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HETEROGENEOUS EFFECTS OF HEALTH INSURANCE ON BIRTH RELATED OUTCOMES: UNPACKING COMPOSITIONAL VS. DIRECT CHANGES

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ABSTRACT

When women of childbearing age gain health insurance, we expect their birth outcomes to improve, but comparing births that occur before and after policy changes may confound two separate impacts of coverage. For one, health insurance could affect who gives birth, through reduced costs of contraception. Health insurance could also directly improve maternal and child health among those who give birth, through additional prenatal resources. We address this question using the Affordable Care Act young adult provision, comparing birth related outcomes for those aged 24-25 years after the law, to outcomes among older young adults. We show that since the law subsidized contraceptives mainly among higher socioeconomic groups, the composition of mothers shifted towards less advantaged groups. Accounting for this shift, we find evidence of direct improvements in prenatal care and pregnancy-related health (reduced gestational diabetes and hypertension).

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I. INTRODUCTION

Whether access to health insurance can improve birth related outcomes has been a key question in the economics and demography literature. Medicaid expansions for pregnant women in the 1980s and 90s were mainly motivated by infant health concerns and have been shown effective in improving birth outcomes (Currie and Gruber 1996). However, recent research has reached mixed conclusions on whether Medicaid expansion for pregnant women also leads to changes in fertility outcomes (Bitler and Zavodny 2010; DeLeire, Lopoo, and Simon 2011). At the same time, the literature on access to contraceptives (Ananat and Hungerman 2012; Kearney and Levine 2009; Bailey 2012; Goldin and Katz 2000) finds unambiguous falls in fertility that changed the composition of those becoming new mothers.

If new health insurance affects fertility decisions, those who give birth post-policy may have different birth outcomes partly because of underlying factors that vary among socioeconomic groups. The largest recent expansion in insurance coverage for women of childbearing age occurred through the Affordable Care Act (ACA) young adult provision. In our work, we exploit this policy change to examine changes in composition of new mothers and resulting implications for pregnancy and birth outcomes.

The ACA expansion in private health insurance for young adult dependents targeted ages 19-26, which are among the most biologically fertile years for women (ACOG 2014). Studies already show that this provision leads to lower rates of child-bearing (Abramowitz 2018; Heim, Lurie, and Simon 2018) which is not surprising as this provision increases access to contraception without substantially changing insurance status for childbirth itself due to existing Medicaid policy (Y. A. Antwi et al. 2016) nor does the provision cover the newborn. We find that the provision caused the U.S. young-adult birth composition to shift, decreasing the share of children born to married, non-minority mothers and mothers not receiving WIC benefits. We show that understanding the compositional effect is also of practical importance for studying subsequent outcomes: evidence of improved prenatal health is much stronger when we control for compositional changes in mothers. The "total effect" estimate comparing the average mother before and after the policy would have resulted in smaller effect size compared to the direct effect the policy has on mothers from similar background. But we find that even with controls for maternal characteristics, there is inadequate statistical precision to assess direct effects of improved private insurance access on birth outcomes. Our work complements medical literature that examines these outcomes (Daw and Sommers 2018) which also finds no significant changes in birth weight but some evidence on early and adequate prenatal care.

Examining the compositional effects of the ACA young adult provision on maternal and infant health is an important addition to the literature for understanding the full scope of the provision's impact. The composition changes occurring among mothers in the fertility setting is similar to the mortality selection concept in the early-life health literature where children of weaker underlying health would die due to an adverse health shock, leaving those reaching adulthood to look more healthy at first glance (Bozzoli, Deaton, and Quintana-Domeque 2009).

The importance of understanding the heterogeneity of fertility response when studying birth outcomes has been demonstrated in the prior economics literature on contraceptive access. The pill literature finds that following the availability of reliable birth control, women from more advantaged socio-economic backgrounds disproportionally withdraw from motherhood, leading to an immediate worsening of birth outcomes in population-level studies (Ananat and

Hungerman 2012). One paper studying the effects of family planning programs in the 1970s finds that children born after the program had higher family incomes and one third of the increase is due to the programs' targeting lower income families so their reduction of fertility is strong (Bailey, Malkova, and Mclaren 2017). Pop - Eleches (2006) finds that children born after the abortion ban in Romania attained more education and better labor market outcomes because of changes in mother's composition. After controlling for parents' socio-economic background, children born after the ban experienced worse outcomes. These papers highlight the importance of mother's composition changes in the context of studying contraceptive access and child outcomes. Aside from these examples, we are unaware of papers in a contemporary US context studying how heterogeneity of responses affects composition of the population giving birth and how that selection effect may cancel out the actual direct effect of the policy. On the other hand, access to contraceptives reduces unplanned births (Kearney and Levine 2009) and could thus potentially improve birth outcomes (Joyce and Grossman 1990). The compositional effect on birth outcomes is essentially ambiguous. Ignoring such dynamics of fertility responses could introduce noise and bias in assessing how health insurance affects birth related outcomes, an important relationship given the U.S. ranks almost at the bottom of all OECD nations in infant health (as measured by mortality, OECD) and because the U.S. experienced substantial gains in health insurance through the ACA.

