Natalist Policies in the United States

Leonard M. Lopoo
Kerri M. Raissian

Abstract

This retrospective reviews the policies that affect the fertility of American women, both policies designed to alter fertility intentionally as well as those that change childbearing unintentionally. Becker's seminal work on the economics of fertility serves as the theoretical foundation for this literature. After describing Becker's economic model, we review the empirical literature on fertility responses to social welfare policies, tax policies, the mandated health care coverage of infertility treatments, abortion policies, and government-sponsored family planning services. We also address several Supreme Court cases that have played an important role in the interpretation of these policies. Where relevant, this retrospective describes the distributional effects of these natalist policies. We also discuss the limitations of this literature and identify important gaps. Unlike most developed countries that have created strategies to increase fertility to support their ageing population, the United States spends considerably less time and thought on this issue. Our reading of the literature suggests that we have many public policies that have affected and continue to influence the fertility choices made by families in the United States and that this is a topical area that deserves more attention in policy debates. © 2012 by the Association for Public Policy Analysis and Management.

INTRODUCTION

Despite the absence of one overarching, explicitly stated population policy, the United States has many public policies that theoretically should affect fertility. Several of them, such as Title X of the 1970 Public Health Service Act, are explicit in their intention to allow women to gain greater control of their childbearing. Social scientists have also long been aware that many public policies that have objectives that ostensibly have nothing to do with fertility, alter the costs of parenthood, and therefore, may influence reproduction, albeit unintentionally. The literature on the fertility effects of the Aid to Families with Dependent Children (AFDC) program is but one example. In addition to these explicit and implicit natalist policies, the United States has undergone a number of important legal decisions, including *Roe vs. Wade* and several subsequent rulings on abortion, that have impacted the fertility of American women. These natalist policies and the legal decisions clarifying these natalist policies are the subject of this policy retrospective.

We will first examine one of the most well-researched areas relevant for this retrospective: the literature on social welfare policies that affect nonmarital

Journal of Policy Analysis and Management, Vol. 00, No. 0, 1–41 (2012)

Published by Wiley Periodicals, Inc. View this article online at wileyonlinelibrary.com/journal/pam
DOI:10.1002/pam.21646

childbearing. These programs include Temporary Assistance to Needy Families (TANF), Child Support Enforcement (CSE), Medicaid, and the Food Stamp Program (FSP) (now called the Supplemental Nutrition Assistance Program or SNAP). These programs may affect fertility via different mechanisms. For example, the CSE program was designed specifically to make nonmarital childbearing more costly to men. Others programs affect the likelihood of nonmarital childbearing because the programs are means-tested, but are designed for parents specifically. Because married families earn more money on average than single parents, many of these programs may induce parenthood while they deter marriage. The second topical area is a small public finance literature that shows there are substantial tax consequences for having a child. We describe the features and changes in the Internal Revenue Code relevant to the fertility of American women and summarize the existing policy analysis. We then review the developing literature that examines the effect of state-level mandates that health insurance providers cover infertility treatments. These programs give more families access to these expensive treatments, which should increase the fertility of U.S. women. Thereafter, we describe the history of abortion policy in the U.S. and the changes in fertility that resulted from its legalization. As this history shows, access to abortion has differed for women geographically and by age, and this variation may have produced important changes in childbearing. One of the most, if not the most, substantial factor affecting the fertility of American women in the last half century was the introduction of Enovid, the first birth control pill. We next discuss its impact on fertility along with several policies and programs designed to provide women greater access to contraceptives and other family planning services. For every topical area, we briefly explain the program or policy and why researchers believe these policies may have natalist effects. We then review the relevant literature and draw conclusions.

This retrospective makes clear that the United States has many explicit and implicit natalist policies, and they potentially affect the complex choices that families make regarding their childbearing. Understanding the role that policy can play in fertility choices is important for several reasons. Most simply, one would want to know if the explicit natalist policies are efficacious. For example, the justification made for family planning programs designed for low-income women is to allow them to have the same control over their fertility that middle-income women experience. Knowing if this can be accomplished is important for policymakers to consider as they fine-tune these policies and allocate funding. As we illustrate below, some of the empirical research on these explicit natalist policies suggests that they have played a major role in the demographic changes that we have observed in the United States over the last 40 to 50 years.

In addition, many public policies may alter the fertility of women unintentionally. Informing policymakers of the natalist implications of these public policies seems prudent as they debate, design, and implement them. It is not only important to know if the policies have implicit natalist effects, but also to know which types of policies are pronatalist and which are antinatalist. One should also consider the distributional effects of these natalist policies. Many of the programs and policies investigated target the low-income population. Understanding if there are differential effects for families in different portions of the income distribution would also be informative for policymakers as it relates to the demographic composition of the population.

Finally, although it is important to understand the effects of any given policy, it is also important to evaluate these policies as a collective. The lack of a cohesive population policy presents a unique evaluation challenge to policymakers and scholars. One may wish to know if the various natalist policies, explicit or implicit, complement or compete with each other. As this retrospective will demonstrate, some policies increase fertility while others moderate it. While we are unable to comment

on the overall effect of these policies, this retrospective provides an opportunity to review, and perhaps spark a larger discussion about, U.S. natalist policies.

The literature on fertility and policy is massive; therefore, we have chosen to set some boundaries for the papers we review in this policy retrospective. As implied earlier, we confine the subject matter to government policies in the United States, acknowledging that there exists a large literature on this topic for Europe and many Asian countries.[1] Second, we have chosen to constrain our review of the literature to changes that have occurred since 1960, a period that includes the introduction of the birth control pill and abortion legalization. Third, we limit our review to those studies that report fertility, i.e., birth, responses. There are many policy studies that show a pregnancy effect without reporting the fertility effect, and we do not include them. It is plausible that a policy could affect pregnancy rates and abortion rates simultaneously. Thus, a study that shows an increase in pregnancy in response to a policy could also produce an increase in abortion, which might result in a smaller increase in births than suggested by the increase in pregnancy or even no change in births. If a study (or an entire body of literature) on the topic only describes the pregnancy or abortion response without describing fertility effects, we chose not to include it in this retrospective. Finally, within the topical areas described earlier, we constrain the review to include only empirical work that demonstrates responses to government policies created or adjudicated at the federal level with one important exception: the state-mandated coverage of infertility treatments within health insurance policies. We note that many of the studies reviewed use variation at the state level to identify effects, but we emphasize that the policies were legislated at the federal level. We make an exception for the infertility coverage because at this point nearly one-third of all states have mandated this coverage and results from this literature have important policy implications.[2]

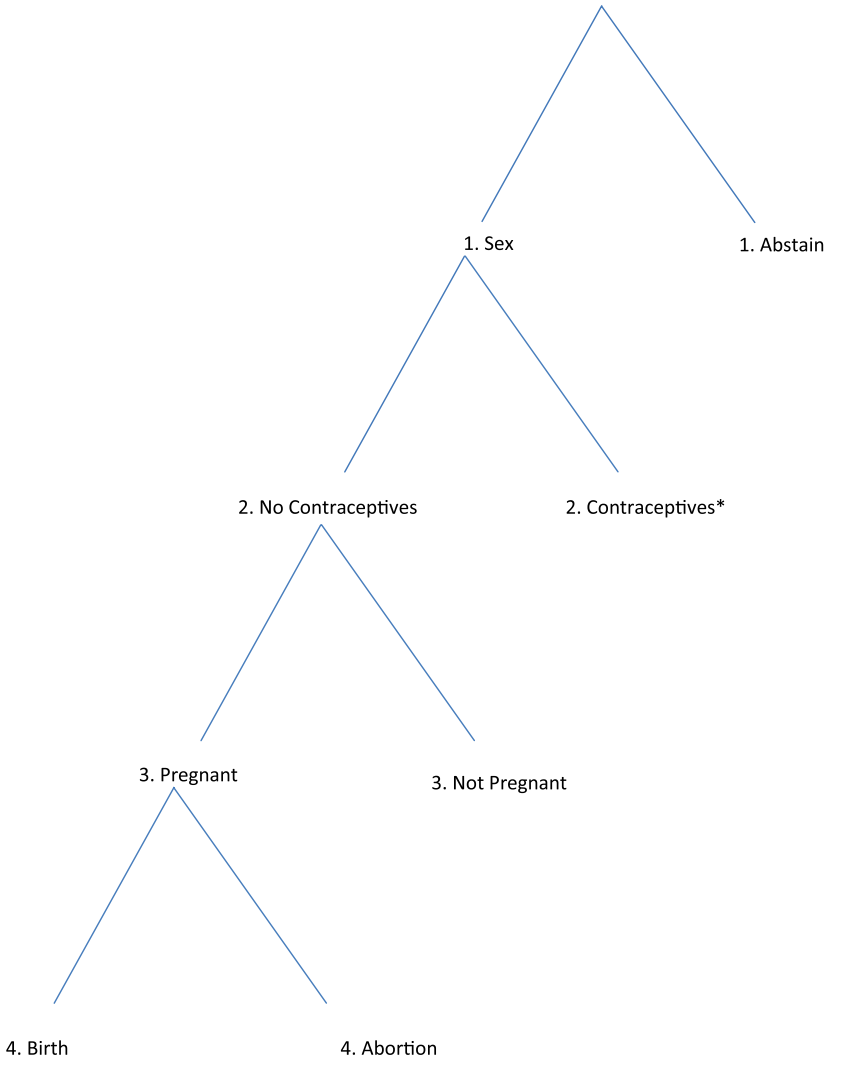
This paper proceeds as follows. We first provide a theoretical framework to guide our discussion of the policies under review. We include a summary of Becker's seminal work on the economics of fertility as these ideas largely motivate the empirical literature surrounding policies that alter the cost of a child. Our policy review begins in the third section where we detail the considerable literature on social welfare policies that may have influenced the nonmarital fertility of low-income women. In the subsequent section, we describe important changes in tax policy in the United States. In the fifth section, we cover the literature on state mandates for infertility treatments. Then, we describe abortion policy in the United States, followed by research on policies that provide low-income families greater access to family planning services. We conclude in the final section.

THEORETICAL BACKGROUND

To understand the theory that motivates the literature in this policy retrospective, it is helpful to first briefly describe the sequence of decisions people make that can lead to parenthood. It is also important to note that despite individuals' intentions, some of the outcomes are beyond their control, that is, the outcomes are not deterministic. Figure 1 is a simplification of this series of decisions and outcomes, and we describe it from the perspective of a woman. The first step in this process is a decision if and when to become sexually active. Should she choose to become sexually active, she must next consider her use of contraceptives. Figure 1 glosses

[1] See, for example, Gauthier and Philipov (2008), Grant et al. (2004), Hesketh and Zhu (1997), and Hesketh, Lu, and Xing (2005).

[2] For example, in 2009 legislation was introduced in both the U.S. House of Representatives and the Senate to mandate infertility treatment coverage nationally (Buckles, 2011).



* Clearly, some proportion of women who use contraceptives will become pregnant and will then make the abortion or birth decision. We have removed this branch of the tree for simplicity.

Figure 1. Decision and Outcome Sequence Leading to Parenthood.

over the fact that she has a variety of contraceptives from which to choose and that she must (often in concert with her partner) make this choice for each act of sexual intercourse. Regardless of her decision to use contraceptives, she may become pregnant, although the probability of a pregnancy will decline with the use contraceptives. Finally, should she become pregnant, she must decide whether to give birth or have an abortion.

Historically, much of the empirical research on fertility in the social sciences, including several topical areas in this retrospective, was motivated by Becker's (1960) seminal theoretical work in the area. Becker was curious regarding a counterintuitive empirical regularity he noticed: as countries developed, the total fertility rates of women within the country typically declined, which implied that children were

inferior goods. Yet, most social scientists, including Becker, believed that children were normal goods.

To explain this contradiction, Becker argued that the demand for children could be treated much like one would treat the demand for consumer durables. Given a set of preferences, the quantity of children demanded is a function of prices and income. Importantly, Becker also posited that one must separate the quantity of children demanded from the quality of children demanded, which he defined simply as the investments made in children, such as their nutrition and education. Following the patterns observed for many other consumer durables, Becker asserted that as income increases, one should expect to see an increase in the quantity and quality of children demanded, with quality probably receiving a much higher proportion of additional expenditures. Subsequent empirical work often found that the income effects may actually drive the quantity of children demanded down, if parents substitute investment in the quality of children for investments in the quantity of children (Levine, 2004).[3]

More important to the empirical research on fertility, Becker (1960, 1965, 1991) also explains that one can derive the cost for child services as the sum of all of the prices of the individual inputs into the production of children. These inputs are comprised of the direct costs of investment, such as clothing, education, and food, as well as the indirect costs of children, including the opportunity costs for women who forgo some of their earnings to care for their children. As these costs increase, one should expect the quantity of children demanded to fall: the impact on the quantity demanded depends on the proportion of the budget devoted to that particular item and the availability of substitutes. As the cost of college increases, for instance, one should expect parents to have fewer children, all else equal, because it constitutes such a large portion of the cost of a child and has few substitutes.

Much of the empirical literature that we review for this policy retrospective is founded on this model and its straightforward implications. If one holds the benefits of a birth constant, one should consider how the costs of becoming a parent are changed by the policy. If the costs increase, one should ordinarily expect a decline in fertility. If the costs are reduced by the policy or program, all else equal, one should expect the quantity of children demanded to increase.

Importantly, Becker's model assumes that women understand completely the costs and benefits of parenthood and that they can control their fertility perfectly at no cost (Levine, 2004). As a result, Becker's model provides no guidance on the expected changes in the decision to use contraceptives or obtain an abortion conditional on a pregnancy as policies are altered. For instance, Becker does not need to account for the abortion decision because a woman would only become pregnant if she intended to become a mother.

Other policies described in this retrospective, however, consider how changes in policy alter the decision calculus that women (and men) make at different steps within this sequence. Sometimes this literature expands on Becker's theory, while in other branches, the literature simply models the decision in isolation. Consider that federal funding for family planning reduces the cost of contraceptive use. As a result, one should expect more women to use them, thereby reducing fertility. As we detail below, the legalization of abortion affected both the contraceptive decision and the likelihood of abortion conditional on a pregnancy. While Becker's model showing that changes in the cost of a child are often the guiding theoretical explanation for fertility changes, where relevant, we will elaborate on the specific changes a

[3] This policy retrospective is principally about change in the quantity of children, although we make references to the quality–quantity tradeoff where relevant.

policy may have had on the decisions within the parenthood sequence illustrated in Figure 1.

Finally, the theoretical work of Becker and a variety of other economists considers the total demand for children. In other words, they ask how price or income changes alter the number of children a parent has. Many empirical analysts have suggested that the changes observed as the result of a cost change may modify the timing of childbearing, while leaving the total number of children the mother has over her lifetime unchanged (e.g., Bitler & Zavodny, 2010; Leibowitz, 1990; Levine, 2004; Whittington, 1992). Thus, a family may have two children both in the absence or the presence of a particular policy. The presence of the policy simply modifies the age at which the mother has her children. While not always the case, some of the empirical work on fertility cannot distinguish whether the policy alters the timing of childbearing, the total number of children, or both. We highlight these differences throughout this retrospective.

POLICIES THAT AFFECT THE COST OF NONMARITAL CHILDBEARING

Aid to Families with Dependent Children/Temporary Assistance for Needy Families

Studies of the welfare program easily constitute the largest relevant literature for this policy retrospective. To properly review this literature, it is important to provide some of the legislative history of the program, changes in the composition of recipients of welfare over time, and important theoretical contributions to our understanding of the effect that welfare may have on the fertility of the low-income population.

