

Is There an Effect of Incremental Welfare Benefits on Fertility Behavior?
A Look at the Family Cap

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[Published in *Journal of Human Resources* (2004), 39(2), 295-325.]

This analysis exploits the variation across states in the timing of policy implementation to determine if family cap policies lead to a reduction in births to women ages 15 to 34. Vital statistics birth data for the years 1989 to 1998 offer no such evidence. The data reject a decline in births of more than one percent. The finding is robust to multiple specification checks. The data also reject large declines in higher-order births among demographic groups with high welfare participation rates.

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I. Introduction

Over the past decade, states across the country have been experimenting with welfare reform. One of the most controversial reform policies, the “family cap” or “child exclusion,” is motivated by the notion that an incremental increase in cash assistance for each additional child increases a woman’s propensity to bear additional children. With the intention of heightening personal responsibility, 18 states have responded to this concern by implementing family caps that end the traditional practice of providing families on welfare with additional cash benefits when a new child is born into the family. An additional five states have altered the form of the additional benefit, but not eliminated it entirely. This paper uses the variation across states in the timing of family cap implementation to identify whether the denial of incremental benefits leads to a reduction in births.

A woman’s decision to give birth is part of a complex series of decisions influenced by social, religious, economic, and other demographic and personal factors. The question of how welfare benefits affect this decision focuses on the role of economic factors in determining this choice. The primary economic question is whether the availability of fewer resources at the margin decreases a woman’s propensity to bear additional children. The potential direct effect of the policy is to reduce higher-order births: a decrease in marginal resources raises the price of an additional child and may thereby deter a woman from having additional births. Insofar as the policy sends a message that welfare is less generous than previously, it may also lead a woman to delay childbearing until she is financially secure and thereby reduce first births as well.

The economic theory underlying this question is that of rational choice, and in particular the role of incentives as important determinants of behavior. There is an extensive literature on

various potential incentive effects of the welfare system. Econometric studies generally show that labor supply is reduced by the Aid to Families with Dependent Children (AFDC) program and that higher potential benefits induce greater participation in this program. The evidence regarding the effects of AFDC on family structure is more mixed, but recent studies have found a weak effect. (For a survey of the literature see Moffitt 1998, 1992.)

Identifying the causal effects of welfare on fertility decisions is not straightforward. A regression of the number of births a woman has on the welfare benefits she receives confounds the direction of causality. The amount of cash assistance a woman receives is determined by the number of children she has. Many studies have tried to identify the causal relationship by exploiting cross-sectional variation in state benefit levels and birth rates. The main weakness of this strategy is that there may be fixed differences in birth rates across states that can not be controlled for in a cross-sectional analysis.

This paper addresses these problems by using a plausibly exogenous source of variation in incremental benefits and data from a panel of states. The nineties was a decade of unprecedented welfare reform and experimentation at the state level. Rather, welfare reform has been a political movement during the time period being studied and state policies have been adopted based on the politics and priorities of the state. The implementation of family caps does not appear to be driven by movements in birth rates. The legislative “quasi-experiment” is therefore reasonably considered exogenous. Furthermore, the variation in timing across the 18 states that eliminate incremental benefits provides us with multiple quasi-experiments from which we can identify the effect of the policy. The effect of the family cap on fertility behavior is identified using state-level panel data: the analysis compares the change in birth rates for a state that implements a family cap to the change in birth rates among states that do not implement a

family cap, controlling for level differences in birth rates across states and years as well as differences in linear birth rate trends across states.

Vital statistics birth data for the years 1989 to 1998 offer no evidence that family cap policies lead to a reduction in births to women ages 15 to 34. When state effects, month effects, and state-specific linear time trends are controlled for, a decline in births of more than one percent can be rejected at the 95 percent confidence level. (The upper bound of the confidence interval is an increase in births of 1.1 percent.) This finding of no effect on births is maintained across multiple specification checks. The set of confidence intervals around six alternative estimates has a lower bound of a one percent decline and an upper bound of a two percent increase. The data also reject large declines in higher-order births among demographic groups with relatively high welfare participation rates. Curiously, the data suggest increases in higher-order births to unmarried black and white high-school dropouts and to unmarried black teens approximately one year after the implementation of a family cap. The data reject a decline in births of more than four percent for unmarried white high-school graduates and unmarried white teens.

II. Background

A. Family cap policies

In August of 1996 the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) replaced the AFDC program with a block grant program called Temporary Assistance for Needy Families (TANF) and gave states flexibility to create new cash assistance programs for families with dependent children, effective July 1997. Before the passage of the PRWORA legislation, many states received waivers from the federal government allowing them to experiment with the rules of welfare. Starting with New Jersey in 1992, nineteen states

received approval to implement family cap policies under waivers. An additional four implemented family cap policies as part of their state TANF programs.

The AFDC program required all states to increase a family's benefit amount when an additional child was born into the family. In contrast, under strict family cap policies there is no increase in cash assistance when a child is born to a mother who was receiving welfare at the time of conception.¹ Some states have implemented partial family cap policies: two states provide only a partial increase in benefits for an additional child; two others provide the increase in assistance in the form of in-kind benefits; and one state gives the incremental increase in cash benefits to a third party to act on behalf of the child. Appendix Table 1 lists policy types and approval and implementation dates by state.

B. Other recent welfare reforms

The time period being studied was a time of active welfare reform and experimentation.² Since 1962 the Social Security Act has authorized the Secretary of the Department of Health and Human Services to waive specified requirements of the act in order to enable a state to carry out any experimental, pilot, or demonstration project that the Secretary deems in accordance with the objectives of AFDC. The Reagan, Bush, and Clinton administrations all granted such waivers liberally. By mid-August 1996, the Clinton administration had approved more than 70 waivers for more than 40 states.

Many AFDC waiver projects aimed to encourage labor force participation and human capital development. Restrictive reforms in this vein include tightened work requirements, time limits on benefit durations, and benefits linked to school attendance or performance.

Liberalizing reforms include a more generous treatment of earnings and resources and an increased vehicle asset limit. Some states received waivers to expand transitional medical and

childcare benefits in an effort to encourage recipients to leave the welfare program. Some waivers authorized states to expand eligibility for two-parent (unemployed) families, mitigating the discriminatory affect of AFDC against dual parent families. Many states incorporated provisions of their AFDC waiver projects into their TANF plans. Appendix Table 2 lists approval and implementation dates of states' first major waivers.

To empirically identify the effect of family cap policies on birth rates, it is necessary to control for the effect that other welfare initiatives might have on fertility decisions. Tightened work requirements in the form of less generous child exemptions are such a policy. Under AFDC, primary caretaker relatives of children up to six years of age, or up to three if the state guaranteed childcare, were exempt from the requirement that adults participate in the Job Opportunities and Basic Skills Training (JOBS) program. Thirteen states received waivers to alter their JOBS requirements, most commonly to lower the age of the youngest child that qualifies a recipient for a work exemption and in some cases to eliminate the caretaker exemption. All states imposed tightened work requirements under TANF requiring welfare recipients to work sooner in terms of the age of their youngest child. The implementation of stricter requirements may affect fertility behavior, since the cost to a woman of having a child is higher when the child does not qualify her for a work exemption or does so for a shorter amount of time. The econometric analysis below therefore controls for changes in work exemption rules. Appendix Table 3 lists work exemption policies and implementation dates by state.

If women perceive children to be a hindrance to financial self-sufficiency, time limits on benefits may affect reproductive decisions by signaling that welfare assistance is only temporary.³ AFDC imposed no restrictions on the length of time a family could receive welfare assistance. Twenty-four states received waivers to implement time limits and PRWORA requires

all states to impose benefit time limits. PRWORA also mandates that state TANF plans subject teen parents to stay-in-school and live-at-home provisions. In order to isolate the effect of a family cap policy from any effect these restrictive provisions might have on birth rates, the econometric estimation controls for the implementation of time limits and state TANF plans. Appendix Table 2 lists time limit and TANF implementation dates.

