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

JEL No. I1, J13, J18

Keywords: Medicaid, Fertility, Parity

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I. 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 percent of all births in the United States were principally financed by the program, up from 15 percent in 1985 (Singh, Gold, and Frost 1994; Markus et al. 2013). The Affordable Care Act (ACA) has extended coverage to even more low-income mothers and children (Kaiser Family Foundation 2014). While ostensibly a program 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 $4,334 in 1989 (Health Insurance Association of America, 1989) - which is roughly $8,500 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). However, it is possible that previous attempts to isolate these fertility effects 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 4th child, i.e., the “intensive margin.” Utilizing the natural experiment created by the federally 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, Lopoo, and Simon (2011) as a conceptual starting point for our empirical modeling in this paper. We further incorporate the work of Hamersma and Kim (2013) who argue that use of Medicaid state income eligibility thresholds directly reduces mismeasurement and, thus, bias, which could be caused by imputing eligibility. 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 as robust across specifications.

II. 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, Hao and Cherlin 2004, Hoynes 1997, Kaestner et al. 2003, Kearney 2004, and Lopoo and DeLeire 2006). However, researchers have also studied links between fertility and Child Support Enforcement (see e.g., Garfinkel et al. 2003 and Plotnick et al. 2004) and the Supplemental Nutrition Assistance Program (Almond, Hoynes, and Schanzenbach 2011). Lopoo and Raissian (2012, 2014) summarize this literature and find mixed evidence of a fertility effect resulting from social welfare policies. In some instances, 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 percent more likely to have a child during the experimental period than women in the control group who had a cost-sharing plan (Leibowitz 1990).[1] Consistent with much of this literature, Leibowitz writes that the observed differences are most likely a change in the timing of childbearing, or what 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, there have been three articles asking if Medicaid expansions are related to fertility. Joyce, Kaestner, and Kwan (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 percent increase in births among white women and no change among African American women.

[1] 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 $1,000 for their insurance coverage.

Zavodny and Bitler (2010) and DeLeire, Lopoo, and Simon (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.[2] As we explained earlier, these measures treat births the same regardless of parity, conflating changes in fertility on the extensive and intensive margins.[3] Given the potential differences by parity, our research separately models first, second, third, and fourth (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 certificate data, we are able to estimate

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

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

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 for women with low skill levels.

III. 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, women who become pregnant unintentionally may also become more likely to give birth rather than have an abortion due to the reduced childbirth costs incurred by insured adults (Zavodny and Bitler 2010). At the same time, it is plausible, particularly if the expansions improved child health, that parents would opt to have fewer children based on the quality-quantity trade-off parents face (Becker and Lewis 1973) because access to care would require fewer pregnancies to obtain the same number of healthy (or "high quality") adult children.

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, Lange, and Mazumder (2014), who investigated the link between school costs and family size among African American families, demonstrates that cost changes can affect the extensive and intensive margins

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

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 gain many potential parents expect from entering parenthood may be so large that small cost savings from the Temporary Assistance for Needy Families program or Medicaid, for example, do not affect their fertility decision on the extensive margin appreciably. If, however, there are diminishing marginal returns to parenthood for each birth, 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 differences on the intensive and extensive margins caused by policy change.

IV. 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.[5] 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

[5] 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.

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.[6] 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. Table 1 illustrates the average income thresholds facing pregnant women and infants, as a percent of the Federal Poverty Level, from 1986 to 1997.[7]

Table 1

Average Income Thresholds for Pregnant Women - Percent of FPL

State	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997
Alabama	16	15	57	100	125	133	133	133	133	133	133	133
Alaska	78	77	77	100	125	133	133	133	133	133	133	133
Arizona	39	38	100	100	127	140	140	140	140	140	140	140
Arkansas	36	65	98	100	125	159	150	133	133	133	133	133
California	112	110	105	146	185	185	185	185	185	199	200	200
Colorado	46	45	44	59	119	133	133	133	133	133	133	133
Connecticut	82	81	94	185	185	185	185	185	185	185	185	185
Delaware	39	40	100	100	100	145	166	185	185	185	185	185
Dist. of Columbia	64	91	100	100	142	185	185	185	185	185	185	185

[6] 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.

[7] 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.

State	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997
Florida	47	60	100	125	150	150	173	185	185	185	185	185
Georgia	46	45	43	100	125	133	133	168	185	185	185	185
Hawaii	56	55	55	100	185	185	185	185	185	185	185	185
Idaho	40	39	38	71	119	133	133	133	133	133	133	133
Illinois	60	59	78	100	125	133	133	133	133	133	133	133
Indiana	34	33	43	75	125	142	150	150	150	150	150	150
Iowa	67	66	63	167	185	185	185	185	185	185	185	185
Kansas	61	60	79	125	150	150	150	150	150	150	150	150
Kentucky	35	51	106	125	157	185	185	185	185	185	185	185
Louisiana	34	33	32	100	125	133	133	133	133	133	133	133
Maine	71	70	97	185	185	185	185	185	185	185	185	185
Maryland	55	77	100	143	185	185	185	185	185	185	185	185
Massachusetts	96	97	142	185	185	185	185	185	185	185	185	185
Michigan	71	69	121	185	185	185	185	185	185	185	185	185
Minnesota	93	91	136	185	185	185	185	230	275	275	275	275
Mississippi	16	37	142	185	185	185	185	185	185	185	185	185
Missouri	36	36	100	100	125	133	133	159	185	185	185	185
Montana	53	52	50	74	125	133	133	133	133	133	133	133
Nebraska	59	58	78	100	125	133	133	133	133	140	150	150
Nevada	38	37	40	57	119	133	133	133	133	133	133	133
New Hampshire	71	70	67	72	118	133	141	160	170	176	185	185
New Jersey	74	87	100	100	100	151	185	185	185	185	185	185
New Mexico	34	33	100	100	125	159	185	185	185	185	185	185
New York	81	80	76	85	185	185	185	185	185	185	185	185
North Carolina	46	59	100	100	159	185	185	185	185	185	185	185
North Dakota	57	56	54	63	118	133	133	133	133	133	133	133
Ohio	40	39	38	100	125	133	133	133	133	133	133	133
Oklahoma	55	54	100	100	125	133	146	150	150	150	150	150
Oregon	72	73	90	92	121	133	133	133	133	133	133	133
Pennsylvania	56	55	88	100	125	133	133	185	185	185	185	185
Rhode Island	87	96	121	185	185	185	185	185	185	245	250	250
South Carolina	26	44	100	143	185	185	185	185	185	185	185	185
South Dakota	43	47	73	100	125	133	133	133	133	133	133	133

State	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997
Tennessee	29	64	100	100	150	168	185	185	185	185	185	185
Texas	35	34	55	110	132	137	185	185	185	185	185	185
Utah	66	65	62	100	125	133	133	133	133	133	133	133
Vermont	81	85	143	185	185	185	192	200	200	200	200	200
Virginia	47	46	72	100	125	133	133	133	133	133	133	133
Washington	75	82	90	138	185	185	185	185	185	185	185	185
West Virginia	38	69	125	150	150	150	150	150	150	150	150	150
Wisconsin	91	89	85	82	130	155	155	155	155	168	185	185
Wyoming	47	46	58	100	125	133	133	133	133	133	133	133

Note: Married women became fully eligible for Medicaid on July 1, 1986. This date is used as the starting point for the analysis because it represents the start of the period where all women can receive Medicaid. Before this point, average eligibility would be calculated as some weighted function of single and married women, where the "poverty threshold" for married women is effectively 0%.