The ACA young adult provision extending dependent coverage for young adults up to age 26 has been shown to improve access to private health insurance (Antwi, Moriya, and Simon 2013; Sommers and Kronick 2012; Cantor et al. 2012). A number of studies have examined the provision's effect on various outcomes, such as labor market decisions (Heim, Lurie, and Simon 2014), out-of-pocket health care spending (Busch, Golberstein, and Meara 2014), access to inpatient and outpatient health care (Akosa Antwi, Moriya, and Simon 2015; Sommers et al. 2013), mortality (Mcclellan 2017), preventive care and health behavior outcomes (Barbaresco, Courtemanche, and Qi 2015), and time use (Colman and Dave 2018), among others.

Health insurance access also has substantial implications on childbearing behaviors and birth outcomes. More than a third (37.9%) of mothers who gave birth in 2009 were between the ages of 19 and 26. Young adults in this age group tend to be rather low users of other health care services: for example, labor and delivery is the most common cause of hospitalization among females between the ages of 20 and 21, comprising 62.3 percent of their hospital discharges in 2011 (U.S. Department of Health and Human Services 2011). Trudeau and Conway (2018) study the state young adult provisions and the state contraceptive mandates prior to the ACA. State young adult provisions are rather weak counterparts to the federal ACA young adult provision, as the state version applied only to about one half of employers whereas the federal young adult provision applied to all employers and all individual market plans (USDOL 2019). Not surprisingly, Trudeau and Conway (2018) find that the state young adult provision did not affect coverage or birth rates, thus they do not study effects on birth outcomes or discuss selection effects. Heim, Lurie, and Simon (2018) and Abramowitz (2018) document that the federal young adult provision led to modest reductions in fertility using tax data and the American Community Survey respectively. However, no paper thus far considers how differential fertility responses may affect the composition of mothers from a lower socio-economic background in the context of the ACA young adult provision. In this paper, we thus start by examining how composition may affect birth outcome estimates if one were only to look for standard difference-in-difference trend breaks among all treatment-age women compared to all control-age women. Using 2009 to 2015 birth certificate data and a difference-in-differences framework comparing young adults between the ages of 24 and 25 to slightly older adults (ages 27-28) before and after the provision is the typical way we would approach this research question. From there, we undertake steps to shed light on composition of births, as has been done in prior papers on contraceptive access.

First, we examine heterogeneity in fertility response to the availability of dependent coverage, along maternal characteristics not available or not used in prior studies conducted with household survey or tax data, such as pre-pregnancy health (including smoking, diabetes, and hypertension) and welfare program participation. We find that the young adult provision has increased the share of children born to unmarried mothers, mothers belonging to minority racial and ethnic groups, mothers who reported smoking in the pre-pregnancy period, and mothers receiving welfare benefits. The results suggest that women from higher socioeconomic backgrounds disproportionately choose to delay or avoid childbirth, which is a useful insight for our analyses on maternal and infant health. This exercise also reaffirms the overall fertility reduction result in prior literature, now using a new data source (birth certificates).

We next build on our evidence of differential fertility responses to examine the impact of the young adult provision on prenatal and birth outcomes. We show results with and without controls for maternal characteristics, demonstrating that composition matters in expected ways when studying prenatal care, but we find that statistical precision is low when we examine birth outcomes.

We find that the young adult provision directly improved prenatal care: a higher proportion of mothers initiated prenatal care in the first trimester, amounting to about a 2% increase. We also find improvement in maternal health. Mothers in our study were between 6 to 12% less likely to have gestational diabetes and hypertension.

We did not find significant improvements in birth outcomes. The improved maternal health would in theory imply an improvement in infant health as well. However, birth weight and gestational age showed little change when controlling for composition. In fact, when we estimate this relationship without controls, there was even a small but significant reduction in birth weight. It might be simply that infant health was not sensitive enough to improved prenatal care, or it could mean that the shift in unmeasured maternal composition negatively affected infant health, masking the effect of improved prenatal care on birth outcomes in our study.

Shifts in maternal composition to those with less advantaged socio-economic backgrounds would have a negative implication for prenatal care and birth outcomes because prior research (e.g. Geronimus 1996) has established that some groups of racial minority mothers tend to bear children with lower birth weight and a higher proportion of pre-term births compared to white mothers. However, having access to private health insurance could also have a direct positive effect on prenatal care and birth outcomes. Antwi et al. (2016) find that the young adult provision has caused pregnant women to substitute from Medicaid to private insurance, which may improve prenatal care and birth outcomes (Currie and Gruber 1996). In addition, being insured prior to pregnancy may improve early initiation of prenatal care (Rosenberg et al. 2007).