C. Previous studies on the effect of welfare on fertility

There is a large body of research from the past three decades on the effects of the welfare system on family structure. Studies from the 1970s and early 1980s generally fail to demonstrate an effect of welfare on marriage and fertility outcomes. Moffitt (1992) provides an overview of this early research. A second-wave of studies dating back to the mid-1980s offers mixed findings but has led to a tenuous consensus that the welfare system probably does affect marriage and fertility outcomes. However, as Moffitt (1998) observes, “there is considerable uncertainty surrounding this consensus because a significant minority of the studies finds no effect at all, because the magnitudes of the estimated effects vary widely, and because there are puzzling and unexplained differences across the studies by race and methodological approach.”

Many studies utilize cross-state comparisons of benefit levels to estimate the effect of welfare on fertility. Using data from the National Longitudinal Survey of Youth (NLSY), Lundberg and Plotnick (1990, 1995) find strong effects on pregnancy probabilities and resolution decisions for white teenage women but not for blacks. On the other hand, Acs (1994, 1996) analyzes data from the NLSY and finds no effect of welfare for either whites or blacks on the probability that a woman age 23 to 25 has a second birth or on the probability that a woman age 14 to 23 has a first birth. Duncan and Hoffman (1990) and An, Haveman, and Wolfe (1993) also rely on cross-state comparisons of benefit levels but these studies use data from the Panel Study

on Income Dynamics (PSID). The former analyze the fertility decisions of black teens and do not find a significant effect of welfare on AFDC-related births. The latter do not find a significant effect on the probability that a woman age 13 to 18 has a nonmarital birth.

A weakness of these studies, as well as other studies relying on cross-state comparisons, is that the results are potentially biased by unobserved differences across states. Benefit levels and fertility decisions may covary across states for reasons other than a direct welfare effect. In an analysis of PSID data from 1969 to 1989, Hoynes (1997) confirms that results are sensitive to the inclusion of state fixed effects and explores the possibility that population composition varies across states in ways related to welfare program generosity. Her results show that when the estimation procedure controls for individual effects, there is no evidence that welfare raises the propensity to form female-headed households for either whites or blacks. Similarly, Moffitt (1994), using Current Population Survey (CPS) data from 1968 to 1989 and a cross-state comparison of levels, finds positive welfare effects for whites on the probability that a woman with less than 12 years of education is a household head. But when he controls for state fixed effects in the estimation, he finds a negative effect. In contrast, Rosenzweig's (1999) analysis of NLSY data controls for state and cohort fixed effects and finds a significant and quantitatively large positive effect of AFDC on nonmarital childbearing through age 22. Hoffman and Foster (2000) demonstrate the sensitivity of estimates to the age group examined. They reproduce Rosenzweig's main findings using data from the PSID, but when they examine fertility separately by age, they find an AFDC effect only for women in their early twenties and not for teenage women.

Some recent work focuses on the effect of marginal welfare benefits. Argys and Rees (1996) examine the relationship between welfare generosity and fertility behavior using data on

an NLSY sample of 1,344 unmarried women who received welfare payments at some point between 1979 and 1991. Controlling for state fixed effects, they find neither the welfare guarantee level nor the marginal benefit level to be a significant determinant of conception probability. Robins and Fronstin (1996) estimate the effects of changes in benefit increments on family-size decisions among a CPS sample of never-married women. Their analysis finds that both the benefit level and the incremental benefit for a second child positively affect family size decisions of black and Hispanic women, but not of white woman. Fairlie and London (1997) estimate the probability of a higher-order birth for a sample of AFDC recipients and a comparison sample of non-AFDC women using data from the Survey of Income and Program Participation (SIPP). They find a stronger effect for the non-AFDC sample than for the AFDC sample and therefore conclude that the observed relationship is spurious.

The literature demonstrates that the estimation of a fertility effect of welfare is sensitive to the data set being analyzed, the age of the population being studied, and the methodology employed. The majority of studies have relied on cross-state comparisons, which are potentially unable to identify a causal link between welfare and fertility. The present study improves upon previous studies in a number of ways. To identify an effect of incremental welfare benefits on fertility decisions, the analysis relies on an arguably exogenous source of change in incremental benefit levels induced by the political welfare reform movement. The changes in benefits implemented under family cap policies occur at different times across states, so the estimation can control for the effects of time. In addition, the analysis uses state-level panel data so the estimation can control for state fixed effects. Furthermore, the present study uses vital statistics data on all births to all women and can therefore directly observe whether results extend across race and age groups.

D. Studies of family cap policies

Two states, Arkansas and New Jersey, have released evaluations of their family cap policies. Both evaluations employ an experimental design: women receiving welfare were randomly assigned to a treatment group that was subject to a benefits cap, and a control group that was not. The Turturro et al. (1997) evaluation of the Arkansas program for years 1994 to 1997 finds no statistical difference in the number of births born to women in the two groups. Camasso et al. (1999) use two analytical approaches to evaluate New Jersey's experience: an experimental design as described above and a pre-post analysis of the entire welfare caseload over a 6-year period that includes the implementation of the family cap. Both analyses suggest that pregnancies and births among women on welfare declined after program implementation and that the number of abortions increased.

Studies of targeted experiments such as these are always open to questions about whether the results apply to other contexts. In addition to this general problem, methodological weaknesses inherent in the Arkansas and New Jersey studies make the results of these studies difficult to interpret. First, there is evidence that the experimental design was contaminated in both demonstrations. Loury (2000) reports that in both demonstrations many members of the treatment and control groups did not know which policy applied to them.⁴ Furthermore, in New Jersey, more than one-quarter of case workers admitted to evaluators that they used discretion when making treatment or control assignments, thereby negating the randomness of the assignment. Loury also points out that the surveys on which the evaluations were based had low response rates and that the respondents were not representative of the larger AFDC caseload.

In addition to implementation problems, the Arkansas and New Jersey evaluations both restrict the analysis to women on welfare, which makes it impossible to identify the effect of the

family cap on fertility behavior separately from the effect on welfare participation. The existence of a family cap may lead to compositional changes among the welfare population that preclude the analysis from estimating a causal effect on births. On the one hand, a family cap policy may encourage women who desire multiple children to find alternative means of financial support, such as a paying job or a husband. To the extent that this occurs, the welfare population *post*-family cap includes fewer women who desire multiple children. All else equal, birth rates are unchanged, but there are fewer births to women on welfare. On the other hand, the presence of a family cap may signal that welfare is not a generous source of financial support, leading some young women to delay becoming a mother until they are more financially secure. Some women who might have given birth and enrolled in welfare *pre*-family cap do neither when a family cap policy is in effect. Holding all else constant, birth rates are lower as a consequence of the family cap but there is no change in the number of births to women on welfare. Due to these concerns, the empirical analysis below is not conditioned on welfare receipt and avoids confounding the effect of the family cap on birth rates with the effect on participation.⁵

In a contemporaneous working paper, Horvath-Rose and Peters (2000) examine aggregate vital statistics birth data from 1984 to 1996 to analyze the effect of family cap policies on state-level nonmarital birth ratios (the ratio of nonmarital births to total births). Their study concludes that the family cap decreases nonmarital fertility for all race and age groups. The main regressions of their study control for state and year fixed effects, high-school completion rate by adults age 18 to 24, proportion of state population living in urban areas, proportion of state population that are fundamentalist adherents, and indicator variables for the following welfare policies: minor parent provision waiver, time limit waiver, work requirement waiver, AFDC-UP waiver, child support waiver, expanded income disregard and asset limit waiver, school

attendance and performance requirement waiver, parental consent requirement for an abortion, requirement for sex education in schools.

There are three major limitations to the Horvath-Rose and Peters (2000) framework. First, by defining the dependent variable in terms of the nonmarital birth ratio, the analysis confounds the marriage and fertility responses to the family cap. Furthermore, though marital status is fundamental to their study, the authors do not account for the changes in the reporting of marital status in vital statistics data that occurred during the time period they study.⁶ Second, there are potential problems with their waiver variables, which differ substantially from those in the 1999 CEA report and 1998 Urban Institute report. Third, the results appear to be implausible. For women ages 20 to 49, the “unlagged” analysis finds a decrease of 1.4 percentage points off a base of 21 percent for white women and a decrease of 3.1 percentage points off a base of 51 percent for black women. For teenage women, the analysis finds a decrease of 4.5 percentage points off a base of 58 percent among whites and 3.2 percentage points off a base of 87 percent among blacks. It is curious that such large effects are observed for the entire population of women age 20 to 49, most of whom will never be at-risk of welfare receipt.

