimated to assess the robustness of the results in Tables 3 and 4. Included in these alternatives were specifications that excluded fixed state effects and specifications that made use of alternative sets of state-level controls.[28] While removing the state effects had profound effects on all of the variables measured at state-level, none of the other alternative specifications yielded results that were appreciably different than those presented in Tables 3 and 4. Even though the specifications run without state effects produced estimates of the coefficients on the child support variables that were different from those shown in Tables 3 and 4, they were no more consistent across the various child support measures. Furthermore, these specifications were deemed inferior to those with state effects as they provide no controls for unmeasured factors that vary across states and may be correlated with CSE or the other policy variables.

CONCLUSIONS

In this paper I used data from the SIPP to examine whether the strength of child support enforcement efforts at the state level have any measurable effects on the likelihood of a nonmarital births or marriage among never married women. Tough child support enforcement raises the likelihood that women will be able to establish and collect on a child support order in the event of a nonmarital birth. All else equal this

[28] For example, I estimated models that included an indicator for TANF implementation, a measure of state welfare program generosity, and alternative wage measures (the 50th percentile of male and female wages). These variables were included from the Table 3 and 4 models because they are highly collinear with each other (male and female median wages) or highly collinear with variables that are already in the model, making it difficult to interpret the change in odd ratios associated for these variables and the measures they are correlated with. As these excluded variables are highly correlated with variables included in the Table 3 and 4 models, nothing is gained by their inclusion.

“incentive effect” would lead to an increase in nonmarital births. Aggressive child support enforcement efforts may also lead to an increase in marriage as men may find that it is less expensive or more efficient to marry the mother of their child rather than to pay support for a non-custodial child. On the other hand increased threat of child support payments that accompanies stringent enforcement efforts may cause men to abstain from sex or be more vigilant in their use of contraceptives. This “deterrent effect” would lead to reduced nonmarital births, but would have an ambiguous effect on marriage.

From the SIPP data I created 2 sub-samples. The first sub-sample tracts women from the time they turn 16 until they have a nonmarital birth, they marry, or 8-years elapse. The second sub-sample tracts women from 9-months after their first birth until they have second nonmarital birth, marry, reach the ages of 45, or 8-years elapse. For each sub-sample I estimate nonmarital birth and marriage rates as a function of basic demographic variables (age, education, race, metro status), various measures of child support enforcement, welfare reform indicators, state unemployment rates, and birth-interval duration, year, month, and state dummy variables.

Estimates from these models indicate that race (origin) and education level are important determinants of nonmarital births and first marriage. There is some evidence that child support affects nonmarital fertility and marriage. Both the CSE index and fraction of OCSE AFDC/TANF cases with collections have statistically significant and negative effect on the risk marriage, relative to no birth and no marriage, among childless women. Additionally, the CSE index had a negative and statistically significant effect on the relative risks of birth and marriage among never married 1women with one child. These estimates are largely consistent with results reported elsewhere in the literature that suggests increased efforts at child support may be effective in reducing nonmarital fertility.

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Table 1. Birth Interval Endings, Censorings, and Kaplan-Meier Birth and Marriage Rates

First birth interval

Year	Beginning period cases	Births	Marriages	Censorings	Nonmarital birth rate	Marriage rate
1	3,546	63	36	403	0.02	0.01
2	3,044	95	79	342	0.03	0.03
3	2,528	101	94	320	0.04	0.04
4	2,013	89	88	278	0.04	0.04
5	1,558	77	97	216	0.05	0.06
6	1,168	59	86	219	0.05	0.07
7	804	29	82	166	0.04	0.10
8	527	16	64	145	0.03	0.12
9	302	6	30	112	0.02	0.10
10	154	7	24	120	0.05	0.16
11	3	2	1	0	0.67	0.33

Second birth interval

Year	Beginning period cases	Births	Marriages	Censorings	Nonmarital birth rate	Marriage rate
1	761	80	72	65	0.11	0.09
2	544	79	52	55	0.15	0.10
3	358	48	19	34	0.13	0.05
4	257	28	23	32	0.11	0.09
5	174	16	11	33	0.09	0.06
6	114	15	12	13	0.13	0.11
7	74	5	5	17	0.07	0.07
8	47	4	2	14	0.09	0.04
9	27	1	2	13	0.04	0.07
10	11	1	0	10	0.09	0.00

