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Abstract

A simple analysis of economic incentives implies that stricter child support enforcement will tend to reduce nonmarital childbearing by raising the costs of fatherhood for unmarried men. We investigate this hypothesis with a sample of women from the Panel Study of Income Dynamics, to which we add information on state child support enforcement. We examine childbearing behavior between the ages of 15 and 44 before marriage and during periods of non-marriage following divorce or widowhood. The estimates indicate that women living in states with more effective child support enforcement are less likely to bear children when unmarried, especially if they are young, never-married, or black. The findings suggest that policies that shift more costs of nonmarital childbearing to men may reduce nonmarital childbearing.

Key Words: Child support, Nonmarital childbearing

The Impact of Child Support Enforcement Policy on Nonmarital Childbearing

Births outside of marriage have grown dramatically over the past three decades in the United States. In 1965, the nonmarital birth ratio — the percentage of births to unmarried mothers relative to all births — was 7.7 percent. By 1980 it had risen to 18.4. Since 1993, the figure has been between 31 and 33 percent. Among teenagers the ratio is much higher – 82 percent in 2003 (Ventura & Bachrach 2000, Child Trends 2005).

The nonmarital birth rate (number of births per 1000 unmarried women) has also grown steadily. In 1965 it was 23.4 among all women 15-44 and 16.7 among teenagers 15-19. By 1980 it had risen to 29.4 among all women 15-44 and 27.6 among teenagers. In 1993 the corresponding birth rates were 45.3 and 44.5. In recent years the rate for teenagers has fallen under 40 while the overall rate has leveled off in the mid-40 range (Ventura & Bachrach 2000, Child Trends 2005).

The chain of behaviors that result in a nonmarital birth include: the choice to have sexual relations, frequency of intercourse, use of contraception, and, should a pregnancy occur, the decision to abort, marry and bear the child, or have a nonmarital birth. Decisions about sexual intercourse and marriage involve two persons rather than one. The same often is true for decisions about contraceptive use and abortion. Yet research that considers how government policies may influence both women's <u>and</u> men's behavior that leads to nonmarital births is sparse.

The studies that have done so have focused on whether the incentives of child support policy influence nonmarital fertility. As Sorenson (1997) and others have observed, the government's poor record of establishing paternity and enforcing payment of child support by nonresident fathers may partly be responsible for men's failure to take responsibility for contraception or to marry their sexual partners. More generally, as discussed more fully below, child support policy's incentives that discourage unwed fatherhood tend to be stronger than its incentives that encourage unwed motherhood. This situation leads to the hypothesis that, on net, better child support enforcement will be associated with lower nonmarital birthrates.

Five prior studies have examined this hypothesis and all report evidence in support. Case's (1998) and Garfinkel et al.'s (2003) analyses of state level data report that states with more stringent child support policy and enforcement have lower nonmarital birth rates. Aizer & McLanahan (2006) finds a similar relationship with state level data, but for the number of nonmarital births, not the rate. Huang (2002), Plotnick et al. (2004) and Aizer & McLanahan (2006) use micro-data from the National Longitudinal Survey of Youth to examine whether child support enforcement is related to the likelihood that a woman's first birth is premarital. Focusing on the teenage years, Plotnick et al. (2004) finds that young women living in states with higher rates of paternity establishment are less likely to become unwed mothers. Aizer & McLanahan (2006) similarly reports a negative relationship between expenditures on enforcement and the likelihood of nonmarital childbearing. Huang (2002) examines both expenditures and the strictness of child support laws. He reports similar relationships when women are age 20 or older but, unlike Plotnick et al., not when they are teenagers.[1]

The analysis here replicates and extends this line of research in several ways. It uses the Panel Study of Income Dynamics, a data set not previously brought to bear on this issue. Instead of focusing on first births like the three other micro-data articles, it considers all nonmarital childbearing, whether occurring before a first marriage or during periods of non-marriage following divorce or widowhood, and regardless of parity. The study considers four alternative indicators of support enforcement, of which two have not been examined in prior studies. The findings offer qualified support for the hypothesis that stronger child support enforcement creates incentives that reduce the likelihood of nonmarital childbearing. Nonmarital childbearing among blacks shows the strongest relationship with the incentives of the child support system.

U.S. Child Support Policy and Its Expected Effect on Nonmarital Childbearing

Until very recently, financial responsibility for children born outside marriage rested primarily with the mother, her family, and government. Mothers who met the income test, which included the vast majority of unwed mothers, were eligible for AFDC, Food Stamps, Medicaid, and in many cases, housing subsidies. In contrast, unwed fathers were more or less free to shirk their parental obligations and most did so (Garfinkel 1992).

During the past 30 years the federal government has taken a number of steps to prevent unmarried fathers from abandoning their children financially (Garfinkel, McLanahan & Robins 1994, Lerman & Sorenson 2003). In 1975, Congress created the Child Support Enforcement Program which established local offices of child support enforcement and authorized Federal matching funds for states to help locate absent parents, establish paternity, establish child support orders and obtain child support payments (U.S. House of Representatives 2000: section 8). The 1984 Child Support Amendments extended this legislation by requiring states to withhold child support obligations from the paychecks of delinquent fathers and to develop legislative guidelines to be used in determining child support awards. The Family Support Act of 1988 mandated states to adopt presumptive guidelines for child support awards and initiate automatic withholding from fathers' paychecks, regardless of delinquency. The Act also included provisions aimed at strengthening paternity establishment for children born to unmarried parents.

Reforms in the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) sought to further improve the child support system's ability to establish paternity, to locate nonresidential fathers, and to collect support payments. It also eliminated the requirement that states provide a "pass through," which allowed mothers on welfare to keep the first $50 of child support each month. Throughout most of the post-1975 period, intensified efforts to establish paternity and enforce child support were generally viewed as ways to reduce the financial costs of public welfare rather than as strategies for preventing nonmarital births. The latter line of thought received a hearing, though, in the debate about the PRWORA.

