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Pregnancy Medicaid Expansions and Fertility: Differentiating Between the Intensive and Extensive Margins

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Abstract The theoretical and empirical links between public health insurance access and fertility in the United States remain unclear. Utilizing a demographic cell-based estimation approach with panel data (1987–1997), we revisit the large-scale Medicaid expansions to pregnant women during the 1980s to estimate the heterogeneous impacts of public health insurance access on childbirth. While the decision to become a parent (i.e., the extensive margin) appears to be unaffected by increased access to Medicaid, we find that increased access to public health insurance positively influenced the number of high parity births (i.e., the intensive margin) for select groups of women. In particular, we find a robust, positive birth effect for unmarried women with a high school education, a result which is consistent across the two racial groups examined in our analysis: African American and white women. This result suggests that investigating effects along both the intensive and extensive margin is important for scholars who study the natalist effects of

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social welfare policies, and our evidence provides a more nuanced understanding of the influence of public health insurance on fertility.

Keywords Medicaid · Fertility · Parity

JEL Classification I1 · J13 · J18

Introduction

Created in 1965, the Medicaid program provides public health insurance for many low-income groups, including pregnant women, infants, and children. Since its inception, the program has grown tremendously, both in terms of expenditures and coverage. For instance, in 2010, 44% of all births in the United States were principally financed by the program, up from 15% in 1985 (Singh et al. 1994; Markus et al. 2013). The State Children's Health Insurance Program (SCHIP) and the Affordable Care Act (ACA) extended coverage to even more low-income mothers and children (Kaiser Family Foundation 2014), although there is currently strong political pressure to scale back the ACA with multiple ACA repeal bills nearly passing in 2017. While ostensibly designed to cover health care expenditures, critics have long speculated that the benefits provided through government social welfare programs—such as cash welfare (AFDC/TANF) and Medicaid—induce low-income women to bear children by reducing the financial costs of giving birth. Theoretically, any large reduction in the cost of childbearing could increase fertility (Becker 1960, 1991). Given that the average cost of childbirth in the United States was $4334 in 1989 (Health Insurance Association of America 1989)—which is roughly $8500 in 2016 dollars—public health insurance expansions to pregnant women should greatly reduce the costs of childbirth for the previously uninsured, potentially affecting fertility.

There is a large empirical literature investigating the link between social welfare programs and childbearing at the population level, most of which suggests that social policies do not have large fertility effects, if any at all (Lopoo and Raissian 2012, 2014; Moffitt 2003). Part of that literature is a series of articles that investigates the fertility effects of the Medicaid program. While some of the broader literature on social policy and fertility estimates the policy effects by parity (see e.g., Joyce et al. 2004; Milligan 2005; Baughman and Dickert-Conlin 2009), the Medicaid literature has not.[1] Moreover, it is possible that previous attempts to isolate these fertility effects for the Medicaid program were hampered by heterogeneity in the response by birth parity. As we show below, the decision to become a parent, i.e., the "extensive margin," may be affected by social policy very differently from its influence on a 2nd, 3rd, or even higher-order birth, i.e., the "intensive margin." Utilizing the natural experiment created by the federally

[1] DeLeire et al. (2011) provide one online table that investigates first births along as a robustness check. They do not, however, run models with higher-parity births separately.

mandated Medicaid expansions to pregnant women occurring in the late 1980s and early 1990s, our goal in this paper is to investigate the heterogeneous impacts of increased public health insurance access on births at different parities.

More specifically, we use the demographic cell-based estimation approach employed by Zavodny and Bitler (2010) and DeLeire et al. (2011) as a conceptual starting point for our empirical modeling in this paper. We define our policy variable as “state Medicaid income eligibility threshold as a percent of the poverty line” (as do Zavodny and Bitler 2010), following the argument of Hamersma and Kim (2013) that use of thresholds directly reduces mismeasurement and, thus, bias, which could be caused by imputing eligibility as done in other work on Medicaid expansions. Through this combination of a more refined measure of access to public health care for pregnant women and an analysis of birth trends by parity, we attempt to better understand the behavioral impacts of a large-scale expansion of health insurance eligibility.

In our empirical modeling, we do not find consistent effects of the Medicaid expansions on the decision to enter motherhood—i.e., on the extensive margin. However, we see statistically significant and robust increases in higher-order births for unmarried, African American and white women with a high school education. We note similar results for other unmarried women categories, but these results are not robust across specifications.

Relevant Literature

Policy scholars have completed a large body of research attempting to estimate the relationship between social welfare programs and fertility. The largest set of work investigates the relationship between the AFDC/TANF program and fertility (see e.g., Duncan and Hoffman 1990; Hau and Cherlin 2004; Hoynes 1997; Kaestner et al. 2003; Kearney 2004; Lopoo and DeLeire 2006). However, researchers have also studied links between fertility and Child Support Enforcement (see e.g., Garfinkel et al. 2003; Plotnick et al. 2004) and the Supplemental Nutrition Assistance Program (Almond et al. 2001). Lopoo and Raissian (2012, 2014) summarize this literature and find mixed evidence of a fertility effect resulting from social welfare policies. In some instances, the authors estimate a pronatalist effect, but it is often statistically insignificant and, when significant, small.

More relevant for this study, research from the RAND Health Insurance Experiment (HIE) was the first to document a relationship between health insurance coverage and fertility. In this experimental setting, women who were offered free health insurance were 29% more likely to have a child during the experimental period than women in the control group who had a cost-sharing plan (Leibowitz 1990).[2] Consistent with much of this literature, Leibowitz writes that the observed differences are most likely a change in the timing of childbearing, or what

[2] The RAND HIE had three different cost-sharing plans, but because the policies had a maximum out-of-pocket expenditure, most members of the control group paid approximately $1000 for their insurance coverage.

demographers call a "tempo effect." It is difficult to determine from this investigation if women in the treatment group had more children than they would in the counterfactual state or if they simply changed the point in their life-course when they had children.

To date, four published articles examine the link between Medicaid expansions and fertility. Joyce et al. (1998) use data from 15 states between 1986 and 1992 for 19- to 27-year olds with a high school education or less to estimate the relationship between Medicaid expansions and state fertility rates. The expansions were measured with two indicator variables: the first was coded one if the state increased the eligibility threshold to the federal poverty line following the 1986 Omnibus Reconciliation Act (OBRA), and a second if the state expanded eligibility to income-to-needs ratios between 1.0 and 1.85 of the federal poverty line following the 1987 and 1989 OBRAs. They find that the first expansion is associated with a 5% increase in births among white women and no change among African American women.

Kearney and Levine (2009) used expansions of the family planning services provided through the Medicaid program to women who would have lost their eligibility after giving birth or because their incomes made them ineligible. Using the vital statistics data and a difference-in-differences model, they find a reduction in fertility around 2% for non-teens and about 4% for teens due to increased contraception use.

