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The Effects of Family Caps on the Subsequent Fertility Decisions of Never-Married Mothers

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Abstract

Family caps seek to reduce fertility among welfare recipients by denying additional cash assistance to recipients that have children while on welfare. A necessary condition for family caps to be an effective policy tool is that welfare recipients respond to financial incentives in making decisions that affect subsequent fertility outcomes. In this paper I use data from the 2001 panel of the Survey of Income and Program Participation (SIPP) to examine whether welfare mothers respond to the incentive provided by the AFDC/TANF system in determining whether or not to have a second child. The estimates presented in this paper suggest that family caps do not have an effect on subsequent child-bearing among never-married women.

Introduction

One of the more controversial welfare reform provisions implemented by many states during the waiver period, and by a few states as part of their TANF plans, was the family cap. Family caps either limit or eliminate the incremental increase in cash assistance associated with an additional family member. Supporters of family caps argue that limiting the incremental increase in cash assistance associated with having an additional child provides a strong incentive for women on welfare not to have additional children. By potentially reducing fertility rates among women who are on welfare, family caps may reduce prolonged welfare use and dependency. Some critics of family caps contend that women on welfare do not respond to the small incremental increase in cash assistance associated with another family member by having additional children, but that these additional funds do provide needed resources for growing families. From this point of view, family caps increase hardship for welfare-reliant families without reducing fertility among welfare mothers. Other critics of family caps acknowledge that eliminating the increase in cash assistance associated with an additional child does reduce fertility among welfare recipients, but are concerned about increased abortion rates in response to these policies.

For family caps to be an effective policy, the higher order fertility decisions of welfare recipients must be responsive to financial incentives. In this paper I use data from the 2001 panel of Survey of Income and Program Participation (SIPP) merged with state-level data on benefit levels and the timing of welfare reform implementation to assess the extent to which family caps affect second births to never-married women. The following sections of this paper review the findings of previous studies of the effect of incremental benefits and family caps on birth outcomes, describe family cap policies and other welfare reform provisions, document the data and methods used in this analysis, and present the results and conclusions.

Previous Literature

There is a small, but growing, body of literature that examines the link between incremental benefits and fertility rates among actual and potential welfare recipients. Few of these papers explicitly address the family cap policy. Instead, these studies use data prior to the implementation of family caps and interstate variation in incremental benefits to examine the link between the generosity of incremental benefits and higher order fertility among women affected by welfare policy. This literature has failed to provide a decisive answer to the question of whether or not welfare recipients alter their fertility behavior in response to level of incremental benefits. Some studies find small effects in the expected direction (Powers 1994, Argys et al. 2000), while others find no effects, or effects that are inconsistent with the predictions of economic theory (Fairlie and London 1997; Acs 1997).

The difficulties researchers have had in linking subsequent fertility decisions to the incentives provided by the AFDC program are twofold. First, the variation in additional cash and in-kind assistance available upon the birth of an additional child was small; perhaps too small to affect behavior. Secondly, the fact that benefit levels have not changed much in real terms has meant that the primary source of variation available to identify the effects of incremental benefits in many existing studies is interstate. This reliance on interstate variation to identify the effect of incremental benefits presents a number of difficulties. One potential difficulty of using interstate variation to identify the effect of the incremental benefit on fertility outcomes is that there are other, unobservable factors, which vary across states and can affect fertility decisions. The effects of these unobservable factors on fertility decisions may dwarf the effects of differences in incremental benefit levels, leading to bias and/or imprecise estimates. Another problem with using interstate variation to identify the effects of incremental benefits on fertility outcomes is that incremental benefits are highly correlated with actual benefit levels. Because actual and incremental benefit levels were highly correlated, researches have had difficulty disentangling the effects of the benefit level from the effects of the incremental benefit.

A number of recent studies have addressed these problems by using data that at least partially span the period of family cap implementation. One such study, conducted by Horvath and Peters (1999), uses state level panel data that covers the period 1984 to 1996 to examine the impact of family caps on non-marital birth ratios (the ratio of non-marital births to total births). By estimating models with state fixed effects they rely on within state variation in family cap implementation and variation between states in the timing of family cap implementation to identify the effect of family caps and other welfare reform provisions. This approach avoids the pitfalls associated with reliance on interstate variation discussed above, but may lead to bias standard error estimates if nonmarital birth ratios are serially correlated (Bertrand, Duflo, and Mullainathan 1997). They find that family caps implementation is associated with large decreases in non-marital birth ratios. These results are consistent across age and racial groups.

In a study that is similar in design to that of Horvath and Peters, Kearney (2004) examines the impact of family cap implementation on the log number of births within a state. Like the estimates provided by Horvath and Peters, Kearney's estimates are identified on the basis of within state variation in family cap implementation and variation between states in the timing of implementation. Unlike Horvath and Peters, Kearny finds no systematic effects of family cap implementation on births. This result is robust across a variety of specifications and population groups.

Despite the similarities of the Kearney and Horvath-Peters studies, there are several notable differences that may account for the differences in results. First, there is a small difference in the time frames (1984 through 1996 versus 1989 through 1998) which may be significant because most family caps were implemented during or after 1996. Kearney has limited information on outcomes in family cap states post family cap implementation, but Horvath and Peters have even less information. Secondly, Kearney uses log births as the dependent variable as apposed to the non-marital birth ratio. This is potentially important as the log births variable is not mechanically affected by changes in marital behavior independent of

family cap effects, whereas the non-marital birth ratio variable is. One other difference is in the welfare reform variables that were used. Kearney uses the welfare reform waiver dates and classifications contained in a 1999 report of the of the President's Council of Economic Advisors (CEA 99), while Horvath and Peters used the Department of Health and Human Services as the source for their welfare variables.[1] It is clear that there are differences in waiver dates and classifications between these studies, but is not clear whether there are differences in family cap implementation dates and coding. Neither study can isolate the effect of family caps on welfare recipients, but Kearney does a better job of examining the impact of family caps on groups that are more likely to be affected by such policies. For example, Kearney estimates some models where just higher order non-marital births are used as the dependent variable. These models are estimated by race and education level (less than high school versus more than high school). That the impact of family caps is small and not statistically significant in specification using higher order births and estimated over samples of more disadvantaged women is telling.

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 examine the effects of policy (including family caps) on the living arrangements of low income families. They find that family caps are associated with decreases in single parenting among families that are more likely to be affected by the policy. They also find that family caps are associated with increases in two-parent families.

The estimates provided by Acs and Nelson should be interpreted with some caution. The NASF only samples respondents from 13 states and only 6 of these states had implemented family cap by 1999. More important, given the differences-in-differences-in-differences estimation strategy, only two of these states implemented a family cap between 1997 and 1999. These states were California and Florida. Florida effectively implemented a partial family cap in

[1] The author was unable to determine the source of the waiver variables used by Horvath and Peters (1999).

August 1997 and California effectively implemented a family cap in July of 1998. The implication of these implementation dates is that the estimates reported by Acs and Nelson will be identified entirely on the basis comparisons of changes living arrangements between groups thought to be affected and groups thought not be effected by family caps in California and Florida between 1997 and 1999. Furthermore, because Florida effectively implemented its partial family cap more than half way though 1997, it is not clear that it should be counted as a non-family cap state in 1997.

Several recent papers have examined the impact of family caps and other policies on outcomes other than, or in addition, to births (Levine 2002, Joyce et al. 2004). Using a differences-in-differences-in-differences framework in which differences in birth rates, abortion rates, and abortion ratios between women with no children and women with more than one child in family cap states are compared to these differences in non-family cap states, Joyce et al. (2004) find that family caps have no effect on births or abortions. Levine (2002) examines the impact of abortion and welfare policies on state births rates, abortion rates, pregnancy rates (the sum of the birth and abortion rate) using aggregate state-level data from the vital statistics system. Using individual level data from the National Survey of Family Growth he also examines the effect of these polices on sexual activity and contraceptive use. In most of the specification estimated by Levine the family cap coefficients are not statistically significant. Where the family cap coefficients approach statistical significance, they seem to be at odds with the predictions of economic theory. For example, family caps are associated with an increase in birth and pregnancy rates and these results are statistically significant in some of the specifications. Levine concludes that there is little evidence that welfare policy has any effect of fertility decisions.