Thresholds in 1986 ranged from 16 percent of FPL (Alabama), effectively barring nearly everyone with income from public health insurance coverage, to 112 percent of FPL (California), already above the first federally mandated level of 100 percent of FPL. By 1990, all states met or exceeded 100 percent FPL, and soon all states exceeded 133 percent FPL (the subsequent mandated level) and many moved to 185 percent. 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.

V. 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.[8] Moreover, since the early Vital Statistics data contains 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 2, we use race, age, parity, education, and marital status to define our demographic cells.

Table 2

Demographic Cell Construction

Race	Age	Parity Groups	Education	Marital Status
Black White	20 to 34 35 to 44	1st Birth 2nd Birth 3rd Birth 4th+ Birth	< High School High School Diploma Some College College Plus	Unmarried Married
[2 groups]	[2 groups]	[4 groups]	[4 groups]	[2 groups]

We selected the groups in Table 2 for several reasons, perhaps most importantly to create demographic cells with variation in the number of births. Cutting the cells too finely generated a large number of cells with zero births, particularly among African Americans. We also selected the two age groups to align with more "traditional" and "at-risk" births for the expectant mothers. Furthermore, by using broad age categories, we mitigate estimation issues which could stem from using smaller age categories when the general age at first conception is likely to be increasing over time. Secondly, the parity groups allow us to examine incremental changes up to the third child, while four or more children

[8] 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.

are combined to make the regression output more tractable. The education levels and marital statuses[9] align with common demographic standards.[10] 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.[11]

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.[12] Please refer to the Appendix 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).[13]

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[9] Initially, we do not separate cells by marital status since it's 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.

[10] 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 online appendix.

[11] 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 3, this implies a maximum number of 43 * (51 * 2 * 2 * 4 * 4 * 2) = 43 * 6528 = 280,704 cells for our 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 births over the entire duration of our analysis. With this restriction, the number of demographic cells declines from 6528 to 5982, yielding a maximum of 257,226 observations.

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

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

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

(1)

$$ln(birth)_{stqc} = \alpha + \beta_1(Medicaid)_{st(q-3)c} + \beta_2(Unemployment\ Rate)_{st(q-3)} + \beta_3(Abortion)_{st(q-3)}$$
$$+ \beta_4 \ln(pop)_{st} + \pi Welfare_{st(q-3)p} + \gamma_s + \delta_t + \vartheta_q + \sigma_c + \varepsilon_{stqc}$$

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 were outlined in Table 2. The key variable of 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. The parameters $\gamma_s, \delta_t, \vartheta_q, and\ \varphi_p$ represent state, year, quarter, and parity 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). Avoiding the imputation of eligibility by simply using the income threshold as a covariate has some advantages. First, it reduces the inevitable measurement error of eligibility determination. When classical, such error generates attenuation bias that can lead to null findings when, in fact, the policy actually impacted behavior. 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-percent-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.

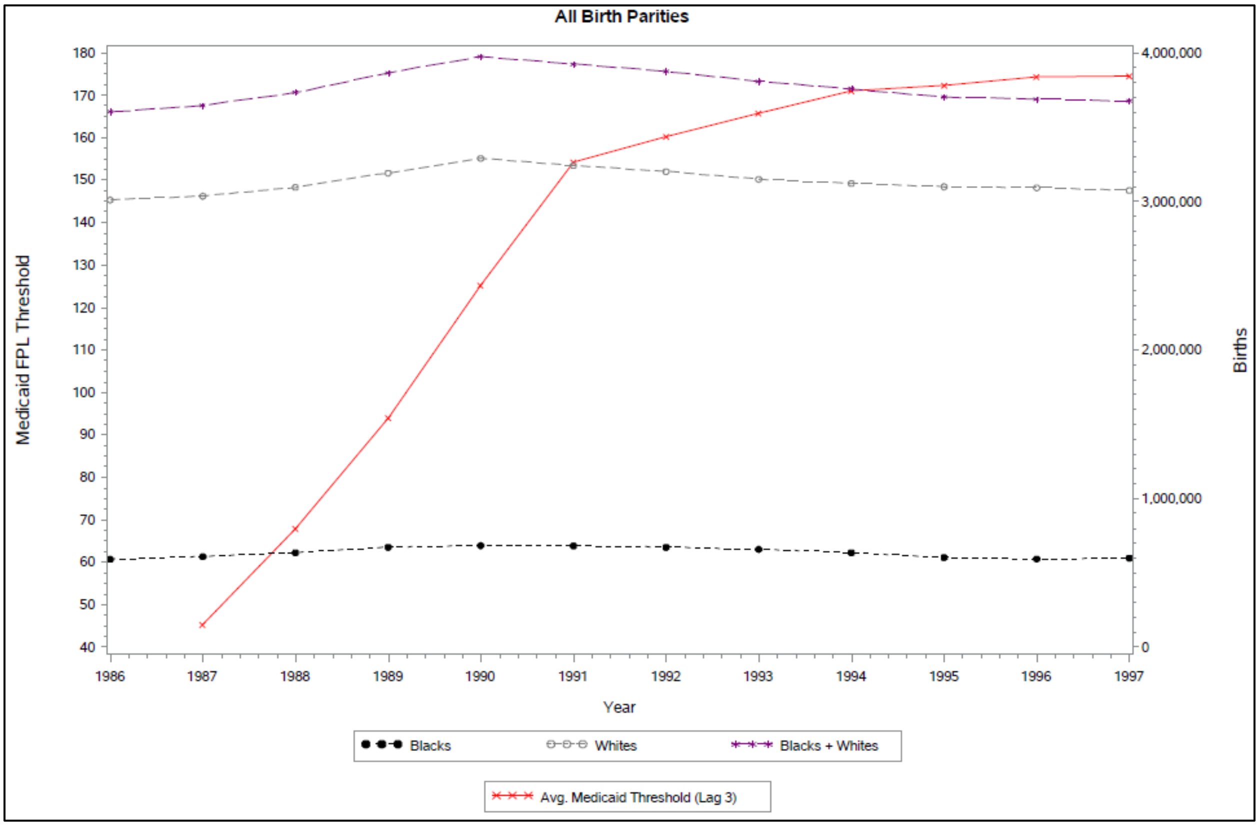
Our key innovation in this study is the addition of an interaction term between the parity fixed effects and the Medicaid threshold, allowing us to discover whether the threshold affects higher-order births differently than first births. After estimating equation 1, we will replace the Medicaid parameter with a vector of four parameters indicating the Medicaid threshold's relationship to first, second, third, and fourth (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.

VI. 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 1 shows the annual number of births for African American and white women combined from 1986 to 1997 - for all birth parities.