Our paper is first paper in the fertility, birth outcomes, health insurance space to consider and estimate selection vs direct (causal) effects. The key issue here is that since insurance affects both access to contraception as well as prenatal and preconception health care, birth outcomes are possibly affected in multiple ways (i.e. birth outcomes could improve directly because of better timing and better mothers' health care) but if we were to fit regressions for birth outcomes without accounting for the separate effects on fertility, and if the effects on fertility are targeted towards certain types of women, then the estimates could reflect both the direct effects on birth outcomes as well as the fact that the fertility effects changed the composition of who gives birth. A separate literature on contraceptive policy (contraceptive mandate, pill access) exists but those policies have different mechanisms at play: those papers primarily study fertility effects and to the extent there are any birth outcomes effects, they do not occur through prenatal health care. Past papers on fertility, birth outcomes and health insurance (or contraceptive policy in general) have not investigated the selection vs causation issue. As mentioned, there are several papers on fertility effects of the young adult provision, and there is one paper on birth outcome effects of the young adult provision (Daw and Sommers 2018), but neither consider the selection vs. causation themes of our paper.

Dills and Grecu (2017) study the effect of state contraceptive mandates and discuss the concept of direct vs. selection effects, but they do not explore selection empirically; they do not estimate whether selection changes the results on birth outcomes. Dills and Grecu (2017) find a decrease in fertility only among Hispanic women, a group not expected to be the target of the policy, and they do not find evidence of an improvement in birth outcomes. One might expect that contraceptive mandates could directly improve birth outcomes because the laws may allow couples to conceive a child at a time of their choosing, when they are more able to invest in prenatal inputs. If contraceptive mandate policy only affects some women (higher SES) who now also reduce their fertility, then more of the women giving birth after the policy will be of lower SES, and that may prevent uncovering of direct evidence of improved birth outcomes. However, Dills and Grecu (2017) actually find that fertility declines are not concentrated among higher SES women. They find fertility declines among young Hispanic women, who are not the most likely group covered by private employer coverage, thus this result is puzzling. They also find some selection evidence: more mothers are smokers post policy and more of the mothers do not provide paternal information; they do not test whether incorporating selection changes the results on birth outcomes.

Daw and Sommers (2018) examined the ACA young adult provision's effect on birth outcomes. Their paper did not touch on fertility changes thus does not discuss possible selection. They include estimates for birth outcomes with and without adjusting for some maternal characteristics, but only to note that they are similar. Our work extends the findings on birth outcomes to illustrate that there is much to be learned on heterogeneity of impact by incorporating fertility results into this analysis and by comparing estimates with and without controls for maternal characteristics. In fact, when we re-examine Daw and Sommers (2018) results, we find evidence consistent with our point: their estimated effects of improved birth outcomes and maternal behaviors of the federal young adult provision are actually clearer after adjusting for maternal characteristics.

Our work also adds to the understanding of the effect of health insurance access on childbearing-related health care use as it demonstrates that having access to health care prior to pregnancy and having private insurance over Medicaid during pregnancy leads to an improvement in prenatal care: early initiation.

II. THEORETICAL ARGUMENTS AND EMPIRICAL METHOD

Theoretical Arguments

The compositional relationship between health insurance and infant health could act in two directions. Previous studies and our own analyses consistently suggest that the ACA young adult provision has led to a decrease in fertility, especially among women of higher socioeconomic status. This compositional change would lead to *worsened* birth outcomes, as the remaining mothers have lower infant health outcomes. However, the ACA young adult provision has also likely reduced the proportion of unwanted pregnancies even conditional on socioeconomic status, and in this sense, we may observe *improved* birth outcomes due to selection.

In contrast to the compositional effects through fertility consequences, the *direct* effect of the ACA young adult provision on birth outcomes would occur through improved health insurance access prior to pregnancy and shifting pregnant women from Medicaid to private insurance. Pre-pregnancy insurance coverage leads to improved prenatal care (Rosenberg et al. 2007). Medicaid reimburses providers at lower rates than do private insurance (Alexander and Schnell 2019; Currie and Gruber 1997). On the other hand, private insurance has higher cost sharing than Medicaid, which is first dollar coverage for this population. Although the RAND experiment and more recent studies (Swartz 2010) show that higher cost-sharing may lead to a delay of care and worsen health for sicker populations, the evidence suggests that higher cost-sharing has a less adverse effect on healthy populations. Since the population covered by the young adult provision is from more advantaged socio-economic backgrounds than the average Medicaid beneficiary, , moving from Medicaid to employer-sponsored private plans (which have copays but are better accepted by providers) may improve office-based health care access without the downsides associated with higher cost sharing. A positive direct effect combined with an ambiguous compositional effect suggests that the relationship between the insurance offered through the ACA young adult provision and birth outcomes is ultimately an empirical question.