The results of this stream of legislation have been striking with respect to children born outside marriage. The paternity establishment ratio (the number of paternities established in a given year divided by the number of nonmarital births) increased from 20 percent in the early 1980s to 65 percent by 1997 (Nichols-Casebolt & Garfinkel 1991, U.S. House of Representatives 2000, table 8-22). Between 1981 and 1997 the proportion of never married mothers with a child support award grew from 14 to 47 percent and the proportion receiving a child support payment rose from 7 to 22 percent (Lerman & Sorenson 2003).

How child support policy affects parenthood and marriage choices

Sociological and psychological theories, as well as substantial empirical research, conclude that nonmarital parenthood is a product of many influences (Moore, Miller, Glei, & Morrison 1995, Kirby 2001). These include biological factors, individual social and personality characteristics, and family, peer, and community influences. Economists and some sociologists argue in addition that the likelihood of nonmarital parenthood may partly be influenced by economic incentives embodied in the labor market, such as women's earnings opportunities or availability of marriageable males, and in public policies (Duncan & Hoffman 1990, Lundberg & Plotnick 1995, South 1996, Willis 1999, Wilson 1987, Wolfe et al. 2001).

The stronger the administration and enforcement of child support laws, the higher the likelihood that fathers who do not live with their children will be required to make substantial financial contributions over many years to their support. Child support policy, therefore, increases the expected costs of fatherhood for nonresident fathers.[2] This financial disincentive would, other things equal, make men more reluctant to father children outside marriage and, if a nonmarital pregnancy occurs, make them more likely to marry before the birth and reside with their children.

For women, improved enforcement that leads to higher child support payments would appear to reduce the costs of children and create more incentive to have children outside marriage. However, given that a large proportion of both teenage and non-teenage women who give birth while unmarried are likely to go on welfare (Duncan & Hoffman, 1990; Foster, Jones & Hoffman 1998; Haveman & Wolfe 1994) and given that welfare policy taxed child support payments during the time period we analyze (1980-1993), this countervailing effect is likely to have been small. From 1980 to 1984, a mother on welfare retained none of the support paid by the nonresident father. Rather, all payments were used to offset benefits paid from public funds. Between 1985 and 1993, a mother on welfare was allowed to keep only the first $50 of child support each month. All payments above the $50 pass through went toward reducing public spending on welfare and did nothing to increase her children's standard of living. Indeed, from the mother's viewpoint stricter child support enforcement may actually have increased the cost of raising children if she had been getting informal support from the father and if that support declined or ended because of stricter enforcement (Waller & Plotnick, 2001).

One may reasonably conclude that child support policy during 1980-1993 did little to affect low-earning women's incentives regarding nonmarital childbearing. Consequently, the effect of the disincentive on unmarried men's motivation to avoid nonmarital parenthood is likely to have been stronger than the reverse incentive effect for unmarried women. This reasoning leads to the hypothesis that, on net, better child support enforcement will be associated with lower premarital birthrates, particularly among women with a significant chance of needing public assistance in the event of a nonmarital birth and their male partners. [3]

Evidence that other public policies are related to nonmarital childbearing provides indirect support for expecting child support policy to be related to this behavior as well. Welfare benefits largely limited to single mothers make lone motherhood more affordable and reduce the gains from marriage. Empirical evidence suggests that these incentives undermine marriage and promote nonmarital childbearing, though the importance of these effects remains uncertain (Aasave 2003, Blackburn 2000, Moffitt 1998). Lopoo & DeLeire (2006) find a negative relationship between implementation of the 1996 reform's minor parent provisions and fertility among teens under age 18. Family planning policies, abortion policies, and the availability of family planning and abortion services also are related to the likelihood of nonmarital childbearing (Lundberg & Plotnick 1995, Kirby 2001).[4]

Statistical Models, Data, and Explanatory Variables

States are ultimately responsible for administering family law, including child support law. State success in enforcing child support law varies widely. When the federal government began pushing enforcement reforms in the early 1980's, some states were already relatively effective in paternity establishment, but most were not. Nearly all have improved their records; some dramatically, others not as much. We exploit the varying vigor and commitment with which states implemented their support enforcement programs to test this study's hypothesis.

We carry out the test by constructing spells of non-marriage and estimating a discrete time logit hazard model of whether a woman had a nonmarital birth before the spell ends. We estimate the model using data in the PSID during 1980-1993, a period when child support policy and enforcement underwent enormous changes.[5] The sample initially consisted of 15,201 women whose marriage and childbirth histories were available in the 1985-1993 Marriage History File and the 1985-1993 Childbirth and Adoption History file.[6] We used the information in these two files to construct all periods when a woman faces the risk of having a nonmarital birth. For a typical woman, the first risk period starts, by assumption, at age 15.[7] It ends either with her first marriage or a nonmarital birth. The risk period is censored if neither occurs before the last year in the data set or age 45, whichever comes first. If it ends with marriage and the marriage dissolves before age 45, a second risk period begins. It may end via a second marriage, a nonmarital birth, or censoring. We proceed in parallel fashion for the period following the end of a second or higher order marriage. If a risk period ends with a nonmarital birth, we assume a postpartum infertility period of two months.[8] Then, if the woman is still unmarried, another risk period begins. Each risk period is divided into risk years, the unit of analysis in the multivariate hazard models. To obtain personal and family background variables, we merged risk period data with information from the PSID family and individual files.

The child support policy variables are available starting in 1980. We use information on annual state of residence to append values for these variables to each risk year. To avoid left censoring, we restrict the sample to women who have one or more risk years starting no earlier than 1980. The sample thereby includes all women who turned 15 during the sample period. It also includes women who divorced or had a non-marital birth during the sample period and, so, began a new risk period. The resulting sample contains 5,195 women who reported 1,220 nonmarital births during the 22,107 risk years that are observed.[9]

We examine four alternative indicators of support enforcement. Because higher values of each indicator reflect better enforcement, the theory predicts each will have a negative relationship with the likelihood of a nonmarital birth.