III. Data and Empirical Strategy

A. Data

Data on births are from the 1989 to 1998 *Vital Statistics Natality Data, Public-Use Data Files* compiled by the U.S. National Center for Health Statistics (NCHS). The public-use data files include all births occurring within the United States. I limit my sample to births occurring to women age 15 to 34 because women in this age group are more likely to be at-risk of welfare receipt – and hence affected by a family cap policy – than older women. The vital statistics data files identify the state of residence and month of birth, as well as mother’s education, mother’s

race, mother's marital status, and live-birth order. I use this information to create a data file of state birth counts.

Information on welfare policies is obtained from three sources. The first source is a 1999 technical report of the Council of Economic Advisors (CEA), which relied on experts from the Department of Health and Human Services as well as non-governmental research institutions. The second source is a 1998 Urban Institute report on state TANF programs. The third is a report by Crouse (1999), prepared for the U.S. Department of Health and Human Services, which summarizes information contained in a 1997 report of the U.S. Department of Health and Human Services *Setting the Baseline: A Report on State Welfare Waivers*.

B. Empirical strategy

The effect of the family cap on fertility can occur through two channels: conception and abortion. Economic reasoning predicts that the existence of a cap on benefits for additional children raises the price of a child and might therefore lead some women to avoid pregnancy or, once becoming pregnant, avoid a birth by having an abortion. This reasoning finds a foundation in Becker's models of the family. Becker (1981) uses the price of children and the real income to explain, among other things, why a rise in the wage rate of employed women reduces fertility and why various government programs, such as AFDC, might significantly affect the demand for children. Assuming that some women respond to the price increase of an additional child, we expect that the number of higher-order births in a state that has effectively raised the price of additional children will fall relative to the number of higher-order births in a state that has not, all else being constant.

The analysis of this paper identifies the sum of the conception and abortion responses and reports the net effect of the family cap on reproductive behavior. It is estimated at the aggregate

level, looking at the number of births in a state-month cell. The identification strategy of this paper is to compare the change in the number of births occurring in a state that becomes a family cap state to the change in the number of births that occurs in states that do not make the family cap transition. Relative to states that have not yet passed a family cap, or that did so in the past, this analysis identifies the incremental change in births that is associated with the introduction of the family cap.

The estimation technique applies ordinary least squares (OLS) regression to model fertility in state s in month t . The estimating equation, Equation 1, takes the following form:

$$(1) \ln(y)_{st} = \alpha + \beta_1 * famcap_{st} + \beta_2 * wke1_{st} + \beta_3 * wke2_{st} + \beta_4 * wke3_{st} + \beta_5 * \ln(welfare\ benefits)_{sy} + \beta_6 * TANF_{st} + \beta_7 * time\ limit_{st} + \beta_8 * \ln(female\ pop\ 15-34)_{sy} + \beta_9 * (prop\ 15-19)_{sy} + \beta_{10} * (prop20-24)_{sy} + \beta_{11} * (prop25-29)_{sy} + \beta_{12} * (unemp\ rate)_{s(t-9)} + \gamma_s + \sigma_t + \xi_s * time + \varepsilon_{st}$$

The variables are defined as follows: y_{st} – total number of births in state s in month t to women age 15 to 34; $famcap_{st}$ – a binary indicator for whether state s implemented a family cap at least six-months prior to month t ; $wke1_{st}$ – work exemption 1 – a binary indicator for whether state s implemented an exemption for mothers with a child as old as six months to three years; $wke2_{st}$ – work exemption 2 – a binary indicator for whether state s implemented an exemption for mothers with a child newly born to six months old; $wke3_{st}$ – work exemption 3 – a binary indicator for whether state s removed exemptions based on the age of a mother’s child; $welfare\ benefits_{sy}$ – the maximum monthly benefit for a family of three on AFDC/TANF in state s in year y in 1998 dollars⁷; $TANF_{st}$ – a binary indicator for whether a state implemented its TANF plan at least six months previous to month t ; $time\ limit_{st}$ – a binary indicator for whether a state implemented a time limit on welfare benefits at least six months previous to month t ; $female\ pop\ 15-34_{sy}$ – the

female population age 15 to 34 in state s in year y , based on U.S. census figures; $prop15-19_{sy}$, $prop20-24_{sy}$, $prop25-29_{sy}$ – the proportion of the female population age 15 to 34 in the different five-year age groups in state s in year y (the omitted category is the proportion age 30 to 34); $unemp\ rate_{s(t-9)}$ – the average unemployment rate in state s in the six months around the month of conception, $t-9$; γ_s – a binary indicator for state s ; σ_t – month fixed effects for month t ; $\xi_s * time$ – linear time trend specific to state s .

The analysis sample consists of 6,120 observations (51 states over 120 months). The dependent variable is the natural logarithm of the total number of births in state s in month t to women age 15 to 34. In subsequent specifications, the model is estimated for higher-order births to women in more narrowly defined demographic groups. The distribution of total monthly births in a state is highly skewed, so a log transformation of the birth count is used. An advantage of this transformation is that coefficients reflect percentage changes, which aids in the interpretation of results. A potential problem with defining the dependent variable at the state-level is that the analysis will provide unbiased estimates of the effect of the family cap on birth rates only if there is not widespread migration in response to family cap policies. This assumption finds support in Levine and Zimmerman (1999), which evaluates the extent to which differences in welfare generosity across states leads to interstate migration and concludes that welfare-induced migration is not a widespread phenomenon.

The variable of primary interest is the binary indicator for a family cap. I refer to the 18 states that between 1989 and 1998 eliminate additional cash assistance for a child born to a mother on welfare as “ever-treated” states and to the other 32 states and the District of Columbia as “never-treated” states. For the never-treated states, the family cap indicator is always equal to zero. For the ever-treated states, the family cap indicator takes on a value of one if the

observation represents a month that occurs at least six months after the state's family cap policy was implemented. Allowing a six-month lag recognizes that conception responses can not take place within nine months of the policy implementation and that most abortion responses will occur in the first trimester of pregnancy. An alternative specification defines the family cap to incorporate a 12-month lag.

The three work exemption variables are included in the model to control for the effect that tightened work restrictions might have on fertility decisions. They are mutually exclusive and the omitted category is the traditional AFDC/JOBS exemption policy. As discussed above, the relative cost of having a child is higher when the child does not exempt the mother from work requirements. Economic reasoning thus implies the sign of β_2 , β_3 , and β_4 to be non-positive, and since *wke1* represents the least strict non-AFDC policy and *wke3* represents the strictest, we expect that $\beta_4 \leq \beta_3 \leq \beta_2$. The welfare benefit level is controlled for in the model to account for any change in benefit levels that may be correlated with the introduction of a family cap policy and may independently affect fertility decisions. Eleven states explicitly changed their benefit levels under TANF and the inflation-adjusted level of benefits declined in almost all states during the nineties. All else equal, a higher benefit level makes raising a family on welfare less costly, and economic reasoning thus predicts that the sign on β_5 is positive.

The estimation also controls for the implementation of time limits and state TANF plans. As discussed above, a time limit on benefit durations may affect reproductive decisions insofar as it signals that assistance is temporary and encourages women to delay childbearing until they are financially secure. TANF plans may affect fertility behavior through mechanisms other than a family cap; all state TANF plans subject teen parents to stay-in-school and live-at-home provisions. The advantage of including these control variables in the model is that the estimated

effect of the family cap will not include any effect these restrictive provisions may have on birth rates. A potential disadvantage is that the data may not have enough statistical power to separately identify partial effects for highly correlated welfare reform variables, especially in empirical specifications that limit the sample to narrowly defined demographic groups. Robustness checks are performed to determine whether the estimated effect of the family cap is sensitive to the inclusion of these controls.

The regression model explicitly controls for the natural logarithm of the female population age 15 to 34. If the female population is trending non-linearly, then the state-specific linear time trend will not adequately capture population movements, which undoubtedly affect birth counts. The proportion of women in each five-year age group is controlled for to account for idiosyncratic demographic shifts that might be spuriously correlated with the implementation of family cap policies. Data on population are obtained from the U.S. census bureau. The model explicitly controls for the average state unemployment rate in the three months before and after conception. This variable is included to capture shifts in economic conditions that are not uniform across states nor are adequately described by a linear state trend. Individual state fixed effects are accounted for in the regression to control for level differences in birth rates across states. (Ever-treated states have higher birth totals than never-treated states on average.) Indicator variables for the particular month are included to account for any idiosyncratic movements in birth rates common to all states.