Figure 1

Trends in U.S. Births and Access to Medicaid



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

Figure 2

Trends in U.S. Births and Access to Medicaid, by Race

Panel A: African Americans

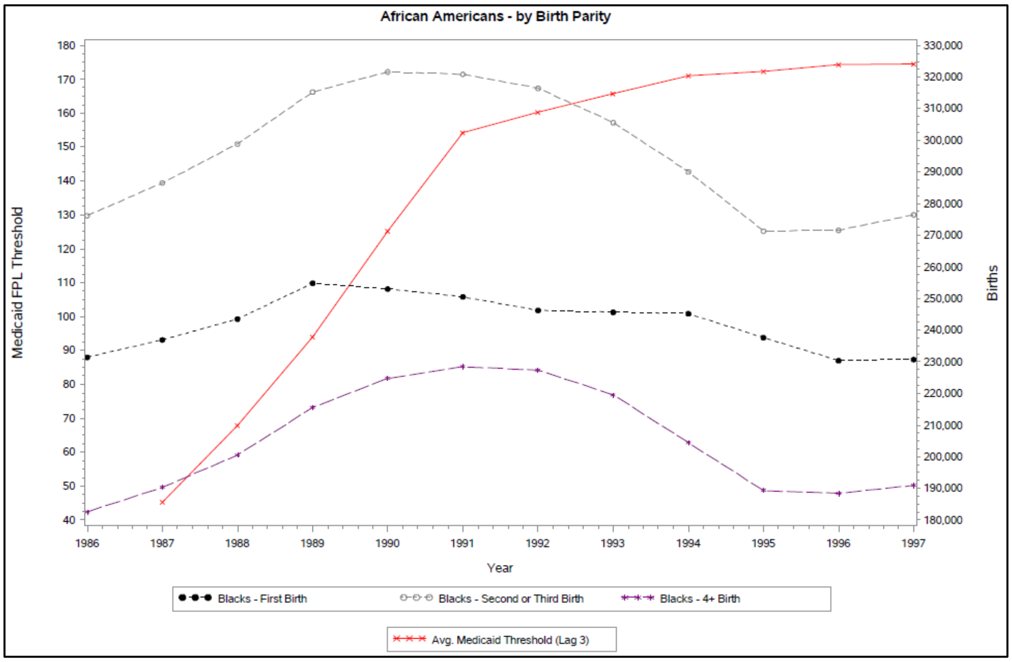
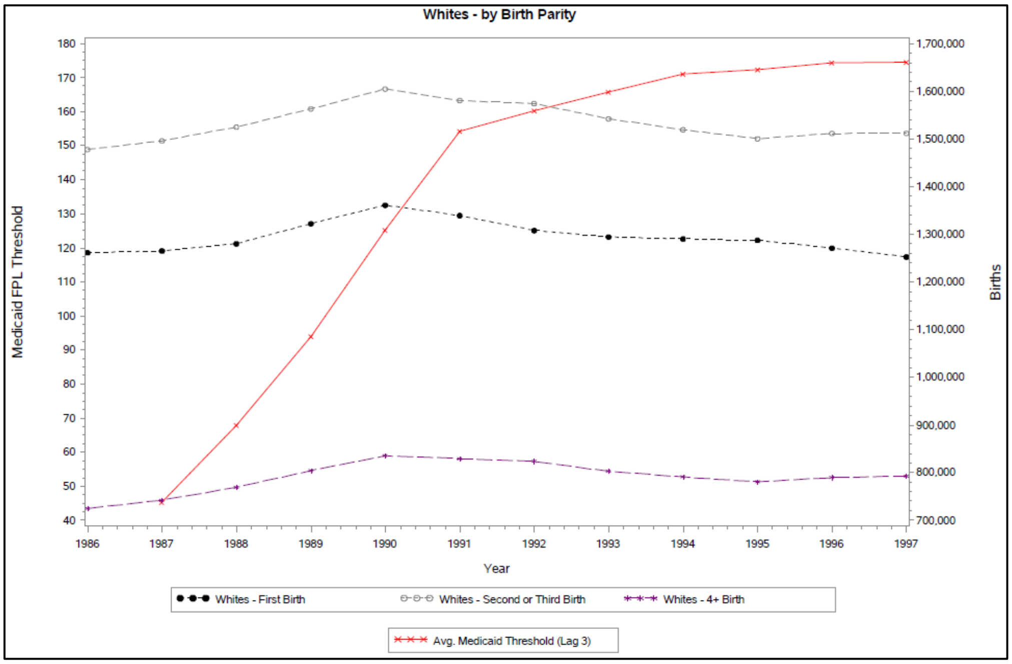


Figure 2 (Continued)

Trends in U.S. Births and Access to Medicaid, by Race

Panel B: Whites



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 less pronouncedly in Figure 2. 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 heterogeneous impacts - especially by birth parity - are what we seek to isolate in this paper.

VII. 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 3 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).

Table 2
Impact of Medicaid Eligibility Expansions on Births
Modeling by Race and Margin | 1987-1997

Medicaid Threshold X Birth Parity	**African American**	**White**
Extensive Margin	-0.0405 [0.0244]	-0.0614*** [0.0120]
Intensive Margin	0.0809*** [0.0127]	0.0713*** [0.0061]
Sample Size	**17,071**	**17,544**

Notes: 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: † $p<0.1$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Of course, this set of results does not account for the importance of socioeconomic status or marriage 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 and certainly points to more substantive models that allow for differences by parity. We turn to those models now.

Table 4 provides our primary estimates for Medicaid eligibility allowing for differences by parity and education level, estimating separate Medicaid effects by subgroup.

Table 4

Impact of Medicaid Eligibility Expansions on Births

Modeling by Race, Educational Attainment, and Parity | 1987-1997

Educational Attainment	**Medicaid Threshold X Birth Parity**	**African American**	**White**
Less than High School	1st Birth	0.2042	0.0748
		[0.2763]	[0.1646]
	2nd Birth	0.3488	0.2332
		[0.2862]	[0.1682]
	3rd Birth	0.2974	0.1715
		[0.2471]	[0.1361]
	4th+ Birth	0.4142	0.2125†
		[0.2536]	[0.1089]
	Sample Size	**14,577**	**17,544**
HS Diploma	1st Birth	0.1066	0.0015
		[0.1677]	[0.0873]
	2nd Birth	0.1981*	0.1135†
		[0.0921]	[0.0631]
	3rd Birth	0.1283†	0.1139*
		[0.0662]	[0.0517]
	4th+ Birth	0.1550***	0.1459***
		[0.0430]	[0.0316]
	Sample Size	**16,168**	**17,544**
Some College	1st Birth	-0.0158	-0.0420†
		[0.0356]	[0.0231]
	2nd Birth	0.0394	0.0126
		[0.0446]	[0.0290]
	3rd Birth	-0.0163	0.0166
		[0.0473]	[0.0268]
	4th+ Birth	0.0005	0.0061
		[0.0482]	[0.0432]
	Sample Size	**16,469**	**17,544**
College Plus	1st Birth	-0.0771	-0.0479
		[0.0484]	[0.0308]
	2nd Birth	0.0279	-0.0088
		[0.0443]	[0.0325]
	3rd Birth	-0.0206	-0.0239
		[0.0335]	[0.0334]
	4th+ Birth	-0.0302	-0.0900
		[0.0469]	[0.0615]
	Sample Size	**15,437**	**17,544**

Notes: 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.

To help illuminate the magnitude and statistical significance, Figure 3 and Figure 4 display the point estimates with the requisite 95% confidence intervals for African American and white women, respectively.