Zavodny and Bitler (2010) and DeLeire et al. (2011) use data from all 50 states and the District of Columbia. In addition, they construct the fraction of a national population eligible for Medicaid in each state based on the policies in place each year, a technique first used by Currie and Gruber (1996). In the case of Zavodny and Bitler, similar to the current study, they also use the expansion-related income thresholds for Medicaid eligibility expressed as a fraction of the poverty line. Neither Zavodny and Bitler (2010) nor DeLeire et al. (2011) find a statistically significant relationship between the simulated eligibility measure and fertility. Zavodny and Bitler do show a positive and statistically significant relationship between the Medicaid threshold and fertility among white women with less than 12 years of education.

Importantly, the outcome used in all of these earlier studies on Medicaid and fertility is either an aggregate birthrate or the number of births.[3] As we explained earlier, these measures treat births the same regardless of parity, conflating changes in fertility on the extensive and intensive margins. Given the potential differences by parity, our research separately models first, second, and third (or more) births. Moreover, a second limitation of the previous literature is in the potential for mismeasurement of eligibility when simulating the generosity of the state-level Medicaid program, as in DeLeire et al. (2011). By using the state Medicaid thresholds directly, which are a function of the federal poverty line and do not vary by family size or marital status of the parents, we utilize a more refined measure which increases the accuracy of our estimates. And, finally, by using an algorithm to assign values to mothers with missing educational attainment data in the birth

[3] Both outcome variables are estimated using the natural log of the birth measure.

certificate data, we are able to estimate our demographic cell-based approach using all births recorded in the United States.[4] All of these efforts provide the strongest evidence to date that Medicaid expansions to pregnant women significantly impacted higher-order fertility.

Theoretical Background

Theoretically, the Medicaid expansions we investigate are likely to produce pronatalist responses. To begin, the cost of prenatal care, delivery, and well-child visits are covered by this public insurance program, which reduces the cost of giving birth substantially. Following the neoclassical microeconomic model, these cost reductions should lead to an increase in the demand for children (Becker 1960, 1991) which would drive up the number of intentional pregnancies. Moreover, unintentional pregnancies may be more likely to result in birth rather than abortion due to the reduced costs (Zavodny and Bitler 2010). At the same time, it is plausible that parents would opt to have fewer children based on the quality-quantity trade-off parents face (Becker and Lewis 1973) if expansions improve child health.

As mentioned earlier, most reviews that cover the relationship between policy and fertility find very little evidence of a connection, and this is especially true for the small literature studying the relationship between Medicaid and fertility. However, recent evidence from Aaronson et al. (2014), who investigated different family sizes among African American families based on the cost of school, demonstrates that cost changes can affect the extensive and intensive margins differently, and this may explain some of the null findings in the literature. The decision to become a parent, i.e., have one's first child, is a major life decision. The birth of the first child confers benefits that are hard to quantify, but include the opportunity to love and be loved in a unique way and, for some, symbolic entry into adulthood (Anderson 1990). The gain many potential parents expect from entering parenthood may be so large that small cost savings from public programs, for example, do not affect their fertility decision on the extensive margin appreciably. Moreover, they may not have clear information on medical costs (or cost savings from Medicaid) when giving birth for the first time.[5] If, however, there are diminishing marginal returns to parenthood for each birth (because some benefits are only conferred once, upon entering parenthood) these same cost savings may alter one's decision to become a parent for, say, the third or fourth time. Thus, one might expect to observe different responses on the intensive and extensive margins caused by policy change.

[4] In the early period, education data were not collected in California, New York, Texas, and Washington. Zavodny and Bitler (2010) exclude these observations when examining models by mother's education; however, we recover these observations using the methodology outlined in the Appendix.

[5] While it is possible that some uninsured women would not have had to pay the full cost of birth in the absence of expanded Medicaid, it is still the case that discounted prices or limited charity care would likely be inferior to a fully covered Medicaid birth.

Legislative Background

A shared link between this paper and its predecessors in the literature is the natural experiment exploited to derive potentially causal impacts of expanded access to public health insurance. The federal government began mandating major expansions in coverage for pregnant women and children for the state-run Medicaid programs beginning in the mid-1980s.[6] Before this series of legislative acts, Medicaid was typically tied to the Aid to Families with Dependent Children (AFDC) program, the cash welfare system targeting low-income single mothers. Under this specific initial targeting, other low-income populations, such as single women pregnant for the first time or married women, were often categorically ineligible, even though many of them would have met the AFDC income thresholds established at the state level.

Seeking, in part, to address the comparatively high infant mortality rate in the United States (Currie and Gruber 1996), Congress began enacting legislation that gradually expanded Medicaid access to many low-income populations. Starting annually in 1984, there were six major legislative actions mandating coverage expansions to pregnant women and their children.[7] With these acts, Congress substantially decoupled the Medicaid and AFDC programs and greatly increased the number of individuals eligible for public health insurance coverage in the United States. Moreover, since Congress allowed the states several years to fully convert and expand their programs to meet the requirements of the new federal minimum standards, researchers can use the rollout of the state-level response to these mandates as a treatment in a quasi-experimental research design.

Similar to the needs standards established by the individual states in the administration of their AFDC programs, there were initially large differences in the income thresholds used by the states to determine eligibility for public supports. Figure 1 illustrates the average income thresholds facing pregnant women and infants by state, as a percent of the Federal Poverty Level, from 1986 to 1997.[8]

Thresholds in 1986 ranged from 16% of FPL (Alabama), effectively barring nearly everyone with income from public health insurance coverage, to 112% of FPL (California), already above the first federally mandated level of 100% of FPL. By 1990, all states met or exceeded 100% FPL, and soon all states exceeded 133%

[6] As noted earlier, the Medicaid program in the United States dates back to 1965. It is designed as a state and federal partnership, whereby states receive significant federal funds to offset healthcare costs borne at the local level. In exchange for these federal funds, states were mandated to provide select services and cover select populations and, in the initial years, the administration of the state-level public health insurance program (Medicaid) was typically linked to the state-level cash assistance program (AFDC). Both the population and services have change greatly over time—the increase in generosity for the former is the natural experiment we examine in this analysis.

[7] The annual expansions are as follows: the Deficit Reduction Act of 1984, the Consolidated Omnibus Budget Reconciliation Act of 1985, the Omnibus Reconciliation Act of 1986, the Omnibus Reconciliation Act of 1987, the Medicare Catastrophic Act of 1988, and the Omnibus Reconciliation Act of 1989.

[8] In the earlier years, these thresholds were often set in dollars rather than percent of FPL. We use Hill (1992) as the primary source for thresholds in the early period, and follow him in taking the maximum of the AFDC Payment Standard and the Medically Needy Income threshold and then dividing by the annual FPL to generate the numbers reported in the table.