The identification strategy relies on the assumption that birth rates in states that implement family caps are not trending differently than birth rates in other states pre-family cap. This assumption is tantamount to assuming that the introduction of family cap legislation is exogenous to birth rate trends. Two empirical tests support this assumption. First, Figures 1, 2,

and 3 plot annual fertility rates, defined as births per 1,000 women in the relevant age group. The data reveal no sign of divergent trends prior to the mid-1990s for either women age 15 to 19 or women age 20 to 34. Second, as discussed below, the data suggest that the *approval* of a family cap waiver is not positively associated with births, which would be the case if an increase in birth rates led a state to request a waiver to implement a family cap. Nonetheless, one would prefer not to have to rely on this assumption. To relax it to some degree, the model is estimated with controls for state-specific time trends, denoted in months. These controls allow fertility rates to trend uniquely, albeit linearly, for each state without undermining the viability of this empirical strategy.

IV. Results and Conclusion

A. Differences in fertility rates, by treatment status and demographic group

Table 1 lists mean total monthly births and fertility rates for the overall sample of states. It also lists birth counts separately for never-treated states pre- and post-1996 and for ever-treated states pre- and post- family cap implementation (with a six-month lag). The first row of the table reports that for women age 15 to 34, the average monthly fertility rate – defined as births per 1,000 women – declined from an average of 7.6 to 7.4 in ever-treated states; the fertility rate declined from an average of 7.6 to 7.3 in never-treated states. There was no change in the average monthly fertility rate among women age 20 to 34 in either group of states. The fall in the fertility rate of teens was slightly greater in never-treated states. Under the assumption that fertility rates would have trended similarly in the two groups of states in the absence of the policy intervention, these unadjusted “difference-in-differences” imply that the family cap did not noticeably affect fertility rates of women age 15 to 34. Figures 1, 2, and 3 make the point

visually that at no point between 1989 and 1998 does average monthly fertility decline more sharply (or increase less steeply) for states that implement caps relative to states that do not.

One problem with looking at the fertility rates of all women age 15 to 34 is that many women will never be at-risk of welfare receipt and therefore will not respond to welfare reform initiatives such as the family cap. To narrow the analysis, twelve demographic groups are identified based on race⁸, marital status, and education. Women age 20 to 34 are divided into high-school graduates and high-school dropouts; women age 15 to 19 are classified simply as teens in order to avoid mislabeling young women still in school as dropouts. According to data from the March 1989 *Current Population Survey* (CPS), unmarried high-school dropouts have the highest welfare participation rate – 61.9 percent among black women and 36.9 percent among white women. The rate among unmarried high-school graduates is 22.0 percent among blacks and 5.5 percent among whites.

Unadjusted difference-in-difference calculations suggest that relative to states that did not, states that implemented family caps experienced a greater decline in the number of births born per month to white unmarried high-school dropouts, but not in the number of births born to black unmarried high-school dropouts. The same is true for births born to unmarried high-school graduates. However, states that implemented family caps also experienced larger relative declines in the total number of births born to *married* white women. As the proportion of married women who receive welfare is extremely low (5.3 percent among high-school dropouts and 1.2 percent among graduates), this suggests that something other than the family cap is driving these trends. Indeed, the regression-adjusted estimates for these groups show no relative decline, as discussed below.

Population estimates are not available for these narrowly defined demographic groups so it is not easily determined whether differential demographic shifts are behind these trends. To be clear, the birth totals listed for the twelve demographic groups in Table 1 do not account for demographic shifts in the composition of the female population. The top panel of Table 1 shows that mean monthly birth totals fall more in ever-treated states even though fertility rates do not. This discrepancy suggests that population demographics shifted differently between the two groups of states. This highlights the importance of accounting for state-specific time trends and population demographics in the regression analysis.

B. Regression results - all births to women 15 to 34

Table 2 displays the results from estimating Equation 1 and six specification checks for the full sample of women age 15 to 34. In the base specification the dependent variable is the natural logarithm of the total number of births in state s in month t . The family cap indicator equals one if the state of observation eliminated benefits for an additional child six or more months previous to the month of observation. The base specification controls for state effects, month effects, state-specific linear time trends, population demographics, lagged state unemployment, and the full set of welfare reform variables. The table reports robust standard errors that incorporate White's correction for an arbitrary covariance structure between observations within a state and year (see Bertrand et al. 2001)⁹.

The estimation of Equation 1 yields a point estimate of the effect of a family cap of 0.002, with a standard error of 0.005. The 95 percent confidence interval around this estimate ranges from a negative effect of 0.8 percent to a positive effect of 1.1 percent. The data reject the hypothesis that a family cap leads to a decline in births of one percent or more. Benefit levels have a statistically significant positive effect on birth rates and unemployment has a statistically

significant negative effect. Controls for the implementation of tightened work exemptions, a time limit on benefits, and a state TANF plan enter the model insignificantly. This finding of no effect is maintained across the six alternative specifications. The set of confidence intervals around the six alternative estimates has a lower bound of -0.01 and an upper bound of $.02$.

The specification presented in Column 2 removes the time limit and TANF variables from the model. The motivation for this specification is that it may be difficult for the data to parse out the effects of highly correlated welfare reform variables, and perhaps this is why estimation of Equation 1 fails to detect a negative effect of the family cap. The results do not support this explanation; the estimated coefficient on the family cap indicator is unchanged. Column 3 displays the estimated coefficients when the model does not account for differential time trends across states. Perhaps the time trend variables in the base specification are capturing the variation in birth rates that would otherwise identify a family cap effect. The results do not support this explanation either. The point estimate of β_1 remains 0.002 .

In the specification listed in Column 4, the family cap indicator incorporates a twelve-month lag. The base case of a six-month lag allows an initial abortion response from women in their first trimester of pregnancy; a twelve-month lag assumes more time is needed for a response. The estimated coefficient is positive and significant: 0.009 with a standard error of 0.004 . This is a surprising result, as there is no reason to suspect that the elimination of benefits for an additional child leads to an increase in births. The result will be explored in more detail below.

The specification in Column 5 investigates the effect of the *approval* of a family cap policy, rather than the implementation. The estimated coefficient is again 0.002 . This finding affirms the assumption that family cap policies are not adopted in response to positive shifts in

birth rates. A more skeptical interpretation is that the adoption of a family cap is positively correlated with births, but that it is offset by an “anticipation” effect whereby women avoid pregnancy when they learn that a family cap will soon be imposed. Because there appears to be no decline in births after the family cap is implemented, this interpretation seems highly unlikely.¹⁰

The two final specification checks reported alter the form of the dependent variable. The regression reported in Column 6 estimates the equation for the natural logarithm of the fertility rate. The mean fertility rate across the 6,120 state-month cells in the sample is 7.6 births per 1,000 women. Defining the dependent variable this way essentially moves the natural logarithm of the female population age 15 to 34 from the right-hand side of the equation to the left-hand side; that is, it restricts the coefficient on the population variable to be one. In this specification, the estimated effect lies between a decrease of 0.6 percent in the fertility rate and a 1.1 percent increase. Again, the data suggest that if the effect of the family cap is negative, it is smaller in absolute terms than one percent.

The final specification defines the dependent variable as the natural logarithm of the nonmarital birth ratio – defined as the number of births to unmarried women divided by the total number of births – as do Horvath-Rose and Peters (2000). The estimated coefficient on the family cap indicator is 0.002 with a standard error of 0.006. In contrast, Horvath-Rose and Peters (2000) find a statistically significant lagged decline in the nonmarital ratio of women age 20 to 49: they estimate a decline of eleven percent for white women and approximately eight percent for black women. The data in the present analysis reject a negative effect larger than 1.2 percent for all women age 15 to 34 at the 95 percent confidence level.