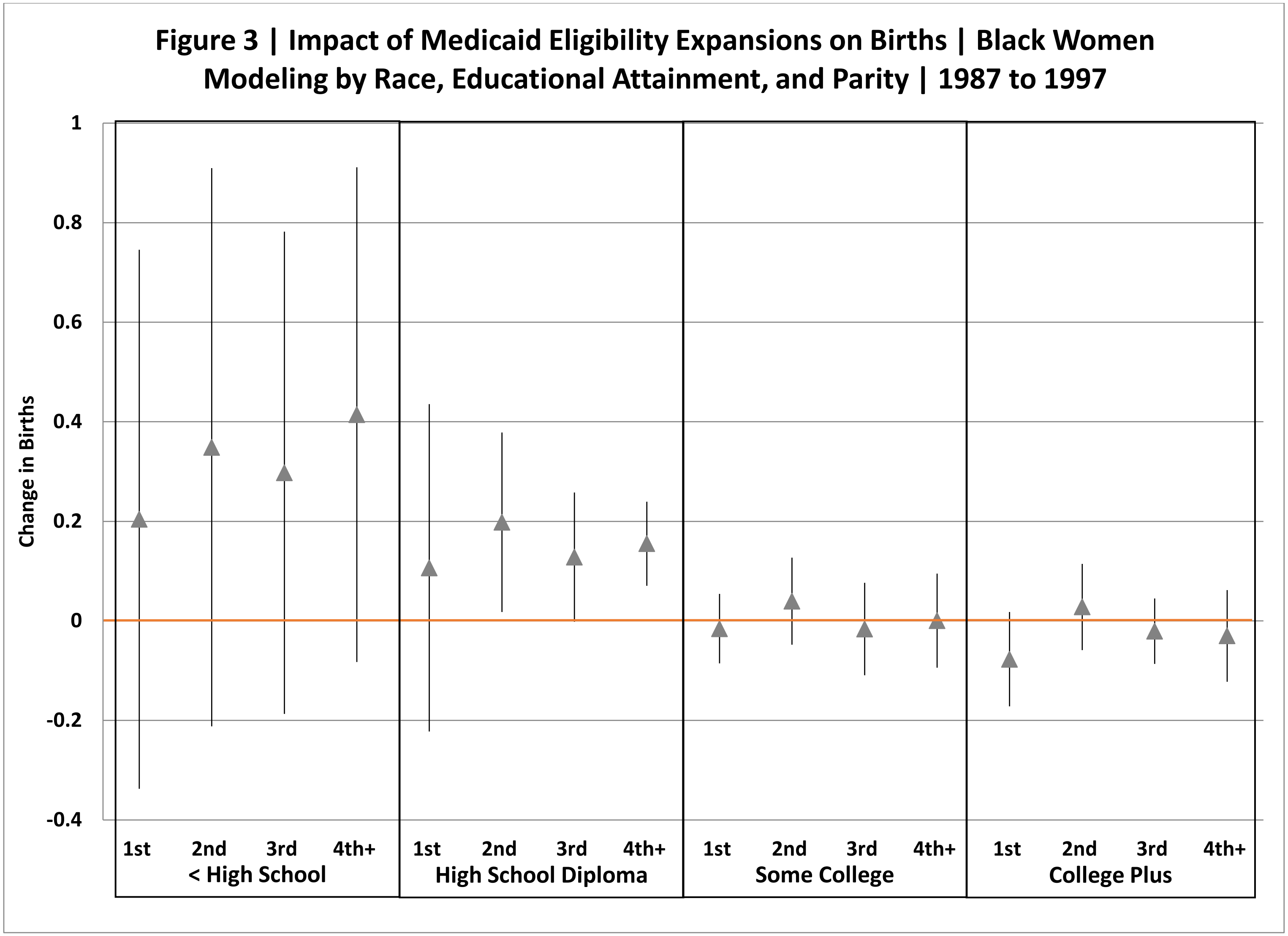


Figure 3 | Impact of Medicaid Eligibility Expansions on Births | Black Women
Modeling by Race, Educational Attainment, and Parity | 1987 to 1997
Change in Births
1
0.8
0.6
0.4
0.2
0
-0.2
-0.4
1st
2nd
3rd
4th+
< High School
High School Diploma
Some College
College Plus

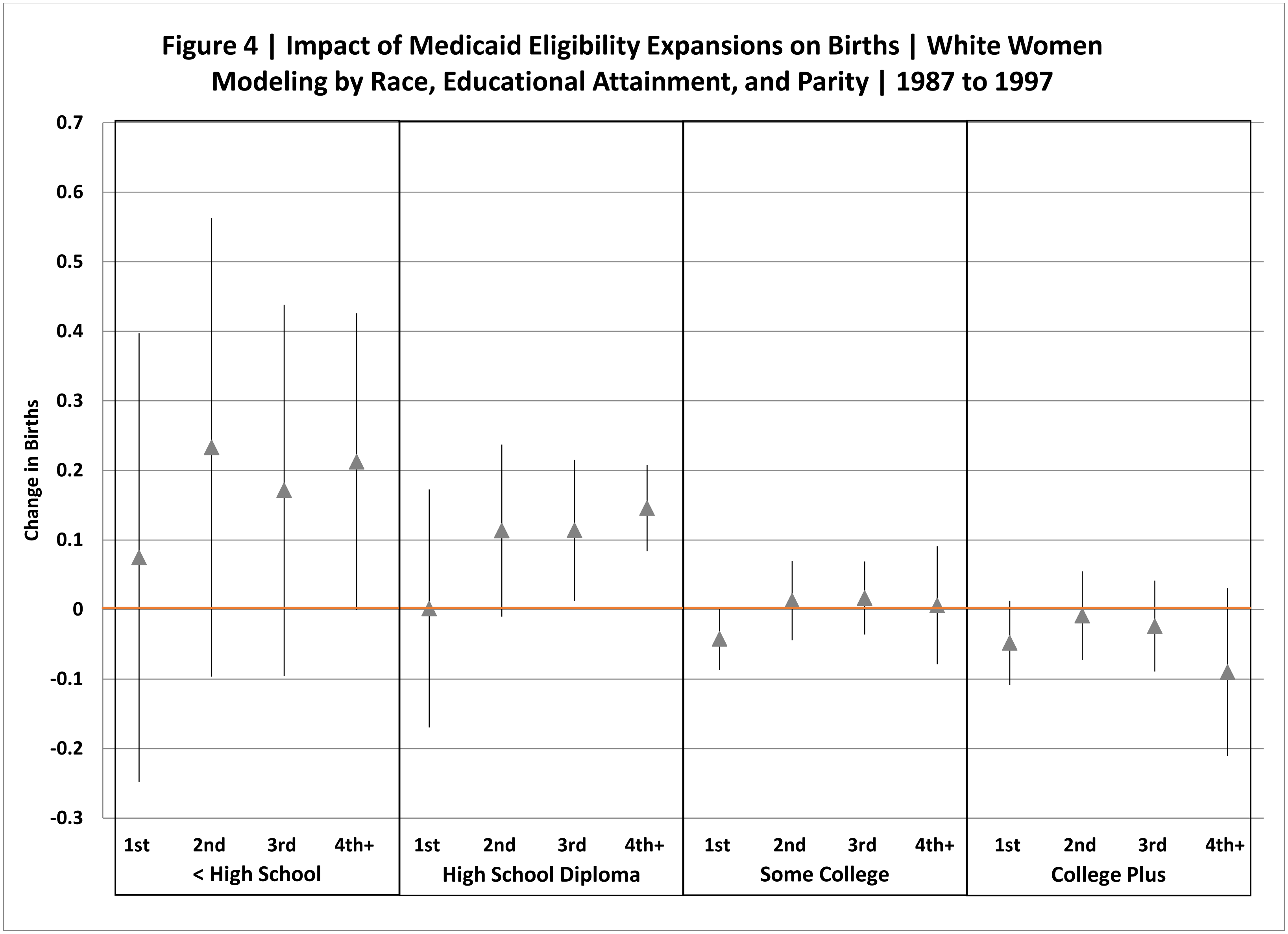


Figure 4 | Impact of Medicaid Eligibility Expansions on Births | White Women
Modeling by Race, Educational Attainment, and Parity | 1987 to 1997
Change in Births
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
-0.1
-0.2
-0.3
1st
2nd
3rd
4th+
< High School
High School Diploma
Some College
College Plus

We estimate fairly large positive coefficients for the Medicaid effect at all parities for women with less than a high school education, but none of these point estimates are statistically significant. Among women with a high school education, we do not find a statistically significant relationship between Medicaid and births on the extensive margin for African American or white women. However, the point estimates are fairly large and significant on the intensive margin. A 10-percent-of-FPL increase in the threshold is associated with a 1.1 to 2 percent increase in births for both African American and white women. 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. Given this set of results, while we report estimates for all education levels for consistency, we concentrate on the results among women with a high school education in our subsequent analyses.