In addition to the studies that make use of the natural experiment provided by incremental benefits that vary across states and over time, randomized evaluations of the family cap were conducted in Arkansas and New Jersey (Camasso et al. 1998a and 1998b; Turturro et al. 1998; Jagannathan and Camasso 2003; Jagannathan et al. 2004). Although the evaluation of Arkansas'

family cap provided little in the way of evidence of family cap effects, the results from the New Jersey evaluation indicated that there were statistically significant differences in the likelihood of a birth between women assigned to the treatment and control groups. The estimated effects are the largest for women entering New Jersey's AFDC program (Camasso et al. 1998a; Jagnnathan and Camasso 2003: Jagannathan et al. 2004). There are also racial differences with new Black welfare entrants showing a greater response to the treatment than their white and Hispanic counterparts (Jagannathan and Camasso 2003; Jagannathan et al. 2004).

While the experimental evidence from the New Jersey evaluation provides some evidence that family caps are an effective policy tool, many researchers remain unconvinced. Some observers have noted that there were severe problems in the implementation of the experimental design that make it very difficult, if not impossible, to draw conclusions as to the effectiveness of family caps (Loury 2000; Rossi 2000). In both the Arkansas and the New Jersey evaluations there is substantial evidence to suggest participants were confused about which treatment group they were assigned to and were not sure which program rules applied to them. For example, 62 percent of the control group recipients in the New Jersey evaluation could not identify themselves as belonging to the control group and it is doubtful whether they realized that they were not subject to the family cap (Camasso et al. 1998a).

Although the randomized evaluations in Arkansas and New Jersey offered the promise of providing a definitive answer to the question of whether family caps are effective in their aim of reducing fertility among welfare recipients, numerous questions concerning implementation made it very difficult to draw meaningful conclusions.

The existing non-experimental studies also have limitations. The biggest of these limitations is the use data that does not fully span the period of family cap implementation. For example, the data used by Horvath and Peters (1999), Kearney (2004) , Levine (2002), and cover the periods 1984 through 1996, 1989 through 1998, and 1985 through 1996 (or 1988 and 1995 in the case of the National Survey of Family Growth data used) respectively. The data used by

Joyce et al. (2004) and Acs and Nelson (2004) cut somewhat deeper into the family cap era (1999), but are limited in that they do not cover the full set of states.[2] Most states that implementated a family cap policy did so in 1997 or 1998. To the extent behavioral responses to family cap implementation is not immediate, data extending through 1998 or 1999 may not be sufficient measure the true effect of family cap policies. Moreover, with the exception of Acs and Nelson (2004) and Levine (2002), the existing studies of the effects of family cap rely on state-level panel data. While this is not a problem per se, this reliance on state-level panel data will lead to biased estimates if the dependent variable is non-stationary or serially correlated.

In this paper I address these limitations by using individual-level data gathered from the 2001 SIPP that allows for the effect of family cap policies to be studied over the period 1990 through 2000. Relative to other longitudinal micro data sources available to study the impact of family caps, the SIPP provides large sample sizes, and detailed information fertility histories, marriage histories, and program participation.

Family Caps and Welfare Reform

The last decade has seen unprecedented efforts at welfare reform. In the early 1990s the Department of Health and Human Services began granting states waivers to run experimental welfare reform programs. These waivers generally took the form of time limiting benefit receipt, requiring recipients to work, reducing the effective tax rate on earnings, reducing the number of recipients who are exempt from training associated with the Job Opportunities and Basic Skill Training program (JOBS), and making it easier for states to sanction recipients for failing to comply with JOBS requirements. Efforts at welfare reform culminated in September of 1996 when President Clinton signed the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA). This law replaced the Aid to Families with Dependent Children program (AFDC) with Temporary Assistance for Needy Families (TANF) block grants. In order to

[2] As noted above Acs and Nelson (2004) use data from the 1997 and 1999 NSAF which only identifies 13 states. Joyce et al. (2004) restrict their analysis to 24 states.

receive TANF block grants states must limit the lifetime receipt of TANF benefits to 60 months and require TANF recipients to work within 2-years of receipt. Although the PRWORA legislation mandated minimum time limits and minimum work requirements, it gave states much greater latitude in designing their public assistance caseloads.

One of the more controversial welfare reform provisions that was implemented by 21 states as part of pre-PRWORA waivers, and by 2 states as part of their TANF plans, is the family cap. Table 1 list the states with family caps along with some characteristics of their family cap provisions. The vast majority of states with family caps have full family caps that deny additional cash assistance to families that have additional children while on welfare. These states generally do not provide increases in cash assistance to families that have children while on welfare, but there are notable exceptions. For instance, most states exempt children born within the first 10 months of a welfare case from the family cap. Additionally, most states allow capped children to be counted as part of the families cash grant after a period of non-welfare receipt. The amount of time could be as long as 5-years or as short as 2-months. For the most part, the exceptions to these exemption policies are the states with flat benefit schedules or states that have partial family caps.

An important feature of the family caps is that their effects are moderated by the way that food stamp benefits are determined. Because the formula used to compute food stamp benefits taxes cash assistance at a marginal rate of 30 percent families with a capped child will have higher food stamp benefits than families of the same size without a capped child, all else equal. This means that the overall difference in resources available to families with, and without, capped children will be less than the difference in cash assistance. Additionally, there are a number of states that deny additional cash assistance to families when capped children are born, but make up this difference in cash assistance in the form of a voucher, paid to the family head or a third party, that can be used to make approved purchases.

Table 2 shows the increase in cash and in-kind resources associated with the birth of a second child in 2000 for the 23 states that implemented family cap during the 1990s. The first three columns show the increase in cash, in-kind and total resources, assuming that family caps were never implemented. The last 3 columns show the increase in resources available with family caps in place. That the food stamp benefit determination formula moderates the effect of the family cap is evident in Table 2. The difference between the total resources available to families with a capped child and families without capped child is substantially smaller than the difference cash resources available to these families. The source of this difference is the increased food stamped benefits available to families with capped children.

In attempting to assess the impact of family caps on the subsequent childbearing of welfare mothers it is important to recognize that family caps were not implemented in isolation. Most states that implemented family caps a part of a pre-PRWORA waiver also implemented other reform measures. Additionally, all states eventually implemented TANF programs that required recipients to work after a period of time and most states eventually set limits on the amount of time families could continue to receive aid.[3] Table 3 shows the correlations between the different welfare reform initiatives for the sample used in this analysis. In constructing the welfare reform measures in Table 3, all states are coded as having a work requirement after the implementation of their TANF plans. In addition, all states, with the exception of Vermont and Michigan, are coded as having a time limit after the implementation of their TANF plans.

Because family caps are correlated with other welfare reform initiatives, some of which may affect subsequent childbearing among recipient families, it will be important to provide some controls for these other reform initiatives in the analysis that follows. I experimented with a number of methods for characterizing each state's welfare policy environment, ultimately settling on a simple classification scheme that utilizes a family cap indicator, an indicator of a pre-

[3] Two states, Michigan and Vermont, have decided not to comply with the time limit provisions in PWRORA.

PRWORA (other than a family cap), and an indicator of TANF implementation. Attempts to more richly characterize state welfare policy environments by including the specific type of pre-PRWORA waivers implemented by states and characteristics of their TANF program did not substantively change the conclusions reached below, nor did they provide interesting insights into other welfare polices that might affect non-marital fertility.

Data

To construct the data used for this paper I merged individual level data from the 2001 SIPP with state-level information on benefit levels and welfare reform implementation dates. The SIPP is a longitudinal data set published by the United States Census Bureau. Once every four months (wave) SIPP participants are asked about their income, earnings, and program participation over the previous four months. In addition to the standard questionnaire that is administered every 4 months, there is one topical module questionnaire for each wave. These topical modules focus on obtaining retrospective, or more detailed information, about a particular areas of interest to researchers. In this paper I make use of the first two topical module files and the second wave core data file. The first topical module file (W1TMF) contains detailed information about each respondent's employment and recipiency history. The second topical module file (W2TMF) contains information on marriage, fertility, migration, education and work disability histories, as well as information on family relationships.

To construct the sample I first identified all black, white and Hispanic women who were potentially eligible for a second birth sometime between January 1990 and December 2000 based on the information contained in the W2TMF.[4] Because the W2TMF only contains the year of birth of each female respondent's first and last child, it is impossible to determine the precise month in which the two children were born. In pinpointing the precise timing of births I rely on the information contained in the second wave core data file (W2CDF). In cases where the

[4] A woman is assumed to be eligible for a second birth 9 months after giving birth to her first child.