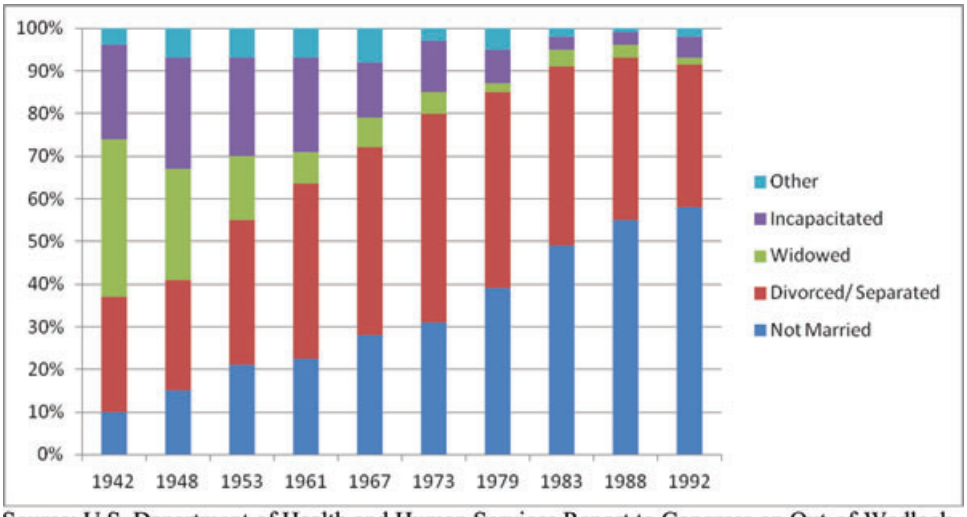
Background

The origins of the welfare program date back to 1935 when the Aid to Dependent Children (ADC) program was first created by the Social Security Act. The original act authorized aid to fatherless children, and during the early years of the program, households headed by widows comprised the bulk of the caseload, while never married mothers constituted a small proportion of beneficiaries (see Figure 2). The ADC program, later renamed AFDC, provided cash assistance monthly to qualified low-income families; most recipient families were composed of single women with children.

Over time, the population served by AFDC both increased and changed in noticeable ways. The per capita caseloads increased in the 1970s, remained flat during much of the 1980s, and rose fairly rapidly during the late 1980s and early 1990s (Moffitt, 2008). The composition of caseloads changed dramatically over time as well. As Figure 2 shows, the fraction of households headed by widows declined consistently while the fraction of "not married" mothers continually increased over time.

In 1984, Charles Murray published *Losing Ground,* which fundamentally altered the way most analysts think about the social safety net in the United States. Murray pointed out that although welfare was designed to provide assistance to low-income families, it also reduced the cost of children. The AFDC program itself provided a cash subsidy,[4] but recipients were automatically qualified for other programs, such as Medicaid and Food Stamps. As a result, enrolling in AFDC

[4] In 1994, the maximum benefit for a family of three in the contiguous United States ranged from $120 in Mississippi to $680 in Connecticut (Blank 1997).



Source: U.S. Department of Health and Human Services Report to Congress on Out-of-Wedlock Childbearing (1995).

Figure 2. AFDC Recipients by Marital Status.

provided a nontrivial bundle of cash and in-kind goods that could potentially increase fertility among the low-income population (as well as affecting their likelihood of marriage and their living arrangements). The AFDC underwent several major reforms during its long history, many of which were designed to increase self-sufficiency among recipients. These reforms included reductions in the tax rate on benefits (called the benefit reduction rate) incurred as women earned labor income as well as the introduction of job training (e.g., the Job Opportunities and Basic Skills Training (JOBS) program).[5] For a variety of reasons, these reforms largely failed to improve the self-sufficiency of AFDC recipients. In response to these failures, the federal government gave many states the opportunity to experiment with their welfare programs. From January 1993 to August 1996, the Department of Health and Human Services (DHHS) granted 43 states welfare "waivers" (Department of Health and Human Services, 2001). States experimented with a variety of welfare criteria, including increasing work requirements, creating family caps, placing time limits on the length of receipt, and imposing sanctions of recipients who failed to abide by welfare regulations. The period in the early 1990s when many waivers were granted is largely considered the first phase of welfare reform.

The changing caseload composition, increased labor force participation of middle-class mothers, and the interest in increasing self-sufficiency among the poor along with the argument that social welfare programs were contributing to poverty culminated in President Clinton signing the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA), transforming the AFDC program into the Temporary Assistance for Needy Families (TANF) program. In addition to its mandate to increase employment and self-sufficiency among recipients, an unambiguous goal of TANF was to "prevent and reduce the incidence of out-of-wedlock pregnancies and establish annual numerical goals for preventing and

[5] See Moffitt (2008) for details.

reducing the incidence of these pregnancies" (Personal Responsibility, 1996).[6] In Section 403, the federal government committed to rewarding up to five states for reducing their nonmarital birthrates without increasing the rate of induced pregnancy terminations. Unwed teenagers received special attention in the legislation with the "Minor Parent Provisions" (MPPs). The MPPs allowed states to use federal funds to aid teen mothers under age 18 if they were (1) attending secondary school or another educational forum related to obtaining employment and (2) living with their parents or in another adult-supervised setting (teen mothers living with their husbands are excepted from the latter provision; Haskins & Blank, 2001).

Despite having national goals, a major hallmark of TANF was the transfer of program logistics from the federal government to state governments. TANF is now a highly decentralized program with no two states having identical benefit schedules, sanctions, or incentives. TANF is funded as a block grant, meaning each state is given a lump sum of money and has extensive discretion on how to spend TANF dollars (e.g., the state could use the money for child care subsidies or job training programs rather than cash benefits to recipients). As intended, many states adopted additional policies (family caps, earnings disregards, sanctions, work exemptions, work requirements, varying time limits, to name a few) to complement the federal changes. The across state and timing variation generated by the decentralized nature of TANF and welfare waivers are the sources of identification for many evaluations highlighted in this review.

Fertility Effects of the TANF Program

Because the research on the AFDC program has become dated at this point, we focus our attention on the TANF literature. Nevertheless, it is worth mentioning Moffitt's (1998) summary of the empirical effects of AFDC on family formation and fertility:

> if there were a sizeable effect of welfare on demographic behavior, it would probably be more evident with the available statistical methods than appears to be the case in the research literature. The findings reported in the chapters are . . . consistent with the existence of a small, real effect but one that is difficult to detect and sensitive to the methodology used (p. 5).

Taken as a whole, welfare reform can be seen as a "contractionary policy" response (Moffitt, 1999), that is, the value of TANF benefits is likely to be perceived as less than the value of AFDC benefits. While TANF still reduces the cost of children for families, it is no longer as generous: benefit receipt brings several new requirements, and they are time-limited. As such, welfare reform should lead to a decrease (or delay) in fertility for women on the margin relative to the decision the same woman would make during the AFDC era.

Although Moffitt's 1998 review found little effect of AFDC benefit levels on fertility, Blank (2002) concludes that there is no reason to expect the fertility response to TANF to be similarly small. She writes

> a host of other behavioral incentives and mandates have been imposed on welfare recipients. These might have different and stronger effects, particularly if these changes

[6] TANF, unlike its predecessor, is not an entitlement program, meaning states are no longer required to provide benefits to anyone who was eligible to receive them. To obtain benefits, recipients have to adhere to eligibility rules, which involve participants making strides toward self-support through job searches, education, and working. Additionally, the federal government imposed a five-year time limit on how long any given recipient could receive cash benefits from federal funds. Please see Haskins and Blank (2001) for a detailed overview of TANF changes and funding sources.

> seriously limit welfare benefit availability. Second, programs like family caps and minor parent provisions are directly aimed at changing fertility behavior and might have larger and more direct effects than changes in benefits or availability (p. 1155).

Scholars have analyzed the effect of welfare reform as a whole (e.g., as a collective bundle of policies), and they have asked separately about several new components of TANF, for example, family caps, as well as estimating effects for subgroups within the population, focusing on teenagers in particular. We report findings from this literature below. One final note about the era of welfare reform is important to consider beforehand: TANF was implemented in a period of economic recovery. While almost all of the literature uses a differencing strategy that identifies the effect of welfare reform relative to some comparison group, that coefficient is implicitly interacted with economic recovery. We have no way of knowing how our understanding of welfare's effect on fertility would have been altered had reform been implemented in different economic circumstances.

We will proceed by reviewing the literature on the overall effect of TANF, followed by the effect of family caps, and conclude with the effect of reform on teenagers.

TANF's Net Effect on Fertility

The literature on TANF's effect on fertility is suggestive but, ultimately, inconclusive. Moffitt (2002) points out that empirical studies have been thwarted by the fact that TANF was implemented in most states in roughly the same time period and that TANF is substantially more complex than its predecessor, making identification difficult. Many of these studies use a difference-in-differences (DD) estimator, which relies on correctly identifying the "treated group" (those targeted or affected by the policy) and a "comparison group" (a group unaffected by the policy whose changes in trends closely approximate what would have happened to fertility in the treated group in the absence of welfare).

Grogger, Karoly, and Klerman (2002) conducted a literature synthesis on the consequences of welfare reform, and their reading of the literature finds no effect of mandatory work-related activities (i.e., that women had to work or be engaged in activities to acquire human capital) on fertility decisions. Joyce, Kaestner, and Korenman (2003) identify women in a given state as being "high-risk" (the treatment group) or "low-risk" (the comparison group) of welfare receipt based on the mothers' marital status and educational attainment. Using a DD estimator, they find mixed results that suggest, if anything, welfare reform increased fertility among black and white women by 2 to 3 percent; however, the results are sensitive to the choice of a comparison group. Using the March Current Population Survey (CPS), Kaushal and Kaestner (2001) employ a similar research design: a DD estimator that identifies a woman in a given state as being high or low risk for welfare receipt based on her marital status, educational attainment, and in some specifications, the number of children she has. They conclude there is little effect of reforms on fertility decisions. They develop a measure of low-, medium-, and high-intensity reform states, and show that low-intensity reform is associated with larger declines in fertility compared to high-intensity states. This finding is counterintuitive, and they suggest the result may be due to endogenous policy adoption and urge caution in interpreting their results.

In addition to policies designed to motivate changes in individual behaviors, PRWORA also created the "Out-of-Wedlock Birth Reduction Bonus" (or "Illegitimacy Bonus") as an incentive for states to create or modify their programs. The federal government announced that it would award $100 million annually for five years to the five states that achieved the greatest reduction in their nonmarital birth ratio (NMBR) without an offsetting increase in abortions. Korenman et al. (2006)

ask if the declines in the NMBRs were an increase over the decline that would have been observed in the absence of the bonus.

Korenman et al. (2006) limit their analysis to Alabama, Michigan, and Washington, D.C. because these jurisdictions each won the bonus at least four times, claiming 60 percent of the total award dollars. Their analytic strategy involves decomposing the NMBR into three distinct parts, which allows them to better determine if compositional changes or changes in fertility behaviors are leading to a reduction in the NMBR. In these three jurisdictions, African-American women are high risk for having a nonmarital birth, thus the ratio is sensitive to changes among this group. Only in Michigan were the reductions in the state's NMBR driven by women being less likely to have a nonmarital birth. Their analysis reveals that the dramatic declines in D.C.'s ratios were largely attributed to compositional changes, the proportion of African-American women as a share of the population shrank. In Alabama, African-American women had fewer children, and this drove down the overall ratio, but paradoxically the ratio actually *increased* on average within race. Finally, there is also little evidence to indicate that states directly engaged in efforts to reduce the NMBR in order to win the bonus, or that after winning the bonus, they reinvested the money toward strengthening or sustaining efforts to further reduce the ratio (Nowak, Fisherman, & Farrell, 2003).

Family Caps' Effect on Fertility

Family caps (or the child exclusion policy) were implemented to reduce out-of-wedlock childbearing. Under AFDC, a family's benefit level was a function of their family size. Some policymakers argued this created perverse incentives for women to have more children to increase their benefit level. Family caps eliminate any increase in benefits for mothers who have additional children while they are receiving welfare. The value of this potential benefit varies by state, but in 1996, a family of four received an average of $72 more per month than a family of three .[7,8] Of the 24 states that implemented family caps, 19 did so between 1992 and 1996 through waivers from the federal government (Kearney, 2004; Romero & Agenor, 2009).

Perhaps because family caps were designed and implemented in an era of experimentation, they are not uniform across states. These variations have direct impacts on how the family cap changes the cost of an additional child.[9] Some states reduce rather than eliminate benefits for additional children. Others provide vouchers for goods and services to offset the loss in benefits, while some states increase earnings pass-throughs and child support disregards. These variations serve to lessen and perhaps even negate the impact of the family cap on the family budget. States may also reduce the JOBS or work exemptions. Women who have young children are often exempt from working or making progress toward finding employment. However in some family cap states, the time allowed for exemption is reduced, that is, women have to return to work sooner. Under this scenario, women face a difficult trade-off—if she stays at home, she forgoes welfare assistance but if she goes to work, she forgoes time with her child and must make childcare arrangements. This makes having additional children while on welfare more costly. In addition, states will vary in their strictness, implementation, and how well they inform women of

[7] Of course there are deviations from the average. The maximum difference (in Hawaii) was $147 per month and the minimum difference (in Mississippi) was $24 per month.
[8] Authors' calculations using the National Data Set made available through the University of Kentucky Center for Poverty Research.
[9] The following description of family caps' variation comes from information provided by the DHHS (2001, Table III).

these policies. Despite this policy heterogeneity, many studies use a binary variable to indicate if a state is subject to a family cap missing the subtlety of the policy.

Based on Becker's economic theory of fertility, one should expect the withdrawal of these additional benefits to decrease women's propensity to bear additional children. Compared to a world without caps, family caps increase the cost of additional children for the family. Kearney (2004) argues family caps may also decrease or delay first births if the adoption of the policy signals a decline in welfare's overall generosity. However, despite a clear theoretical prediction, analyses that have sought to test and quantify the effect of family caps have generated empirically ambiguous results. This may be due to the various identifying assumptions different authors make, a discussion we will return to after reviewing the literature. [10]

Horvath-Rose and Peters (2001) were among the first to use national data to ask about the effect of the family caps on birthrates. Using a DD strategy, they compared aggregate, state-level NMBRs in states with and without the family cap and find that the caps reduce fertility. Given that changes in either marital or nonmarital births can lead to a change in the ratio, it is difficult to know what is driving the result. Furthermore, their findings have been criticized because there are very few observations after welfare reform and the dates of the waivers used in their paper differ substantially from those reported elsewhere (Kearney, 2004). Sabia (2008) specifically addresses these coding concerns and finds that differences in coding are not driving the results. If anything, Sabia demonstrates the findings from Horvath-Rose and Peters are smaller in magnitude due to their coding scheme. Like Horvath-Rose and Peters, Sabia (2008) finds negative effects for nonmarital birthrates among all women, but shows that declines among black women drive these results.

In contrast, several of the studies find that the family cap or incremental increases in benefits have no effect on women's fertility (Dyer & Farlie, 2004; Grogger & Bronars, 2001;[11] Joyce et al., 2004; Kearney, 2004; Romero & Agenor, 2009) but there is some nuance to the results. Using birth certificate data from the National Center for Health Statistics (NCHS) between 1989 and 1998, Kearney (2004) uses a DD estimator where the log of overall birthrates for women aged 15 to 34 in states that passed the family cap are compared to the log of the same aggregate birthrates in states that did not implement a family cap. Her results show that the family cap had no effect on total birthrates. When Kearney limits her analysis to higher order births, she finds positive and significant effects (that the family cap actually increased births) among white and black women without a high school degree as well as among unmarried, black teen women. She acknowledges that her results contain some positive spurious correlation. Of interest is whether or not this bias extends to the primary findings on total birthrates and whether or not the nature of the bias remains positive.

Two studies (Dyer & Fairlie, 2004; Joyce et al., 2004) use a difference-in-difference-in-differences (DDD) model to identify the effects of the family cap. In addition to using the across-state variation used in the DD studies, these studies exploit within state variation. Within a family cap state, there is a group of women that is more

[10] Two experiments were conducted at the state level (one in Arkansas, the other in New Jersey) to estimate the effect of family caps on fertility. The Arkansas experiment showed no policy effect (Turturro, Benda, & Turney, 1997), while the New Jersey experiment found a strong, negative effect of family caps on fertility (Camasso, 2004; Camasso et al., 1998). While we do not review these experiments further, Grogger, Karoly, and Klerman (2002) note that "the methodological issues surrounding the two random assignment analyses of family caps are so severe as to require that those results be strongly discounted" (p. 146).