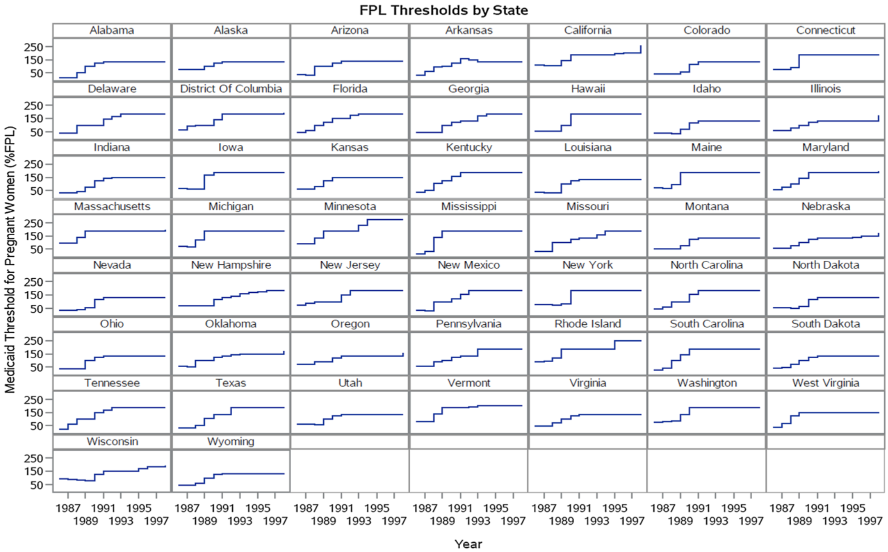


Fig. 1 Average income thresholds for pregnant women—percent of FPL

FPL (the subsequent mandated level) and many moved to 185%.[9] This variation across states and over time allows us to investigate possible changes in fertility patterns by parity that coincide with these changes in access to maternity care.

Data and Estimation Approach

Our analysis requires a rich national dataset with information on births by parity coupled with detailed demographic information and supplemented with state-level policy data. We begin with birth counts from Vital Statistics for 1987 through 1997, which defines a period during which all women in the United States could have qualified for public health insurance with a pregnancy, i.e., regardless of marital status or the number of previous children, based upon family income considerations alone.[10] Moreover, since the early Vital Statistics data contain limited information on ethnicity, we examine birth counts for two racial groups only: African Americans and whites.

In addition to race, as illustrated in Table 1, we use age, parity, education, and marital status to define our demographic cells.

As displayed in Table 1, of the 35 million U.S. births to black and white women from 1987 to 1997, 15% were to African American women and the remaining 85% were to white women.

[9] Annual thresholds are provided by state-year in Table 1 of the Appendix ESM.

[10] We aggregate data to the quarterly level to allow for threshold changes occurring throughout the course of a given year. Additionally, note that married women became categorically eligible on July 1, 1986 (though still subject to the income test). Allowing 9 months for gestation, this means that the first observation in estimation will be in 1987.

Table 1 Demographic cell construction

Race	Age	Parity groups	Education	Marital status
Black (15%)	20–34 (89%)	1st Birth (35%)	< High School (17%)	Married (76%)
White (85%)	35–44 (11%)	2nd Birth (35%)	High School Diploma (37%)	Unmarried (24%)
		3rd + Birth (30%)	High School Plus (46%)	
[2 groups]	[2 groups]	[3 groups]	[3 groups]	[2 groups]

Numbers in parentheses show the corresponding percent of the nearly 35.3 million births in the Vital Statistics database from Q2 1987 to Q4 1997

We selected the remaining cells in Table 1 for several reasons—perhaps most importantly to create demographic cells with variation by birth parity. Cutting the cells too finely generated a large number of cells with zero births, particularly among African Americans. We made two important decisions regarding age groups. First, we excluded teenage births from our sample, as within this group there is very little variation in parity (mostly first births), educational attainment, or marital status. Moreover, we expect teenagers to be less sensitive to the details of Medicaid policy, as most teen pregnancies are unintended. Second, we categorized the rest of the sample into two age groups, aligning with the medical practice of treating mothers aged 35 and up as "high-risk" pregnancies (this older group constituted 11% of the births in the analysis period). We avoid choosing narrower age bands, not only to avoid the problem of small cells, but also to mitigate issues of compositional change within cells as age-at-first-birth increased across cohorts during the sample period.[11]

Our choice of three parity groups allows us to examine incremental changes for the second child separately from those of higher parity (3+). As displayed in Table 1, the birth sample was split in roughly the same proportion across these three birth categories.

The education levels and marital statuses[12] align with common demographic standards.[13] We use the former as a proxy for earnings capacity, and household income, since Vital Statistics does not report any income data. Moreover, we expect that, as the mandated Medicaid thresholds increase significantly over time, women with higher levels of education—particularly those with exactly a high school diploma—increasingly gain access to the program. So, effects within this range could be particularly pronounced. For the sample proportions of these demographic

[11] As a specification check, we also estimated models breaking age up into three groups: 20–27, 28–34, and 35 and older. Results were nearly identical.

[12] Initially, we do not separate cells by marital status since it is endogenous with the fertility choice. Because unmarried women have lower incomes than married women, all else equal, we provide results separating married and unmarried women as a robustness check. As we show below, this distinction is important so all sample size counts reported include a distinction between married and unmarried women.

[13] In the Vital Statistics data, reporting of mother's educational attainment was not mandated until 1992. Thus, for some large states—namely California, New York, Texas, and Washington—data are missing in this early period. To recover these observations, we use an allocation algorithm as outlined in the Appendix ESM.

variables, roughly 17% of all births were to mothers with education levels less than a high school diploma, 37% occurred to mothers with exactly a high school diploma, and the remaining 46% occurred to mothers with education levels beyond high school. Additionally, 76% of approximately 35 million births were to married mothers.

Finally, to account for gestation after the July 1, 1986 threshold—after which all potentially pregnant women qualified for public health insurance based upon income considerations alone—the first quarter used in estimation is 1987 Q2, while the last used in our core modeling is 1997 Q4. This corresponds to 43-year-quarter observations for each demographic cell in a particular state. Because some cells do not have any births in some quarters, our sample sizes vary depending on the variables used in each specification.[14] While we could prevent some cell losses by aggregating up to annual instead of quarterly cells, births are known to be both seasonal and cyclical, and recent work by Buckles, Hungerman, and Lugauer (manuscript, 2017) highlights the problems with using annual data when studying fertility and business cycles.

In addition to the Vital Statistics birth certificate data constituting the primary outcome variable in our analysis, we use annual estimates of demographic-cell-specific population counts, derived from the 1980, 1990, and 2000 Public Use Microsamples (PUMS) collected during the Decennial Censuses.[15] Please refer to the Appendix ESM for more details on the construction of these population counts. To define the Medicaid eligibility thresholds as outlined in the last section, we used archived Maternal and Child Health (MCH) Updates produced by the National Governors Association, as well as Hill (1992). Finally, we merge several economic and policy controls into the dataset, including the AFDC/TANF benefit level, the quarterly unemployment rate, and variables related to welfare reform (family cap, time limit, and implementation indicators).[16]

After assembling the demographic cell-based data set, we estimate the following model:

$$\begin{aligned}\ln(\text{birth})_{\text{stqc}} = {} & \alpha + \beta_1(\text{Medicaid})_{\text{st}(q-3)c} + \beta_2(\text{Unemployment Rate})_{\text{st}(q-3)} \\ & + \beta_3(\text{Abortion})_{\text{st}(q-3)} + \beta_4\ln(\text{pop})_{st} + \pi\text{Welfare}_{\text{st}(q-3)p} \\ & + \gamma_s + \delta_t + \vartheta_q + \sigma_c + \varepsilon_{stqc},\end{aligned} \tag{1}$$

where the outcome *birth* is the log number of births in state s in year t in quarter q for a given demographic cell, c, which are outlined in Table 1. The key variable of

[14] Similar to DeLeire et al. (2011), we estimate the models through 1997 to allow a sufficient period for estimation. Given the demographic cells outlined in Table 1, this implies a maximum number of 43 * (51 * 2 * 2 * 3 * 3 * 2) = 43 * 3672 = 157,896 aggregated observations for analysis. However, we were concerned about including time series for cells with zero counts in some years. Small change for these cells over time could produce very large proportionate changes. As a result, we fix the panel at the most disaggregated level to only those cells which have sample weights over the entire duration of our analysis. With this restriction, the number of demographic cells declines from 3672 to 3435, yielding a maximum of 147,705 observations. Additionally, this choice excludes just 3357 of the underlying 35,253,495 births used to create the demographic cells.