W2TMF indicates that all children are living with their mother, the W2CDF should contain a record for each of the mother's children that indicates a year and month of birth. The birth years obtained from the W2CDF were checked against the information on the birth year of first and last child contained in the W2TMF to eliminate cases with twins and cases where there are apparent inconsistencies between the W2CDF and the W2TMF. In all there were 4,609 black, white, or Hispanic mothers in the W2TMF that indicated their first child was born during or after 1989 and that all of their children were living with them. One-hundred and seven of these cases were dropped because either the W2TMF or the W2CDF indicated the presence of twins. An additional 171 cases were dropped because there was some apparent inconsistency between the information on birth timing contained in the W2TMF and that contained in the W2CDF. In all I am left with 4,331 cases for which I can determine the start and end of second birth intervals with reasonable confidence.

In addition to providing information on the timing of births, the W2TMF also provides information on the timing of marriages. In particular, it provides the year of each respondent's first three marriages, making it possible to determine the earliest and latest date at which any women in the sample was first married. Because my interest is in estimating second non-marital birth intervals it is necessary to eliminate from the sample women who were married prior to the start of their second birth interval, and to determine dates of marriage for women who were not married at the start of their first birth interval. To be included in the primary sample for analysis a respondent must be never-married, or her first marriage must have occurred after the start of her second birth interval.[5] Including only 1,197 women who started their second birth intervals never-married between January 1990 and December 2000, and eliminating women who resided in states that are not uniquely identified in the SIPP,[6] women who resided in states that are not well

[5] Because the earliest potential date of marriages is used it is likely that I have falsely determined that some women are married at the start of their second birth interval.

[6] Main, Vermont, South Dakota, North Dakota, and Wyoming are not uniquely identified by the SIPP. Fourteen women were eliminated from the sample because they resided in one of these states.

represented in the sample,[7] women who moved between states subsequent to the time they became eligible for a second birth,[8] and women with missing welfare history information,[9] I am left with a sample of 902 women spanning 30,398 person months.

To determine welfare history I relied on information contained in the first 2001 SIPP topical module file. The information in this file makes it possible to determine the date at which a SIPP sample member first received AFDC/TANF. A respondent is coded as having a history of welfare receipt if the she is currently receiving welfare or has received welfare sometime in the past, regardless of whether she received welfare during her second birth interval. In all, 256 women (or 28 percent of the sample) report some history of welfare receipt.

Based on comparisons with administrative sources it appears as though welfare receipt may be significantly underreported in my sample. Administrative caseload numbers indicate that for much of the 1990s the number of families receiving AFDC/TANF exceeded 4 million per month and in both March and April of 1994 there were over 5 million families receiving AFDC/TANF. Most of these families were composed of single women with children. Historically about 45 percent of female-headed families received ADFC-Basic at any point in time. Estimates from the first two 2001 SIPP topical modules provide an estimate of 4.4 million women with children that ever received AFDC/TANF benefits.[10] Twenty-seven percent of never

[7] Women residing in Alaska, Delaware, Washington D.C., Hawaii, Idaho, Montana, Nevada, New Mexico, Rhode Island, and Utah were dropped because these states were not well represented in the sample. Forty-one women were dropped from the sample because they resided in one of these states.

[8] There were 228 women who changed state of residence between the time they became eligible for a second non-marital birth and the timing of second wave of the 2001 SIPP. These women were dropped from the analysis sample because, given the information supplied in the W2TMF, it is not possible to determine in which state they were living during each month of their second birth interval.

[9] There were 12 women for whom welfare histories were not available because they entered the SIPP during Wave 2, and thus did not respond to the Wave 1 Topical Module.

[10] This estimate was obtaining using a sample of women with children (as measured in the W2TMF and who were either receiving AFDC/TANF or reported having received AFDC/TANF at some point (as measured in the W1TM). This sample is similar to the primary analysis except that the analysis sample is limited to black, white, and Hispanic women who eligible for a second non-marital birth at some point between 1990 and the end of 2000, were never married at the start of their second birth interval, had all of their children living with them, did not have twins, had consistent fertility histories across the W2TMF and W2CDF, and did not reside in states that were excluded from the analysis (see footnotes 4 and

married mothers in the SIPP reported receiving AFDC/TANF at some point in the past. In all likelihood, a significant fraction of the women that are coded as having no history of welfare receipt, were welfare recipients at some point during the sample period. There are also other problems with the quality of response to the welfare history questions in the W1TMF. For example, many women report that they began receiving welfare when they were minors and substantially prior to the time their first child was born. Clearly these women misunderstood the intent of the questions.

The cumulative measure of welfare receipt that is being used in this analysis is very different from measures used in other large surveys and administrative sources, making it difficult to assess how bad the undercount problem is in the SIPP, both absolutely, and relative to other survey data sources. While it is difficult to make comparisons between the retrospective questions in the SIPP and other surveys, the problem of underreports of welfare receipt and welfare income are common across all national surveys and may be relatively larger in other surveys. In the March CPS, which ask respondents whether they received AFDC at anytime during the prior calendar year, the undercounting of welfare receipt is a serious problem. In the 1990s the ratio reflecting the number of single women who reported receiving AFDC in the CPS to the AFDC Caseload was between 0.44 and 0.61, with the lower numbers falling later in the decade.[11] These fractions are pretty low when you consider that the March CPS measures cumulative welfare use over the course of a year and the AFDC/TANF caseload numbers reflect the average monthly caseload during a year.

Because it is likely that some of the women in my sample who did not report receiving welfare were are former welfare recipients, it is appropriate not to restrict the analysis sample to women with some welfare history even though these women are most likely to be directly affected by a family cap. Rather I have chosen to provide descriptive statistics and estimates

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5). The estimate of 4.4 million women who ever received welfare is based on a sample of 1,562 women matched across the first two topical modules.

[11] Numbers are based on author's caculations.

across three samples; the entire simple of women who started a second non-marital birth interval between 1990 and the end of 2000, and sub-samples of these women who did and did not report receiving welfare benefits at some point in the past.

Sample means and standard deviations of the variables used in the analysis of this sample by welfare history are shown in Table 4. All means and standard deviations are calculated using the W2CDF person weights. Time varying variables (shown with a t subscript) were recorded during the first month that an individual was eligible for a second birth. Most of the variables in Table 4 are self explanatory, but some may require additional explanation. The variable *birth rate* is an estimate of the monthly non-marital birth probability over the first 48 months (4-years) of a second birth interval. The race-ethnicity categories were coded to be mutually exclusive by identifying non-Hispanic whites, non-Hispanic blacks, and Hispanics. *Benefit* is the maximum AFDC/TANF for a family of 2 in hundreds of January 2000 dollars. *Incremental benefit* is the increase in maximum AFDC/TANF benefits available upon the birth of a second child. In most states that have a family cap this increase in cash assistance will be zero. There are, however, several states with family caps in place that offer a partial increase in benefits upon the birth of a second child. For example, in Connecticut's benefits increase by $50 when a capped child is born regardless of the current family size. *Benefit sum* is the sum of maximum AFDC/TANF and residual food stamp benefits and *incremental benefit sum* is the increase cash and food stamp benefits available upon the birth of a second child.[12] The variable *family cap* indicates whether a state has a family cap in place, *other waiver* indicates the presence of statewide pre-PRWORA

[12] Because income from the AFDC/TANF benefits is taxed against the food stamp benefit, the benefit sum is not simply the sum of the maximum AFDC benefit and the maximum food stamp benefit: calculation of the residual food stamp benefit is required. The residual food stamp benefit is calculated as

$$Residual\ Food\ Stamp\ Benefit = MFSB - 0.30 \cdot \max\{MBEN - STDED, 0\}$$

where $MFSB$ is the maximum food stamp benefit, $MBEN$ is the maximum AFDC/TANF benefit, and $STDED$ is the standard income deduction for the food stamp program.

welfare reform waivers other than a family cap, and TANF indicates whether a TANF has been implemented.[13]

Despite the fact that the non-welfare history sample is likely to contain some current and former welfare recipients, there are notable differences between the welfare and non-welfare samples. The welfare sample looks more disadvantaged along a number of dimensions. Women in the welfare sample have higher monthly second birth rates, a higher fraction of them were teen mothers, they have lower levels of educational attainment, and are more likely to be black (rather than white or Hispanic). These are the sorts of differences between the welfare and non-welfare samples that we would expect to see if the information of welfare histories were accurate.