[11] Grogger and Bronars (2001) use the size of the incremental benefit (not the family cap policy) to measure a woman's propensity to have additional children. They argue if women are not responding to the presence of the incremental benefit, then its termination is unlikely to influence their behavior.

likely to be affected by welfare reform than other women in the same state. The benefit of a DDD research design is that an appropriate within-state control group negates the need for time-varying controls. Both studies define the treatment group similarly (single, less educated women with children) but Joyce et al. use single, less educated women without children as their control group while Dyer and Farlie use married, less educated women as their counterfactual. Both studies find that birthrates do not respond to family cap policies.

This literature clearly disagrees about the effects of family caps on birthrates. All of the studies reviewed were carefully done, so reconciling the findings is difficult. One potential explanation is the use of time-varying state level controls. Both studies that find a negative relationship between family caps and birthrates use an extensive array of controls (socioeconomic, political, or both). Kearney (2004) and the two DDD studies do not include a rich set of state- or group-level time-varying controls. It is possible that the use of time-varying controls helps to overcome bias. There is also inconsistency in the choice of the dependent variable, some researchers use the total birthrate whereas others choose a higher order birthrate, and the studies focus on women of different ages; however, our reading of the papers does not suggest these differences explain the various findings.

Finally, Horvath-Rose, Peters, and Sabia (2008) provide evidence that the adoption of family caps is not exogenous, which raises questions about the fundamental identifying assumptions of the models used in the previously discussed papers. This paper finds that while family caps are associated with a decrease in nonmarital birthrates (and the NMBR), the caps are associated with an increase in marital birthrates, a group that should be largely unresponsive to the policy change.[12,13] Obtaining significant coefficients for the placebo group leads the authors to speculate that some omitted variable is driving the results. This finding casts doubt on the validity of the DD studies, but it also casts doubt on the validity of the within-state control groups used in the DDD studies. We conclude that while the majority of the evidence leans toward concluding family caps did not affect fertility, this remains an open question as the current set of studies have not adequately addressed the issues of policy endogeneity or employed a robust counterfactual in their research designs.

Teenage Childbearing and the MPPs' Effect on Fertility

Teenage childbearing received special attention in PRWORA. Teen birthrates declined sharply in the 1960s and 1970s and remained relatively flat until the late 1980s when they began to rise again. They peaked in 1991, and until very recently, have been declining steadily over time. In the 1990s, however, the relatively high levels of teen birthrates were troubling to policymakers. If language in PRWORA is a reflection of concerns held by Congress, then it seems they were moved to act by both the negative effects of teenage childbearing on children[14] and the direct

[12] One exception is women who marry in response to the policy, but there has been little evidence to support this alternative hypothesis.

[13] One prewelfare reform study (Fairlie & London, 1997) assesses the effects of incremental increases on birthrates for groups affected by welfare and those that should not be (single women without children and married women with at least one child) and finds a similar pattern: women who should not be affected by the AFDC policy are responding.

[14] There is little research consensus about the actual consequences of teenage childbearing for mothers or their children. While much research documents negative outcomes for teen mothers and their children, Geronimus and Korenman (1992) were the first of many to demonstrate that there is adverse selection of mothers into teenage childbearing. Therefore, it may be that unobserved factors, and not the mother's age at the time of delivery, are the real cause of negative outcomes.

monetary costs. PRWORA states that children born to unmarried, teenage mothers have "lower cognitive scores, lower educational aspirations, and a greater likelihood of becoming teenage parents themselves" (1996, Section 9I). It also states that mothers aged 17 and younger are more likely to go on welfare and are more likely to stay on welfare, causing a young woman to enroll in welfare both "younger and longer" (1996, Section 8A). Finally, PRWORA estimates the combined public cost of teenage childbearing from welfare, Food Stamps, and Medicaid to be $120 billion between 1985 and 1990 (1996, Section 6G).

To reduce nonmarital teenage childbearing, Congress enacted the MPPs in the TANF legislation. The MPPs require that in order to receive benefits, unwed, minor mothers (i.e., mothers 17 and younger) must attend school or a training program unless they have completed high school or its equivalent, and these mothers must live with an adult (usually a parent or guardian). Minors are subject to all of the TANF reforms, but in addition, they must comply with the MPPs in order to receive welfare benefits. Therefore, one would expect the MPPs to further depress birthrates among minors, as this represents an additional reduction in benefits for this demographic group. At the same time, Hao and Cherlin (2004) assert that welfare reform might have the unintended consequence of increasing birthrates among young women. If TANF reduces parental supervision because a single mother enters the labor force, then it may be more difficult for parents to control adolescents' risky behavior, and the end result could be more teen pregnancies. Most studies focus on the effect of welfare reform on teen fertility (which is the combined effect of all welfare policies that affect women over 18 as well as the MPPs).[15]

Kaestner, Korenman, and O'Neil (2003) combine the National Longitudinal Survey of Youth (NLSY)79 and NLSY97 to measure the change in teen fertility before and after welfare reform.[16] They use each woman's family structure and the education level of her parents to identify her relative risk of welfare receipt. The DD estimate is crafted by subtracting the change in fertility rates among the low-risk women from the change in fertility rates among the high-risk group. They find that welfare reform is associated with substantially reduced fertility among older (19-year old) teenage women (30 to 50 percent and nearly always statistically significant—though we note the inclusion of state fixed effects often attenuates the estimates and, in some cases, makes them insignificant), but their results do not provide firm evidence for a similar conclusion regarding the fertility of younger (17-year old) teenage women.). Because nearly 20 years separates the teens in the prereform period from those in the postreform period, it is difficult to attribute welfare reform as the sole cause of the observed differences in the likelihood of childbearing (Acs & Koball, 2003; Offner, 2005).

Offner (2005) assesses TANF's impact on a series of outcomes (dropping out of school, teens' incidence of living at home, and nonmarital births) using the March CPS from 1989 to 2001.[17] Following Kaestner, Korenman, and O'Neil (2003), he uses a DD estimator and assigns high-risk women as the target group and uses low-risk women as a comparison to net out general social trends; however, unlike the previous study, the data in the time series is not interrupted. Thus the threat of compositional changes or some other omitted variable driving the result is lessened. The study indicates that TANF is responsible for a 1.1 to 1.6 percentage point (or

[15] Horvath-Rose and Peters (2001) find the MPPs have positive effects on teen fertility, but they include women aged 18 and 19 in their treated group.
[16] Data from the NLSY79 comprise the "pre" period, while data from the NLSY97 comprise data from the "post" period.
[17] The CPS does not provide the teenage birthrate; instead, it reveals if the teenager is living with her own children.

16 percent) decrease in the likelihood of teenage birth. Offner summarizes his results as being consistent with those of Kaestner and his colleagues.

In contrast to the studies described above, Hao and Cherlin (2004) look at the effect of welfare reform on very young teens (aged 14 to 16) using the NLSY97 and conclude that welfare reform did not decrease teen fertility and may have increased teen fertility among families receiving welfare. The study uses a "pre-reform" cohort (i.e., women who were ages 14 to 16 immediately prior to welfare reform) as the comparison group and a "reform" cohort (i.e., women who were ages 14 to 16 immediately following welfare reform) as the "treated group." The authors describe their research design as a DD, but their results and description are better categorized as a simple difference in fertility among the two groups. All of the data for the comparison group is in the "pre" period, while all of the data for the treated group is in the "post"-welfare reform period. The regressions almost always reveal a negative effect of welfare on birthrates, though we note this could be biased because the secular trend is not removed. Even when they interact, welfare reform with a series of variables designed to proxy for target populations (e.g., poverty status, parent receives AFDC, or single-parent family), the main effect of welfare is usually negative though imprecisely estimated.

Lopoo and DeLeire (2006) use natality data from the NCHS to specifically address whether the MPPs affected fertility behaviors. They compare birthrate trends for young women aged 15 to 17 to a control group comprised of 18-year olds (both groups are subject to TANF reforms, but only the former group is subject to the more restrictive MPPs). Consistent with the earlier findings, they show that the younger teenagers experience a more rapid decline in birthrates when the MPPs are adopted relative to the control group. The authors conclude that the MPPs resulted in an additional 0.7 percentage point (or 22 percent) decline in birthrates among the younger teens and that this decline was experienced mainly by white and African-American teens, although they acknowledge that what they report could probably be more accurately described as a cumulative effect of a number of welfare reforms.

Finally, Hao, Astone, and Cherlin (2007) argue that welfare reform and a teenager's decision to have a child are part of a much larger set of decisions (such as school enrollment) and are influenced by multiple policies (they focus on CSE). In particular, a young woman's decision to enroll in school and her decision to bear a child may be determined simultaneously. They attempt to model these relationships using an event history model. They find that welfare reform has a small and marginally significant (10 percent level) depressing effect on motherhood. They find that delays in motherhood increase a woman's likelihood of current school enrollment, which creates an indirect relationship between welfare reform and declines in teenage women's fertility.

The literature seems to support the conclusion that welfare reform has reduced teen pregnancy, and policies targeted toward minors have created additional decreases in their fertility. Often when studies fail to find an effect, the coefficient implies a reduction, but the standard error is too large to reject a null effect. While the decline is consistent with theoretical expectations, the reductions in teen fertility attributed to welfare reform seem to be larger than the estimated effects of AFDC on teen pregnancy. After conducting an extensive literature review, Haveman and Wolfe (1995) concluded that AFDC generosity likely caused a small increase in teen fertility. It is possible that TANF had such a sizeable effect because welfare reform was not only a reduction in benefits, it sought to change fertility behaviors and created message effects (Blank, 2002). Furthermore, welfare reform signaled a decline in the generosity of lifetime benefits (Kearney, 2004). Taken together, this may explain why welfare reform produced a larger fertility decline among teen women than would have been predicted by the AFDC literature.

Child Support Enforcement

Title IV-D of the Social Security Act created the CSE program in 1974. The program recognizes an obligation on the part of nonresident parents, typically fathers, to contribute to the financial well being of their children. The program consists of establishing paternity, creating an award agreement, and enforcing the nonresident parent's obligation to pay that award. Since its inception, the program has been modified several times, including several changes created by PRWORA in 1996. With each revision, measures were added to the CSE program to improve paternity establishment and collection rates, and CSE offices have been very successful in both dimensions (Plotnick et al., 2004; U.S. Congress, House of Representatives, Committee on Ways and Means, n.d.).

Much of the research attention on the CSE tends to focus on mothers who also receive TANF (or its predecessor AFDC), and most, if not all, of the research relevant for this review occurred before the Deficit Reduction Act (DRA) of 2005. Prior to the DRA of 2005, mothers receiving TANF were only awarded the first $50 paid in child support; the remaining payment was first used to compensate the state for TANF benefits paid to the mother. In some respect, then, a major program goal was to refill government coffers for welfare payments. Despite the reality that many custodial parents do not receive their full child support award, and what they do receive could be less than $50 per month, theoretically this could still generate fertility responses (Garfinkel et al., 2003).[18]

Predicting behavioral responses to changes in CSE policy is difficult because the incentives created for mothers and fathers are often at odds with one another. An increase in the strictness of CSE—that is, more paternity establishment, higher proportions of awards, greater amounts in the awards, and higher collection rates—raises the cost of a nonmarital birth to a potential father. As such, one should expect unmarried men to be less inclined to father children after the change in CSE (Garfinkel et al., 2003; Hanewall & Lopoo, 2008; Pirog & Ziol-Guest, 2006; Plotnick et al., 2004).

The benefits—and constraints—of the TANF program greatly complicate the estimated effect of child support for mothers. As explained earlier, for mothers who received TANF payments, child support payments from fathers are sent directly to the government to offset TANF benefits received by the mother. Child support provides little additional assistance, other than the $50 pass-through allowed in certain states, to unwed mothers who receive public assistance. Because women receiving TANF benefits do not receive all of the father's contributions, they are only $50 a month better off than they were prior to the CSE policy change. Although still a positive incentive for an unwed birth relative to a scenario without CSE, the behavioral responses should be much smaller than would be expected if the mother received the complete CSE contribution.

Practically speaking, the creation of a CSE program may actually increase the cost of a child for an unwed mother. In the absence of CSE, many fathers make contributions to their children informally. For the partners of these men (in the presence

[18] The DRA created financial incentives for states to pass through more money to custodial parents and recapture less money to offset TANF costs. However, there is an absence of research following the DRA, and so we are unable to summarize the effects of the 2005 legislative change (the studies in this review all evaluated fertility responses prior to the DRA). Even with the 2005 changes, it is still true that defraying the costs of the welfare program is a major component of CSE; therefore, this review is still informative about the fertility responses one might expect in the current policy environment. Also, the DRA only created an incentive for states to pass through more money. If states did not change their collection schemes, then the programs and behavioral responses of mothers and fathers prior to the DRA are identical to the programs and responses following the DRA.

of a CSE program), these informal contributions are now substituted by formal payments to the CSE offices to replenish public funds. If the informal contributions were greater than $50, then mothers actually experience a reduction in benefits as a result of CSE. In this case, the theory is not contradictory at all and implies CSE should reduce both parents' incentive for nonmarital fertility (Garfinkel et al., 2003; Hanewall & Lopoo, 2008; Plotnick et al., 2004). Economic theory also predicts that fathers who already pay child support will be less likely to have additional children, due to the reduction in financial resources that such fathers can contribute to future children (Garfinkel et al., 2003; Hanewall & Lopoo, 2008; Plotnick et al., 2004).

A handful of studies have investigated the relationship between CSE and fertility, and the results from these studies are quite consistent despite the different measure of CSE strictness utilized (Pirog & Ziol-Guest, 2006). Plotnick et al. (2004) use data from the NLSY97and find that CSE strictness reduces the likelihood of nonmarital teenage childbearing among non-Hispanic, white teenagers. The results were not statistically significant for non-Hispanic African-American teens. They attribute this CSE effect primarily to a state's paternity establishment rate arguing that this is information that is probably widely known among males, while collection rates for CSE are probably less well known.

With detailed natality data from the NCHS, Garfinkel et al. (2003) estimate the relationship between the nonmarital birthrate for females aged 15 to 44 and CSE. The authors measure CSE strictness as the natural logarithm of the product of the paternity establishment rate and the collection amount. Based on their models, Garfinkel et al. (2003) argue that the nonmarital birthrate in the United States would have been 2.5 to 3.9 percent higher in 1996 in the absence of a strong CSE program.

Case (1998) uses the sex composition of state legislatures as an instrumental variable to identify the relationship between CSE and nonmarital fertility. She argues that women in the legislature have stronger interests in legislation targeting women and children's issues. Using state-level natality data on women 15 to 44, she asks if genetic testing to establish paternity, long-arm statutes that allow states to pursue fathers in other states, state guidelines that allow paternity establishment as young as age 18, laws that make wage garnishment mandatory if CSE payments are in arrears, and the presence of presumptive guidelines in the state are related to nonmarital fertility. She estimates each relationship separately. The instrumental variables models show a negative and significant relationship between nonmarital fertility and genetic testing, paternity establishment at age 18, and presumptive guidelines.

Aizer and McLanahan (2006) use data from 1985 to 1999 from the NCHS detailed natality file. Unlike the previous research, they use a DD model to estimate the CSE effect comparing single women (treatment group) to married women (control group) residing within the same state. Their measure of CSE strictness is the mean annual child support expenditures over the three previous years in the state. They use state-by-year fixed effects in their models. Therefore, their identification comes from differences in birthrates observed between married and single mothers within the same state in a given year. They find that a 1 percent increase in CSE expenditures leads to a decline in single fertility relative to married fertility of 0.03 percent. The estimated elasticity increases to 0.09 percent when the analytic sample is constrained to include only low-education mothers.

Aizer and McLanahan (2006) also show that increases in CSE expenditures are associated with increases in early prenatal care, more prenatal care visits, and fewer low birth weight babies for nonblack mothers and African-American mothers (with the exception of the low birth weight outcome). They interpret this set of results to

suggest that CSE reduces fertility, but that there is positive selection: when men do become fathers, they tend to partner with women who are more likely to invest in their children.