C. Regression results – higher-order births, by demographic group

The above analysis fails to detect a decline in births associated with the implementation of a family cap. But perhaps additional births to women at-risk of welfare receipt comprise only a small fraction of births born to women ages 15 to 34, making it statistically difficult for the analysis to detect an effect. In order to target the analysis to fertility decisions that are potentially affected by welfare reform, I limit the analysis to higher-order births.¹¹ In addition, Equation 1 is estimated separately for the twelve demographic groups defined above to so that we can compare estimated effects across populations with different welfare participation rates. The trade-off inherent to this approach is that while the power of the analysis is strengthened by the focus on a more targeted sample, it is weakened by a decrease in sample size.

Table 3 reports the results for the eight demographic groups age 20 to 34 and Table 4 reports results for the four groups of teenage women. The estimated effect of the family cap is not statistically different from zero for any of the groups, but the standard errors are roughly five times as large as the standard errors for the full sample estimates. Estimating Equation 1 for higher-order births born to black, unmarried high-school dropouts yields a coefficient of 0.057 on the family cap indicator, with a standard error of 0.025. With a welfare participation rate of 62 percent, this is the group most likely to be affected by welfare reform. The estimated coefficient on the family cap indicator for white, unmarried high-school dropouts – the group with the second highest rate of welfare participation – is also curiously positive and statistically significant. This counter-intuitive finding is investigated further in Table 5. These estimates stand in stark contrast to the findings of Camasso et al. (1999) in the case of New Jersey's family cap. Their study reports that births declined by nine percent among longer-term welfare recipients and by twelve percent among their sample of new applicants.

A sizeable percentage of black, unmarried high-school graduates receive welfare – 22 percent, according to the 1989 March Current Population Survey (CPS). But the data provide no evidence that a family cap leads to a decrease in higher-order births among this group either. The estimated coefficient is 0.010; the 95 percent confidence interval for additional births extends from a decline of 2.7 percent to an increase of 4.7 percent. Black, married high-school dropouts also report a relatively high rate of welfare participation, 12.9 percent. But women in this group also appear to be unresponsive to the family cap. The data yield a point estimate of 0.042, with a 95 percent confidence interval ranging from -0.033 to 0.118.

The other demographic groups listed in Table 3 have low rates of welfare participation and not surprisingly, do not appear to respond to the implementation of a family cap. As shown in Table 4, the data do not suggest a family cap effect among teenage women either. The estimated coefficient on the family cap indicator is -0.012 for higher-order births to unmarried white teens, with a standard error of 0.014. The data reject a decline of more than four percent at the 95 percent confidence level. The estimated coefficient is 0.069 for unmarried black women and is curiously statistically significant.

A potential objection to looking separately at births to unmarried and married women is that marriage decisions may be affected by the introduction of a family cap. (Misreporting of marriage in Vital Statistics data is also an issue, as discussed above.) Some pregnant unmarried women may respond to a family cap by getting married. If this were the case, the data would indicate a decrease in births to unmarried women and a corresponding increase in births to married women. However, the results in Tables 3 and 4 do not suggest that the introduction of a family cap is associated with an increase in the number of higher-order births born to married women.

Table 5 explores the positive coefficient on the family cap indicator found among three samples of women: black unmarried high-school dropouts, white unmarried high-school dropouts, and black unmarried teens. For the sake of completeness, the table also reports results for white unmarried teens. The family cap indicator is replaced with a set of seven indicator variables that control for three-month intervals before and after the implementation of the family cap: three to six months before, zero to two months before, one to three months after, four to six months after, seven to nine months after, ten to twelve months after, and more than a year after. If the positive coefficient is picking up a spurious correlation between birth rates and the introduction of a family cap policy, this might be evidence that the policy is not “exogenous” to birth rates. The results of this exercise suggest that this is not the case. For unmarried high-school dropouts, the positive association does not appear until more than a year after the family cap has been implemented. For unmarried black teenagers the positive association does not appear until several months after the policy. There is some spurious positive association that is explained neither by population shifts nor by unemployment rates. When additional years of data become available, future research should explore this curious finding.

To explore this positive estimate further, I estimate Equation 1 for higher-order births to high-school dropouts with the dependent variable specified as the natural logarithm of the nonmarital birth ratio, rather than the natural logarithm of the total number of births. The estimated effect on the nonmarital birth ratio of high-school dropouts is not significantly different from zero for either white or black women. The estimated coefficient on the family cap indicator is -0.015 for blacks, with a standard error of 0.013 , and 0.012 for whites, with a standard error of 0.013 . This implies that there was not an upward shift in births to unmarried women relative to

married women, which provides some assurance that there is not some confounding factor affecting the fertility decisions of women with high rates of welfare participation.

D. Discussion and policy implications

This paper has found no systematic effect of the family cap on fertility rates. The data reject a decline in births of more than one percent to women age 15 to 34. The data detect no significant decline in higher-order births among demographic groups with relatively high welfare participation rates. If this empirical result is correct, then the widespread adoption of family cap policies appears ineffective at best and misguided at worst. Women are not responding by having fewer additional births, and consequently, fewer resources are being provided per child on welfare.

Future research could help determine whether these results are conclusive. The data suggest that for some demographic groups there is a positive shift in births several months after a family cap policy is implemented. The present analysis is unable to account for this curious result. In addition, the analysis incorporates limited post-family cap data. Most states that eliminated cash assistance for additional children did so in 1995 and 1996 and vital statistics birth data is only available through 1998. It is possible that effects on fertility will not be evident for another couple years. When additional years of natality data become available, future research should examine the positive association and investigate whether the finding of no response holds in the long run.