The first is an index of the extent to which a state has adopted child support enforcement legislation. This legislative index covers each step of the enforcement process: establishing paternity, obtaining an award, and collecting payments. It includes eight forms of child support legislation: genetic tests, paternity establishment, numerical guidelines, presumptive guidelines, wage withholding under delinquency, immediate wage withholding for new cases, universal wage withholding, and state income tax intercept. Genetic testing permits the father's genetic test results to be used to establish paternity. Paternity establishment allows for the chance to establish the paternity throughout the child's minority until age 18. Numerical guidelines provide a nonbinding set of guidelines to advise judges in enforcing child support laws. Presumptive guidelines require that these guidelines be used unless the judge can cite "good reason" to deviate. Wage withholding under delinquency indicates that the state has a system similar to income tax withholding that allows deductions of child support obligations and any arrearages from the obligor's paycheck. Federal law now requires all new or modified support orders for welfare recipients to include immediate withholding of support, but this was not so in the 1980s and early 1990s. Last, a state income tax intercept gives the state authority to garnish state income tax refunds up to the amount of overdue child support. The index ranges from zero for states with no law to eight for states with all eight laws. We assume a one-year lag between legislative enactment and implementation.[10] Case (1998) and Huang (2002) used similar indicators of legislation in their studies of nonmarital childbearing.

The second is, for each state, enforcement expenditures reported by the Office of Child Support Enforcement divided by the number of single-mother families. The latter is obtained from the March Current Population Survey. Other studies of behavioral effects of child support policy (Aizer & McLanahan 2006, Huang et al. 2002, Huang 2002, Freeman & Waldfogel 2001, Holzer et al. 2005) have used such an indicator.[11]

These two indicators reflect inputs to the enforcement process. We also examine two more proximate indicators of enforcement success (i.e. the outcome). Neither has been used in prior studies of nonmarital childbearing. The "effectiveness ratio" is the child support collected on behalf of never-married mothers as a percentage of the total amount of child support owed for all never married mothers, assuming the state used the Wisconsin percent-of-income guidelines. The guidelines are a way of standardizing child support obligations across states.[12] The amount owed depends upon the number of children owed support and the income of the non-resident father. Non-resident father income is estimated as a function of the demographic characteristics of the mother and state median earnings and unemployment rates. The ratio is calculated for each never married mother in the state and aggregated to obtain the state-level indicator.

The fourth indicator first uses CPS data to compute the total amount of child support received by never-married mothers divided by the number of never-married mothers in the state (including those who received none). This ratio reflects both the strength of child support enforcement, and also differences across states in fathers' ability to pay child support and in other factors, such as labor market conditions and welfare generosity, that affect the difficulty of collecting child support. For example, low-income fathers have less ability to pay support, and high unemployment further reduces a father's ability to pay support. Race/ethnicity may be important because black fathers are more likely to have children on welfare, who will gain little or nothing from his child support contributions.[13]

We, therefore, purge this measure of differences across states in the difficulty of enforcement possibly created by demographic and other state characteristics. This is done by first regressing the child support payment on demographic characteristics of the mother – years of schooling, race and ethnicity, whether an immigrant, age, number of children, whether any child is less than age six, and state characteristics – the median earnings of males age 18 to 65 in the state, the maximum AFDC plus food stamp benefit and the state unemployment rate. We use the results to predict the average amount of child support received in each state. The prediction better reflects the quality of state enforcement activities since it is purged of confounding factors. We take the ratio of the average actual to the average predicted amount to obtain an adjusted child support payment ratio, and standardize it. This measure is similar, though not identical, to the effectiveness ratio.

Because child support laws and expenditures on enforcement help determine the effectiveness and adjusted child support payment ratios, one may prefer estimates based on the first two indicators. On the other hand, the latter two reflect aggregate conditions over which an individual has no control and, hence, are likely exogenous to behavior. And both directly capture three key aspects of the enforcement process in one parameter: 1. the probability of having a child support obligation, 2. conditional on having an obligation, the probability that some or all it will be collected, and 3. the amount of the obligation that is paid. Thus, both may be better indicators of enforcement success than measures of laws and expenditures.[14]

Table 1 shows the most and least successful states based on a ranking of states' mean values of each enforcement indicator during 1980-1993. Minnesota, New Hampshire, North Dakota, Utah, Wisconsin, and Wyoming rank among the top five on two indicators. Mississippi ranks among the bottom five on all four. Washington D.C. falls among the bottom five on three indicators; South Carolina on two.

Other factors in addition to child support enforcement may affect decisions about nonmarital childbearing. Welfare benefits are an important state policy that may affect fertility and marriage behavior. We measure them as the AFDC cash benefit (in 1987 dollars) provided to a four person family with no other income – i.e. the welfare guarantee. We control for the restrictiveness of state abortion policy and the availability of abortion services, both of which may affect whether a woman carries a nonmarital pregnancy to term (Lundberg & Plotnick, 1995) with three indicators. One is a dummy variable indicating a state law requiring parental consent for a minor to obtain an abortion. A second dummy indicates whether a state restricts use of Medicaid funds for abortions. Both are from Levine (2004). Abortion availability is indicated by the fraction of counties in a state with no abortion provider of any size.[15]

One would expect the welfare guarantee to be positively related to the likelihood of a nonmarital birth. The abortion variables would be positively related to the extent they induce more women to carry nonmarital pregnancies to term, but would be negatively related if they lead more women to avoid unwanted nonmarital pregnancies. Hence, theory provides an ambiguous prediction on the sign of such variables' coefficients.