Figure 1 shows the Kaplan-Meier estimate of the cumulative probability of a non-marital birth by the presence of a family for the entire sample in the panel (a) and by welfare history in panels (b) and (c). The crude estimates in Figure 1 indicate very little in the way of a family cap effects, but because the estimates presented in Figure 1 do not control for demographic variables, policy variables (including other welfare reform initiatives), or other factors that vary across states or over time that are correlated with non-marital fertility, it is impossible to make any firm determination as to whether family caps reduce non-marital fertility. To sort out whether family caps have an effect on non-marital fertility a richer statistical model of fertility is needed. This model is described in the next section.

Estimation

The primary approach taken toward estimation in this analysis is attributable to Prentice and Gloeckler (1978). They provide an extension to the Cox proportional hazard model that allows for estimation of grouped or discrete duration data in a manner that does not require making functional form assumptions concerning the parametric form of the baseline hazard. In the analysis that follows there are two ways in which a non-marital birth interval can end. A non-

[13] 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.

marital birth interval can end because of the birth of a second child. Alternatively, a non-marital birth interval can end in marriage. Because I am primarily interested in estimating the likelihood of a second non-marital birth, and marriages to never-married women with some welfare exposure are rare events, I chose to treat non-marital birth intervals that end because of a marriage as censored.

Consider the chance that an individual's second birth falls in the interval $[t, t+h)$ conditional on the individual having not given birth until time t. The limit of this conditional probability as $h \to \infty$ is known as the birth rate. More formally

$$\lim_{t \to \infty} \frac{\Pr(t <= \tau < t+h \mid \tau > t)}{h} = \lambda(t)$$

The proportional hazard assumption amounts to a functional form restriction on $\lambda(t)$. More specifically, the proportional hazards model assumes that

$$\lambda(t) = \lambda_0(t) \cdot \exp\left(x(t) \cdot \beta\right)$$

where the function $\lambda_0(t)$ is know as the baseline hazard, $x(t)$ is a vector of covariates that are allowed to depend on time, and β is a vector of parameters. Next, consider the probability that an individual has a second birth interval of length t or greater. This probability is known as the survivor function. In the proportional hazard model the survivor function takes the form

$$S(t) = \exp\left(-\int_0^t \lambda(u)du \right).$$

Assuming the vector of covariates is constant over the interval $[j, j+1)$ for all j, $S(t)$ can be rewritten as

$$S(t) = \exp\left(-\sum_{j=0}^{t} \int_j^{j+1} \lambda(u)du \right) = \exp\left(-\sum_{j=0}^{t} \exp\left[\gamma(j) + x(j) \cdot \beta\right] \right)$$

where

$$\gamma(j) = \ln\left[\int_{j}^{j+1} \lambda_0(u)du\right].$$

Once the survivor function is known, the likelihood functions follows straightforwardly. Consider a sample consisting of n birth intervals. Let T_i denote the length of the $i'th$ individual's birth interval. These birth intervals can either be uncensored or censored. For censored birth intervals I adopt the convention that the person is known not to have given birth until the end of the $T_i'th$ period. In addition to the length of birth intervals a sequence of explanatory variables $x(t)$, $(t = 1,2..,T)$, are observed for each individual in the sample. Letting $\delta_i = 0$ for all right censored observations and $\partial_i = 1$ for all observations with completed birth intervals the likelihood function is

$$L(\gamma,\beta) = \prod_{i=1}^{n}\left[1 - \delta_i \exp\{-\exp[\gamma(T_i) + x_i(T_i)\cdot\beta]\}\right]$$

$$\times \prod_{t=1}^{T_i-\delta_i}\left[\exp\left\{-\sum \exp[\gamma(t) + x_i(t)\cdot\beta]\right\}\right]. \qquad (1)$$

Note that the first part of equation (1) is the probability of giving a birth in period T_i, assuming the birth interval was not censored, while the second part of equation (1) is the probability of that a birth interval lasts for at least $T_i - \delta_i$ months. Given (1) the log likelihood function is provided by

$$l(\gamma,\beta) = \sum_{i=1}^{n}\left\{\ln\left[1 - \delta_i \exp\{\gamma(T_i) + x(T_i)\cdot\beta\}\right] - \sum_{t=1}^{T_i-\delta_i}\exp\{\gamma(t) + x_i(t)\cdot\beta\}\right\}. \qquad (2)$$

For the purpose of this analysis, several restrictions on $\gamma(\cdot)$ will be imposed. The need for these restrictions is the result of limitations in the SIPP data. The fact that the data set is fairly small and the probability of a birth in any given month is low means that it will be impossible to let $\gamma(\cdot)$ vary every period. The approach taken in this analysis is to let $\gamma(t)$ take the form of a step function where there is a potential for a step every year over the first 4-years of a birth interval

and a step every 2-years the second 4-years of a birth interval. Another feature of the data is that individuals are followed for a maximum of 96 months. This means that it will be impossible to make any inferences about baseline birth rates after 8-years.

The model outlined above has several advantages relative to alternative models such as dynamic probit and logit models. The first advantage is one of interpretation. Because the monthly hazard rates are in the form $\lambda(t) = \exp(\gamma(t) + x(t) \cdot \beta)$ the coefficients can be interpreted as the marginal fractional changes. The other major advantage of this model relative to alternatives such as dynamic probit and logit is one is that in the model outlined above it is very easy to produce estimates of the impact of the independent variables on annual birth probabilities and to determine standard errors for these estimates. Such estimates would be much harder to obtain using a dynamic probit or logit model.[14]

Results

Several approaches were taken to identify the affect of family caps on non-marital fertility among welfare-affected mothers. The first approach is to let family caps affect fertility indirectly by changing the incremental cash and in-kind benefits associated with having an additional child. The assumption here is that the effect of implementing a family caps is purely incentive based. When a family cap is implemented the incremental benefit drops, and the incentive to have an additional child while on welfare is reduced. An important feature of family caps is that the effect of a full family cap, which denies any additional cash assistance in the event of the birth of another child, will be moderated by the food stamp benefit determination formula. Because a family caps affects cash and in-kind benefit determination, and it is not clear how much recipients value in-kind transfers relative to cash assistance, I estimate some specification

[14] All the models estimated in the result section were also estimated using dynamic logit and probit models. In practice there was very little difference in the implied effects between the estimates from the model described here and those from the dynamic logit and probit models.

in which the effect of family caps are allowed to feed through AFDC/TANF benefits and some others where family caps affect behavior through the benefit sum.

In addition to the specifications in which the family caps affect fertility indirectly through cash and in-kind benefit determination, I also estimate a specification that allow family caps to affect non-marital fertility directly. Implementation of a family caps policy on the state level may send a strong normative message to welfare recipients (or other single women) that it is unacceptable to have additional children while on welfare. This message may have an effect above and beyond that provided by monetary incentives.

Estimates of the effect of policy variables the non-marital birth rate among all never-married women with one child are shown in Table 5. Columns (1) and (3) allow the effect of the family cap to feed directly through the incremental increases in cash and in-kind assistance associated with having another child. In columns (2) and (4) the effect of the family cap is allowed to affect fertility decisions by influencing the percentage increase in resources available to families when another child is born. In column (5) the family cap is allowed to affect fertility decisions directly. Because the coefficients estimates correspond to a proportional hazards model they can be interpreted as the fractional change in the monthly birth rate associated with a marginal increase in the corresponding independent variable.

In addition to the variables shown, all specifications contain duration effects, age, age squared divided by 100, a dummy variable indicating teen motherhood, a set of dummy variables indicating whether the respondent is white, black or Hispanic, a dummy variable for living in a metropolitan area, and a set of education dummy variables that indicate the highest level of education attained by a respondent; less than high school, high school, or some post-high school training. In an attempt to control for differences in unmeasured state-level factors that may affect fertility and are potentially correlated with the policy variables, state-fixed effects are included in all specifications. These unmeasured factors may include, but are not limited to, the availability of abortion services, differences in child support enforcement across stated, differences in levels

of child care subsidization and/or availability, and location specific differences in preferences affecting demand for children. Because nominal AFDC/TANF benefit levels did not change much over the course of the 1990s, the state fixed effects also serve as a proxy for the baseline level of welfare generosity across states. In addition, year effects are included in Table 5 specifications to control for changes policy or exogenous behavioral changes that occurred at the national level throughout the 1990s that may affect non-marital fertility.[15]

Examining Table 5, there is no evidence that non-marital fertility decisions respond to financial incentives. Neither the incremental benefit variables nor the family variable variables are statistically significant at standard confidence levels. Furthermore, all of the coefficients have signs that are consistent with low incremental benefits and family caps being associated with increased non-marital birth rates. Although the effects of the waiver and TANF variables are consistently negative they are not statistically significant at standard significance levels. High state monthly unemployment rates are associated with increased non-marital fertility. This positive unemployment is consistent with the hypothesis that favorable labor markets constitute an opportunity cost to having additional children.