[15] The source of this data is IPUMS USA (Ruggles et al. 2015).

[16] See DeLeire et al. (2011) for details.

interest is *Medicaid*, the Medicaid threshold as a fraction of FPL, i.e., coded so that 1 represents a threshold of 100% FPL. We again lag this by three quarters to align with the time of conception. The control variables include the quarterly unemployment rate (at time of conception), an indicator for state-level restrictions on the use of Medicaid funds for abortions, and the natural log of the state population for the demographic cell. *Welfare* is a vector of state-level welfare program characteristics, including the AFDC/TANF benefit level (for family size p), a family cap provision indicator, a time-limit waiver indicator, and an indicator for the post-TANF period.[17] *Abortion* and *Unemployment* are scalars for state-level rates of each, again aligned to conception. The parameters $\gamma_s, \delta_t, \vartheta_q$, and σ_c represent state, year, quarter, and cell fixed-effects, respectively, and ε_{stqc} is a state-level clustered standard error. Finally, regressions are weighted by the demographic cell group weight and are often estimated separately by homogeneous sets of women.

Note that the Medicaid variable is coded differently here than in Joyce et al. (1998) and DeLeire et al. (2011), in alignment instead with recent work on Medicaid expansions and insurance coverage by Zavodny and Bitler (2010) and Hamersma and Kim (2013). Using the income threshold directly as a covariate and avoiding the imputation of eligibility—a required step to create the simulated instrument pioneered by Cutler and Gruber (1996) and used in much of the literature—has some advantages. First, it reduces the inevitable measurement error of eligibility determination that depends upon caseworker information often unavailable to the researcher.[18] When classical, such error generates attenuation bias that can lead to null findings when, in fact, the policy actually impacted behavior. When nonclassical, such error generates bias without a predicted sign. Second, the poverty threshold represents the policy lever over which policy makers actually have control; a change in policy *is* operationalized as a change in the threshold. This allows for easy interpretation of the coefficient on *Medicaid*: it is the estimated percentage change in births expected given a 100-%-of-FPL change in the Medicaid threshold (ex. a 1-FPL-unit change from 100% FPL to 200% FPL). This can easily be scaled down to consider smaller changes.

Our key innovation in this study is the addition of an interaction term between the parity fixed effects and the Medicaid threshold, allowing us to discover whether the threshold affects higher-order births differently than first births. After estimating Eq. 1, we will replace the Medicaid parameter with a vector of three parameters indicating the Medicaid threshold's relationship to first, second, and third (or higher-order) births. To the extent that these coefficients differ from zero, we will be able to identify the marginal impact of the Medicaid thresholds on births by parity.

[17] We also ran a set of models that exclude these policy measures, and the results were substantively identical.

[18] For example, Cutler and Gruber (1996) report that 25 percent of child Medicaid participants in their sample were imputed to be ineligible.

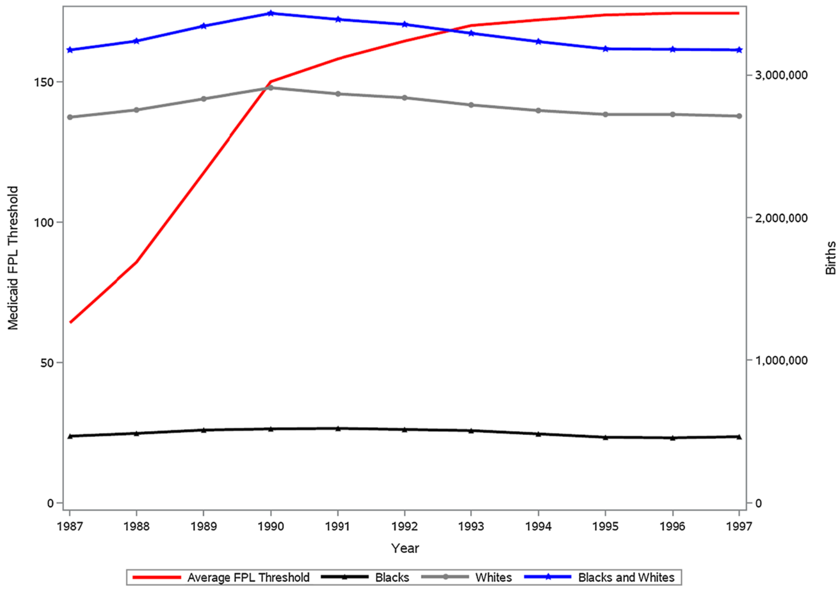


Fig. 2 Trends in U.S. births and access to Medicaid: women aged 20–44

Descriptive Statistics

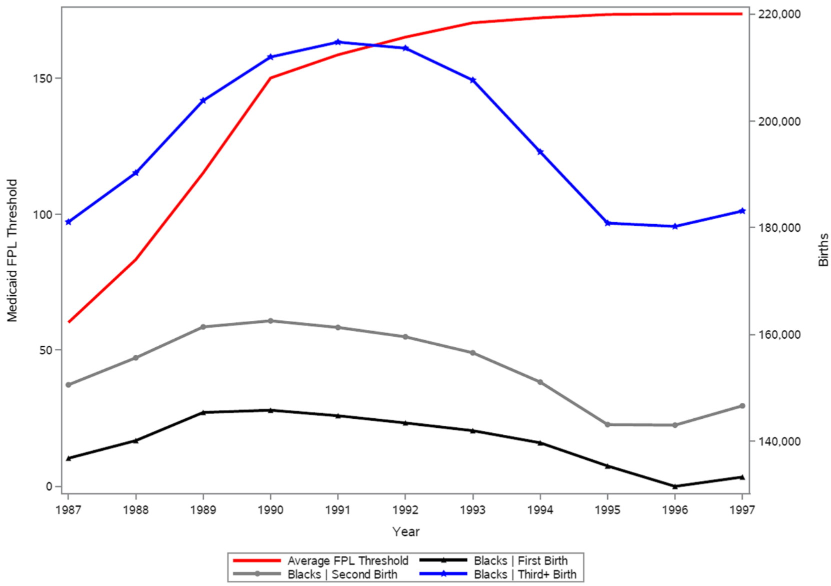
Having outlined the key variables utilized in this analysis, it is useful to examine the general trends in U.S. births relative to state Medicaid thresholds. Figure 2 shows the annual number of births for African American and white women combined from 1986 to 1997—for all birth parities.