Whatever the relationship between child support incentives and fertility and marriage decisions, it is well known that women's personal and family background variables are related to her likelihood of nonmarital childbearing (Kirby 2001). These variables partly capture differences in family resources and in non-monetary benefits and costs associated with nonmarital childbearing. The models include a limited number of such exogenous variables. We include age at the start of each risk period, as well as age squared to see if there is a non-linear relationship between age and nonmarital fertility. There are two race/ethnicity dummy variables – non-Hispanic black and Hispanic. The omitted category is non-Hispanic white. We indicate religion with dummy variables for being Baptist, other Protestant, and Catholic. The omitted category is "other religion or none." Mother's marital status when the respondent was born and mother's and father's education are also control variables.[16]

Unobservable state characteristics may be associated with child support, welfare, or abortion policy, and with nonmarital fertility. For example, a state with residents who hold more traditional family values may enforce child support more stringently and have a lower rate of nonmarital childbearing. To account for this possibility we, like many other studies, include state fixed effects in all models. To account for secular changes over time that may be correlated with changes in child support policy, we take two approaches. One adds year effects; the other adds state effects interacted with a linear time trend.[17] To be able to estimate state effects, we exclude women in smaller states where no one reported a nonmarital birth.

Results

Table 2 reports the results from eight separate models that use all risk periods in the sample. Each model includes one enforcement measure. All also include the welfare, abortion, personal, and family background variables, and one of two fixed effect specifications.

Consistent with theoretical expectations, all coefficients in column one are negative. Two are significant at the one percent level, one at the five percent level, and one at the ten percent level. Because the enforcement measures are scaled in different metrics, the third value in each cell shows the change in probability of nonmarital childbearing associated with a one standard deviation increase in each measure.[18] Despite the diversity of measures, the changes are closely grouped between -.0073 and -.0108, or between -15 and -22 percent of the sample mean of 0.0483. The alternative fixed effect specification in column two yields weaker results. Two coefficients are negative and marginally significant, but the other two are insignificant. For the marginally significant coefficients, the change in probability associated with a one standard deviation increase in the enforcement measure is -.0049 and -.0064 (-10 and -13 percent). The effectiveness ratio and child support payment ratio show stronger associations with nonmarital childbearing than the laws index and expenditure measure.

Though they are not the focus of this study, findings on the welfare, abortion, and personal and family background variables are of interest per se. The more that they are congruent with the thrust of prior research, the more one may have confidence in the results for the child support policy measures. Table 3 presents coefficients for these variables from the model in table 2, row three, column one. Coefficients from the other models were similar.

There is no relationship between the welfare guarantee and nonmarital childbearing. Living in a state with restrictions on Medicaid funding of abortion has a significant association with nonmarital childbearing. Living in such a state is associated with a 0.0166 increase in the probability of having a nonmarital birth each year compared to a state with no restriction, relative to a mean of 0.0483.[19] Black and Hispanic women are much more likely to have a nonmarital birth relative to non-Hispanic white women. Being black or Hispanic is associated with a 0.0488 or 0.0367 increase in the probability. We observe a strongly significant non-linear age effect. The likelihood of a nonmarital birth increases until age 24, then declines. Religion is related to nonmarital childbearing. Relative to women raised with no religion or a non-Christian one, Protestants other than Baptists are less likely to have nonmarital births. The effect is large in substantive terms, but weaker than the race and ethnicity effects. The smaller negative effects for Catholics and Baptists are significant at the ten percent level. Being born to a mother who had never married at the time of the respondent's birth is strongly associated with a greater likelihood of the daughter becoming an unwed mother herself. Mother's education has a monotonically negative relationship with a daughter's likelihood of having a nonmarital birth. The coefficient achieves significance only for "graduated college." Father's education is negatively associated with nonmarital childbearing as well, but the coefficient on "graduated college" is insignificant. The results for the welfare, abortion, and demographic variables are consistent with much of prior research (Kirby 2001).

Age and marital status interactions

There are theoretical grounds for expecting the relationship between child support and nonmarital childbearing to differ between younger ands older women and between women before and after a marriage. One hypothesis is that policy would more strongly affect the behavior of older women and women who had been married because, being older and more experienced, their behavior (and the behavior of their likely partners) would be more rational and therefore more likely to be affected by economic incentives. Our conceptual analysis, however, suggests that the asymmetric incentives of child support enforcement are more important for low-income women who are most likely to be welfare recipients for a long time and their male partners. This reasoning implies that child support enforcement will have bigger deterrent effects for younger and never married women, who tend to have lower income than their comparison groups. Which hypothesis has more support is an empirical question.

Tables 4 and 5 present the child support policy coefficients estimated on sub-samples based on age and marital status. The models include the same control variables as in table 2.

The estimates show that child support enforcement has a stronger relationship with nonmarital births among younger women. Seven of the eight coefficients in the top panel of table 4 are negative and three are statistically significant. For the significant coefficients in column one, the changes in probability associated with a one standard deviation increase in the enforcement measures are similar to each other and to the predicted changes for full sample: –.0112 for both rows 3 and 4 or –21 percent of the young sub-sample mean of 0.0523. For the significant coefficient in column two the corresponding figure is –14 percent. For older women, in contrast, of the six negative coefficients, only one is significant. For that one the corresponding change in probability is –.0134, or –34 percent of the older sub-sample mean of .0391.

The contrast between never married and formerly married women is similar. Seven of eight coefficients for never married women in the top panel of table 5 are negative, with five significant. For the significant coefficients in column one the changes in probability associated with a one standard deviation increase in the enforcement measures are closely grouped, as in table 2. The estimates range between -.0081 and -.0118, or –15 to –22 percent of the never married sub-sample mean of .0535. For the significant coefficient in column two, the corresponding change is -.0054, or –10 percent of the sub-sample mean. For all five significant coefficients, the predicted change is similar to the corresponding prediction for the full sample.