VIII. 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. Table 5 shows similar models where births are disaggregated by marital status for African American women, and Table 6 reports the same results for white women.

Table 5

Impact of Medicaid Eligibility Expansions on Births for Black Women
Modeling by Marital Status, Educational Attainment, and Parity | 1987-1997

Educational Attainment	Medicaid Threshold X Birth Parity	Married	Unmarried
Less than High School	1st Birth	0.0947	0.2211
		[0.1773]	[0.3119]
	2nd Birth	0.3043	0.2755
		[0.2351]	[0.2538]
	3rd Birth	0.2979	0.2176
		[0.2628]	[0.1981]
	4th+ Birth	0.3819	0.3365
		[0.3037]	[0.2050]
	Sample Size	**12,943**	**14,190**

HS Diploma	1st Birth	0.2312	0.1219
		[0.2866]	[0.1529]
	2nd Birth	0.3863	0.1739**
		[0.2742]	[0.0588]
	3rd Birth	0.2853	0.0930*
		[0.2231]	[0.0396]
	4th+ Birth	0.2017	0.1494***
		[0.1637]	[0.0331]
	Sample Size	**15,308**	**15,179**

Some College	1st Birth	-0.0090	-0.0449
		[0.0317]	[0.0599]
	2nd Birth	0.0582	0.0105
		[0.0397]	[0.0684]
	3rd Birth	-0.0141	-0.0213
		[0.0439]	[0.0661]
	4th+ Birth	-0.0545	0.0740
		[0.0437]	[0.0680]
	Sample Size	**15,781**	**15,566**

College Plus	1st Birth	-0.0691	-0.0699
		[0.0428]	[0.0667]
	2nd Birth	-0.0003	0.0610
		[0.0486]	[0.0587]
	3rd Birth	-0.0457	0.0614
		[0.0441]	[0.0654]
	4th+ Birth	-0.0599	0.0039
		[0.0539]	[0.0893]
	Sample Size	**14,792**	**13,502**

Notes: 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.

Table 6

Impact of Medicaid Eligibility Expansions on Births for White Women
Modeling by Marital Status, Educational Attainment, and Parity | 1987-1997

Educational Attainment	**Medicaid Threshold X Birth Parity**	**Married**	**Unmarried**
Less than High School	1st Birth	0.1165	0.3401
		[0.2046]	[0.3044]
	2nd Birth	0.2203	0.4796†
		[0.2117]	[0.2771]
	3rd Birth	0.1490	0.3922
		[0.1795]	[0.2566]
	4th+ Birth	0.2121	0.3577†
		[0.1695]	[0.1931]
	Sample Size	**17,544**	**17,501**
HS Diploma	1st Birth	0.0802	0.0409*
		[0.1339]	[0.0200]
	2nd Birth	0.1401	0.1437***
		[0.1098]	[0.0208]
	3rd Birth	0.1022	0.0931**
		[0.0917]	[0.0321]
	4th+ Birth	0.1328†	0.0533
		[0.0676]	[0.0426]
	Sample Size	**17,544**	**17,458**
Some College	1st Birth	-0.0225	-0.0785
		[0.0193]	[0.0560]
	2nd Birth	0.0022	0.0186
		[0.0293]	[0.0643]
	3rd Birth	-0.0100	0.0303
		[0.0259]	[0.0712]
	4th+ Birth	-0.0176	-0.0263
		[0.0411]	[0.1089]
	Sample Size	**17,544**	**17,458**

Educational Attainment	**Medicaid Threshold X Birth Parity**	**Married**	**Unmarried**

College Plus	1st Birth	-0.0481	-0.0198
		[0.0340]	[0.0613]
	2nd Birth	-0.0263	0.1687**
		[0.0333]	[0.0580]
	3rd Birth	-0.0414	0.2523***
		[0.0339]	[0.0656]
	4th+ Birth	-0.0944	-0.0478
		[0.0616]	[0.2174]
	Sample Size	**17,544**	**17,372**

Notes: 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.

Among black women with a high school education, we find statistically insignificant estimates along the extensive margin regardless of marital status. Consistent with expectations, however, unmarried women have positive coefficients along the intensive margin. For second births, a 10-percent-of-FPL increase in the income threshold is associated with a statistically-significant 1.7 percent increase in the number of births, and for third and fourth (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 are larger than the estimates for the unmarried, so we simply cannot draw strong conclusions for married black women with a high school education.

For white women, we see a similar pattern for unmarried women and for women with a high school level of education. In fact, for the first time, we observe a positive estimate on the extensive margin. The results indicate that a 10 percentage point increase in the income-eligibility threshold is associated with a 0.4 percent increase in first births, a 1.4 percent increase in second births, a 0.9 percent increase in third births, and a 0.5 percent increase in fourth or higher births. With the exception of the highest parity category, all of the estimates are statistically significant. The married women in this education group again have less precise estimates, though most are fairly similar to the unmarried and one is marginally significant. Interestingly, we also observe a positive and statistically significant relationship for unwed white women with a college education. We find that a 10 percentage point increase in the income eligibility threshold is associated with a 1.7 percent increase in second births and a 2.5 percent increase for third births. None of the other estimates is statistically significant. In summary, while we do see some evidence of pronatalist effects for Medicaid across race and education categories, we interpret these results to show that the robust findings occur among unmarried, high school graduates 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 at a point in time 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 Table 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 is 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 positive 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 1987-1992 only. Because the time series is so short, there has not been enough time for the demographics to have changed appreciably. Further, we are using the portion of the panel with the vast majority of the variation in the income thresholds and prior to the family planning waivers.

In the first column of Table 7, we replicate the models reported in Table 5 for African Americans, and in the third column, we replicate the results reported in Table 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.