Medicaid

Created in 1965, the Medicaid program was originally designed to provide health care for single-parent families eligible for the AFDC program and for low-income blind, elderly, and disabled individuals. We note that the Medicaid program offers a broad array of services to women and children, and thus may affect many decisions in the sequence presented in Figure 1. Here, our review focuses on the net effect of Medicaid's health care services for eligible pregnant women and children, which would include a combination of a health insurance effect and a family planning effect.[19]

Because the Medicaid program provides health insurance for children and pregnant women, it may affect fertility by reducing the cost of childbearing (Bitler & Zavodny, 2010; DeLeire, Lopoo, & Simon, 2011; Joyce, Kaestner, & Kwan, 1998). This hypothesized health insurance effect is supported by evidence from the RAND Health Insurance Experiment, where a treatment group that was offered free medical care experienced rates of fertility that were 29 percent higher than a comparison group with health insurance that required cost-sharing (Leibowitz, 1990). Pregnant women may also have become more likely to give birth rather than opt for an abortion with the health insurance coverage provided by the Medicaid program, which would have increased fertility rates (Bitler & Zavodny, 2010). At the same time, eligibility of Medicaid also provides funding for family planning services, which should reduce the cost of contraceptive services making it easier to avoid pregnancy and thereby lower the probability of birth.

Over time, an increasing number of families have become eligible for Medicaid due to federal and state expansions of the program (see Gruber, 2003 for details). The program underwent a series of eligibility expansions beginning with the Deficit Reduction Act (DEFRA) of 1984 when eligibility was extended to pregnant women who would qualify for AFDC assuming their child was already born (Bitler & Zavodny, 2010). The Omnibus Budget Reconciliation Act (OBRA) of 1986 permitted states to expand eligibility for children and pregnant women with incomes up to 100 percent of the federal poverty level (FPL). The OBRA of 1987 allowed states to expand eligibility further to 185 percent of the FPL, while the OBRA of 1989 mandated that states expand eligibility to 133 percent of the FPL for pregnant women and children aged 5 or younger (Joyce, Kaestner, & Kwan, 1998). Thus, by 1992 pregnant women and children aged 5 or younger with incomes below 133 percent of the poverty level qualified for the program, and in some states, such as California, Michigan, and Texas, the eligibility threshold was substantially higher. In total, from 1987 to 1992, both the number of children aged 18 or younger and the number of women between the ages of 15 and 44 who were eligible for Medicaid more than doubled (Cutler & Gruber, 1996). Between 1993 and 1997, the federal government approved a series of waivers that allowed many states to further expand income eligibility for their Medicaid programs. Given these expansions, over time more and more families qualified for publicly provided health insurance, which lowered the cost of giving birth and may have increased the number of children

[19] Following Roe v. Wade, Medicaid was also used to fund abortion, although the federal funding was subsequently halted with the Hyde Amendments (see the Abortion Policy section for more details). While there was a time when Medicaid could have also generated an abortion effect too, all of the data used in the research literature postdates 1976, when federal funding of Medicaid was stopped.

families could afford. At the same time, more individuals became eligible for family planning services, which might have reduced the fertility of women.[20]

The net effect of the program, which includes both the family planning component and the health insurance effect, is the subject of a small literature utilizing these expansions and birth certificate data from the NCHS. Joyce, Kaestner, and Kwan (1998) published the first paper, investigating the fertility response among unmarried white and African-American women aged 19 to 27 with 12 years or fewer of education. Their data were limited to 15 states between 1986 and 1992, and their results suggest a 5 percent increase in birthrates among eligible white women and no response among African-American women. Subsequent work by Bitler and Zavodny (2010) and DeLeire, Lopoo, and Simon (2011) also use NCHS data, but over a longer time period and include all states. Both sets of authors use a policy simulation based on the policy changes that went into effect in each state over time, a technique initially employed by Currie and Gruber (1996). Bitler and Zavodny also used a time-varying measure of the income threshold for eligibility (such as 100 percent of the FPL), allowing it to change as legislation dictated. Neither study finds a fertility response for highly educated and married women. Among white high school dropouts, however, Bitler and Zavodny estimate a 7.7 percent increase in the birthrate. DeLeire, Lopoo, and Simon also estimate a positive effect on white teens and high school dropouts, but their estimate is statistically insignificant. This research literature suggests that Medicaid has no overall fertility effect and does not seem to alter the fertility of most demographic groups. However, there is some evidence that the Medicaid program has led to an increase in births among single women with low-education levels.

Food Stamp Program/Supplemental Nutrition Assistance Program

The FSP, now called the SNAP, was initiated as a pilot program in 1961, and all counties in the United States were required to offer the program by 1975 (Almond, Hoynes, & Schazenbach, 2011). The SNAP program is federally funded and provides vouchers to qualified low-income households that can be redeemed at local retail food outlets for eligible food items. Its goal is to ensure that families with limited resources are not precluded from having a nutritionally adequate diet. In 2011, the maximum monthly value of the voucher ranged from as little as $200 for a single person household to over $1,000 for families with seven or more people (U.S. Department of Agriculture, n.d.).

Eligibility for the program does not depend on having children, although enrolling in the TANF program automatically qualifies one for SNAP. Furthermore, the value of the vouchers increases with the number of children, which might produce pronatalist effects. The FSP/SNAP might also change the composition of a birth cohort. Because the FSP/SNAP improves nutrition, it should improve fetal health leading to fewer fetal deaths (Almond, Hoynes, & Schazenbach, 2011; Currie & Moretti, 2008). Currie and her colleagues also argue that in-kind benefits from the FSP/SNAP can be thought of as an income subsidy because these benefits have the same effect on spending as would a cash transfer (Almond, Hoynes, & Schazenbach, 2011; Currie & Moretti, 2008). FSP/SNAP benefits then should produce increases in the purchase of all normal goods, including children.

The FSP became universal in 1975, which ended any across-state variation in the eligibility criteria or value of the FSP vouchers. The lack of present-day subnational

[20] A separate literature focuses on expansions of the Medicaid Family Planning Services for women who are not eligible for Medicaid. We describe that literature in the Medicaid Family Planning section.

variation makes estimating the impact of the FSP/SNAP on any outcome quite difficult. To date, only two studies provide any evidence at all about the natalist effects of the FSP/SNAP. While not the focus of their study, Almond, Hoynes, and Schazenbach (2011) used the rollout of the FSP in the 1960s and early 1970s to identify its impact on several health outcomes among newborns. Different counties, even within the same state, adopted the program at different times between the initial pilot and the federal legislation making the program universal. In one of Almond, Hoynes, and Schazenbach's robustness checks, the authors estimate the impact of FSP on birthrates finding small and statistically insignificant effects. In contrast, Currie and Moretti (2008), who use a similar identification strategy with data from California, find statistically significant fertility effects for white first births, white teens, and all births among African Americans. Once they remove data from Los Angeles, however, their estimates are no longer statistically significant for whites. Currie and Moretti argue that this result might be the result of the in-migration of young white women into Los Angeles County in response to its early adoption of the FSP.

TAX POLICY

The U.S. Internal Revenue Code allows taxpayers several allowances for children, including the personal exemption, the Child Tax credit, and the Earned Income Tax Credit (EITC) (described in more detail below). Crump, Goda, and Mumford (2011) estimate that this collective child tax subsidy was worth about $2,000 in 2005 or between 7 and 15 percent of the annual cost of a child. A handful of studies have asked if reductions in one's tax liability created by parenthood has an identifiable effect on fertility.[21]

The earliest and most well-known work in the area is Whittington,Alm, and Peters (1990). They use time series data from 1913 to 1984 to determine if the value of the personal exemption for qualifying dependents (in this case, children) in the U.S. Internal Revenue Code is related to birthrates. While the personal exemption was added to the Internal Revenue Code to insulate the minimum income necessary to live from tax liability (the exemption is based on the cost of an adequate diet), it does subsidize parents for giving birth and increases in value with a parent's marginal tax rate. Whittington, et al. (1990) argue that the value of the personal exemption represents a nontrivial cost savings; it reduces the annual cost of a first child between 4 and 9 percent, and the savings are potentially larger for higher parity births. They estimate a small elasticity of the birthrate with respect to the personal exemption (ranging between 0.127 and 0.248), but the point estimates are robust to a variety of specifications. In a similar study, Whittington (1992) used the Panel Study of Income Dynamics to answer the same research question focusing on married couples from 1979 to 1983. She finds a positive relationship between the average value of the tax exemption and observed births (elasticity estimates in most models are around 0.8), which she acknowledges might represent a timing effect.

Two recent papers by Baughman and Dickert-Conlin (2009) and Herbst (2011) provide evidence with respect to the fertility effects of the EITC. The EITC was

[21] In their interesting study on the timing of births, Dickert-Conlin and Chandra (1999) show that the tax consequences of the date a child is born can lead to the manipulation of the delivery date. Families that bear children at any point in a particular year can claim the personal exemption on their tax return. Considering the personal exemption, EITC, and standard deduction, pregnant women with due dates near the end of the year stood to gain a substantial tax benefit if they gave birth on December 31 of year *t* rather than January 1 of year *t+1*. Dickert-Conlin and Chandra (1999) ask if these parents (and their physicians) were more likely to manipulate their date of delivery to gain the tax savings. Their findings suggest that mothers with the greatest tax savings are the most likely to time their births.

first created in 1975 to provide a refundable tax credit for low-income workers with children. It was designed to reward work, and has a unique payment structure. For people who work a small number of hours, the credit increases (the percentage of the credit is constant) for each additional dollar of income earned until a maximum credit is reached. This "phase-in" structure was designed to induce low-income families to work more hours. Once the maximum credit is reached, families may continue to earn more money, but their credit remains fixed for a range of incomes; this is called the flat-region of the tax. The EITC also includes a phase-out region, where the refundable credit gradually declines, to mitigate the incentive to reduce work hours. In addition to the federal EITC, in 2010, 23 states and the District of Columbia offered a state EITC (Williams, Johnson, & Shure, 2010).

Baughman and Dickert-Conlin (2009) use data from the NCHS natality file to estimate the relationship between changes in the EITC and fertility rates. Their results suggest that the EITC has few statistically significant fertility effects. They do report, however, that the EITC is associated with very small reductions in fertility for higher parity births for white women. The authors speculate, following Becker (1991), that parents may be substituting quality for quantity as a result of the tax credit. Similarly, Herbst (2011) finds the EITC reduced abortion and this reduction does not alter birthrates. He argues that the opportunity costs of childbearing operating through the EITC likely changed families' sexual and contraceptive behaviors.

Theoretically, the EITC should induce fertility at both the intensive (mothers having more children) and extensive margins (women having their first child) because the credit is trivial for childless families (before 1993 families without children could not qualify) and is larger for families with two or more children than those with one child. While these tax credits may encourage childbearing, all else equal, the EITC has labor supply effects that complicate the theoretical predictions. The EITC creates both income and substitution effects that may alter fertility behavior (Baughman & Dickert-Conlin, 2009; Herbst, 2011). If one assumes, as did Becker (1960), that children are normal goods, the income produced by the subsidy from the EITC should unambiguously produce an increase in fertility. For women with incomes in the phase-in region of the credit, there is an opportunity cost of having additional children that makes the fertility prediction ambiguous. If a filer is in the flat region, there is no opportunity cost for the marginal work hour; therefore, the credit should produce an unambiguous fertility increase. For women in the phase-out region of the EITC, the marginal work hour actually reduces the credit; thus, women in this region should experience a positive fertility effects as well.

Taking into consideration the complete tax subsidy for children, which includes the personal exemption, the Child Tax Credit, and the EITC, Crump, Goda, and Mumford (2011) provide a follow-up to Whittington, Alm, and Peters (1990) updating three aspects of the study. First, they extend the time series from 1984 (when Whittington, Alm, and Peters stopped) to 2005. In addition, they include the Child Tax Credit and the EITC in their tax subsidy calculations, arguing that since 1980 the personal exemption has played a decreasing, but nontrivial, role in the total tax subsidy one receives for children. Second, Crump, Goda, and Mumford (2011) perform a number of econometric tests to determine if the model specification the authors chose in the original study is appropriate. Finally, the authors attempt to estimate if the fertility effects observed are timing effects, or if they alter total fertility.

Crump, Goda, and Mumford (2011) argue that Whittington, Alm, and Peters' (1990) model was probably incorrect, but even if correct, the reduction in fertility is produced by the personal exemption. Once they include the Child Tax Credit and the EITC in the tax subsidy, they do not observe a statistically significant relationship

between taxes and fertility over the long term, but there may be timing effects. This result is consistent with the other research suggesting that the personal exemption increases fertility and the EITC likely decreases it.

STATE-MANDATED COVERAGE OF INFERTILITY TREATMENTS

According to the 2002 National Survey of Family Growth, of the nearly 62 million women in their reproductive years, approximately 12 percent utilized some form of medical assistance to become pregnant or reduce the probability of miscarriage. Age, education, and income are all positively correlated to the likelihood of receiving infertility treatments (Chandra et al., 2005). The cost of these treatments ranges considerably from $200 to $3,000 for hormone therapy to as much as $15,000 for tubal surgeries (Schmidt, 2007). The mean cost of assisted reproductive technology (ART), which includes *in vitro* fertilization (IVF),[22] ranges from $7,000 to $11,000 per cycle. Most couples require multiple cycles, which drives the mean cost per infant delivered to between $38,000 and $85,000 (Henne & Bundorf, 2008). The vast majority of health insurance plans do not cover infertility treatments, which leaves many patients paying these costs out-of-pocket (Bitler & Schmidt, 2006; Henne & Bundorf, 2008; Schmidt, 2007).

In an attempt to address the undercoverage of infertility treatments, over time several states have mandated that insurers cover the treatments beginning with West Virginia in 1977.[23] Today, 15 states have passed legislation requiring private insurance companies to provide infertility treatments in some form (Resolve, n.d.). Some providers are required to include infertility treatments as part of standard health care coverage. Requirements of coverage vary by state. Some states require that health care providers simply offer coverage (a "mandate to offer"), while other states require these services be a part of all health care plans (a "mandate to cover").[24, 25] "Mandate to cover" states can be further subdivided into those that require providers to offer "comprehensive" coverage and those that provide "limited" coverage. Comprehensive coverage is far more extensive than limited coverage; it covers the cost and diagnosis of infertility treatments, including multiple treatments of expensive and sophisticated interventions such as ART. A limited coverage state may restrict the use of ART or exclude it from coverage (Henne & Bundorf, 2008). As a result of these legislative changes, many families with health insurance coverage (but that could otherwise not afford infertility treatments) may now choose to utilize infertility treatments. Furthermore, many couples that were receiving the treatments prior to the coverage may increase the number of treatments they receive or switch to more efficacious ones (Schmidt, 2007). Either change in the use of infertility treatments could lead to an increase in childbearing.

A small research literature investigating this hypothesis has recently surfaced. Schmidt (2007) employed data from the NCHS natality series from 1981 to 1999 and finds that these mandates increased the first-birth birthrate for white women,

[22] In 1998, IVF constituted 96 percent of ART cycles, while gamete intrafallopian transfer (2 percent) and zygote intrafallopian transfer (2 percent) constituted the remainder (Jain et al., 2002).

[23] By requiring all carriers to cover these treatments, then these mandates reduce the problems associated with adverse selection. At the same time, these mandates may produce overconsumption of services, that is, create a moral hazard problem. See Bundorf, Henne, and Baker (2008) for a detailed explanation of these economic issues.

[24] There is considerably variation in this requirement. Some states set low mandatory minimum coverage payments, some states define which infertility treatments are to be included, and others specify certain eligibility requirements, for example, a couple must have experienced infertility for a number of years (American Society for Reproductive Medicine, n.d.).

[25] Only two states (California and Maryland) extend infertility coverage to women on Medicaid.

aged 35 and older by around 20 percent. Her results were consistent whether the state implemented a mandate to cover or a mandate to offer. Schmidt also explained that some of the changes required the provider to cover IVF. White women over age 35 in states that mandated IVF coverage had even higher fertility responses (~22 percent).