The estimates shown in Table 5 may be indicative of a lack of family cap and other policy effects. The estimates may also reflect the fact that the models were estimated over a sample of all never-married women with one child, irrespective of current or past welfare receipt. From the perspective of economic theory, family cap policies may lead to reduced fertility among welfare recipients. Family caps may also lead to reduced welfare receipt if the family cap makes welfare a less attractive option, or if women planning on having a child leave welfare in an attempt to avoid having the child excluded from the benefit calculation. It would be ideal to estimate a model in fertility and welfare receipt were modeled jointly. Unfortunately, estimating such a model would require data that does not exist at the present time. To examine the extent to

[15] One policy change with implications for subsequent non-marital fertility that occurred on the national level was a change to the structure of the Earned Income Tax Credit (EITC) that allowed for more generous subsidies for families two or more children as compared with one child beginning in 1991.

which welfare history status has an impact on the effect of family cap policies I estimated separate models for women with a history of welfare receipt and women without. These models will not shed insight into how joint decisions about welfare use and fertility are made, but they will answer they question as to whether women who are pre-disposed to use welfare are affected by family cap policies to greater extent than those with no reported exposure to the welfare system.

Estimates of the effect of the policy variables on birth rates for women with a prior history of welfare receipt and one child are shown in Table 6. In contrast to the estimates over all women, the estimates of the impact of the family cap variables on second non-marital birth rates among former and current welfare recipients are not blatantly inconsistent with the hypothesis that family cap policies reduce non-marital births. None of the family cap variables are have statistically significant effects, but all of the coefficient estimates have signs that are consistent with lower incremental benefit levels and family cap implementation leading to reduced non-marital births. Neither the waiver nor TANF indicators have a statistically significant impact on non-marital births. Higher state monthly unemployment rates are associated increased fertility among women on welfare, but this effect is smaller than it was for the entire sample of single women and is not statistically significant.

In comparing these estimates to those reported elsewhere it is important to account for differences in the unit of measurement. While most other studies examine the effect of incremental benefits on the probability of a subsequent non-marital birth in a calendar year, the estimates reported in Table 6 show the effects of incremental benefits and family cap implementation on monthly birth rates. To assess how the estimates reported in Table 6 compare with estimates from other studies it is useful to determine how the predicted changes in monthly birth rates correspond to changes in annual birth rates. In Table 7, the Table 6 parameter estimates are used to show the effect of family cap implementation on annual birth probabilities for the average sample member residing in the average state by Table 6 specification and birth

interval year. The estimates reported in Table 7 imply that family caps have large effect on second births probabilities among women with some prior exposure to the welfare system. The effects of family cap implementation reported in Table 7 range from a 17 percent to a 33 percent reduction in annual second birth probabilities, depending on the specification and birth interval year. Even though the estimates of the impacts of family caps are large, there is not a single cell in Table 7 where the hypothesis that family caps have no effect on second births among women with a history of welfare use can be rejected.

How do these estimates compare with those reported by other researchers? As noted in the introduction many of the studies that examine the relationship between incremental benefits and higher order fertility decisions find results that are imprecisely estimated or inconsistent with economic theory. Among those studies that do report positive effects of incremental benefits on subsequent fertility decisions, the largest effects are reported by Fairlie and London (1997) and Argys et al. (2000). Using data from the 1990 SIPP, Fairlie and London estimate that family cap implementation, feeding through the incremental cash benefits, would decrease the annual probability of giving birth by approximately 60 percent. Although this estimate is large it is not statistically significant at standard significance levels. Argys et al., using data from the National Longitudinal Survey of Youth (NLSY) estimate that the implementation of a full family cap, feeding through the incremental benefit sum, would reduce the annual probability of a subsequent non-marital birth from 0.120 to 0.087, a 25 percent decrease.[16]

The only studies that examine that examine the effect of family caps directly, utilize samples from comparable populations, and find negative effects of the family cap on subsequent births come from experimental analysis of New Jersey's family cap policy (Camasso et al. 1998a and 1998b; Turturro et al. 1998; Jagannathan and Camasso 2003; Jagannathan et al. 2004). Estimates from these studies indicate that statewide implementation of New Jersey's family cap

[16] Computed from author's calculations of the impact of reducing the incremental benefit sum by $50 on the probability of a birth in a year in a specification with state fixed effects (see Argys et al. 2000, page 584).

would have reduced subsequent births among current and former welfare recipients by approximately 10 percent. Subgroup analysis of the New Jersey data reveals that the family cap effects are concentrated among blacks with new AFDC cases (Jagannathan et al. 2004). For this group family cap implementation is estimated to reduce birth rates by nearly 20 percent.[17]

Estimates of the effect of the policy variables on birth rates for women with no reported history of welfare receipt and one child are shown in Table 8. Because of the underreporting of welfare receipt in the SIPP we cannot be sure that women in this sub-sample never received welfare. What is clear, however, is that this sub-sample looks significantly less disadvantaged than the recipient sub-sample (See Table 4). The estimates of the incremental benefit variables indicate that lower incremental benefits are associated with higher non-marital birth rates, but with one exception, these effects are imprecisely estimated and are not statistically significant. The estimate of the direct effect of a family cap on second birth rates (column (5)) indicates that family cap implementation is associated with higher non-marital birth rates among women not reporting current or prior welfare use. This estimated family cap effect is statistically different from zero at a 5% significance level. While none of the other welfare policy variables are statistically significant in Table 8, the unemployment rate impact on second birth rates is large, positive and statistically different from zero. Higher unemployment rates are associated with higher second birth rates. This unemployment rate effect makes sense if the unemployment rate is inversely to the opportunity cost of having a second child.

To get a sense of how the estimates incremental benefit and family cap variable estimates in Table 8 relate to annual birth probabilities, I simulated the effect of family cap implementation annual birth probabilities during the first 4-years of a birth interval. These simulations are shown in Table 9. As with the simulations shown in Table 7, the Table 9 simulations are based on the

[17] Given the large standard errors in Tables 7 it would be difficult to detect family cap effects in the range of -10 to -20 percent. Assuming the true effect of family cap implementation is to the annual birth rate among current and former welfare recipients by -10 percent, the statistical power of a two tailed hypothesis test (with a significance level of 0.10) ranges from 0.10 to 0.14. Assuming a true effect of -20 percent, the range of statistical power is from 0.18 to 0.32.

average sample member residing in the average state. According to the Table 9, family cap implementation would increase the annual second birth rate for the average sample member by anywhere between 25 and 90 percent, depending on the specification and the birth interval year. Although these impacts are in some cases quite large, none of them are statistically significant at standard confidence levels.

It is difficult to know what to make of the estimates in Tables 6 and 8. On one hand the signs and magnitudes of the estimates of the incremental benefit and family cap variables in Table 6 are consistent with family caps having an incentive effect. The coefficients in these specifications are not statistically significant, but the model was only estimated over 256 women. On the other hand, the estimates in Table 8 are wildly inconsistent with any reasonable set priors. Family cap implementation may be neutral with respect to non-marital fertility, but it should not have the effect of promoting second births among single women. This is especially true when you consider that the welfare undercount problem in the SIPP means that some of the women in this non-welfare sample may well have been receiving welfare over the course of their second birth interval. I am left with the tentative conclusion that the large effects reported in Table 8 are the result of spurious correlation between the family cap implementation and non-marital fertility.

To try to get at what is driving the results in Table 8 I estimated a number of alternative specifications. To determine the extent to which more educated women in the non-recipient sample were contributing to the Table 8 results I estimated a specification in which women with post high school training were excluded and with a re-scaled set of weights designed to make non-recipient sample look more like the recipient sample. While the estimates of the incremental benefit and family cap variables were slightly lowered from those shown in Table 8, they were still quite high and the signs of the coefficients remained inconsistent with any reasonable explanation of how family caps work.