Table 7

Robustness Checks - Tests for Compositional Changes
by Race, Marital Status, Educational Attainment, and Parity

		African American		***White***	
Group	**Medicaid Threshold X Birth Parity**	**Core Model: All Years**	**Limited: 1987 to 1992**	**Core Model: All Years**	**Limited: 1987 to 1992**
Married, Less than High School	1st Birth	0.0947	0.1220	0.1165	0.1399
		[0.1773]	[0.1853]	[0.2046]	[0.1746]
	2nd Birth	0.3043	0.2842	0.2203	0.2154
		[0.2351]	[0.2377]	[0.2117]	[0.2091]
	3rd Birth	0.2979	0.3006	0.1490	0.1566
		[0.2628]	[0.2590]	[0.1795]	[0.1730]
	4th+ Birth	0.3819	0.3374	0.2121	0.2049
		[0.3037]	[0.2806]	[0.1695]	[0.1908]
	Sample Size	**12,943**	**6,923**	**17,544**	**9,384**
Married, HS Diploma	1st Birth	0.2312	0.2427	0.0802	0.1019
		[0.2866]	[0.2733]	[0.1339]	[0.1383]
	2nd Birth	0.3863	0.3275	0.1401	0.1172
		[0.2742]	[0.2645]	[0.1098]	[0.1143]
	3rd Birth	0.2853	0.2592	0.1022	0.0872
		[0.2231]	[0.2174]	[0.0917]	[0.0931]
	4th+ Birth	0.2017	0.1911	0.1328†	0.1392†
		[0.1637]	[0.1453]	[0.0676]	[0.0730]
	Sample Size	**15,308**	**8,188**	**17,544**	**9,384**

		African American	***White***

Group	Medicaid Threshold X Birth Parity	Core Model: All Years	Limited: 1987 to 1992	Core Model: All Years	Limited: 1987 to 1992
Married, Some College	1st Birth	-0.0090	-0.0021	-0.0225	-0.0099
		[0.0317]	[0.0329]	[0.0193]	[0.0229]
	2nd Birth	0.0582	0.0357	0.0022	-0.0258
		[0.0397]	[0.0420]	[0.0293]	[0.0312]
	3rd Birth	-0.0141	-0.0287	-0.0100	-0.0170
		[0.0439]	[0.0464]	[0.0259]	[0.0282]
	4th+ Birth	-0.0545	-0.0164	-0.0176	0.0071
		[0.0437]	[0.0478]	[0.0411]	[0.0415]
	Sample Size	**15,781**	**8,441**	**17,544**	**9,384**
Married, College Plus	1st Birth	-0.0691	-0.0506	-0.0481	-0.0496
		[0.0428]	[0.0417]	[0.0340]	[0.0344]
	2nd Birth	-0.0003	-0.0208	-0.0263	-0.0399
		[0.0486]	[0.0472]	[0.0333]	[0.0334]
	3rd Birth	-0.0457	-0.0407	-0.0414	-0.0482
		[0.0441]	[0.0477]	[0.0339]	[0.0359]
	4th+ Birth	-0.0599	-0.0335	-0.0944	-0.0490
		[0.0539]	[0.0617]	[0.0616]	[0.0601]
	Sample Size	**14,792**	**7,912**	**17,544**	**9,384**
Unmarried, Less than High School	1st Birth	0.2211	0.2058	0.3401	0.3250
		[0.3119]	[0.3146]	[0.3044]	[0.2958]
	2nd Birth	0.2755	0.2400	0.4796†	0.4137
		[0.2538]	[0.2601]	[0.2771]	[0.2884]
	3rd Birth	0.2176	0.2528	0.3922	0.3676
		[0.1981]	[0.2015]	[0.2566]	[0.2611]
	4th+ Birth	0.3365	0.3050	0.3577†	0.3454†
		[0.2050]	[0.1904]	[0.1931]	[0.2054]
	Sample Size	**14,190**	**7,590**	**17,501**	**9,361**
		African American		***White***	

Group	Medicaid Threshold X Birth Parity	Core Model: All Years	Limited: 1987 to 1992	Core Model: All Years	Limited: 1987 to 1992
Unmarried, HS Diploma	1st Birth	0.1219	0.0791	0.0409*	0.0165
		[0.1529]	[0.1440]	[0.0200]	[0.0245]
	2nd Birth	0.1739**	0.0925	0.1437***	0.0740**
		[0.0588]	[0.0568]	[0.0208]	[0.0246]
	3rd Birth	0.0930*	0.0952*	0.0931**	0.0788*
		[0.0396]	[0.0441]	[0.0321]	[0.0366]
	4th+ Birth	0.1494***	0.1656***	0.0533	0.1110**
		[0.0331]	[0.0343]	[0.0426]	[0.0409]
	Sample Size	**15,179**	**8,119**	**17,458**	**9,338**
Unmarried, Some College	1st Birth	-0.0449	-0.0899	-0.0785	-0.0860
		[0.0599]	[0.0687]	[0.0560]	[0.0548]
	2nd Birth	0.0105	-0.0356	0.0186	-0.0652
		[0.0684]	[0.0671]	[0.0643]	[0.0757]
	3rd Birth	-0.0213	-0.0215	0.0303	-0.0210
		[0.0661]	[0.0674]	[0.0712]	[0.0786]
	4th+ Birth	0.0740	0.0840	-0.0263	0.0235
		[0.0680]	[0.0559]	[0.1089]	[0.0963]
	Sample Size	**15,566**	**8,326**	**17,458**	**9,338**
Unmarried, College Plus	1st Birth	-0.0699	-0.1016	-0.0198	-0.0069
		[0.0667]	[0.0709]	[0.0613]	[0.0642]
	2nd Birth	0.0610	0.0331	0.1687**	0.0890
		[0.0587]	[0.0507]	[0.0580]	[0.0821]
	3rd Birth	0.0614	0.0453	0.2523***	0.1867*
		[0.0654]	[0.1053]	[0.0656]	[0.0748]
	4th+ Birth	0.0039	0.0971	-0.0478	-0.0251
		[0.0893]	[0.0889]	[0.2174]	[0.2461]
	Sample Size	**13,502**	**7,222**	**17,372**	**9,292**

Notes: 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: † $p<0.1$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

In Table 7, 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 second births, the point estimate is positive and statistically significant for the full sample, while positive, smaller, and statistically insignificant for the limited sample. For third births and four (plus) births, the findings are nearly identical.

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 find similar point estimates across the two samples, at each level of parity, reinforcing our finding of larger intensive-margin effects. In the only real exception, we observe a much larger point estimate for the four (plus) parity for the 1987 to 1992 period. Collectively, we conclude that neither a demographic change nor family planning waivers are the source of our results.

IX. Discussion and Conclusions

Recent expansions of publicly provided health insurance have largely focused on healthcare access (see e.g., Antwi, Moriya, and Simon 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. 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-percent-of-FPL increase in the eligibility threshold is associated with between a 0.9 percent and 1.7 percent increase in higher order births, whereas similar white women experienced an increase of 0.9 to 1.4 percent in response to such an expansion. 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. In general, our findings suggests that the Medicaid program is pronatalist, but it is 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 fourth 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 fourth 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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Pregnancy Medicaid Expansions and Fertility

Data Appendix

I. Mother's Educational Status in the Natality Data

The birth certificate data collected by the National Center for Health Statistics is missing the educational level for a significant number of women in the early period. For example, in 1988, 1,093,417 of 3,897,495 births - or roughly 28% of birth certificates - are missing the education level of the mother. Missing data occur because states were not mandated to provide the mother's education status on the birth certificates until 1992. In other words, reporting before this point was voluntary. Thus - and as shown in the table below - missing data on mother's education are highly correlated with living in select states (e.g., California, New York, Texas, and Washington).