While Schmidt limited her study to changes in first births, a number of scholars have asked if these mandates have led to an increase in higher parity deliveries. These insurance mandates have been shown to have led to increases in the use of ovulation-inducing drugs and ART (Henne & Bundorf, 2008; Jain, Harlow, and Hornstein, 2002), and both types of infertility treatments increase the likelihood of high-parity deliveries (Bundorf, Henne, & Baker, 2008; Centers for Disease Control, 2000). However, one might also expect to see a decline in higher parity deliveries. The reason many infertility treatments increase the likelihood of multiple births relates to the cost of these treatments. For instance, because IVF is so expensive, patients often attempt to minimize costs by having multiple embryos transferred during a single cycle, which often results in a high-parity delivery. If embryonic transfer treatments are covered by health insurance, as is the case in several states, then fewer embryos may be transferred within each IVF cycle, reducing the likelihood of multiple births for IVF recipients (Bundorf, Henne, & Baker, 2008; Henne & Bundorf, 2008; Jain, Harlow, and Hornstein, 2002).

Some of the clinical evidence suggests a reduction in the likelihood of multiple births in states with comprehensive coverage and no change in multiple births in states with other levels of coverage (Henne & Bundorf, 2008). In contrast, the empirical evidence using national natality data suggests that these insurance mandates, especially within comprehensive coverage states, are associated with an increased likelihood of multiple births among women who are white, married, and over the age of 30 (and in some instances as young as 25; Bitler, 2008; Buckles, 2011; Bundorf, Henne, & Baker, 2008).

This apparent inconsistency may be explained by differences in data, patient characteristics, or differences in embryo transfer practices (i.e., less embryos per cycle). While both studies use aggregated data, the clinical data report birth outcomes for those who sought and received fairly sophisticated treatments, whereas the national data contain all births—those that occurred with these sophisticated treatments, less sophisticated but perhaps effective treatments such as ovulation-inducing drugs, and births that occurred without the use of infertility treatments. Bundorf, Henne, and Baker (2008) also analyze the clinical data and find that in comprehensive coverage states, utilization among "poor prognosis" patients (i.e., those who are the least likely to experience a birth after infertility treatments) increases. Therefore, the clinical birthrate could be lower in comprehensive coverage states because the latent fecundity of those seeking clinical treatments in these states is lower on average. However, the overall fertility rate in a comprehensive state may be higher because patients have a wider variety of treatment options available to them, which in the aggregate increases higher parity births.

ABORTION POLICY

Understanding the empirical research on U.S. abortion policy requires some historical background.[26] Abortions were initially made illegal in the United States in the late 1800s in response to public health concerns, and there was little legal challenge to this decision until the early 1960s. At that time, several state courts handed

[26] The abortion history described below largely comes from Levine (2004).

down rulings that created ambiguity regarding the legality of abortion, and in 1969 the California Supreme Court declared the state's law prohibiting abortion unconstitutional. Subsequently, four states, New York, Washington, Hawaii, and Alaska, enacted legislation that legalized abortion in 1970. In 1973 the U.S. Supreme Court ruled in *Roe vs. Wade* that a Texas law prohibiting abortion, with the exception of cases in which the mother's life was in danger, violated a woman's right to privacy.

Immediately following the *Roe vs. Wade* decision, the Medicaid program (explained in greater detail earlier) covered the cost of an abortion for eligible women; however in 1976, Congress passed the Hyde Amendment to the Medicaid program, which prohibited the expenditure of federal funds for abortions. The amendment did not prohibit states from paying the full cost of the abortion, and as of the early 2000s, 17 states paid for abortions through their state Medicaid program (Levine, 2004).

Parental involvement laws and mandatory delay laws are two other important categories of abortion restrictions that have surfaced since *Roe vs. Wade*. Parental involvement laws either require minors to obtain parental (or guardian) consent or parental notification before they can obtain an abortion. Parental involvement laws were found to be constitutional in *Planned Parenthood of Central Missouri vs. Danforth* in 1976 and *Bellotti vs. Baird* in 1979 (Dennis et al., 2009), provided states have a mechanism for the court to waive the parental involvement and authorize the abortion. In 2008, 34 states had laws that required some form of parental involvement before a minor could receive an abortion (Dennis et al., 2009).

Mandatory delay laws require a specified period of time after their initial inquiry before a woman can receive an abortion. In some states these laws also require pregnant women to receive counseling as well. Mandatory delay laws are fairly new having been declared constitutional by the Supreme Court in the 1992 case *Planned Parenthood of Southeastern Pennsylvania vs. Casey* (Joyce et al., 2009). In 2009, 24 states had laws that required women to receive counseling and delay their abortion for at least 24 hours (Joyce et al., 2009).

In Becker's classic model of fertility, the abortion decision is irrelevant because fertility is deterministic and women have perfect information regarding the costs and benefits of bearing a child (Levine, 2004). Levine builds on this model incorporating the abortion decision. Following standard neoclassical microeconomic theory, he argues that pregnant women weigh the cost of giving birth to the cost of having an abortion, choosing the lower cost alternative. Because its legalization lowered the cost of abortion for many women, some proportion of the pregnant women who would have given birth, conditional on a pregnancy in a world without legal abortion, will now choose to abort their pregnancies. For pregnant women who would have chosen to have an abortion prior to legalization, their choice would remain the same. [27]

There is a fairly extensive early literature that investigated the impact of abortion on birthrates, but much of this research involves small-scale studies in specific cities and is reviewed elsewhere.[28] We commence our review of abortion policy focusing

[27] Legalization can create a scenario in which women who would have chosen an abortion before legalization choose to give birth after legalization. Kane and Staiger (1996) and Levine (2004) show that this is possible if the assumption of perfect information is relaxed. A reduction in the cost of an abortion could lead to an increase in births if a woman's contraceptive intensity declines as a result of the decrease in cost of an abortion, that is, more women use abortion as an insurance policy in the event of a pregnancy. While pregnant, however, the woman receives information that makes a birth more appealing than she realized—that her mother will provide free childcare, for instance. Thus, some proportion of women who thought they would have aborted in the event of a pregnancy, once pregnant, chose to give birth.

[28] For example, see Levine (2004), pp. 78-81.

on the more recent generation of studies, largely using natural experiments and national-level data. Levine et al. (1999) estimate the impact of abortion legalization on fertility rates using three quasi-experimental techniques. First, they compared the changes in fertility rates in the states that legalized abortion prior to *Roe vs. Wade* (the treatment group) to those states where abortion was illegal until the Supreme Court decision (their control group) using data between 1971 and 1973. They find that fertility rates in the states that legalized abortion before *Roe vs. Wade* fell by 4 percent relative to states where abortion remained illegal. Second, they compared the fertility responses in the states where abortion was legalized with *Roe vs. Wade* (their treatment group in a second set of analyses) to those states that had legalized abortion earlier (the control group) using data from 1974 to 1980. They find a similar drop in fertility rates in these states compared to the early changers. Third, they compared the abortion rates for states that legalized before 1973 to the rates in states more than 750 miles away, states that were presumably too far for people to travel for abortions and therefore provide a more accurate comparison group. They find a reduction of 11 percent in the first states that legalized abortion relative to the faraway states, suggesting that women were traveling to obtain abortions in large numbers prior to *Roe vs. Wade*. In a similar study, Klerman (1999) uses NCHS natality data and a DD model to exploit variation across states due to individual state legal changes and *Roe vs. Wade*. His findings show that abortion legalization had larger negative effects on first births than higher parity births. Abortion legalization also had larger effects on the fertility of unmarried women than married women.

A recent paper by Ananat, Gruber, and Levine (2007) asks if the fertility changes observed due to abortion legalization permanently altered the fertility of women or if the changes observed were temporary or timing effects.[29] They (Ananat, Gruber, and Levine, 2007) use data from the 1970 U.S. Census and the 1968 to 1999 NCHS natality data to create a large fertility history for many cohorts of women. Next, they show that the 1946 to 1955 cohorts were in their peak fertility interval during the 1970 to 1972 period, the time frame when abortion was legal in the repealing states and illegal in the rest of the country. They argue that if there is a completed fertility effect of abortion legalization, one should observe it for women born between 1946 and 1955 in the repeal states. They compared the completed fertility of women born between 1946 and 1955 in the repeal versus the nonrepeal states to the difference in completed fertility among women born before 1946 in the repeal states versus the nonrepeal states. They further test their hypothesis by comparing the difference in fertility for the 1946 to 1955 cohort in the repeal and nonrepeal states to the difference in completed fertility for those born after 1955. They argue that legalization of abortion should produce no differences in these two cohorts because abortion was legal throughout the county for these cohorts of women.

The authors find a reduction in completed fertility for the 1946 to 1955 cohort of 0.054 births relative to the cohort born before 1946. Furthermore, they observe no DD comparing the 1946 to 1955 cohort to women born in 1955 or later. They argue this evidence shows that abortion legalization affected more than the timing of births, it reduced completed fertility. Their results also show that abortion

[29] If the reduction in births that occurred when abortion was made legal were replaced by births later in the life cycle, then the fertility impact of abortion is potentially less meaningful. For instance, Donohue and Levitt (2001) argue that the reduction in crime observed during the 1990s was the result of the legalization of abortion, where future criminals were aborted in the early 1970s. Ananat et al. (2007) argue that if these births are replaced later in the mother's life cycle, then one would expect to observe crime rates rising back to their earlier levels over time (unless the delay in childbearing created different outcomes for children, perhaps due to the maturation of the parents).

legalization increased the number of childless women in the 1946 to 1955 cohort by 3.45 percent.

Other studies have investigated fertility responses to limits on abortion access after *Roe vs. Wade*, including federal restrictions on the Medicaid funding of abortions, parental involvement laws, and mandatory delay laws. All of these restrictions should increase the cost of an abortion. Thus, some proportion of pregnant women who would have had an abortion prior to these restrictions will now choose to give birth.

In general, the findings using Medicaid funding restrictions are mixed. Several find little impact on fertility, and if there is any impact, restricted funding probably reduces childbearing among the low-income population (Kane & Staiger, 1996; Levine, Trainor, & Zimmerman, 1996; Mathews, Ribar, & Wilhelm, 1997). Klerman (1999) finds no effect due to changes created by the Hyde Amendments between 1977 and 1981. However, between 1982 and 1992, several states reduced or halted the use of Medicaid funds for abortions entirely. Klerman uses this state-level funding variation to estimate large positive effects, particularly for higher parity births, with larger estimates for African Americans than for whites. He argues that most low-income women qualified for Medicaid as mothers, which explains the larger estimate for higher parity births.

Findings on parental consent laws and their influence on teenage births are also mixed (Bitler & Zavodny, 2001; Joyce, Henshaw, & Skatrud, 1997; Kane & Staiger, 1996; Levine, 2004).[30] Estimating a fertility response is complicated because teenagers can cross state lines to receive abortions, an option that does not exist for the Medicaid studies because Medicaid would not fund an abortion for a citizen of another state (Levine, 2004). Joyce, Kaestner, and Colman (2006) find a 4 percent increase in birthrates among teens subject to a new parental notification law in Texas relative to a group of young mothers just outside of the age range to which the law applied. Matthews, Ribar, and Wilhelm (1997) found no consistent relationship between parental involvement laws and birthrates, but the author used data on all births, not just teenagers which could have muted findings (Dennis et al., 2009; Levine, 2004). Kane and Staiger (1996) used county-level data drawn from the NCHS natality statistics to estimate the influence of parental involvement laws (as a proxy for abortion costs) along with changes in the availability of abortion providers. They find that these laws led to a decrease in births for 15- to 17-year olds. However, the authors write that their results do not provide strong evidence of an effect. Levine (2003) used the NCHS data as well as abortion data from the Guttmacher Institute and found no birth effects. The only study to date on the relationship between mandatory delay laws and birthrates, Joyce, Henshaw, and Skatrud (1997), found inconsistent results.

A recent study by Colman and Joyce (2011) also demonstrates the impact supply-side regulations on abortions can have on fertility.[31] They evaluate the effect of

[30] There is a nascent literature that attempts to separate the effects of abortion legalization and access to oral contraceptives on the fertility of minors. Because this literature relies heavily on variation created by Supreme Court decisions that relate to rescinding bans on the sale of oral contraceptives, we review this literature in section titled The Birth Control Pill and Abortion Legalization among Minors.

[31] There is considerable heterogeneity in new supply-side regulations on abortion across states, which have created new obligations for abortion providers and the facilities in which they operate, including fulfilling staffing requirements and credentials, meeting ambulatory surgical center guidelines, and that abortions be carried out in a hospital followed by a period of hospitalization (National Abortion Federation, n.d.). When the Supreme Court decided Planned Parenthood v. Casey, they affirmed the states' right to enact laws that protect the health and safety of the mother; however, the Court also determined these laws must not impose "an undue burden on the woman's decision before fetal viability." These new regulations might be expected to increase the cost of an abortion. If an abortion provider is unable to

the Woman's Right to Know (WRTK) Act, legislation that created several supply-side regulations when the act went into effect in Texas on January 1, 2004. The WRTK required three things: that women receive information on alternatives to the abortion procedure at least 24 hours prior to undergoing an abortion, that women be given the opportunity to read "A Woman's Right to Know," a pamphlet that explains fetal development, and that all abortions performed at or after 16 weeks of gestation be performed in an ambulatory surgical center (ASC).[32] In 2004, none of the nonhospital abortion providers in Texas met the requirements of an ASC, which meant the supply of late-term abortion providers was sharply reduced. Colman and Joyce compare the change in Texas's late-term abortion rate to the change in neighboring states' late-term abortion rate.[33] They find that the WRTK Act led to 55 percent fewer late-term abortions for all women and 80 percent fewer late-term abortions for teenagers (they suggest teenagers confirm or realize they are pregnant later in their pregnancy, making them particularly sensitive to this policy change). Few women were able to shift their late-term abortion to an early-term abortion. The authors attribute the policy change to an additional 6,631 births, and although this is a modest increase in fertility, they argue it is likely more pronounced among women with fewer resources and teen mothers. Their evidence also demonstrates that even by the end of 2006, late-term abortion was still only at 46 percent of the pre-WRTK level, suggesting service providers were not able to adjust to the new requirements easily.

POLICIES THAT AFFECT ACCESS TO CONTRACEPTIVES AND RELATED HEALTH SERVICES

We review the literature on the introduction of the birth control pill and family planning services in this section. One might reasonably argue that the birth control pill was not a policy change, it was a technology change. However, the fertility effects from oral contraceptives were largely influenced by a number of policy changes and Supreme Court decisions. We review these below. We also emphasize that the fertility effects in this topic have less to do with changes in the cost of a child, and more to do with changes in the cost of pregnancy and childbearing prevention. This literature is clearly founded on the premise that if one makes the cost of pregnancy and childbearing prevention less expensive, it is easier for people to manipulate their fertility.

The Birth Control Pill

Enovid, the first oral contraceptive, was introduced in the United States in 1957 as a treatment for gynecological disorders by G. D. Searle and Company (Junod & Marks, 2002). In June of 1960, the FDA approved a supplemental application for Enovid as an oral contraceptive, although the contraceptive effects of the drug were well understood by many physicians long before then (Bailey, 2010; Junod & Marks, 2002). As Bailey (2006, 2010) points out, the 1960s was a period of important social change with, among many other things, the growth of the Women's Movement,

pass this cost to the patient, then one would expect a decrease in the supply of providers. Both would depress the abortion rate and potentially increase the birth rate.

[32] As the WRTK entails three mandates, we cannot be certain that one particular mandate is the major cause in the late-term abortion decline. It is possible that all three treatments together have produced the observed change. However, Colman and Joyce observe no change in early-term abortions following the WRTK Act, suggesting the first two provisions are not driving the fertility response.

[33] They also use a comparison group comprised of 32 states that do not neighbor Texas and have comparable data. The results are robust to this additional analysis.

the passage of important Civil Rights legislation, and the Vietnam War. Given the large number of social changes that were occurring at the time, as well as the legalization of abortion, researchers have found it difficult to separate the effect of the birth control pill on fertility from the influence of these other changes. Much of the literature on oral contraceptives (and the family planning changes that were occurring at this time) attempts to separate policy effects from these other changes.