To examine whether the results in Tables 8 were due to spurious correlation between family cap implementation and non-marital fertility I estimated the Table 8 specification with

state time trends.[18] These trends should net out any state-specific trends in non-marital fertility that may have been erroneously attributed to the incremental benefit and family cap variables. Adding the state specific time trends actually increased the magnitude and significance levels of the incremental benefit and family caps variables relative to Table 8 while retaining the signs.

I also estimated both the Table 6 and Table 8 specifications without the state effects. This allows between and within state variation in the incremental benefit and family cap variables to be used in the identification of the coefficients. Because roughly half of the variation in these variables is between-state, excluding the state effects allows substantially more variation to be used. When state effects and excluded from the Table 6 and 8 specifications the magnitudes of the incremental benefit and family cap variables diminish. Additionally, none of these variables are statistically significant at standard confidence levels.

Another hypothesis concerning the odd results in Table 8 is that the other waiver variables do not do an adequate job of controlling for important characteristics of state AFDC programs. To examine this hypothesis, the Table 6 and Table 8 models were estimated with alterative controls for state welfare program characteristics. Replacing the *other waiver* variable with specific characteristics of state's AFDC waivers produced estimates very similar to those shown in Tables 6 and Table 8.[19] This result is not surprising given that most state implemented their family cap policies as part of their TANF programs.

One final hypothesis concerning the results in Table 8 is that states implemented family caps in response to increases in higher order fertility. If states adopted family cap policies in response to increasing fertility, and family caps are ineffective in reducing the incidence of higher order births to single women, then birth rates in family caps states would increase more rapidly in family cap states than in non family cap states after the implementation of the cap.

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[18] I also estimated the Table 6 specification with state time trends added but the additional parameters were a stretch for the data.

[19] The other waiver variable was replaced with controls for earnings disregard waivers, JOBS waivers, JOBS sanction waivers, work requirement waivers, earnings disregard waivers, and time limits waivers.

To examine the plausibility of this hypothesis I coded a variable that indicate\s whether a state *ever* implemented a family cap. I then created a series a variables by interacted this family cap state indicator with the year effects. This series of ever a family cap state (=1) – year interaction terms was included in a Table 5 like specifications which were estimated over only the person months in which family caps were not in place.[20] I then compared the trend in year effects between the states that eventually implemented family caps and states that never implemented family caps. If states implemented family caps in response to rising second birth rates, then the trend in year effects for the states that eventually implemented family caps would be increasing, relative to the trend in year effects for states that never implemented a family caps. I did not observe this pattern. Indeed, differences between the trends were small and not statistically different from zero.

Limitations

The above analysis is based on the best non-experimental micro-level data currently available to study the effects of family cap implementation, but it is not without its limitations. Chief among these limitations are the high standard errors associated with the coefficients estimates on the incremental benefit and family cap variables, and the resulting low power of test used to determine the determine the statistical significance of family cap effects. The standard errors are large enough that it likely that only very large family cap effects would be detectable with any degree of frequency. Using the column 5, Table 6 model for illustrative purposes, and assuming the true effect of a family cap is to reduce births by 10 percent on a monthly basis, the statistical power of a two tailed hypothesis is merely 0.08. If the effect of a family cap implementation was to reduce births by 20 percent on a monthly basis the power of a two tailed hypothesis test at a significance level of 0.10 is roughly 0.13. An analysis of the power curve

[20] For example, California implemented a family cap in July 1998 and Illinois implemented a family cap in March 1996. The regressions were run using only person months before July 1998 and March 1996 in California and Illinois respectively.

indicates that it is not until the family cap effect reaches minus 60 percent that the statistical power of a two-tailed test with $\alpha = 0.10$ approaches 0.50.

While these results are troubling they are fairly typically of prior studies that use similar data and methods. For example, Fairlie and London (1997) report a family cap effect (on the likelihood of a nonmarital birth) of minus 60 percent that is not statistically significant at the 0.10 level. Acs (1996) appears to have encountered similar difficulties, albeit with smaller samples sizes. That more recent studies have not been characterized by relative imprecise estimates may well be related to the fact that they primarily rely on state-level panel data as apposed to micro data (Horvath and Peters 1999; Kearny 1994; Levine 2002; and Joyce et al. 2004).[21]

Despite the imprecision of the estimates the results presented in this paper are still valuable. They do provide bounds, albeit wide ones, on the effect of family cap implementation. They also provide some balance to a literature that has increasingly relied on state-level panel data, a data type not without limitations, to provide scholars and policy makers with information about the effect of family cap policies. Lastly, results presented in this study suggest if new sources of micro data must be developed if more is to be learned about the effect of family cap policies, and other financial incentives, on nonmarital fertility. The SIPP might be the best currently available data sources to study these issues, but it is apparently not good enough to generate very precise estimates.

Conclusions

Between December of 1992 and the present date 23 states have implemented full or partial family caps. Family caps seek to reduce fertility among welfare recipients by denying additional cash assistance to welfare recipients that have additional children while on welfare. A necessary condition for family caps to be an effective policy tool is that welfare recipients

[21] Some of the estimates in Levine (2002) are produced using micro data from the National Survey of Family Growth. These estimates relate family cap implementation, and other policy variables, to the likelihood of sexual activity, unprotected sexual activity, and the failure to use contraception at least intercourse. These measures are clearly related to the incidence of births, but they are not directly comparable.

respond to financial incentives in making decisions that affect subsequent fertility outcomes. Although there are numerous studies that examine the link between the generosity of AFDC benefits and subsequent childbearing decisions, a firm link between the two has not been established.

In this paper I use data from the 2001 panel of the SIPP to examine whether or not welfare affected mothers respond to the incentive provided by the incremental increase in cash and in-kind assistance associated with having another child in making decisions that affect subsequent child bearing. With respect to the previous, non-experimental studies, this study has the distinct advantage of using individual level data from a period of time in which many states were implementing or already had family caps in place. The primary advantage of using data that spans the period of family cap implementation is that it provides for additional intrastate variation in the incremental benefits available to women who have child while on welfare is introduced, making it possible to estimate models with state fixed effects that control for unmeasured factors that vary across states and influence fertility decisions.

The most careful reading of the estimates presented in that there is no evidence that family caps influence the subsequent fertility decisions of unwed mothers. The signs of the coefficient estimates obtained over a sample of never-married women with a self reported history of welfare use were consistent with the notion that family caps reduce non-marital births among women in this population. However, in these specifications, none of the coefficients on the incremental benefit and family cap variables were statistically significant. Estimates obtained over a sample of never-married women with no self reported history of welfare use indicate that lower incremental benefits and family cap implementation are associated with increased second birth rates among. For these women, a few of the coefficient estimates are statistically significantly different from zero at standard confidence levels. These estimates raise concerns as they suggest the possibility of spurious correlation between family cap implementation and non-marital fertility.

The conclusions reached by this research are consistent with much of the recent work on the effects of family caps on non-marital fertility. For example, using different sources and types of data Kearny (2004), Joyce et al. (2004), and Levine (2002) all conclude that family caps have no effect on non-marital fertility. In the only other study that uses individual-level data, Levine (2004) also estimates that family caps have a positive effect on non-marital fertility. What the findings from this most recent round of quasi-experimental research suggest is that family cap policies may save states money, but that their net effect on the wellbeing of affected families is negative.

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Table 1
Overview of State Family Cap Policies

State	Full Family Cap	Partial Family Cap	Voucher	Implementation Date [a]
Arizona	X	__	__	September 1996
Arkansas	X	__	__	May 1995
California	X	__	__	July 1998
Connecticut [c]	__	X	__	November 1996
Delaware	X	__	__	January 1998
Florida [d]	__	X	__	August 1997
Georgia	X	__	__	November 1994
Idaho [b]	X	__	__	July 1997
Illinois	X	__	__	October 1996
Indiana	X	__	__	March 1996
Maryland	__	__	X	January 1997
Massachusetts	X	__	__	September 1996
Mississippi	X	__	__	August 1996
Nebraska	X	__	__	September 1997
New Jersey	X	__	__	August 1993
North Carolina	X	__	__	May 1997
North Dakota	X	__	__	May 1998
Oklahoma	__	__	X	August 1997
South Carolina	__	__	X	August 1997
Tennessee	X	__	__	January 1998
Virginia	X	__	__	May 1996
Wisconsin [b]	X	__	__	January 1996
Wyoming	X	__	__	November 1997

Sources: Department of Health and Human Services Office of Assistant Secretary for Planning and Evaluation (ASPE) website (http://aspe.hhs.gov/search/hsp/Waiver-Policies99/policy_CEA.htm), the State Policy Demonstration Project website (http://www.spdp.org), and the Urban Institute's Welfare Rules Database.