As displayed, we see a marked increase in the average Medicaid thresholds selected by the individual states as more states comply with the federal mandates. For example, by 1989 the average threshold in the U.S. approaches 100% of FPL, while it exceeds 170% by the end of the period in 1997. Births increase slightly until 1990 and decline thereafter. Thus, at an aggregated level, it does not appear that there is much of a correlation between the Medicaid expansions and births in the United States. Recall this is exactly what researchers found in previous papers.

Figure 3 displays the trends in births by parity for African American and white women, respectively.

Relative to the extensive margin (i.e., selection into motherhood or first births), the intensive margin for higher-order births for African Americans (Panel A) appears to decline more pronouncedly in Fig. 3. In other words, in an era of general declines in fertility, these higher-order births may be buoyed by public health insurance expansions. The relationship for white women is less clear, as it appears the aggregated trends are simply declining after 1990. However, this exercise illuminates one important point driving the purpose of this paper: examination of trends at an aggregated level masks more disaggregated-level trends. These

Panel **A**: African Americans



Panel **B**: Whites

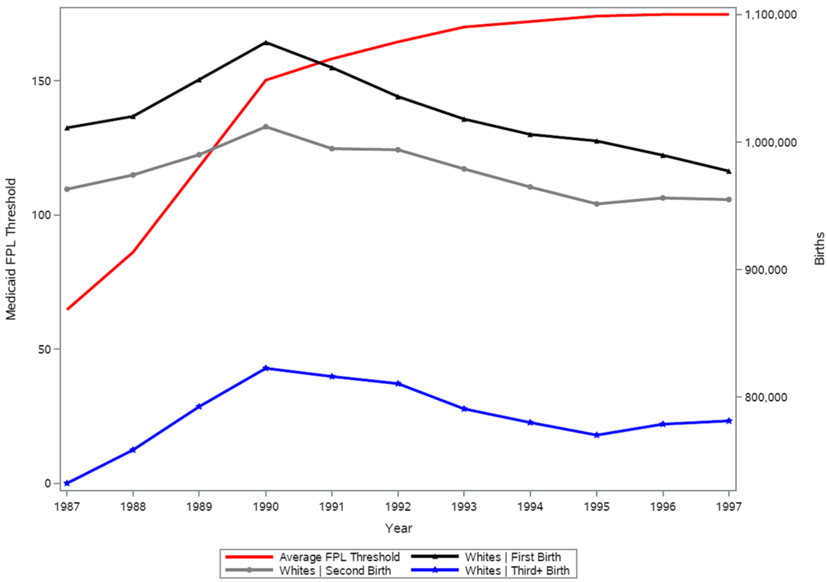


Fig. 3 Trends in U.S. births and access to Medicaid, women aged 20–44 by race

heterogeneous impacts—especially by birth parity—are what we seek to isolate in this paper.

Results

We begin our analysis with regression models that distinguish only on a binary measure of parity to compare our results generally with the extant literature, which shows very little evidence linking Medicaid and fertility. Table 2 provides results by race collapsing the birth outcomes into the number of births on the extensive margin (i.e., first births) and on the intensive margin (i.e., second or higher births). Because educational attainment is highly correlated with Medicaid eligibility, we also break up the results into those with less than or equal to a high school education and those with more than a high school education.

Of course, this more aggregated set of results does not account for the importance of marriage or the heterogeneity across education groups so one should not make too much of the point estimates; however, the direction of the relationship between fertility and Medicaid eligibility is quite different on the two margins. If correct, this might explain the null findings in the earlier literature. Moreover, when we split the sample by education levels, we also begin to observe the subgroups where the expansions seems to have had the greatest effect: those with a high school education or less. This set of results encourages more substantive models that allow for differences by parity. We turn to those models now.

Figure 4 displays our primary estimates for Medicaid eligibility allowing for differences by parity and education level, estimating separate Medicaid effects by subgroup. We illuminate the magnitude and statistical significance by displaying point estimates and the requisite 95% confidence intervals; the corresponding numerical results are found in Table 2 of the Appendix ESM.

We estimate fairly large positive coefficients for the Medicaid threshold for women with less than a high school education—particularly on the intensive margin—but none of these point estimates are statistically significant at the 95% level given their large standard errors. Among women with a high school education, we also do not find a statistically significant relationship between Medicaid and births on the extensive margin. However, the point estimates are larger and generally statistically significant on the intensive margin for both African American and white women. A 10-%-of-FPL increase in the threshold is associated with a 1.5–2.2% increase in higher-order births for African American women and a 1.2% increase for white women. Our increased precision in estimation for high school graduates may stem from their relative homogeneity (compared to wide variation in the less-than-high-school sample) and their larger sample proportion (about 37% of the sample are high school graduates, compared to only 17% dropouts). Finally, we do not observe statistically significant relationships between fertility and Medicaid eligibility among women with some college or a college education, consistent with their likely limited eligibility for Medicaid—and this is despite having fairly precise estimates, since these women account for about half of the sample. Our results are consistent with both (a) falling importance of Medicaid access as education rises

Table 2 Impact of Medicaid eligibility expansions on births modeling by race, education, and birth margin| 1987–1997

Medicaid threshold X birth parity	African American			White		
	All education levels	≤ High school	> High school	All education levels	≤ High school	> High school
Extensive margin	− 0.039	0.090	− 0.040	− 0.053***	− 0.014	− 0.049†
	[0.023]	[0.170]	[0.040]	[0.011]	[0.081]	[0.029]
Intensive margin	0.088***	0.203*	0.005	0.073***	0.144*	− 0.003
	[0.013]	[0.080]	[0.043]	[0.006]	[0.055]	[0.035]
Sample size	13,029	12,599	12,814	13,158	13,158	13,158

The outcome variable is modeled using the natural log of births and regressions are weighted by the population of women in each racial subgroup. Models include state, year, quarter, state-year, and state-cell fixed effects. All models also include controls for state unemployment rates, maximum cash welfare benefits for a family of three (AFDC or TANF), the natural logarithm of the state population for each racial subgroup, and indicators for family cap provisions, time-limit welfare waivers, the implementation of TANF, and state-level restrictions on the use of Medicaid funds for abortions. Standard errors are clustered at the state-level and are in brackets with statistical significance indicated as follows: †$p < 0.1$, *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$