For formerly married women, only one of the four negative coefficients is significant. For that coefficient, the corresponding change in probability is –.0094, or –38 percent relative to the formerly married sub-sample mean of .0249. As in table 2, the fixed effect specification in column one of tables 4 and 5 provides stronger support for the theoretical prediction.

Interacting enforcement measures with race and ethnicity

Racial and ethnic differences in nonmarital fertility are substantial, of great interest, and likely reflect different social processes. To examine this issue we extend the models by interacting the child support policy variables with dummy variables for Hispanic and white.[20] Panel one of table 6 presents the child support policy coefficients for blacks – the reference group. The second and third panels show the coefficients on the interaction terms. The models include the same control variables as the models in table 2.

There are important race and ethnic differences. Black nonmarital childbearing is closely associated with child support enforcement policies. Six point estimates are negative and significant - two at the one percent level, three at the five percent level, and one at the ten percent level. As above, results from the fixed effect specification in column one are stronger. For the coefficients in column one, the changes in probability of nonmarital childbearing associated with a one standard deviation increase in the enforcement measures range between –15 and –24 percent of the black sub-sample mean of .0699. For the two significant coefficients in column two, the corresponding changes are -13 and –15 percent of the sub-sample mean. For all six significant coefficients, the predicted change is similar to the corresponding predicted change for the full sample.

For Hispanics, the coefficients on the laws index, expenditure measure and effectiveness ratio in both columns are insignificant. For these six models the relationships between enforcement and nonmarital childbearing for Hispanics are statistically indistinguishable from those for blacks. That is, Hispanic nonmarital childbearing is negatively associated with enforcement in four of the six models. Results differ for the adjusted child support payment ratio. Though both coefficients are insignificant, they are positive and large relative to the corresponding estimates for blacks. The point estimates for the net relationship for Hispanics, obtained by summing the corresponding coefficients in panels one and two, suggest that this enforcement variable is not significantly related to Hispanic behavior.

For whites, there are five positive coefficients that correspond to significant negative coefficients for blacks. These are in column one, rows 1, 3 and 4 and column two, rows 3 and 4. Summing each estimate for whites with the corresponding one for blacks yields a net association between enforcement and nonmarital childbearing for whites that is essentially zero.[21] The coefficient on the expenditure measure that corresponds to the significant estimate for blacks in row 2, column one is negative and insignificant. So, similar to Hispanics, this relationship for whites is basically the same as that for blacks. The two insignificant coefficients for blacks are matched by insignificant coefficients for whites.[22]

The pattern of coefficients clearly indicates that black nonmarital childbearing has the strongest relationship with the incentives of the child support system, followed by Hispanics. For whites the association between enforcement policy and nonmarital fertility is negligible.

<u>The magnitude of the deterrence effect</u>

Earlier calculations suggest that a one standard deviation increase in an enforcement measure is associated with about a 15 to 20 percent decline in the probability of having a nonmarital birth relative to the appropriate sample or sub-sample mean. Here we use the results for the effectiveness ratio in row three, column one of table 2 to simulate the potential effect of alternative ways of specifying better enforcement. For each risk year, we first use observed values for all the explanatory variables to compute the baseline probability it will end with a nonmarital birth. The mean simulated probability is 0.0483. We then compute the probabilities after increasing the effectiveness ratio by 20 percent for all observations. The simulated mean probability falls to 0.0429, or by eleven percent. The second simulation considers a world in which poorer performing states improve their effectiveness to the median state value during 1980-1993 (17.7). In this scenario the simulated mean probability falls to 0.0419, or 13 percent. The third simulation sets every state's effectiveness ratio to no less than that attained by the state at the 70th percentile during this period (20.0). The simulated mean probability drops to 0.0410, or 15 percent below the baseline. If one uses the smaller coefficient in column two, the predicted declines are 7, 9, and 10 percent.

Table 3 shows that restrictions on Medicaid funding of abortions act opposite to support enforcement. To compare the magnitude of the two relationships, we first observe that the portion of person-years when such restrictions were in place was 0.65. If the extent of restrictions decreases 20 percent to 0.52, the predicted mean probability of a nonmarital birth falls from the baseline of .0483 to .0462, or less than five percent. This suggests that child support policy is more strongly related to nonmarital childbearing.

All states and the federal government agree that better enforcement is desirable. All can take steps to improve it. So one could realistically expect enforcement to improve in ways similar to the ones we simulated. In contrast, given the lack of such consensus on policies explicitly aimed to deter or facilitate abortion, prospects for a significant reduction in the extent of restrictions on abortion are poor. Thus, whatever the relative strengths of the enforcement and abortion restriction variables, the findings on the enforcement variables are more relevant when considering public policy to reduce nonmarital fertility.

Discussion and Conclusion

To test the hypothesis that the stringency of child support enforcement is inversely related to the likelihood of nonmarital childbearing, this study draws on the PSID to analyze spells of non-marriage among women age 15 to 44. Instead of focusing on first births like other research with micro-data, it considers all nonmarital childbearing regardless of parity, whether occurring before a first marriage or during periods of non-marriage following divorce or widowhood. Discrete time hazard models include four alternative indicators of support enforcement, two of which have not been examined in prior studies of this issue. The estimates show that during 1980-1993, women who lived in states that did a better job of enforcing child support were less likely to have nonmarital births.

Two measures of enforcement reflect inputs to the process, two reflect outcomes, and the four are scaled in different metrics. Yet increasing the measures by a comparable amount – one standard deviation – yields predicted declines in the probability of nonmarital childbearing that fall within a narrow range. That this exercise produces similar size predictions for the measures suggests the findings are not an artifact of the choice of measure. Allowing the coefficient on the enforcement variable to differ by key demographic characteristics shows that the relationship is stronger for younger and never married women, and is strongest for blacks, followed by Hispanics.