[a] The implementation date is the month year in which families residing in each state could first have capped children. Because most states have an exemption for families that were expecting children when the family cap was enacted, this date is typically the reported date of implementation plus 10 months. The exceptions to this rule are Wisconsin and Idaho, which have flat benefit schedules.

[b] Idaho and Wisconsin have flat benefit schedules

[c] Connecticut increases benefits by $50 when a capped child is born.

[d] Florida increases benefits by half of the normal benefit increment when a capped child is born.

Table 2

Increase in Cash and In-kind Resources Associated with the Birth of a Second Child (2000 Benefits Data)

State	Increase in Cash Assistance without Cap	Increase in In-kind Assistance without Cap	Increase in Total Resources without Cap	Increase in Cash Assistance with Cap	Increase in In-kind Assistance With Cap	Increase in Total Resources With Cap
Arizona	72	79	151	0	101	101
Arkansas	42	88	130	0	101	101
California	121	65	186	0	101	101
Connecticut	123	64	187	50	86	136
Delaware	68	81	149	0	101	101
Florida	62	82	144	31	92	123
Georgia	45	88	133	0	101	101
Idaho	0	101	101	0	101	101
Illinois	99	71	170	0	101	101
Indiana	59	83	142	0	101	101
Maryland	89	74	163	0	163	163
Massachusetts	93	73	166	0	101	101
Mississippi	24	94	118	0	101	101
Nebraska	71	80	151	0	101	101
New Jersey	102	70	172	0	101	101
North Carolina	36	90	126	0	101	101
North Dakota	94	73	167	0	101	101
Oklahoma	67	81	148	0	148	148
South Carolina	42	88	130	0	130	130
Tennessee	43	88	131	0	101	101
Virginia	60	83	143	0	101	101
Wisconsin	0	101	101	0	101	101
Wyoming	20	95	115	0	101	101
Average State [a]	71	81	152	2	105	106

Sources: Department of Health and Human Services Office of Assistant Secretary for Planning and Evaluation (ASPE) website (http://aspe.hhs.gov/search/hsp/Waiver-Policies99/policy_CEA.htm), the State Policy Demonstration Project website (http://www.spdp.org), and the Urban Institute's Welfare Rules Database.

[a] In 2000 the average state's AFDC/TANF benefit for a family of two was $339 and the average state's benefit sum for a family of two was $515.

Table 3
Sample Correlations Between Welfare Reform Initiatives

	Family Cap	Earnings Disregard	JOBS Eligibility	JOBS Sanction	Work Requirement	Time Limit
Family Cap	1.0000	___	___	___	___	___
Earnings Disregard	0.2599	1.0000	___	___	___	___
JOBS Eligibility	0.0327	-0.0853	1.0000	___	___	___
JOBS Sanction	0.0884	-0.0772	0.7451	1.0000	___	___
Work Requirement	0.4174	0.6523	-0.2133	-0.2670	1.0000	___
Time Limit	0.5617	0.4730	-0.0001	0.0681	0.7828	1.0000

Table 4
Sample Means and Standard Deviations [a]

Variable	All Single Women (*N=902*)	Welfare History (*N=256*)	No Welfare History (*N=646*)
Birth rate (Average monthly non-marital birth probability over the first four years of the second birth interval)	0.0103 (0.0007)	0.0140 (0.0015)	0.0089 (0.0007)
Welfare history (=1)	0.2746 (0.4466)		
Age	21.8343 (4.9943)	20.2652 (3.6090)	22.4284 (5.3100)
Teen mother (=1 if AGE<18 when first child was born)	0.2096 (0.4073)	0.2815 (0.4506)	0.1824 (0.3865)
High school dropout$_t$ (=1)	0.2212 (0.4153)	0.2874 (0.4534)	0.1962 (0.3974)
High school graduate$_t$ (=1)	0.3577 (0.4796)	0.3513 (0.4783)	0.3601 (0.4804)
Some college$_t$ (=1)	0.4210 (0.4940)	0.3613 (0.4813)	0.4437 (0.4972)
White and non-Hispanic (=1)	0.4623 (0.4988)	0.3604 (0.4811)	0.5008 (0.5003)
Black and non-Hispanic (=1)	0.3605 (0.4804)	0.4909 (0.5010)	0.3111 (0.4633)
Hispanic (=1)	0.1772 (0.3821)	0.1486 (0.3564)	0.1880 (0.3910)
Metro Resident (=1)	0.7769 (0.4165)	0.7976 (0.4026)	0.7691 (0.4217)
Benefit$_t$ (Maximum AFDC/TANF benefit for a two-person family in hundreds of 2001 dollars)	3.5639 (1.5220)	3.8390 (1.5888)	3.4598 (1.4811)
Incremental benefit$_t$ (The increase in AFDC/TANF benefits associated with the birth of a second child in hundreds of 2001 dollars)	0.6742 (0.4837)	0.7518 (0.5072)	0.6448 (0.4857)
Benefit sum$_t$ (The sum of the AFDC/TANF and Food Stamp benefits for a two-person family in hundreds of 2000 dollars)	5.4080 (1.0766)	5.6139 (1.1238)	5.3300 (1.0486)

(Table continues)

Table 4 (continued)
Sample Means and Standard Deviations

Variable	All Single Women (*N=902*)	Welfare History (*N=256*)	No Welfare History (*N=646*)
Incremenal benefit sum_t (The increase in AFDC/TANF and Food Stamp benefits associated with the birth of a second child in hundreds of 2000 dollars)	1.5438 (0.3516)	1.6001 (0.3636)	1.5225 (0.3449)
$Famcap_{tt}$ (=1)	0.2280 (0.4198)	0.1956 (0.3974)	0.2402 (0.4276)
Other $reforms_{tt}$ (=1 if the state in which a respondent resides has a statewide waiver other than a family cap	0.1347 (0.3417)	0.1563 (0.3639)	0.1266 (0.3328)
$TANF_t$ (=1)	0.4098 (0.4921)	0.3140 (0.4650)	0.4460 (0.4975)
Unemployment $rate_t$ (State monthly unemployment rate)	5.5618 (1.5445)	5.8524 (1.5764)	5.4517 (1.5190)

[a] All means are weighted using W2TMF person weight.

Table 5

*Estimates of AFDC/TANF Policy Parameters on the Non-marital Birth Rate (N=902, N*T=30,398, Robust Standard Errors in Parenthesis)* [a]

Variable	(1)	(2)	(3)	(4)	(5)
Incremental benefit (Increase in cash benefits available upon the birth of a child in 100s of $)	-0.1005 (0.2652)	___	___	___	___
Benefit increment percent (Percent increase in cash benefits available upon the birth of a child)	___	-0.0054 (0.0103)			
Incremental benefit sum (Increase in cash and food stamp benefits available upon the birth of a child in 100s of $)	___	___	-0.0913 (0.3799)	___	___
Benefit sum percent (Percent increase in cash and food stamp benefits available upon the birth of a child)	___	___	___	-0.0056 (0.0231)	
Family cap (=1 if the state has a family cap)	___	___	___	___	0.2524 (0.2464)
Waiver (=1 if the state has a waiver other than a family cap)	-0.0018 (0.2523)	-0.0008 (0.2585)	-0.0048 (0.2635)	-0.0056 (0.2640)	0.0020 (0.2558)
TANF (=1 if state has implemented TANF)	-0.1802 (0.3817)	-0.1874 (0.3822)	-0.1796 (0.3814)	-0.1841 (0.3816)	-0.2061 (0.3845)
Unemployment rate (State monthly unemployment rate)	0.2132** (0.1048)	0.2097** (0.1027)	0.2102** (0.1051)	0.2086** (0.1030)	0.2142** (0.1024)
$-\ln(liklihood)$	1567.57	1567.49	1567.62	1567.62	1567.05

[a] Estimates are weighted using the W2TMF person weights. In addition to the variable shown all specifications include controls for age (and its square), race-ethnicity, metro status, duration effects, month effects, year effects and state effects.