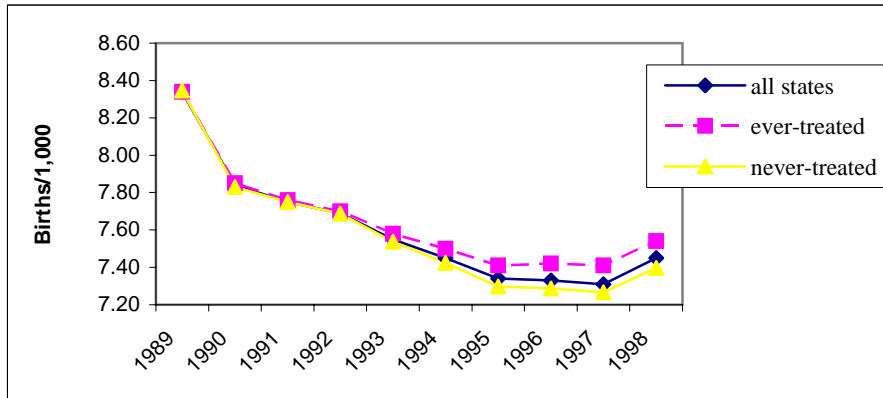


Figure 1
Monthly Fertility Rates: All women age 15 to 34

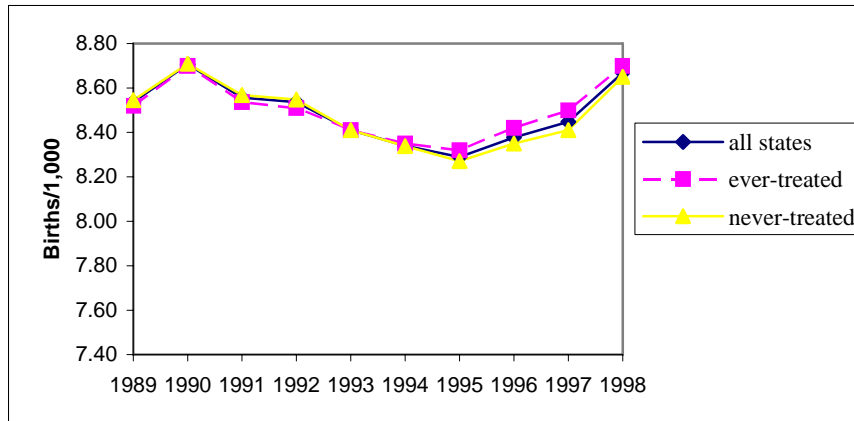


Figure 2
Monthly Fertility Rates: All women age 20 to 34

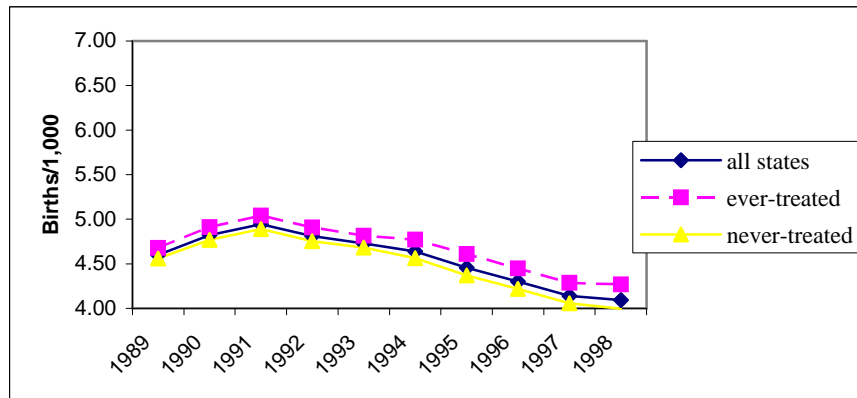


Figure 3
Monthly Fertility Rates: All women age 15 to 19

Table 1
Mean Monthly Birth Totals: 1989 to 1998

	Overall	Never-treated		Ever-treated		D-in-D
		1989-95	1996-98	pre-cap	post-cap	(d-c) –
		(a)	(b)	(c)	(d)	(b-a)
<i>All women age 15-34</i>	5,809	5,221	4,933	7,362	6,152	-922
<i>per 1,000 women</i>	7.6	7.6	7.3	7.6	7.4	0.1
<i>All women age 20-34</i>	4,988	4,486	4,222	6,328	5,288	-776
<i>per 1,000 women</i>	8.5	8.5	8.5	8.5	8.5	0.0
<i>All female teens 15-19</i>	820	735	711	1,034	863	-147
<i>per 1,000 women</i>	5.0	4.7	4.1	4.8	4.4	0.2
<i>Unmarried HS dropouts age 20-34</i>						
(1) <i>black</i>	130	127	105	153	136	5
(2) <i>white</i>	283	196	228	472	291	-213
<i>Unmarried HS grads age 20-34</i>						
(3) <i>black</i>	371	332	316	450	463	29
(4) <i>white</i>	546	466	554	666	583	-171
<i>Married HS dropouts age 20-34</i>						
(5) <i>black</i>	30	32	23	35	25	-1
(6) <i>white</i>	432	346	307	690	392	-259
<i>Married HS graduates age 20-34</i>						
(7) <i>black</i>	252	233	200	303	306	36
(8) <i>white</i>	2,996	2,806	2,530	3,630	3,118	-236
<i>Unmarried teens</i>						
(9) <i>black</i>	236	212	191	295	288	14
(10) <i>white</i>	380	318	378	480	408	-132
<i>Married teens</i>						
(11) <i>black</i>	21	21	11	21	12	1
(12) <i>white</i>	276	209	150	276	167	-50

Sources: *Vital Statistics Natality Data, Public-Use Data Files* 1989 to 1998, compiled by the U.S. National Center for Health Statistics (NCHS); annual population estimates are from the U.S. census bureau.

Notes: "Ever-treated" states are the 18 states that eliminated additional cash benefits between 1989 and 1998; "post-family cap" is defined as six months after the implementation of the family cap policy. The proportion of demographic group on welfare, not restricted to mothers, based on weighted means from the 1989 March *Current Population Survey*: 1) 61.9 (n=267); 2) 36.9 (820); 3) 22.0 (1,077); 4) 5.5 (5,843); 5) 12.9 (75); 6) 5.3 (1,129); 7) 2.7 (556); 8) 1.2 (8,240); 9) 8.8 (642); 10) 1.7 (4,394); 11) 40.4 (6); 12) 5.3 (203).

Sources: Birth data are from *Vital Statistics Natality Data, Public-Use Data Files*, years 1989 to 1998, compiled by the U.S. National Center for Health Statistics (NCHS); annual population estimates by state and age group are from the U.S. census bureau (figures are not available by race.)

Notes: Unless otherwise noted, all regressions include a full set of state and month indicators as well as state-specific linear time trends. The binary *famcap*, *time limit*, and *TANF* variables equal one if the particular policy has been implemented for at least six months. Standard errors are White's robust standard errors adjusted for clustering at the level of state-year.

Table 3
 Dependent variable: $\ln(\text{births})$
 Higher-order births to women age 20 to 34

	Unmarried				Married			
	High School Dropout		High School Graduate		High School Dropout		High School Graduate	
	Blacks (1)	Whites (2)	Blacks (3)	Whites (4)	Blacks (5)	Whites (6)	Blacks (7)	Whites (8)
<i>Family cap</i>	0.057 (0.025)	0.063 (0.028)	0.010 (0.019)	-0.012 (0.014)	0.042 (0.039)	0.042 (0.027)	0.010 (0.019)	-0.004 (0.005)
<i>Ln(max monthly benefits)</i>	0.387 (0.238)	0.476 (0.216)	-0.318 (0.133)	-0.122 (0.100)	-0.157 (0.273)	0.210 (0.215)	-0.079 (0.126)	0.017 (0.039)
<i>Work exemption 1</i>	0.039 (0.029)	-0.027 (0.034)	-0.058 (0.024)	0.017 (0.016)	0.036 (0.038)	0.010 (0.032)	0.011 (0.021)	0.007 (0.005)
<i>Work exemption 2</i>	0.059 (0.043)	0.038 (0.033)	-0.047 (0.030)	-0.021 (0.019)	-0.037 (0.056)	0.025 (0.035)	-0.0002 (0.027)	-0.025 (0.006)
<i>Work exemption 3</i>	0.063 (0.042)	0.113 (0.038)	-0.042 (0.033)	0.082 (0.030)	-0.203 (0.075)	0.042 (0.039)	-0.081 (0.045)	-0.022 (0.006)
<i>Time limit</i>	-0.017 (0.023)	0.016 (0.034)	-0.014 (0.019)	0.002 (0.012)	0.065 (0.035)	0.030 (0.033)	-0.002 (0.016)	0.001 (0.004)
<i>TANF-official</i>	0.065 (0.046)	-0.075 (0.039)	0.048 (0.031)	-0.015 (0.014)	-0.030 (0.052)	-0.044 (0.030)	0.003 (0.027)	0.006 (0.006)
<i>Ln(female pop 15 to 34)</i>	1.76 (0.968)	0.915 (1.06)	1.48 (0.643)	-0.347 (0.477)	1.08 (1.41)	0.341 (0.994)	0.427 (0.722)	0.209 (0.185)
<i>Proportion age 20 to 24</i>	1.44 (1.70)	-3.56 (2.34)	2.06 (1.00)	0.443 (1.23)	4.50 (2.50)	-6.29 (2.35)	1.10 (1.20)	0.294 (0.394)
<i>Proportion age 25 to 29</i>	2.83 (2.16)	0.675 (2.28)	2.55 (1.40)	-0.528 (1.52)	8.88 (3.14)	-2.81 (2.10)	2.31 (1.43)	1.04 (.490)
<i>Lagged state unemployment rate</i>	0.045 (0.079)	0.023 (0.094)	-0.119 (.064)	-0.025 (0.043)	-.132 (0.103)	-0.070 (0.082)	-0.219 (0.068)	-0.067 (0.018)
<i>Constant</i>	-21.8 (12.3)	-7.99 (13.0)	-15.2 (8.15)	10.8 (5.82)	-15.5 (18.1)	2.35 (12.3)	-1.96 (8.98)	4.69 (2.30)
<i>R</i> ²	0.97	0.96	0.99	0.99	0.93	0.97	0.98	0.99
<i>Sample size</i>	4,981	6,116	5,701	6,120	4,812	6,120	5,989	6,120

Sources: Birth data are from *Vital Statistics Natality Data, Public-Use Data Files*, years 1989 to 1998, compiled by the U.S. National Center for Health Statistics (NCHS); annual population estimates by state and age group are from the U.S. census bureau (figures are not available by race).

Notes: All regressions include a full set of state and month indicators as well as state-specific linear time trends. The binary *famcap*, *time limit*, and *TANF* variables equal one if the particular policy has been implemented for at least six months. Standard errors are White's robust standard errors adjusted for clustering at the level of state-year.