That nonmarital childbearing among younger, never married, black, and Hispanic women appears more sensitive to the stringency of child support enforcement accords with theoretical expectations. This is because, compared to older, divorced, and white women, younger, never married, black, and Hispanic women are more likely to receive welfare if they become unwed mothers. Consequently, they and their partners would be influenced to a greater extent by the incentives of child support policy.

The consistently weaker results for one of the fixed effect specifications qualify this study's support for the core hypothesis. Finding no evidence that enforcement is related to nonmarital childbearing among older, ever-married, and white women also lowers confidence in the findings.

This study's findings parallel those of other analyses, which all find a negative relationship between strictness of enforcement and nonmarital childbearing. Such findings support the theoretical expectation that the asymmetric incentives for men and women created by enforcement result in fewer nonmarital births. The studies differ, though, in the estimated size of the deterrence effect. Plotnick et al. (2004) reports that a one standard deviation increase in the enforcement measure predicts a 17 percent drop in the rate of nonmarital childbearing. This estimate is essentially identical to ours. Aizer and McLanahan's (2006) corresponding estimate from their micro-data model is higher: 31 percent.[23]

Case (1998) and Garfinkel et al. (2003) do not provide information to compute similar predictions. But they do report simulations that allow one to compute the elasticity of the nonmarital fertility rate with respect to their enforcement measure. For Case (1998) the elasticity is -.06 or -.18 depending on the measure used. The estimates from Garfinkel et al. (2003) are much larger, -.52 and -.80, depending on the model specification. Converting this study's findings into elasticities yields estimates of -.36 to -.70. We cannot derive parallel estimates from Huang (2002). However, Huang's (2002) and Garfinkel et al.'s (2003) simulations are structured in the same way and yield similar results, so one might infer that the implied elasticities in the two studies are similar. If so, there is rough agreement among this and three earlier studies on the strength of the enforcement-nonmarital fertility relationship, with Case (1998) finding a much weaker one and Aizer and McLanahan (2006) a stronger one.

The results here and in all closely related studies except Aizer and McLanahan (2006) derive from data collected before the imposition of time limits on receipt of welfare. Time limits perforce shorten the period when welfare taxes child support payments. As a result, time limits could provide more incentive for women to have children outside marriage relative to the policy regime in effect during the years analyzed by this and other studies. But time limits do not affect men's disincentives. Given that men's and women's incentives act in opposition, the relationship between child support enforcement and nonmarital childbearing may be weaker with time limits in place. This is an important caveat to the findings. Offsetting this to some degree is that in response to the 1996 welfare reforms most states eliminated the pass-through. This policy change increases the tax on child support payments as long as the mother receives TANF.

One direction in which research can profitably move is to analyze post-1996 data.[24] A second is to examine the relationship between enforcement and the chain of behaviors that result in a nonmarital birth: frequency of sexual intercourse, use of contraception, and, should a pregnancy occur, the decision to abort, marry and bear the child, or have a nonmarital birth. Such an analysis would help identify the causal mechanisms linking child support policy with nonmarital childbearing.

Successful intervention programs to prevent teen pregnancy and childbearing have proven difficult to create and sustain (Maynard 1995, Kirby 2001). The results here and in other studies consistently suggest that greater success in enforcing child support, thereby shifting more of the cost of childbearing from unmarried women to their partners, may help reduce nonmarital childbearing, albeit indirectly. This salutary effect of stronger enforcement is in addition to the direct, intended impact of better enforcement on collections and, ultimately, on the standard of living of the children of custodial parents.

If every state's effectiveness ratio had been no less than the value at the 70th percentile, the findings predict a nonmarital birth rate 10 to 15 percent lower. A direct service intervention that reduced nonmarital childbearing by a similar percentage would be viewed as a success. Note that the 70th percentile effectiveness ratio during the 1980s and early 1990s (20.0 out of 100) leaves ample room for better enforcement.

Younger, never married, black, and Hispanic women and their partners are both more likely to become unwed parents and appear to be more sensitive to the stringency of child support enforcement. This implies that the potential impact of tighter enforcement would fortuitously tend to be largest among demographic groups that account for a disproportionate share of nonmarital childbearing.

This paper is but one of a handful suggesting that child support policy affects nonmarital childbearing. They are consistent with the larger rational choice perspective that the incentives created by public policies, whether by design or accident, can make a difference on the margin when people make major demographic decisions. If future research confirms the tenor of these findings, improved child support enforcement may inadvertently turn out to be one of the more potent interventions for reducing nonmarital fertility. It would be a refreshing change to find an unintended consequence of social policy intervention that has large, positive impacts.

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Table 1

Most and Least Successful States in Enforcing Child Support for Four Indicators of Enforcement

Laws index	**Expenditures on enforcement**	**Effectiveness ratio**	**Adjusted child support payment ratio**
Most successful			
1. Wisconsin	Alaska	Wisconsin	N. Hampshire
2. Minnesota	Utah	Wyoming	Utah
3. Florida	New Jersey	Vermont	N. Dakota
4. N. Carolina	Minnesota	N. Hampshire	Montana
5. California	Washington	N. Dakota	Wyoming
Least successful			
51. Mississippi	Mississippi	D.C.	D.C.
50. Kansas	Indiana	Alaska	Mississippi
49. D.C.	Texas	Mississippi	Louisiana
48. Montana	Tennessee	Maryland	S. Carolina
47. N. Mexico, W. Virginia [tie]	S. Carolina, Georgia [tie]	California	Alabama

Rankings are based on the mean value of each indicator for each state during 1980-1993.

Table 2

Relationship of Child Support Enforcement to the Logarithm of the Odds of Nonmarital Childbearing, Full Sample, Alternative Specifications

	Fixed effect specification	
Enforcement variable	State effects and state-time interaction	State and year effects
Laws index	-.0822* (.0332) -.0086	-.0251 (.0353)
Expenditures on enforcement (in $100s)	-.1916# (.0984) -.0074	.1099 (.0953)
Effectiveness ratio	-.0373** (.0128) -.0066	-.0245# (.0132) -.0045
Adjusted child support payment ratio	-.2646** (.0944) -.0108	-.1493# (.0854) -.0064

Third value in cells with significant coefficients shows the change in probability associated with a one standard deviation increase in the enforcement measure. The mean sample probability is 0.0483.