Investigating the fertility of married women in the 1960s, Bailey (2010) documents that a large number of states banned the sale of any article designed to prevent conception, using a group of antiobscenity laws more commonly referred to as the "Comstock laws." The Comstock laws had different definitions of obscenity across states, creating variation in access to contraceptives. In 1965, the Supreme Court struck down the restriction on oral contraceptive sales to married couples in Connecticut with the *Griswold vs. Connecticut* decision. Following the Griswold decision, nearly every state repealed their ban of the sale of contraceptives by 1971 (Bailey, 2010). Bailey used the timing of Griswold, the repeal of the Comstock laws, as well as the introduction of Enovid to identify the effect of the birth control pill. Her results suggest that had some states not prohibited the sale of oral contraceptives, marital fertility rates would have been 8 percent lower in states restricting access to contraception and 4 percent lower overall in the United States. Bailey (2010) estimates that approximately 40 percent of the decline in the marital fertility in the United States between 1955 and 1965 can be attributed to the introduction of the birth control pill.

The Birth Control Pill and Abortion Legalization Among Minors

We reviewed the literature on abortion earlier in this retrospective, including studies that asked about abortion among minors. However, none of that literature included controls for access to the birth control pill. A burgeoning literature that considers the two factors simultaneously attempts to explain the role both factors played on changes in fertility among minors during the 1970s.

Although the FDA approved the oral contraceptive for older, married women in 1960, access to unmarried minors was restricted during the 1960s and early 1970s. Receipt of oral contraceptives required a prescription from a licensed physician and could only be purchased from a licensed pharmacist (Bailey, 2006). Furthermore, a young woman had to reach the age of majority (usually age 20); be married, pregnant, or already a mother; or acquire parental consent to obtain a prescription (Ananat & Hungerman, in press; Bailey, 2006). Over time, minors gained greater access to oral contraceptives for a couple of reasons. First, some states reduced the age of majority, thereby lowering the age requirement for parental consent. States also gradually began to allow minors legal access to medical treatment, including oral contraceptives, if the minor understood the nature and ramifications of the medical care (Ananat & Hungerman, in press; Bailey, 2006; Guldi, 2008). The Griswold decision altered access to oral contraceptives for married women, but it was not until the 1976 decision in *Planned Parenthood of Central Missouri vs. Danforth*, which declared that age could not be used as the sole basis for determining if women had access to contraceptives, effectively providing minors access to oral contraceptives.

Several researchers have used this variation in access to oral contraceptives among minors to estimate its impact on the fertility of young women. The evidence from this literature consistently shows that access to oral contraceptives reduced the fertility of minors during the mid-1960s through the 1970s (Ananat & Hungerman, in press; Bailey, 2006; Guldi, 2008). These changes, however, were likely timing effects as access to oral contraceptives did not change the completed fertility of women within this cohort (Ananat & Hungerman, in press; Bailey, 2006). Furthermore, it appears

that the birth control pill was more frequently used among higher socioeconomic status mothers (Ananat & Hungerman, in press); thus, the mothers who delayed giving birth by using the pill tended to consider educational opportunities and pursue marriage during this period.

All three papers recognize the importance of accounting for abortion access in their models. Bailey (2010) uses the timing of the legalization of abortion among adults in her models, while Guldi (2008) and Ananat and Hungerman (in press) code abortion as a viable alternative for minors only if it was legal in the state and there were no parental consent requirements for minors. Both Guldi (2008) and Ananat and Hungerman (in press) find that abortion reduced fertility, and Guldi claims that abortion may have had larger effects than the oral contraceptive at least among white minors.

Joyce, Tan, and Zhang (2011) suggest that abortion may have had a larger role in the fertility reduction of minors than estimated in this earlier work. They employ data on induced abortion in the state of New York from 1971 and 1972, when abortion was legal there. These data include information on the state of residence for the mother who obtained the abortion. Joyce, Tan, and Zhang (2011) convincingly demonstrate that although abortion was illegal in many states, it was quite common for women to travel across state lines to obtain abortions. For instance, they show that in 1971 the abortion rate in Michigan, a state in which abortion was illegal, was 10.9 per 1,000 teens. Joyce et al. point out that the previous work may be erroneously attributing some of the declines in fertility to the removal of bans on the sale of contraceptives to minors. Even though abortion was technically illegal in these states, the legalization of abortion in other states (especially those in close proximity) was likely responsible for some portion of the decline. Joyce et al. also question the counterfactuals used in Guldi and Ananat and Hungerman, claiming that had they made different choices, their results for the birth control pill would have been smaller, while the abortion effects would not have changed.

Publicly Funded Family Planning

Beginning with the 1964 Economic Opportunity Act, the federal government created a number of mechanisms that provide grant support for family planning services to help low-income women gain greater control over their fertility (Bailey, in press). Today, the Title X Family Planning program and the family planning services delivered through the Medicaid program serve as the cornerstones for publicly provided family planning in the United States, although states may use federal funds from the Social Services Block Grant, the Maternal and Child Health Services Block Grant, the State Children's Health Insurance Program, and the TANF program as well as their own resources to augment the funding from these two primary programs (Gold, 2001; Gold et al., 2009).[34] The evidence on the efficacy of these family planning programs is reasonably consistent, often showing large reductions in the fertility among the low-income population.

Title X Family Planning

The Title X Family Planning Program was enacted in 1970 as part of the Public Health Service Act. The goal of Title X is to provide, through a grant application

[34] While these other funding sources serve as controls in some of the studies on Title X and Medicaid, there is no research on their fertility effects. We, therefore, concentrate on Title X and Medicaid in this review.

process, access to contraception, counseling services, preventive care, and health screenings throughout the country, with a particular emphasis on the low-income population (Gold et al., 2009; U.S. Department of Health and Human Services, n.d.). These services are offered through a network of community health care centers, Planned Parenthood affiliates, and state and local health departments. In calendar year 2008, this network of 4,500 community-based clinics served as the primary provider of contraceptive services for over five million men and women (Department of Health and Human Services, n.d.).

The empirical research on the fertility impacts of Title X nationally is limited to a handful of studies.[35] Researchers from the Guttmacher Institute have generated a couple of simulations to address this topic. Frost, Finer, and Tapales (2008) suggest that 1.4 million pregnancies were averted in 2004 due to publicly financed family planning services (including those funded by Medicaid). Of these averted pregnancies, 641,000 would have produced births. Gold (2001) estimates that between 1980 and 1999, 20 million pregnancies have been averted due to federally funded family planning clinics, of which over seven million would have resulted in births. These simulations have several limitations. First, they assume that researchers can use the contraceptive choices of women who do not use publicly funded family planning services as the counterfactual for women who do use publicly funded family planning services. Second, the simulations are dependent on accurate estimates of contraceptive failure, adjustments for that failure, and the distribution of outcomes (either birth, abortion, or miscarriage) conditional on a unintended pregnancy.

Bailey (in press) uses county-level variation in the funding of family planning programs through the 1964 Economic Opportunity Act through the early years of Title X, that is, between 1964 and 1973. Her results suggest that family planning reduced the number of births in the immediate aftermath of the funding and that these births were not simply delayed; there was a reduction in completed fertility of women who were in their childbearing years during this period. These reductions occurred principally among the low-income population where childbearing was reduced between 19 to 30 percent within the first 10 years these grants existed.

In 1972, the Medicaid program was amended and expanded requiring states to provide contraceptives, along with the appropriate obstetric examinations and testing for qualified women of childbearing age (Gold et al., 2009; Kearney & Levine, 2009). Although Title X primarily provides funding for contraceptive services at health care clinics, the family planning component of the Medicaid program operates as an insurance program reimbursing health care providers for patient care. Furthermore, states are not allowed to require cost sharing for these family planning services, and Medicaid beneficiaries can receive family planning assistance from any health care provider even when that provider is outside of their managed care plan (Guttmacher Institute & Kaiser Family Foundation, 2007; Kaiser Family Foundation, 2002; Kearney & Levine, 2009; Lindrooth & McCullough, 2007).

As explained earlier, over time, an increasing number of families have become eligible to receive health insurance coverage for some or all of their members through state and federal expansions of the Medicaid program (see Gruber, 2003 for a complete legislative history). By 1992, pregnant women and children aged 5 or younger with incomes below 133 percent of the FPL qualified for the program, and in some states, such as California, Michigan, and Texas, the eligibility threshold was substantially higher. In total, from 1987 to 1992, both the number of children aged 18

[35] There are literally hundreds of studies of Title X funding that pertain to the population served, services provided, best practices, and monitoring of outcomes, as well as a variety of other outcomes. See Sonenstein, Punja, and Scarcella (2004) for a review.

or younger and the number of women between the ages of 15 and 44 who were eligible for Medicaid more than doubled (Cutler & Gruber, 1996). Between 1993 and 1997, the federal government approved a series of waivers that allowed many states to further expand income eligibility for their Medicaid programs. Theoretically, as more families became eligible for the Medicaid family planning program, controlling their fertility also became less expensive.

In addition to the growth in eligibility for Medicaid through federal legislation, a number of states obtained waivers that would allow them to expand coverage of the family planning services component of Medicaid to women who otherwise would not qualify for Medicaid. The family planning expansions came in two forms: waivers that increased the maximum income that qualified for family planning (sometimes called income-based waivers), and waivers that provided family planning services for people who were leaving the Medicaid program (sometimes called postpartum waivers; Guttmacher Institute, 2011).

Using Medicaid data from the state of Maryland from 1988 to 1993 that included reimbursement claims for contraceptives, Mellor (1998) shows a 7.2 percent decline in the likelihood of a birth among women enrolled in the AFDC program who received contraceptives from a Medicaid provider. Results from this study are suggestive, but Kearney and Levine (2009) question the identifying variation used as the availability of family planning services may be related to the demand for them.

Several other researchers have used the family planning expansions to identify fertility effects. Lindrooth and McCullough (2007) investigated the fertility response created by the income and postpartum waivers granted to 11 states between 1993 and 2000 to extend family planning services financed through the Medicaid program. For instance, Arkansas, California, and Washington extended the benefit to 200 percent of the FPL (Kearney & Levine, 2009). The postpartum waivers allowed coverage beyond the 60-day limit imposed in the Medicaid program. Several states extended it for two years, and Maryland extended it five years (Kearney & Levine, 2009). Using a DD model with NCHS-detailed natality data, Lindrooth and McCullough find that the income-based waivers reduced fertility by about two percentage points.

Kearney and Levine (2009) extended earlier work by Lindroth and McCullough (2007) and use a DD model to estimate the fertility response created by expansions of the Medicaid program in 25 states between 1993 and 2007 to provide family planning services to women who would have lost their eligibility postpartum or because their incomes were too high. Their evidence suggests that these expansions in eligibility doubled, or perhaps even tripled, the number of women receiving these services. Using NCHS data, they estimate that these expansions reduced births to teens by 4 percent and to nonteens by 2 percent primarily through increased contraceptive use among sexually active women.

While we have discussed several aspects of the Medicaid program, the end of the federal funding of abortions with the Hyde Amendments (see section on Abortion Policy), the health insurance effect of the program (see section on Medicaid), and the family planning services component (see section on Medicaid and section Medicaid family planning), it might be worthwhile to synthesize the findings on Medicaid in one place. Of course, it is difficult to draw definitive conclusions on the overall fertility effect of Medicaid because the studies in this area cover different time periods and use several different sources of variation. Nevertheless, the reviewed research does suggest that expansions of the Medicaid program created eligibility for greater numbers of people who qualify for both publicly provided health insurance as well as family planning services. The handful of papers that estimate the net effect of the Medicaid program indicate that expansions of the Medicaid program had no statistically discernible effect on the overall fertility of American women and most demographic subgroups. However, the health insurance effect may dominate the

family planning effect for low socioeconomic women leading to small pronatalist effects for them. One recent paper (Almond, Decker, & Simon, 2010) uses variation in the timing of the introduction of the Medicaid program between 1966 and 1972, that is, prior to the amendment adding the family planning to Medicaid (but after the introduction of Title X) and supports this conclusion. Almond, Decker, and Simon's (2010) preliminary results show that Medicaid led to a 4 percent increase in the number of nonwhite births and a smaller (and insignificant) increase in white births. The Medicaid Family Planning Services literature that uses expansions of the family planning program to include women who would not otherwise qualify for the Medicaid program suggests that family planning services reduced fertility among this group of women.

CONCLUSION AND DISCUSSION

Summarizing: What Have We Learned?

Unlike most developed countries that have created strategies to increase fertility to support their ageing population, the United States spends considerably less time and thought on this issue. Our reading of the literature suggests that we have many public policies that have affected and continue to influence the fertility choices made by families in the United States.

Theoretically, any policy that alters the cost (or benefit) of becoming a parent may change fertility patterns. This review shows that there are a number of policies that appear to have had some influence on the cost of a child even when that was not the stated objective of the policy. Although there is little indication that the TANF program has natalist effects in general, there is some consistent evidence that the fertility of teenagers declined, relative to the fertility of teenagers during the AFDC, as a result of TANF. Because the CSE program increases the cost of nonmarital childbearing for fathers and mothers (certainly those on TANF), the tightening of paternity establishment and support collections over time appears to have reduced fertility among the low-income population as well. Although Medicaid Family Planning Services reduced the fertility of some near-poor women, when one considers the Medicaid program in total, the net effect of the program for all women seems trivial. Several researchers, however, show that there may be some pronatalist effects of the Medicaid program for young women with low levels of education. We also find little evidence of a natalist effect of the FSP (now called SNAP). The evidence for changes in tax policy is mixed. The exemption granted to parents for their dependents may increase fertility, but the labor supply benefits of the EITC seem to reduce fertility. When one considers the tax subsidy provided for children, which includes both components as well as the Child Tax Credit, there is little evidence of a natalist effect. In contrast, the literature on mandated expansions of health insurance coverage to include infertility treatments seems to have increased the fertility of women older than 35 by around 20 percent.

Other programs were designed explicitly to alter the fertility of women. The legalization of abortion in the early 1970s reduced the overall fertility of women during that period. Furthermore, Ananat, Gruber, and Levine (2007) demonstrate that births foregone through abortion are not replaced later in the life cycle. Recent work on legislation that restricts the supply of abortion providers, however, suggests that these restrictions have small pronatalist effects, at least in the short term. The research literature also clearly demonstrates that policies designed to give women greater control over their fertility, such as the introduction of the birth control pill and the funding of family planning services through Title X and Medicaid, are antinatalist. In fact, the research in these areas suggests that these programs have had fairly substantial impacts on overall fertility patterns in this country.

Limitations of the Literature

Timing Versus Total Demand for Children

Although researchers' understanding of the decision process involved in childbearing as well as the empirical methods used to investigate this research topic have evolved considerably over the past three decades, much work remains to be done. With the exception of some of the abortion and oral contraceptives literature, nearly all of the natalist policy literature fails to determine if the fertility effects observed are changes in the completed fertility of women or merely represent an adjustment in the timing of fertility. Distinguishing between the two is important from a policy perspective. If a policy lowers (or increases) the total number of children a family has, this has implications for the size of the population, which can affect a number of important factors, such as school systems, the labor force, and the financing of the Social Security program, just to name a few. If a program changes the age when parents have a child, this is important too, but for different reasons. It might be the case that the total number of children a mother has stays the same, but she delays motherhood from her teenage years to an older age when she is more mature and has more human capital. This timing effect could affect the environment within which the child is reared. Again, in many instances, particularly in the implicit natalist literature, one cannot discern whether the fertility effect has changed the timing of childbearing, the total number of children a woman will have, or both.