State Code	State Name	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997
1	Alabama	0%	0%	1%	1%	1%	0%	0%	0%	0%	0%	0%	0%
2	Alaska	2%	2%	2%	1%	1%	1%	1%	1%	1%	1%	1%	1%
3	Arizona	1%	1%	1%	4%	2%	1%	2%	2%	2%	2%	2%	2%
4	Arkansas	3%	3%	3%	1%	0%	0%	0%	0%	1%	1%	1%	1%
5	California	100%	100%	100%	1%	1%	1%	1%	1%	0%	1%	1%	1%
6	Colorado	1%	0%	0%	1%	1%	2%	3%	3%	2%	1%	1%	1%
7	Connecticut	7%	7%	9%	11%	9%	5%	5%	5%	4%	3%	3%	3%
8	Delaware	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%
9	District of Columbia	1%	1%	1%	0%	1%	2%	4%	6%	5%	6%	7%	9%
10	Florida	1%	1%	0%	1%	1%	0%	0%	0%	0%	0%	0%	0%
11	Georgia	0%	0%	0%	1%	1%	1%	1%	1%	1%	1%	1%	1%
12	Hawaii	0%	0%	1%	0%	0%	0%	0%	0%	0%	0%	0%	0%
13	Idaho	6%	6%	6%	2%	2%	3%	1%	1%	2%	3%	6%	8%
14	Illinois	0%	0%	0%	0%	0%	1%	0%	0%	1%	1%	1%	1%
15	Indiana	1%	1%	1%	1%	0%	0%	0%	1%	1%	1%	1%	1%

State Code	State Name	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997
16	Iowa	0%	0%	0%	0%	0%	0%	0%	0%	0%	1%	2%	1%
17	Kansas	0%	0%	0%	0%	0%	0%	1%	1%	1%	0%	0%	0%
18	Kentucky	0%	1%	1%	1%	0%	0%	0%	0%	0%	0%	0%	0%
19	Louisiana	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%
20	Maine	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	1%
21	Maryland	1%	1%	1%	3%	3%	3%	4%	3%	2%	2%	3%	4%
22	Massachusetts	1%	1%	1%	0%	0%	0%	0%	0%	0%	0%	0%	0%
23	Michigan	0%	0%	0%	0%	0%	1%	1%	1%	1%	1%	1%	1%
24	Minnesota	10%	7%	6%	3%	3%	3%	3%	2%	3%	2%	1%	1%
25	Mississippi	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%
26	Missouri	0%	0%	0%	1%	1%	1%	1%	1%	1%	1%	1%	1%
27	Montana	0%	0%	0%	0%	1%	0%	0%	0%	0%	0%	0%	0%
28	Nebraska	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%
29	Nevada	2%	2%	2%	0%	1%	1%	0%	0%	1%	2%	2%	2%
30	New Hampshire	0%	1%	1%	1%	0%	1%	1%	0%	1%	1%	0%	1%
31	New Jersey	1%	1%	2%	6%	6%	6%	4%	3%	4%	3%	2%	2%
32	New Mexico	7%	6%	6%	3%	3%	3%	2%	2%	2%	3%	3%	4%
33	New York	3%	3%	57%	57%	58%	3%	3%	4%	3%	3%	2%	1%
34	North Carolina	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%
35	North Dakota	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%
36	Ohio	1%	1%	1%	1%	0%	0%	0%	0%	0%	0%	0%	0%
37	Oklahoma	2%	2%	2%	1%	1%	1%	1%	1%	4%	3%	4%	3%
38	Oregon	2%	2%	2%	2%	2%	2%	0%	1%	1%	1%	1%	1%
39	Pennsylvania	1%	1%	2%	1%	1%	1%	1%	2%	2%	2%	2%	2%
40	Rhode Island	1%	2%	2%	1%	1%	0%	1%	1%	1%	2%	2%	2%
41	South Carolina	0%	0%	0%	2%	2%	2%	2%	4%	4%	4%	5%	5%
42	South Dakota	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%
43	Tennessee	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%
44	Texas	100%	100%	100%	25%	1%	1%	1%	1%	1%	1%	1%	1%
45	Utah	1%	1%	1%	0%	1%	1%	1%	1%	1%	1%	1%	1%

State Code	State Name	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997
46	Vermont	1%	1%	1%	1%	3%	2%	2%	2%	2%	2%	2%	2%
47	Virginia	0%	0%	0%	0%	1%	0%	0%	0%	0%	0%	0%	0%
48	Washington	97%	97%	97%	97%	97%	97%	12%	7%	7%	7%	7%	7%
49	West Virginia	0%	1%	1%	0%	0%	0%	0%	1%	0%	0%	0%	0%
50	Wisconsin	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%
51	Wyoming	1%	1%	1%	1%	1%	0%	0%	0%	0%	0%	0%	0%

In our analysis, missing information on the mother's educational attainment is potentially problematic because we use education in defining our demographic cells. More specifically, we use education to proxy social economic status (SES). To solve this empirical challenge, we estimate the expected percentage of women in each educational category (e.g., < HS, HS Diploma, Some College, College +) from a period which has consistently reported values. In this paper, we use the NCHS from 1992 to 1994. We then create an algorithm which randomly assigns all women with missing data to one of the four educational categories, which rebalances the data based upon the expected proportions.[1] A similar approach is used by the Urban Institute in their Transfer Income Model (TRIM). While TRIM seeks to account for the underreporting of use in various social welfare assistance programs in the United States by revising reported use with administrative level data (see http://trim3.urban.org/T3Welcome.php), the example of "missing data" is very similar to the problem we face with the non-reporting of educational data for select states in the early period.

One of the potential limitation of using the internal NCHS data from a later period (e.g., 1992 to 1994) to help us estimate missing education in the earlier period is that the number of observations for a

[1] Unfortunately, multiple imputation techniques cannot be used to address the missing data issues in this analysis, since we aggregate up to demographic cells *before estimating* our regression models.

specific demographic cells (e.g., unmarried Black women in Maine, aged 20-24 with 4+ children and some college) can be prohibitively small. To ensure a sufficient sample size for reliable estimation, we estimate the expected values at one of the four Census Regions rather than at the state level. The rest of this section provides an overview of how we conducted the rest of this calculation.

A. Part I: Using the Pooled NCHS Data from 1992 to 1994

<u>Step 1</u>: Using the most disaggregated level of the data (e.g., data by race, age, parity, marital status, and educational attainment), we estimate the distribution of educational attainment for each demographic cell by Census Region. The average of these values across all of these subsamples is:

Educational Level Estimate	NCHS Population Estimate (92-94)	Educational Level 1 (%)	Educational Level 2 (%)	Educational Level 3 (%)	Educational Level 4 (%)
NCHS: 1992 to 1994 Estimate	9,761,470	17.0%	37.0%	24.0%	22.1%

<u>Step 2</u>: The values derived in Step 1 serve as the *expected educational values* for a given demographic cell. In other words, the summary above displays the proportions we should expect *when the data are rebalanced*. Thus, the next step is to calculate the current distribution, which includes all observations with missing values; additionally, it is estimated across the entire range of the data (1986 to 1997). Essentially, we are seeking to determine if the educational level is above/below the predicted value for a given subgroup. When the cell has fewer people than expected, we will add to it. When it has more than expect, then we will not allocate any of the birth certificates with missing data to that cell.

<u>Step 3</u> is to create a rescaling variable. We define this variable conceptually as the percent of the unassigned sample which needs to be allocated into a category if the combined sample (e.g., identified and unidentified) were to approximate the educational levels reported by the NCHS 1992-1994 data.

This rescaling variable will be used to calculate the break points for single observation allocation for the birth certificate data in the next section. In other words, this rescaled variable represents the educational category probabilities (e.g., PR[Educ_Adjusted]) which will be used to allocate unassigned observations into select education bins to rebalance the data.