N = 22,107 in all models.
# Significant at .10 level
* Significant at .05 level
** Significant at .01 level

All models in tables 2, 4, 5 and 6 include the personal characteristics and policy variables shown in table 3. Results are similar when other indicators of abortion policy and access are used instead of the Medicaid funding dummy.

Table 3

Relationship of Personal and Family Background Characteristics, Welfare Benefits, and Abortion Policy to the Logarithm of the Odds of Nonmarital Childbearing, Full Sample

Variable	**Coefficient**	**Standard error**	**Marginal effect**[a]
Welfare guarantee (in $100s)	-.00088	.0011	.00004
Restrictive Medicaid funding of abortion	.3606*	.1607	.0166
Black	1.0608**	.1088	.0488
Hispanic	.7974**	.1621	.0367
Age at start of risk period	.5973**	.0599	-.0018 (evaluated at age 25)
Age squared at start of risk period	-.0125**	.0013	
Protestant, non- Baptist	-.4390**	.1170	-.0202
Catholic	-.2288#	.1337	-.0105
Baptist	-.1667#	.0990	-.0077
Respondent's mother never married	.2673**	.0914	.0123
Respondent's mother widowed	.2854	.2851	.0131
Respondent's mother divorced	.1973	.1447	.0091
Mother graduated high school	-.1051	.0796	-.0048
Mother had some college	-.1344	.1202	-.0062
Mother graduated college	-.3276*	.1338	-.0151
Father graduated high school	-.2980**	.0839	-.0137
Father had some college	-.5002**	.1510	-.0230
Father graduated college	-.2089	.1558	-.0096
Constant	-12.133**	1.1167	

a. The marginal effect of a one unit change on the probability of nonmarital birth. The mean probability is 0.0483

N = 22,107. Omitted categories: non-Hispanic white, other or no religion, married/separated, and mother [father] did not complete high school. Coefficients from the model in table 2, row 3, column 1.

Significant at .10 level, * Significant at .05 level, ** Significant at .01 level

Table 4

Relationship of Child Support Enforcement to the Logarithm of the Odds of Nonmarital Childbearing, by Age Group

Risk Periods Through Age 25

	Fixed effect specification	
Enforcement variable	State effects and state-time interaction	State and year effects
Laws index	-.0626 (.0400)	-.0091 (.0397)
Expenditures on enforcement (in $100s)	-.1773 (.1197)	.1214 (.1138)
Effectiveness ratio	-.0556** (.0165)	-.0346* (.0150)
Adjusted child support payment ratio	-.2523* (.1140)	-.0650 (.1002)

N = 15,429

Risk Periods at Age 26 and Higher

	Fixed effect specification	
Enforcement variable	State effects and state-time interaction	State and year effects
Laws index	-.0853 (.0617)	-.0366 (.0767)
Expenditures on enforcement (in $100s)	-.2809 (.1889)	.1017 (.1764)
Effectiveness ratio	-.0212 (.0228)	.0088 (.0245)
Adjusted child support payment ratio	-.4326* (.1963)	-.2720 (.1803)

N = 6,440
\#   Significant at .10 level
*   Significant at .05 level
** Significant at .01 level

Table 5

Relationship of Child Support Enforcement to the Logarithm of the Odds of Nonmarital Childbearing, by Marital Status

Never Married Women

	Fixed effect specification	
Enforcement variable	State effects and state-time interaction	State and year effects
Laws index	-.0689* (.0350)	-.0300 (.0370)
Expenditures on enforcement (in $100s)	-.2090* (.1046)	.0926 (.1022)
Effectiveness ratio	-.0444** (.0129)	-.0246# (.0137)
Adjusted child support payment ratio	-.2620** (.0995)	-.1220 (.0897)

N = 18,034

Formerly Married Women

	Fixed effect specification	
Enforcement variable	State effects and state-time interaction	State and year effects
Laws index	-.1915# (.1145)	-.0086 (.1248)
Expenditures on enforcement (in $100s)	.0268 (.4021)	.4020 (.3078)
Effectiveness ratio	.0153 (.0663)	.0017 (.0525)
Adjusted child support payment ratio	-.5254 (.3496)	-.2716 (.3161)

N = 3,568

\# Significant at .10 level

* Significant at .05 level

** Significant at .01 level

Table 6

Relationship of Child Support Enforcement to the Logarithm of the Odds of Nonmarital Childbearing, with Racial and Ethnic Interactions

Coefficient for blacks

	Fixed effect specification	
Enforcement variable	State effects and state-time interaction	State and year effects
Laws index	-.0856* (.0339)	-.0392 (.0366)
Expenditures on enforcement (in $100s)	-.1675# (.1024)	.1217 (.0991)
Effectiveness ratio	-.0522** (.0135)	-.0330* (.0142)
Adjusted child support payment ratio	-.2989** (.0978)	-.1785* (.0890)

Hispanic interaction term

	Fixed effect specification	
Enforcement variable	State effects and state-time interaction	State and year effects
Laws index	-.0542 (.0559)	.0505 (.0546)
Expenditures on enforcement (in $100s)	.0114 (.1499)	.0569 (.1447)
Effectiveness ratio	.0106 (.0483)	.0025 (.0460)
Adjusted child support payment ratio	.1605 (.1706)	.1489 (.1623)

Table 6 (continued)

White interaction term

	Fixed effect specification	
Enforcement variable	State effects and state-time interaction	State and year effects
Laws index	.0608 (.0364)	.0436 (.0353)
Expenditures on enforcement (in $100s)	-.0524 (.1024)	-.0603 (.0997)
Effectiveness ratio	.0671** (.0256)	.0460# (.0240)
Adjusted child support payment ratio	.1018 (.1219)	.0871 (.1137)

N = 22,107 in all models
# Significant at .10 level
* Significant at .05 level
** Significant at .01 level

Endnotes

[1] Aizer & McLanahan (2006) does not model teenagers separately. One other related study (Sonenstein, Pleck & Ku 1994) finds that a substantial proportion of adolescent males are aware of paternity establishment and may modify their sexual behavior and contraceptive use accordingly, especially if their peers are doing so.