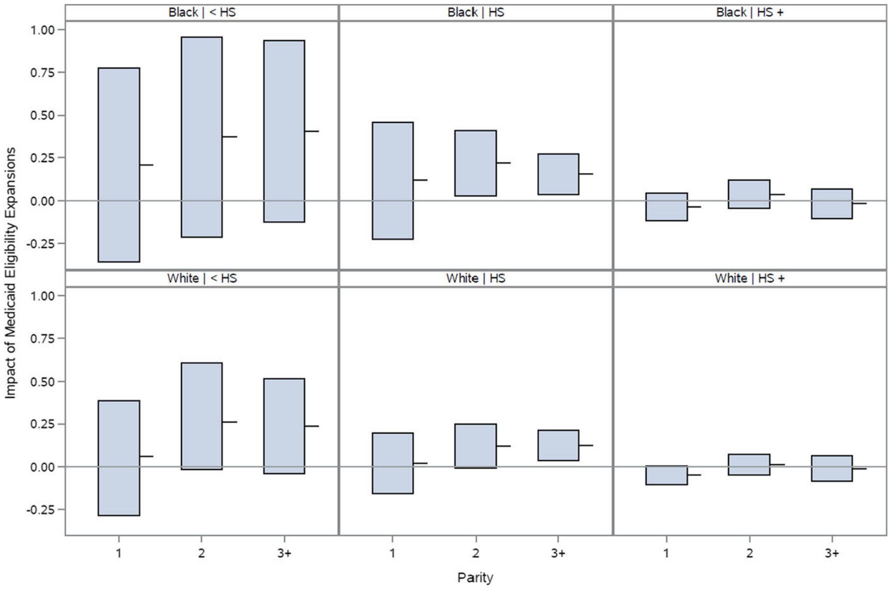


Fig. 4 Impact of Medicaid eligibility expansions on births: modeling by race, educational attainment, and parity| 1987–1997

and (b) a pronatalist effect of Medicaid on higher-parity births among high school graduates and possibly high school dropouts.

Robustness

Marital Status

Having established a relationship between Medicaid eligibility and fertility, we next want to determine if that relationship is the same for married and unmarried women, keeping in mind that marriage may be endogenous with fertility. Given that Medicaid is a program targeting the low-income population, if we are truly capturing Medicaid effects, we would expect them to surface principally among unmarried women. Figure 5 shows similar models where births are disaggregated by marital status for African American women, and Fig. 6 reports the same results for white women. (Both are presented in numerical form in Appendix ESM Tables 3 and 4.)

Among black women, we find statistically insignificant estimates along the extensive margin regardless of education or marital status. Consistent with expectations, however, unmarried women who have not attended college have large, positive coefficients along the intensive margin; these are statistically significant again only for high school graduates due to narrower confidence bands. For second births, a 10-%-of-FPL increase in the income threshold is associated with a nearly 2% increase in the number of births, and for third (or higher) births the estimate is smaller but still statistically significant. Estimated effects for married women, in contrast, are uninformative; they are not statistically different from zero, but some are larger than the estimates for the unmarried, so we simply cannot draw strong conclusions for married black women with a high school education.

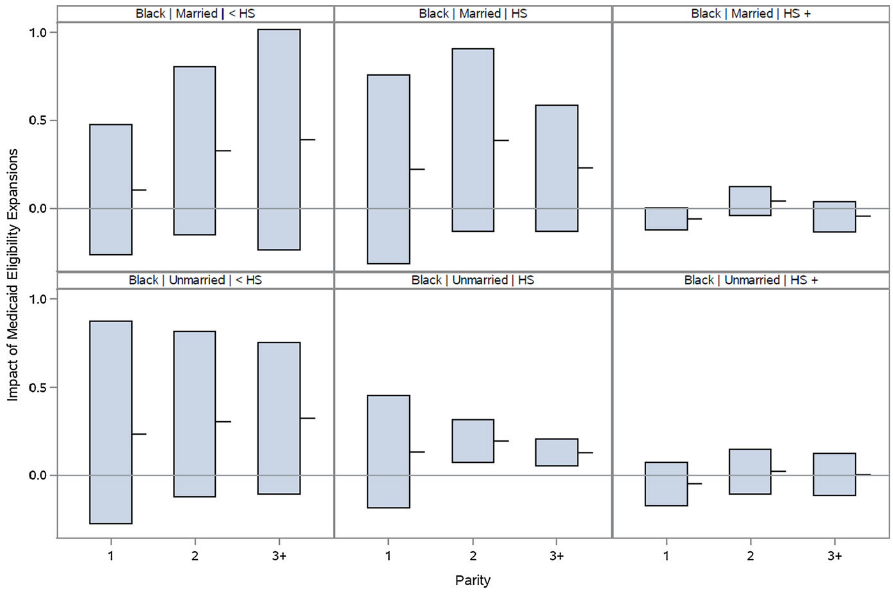


Fig. 5 Impact of Medicaid eligibility expansions on births for black women: modeling by marital status, educational attainment, and parity| 1987–1997

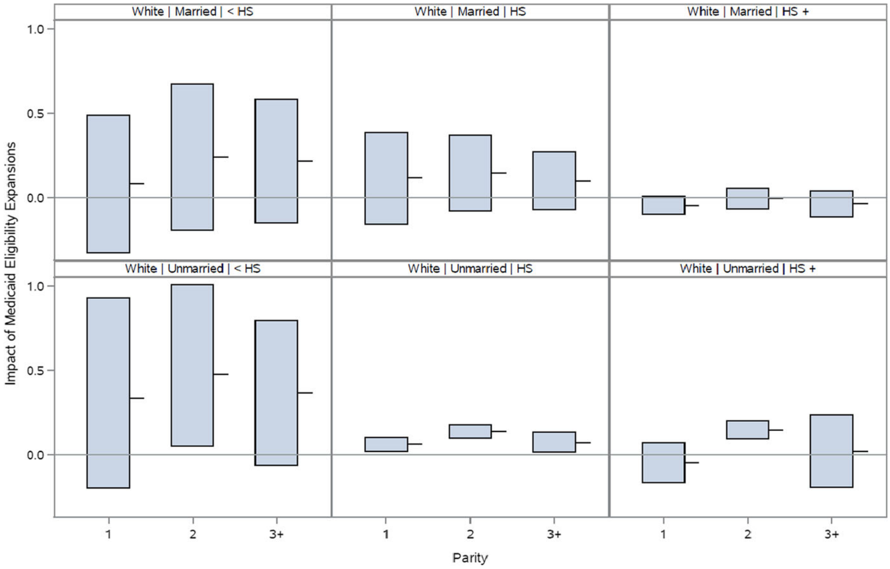


Fig. 6 Impact of Medicaid eligibility expansions on births for white women: modeling by marital status, educational attainment, and parity| 1987–1997

For white women, we see a similar pattern in that unmarried women seem most affected by Medicaid eligibility. Higher-parity births for unmarried women seem to rise both for women without a high school degree (marginally statistically significant changes of 3.7–4.8% for a 10%-of-FPL increase in the income eligibility threshold) and for women with a high school degree (strongly statistically significant changes of 0.8 to 1.4%). In fact, for the first time, we observe a statistically significant (though small) positive estimate even on the extensive margin, though only for high school graduates. Interestingly, we also observe a single positive and statistically significant relationship for unwed white women who have attended college—it appears only for the second birth. It is possible that among unmarried women, even those who have some college may find themselves eligible for Medicaid and thus influenced by its incentives. None of the other estimates for this group is, however, statistically significant. The married women in every education group again have less precise estimates, though most are fairly similar to the unmarried. In summary, while we do see some evidence of pronatalist effects for Medicaid across race and education categories, our most robust findings occur among unmarried women with less education in both racial groups.

Length of Time Series

Based on the length of our time series, we have three immediate concerns that we address simultaneously. First, several demographic factors were changing at the same time Medicaid coverage was expanding, such as an increase in the median age of first marriage for men and women and an increase in the mean age of women giving birth (U.S. Bureau of the Census nd; Mathews and Hamilton 2002). These demographic factors could potentially explain our findings, i.e., investigating fertility over a long time horizon potentially conflates a number of issues simultaneously. For example, if consecutive cohorts of women have different age-specific fertility rates, then we might identify correlations between fertility patterns and Medicaid expansions that are specious.