DD Models

The research papers reviewed in this retrospective ask if and by how much a given policy affected a woman's fertility. Establishing that a certain policy caused, rather than was correlated with, an observed fertility response is difficult for multiple reasons. A fundamental challenge is that we cannot observe the same individual in a world with and without the policy, that is, the counterfactual. Therefore, even the best research designs, such as a randomized controlled experiment, only provide the average treatment effect between a treatment group and a control group and cannot estimate the effect of the policy or intervention on any given person. Another impediment to casual inference occurs when policies are implemented simultaneously (a challenge faced by researchers investigating the fertility effects of the oral contraceptive separate from abortion legalization) or when a policy reform occurs alongside other environmental changes (e.g., the economic recovery in the mid-1990s was concurrent with welfare reform, making it difficult to separately identify economic from policy effects). Given these and other challenges, policymakers should be most confident when researchers consistently reach similar conclusions using diverse data sources and drawing upon multiple statistical tools.

The bulk of the research in this review relies on a DD estimator. This quasi-experimental design can be very persuasive as it can closely approximate the design of an experiment. The most important requirement is that the researcher is able to identify a plausible group to which he or she can compare the treated group. An acceptable comparison group need not be identical to the treatment group in every way, but the model assumes the prepolicy trend in the outcome would have continued unchanged in the absence of the policy change.[36] Researchers must be able to identify and exploit subnational variation in order to identify the difference in the outcome for the treatment and comparison groups before and after the policy

[36] A researcher may condition on covariates in order to meet this assumption.

intervention. This variation may exist because some policies exist in some states, but not in others (e.g., state mandates for infertility treatments) or two similarly situated groups exist within a given state or nationally (e.g., welfare studies that compare 17- and 18-year-old women).

Additionally, there should be no omitted variable that is correlated with the adoption of the policy and the fertility response (time variation generated from varying policy effective dates helps to strengthen this claim). Researchers will often use the population of interest in the state with the policy as the treatment group, while the same population residing in states without the policy acts as the control group. Researchers will need to establish that states with the policy are not different on important unobserved characteristics or that the policy was not adopted in response to changes in the fertility rate. Failure to meet any of these criteria could introduce bias into the estimated policy effects, thereby raising questions about the internal validity of the study.

To address the concern of state-level unobserved factors, researchers may employ a DDD design—a technique commonly used in the welfare literature. In addition to the across-state variation just described, another suitable comparison group within the state must exist; importantly, these two groups share all state-level variables, eliminating many sources of omitted variables bias. A researcher might decide to use married women with children as a comparison group for single women without children; importantly, all married and single women within the same state experience the same state-level variables. In this example, the DDD estimate is the change in fertility among married women less than the change in fertility among single women in a state with a welfare policy change less than the same change in fertility among married and single women from a state without a welfare policy change. However, if the married and single women in a given state vary on unobserved characteristics, this may introduce bias into the estimator. Often controls for state-level time-varying characteristics are not available by subgroup (i.e., married women's unemployment versus single women's unemployment). Without additional information about the two groups, there is no way of knowing if the bias from a group-state level omitted variable is better or worse than the bias from an omitted state-level variable. Stated differently, researchers have no way of knowing if a DD model is better than a DDD model. They will want to select the model that uses the best counterfactual.

Differencing models merely report differences in average outcomes (e.g., fertility rates) between the treatment and control groups before and after the policy took effect (or became known across the population). If the fertility rate was already changing in the years prior to the policy, the DD estimate will not identify this subtlety without explicit modeling. Such a pattern would indicate that the policy's effect on fertility may not be causal (unless there were announcement effects). The DD estimator also cannot reveal if the policy effect grows or fades over time. Researchers may test for leads and lags by including a set of dummy variables rather than one DD estimator (see Angrist & Pischke, 2009 for further details). While including this evidence would make an analysis particularly persuasive, it is not often included in the papers we reviewed.

Identifying Variation

Identification of effects within this literature requires exogenous variation in the policy of interest. Although the researchers who have contributed to this literature have used a variety of natural experiments to estimate the policy effect, their findings should always be considered in the context of that natural experiment. Consider the TANF literature. Optimally, one would want to study the program in isolation, that

is, in comparison to a world without a welfare program. The estimates provided in the TANF literature, however, often capitalize on the variation created by the shift from the AFDC program to the TANF program. In other words, although the literature suggests that the TANF program is reducing the fertility of teenagers, this is in comparison to the fertility of teenagers during the AFDC program. This result does not compare the fertility of teenagers as a result of the TANF program to a counterfactual state where there is no welfare program. If one made that comparison, one might conclude that the TANF program is pronatalist. Thus, we caution consumers of this research to consider the context (i.e., identifying variation) used in these studies when interpreting the results.

There are several papers that use variation created by the introduction of the policy, for example, the literature on the legalization of abortion (Klerman, 1999; Levine et al., 1999), the introduction of Enovid (Bailey, 2010), and the introduction of the FSP (Almond, Hoynes, & Schanzenbach, 2011; Currie & Moretti, 2008). This type of analysis has the benefit of comparing the introduction of a program to a state of nature where the program does not exist, which should produce a cleaner estimate of the effect of the program. However, this research strategy comes at a cost; these programs were all introduced during the 1960s and 1970s, a period with an entirely different social and political climate. One might ask how the availability of abortion or the presence of oral contraceptives affect women today and the results using data from four or five decades ago may be less relevant.

Data

The data used in the papers contained in our review primarily come from three types of sources: administrative vital statistics data (such as the NCHS natality series), cross-sectional survey data (such as the CPS[37]), or survey data that follows a given individual over time (such as the NLSY97). When using cross-sectional data or aggregated panel data,[38] one must ask if compositional effects could occur and if so, could the compositional effects explain the findings. Consider the marital fertility rate. Over time, it may be that the characteristics of married women are changing (perhaps they are older and have higher educational attainment on average), and this demographic shift is happening irrespective of the policy. If one observes lower marital fertility after a given policy, this may be best explained as a compositional change and not a policy effect. The increase in individual-level data is useful, even when it is aggregated, because it allows researchers to test for these compositional changes and to use a richer set of variables when grouping women.

Gaps in the Literature

Becker's theory of fertility is applicable in a wide variety of other policy areas that, as yet, have not been investigated. Child care subsidies, the Supplemental Security Income program, the Family and Medical Leave Act, and the Women, Infants and Children program all theoretically reduce the costs of a child and could produce pronatalist effects. To date, the fertility effects of these programs have not been investigated.

Education subsidies are an even larger and potentially impactful area of research that has been ignored. Several states have created programs designed to fully

[37] Although the Outgoing Rotation Files of the CPS can be used to construct a panel data set, most of the papers contained in this review use a particular module, making the data cross-sectional.
[38] Researchers often aggregate the total birth counts and construct a state-level birthrate. Thus the unit of analysis becomes the state's birthrate in a given time period.

subsidize a four-year postsecondary education, such as the HOPE scholarship in Georgia and the TOPS program in Louisiana. These programs represent enormous subsidies to the cost of a child, and have potentially large impacts on fertility.

National immigration policy has the potential to affect fertility rates in the United States. The 1965 Amendments to the Immigration and Nationality Act shifted the bulk of visas away from Western European countries and distributed the available visas more equitably among all countries. If such a policy led to increased (decreased) immigration from source countries with higher (lower) fertility rates, then the United States might expect to experience an increase (decrease) in its overall fertility rates. The Immigration Reform and Control Act (IRCA) of 1986 first legalized many immigrants, and once legalized, their spouses and children were often granted entry to the United States. IRCA (after granting legal status), the 1990 Immigration Act, and the 1996 Illegal Immigration Reform and Immigrant Responsibility Act all sought to reduce illegal immigration by tightening America's borders. However, Douglas Massey points out that this has had the unintended consequence of reducing out-migration while leaving in-migration unchanged. That is, once immigrants successfully cross the border, they do not return to their country of origin because border control has increased the expense and risks associated with entering the United States (When less is more, 2007). Massey notes that "we've transformed what was before 1986 a circular flow of workers into an increasingly settled population of families" (quoted in Lochhead, 2006, p. A-1). There is an enormous literature that investigates the fertility patterns of recent immigrants showing that it converges to the pattern of native-born women (see, e.g., Ford, 1990; Kent & Mather, 2002). None of this literature, however, shows how changes in immigration policy affect fertility rates directly.

Finally, other than the welfare literature, which could certainly use additional work for clarification, all of the areas we review are understudied. Often the entire literature for a topic consists of a paper or two, many are based on data that are 10 to 20 years old now, and do not reflect the current policy environment, as is the case with the CSE literature. Obviously, considerably more research on natalist policies is needed and could prove incredibly beneficial as the research community, policymakers, and practitioners consider the impact of policy on the population.

Concluding Remarks

The size and age distribution of our population affects a wide variety of factors: the funding of our Social Security program, the financial viability of our health care system, labor markets, and even the strength of our military, among many other things. We have shown in this retrospective that we have a large number of policies that affect the fertility of American women. In some instances, this is intentional. In many others, it is not. We have also demonstrated that many of these policies do not affect the population uniformly across the income distribution. Some of the natalist policies reviewed, such as the legalization of abortion, potentially affect women of all childbearing ages, regions of the country, and races and ethnicities. Other natalist policies tend to target populations, and the low-income population is frequently the focal point of the policy or program. For instance, Title X and the Medicaid Family Planning Services were designed explicitly to allow low-income women to gain the same control over their fertility that more affluent women have. When creating the Title X program in the early 1970s, policymakers were well aware that, although allowing women greater control over their fertility can be empowering, it can also be used as a means of social control, reducing the fertility of low-income women and women of color (Gold et al., 2009). Given this concern, policymakers designed the program to be completely voluntary, to offer a wide range of contraceptive services,

and to disallow as a condition of receipt of social welfare benefits, the use of any or a particular contraceptive service (Gold et al., 2009). Several implicit natalist policies also appear to have fertility effects on the low-income population. In some instances, these effects are pronatalist (Medicaid health insurance program) and in other instances, they are antinatalist (CSE). Although policymakers are attempting to provide programs to help the low-income population, they should be aware that they are also changing the decision calculus with respect to fertility within the low-income population. The spirit of the same caution voiced in the legislation for Title X seems appropriate with respect to implicit natalist policies. Policymakers should balance the efficacy of the policy with the unintended consequences, which may include natalist effects.

We would venture to guess that most policymakers do not consider the fertility effects of most public policies for any subgroup within the United States, much less the complex set of incentives the bundle of applicable policies creates. A low-income woman may be affected by TANF, CSE, abortion availability in her region, the EITC program, SNAP, Medicaid, Title X, and a host of other programs. Likewise, an affluent woman may be influenced by the personal exemption in the Internal Revenue Code, child care tax credit, mandated coverage of infertility treatments, and abortion policy simultaneously. Although we are unable to comment on the net effect of these natalist policies, given the enormity of the consequences of the size and composition of our population, a more systematic consideration of population policy strikes us as an important avenue for both researchers and the policymaking community to begin to pursue.

LEONARD M. LOPOO is Associate Professor of Public Administration and International Affairs, Maxwell School, Center for Policy Research, 426 Eggers Hall, Syracuse University, Syracuse, NY 13244.

KERRI M. RAISSIAN is Doctoral Candidate, Department of Public Administration and International Affairs, Maxwell School, Center for Policy Research, 426 Eggers Hall, Syracuse University, Syracuse, NY 13244.

ACKNOWLEDGMENTS

This project was supported by Award Number R03HD060783 from the Eunice Kennedy Shriver National Institute of Child Health & Human Development. The content is solely the responsibility of the authors and does not necessarily represent the official views of the Eunice Kennedy Shriver National Institute of Child Health & Human Development or the National Institutes of Health. We are grateful to Doug Besharov and five anonymous reviewers for their helpful comments on an earlier version of this policy retrospective.

REFERENCES

Acs, G., & Koball, H. (2003). TANF and the status of teen mothers under 18. New federalism: Issues and options for states. Series A, No. A-62. Washington, DC: The Urban Institute.

Aizer, A., & McLanahan, S. (2006). The impact of Child Support Enforcement on fertility, parental investments, and child well being. Journal of Human Resources, 41, 28-45.

Almond, D., Decker, S. L., & Simon, K. I. (2010, June). The impact of Medicaid's introduction on the use of health care services, health outcomes, and birth rates. Paper presented at the 3rd Biennial Conference of the American Society of Health Economists. Ithaca, NY.

Almond, D., Hoynes, H. W., & Schanzenbach, D. W. (2011). Inside the War on Poverty: The impact of food stamps on birth outcomes. Review of Economics and Statistics, 93, 387-403.

American Society of Reproductive Medicine. (n.d.). State infertility laws. Retrieved June 2, 2011, from http://www.asrm.org/insurance.aspx.

Ananat, E. O., Gruber, J., & Levine, P. (2007). Abortion legalization and life-cycle fertility. Journal of Human Resources, 42, 375-397.

Ananat, E. O., & Hungerman, D. M. (2012). The power of the pill for the next generation: Oral contraception's effects on fertility, abortion, and maternal & child characteristics. Review of Economics and Statistics, 94, 37-51.

Angrist, J. D., & Pischke, J.S. (2009). Mostly harmless econometrics: An empiricist's companion. Princeton, NJ: Princeton University Press.

Bailey, M. J. (2006). More power to the pill: The impact of contraceptive freedom on women's life cycle labor supply. Quarterly Journal of Economics, 121, 289-320.

Bailey, M. J. (2010). "Momma's got the pill": How Anthony Comstock and Griswold v. Connecticut shaped US childbearing. American Economic Review, 100, 98-129.

Bailey, M. J. (2012). Reexamining the impact of family planning programs on U.S. fertility: Evidence from the War on Poverty and the early years of Title X. American Economic Journal—Applied Economics, 4, 62–97.

Baughman, R., & Dickert-Conlin, S. (2009). The Earned Income Tax Credit and fertility. Journal of Population Economics, 22, 537-563.

Becker, G. S. (1960). An economic analysis of fertility. In National Bureau of Economic Research (Ed.), demographic and economic change in developed countries (pp. 225-256). Princeton, NJ: Princeton University Press.

Becker, G. S. (1965). A theory of the allocation of time. Economic Journal, 75, 493-517.

Becker, G. S. (1991). A treatise on the family. Cambridge, MA: Harvard University Press.

Bitler, M. P. (2008). Effects of increased access to infertility treatment on infant and child health: Evidence from health insurance mandates. Unpublished manuscript.

Bitler, M., & Schmidt, L. (2006). Health disparities and infertility: Impacts of state-level insurance mandates. Fertility and Sterility, 85, 858-865.

Bitler, M. P., & Zavodny, M. (2001). The effect of abortion restrictions on the timing of abortions. Journal of Health Economics, 20, 1011-1032.

Bitler, M. P., & Zavodny, M. (2010). The effect of Medicaid eligibility expansions on fertility. Social Science & Medicine, 71, 918–924.

Blank, R. M. (1997). It takes a nation: A new agenda for fighting poverty. Princeton, NJ: Princeton University Press.

Blank, R. M. (2002). Evaluating welfare reform in the United States. Journal of Economic Literature, 40, 1105-1166.

Buckles, K. S. (2011). Infertility insurance mandates and multiple births. Unpublished manuscript.

Bundorf, M. K., M. Henne, & L. Baker. (2008). Mandated health insurance benefits and the utilization and outcomes of infertility treatments. NBER Working Paper No. 12820. Cambridge, MA: National Bureau of Economic Research.

Camasso, M. (2004). Isolating the family cap effect on fertility behavior: Evidence from New Jersey's family development program experiment. Contemporary Economic Policy, 22, 453-467.

Camasso, M. J., Harvey, C., Jagannathan, R., & Killingsworth, M. (1998). A final report on the impact of New Jersey's family development program: Results from a pre-post analysis of AFDV case heads from 1990-1996. New Brunswick, NJ: Rutgers School of Social Work and Center for Urban Policy Research.

Case, A. (1998). The effects of stronger Child Support Enforcement on nonmarital fertility. In I. Garfinkel, S. S. McLanahan, D. R. Meyer, & J. A. Seltzer (Eds.), Fathers under fire: The revolution in Child Support Enforcement (pp. 191-215). New York: Russell Sage Foundation.

Centers for Disease Control. (2000). Contribution of assisted reproductive technology and ovulation-inducing drugs to triplet and higher-order multiple births—United States, 1980-1997. Morbidity and Mortality Weekly Report, 49, 535-538.