[2] For nonresident fathers who plan to provide support above the level required by child support policy, policy may not affect costs. But for no father does it lower the expected financial costs of fatherhood.

[3] Women who expect to be on welfare for a short period have a correspondingly short period when they expect support payments to be taxed and, so, their incentives may largely offset men's disincentives. Whether, on average the disincentive effect is larger is the empirical question we examine.

[4] See Lerman & Sorenson (2003) for a survey of the effects of child support incentives on a range of behaviors.

[5] Testing the model with explicit data on men's behavior would also be desirable. However, men consistently underreport the number of children they father. Underreporting is especially high among unmarried men (Garfinkel, McLanahan, & Hanson 1998.)

[6] The Marriage History File reports a complete retrospective marriage history for a household head or wife of any age at the time of the 1985 interview. For a woman of any age who became a new head or new wife during 1986-1993, detail about only first and most recent marriage was reported. In all waves (1985-1993), detail about only first and most recent marriage was reported for other family unit members aged 12-44 at the time of the interview. Fortunately, we can obtain rather complete retrospective marriage histories for these women, since almost all the women in these two categories had at most two marriages. The 1985-1993 Childbirth and Adoption History File contains a complete retrospective birth history for a head or wife of any age and for other family unit members aged 12-44 at the time of the interview in all waves 1985-1993. The file includes records for women who have never had children.

[7] Births to girls younger than 15 are uncommon. For example, in 2000, the nonmarital fertility rate for 13 and 14 year old girls was 2.1. (Birth data from Centers for Disease Control; total population in age groups from U.S. Census Bureau.)

[8] We assume two months, which is fairly short, because there are many cases in the PSID with a birth interval of only 11 or 12 months.

[9] The sample includes cases from the poverty over-sample in the PSID. Because some women have more than one risk period and all have multiple risk years, the number of cases in the estimates substantially exceeds the number of women. The data are not weighted in the regressions.

[10] Information on legislation was collected mainly from various years of the State Legislative Summary from the National Conference of State Legislatures and the OCSE Legislative Tracking System Report from the U.S. Department of Health and Human Services. Inconsistencies between the two sources were resolved by examining each state's existing laws.

[11] Adjusting expenditures with a state cost of living index would put differences in real terms. Because state cost of living indices are notoriously inaccurate and introduce other measurement error, we did not use one.

[12] Garfinkel, Miller, McLanahan, & Hanson (1998) provide further details on the construction of the effectiveness ratio. They examine the two most common guidelines used (the income shares guideline and the percent of income guideline) and find that state rankings on the effectiveness ratio are not sensitive to the guideline used. We thank Theresa Heintze for sharing the effectiveness ratio data used in this analysis and Lenna

Nepomnyaschy for sharing the regression based measures. Nepomnyaschy (2003) discusses alternative measures.

[13] Misreporting of support payments is common in the CPS and in some states there are relatively few cases to compute the measure. The resulting measurement error makes it harder to obtain statistically significant estimates.

[14] We do not include the rate of paternity establishment separately because it is a critical contributor to the success of the enforcement process.

[15] We also examined two alternative indicators – the fraction of counties without a non-hospital provider of 400 or more abortions, and the fraction of women in a state living in counties without a non-hospital provider of 400 or more abortions. Results for the child support enforcement variables were virtually the same for all three indicators and for the two abortion policy dummy variables.

[16] We use mother's marital status at the birth because we lack adequate data on family structure during sample members' youth. We do not include the woman's own educational attainment because it is endogenous to fertility and marriage behavior. For example, one would expect a woman with higher education or expecting to obtain more education to be less likely to have a nonmarital birth because it may interrupt her education and early career. We also estimated models that included number of children at the start of the risk period. Coefficients on this variable are insignificant in nearly all models and results on the policy variables are very similar to those reported in the tables.

[17] The two specifications of fixed effects are plausible alternatives. A model with state and year fixed effects holds each state's effect constant over time and forces the common part of the constant term to change over time in the same way for all states. Specifying state effects interacted with a time trend lets each state's effect vary over time independently, which is more flexible than the alternative. On the other hand, the time trend interaction forces each state's effect to change linearly, while year effects do not impose linearity on change over time. In the first specification, the coefficient on enforcement is identified by within state changes in enforcement over time. In the second, the effect is identified by within state deviations in enforcement from a linear time trend.

[18] The mean and standard deviation for the un-normalized measures is 3.30 (2.51) for the law index, 181.1 (101.9) for expenditures, and 19.70 (4.59) for the effectiveness ratio.

[19] There does not appear to be consensus in the research on the importance of such restrictions. Some prior research finds an insignificant relationship between abortion policy and nonmarital childbearing (e.g. Wolfe et al. 2001), while other finds a relationship comparable to ours (e.g. Lundberg & Plotnick 1995).

[20] We do not run separate estimates for the three groups because there are too many empty cells for the black and Hispanic subsamples.

[21] A likelihood ratio test does reject the hypothesis that the sum of the black and white coefficients equals zero.

[22] Huang (2002) also reports stronger effects of enforcement on blacks than whites. He does not report separate estimates for Hispanics.

[23] Details of the computations behind the comparisons are available from the authors.

[24] Aizer & McLanahan (2006) do not test whether the enforcement-nonmarital fertility relationship changed after 1996.