Table 4
 Dependent variable: $\ln(\text{births})$
 Higher-order births to women age 15 to 19

	Unmarried		Married	
	Blacks (9)	Whites (10)	Blacks (11)	Whites (12)
<i>Family cap</i>	0.069 (0.018)	-0.012 (0.014)	0.017 (0.058)	-0.013 (0.027)
<i>Ln(max monthly benefits)</i>	0.291 (0.160)	0.018 (0.106)	-0.505 (0.326)	-0.123 (0.117)
<i>Work exemption 1</i>	-0.036 (0.020)	0.019 (0.015)	-0.024 (0.049)	-0.032 (0.028)
<i>Work exemption 2</i>	-0.018 (0.026)	0.003 (0.019)	-0.043 (0.076)	-0.029 (0.037)
<i>Work exemption 3</i>	0.067 (0.033)	0.052 (0.027)	-0.319 (0.134)	-0.084 (0.049)
<i>Time limit</i>	-0.008 (0.016)	0.008 (0.011)	0.049 (0.046)	0.045 (0.020)
<i>TANF-official</i>	0.017 (0.027)	-0.013 (0.017)	-0.034 (0.061)	0.026 (0.036)
<i>Ln(female pop 15 to 34)</i>	-0.100 (0.209)	0.717 (0.319)	-0.182 (0.562)	1.59 (0.258)
<i>Lagged state unemployment rate</i>	-0.104 (0.063)	-0.002 (0.057)	-0.032 (0.135)	-0.036 (0.055)
<i>Constant</i>	4.97 (2.56)	-2.60 (3.56)	6.37 (6.57)	-12.2 (2.89)
R^2	0.99	0.99	0.90	0.98
<i>Sample size</i>	5,493	6,120	4,750	6,107

Sources: Birth data are from *Vital Statistics Natality Data, Public-Use Data Files*, years 1989 to 1998, compiled by the U.S. National Center for Health Statistics (NCHS); annual population estimates by state and age group are from the U.S. census bureau (figures are not available by race.)

Notes: All regressions include a full set of state and month indicators as well as state-specific linear time trends. The binary *famcap*, *time limit*, and *TANF* variables equal one if the particular policy has been implemented for at least six months. Standard errors are White's robust standard errors adjusted for clustering at the level of state-year.

Table 5
 Dependent variable: $\ln(\text{births})$
 Higher-order births

	(1) Black unmarried high school dropouts	(2) White unmarried high school dropouts	(3) Black unmarried teens	(4) White unmarried teens
<i>Famcap – 3 to 6 months before</i>	0.009 (0.029)	0.026 (0.028)	0.010 (0.022)	-0.043 (0.017)
<i>Famcap – 0 to 2 months before</i>	0.017 (0.038)	-0.025 (0.040)	0.032 (0.021)	-0.005 (0.020)
<i>Famcap – 1 to 3 months after</i>	0.019 (0.048)	0.036 (0.032)	0.038 (0.030)	-0.013 (0.022)
<i>Famcap – 4 to 6 months after</i>	0.010 (0.039)	0.031 (0.036)	0.036 (0.021)	-0.026 (0.019)
<i>Famcap – 7 to 9 months after</i>	0.010 (0.050)	0.012 (0.046)	0.057 (0.029)	-0.019 (0.022)
<i>Famcap – 10 to 12 months after</i>	0.039 (0.055)	0.040 (0.038)	0.088 (0.033)	-0.032 (0.021)
<i>Famcap – more than a year after</i>	0.093 (0.035)	0.100 (0.039)	0.095 (0.022)	-0.016 (0.021)
<i>Ln(max monthly benefits)</i>	0.382 (0.239)	0.461 (0.216)	0.287 (0.160)	0.017 (0.105)
<i>Time limit</i>	-0.017 (0.023)	0.022 (0.034)	-0.005 (0.016)	0.008 (0.011)
<i>TANF-official</i>	0.065 (0.046)	-0.076 (0.039)	0.016 (0.026)	-0.014 (0.016)
<i>Work exemption 1</i>	0.041 (0.029)	-0.025 (0.033)	-0.035 (0.020)	0.019 (0.014)
<i>Work exemption 2</i>	0.054 (0.043)	0.036 (0.032)	-0.018 (0.026)	0.002 (0.019)
<i>Work exemption 3</i>	0.063 (0.043)	0.116 (0.039)	0.069 (0.032)	0.051 (0.027)
<i>Ln(female pop 15 to 34)</i>	1.78 (0.982)	0.936 (1.06)	-0.108 (0.208)	0.716 (0.319)

<i>Proportion age 20 to 24</i>	1.52 (1.72)	-3.45 (2.34)	-	-
<i>Proportion age 25 to 29</i>	2.97 (2.17)	0.812 (2.28)	-	-
<i>Lagged state unemployment rate</i>	0.045 (0.079)	0.020 (0.094)	-0.104 (0.063)	-0.001 (0.057)
<i>Constant</i>	-22.1 (12.5)	-8.26 (13.0)	5.10 (2.55)	-2.59 (3.56)
<i>R²</i>	0.97	0.96	0.99	0.99
<i>Sample size</i>	4,981	6,116	5,493	6,120

Sources: Birth data are from *Vital Statistics Natality Data, Public-Use Data Files*, years 1989 to 1998, compiled by the U.S. National Center for Health Statistics (NCHS); annual population estimates by state and age group are from the U.S. census bureau (figures are not available by race.)

Notes: All regressions include a full set of state and month indicators as well as state-specific linear time trends. The binary *famcap*, *time limit*, and *TANF* variables equal one if the particular policy has been implemented for at least six months. Standard errors are White's robust standard errors adjusted for clustering at the level of state-year.

Appendix Table 1
State family cap policies

	Date Implemented	Date Approved	No increase in assistance for additional child	Partial increase in cash assistance for additional child	Increase in assistance provided as voucher	Increase in cash assistance given to third party
Arizona	11/95	5/95	X			
Arkansas	7/94	4/94	X			
California	9/97	8/96	X			
Connecticut	1/96	12/95		X		
Delaware	10/95	5/95	X			
Florida	10/96	6/96		X		
Georgia	1/94	11/93	X			
Idaho	7/97	-	X			
Illinois	12/95	9/95	X			
Indiana	5/95	12/94	X			
Maryland	3/96	8/95				X
Massachusetts	11/95	8/95	X			
Mississippi	10/95	9/95	X			
Nebraska	11/95	2/95	X			
New Jersey	10/92	7/92	X			
North Carolina	7/96	2/96	X			
North Dakota	7/97	-	X			
Oklahoma	10/96	-			X	
South Carolina	10/96	5/96			X	
Tennessee	9/96	7/96	X			
Virginia	7/95	7/95	X			
Wisconsin	1/96	6/94	X			
Wyoming	1/97	-	X			

Appendix Table 2

Welfare policy dates: AFDC waivers, TANF, and Time limits

	AFDC waiver		TANF implemented		Time limit implemented
	<u>Implemented</u>	<u>Approved</u>	<u>Official</u>	<u>Actual</u>	
Alabama			12/96		12/96
Alaska			7/97		7/97
Arizona	11/95	5/95	11/95		11/95
Arkansas	7/94	4/94	-		-
California	12/92	10/92	-	1/98	-
Colorado	-	-	7/97		7/97
Connecticut	1/96	8/94	1/98		1/98
Delaware	10/95	5/95	7/97		7/97
District of Columbia	-	-	3/97		3/97
Florida	10/96	6/96	2/94		2/94
Georgia	1/94	11/93	1/97		1/97
Hawaii	2/97	6/94	2/97		2/97
Idaho	7/97	8/96	7/97		7/97
Illinois	11/93	11/93	2/96		2/96
Indiana	5/95	12/94	5/95		5/95
Iowa	10/93	8/93	10/93		10/93
Kansas	-	8/96	10/96		10/96
Kentucky	-	-	10/96		10/96
Louisiana	-	2/96	1/97		1/97
Maine	-	6/96	11/96		11/96
Maryland	3/96	8/95	1/97		1/97
Massachusetts	11/95	8/95	12/96		12/96
Michigan	10/92	8/92	--		--
Minnesota	-	-	7/97		7/97
Mississippi	10/95	9/95	10/96	7/97	10/96
Missouri	6/95	4/95	7/97		7/97
Montana	2/96	4/95	2/97		2/97
Nebraska	10/95	2/95	11/95		11/95
Nevada	-	-	12/96		12/96
New Hampshire	-	6/96	10/96		10/96
New Jersey	10/92	7/92	4/97	7/97	4/97
New Mexico	-	-	7/97		7/97
New York	-	-	12/96	11/97	12/96