Second, a close examination of Fig. 1 clearly illustrates that nearly all of the variation in the income eligibility thresholds occurred prior to 1993. In fact, only 11 states had any variation after 1992, and for most of those states, they changed in only one year and often very little. If our result is being driven by the 1993 to 1997 period, then our claims of Medicaid driving the fertility responses are less believable.

Third, Kearney and Levine (2009) find evidence that state-level Medicaid family planning waivers that began in December of 1993 had a significant negative influence on fertility. The effect of these family planning waivers, therefore, may be muting the pronatalist effects of the program making it harder to discern the fertility impacts.

One way to address all of these potential problems is to reduce our analytic sample to the first half of the panel, i.e., to analyze data from 1987 to 1992 only. Because the time series is shorter, the demographics should not have changed appreciably. Further, this isolates the portion of the panel with the vast majority of the variation in the income thresholds—and occurs prior to the family planning waivers.

In the first column of Table 3, we replicate the models reported in Fig. 5 for African Americans, and in the third column, we replicate the results reported in Fig. 6 for whites. In the second column of each panel, we show the same models using the 1987 to 1992 period of the time series. We hypothesize that if the point estimates are essentially the same between 1987 and 1992 as for the full time series, while the standard errors may increase, then these three potential issues are not driving our results. We concentrate our attention on women with a high school education since it is among this group that we have the most robust results.

In Table 3, we show that, among African American women, the findings for the full sample are similar to those reported using the shorter time series. The only statistically significant results are among the unmarried high school graduates. For first births, the point-estimate is positive and insignificant for both the full and limited samples. For higher-parity births, the point estimates are positive and statistically significant for both the full and limited samples, and the estimate for 3+ births in particular is almost identical across samples.

Among white women, again, we see that the results for the full sample and the 1987 to 1992 period are largely the same, with statistically significant results primarily among the unmarried women with a high school diploma. Among high school graduates, we continue to find large, statistically significant intensive-margin effects in the limited sample, though some effect sizes are more muted than in the full sample. Collectively, we conclude that neither a demographic change nor family planning waivers are the source of our results.

Table 3 Robustness checks—tests for compositional changes by race, marital status, educational attainment, and parity

Group	Medicaid threshold X birth parity	African American		White	
		Core model: 1987–1997	Limited: 1987–1992	Core model: 1987–1997	Limited: 1987–1992
Married, Less than high school	1st Birth	0.104	0.153	0.082	0.127
		[0.188]	[0.202]	[0.208]	[0.173]
	2nd Birth	0.327	0.303	0.239	0.222
		[0.243]	[0.240]	[0.221]	[0.215]
	3rd + Birth	0.390	0.362	0.216	0.211
		[0.318]	[0.296]	[0.187]	[0.198]
	Sample size	10,062	5382	13,158	7038
Married, high school diploma	1st Birth	0.222	0.238	0.115	0.131
		[0.273]	[0.269]	[0.139]	[0.144]
	2nd Birth	0.388	0.328	0.147	0.123
		[0.263]	[0.256]	[0.114]	[0.119]
	3rd + Birth	0.229	0.211	0.102	0.097
		[0.182]	[0.167]	[0.087]	[0.090]
	Sample size	11,610	6210	13,158	7038
Married, high school plus	1st Birth	− 0.058†	− 0.039	− 0.046	− 0.034
		[0.032]	[0.028]	[0.027]	[0.028]
	2nd Birth	0.042	0.004	− 0.004	− 0.033
		[0.041]	[0.040]	[0.032]	[0.033]
	3rd + Birth	− 0.047	− 0.040	− 0.037	− 0.033
		[0.044]	[0.049]	[0.038]	[0.040]
	Sample size	12,384	6624	13,158	7038
Unmarried, less than high school	1st Birth	0.235	0.241	0.335	0.321
		[0.325]	[0.333]	[0.303]	[0.294]
	2nd Birth	0.306	0.264	0.478†	0.409
		[0.260]	[0.263]	[0.271]	[0.283]
	3rd + Birth	0.323	0.306	0.366†	0.340
		[0.219]	[0.206]	[0.219]	[0.231]
	Sample size	11,008	5888	13,158	7038
Unmarried, high school diploma	1st Birth	0.134	0.098	0.062**	0.039
		[0.163]	[0.154]	[0.022]	[0.025]
	2nd Birth	0.194**	0.107†	0.138***	0.076**
		[0.062]	[0.061]	[0.020]	[0.024]
	3rd + Birth	0.129**	0.132**	0.075*	0.085**
		[0.038]	[0.040]	[0.030]	[0.032]
	Sample size	11,739	6279	13,115	7015

Table 3 continued

Group	Medicaid threshold X birth parity	African American		White	
		Core model: 1987–1997	Limited: 1987–1992	Core model: 1987–1997	Limited: 1987–1992
Unmarried, high school plus	1st Birth	− 0.048	− 0.084	− 0.046	− 0.045
		[0.062]	[0.069]	[0.060]	[0.060]
	2nd Birth	0.023	− 0.026	0.148***	0.052†
		[0.065]	[0.064]	[0.028]	[0.031]
	3rd + Birth	0.005	0.019	0.021	0.005
		[0.060]	[0.051]	[0.109]	[0.118]
	Sample size	11,997	6417	13,158	7038

The outcome variable is modeled using natural log of births and regressions are weighted by population of women in each racial subgroup. High School and regressions are weighted by the population of women in each racial subgroup. Models include state, year, quarter, state-year, and state-cell fixed effects. All models also include controls for state unemployment rates, maximum cash welfare benefits for a family of three (AFDC or TANF), the natural logarithm of the state population for each racial subgroup, and indicators for family cap provisions, time-limit welfare waivers, the implementation of TANF, and state-level restrictions on the use of Medicaid funds for abortions. Standard errors are clustered at the state-level and are in brackets with statistical significance indicated as follows: $^{\dagger}p < 0.1$, $*p < 0.05$, $**p < 0.01$, $***p < 0.001$

Policy Endogeneity

Policy endogeneity is also a concern in this study. If states expanded Medicaid in response to higher-parity births in the state, then one should worry that our conclusion is misleading. We have the advantage, however, that many of the changes made by states were in response to a federal mandate rather than idiosyncratic state decisions correlated with their other characteristics. While the states that made changes to the highest threshold levels went above and beyond the mandates, it seems reasonable that the less generous states simply responded to federal expansion mandates. In Table 4, we report results restricting the sample to those states with the least generous Medicaid eligibility thresholds in 1987, the first year in our data series, measured as the states at 60% or less of the FPL, again, keeping a sample of states whose behavior is more likely to be primarily driven by mandates—with some variation in the timing of rollout. States like California, Massachusetts, and Wisconsin, which had eligibility thresholds that were much more generous than the federal requirements are removed in his analysis.[19]

While the standard errors are larger in Table 4 relative to the results using the full state sample, the point estimates are qualitatively similar in most instances. Given the reduction in sample size, we were expecting some loss of precision. This test, therefore, suggests that policy endogeneity is not a large source of bias in our estimates.