An illustrative example provides clarity and will be explained more in the next section:

																								Break Points			
Year	Race Mom	Census Region	Age Category	Marital Status	Birth Parity Num	Total Women NCHS	EDUC1	EDUC2	EDUC3	EDUC4	EDUC Missing	EDUC1 PCT NCHS	EDUC2 PCT NCHS	EDUC3 PCT NCHS	EDUC4 PCT NCHS	EDUC M PCT NCHS	NCHS 92 94 1	NCHS 92 94 2	NCHS 92 94 3	NCHS 92 94 4	Differen ce1	Differen ce2	Differen ce3	Differen ce4	EDUC1 PR END	EDUC2 PR END	EDUC3 PR END	EDUC4 PR END
1986	Black	1	20 to 24	Married	1	4,406	430	2,072	1,386	411	107	10%	47%	31%	9%	2%	10.40%	44.10%	33.20%	12.40%	1%	-3%	2%	3%	0.1105	0	0.4383	1
1987	Black	1	20 to 24	Married	1	4,406	385	2,041	1,460	398	122	9%	46%	33%	9%	3%	10.40%	44.10%	33.20%	12.40%	2%	-2%	0%	3%	0.3218	0	0.339	1
1988	Black	1	20 to 24	Married	1	4,242	367	1,764	1,135	343	633	9%	42%	27%	8%	15%	10.40%	44.10%	33.20%	12.40%	2%	2%	6%	4%	0.1141	0.2806	0.7139	1
1989	Black	1	20 to 24	Married	1	4,155	379	1,638	1,112	387	639	9%	39%	27%	9%	15%	10.40%	44.10%	33.20%	12.40%	1%	5%	6%	3%	0.0802	0.3823	0.8023	1
1990	Black	1	20 to 24	Married	1	3,600	270	1,456	989	302	583	8%	40%	27%	8%	16%	10.40%	44.10%	33.20%	12.40%	3%	4%	6%	4%	0.1763	0.4	0.7551	1
1991	Black	1	20 to 24	Married	1	3,227	301	1,486	1,022	340	78	9%	46%	32%	11%	2%	10.40%	44.10%	33.20%	12.40%	1%	-2%	2%	2%	0.2336	0	0.5866	1
1992	Black	1	20 to 24	Married	1	2,971	303	1,304	923	330	111	10%	44%	31%	11%	4%	10.40%	44.10%	33.20%	12.40%	0%	0%	2%	1%	0.0418	0.0892	0.6662	1
1993	Black	1	20 to 24	Married	1	2,473	262	1,009	813	304	85	11%	41%	33%	12%	3%	10.40%	44.10%	33.20%	12.40%	0%	3%	0%	0%	0	0.8886	0.9833	1
1994	Black	1	20 to 24	Married	1	2,375	217	1,015	773	299	71	9%	43%	33%	13%	3%	10.40%	44.10%	33.20%	12.40%	1%	1%	1%	0%	0.3777	0.7905	1	0
1995	Black	1	20 to 24	Married	1	2,095	222	836	696	263	78	11%	40%	33%	13%	4%	10.40%	44.10%	33.20%	12.40%	0%	4%	0%	0%	0	0.9998	1	0
1996	Black	1	20 to 24	Married	1	1,854	179	718	686	242	29	10%	39%	37%	13%	2%	10.40%	44.10%	33.20%	12.40%	1%	5%	-4%	-1%	0.1159	1	0	0
1997	Black	1	20 to 24	Married	1	2,281	252	876	839	284	30	11%	38%	37%	12%	1%	10.40%	44.10%	33.20%	12.40%	-1%	6%	-4%	0%	0	1	0	0

Please note that Educ1=High school dropouts; Educ2=High School Diploma; Educ3=Some College; and Educ4=College Plus.

B. Part II: Allocating Birth Certificates with Missing Educational Data

Once the breakpoints are established for the percent of missing observations which would rebalance the sample, we can then apply the thresholds to the disaggregated data.

Step 1 is to generate a random number bound between 0 and 1 for all NCHS observations without reported mother's education. This random number is taken from a uniform random distribution and fits the structure of the break points estimated in the previous section.

Step 2 is to place the NCHS observations with missing data into one of the four educational attainment categories; this allocation is based upon the rebalancing break points. Using the above example of Census Region 1 for Black women illustrates: any unclassified observation in the corresponding Census Region/Age Category/Marital Status/Birth Parity Number cell with a random number less than 0.1105 will be assigned to Educational Category 1 (e.g., high school dropouts). Since the number of high school graduates is already larger than expected (see Difference 2 = -3%, highlighted in blue), no further observations will be allocated. Educational Category 3 (e.g., Some College) will be assigned to all observations with random numbers between 0.1105 and 0.4383, while all observations with a random number above 0.4383 will be assigned to the College Plus Category.

Step 3 is to collapse the data into demographic cells by using the allocated mother's educational status data. We then perform a series of sanity checks to ensure that there are no structural breaks in the corrected series from 1986 to 1997.

II. Sub-Population Estimates

While the annual data collected by the Surveillance, Epidemiology, and End Results (SEER) Program allows for estimation of subpopulations by race and age groups, these data cannot be used to estimate samples below this level (e.g., by parity, marital status, educational level, or all three together). If this issue is ignored and left at a more aggregated level, the very differently sized demographic groups would receive the same weight in the underlying regression equations. This is problematic because mismeasurement will not be random (i.e. classical) - it is always overestimated - leading to biased coefficients of an unknown direction.

To derive a more accurate measure of the demographic cells central to this analysis we use PUMS micro-samples from the 1980, 1990, and 2000 Decennial Census. These data are provided by IPUMS and housed in the Minnesota Population Center at the University of Minnesota (see https://usa.ipums.org/usa/). We start by defining the demographic cells as outlined in Table 3 for the three Census micro-samples. We then interpolate the inter-censual years using PROC EXPAND in SAS;

we use a spline, which is the default technique. Under this methodology, note that the values for 1990 serve as the anchor: they equal the population values derived directly from the 1990 5% PUMS sample.

As demonstrated below in the example for African American women in Alabama in 1986 Q1, this approach provides a much more accurate population counts for smaller cells than more commonly used alternatives, such SEER data:

Mother's Education	Birth Parity	Age Category	Births	SEER Population	PUMS Estimated
< HS	1	20 to 24	51	51,422	3,200
< HS	1	25 to 29	10	46,856	1,787
< HS	1	30 to 34	3	40,468	1,895
< HS	1	35 to 39	1	34,260	1,511
< HS	1	40 to 44	2	25,076	2,150
< HS	2-3	20 to 24	242	51,422	3,249
< HS	2-3	25 to 29	98	46,856	3,053
< HS	2-3	30 to 34	23	40,468	2,579
< HS	2-3	35 to 39	8	34,260	2,514
< HS	2-3	40 to 44	0	25,076	3,143
< HS	4+	20 to 24	86	51,422	1,454
< HS	4+	25 to 29	67	46,856	3,084
< HS	4+	30 to 34	94	40,468	4,296
< HS	4+	35 to 39	42	34,260	3,836
< HS	4+	40 to 44	7	25,076	3,811

As displayed by the highlighted cells, use of the SEER - and the lack of data to estimate populations below race and age groups - would necessitate the assignment of a population of 51,422 for 20 to 24 year old black women in each of the birth parities 1, 2-3, and 4+. Again, this would be the population across all categories and not the demographic cell which is most relevant to this analysis. Thus, estimates derived from the PUMS microsamples leads to estimates which are much closer to the actual sub-population values.