** Statistically significant at the 5-percent level

* Statistically significant at the 10-percent level

Table 6

*Estimates of AFDC/TANF Policy Parameters on the Non-marital Recipient Birth Rate (N=256, N*T=9,244, Robust Standard Errors in Parenthesis)*

Variable	(1)	(2)	(3)	(4)	(5)
Incremental benefit (Increase in cash benefits available upon the birth of a child in 100s of $)	0.3497 (0.3815)	___	___	___	___
Benefit increment percent (Percent increase in cash benefits available upon the birth of a child)	___	0.0161 (0.0144)			
Incremental benefit sum (Increase in cash and food stamp benefits available upon the birth of a child in 100s of $)	___	___	0.5359 (0.5472)	___	___
Benefit sum increment percent (Percent increase in cash and food stamp benefits available upon the birth of a child)	___	___	___	0.0415 (0.0324)	
Family cap (=1 if the state has a family cap)	___	___	___	___	-0.3095 (0.3772)
Waiver (=1 if the state has a waiver other than a family cap)	0.0365 (0.3982)	0.0612 (0.3891)	0.0297 (0.3990)	0.0247 (0.3978)	0.0927 (0.3880)
TANF (=1 if state has implemented TANF)	-0.1524 (0.5750)	-0.1329 (0.5706)	-0.1481 (0.5761)	-0.0992 (0.5730)	0.1262 (0.5719)
Unemployment rate (State monthly unemployment rate)	0.1279 (0.1634)	0.1429 (0.1640)	0.1261 (0.1637)	0.1319 (0.1644)	0.1455 (0.1639)
$-\ln(liklihood)$	587.98	587.78	587.91	587.49	588.07

[a] Estimates are weighted using the W2TMF person weights. In addition to the variable shown all specifications include controls for age (and its square), race-ethnicity, metro status, duration effects, month effects, year effects and state effects.

** Statistically significant at the 5-percent level

* Statistically significant at the 10-percent level

Table 7

Predicted Percentage Change in Annual Birth Probabilities Associated with Family Cap Implementation by Table 6 Specification and Birth Interval Year (Robust Standard Errors in Parenthesis [a])

Incremental Benefit/Family Cap Variable	Birth Interval Year 1	2	3	4
Incremental benefit (Increase in cash benefits available upon the birth of a child)	-21.45	-20.12	-18.38	-16.54
	(20.69)	(19.64)	(18.25)	(16.77)
Benefit increment percent (Percent increase in cash benefits available upon the birth of a child)	-28.00	-26.36	-24.19	-21.88
	(21.06)	(20.16)	(18.94)	(17.65)
Incremental benefit sum (Increase in cash and food stamp benefits available upon the birth of a child)	-21.47	-20.19	-18.51	-16.74
	(20.70)	(19.69)	(18.35)	(16.92)
Benefit increment sum percent (Percent increase in cash and food stamp benefits available upon the birth of a child)	-32.91	-31.06	-28.59	-25.96
	(20.98)	(20.26)	(19.31)	(19.32)
Family caps (=1 if the state has a family cap)	-25.59	-24.34	-22.21	-19.94
	(27.13)	(25.79)	(23.97)	(21.98)

[a] Standard Errors were computed via the $\delta - method$

** Significant at the 5-percent level

* Significant at the 10-percent level

Table 8

*Estimates of AFDC/TANF Policy Parameters on the Non-Marital, Non-Recipient Birth Rate (N=646, N*T=21,154, Robust Standard Errors in Parenthesis)* [a]

Variable	(1)	(2)	(3)	(4)	(5)
Incremental benefit (Increase in cash benefits available upon the birth of a child in 100s of $)	-0.4719 (0.3863)	___	___	___	___
Benefit increment percent (Percent increase in cash benefits available upon the birth of a child)	___	-0.0250* (0.0146)			
Incremental benefit percent (Increase in cash and food stamp benefits available upon the birth of a child in 100s of $)	___	___	-0.6375 (0.5579)	___	___
Benfit sum increment percent (Percent increase in cash and food stamp benefits available upon the birth of a child)	___	___	___	-0.0543 (0.0343)	
Family cap (=1 if the state has a family cap)	___	___	___	___	0.6748** (0.3251)
Waiver (=1 if the state has a waiver other than a family cap)	0.0434 (0.3619)	0.0317 (0.3566)	0.0465 (0.3647)	0.0808 (0.3651)	0.0143 (0.3531)
TANF (=1 if state has implemented TANF)	-0.1011 (0.5404)	-0.1322 (0.5339)	-0.0968 (0.5293)	-0.1314 (0.5343)	-0.1614 (0.5374)
Unemployment rate (State monthly unemployment rate)	0.2978** (0.1401)	0.2828** (0.1371)	0.2969** (0.1408)	0.2984** (0.1392)	0.2802** (0.1356)
$-\ln(liklihood)$	926.97	926.10	926.07	926.29	925.55

[a] Estimates are weighted using the W2TMF person weights. In addition to the variable shown all specifications include controls for age (and its square), teen motherhood, race-ethnicity, metro status, duration effects, month effects, year effects and state effects.

** Significant at the 5-percent level

* Significant at the 10-percent level

Table 9

Predicted Percentage Change in Annual Birth Probabilities Associated with Family Cap Implementation by Table 8 Specification and Birth Interval Year (Robust Standard Errors in Parenthesis [a])

	Birth Interval Year			
Incremental Benefit/Family Cap Variable	1	2	3	4
Incremental benefit (Increase in cash benefits available upon the birth of a child)	38.38	34.69	29.99	25.63
	(36.68)	(32.62)	(27.53)	(23.07)
Benefit increment percent (Percent increase in cash benefits available upon the birth of a child)	65.92	58.93	50.09	42.05
	(48.99)	(42.62)	(34.96)	(28.74)
Incremental benefit sum (Increase in cash and food stamp benefits available upon the birth of a child)	38.34	34.69	29.99	25.63
	(36.68)	(32.62)	(27.53)	(23.07)
Benefit sum increment percent (Percent increase in cash and food stamp benefits available upon the birth of a child)	68.03	59.95	49.81	40.67
	(54.68)	(46.21)	(36.27)	(28.44)
Family cap (=1 if the state has a family cap)	92.36	82.41	69.92	58.71
	(60.48)	(52.44)	(43.02)	(35.61)

[a] Standard Errors were computed via the $\delta - method$

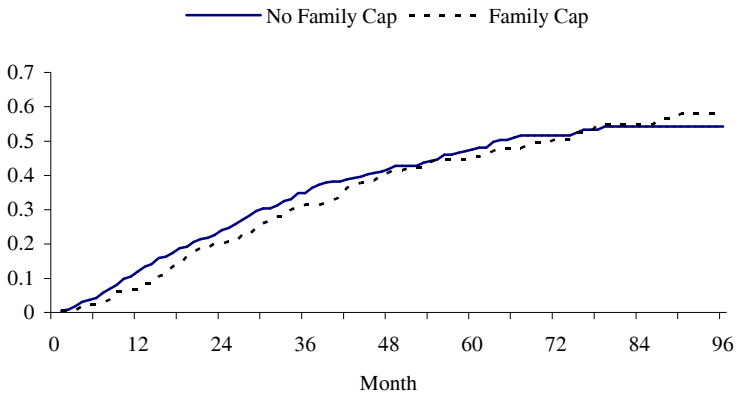
** Significant at the 5-percent level

* Significant at the 10-percent level

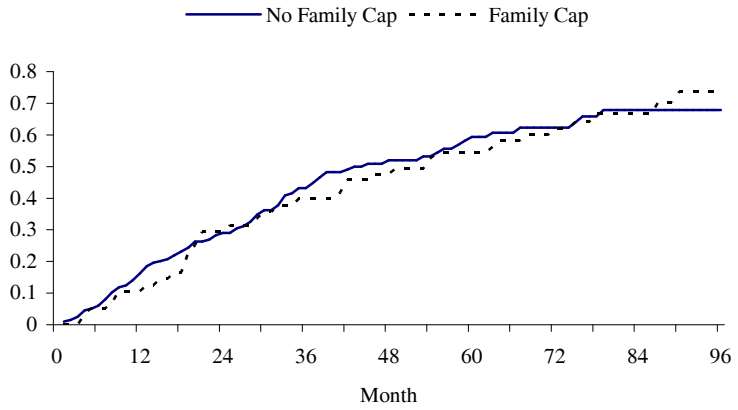
Figure 1

Cumulative Probability of a Second Non-marital Birth By the Presence of a Family Cap (Women Who Where Eligible for a Second Non-marital Birth During the 1990s)

All Single Women



Single Women with Welfare History



Single Women Without Welfare History