Table 2. Sample Means and Standard Deviations by Subsample

Variable	Never married women with no children (N=3546, N*T=15,188)	Never married women with 1 child (N=761, N*T=2,329)
Individual level controls		
Age	16.4583 ——	23.0789 (4.8787)
Metro resident	0.7970 (0.4023)	0.7888 (0.4084)
White	0.6973 (0.4595)	0.4544 (0.4982)
Black	0.1475 (0.3547)	0.3841 (0.4867)
Hispanic	0.1551 (0.3620)	0.1615 (0.3683)
Less than high school	0.3969 (0.4883)	0.3016 (0.4512)
High school graduate	0.5980 (0.4882)	0.3977 (0.4773
Some College	0.0050 (0.0625)	0.3006 (0.4537)
State-level controls		
Family cap (=1)	0.1418 (0.3364)	0.1309 (0.3270)
Other pre-PRWORA waivere	0.1680 (0.3448)	0.1603 (0.3431)
Unemployment rate	5.9079 (1.4393)	5.9777 (1.3808)
25th percentile of hourly wages	9.3798 (0.9288)	9.3576 (0.9700)
Child support enforcement measures		
CSE index	0.2241 (0.5662)	0.2351 (0.5384)
AFDC/TANFC collection rate	0.1978 (0.0983)	0.2005 (0.0959)
CPS collection rate	0.3240 (0.0690)	0.3228 (0.0665)

Notes: Means and standard deviations were computed using person weights from the second wave of the 2001 SIPP. Because the data is in the form of an unbalanced panel, the means and standard deviations were calculated on the basis of variable values during the first year each respondent was at risk for a first or second nonmarital birth.

Table 3. Estimates of the Determinants of Birth and Marriage among Never Married Childless Women
(Odds Ratios Shown, Robust Standard Errors are in Parentheses)

	Model 1		Model 2		Model 3	
Independent variables	Birth	Marriage	Birth	Marriage	Birth	Marriage
Individual level variables						
Black (vs. white)	3.8493**	0.2956	3.8439**	0.2996**	3.8616**	0.2988**
	(0.4416)	(0.0583)	(0.4412)	(0.0590)	(0.4431)	(0.0589)
Hispanic (vs. white)	1.2707	0.8827	1.2739	0.8839	1.2684	0.8835
	(0.1883)	(0.1188)	(0.1888)	(0.1190)	(0.1880)	(0.1189)
High school diploma (vs. less than high school)	0.3464**	0.4053**	0.3469**	0.4083**	0.3456**	0.4079**
	(0.0386)	(0.0485)	(0.0387)	(0.0488)	(0.0385)	(0.0487)
Some college (vs. less than high school)	0.1114**	0.3712**	0.1116**	0.3703**	0.1109**	0.3706**
	(0.0179)	(0.0502)	(0.0180)	(0.0501)	(0.0179)	(0.0501)
Metro Resident (vs. non-metro resident)	0.5739**	0.6482**	0.5752**	0.6433**	0.5739**	0.6436**
	(0.0743)	(0.0751)	(0.0745)	(0.0745)	(0.0743)	(0.0745)
State level variables						
CSE index	0.9729	0.5184**	___	___	___	___
	(0.1725)	(0.0910)				

(table continues)

Table 3 (continued)

	Model 1		Model 2		Model 3	
Independent variables	Birth	Marriage	Birth	Marriage	Birth	Marriage
Fraction of AFDC/TANF Cases with collections	___	___	3.2699 (2.7025)	0.3459** (0.2609)	___	___
CPS collection rate	___	___	___	___	3.7416 (5.5781)	0.7598 (1.0718)
Family Cap (=1)	1.0258 (0.1986)	1.0709 (0.1931)	0.9988 (0.1938)	1.0678 (0.1940)	1.0258 (0.1980)	1.0676 (0.1932)
Other waiver (=1)	1.1012 (0.2275)	1.0311 (0.2027)	1.1404 (0.2364)	0.9843 (0.1939)	1.0783 (0.2238)	1.0188 (0.1999)
Unemployment rate	1.0515 (0.1034)	2.1262** (0.2024)	1.0571 (0.1045)	2.0883** (0.1960)	1.0633 (0.1052)	2.1068** (0.1984)
25th Percentile of hourly wages	1.1594 (0.2452)	0.5515** (0.1146)	1.2859 (0.2876)	0.5015** (0.1090)	1.1460 (0.2425)	0.5538** (0.1142)
Additional Controls						
Age-duration effects	Included	Included	Included	Included	Included	Included
Year effects	Included	Included	Included	Included	Included	Included
State effects	Included	Included	Included	Included	Included	Included

Notes: N=3546, N*T=15,188. All estimates are weighted using 2001 SIPP Wave 2 person weights. ** Statistical significant at the 0.05 level * Statistical significant at the 0.10 level.