Appendix Table 2 continued

Welfare policy dates: AFDC waivers, TANF, and Time limits

	AFDC waiver		TANF implemented		Time limit implemented
	<u>Implemented</u>	<u>Approved</u>	<u>Official</u>	<u>Actual</u>	
North Carolina	7/96	2/96	1/97		7/96
North Dakota	-	-	7/97		7/97
Ohio	7/96	3/96	10/96		10/97
Oklahoma	-	-	10/96		10/96
Oregon	2/93	7/92	10/96		7/96
Pennsylvania	-	-	3/97		3/97
Rhode Island	-	-	5/97		5/97
South Carolina	-	5/96	10/96		10/96
South Dakota	6/94	3/94	12/96		12/96
Tennessee	9/96	7/96	10/96		10/96
Texas	6/96	3/96	11/96		6/96
Utah	1/93	12/92	10/96		1/97
Vermont	7/94	4/93	9/96		--
Virginia	7/95	7/95	2/97		7/95
Washington	1/96	9/95	1/97		8/97
West Virginia	2/96	7/95	1/97		1/97
Wisconsin	1/96	6/94	9/96	9/97	10/96
Wyoming	-	-	1/97		1/97
Total		34			47

Sources: Crouse (1999) – note these are the same dates used in the 1999 CEA report; U.S. Department of Health and Human Services, Assistant Secretary for Planning and Evaluation, *Setting the Baseline: A Report on State Welfare Waivers*; Urban Institute (1998) summary of state TANF policies.

Appendix Table 3
 Caretaker Work Exemption Policies,
 Date of Implementation and Age of Youngest Child Exempted

	No exemption	Date (Waiver or TANF)	Up to 6 months	Date (Waiver or TANF)	Over 6 months	Date (Waiver or TANF)
Alabama					1 year	11/96 (T)
Alaska					1 yaer	7/97 (T)
Arizona					1 year	10/96 (T)
Arkansas			3 mos	7/97 (T)		
California			6 mos	1/98 (T)		
Colorado	county option	(T)				
Connecticut					1 year	10/96 (T)
Delaware			13 wks	3/97 (T)		
D.C.			1 year	3/97 (T)		
Florida			3 mos	10/96 (T)		
Georgia	None	1/97 (T)				
Hawaii			6 mos	2/97 (W)		
Idaho	None	7/97 (T)				
Illinois					1 year	7/97 (T)
Indiana ¹					1 year	10/96 (T)
Iowa	None	1/97 (T)	3 mos	10/93 (W)		
Kansas					1 year	10/96 (T)
Kentucky					1 year	10/96 (T)
Louisiana					1 year	1/97 (T)
Maine					1 year	11/96 (T)
Maryland			12 wks	10/96 (W)	1 year	12/96 (T)
Massachusetts			6 mos	9/96 (T)		
Michigan	None	10/94 (W)	3 mos	9/96 (T)		
Minnesota					1 year	9/97 (T)
Mississippi					1 year	9/97 (T)
Missouri					1 year	12/96 (T)
Montana	None	2/97 (W)				
Nebraska			12 wks	3/96 (W)	1 year	12/96 (T)
			3 mos	7/97 (T)		
Nevada					1 year	12/96 (T)
New Hampshire					3 years	(T)
New Jersey			12 wks	7/97 (T)	2 years	10/92 (W)
New Mexico					1 year	7/97 (T)
New York					1 year	11/97 (T)

Appendix Table 3 continued
 Caretaker Work Exemption Policies,
 Date of Implementation and Age of Youngest Child Exempted

	No exemption	Date (Waiver or TANF)	Up to 6 months	Date (Waiver or TANF)	Over 6 months	Date (Waiver or TANF)
North Carolina					5 years 1 year	7/96 (W) 1/97 (W)
North Dakota			3 mos	7/97 (T)		
Ohio					1 year	10/96 (T)
Oklahoma					1 year	10/96 (T)
Oregon			3 mos	2/93 (W)		
Pennsylvania					1 year	3/97 (T)
Rhode Island					1 year	5/97 (T)
South Carolina					1 year	10/96 (T)
South Dakota			12 wks	12/96 (T)		
Tennessee			16 wks	9/96 (W)		
Texas					4 years	?
Utah	None	10/96 (T)				
Vermont			16 wks	7/94 (W)	18 mos	9/96 (T)
Virginia					18 mos	10/97 (T)
Washington					1 year	1/97 (T)
West Virginia					1 year	1/97 (T)
Wisconsin			12 wks	9/97 (T)	1 year	1/96 (W)
Wyoming			3 mos	1/97 (T)		

Notes: Under TANF, 26 states exempt a mother while the youngest child is under 1 year of age; Vermont and Virginia allow an exemption up to 18 months; Texas is the only state to have a higher age limit, set at 4 years, but the exemption may only be used once for each family.

1. Indiana law only allows exemptions for care of a child under 12 weeks if child is conceived while family is on aid.

Sources: Crouse (1999) – note these are the same dates used in the 1999 CEA report; Health and Human Services, Assistant Secretary for Planning and Evaluation, *Setting the Baseline: A Report on State Welfare Waivers*; Urban Institute (1998) summary of state TANF policies.

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¹ The following example is taken from Camasso et. al. (1999): in the state of New Jersey, under AFDC, a woman on welfare with one eligible child would receive \$322 per month in cash assistance. If the woman gave birth to another child, the family would receive an additional \$102 per month, and an additional \$64 per month for any additional births. Food stamp benefits would also increase, but by less than the maximum due to the incremental income from AFDC benefits. Under the family cap, the family would continue to receive \$322 with the birth of any additional child. The food stamp benefit would increase by more, however, though not enough to offset the decline in cash assistance.

² Information in this section is from the 1998 Green Book.

³ A "time limit" is a length of time for which a family can receive cash assistance and after which a family's benefit is either terminated or reduced or the family is required to participate in work requirements.

⁴ In Arkansas, 46 percent of women in the treatment group and 52 percent in the control group indicated to evaluators that they did not know how much more money that would receive if they had an additional child. In New Jersey, only 39 percent of the actual control group members knew they were in the control group and only 65 percent of treatment group members knew that they were subject to new rules (Loury 2000).

⁵ Declines in welfare caseloads over the 1990s are well-documented (see Ziliak et al. 2000). Whether welfare reform or the robust economy is responsible for the declines, if the composition of the welfare caseload has been altered, then conditioning the analysis sample on welfare-receipt is problematic.

⁶ As reported in the technical appendix of the 1997 vital statistics report, birth certificates in 46 states and Washington D.C. include a direct marital status question. Nevada collects marital status information from the electronic birth registration process, though it is not included on the birth certificate. This procedure was started in Nevada in 1997, after 1995-1996 procedures overestimated the number of births to unmarried women. The remaining three states of Connecticut, Michigan, and New York make marital status determinations based on whether a paternity acknowledgement was received, the father's name is missing, and lastly, whether the father's and mother's surnames are different. A direct question was not added in Texas and California until 1994 and 1997, respectively.

⁷ I thank Robert Shoeni for providing me with this data, which was used in the 1999 CEA report.

⁸ For ease of exposition, women whose race is not classified as "black" in the natality files are considered "white".

⁹ This is implemented using the "cluster" command in STATA.

¹⁰ Another way to potentially address endogeneity is to distinguish between states that adopted the family cap as part of TANF and those that requested family cap waivers. States that request waivers are arguably more likely to be responding to shifts in birth outcomes than are those states that implement a cap as part of the national reform of welfare. However, 19 out of the 23 states that enact any type of family cap legislation requested waivers to do so, making the exercise meaningless. Instead, I re-estimate Equation 1 dropping the five states that had waivers approved before 1995 when a family cap was still a novel idea. The point estimate in this specification is -0.002 , with a standard error of 0.009.

¹¹ Point estimates for the effect of the family cap on first births are not significantly different from zero; results are listed in Schettini (2000).