Chandra, A., Martinez, G. M., Mosher, W. D., Abma, J. C., & Jones, J. (2005). Fertility, family planning, and reproductive health of U.S. women: Data from the 2002 National Survey of Family Growth. Vital and Health Statistics, Series 23, No. 25. Atlanta, GA: National Center for Health Statistics.

Colman, S., & Joyce, T. (2011). Regulating abortion: Impact on patients providers in Texas. Journal of Policy Analysis and Management, 30, 775-797.

Crump, R., Goda, G. S., & Mumford, K. (2011). Fertility and the personal exemption: Comment. American Economic Review, 101, 1616-1628.

Currie, J., & Gruber, J. (1996). Saving babies: The efficacy and cost of recent changes in the Medicaid eligibility of pregnant women. Journal of Political Economy, 104, 1263-1296.

Currie, J., & Moretti, E. (2008). Did the introduction of food stamps affect birth outcomes in California. In R.F. Shoeni, J. House, G. Kaplan, & H. Pollack. (Eds.), Making Americans healthier: Social and economic policy as health policy (pp. 122-143). New York: Russell Sage.

Cutler, D. M., & Gruber, J. (1996). Does public insurance crowd out private insurance? Quarterly Journal of Economics, 111, 391-430.

DeLeire, T., Lopoo, L. M., & Simon, K. I. (2011). Medicaid expansions and fertility in the United States. Demography, 48, 725-747.

Dennis, A., Henshaw, S. K., Joyce, T. J., Finer, L. B., & Blanchard, K. (2009). The impact of laws requiring parental involvement for abortion: A literature review. New York: Guttmacher Institute.

Dickert-Conlin, S., & Chandra, A. (1999). Taxes and the timing of births. Journal of Political Economy, 107, 161-177.

Donohue, J., III, & Levitt, S. D. (2001). The impact of legalized abortion on crime. Quarterly Journal of Economics, 116, 379-420.

Dyer, W. T., & Farlie, R. W. (2004). Do family caps reduce out-of-wedlock births? Evidence from Arkansas, Georgia, Indiana, New Jersey, and Virginia. Population Research and Policy Review, 23, 441-473.

Fairlie, R. W., & London, R. A. (1997). The effect of incremental benefit levels on births to AFDC recipients. Journal of Policy Analysis and Management, 16, 575-597.

Ford, K. 1990. Duration of residence in the United States and fertility of U.S. immigrants. International Migration Review, 24, 34-68.

Garfinkel, I., Huang, C. C., McLanahan, S. S., & Gaylin, D. S. (2003). The role of Child Support Enforcement and welfare in non-marital childbearing. Journal of Population Economics, 16, 55-70.

Gauthier, A. H., & Phillipov, D. (2008). Can policies enhance fertility in Europe? Vienna Yearbook of Population Research, 6, 1-16.

Geronimus, A. T., & Korenman, S. (1992). The socioeconomic consequences of teenage childbearing reconsidered. Quarterly Journal of Economics, 107, 1187-1214.

Gold, R. B. (2001, February). Title X: Three decades of accomplishment. The Guttmacher Report on Public Policy. New York: Guttmacher Institute.

Gold, R. B., Sonfield, A. C., Richards, L., & Frost, J. J. (2009). Next steps for America's family planning program: Leveraging the potential of Medicaid and Title X in an evolving health care system. New York: Guttmacher Institute.

Grant, J., Hoorens, S., Sivadasan, S, van het Loo, M., DaVanzo, J., Hale, L., Gibson, S., & Butz, W. (2004). Low fertility and population ageing: Causes, consequences, and policy options. Santa Monica, CA: RAND Corporation.

Grogger, J., & Bronars, S. G. (2001). The effect of welfare payments on the marriage and fertility behavior of unwed mothers: Results for a twins experiment. The Journal of Political Economy, 109, 529-545.

Grogger, J., Karoly, L., & Klerman, J. (2002). Consequences of welfare reform: A research synthesis. DRU-2676-DHHS. Santa Monica, CA: Rand Corporation.

Gruber, J. (2003). Medicaid. In R. A. Moffitt (Ed.), Means-tested transfer programs in the United States. Chicago: University of Chicago Press.

Guldi, M. (2008). Fertility effects of abortion and birth control access for minors. Demography, 45, 817-827.

Guttmacher Institute. (2011). Medicaid family planning eligibility expansions. State policies in brief. Retrieved November 10, 2011, from http://www.guttmacher.org/statecenter/spibs/spib_SMFPE.pdf.

Guttmacher Institute and Kaiser Family Foundation. (2007). Medicaid's role in family planning. Women's issue brief. New York and Washington, DC: Author. Retrieved May 20, 2011, from http://www.kff.org/womenshealth/7064.cfm.

Hanewall, C., & Lopoo, L. M. (2008). Child Support Enforcement and child poverty: A long-term perspective based on findings from the United States. International Journal of Public Administration, 31, 195-210.

Hao, L., Astone, N. M., & Cherlin, A. J. (2007). Effects of child support and welfare policies on nonmarital childbearing and motherhood. Population Research and Policy Review, 26, 235-257

Hao, L., & Cherlin, A. (2004). Welfare reform and teenage pregnancy, childbirth, and school dropout. Journal of Marriage and Family, 66, 179-194.

Haskins, R., & Blank, R. M. (2001). Welfare reform: An agenda for reauthorization. In R. M. Blank & R. Haskins (Eds.), The new world of welfare (pp. 3-32). Washington, DC: The Brookings Institution.

Haveman, R., & Wolfe, B. (1995). The determinants of children's attainment: A review of methods and findings. Journal of Economic Literature, 33, 1829-1878.

Henne, M. B., & Bundorf, M. K. (2008). Insurance mandates and trends in infertility treatments. Fertility and Sterility, 89, 66-73.

Herbst, C. M. (2011). The Earned Income Tax Credit and abortion. Social Science Research, 40, 1638-1651.

Hesketh, T., Lu, L., & Xing, Z. W. (2005). The effect of China's one-child family policy after 25 years. New England Journal of Medicine, 353, 1171-1176.

Hesketh, T., & Zhu, W. X. (1997). The one child family policy: The good, the bad, and the ugly. Br Med J, 314, 1685-1687.

Horvath-Rose, A. E., & Peters, E. (2001). Welfare waivers and nonmarital childbearing. In G. Duncan & P. L. Chase-Landsdale (Eds.), For better and for worse: Welfare reform and the well-being of children and families (pp. 222-246). New York: Russell Sage.

Horvath-Rose, A. E., Peters, H. E., & Sabia, J. J. (2008). Capping kids: The family cap and nonmarital childbearing. Population Research and Policy Review, 27, 119-138.

Jain, T., Harlow, B. L., & Hornstein, M. D. (2002). Insurance coverage and outcomes of in vitro fertilization. New England Journal of Medicine, 347, 661-666.

Joyce, T. J., Henshaw, S. K., Dennis, A., Finer, L. B., & Blanchard, K. (2009). The impact of state mandatory counseling and waiting period laws on abortion: A literature review. New York: Guttmacher Institute.

Joyce, T., Henshaw, S. K., & Skatrud, J.D. (1997). The impact of Mississippi's mandatory delay law on abortions and births. Journal of the American Medical Association, 287, 653-658.

Joyce, T., Kaestner, R., & Colman, S. (2006). Changes in abortion and births and the Texas parental notification law. New England Journal of Medicine, 354, 1031-1038.

Joyce, T., Kaestner, R., & Korenman, S. (2003). Welfare reform and non-marital fertility in the 1990s: Evidence from birth records. Advances in Economic Analysis and Policy, 3, 1-34.

Joyce, T., Kaestner, R., Korenman, S., & Henshaw, S. (2004). Family cap provisions and changes in births and abortions. Population Research and Policy Review, 23, 475-511.

Joyce, T., Kaestner, R., & Kwan, F. (1998). Is Medicaid pronatalist? The effect of eligibility expansions on abortions and births. Family Planning Perspectives, 30, 108-113, 127.

Joyce, T., Tan, R., & Zhang, Y. (2011). Changes in birthrates of young women following access to the pill and abortion in the early 1970s. Unpublished manuscript.

Junod, S. W., & Marks, L. (2002). Women's trials: The approval of the first oral contraceptive pill in the United States and Great Britain. Journal of the History of Medicine, 57, 117-160.

Kaestner, R., Korenman, S., & O'Neil, J. (2003). Has welfare reform changed teenage behaviors? Journal of Policy Analysis and Management, 22, 225-248.

Kaiser Family Foundation. (2002). The Medicaid resource book. Washington, DC: Author. Retrieved March 12, 2011, from http://www.kff.org/medicaid/loader.cfm?url=/commonspot/security/getfile.cfm&PageID = 14266.

Kane, T. J., & Staiger, D. (1996). Teen motherhood and abortion access. The Quarterly Journal of Economics, 111, 467-506.

Kaushal, N., & Kaestner, R. (2001). From welfare to work: Has welfare reform worked? Journal of Policy Analysis and Management, 20, 699-719.

Kearney, M. S. (2004). Is there an effect of incremental welfare benefits on fertility behavior? A look at the family cap. Journal of Human Resources, 39, 295-325.

Kearney, M. S., & Levine, P. B. (2009). Subsidized contraception, fertility, and sexual behavior. The Review of Economics and Statistics, 91, 137-151.

Kent, M. M., & Mather, M. (2002). What drives U.S. population growth? Population Bulletin, 57, 3-38.

Klerman, J. (1999). U.S. abortion policy and fertility. American Economic Review, 89, 261-264.

Korenman, S., Joyce, T., Kaestner, R., & Walper, J. (2006). What did the "illegitimacy bonus" reward? Topics in Economic Analysis and Policy, 6, Article 8.

Leibowitz, A. (1990). The response of births to changes in health care costs. Journal of Human Resources, 25, 697-711.

Levine, P. B. (2003). Parental involvement laws and fertility behavior. Journal of Health Economics, 22, 861-878.

Levine, P. B. (2004). Sex and consequences: Abortion, public policy, and the economics of fertility. Princeton, NJ: Princeton University Press.

Levine, P. B., Staiger, D., Kane, T. J., & Zimmerman, D. J. (1999). Roe v. Wade and American fertility. American Journal of Public Health, 89, 199-203.

Levine, P. B., Trainor, A. B., & Zimmerman, D. J. (1996). The effect of Medicaid abortion funding restrictions on abortions, pregnancies, and births. Journal of Health Economics, 15, 555-578.

Lindrooth, R. C., & McCullough, J. S. (2007). The effect of Medicaid family planning expansions on unplanned births. Women's Health Issues, 17, 66-74.

Lochhead, C. (2006, May 7). A legacy of the unforeseen: Immigration reform: U.S. policies aimed at controlling the flow of newcomers historically have led to unexpected consequences. The San Francisco Chronicle. Retrieved June 6, 2011, from http://www.sfgate.com/cgi-bin/article.cgi?f=/c/a/2006/05/07/MNGELIN29I1.DTL&ao=2.

Lopoo, L. M., & DeLeire, T. (2006). Did welfare reform influence the fertility of young teens? Journal of Policy Analysis and Management, 25, 275-298.

Matthews, S., Ribar, D., & Wilhelm, M. (1997). The effects of economic conditions and access to reproductive health services on state abortion and birth rates. Family Planning Perspectives, 29, 52-60.

Mellor, J. M. (1998). The effect of family planning programs on the fertility of welfare recipients: Evidence from Medicaid claims. Journal of Human Resources, 33, 866-895.

Moffitt, R. A. (1998). Introduction. In Robert Moffitt (Ed.), Welfare, the family, and reproductive behavior (pp. 1-8). Washington, DC: National Research Council.

Moffitt, R. A. (1999). Explaining welfare reform: Public choice and the labor market. International Tax and Public Finance, 6, 289-315.

Moffitt, R. A. (2002). The Temporary Assistance for Needy Families program. In R. A. Moffitt (Ed.), Means-tested transfer programs in the United States (pp. 291-363). Chicago: The University of Chicago Press.

Moffitt, R. A. (2008). A primer on U.S. welfare reform. Focus, 26, 15-25.

Murray, C. (1984). Losing ground: American social policy 1950-1980. New York: Basic Books.

National Abortion Federation. (n.d.). The TRAP: Targeted regulation of abortion providers. Retrieved December 14, 2011, from http://www.prochoice.org/pubs_research/publications/downloads/about_abortion/trap_laws.pdf.

Nowak, M. W., Fisherman, M. E., & Farrell, M. E. (2003). State experience and perspectives on reducing out-of-wedlock births. Prepared for the Assistant Secretary for Planning and Evaluation, Department of Health and Human Services. Washington, DC: U.S. Department of Health and Human Services.

Offner, P. (2005). Welfare reform and teenage girls. Social Science Quarterly, 86, 306-322.

Personal Responsibility and Work Opportunity Reconciliation Act, 110 U.S.C. Ž 2105 (1996).

Pirog, M. A., & Ziol-Guest, K. M. (2006). Child Support Enforcement: Programs and policies, impacts and questions. Journal of Policy Analysis and Management, 25, 943-990.

Plotnick, R. D., Garfinkel, I., McLanahan, S. S., & Ku, I. (2004). Better Child Support Enforcement: Can it reduce teenage premarital childbearing? Journal of Family Issues, 25, 634-657.

Resolve. (n.d.). Insurance coverage in your state. Retrieved November 14, 2011, from http://www.resolve.org/family-building-options/insurance_coverage/state-coverage.html.

Romero, D., & Agenor, M. (2009). US fertility prevention as poverty prevention. Women's Health Issues, 19, 355-364.

Sabia, J. (2008). Blacks and the family cap: Pregnancy, abortion, and spillovers. Journal of Population Economics, 21, 111-134.

Schmidt, L. (2007). Effects of infertility insurance mandates on fertility. Journal of Health Economics, 26, 431-446.

Sonenstein, F. L., Punja, S., & Scarcella, C. (2004). Future directions for family planning research: A framework for Title X family planning service delivery and improvement research. Washington, DC: The Urban Institute.

Turturro, C., Benda, B., & Turney, H. (1997). Arkansas welfare waiver demonstration project. Final report: July 1994 through June 1997. Little Rock: University of Arkansas at Little Rock, School of Social Work.

U.S. Congress, House of Representatives, Committee on Ways and Means. (n.d.). Retrieved May 27, 2011, from http://waysandmeans.house.gov/media/pdf/110/cse.pdf.

U.S. Department of Agriculture. (n.d.). Retrieved November 18, 2011, from http://www.fns.usda.gov/snap/government/FY12_Allot_Deduct.htm.

U.S. Department of Health and Human Services. (1995). Report to Congress on out-of-wedlock childbearing. Retrieved April 9, 2011, from http://www.cdc.gov /nchs/data/misc/wedlock.pdf.

U.S. Department of Health and Human Services. (2001). State welfare waivers: An overview. Retrieved December 28, 2011, from http://aspe.hhs.gov /hsp/isp/waiver2/waivers.htm.

U.S. Department of Health and Human Services. (n.d.). Family planning. Retrieved March 15, 2011, from http://www.hhs.gov/opa /familyplanning/index/html.

University of Kentucky Center for Poverty Research. National Data Set. Retrieved December 14, 2011. http://www.ukcpr.org/AvailableData.aspx.

When less is more: Border enforcement and undocumented immigration. Testimony before the Subcommittee on Immigration, Citizenship, Refugees, Border Security, and

International Law of the Committee on the Judiciary, 110th Cong., 1 (2007) (testimony of D.S. Massey).

Whittington, L. A. (1992). Taxes and the family: The impact of the tax exemption for dependents of marital fertility. Demography, 29, 215-226.

Whittington, L. A., Alm, J., & Peters, H. E. (1990). Fertility and personal exemption: Implicit pronatalist policy in the United States. American Economic Review, 80, 545-556.

Williams, E., Johnson, N., & Shure, J. (2010). State Earned Income Tax Credit: 2010 legislative update. Retrieved March 9, 2012, from http://www.cbpp.org/cms/index.cfm?fa=view&id=2987.