Table 4. Estimates of the Determinants of Birth and Marriage among Never Married Women with on Child
(Odds Ratios Shown, Robust Standard Errors are in Parentheses)

	Model 1		Model 2		Model 3	
Independent variables	Birth	Marriage	Birth	Marriage	Birth	Marriage
Individual level variables						
Age	0.8989**	0.9616**	0.8986**	0.9615**	0.8986**	0.9609**
	(0.0201)	(0.0193)	(0.0201)	(0.0193)	(0.0201)	(0.0193)
Black (vs. white)	1.1707	0.3137**	1.1665	0.3172**	1.1658	0.3101**
	(0.1954)	(0.0638)	(0.1949)	(0.0646)	(0.1941)	(0.0631)
Hispanic (vs. white)	1.1711	0.8023	1.1485	0.7931	1.1482	0.7918
	(0.2750)	(0.2052)	(0.2697)	(0.2031)	(0.2697)	(0.2028)
High school diploma (vs. less than high school)	0.5218**	1.3462	0.5212**	1.3383	0.5209**	1.3449
	(0.0946)	(0.3155)	(0.0946)	(0.3141)	(0.0944)	(0.3158)
Some college (vs. less than high school)	0.4990**	1.2933	0.4949**	1.2811	0.4949	1.3039
	(0.1048)	(0.3301)	(0.1040)	(0.3272)	(0.1040)	(0.3341)
Teen mother (=1)	0.9445	0.9185	0.9243	0.9012	0.9244	0.9140
	(0.1957)	(0.2402)	(0.1915)	(0.2358)	(0.1916)	(0.2396)
Metro resident (=1)	1.0669	0.8257	1.0537	0.8127	1.0536	0.8149
	(0.2202)	(0.1940)	(0.2169)	(0.1902)	(0.2169)	(0.1910)

(table continues)

Table 4 (continued)

Independent variables	Model 1 Birth	Model 1 Marriage	Model 2 Birth	Model 2 Marriage	Model 3 Birth	Model 3 Marriage
State level variables						
CSE index	0.4693** (0.1256)	0.6343** (0.1954)	___	___	___	___
Fraction of OCSE AFDC/TAN Cases with collections	___	___	0.9889 (1.3447)	0.2892 (0.4779)	___	___
CPS collection rate	___	___	___	___	1.0816 (2.3259)	37.1083 (100.4531)
Family cap (=1)	0.9600 (0.2947)	0.6956* (0.2432)	0.9577 (0.2953)	0.7035 (0.2471)	0.9568 (0.2936)	0.6726 (0.2356)
Other Waiver (=1)	0.7053 (0.2260)	1.0290 (0.4026)	0.7283 (0.2322)	0.9859 (0.3851)	0.7252 (0.2310)	0.9660 (0.3752)
Unemployment rate	1.5737** (0.1887)	2.7397** (0.4502)	1.5700** (0.1860)	2.7056** (0.4410)	1.5719** (0.1875)	2.7813** (0.4565)
25^{th} percentile of hourly wages	1.0697 (0.3269)	0.3672** (0.1428)	1.1112 (0.3548)	0.3501** (0.1426)	1.1102 (0.3369)	0.3821** (0.1465)
Additional Controls						
Duration effects	Included	Included	Included	Included	Included	Included
Year effects	Included	Included	Included	Included	Included	Included
State effects	Included	Included	Included	Included	Included	Included

Notes: N=761, N*T=2,329. All estimates are weighted using 2001 SIPP Wave 2 person weights. ** Statistical significant at the 0.05 level * Statistical significant at the 0.10 level

Figure 1. Trends in Child Support Enforcement Measures (1988 to 1999)