[19] Table 4 should be compared to Tables A3 and A4 in the Appendix.

Table 4 Robustness checks |states with thresholds at or below 60% of FPL in 1987 modeling by race, marital status, educational attainment, and parity| 1987–1997

Educational attainment	Medicaid threshold X birth parity	Black		White	
		Married	Unmarried	Married	Unmarried
Less than high school	1st Birth	0.268	0.385	0.228	0.678
		[0.260]	[0.531]	[0.346]	[0.568]
	2nd Birth	0.442	0.406	0.403	0.781
		[0.370]	[0.447]	[0.368]	[0.529]
	3rd + Birth	0.529	0.360	0.385	0.616
		[0.477]	[0.387]	[0.316]	[0.461]
	Sample size	5891	6149	7482	7482
High school diploma	1st Birth	0.438	0.251	0.255	$0.071^{\dagger}$
		[0.410]	[0.266]	[0.260]	[0.038]
	2nd Birth	0.535	$0.224^{\dagger}$	0.271	0.139***
		[0.410]	[0.113]	[0.218]	[0.035]
	3rd + Birth	0.325	0.121	0.209	0.022
		[0.301]	[0.079]	[0.175]	[0.043]
	Sample Size	6579	6579	7482	7482
High school plus	1st Birth	− 0.073	− 0.054	− 0.080	− 0.162
		[0.057]	[0.130]	[0.065]	[0.141]
	2nd Birth	0.012	− 0.019	− 0.050	$0.084^{\dagger}$
		[0.072]	[0.129]	[0.071]	[0.048]
	3rd + Birth	− 0.085	− 0.068	− 0.094	− 0.159
		[0.076]	[0.113]	[0.083]	[0.220]
	Sample size	6923	6665	7482	7482

The states with thresholds at or less than 60% of FPL in 1987 were as follows: Alabama, Arizona, Colorado, Delaware, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Kansas, Kentucky, Louisiana, Mississippi, Missouri, Montana, Nebraska, Nevada, New Mexico, North Carolina, North Dakota, Ohio, Oklahoma, Pennsylvania, South Carolina, South Dakota, Texas, Virginia, and Wyoming

The outcome variable is modeled using the natural log of births and regressions are weighted by the population of women in each racial subgroup. Models include state, year, quarter, state-year, and state-cell fixed effects. All models also include controls for state unemployment rates, maximum cash welfare benefits for a family of three (AFDC or TANF), the natural logarithm of the state population for each racial subgroup, and indicators for family cap provisions, time-limit welfare waivers, the implementation of TANF, and state-level restrictions on the use of Medicaid funds for abortions. Standard errors are clustered at the state-level and are in brackets with statistical significance indicated as follows: $^{\dagger}p < 0.1$, $*p < 0.05$, $**p < 0.01$, $***p < 0.001$

Discussion and Conclusions

Recent expansions of publicly provided health insurance have largely focused on healthcare access (see e.g., Antwi et al. 2015). However, there is an extensive literature that investigates the unintended consequences of social policy changes for fertility. This paper asks if expansions of the Medicaid program during the mid-

1980s to the mid-1990s altered the fertility of U.S. women. It builds on a literature that has shown little evidence of a Medicaid effect using a demographic cell-based analysis with Medicaid eligibility measured using state income eligibility thresholds. This inquiry also distinguishes between fertility on the intensive and extensive margins recognizing that the incentives created by Medicaid enrollment are complicated. In other words, we explore whether the expansions of public health care affected the distinct decisions to become a mother (i.e., the extensive margin) or have additional children (i.e., the intensive margin).

Our results show that there is a difference in the Medicaid effect on the intensive compared to the extensive margin for unmarried women. Rarely do we find a significant estimate along the extensive margin. However, along the intensive margin, we estimate that a 10-%-of-FPL increase in Medicaid thresholds is associated with increases in higher-order fertility for African American high school graduates ranging between 1.6 and 2.2%. For white high school graduates, the estimates are around 1.2%.

When we further break these numbers down by marital status (which is potentially endogenous), we find that among unmarried, African American and white women with a high school education only, we generally see no Medicaid effect on the extensive margin. However, we do see positive and statistically significant estimates on the intensive margin. For unmarried African American women, we find that a 10-%-of-FPL increase in the eligibility threshold is associated with between a 1.3 and 1.9% increase in higher-order births, whereas similar white women experienced an increase of 0.8–1.4% in response to such an expansion.

For a back of the envelope calculation of the effect of this policy, in 1990 there were 4,158,212 births in the United States of which 1,330,852 were second births and 1,138,242 were parity 3 or higher.[20] Assume the mid-range of our point estimates for the effect of this policy for our calculations, approximately 0.15% for second births as well as those at parity three or higher. In addition, assume a 100% of FPL increase in the Medicaid threshold for the year, which is a bit high, but convenient. In 1990, 28% of all births were nonmarital (Ventura and Bachrach 2000), and about 35% were to high school graduates in 1994 (Mathews and Ventura 1997). Under these circumstances, we would expect 19,563 more second births to unmarried, high school graduates and 16,732 parity three or higher births for this group, or a total of around 36,300 more births for that year. As a point of comparison, Levine et al. (1999) suggest that the complete recriminalization of abortion would produce an additional 440,000 births annually.

While we do sometimes observe statistically significant results for some other groups (white women with a college education for example), the results for these other groups are not robust across a number of specifications and time periods. Interestingly, our point estimates for women with less than a high school education are the largest of all education groups, which is consistent with our hypothesis, but are never statistically significant. While we cannot be certain, one potential

[20] Data come from CDC Public Use Data Tape Documentation—available online @ ftp://ftp.cdc.gov/pub/Health_Statistics/NCHS/Dataset_Documentation/DVS/natality/Nat1990doc.pdf.

explanation for the insignificance of this set of results is the relatively small size of the high school dropout population. With weights, this group represents about 17% of our sample, compared to about 37% for high school graduates. In general, our findings suggest that the Medicaid program is pronatalist, but is probably not increasing the number of new mothers; the program instead appears to be affecting primarily the higher-parity births among African American and white women with a high school education.

One of the weaknesses in the literature on the fertility effects of social policy changes is an inability for researchers to distinguish between what demographers call tempo and quantum effects. Past findings show that some social policies will influence fertility, but it is nearly impossible to discern if the policy is simply changing when the individual has his/her children (tempo) or if the policy is altering the completed number of children (quantum). Our research suffers from the same limitation. It is conceivable that these Medicaid expansions are altering the timing of a third child among unmarried, white women with a high school education, for example. In other words, in the absence of the program, these same women may have had a third child. Medicaid just made it advantageous to have this child following an expansion. However, because this research actually investigates parity, we are in a position to say more than most previous work. The program is not changing the likelihood that an individual becomes a parent. The cost savings may not be enough to induce would-be parents into having a child. In addition, it is interesting that Medicaid expansions are positively influencing fertility at higher parities. If these births are altering quantum, then the fertility effects of the program are generating larger families but not generating more parents.

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