Empirical Method

We use a difference-in-differences (DD) framework comparing the treatment group (24-25 year olds) who are most comparable in age with a control group (27-28 year olds), before and after the ACA young adult provision, to identify the effect of the ACA young adult provision on maternal composition, prenatal care, and birth outcomes. The specification is shown below:

$$Y_{gt} = \beta_0 + \beta_1 * \left(Treat_g * Post_t\right) + G_g + T_t + X_{gt} * \gamma + \varepsilon_{gt}$$

where g denotes the age group and t is the month-year one gave birth. Y_{gt} is the outcome for mothers, or infants born to mothers, in age group g who gave birth in month-year t. $Treat_g$ is a dummy variable that takes the value one if the mother is between the ages of 24 and 25. $Post_t$ is also a dummy variable that is one for the observations from October 2011 and later. G_g are the age fixed effects and T_t are the month-year fixed effects. We also control for the labor market trends (X_{gt}) at the group by month-year level.

Our sample includes mothers between the ages of 24 and 28 years, excluding 26 year olds because of the ambiguity of their treatment status under the policy. Thus, our treatment group amounts to 24-25 year-olds. This narrower treatment group is used instead of all 19-25 yr olds as the sample size in the Natality data is large, and focusing on 24-25 yr olds vs 27-28 yr olds provides closer matches on characteristics of the two populations Using the 24- to 25-year-old mothers as the treatment group mitigates variation from prior young adult provisions in different states, which rarely extended coverage to those older than 24.

To further account for the potential differential effects of labor market trends, we also include an interaction between the unemployment rate and treatment status, in addition to controlling for unemployment rates. We lag unemployment rates by 9 months to analyze fertility and maternal composition.

The young adult provision was announced in March 2010 and went into effect in September 2010. However, most private insurance plans renew at the start of the year, so the full effect was expected to take place in January 2011. Infants conceived after the full effect date would generally be born after October 2011 so we count this as our post period. For the fertility outcome, we exclude the transitional period from November 2010 to September 2011 in our analyses because of the ambiguity of the policy strength. Births before November 2010 would be less affected because these babies were conceived before the announcement of the provision. For prenatal care and birth outcomes, the excluded transitional period is March 2010 to September 2011 because the effect on prenatal care and birth outcomes may have started as early as March 2010 due to increased access to private insurance during pregnancy.

Our analysis uses data collapsed to the year-month-age level, and fertility is calculated using the total count of births for mothers of the same age in each month. Maternal composition outcomes are defined as the share (percentage), among all mothers of the same age in a given month, who had a specific characteristic. For example, the proportion of mothers who were unmarried among all 24-year-old mothers in January 2011 would be the value of the outcome variable for one observation.

We control for age fixed effects, which capture time invariant fertility differences by age, and year-month fixed effects, which allow us to control for unobserved measures common to all mothers within a given month, such as seasonal fluctuations. We control for the log of the female population for studying fertility and for baby's gender when we examine prenatal care and birth outcomes. As our data is collapsed at a highly aggregate level, we use robust standard errors in our main analyses. This is equivalent to the clustered standard errors at the age-month-year level as in Daw and Sommers (2018). We also report the p-value of the wild bootstrap t standard error clustered at the age level with Webb weights for our main results[1].

Since we are using cell-level data in the analyses, we must account for the frequency of the cells through weighting with the appropriate universe (DeLeire, Lopoo, and Simon 2011). For fertility, each cell has the frequency of the corresponding age-specific female population. For birth outcomes, the frequency used for weights is the number of births in each cell, since these outcomes such as birth weight are only relevant for that population.

To partly control for the impact of maternal composition change on prenatal care, we include ethnic/racial composition in the model (the proportion of white mothers, African American mothers, and Hispanic mothers) and the proportion of married mothers; from there, we estimate the results for pre-pregnancy health outcomes and prenatal care, maternal health, and infant health.

By including these population proportions, we control for the changes caused by the shifting composition only in ways observable in our data; we caveat that there may be other non-measurable ways through which selection operates. Though our approach cannot eliminate composition changes within the same race or marital status level, by comparing our results with demographic controls to our results without them, we gain new insights on the effect of composition shifts due to insurance changes.

[1] Webb weights are a 6-point bootstrap weight distribution and has been shown to improve the reliability of inference in cases with few clusters (Webb, 2014).

III. DATA

This study uses the 2009 to 2015 Natality Public Files which contain birth certificate data collected through the National Vital Statistics System by the Centers for Disease Control and Prevention. This provides information on parental demographic information, mother's prenatal care and infant birth outcomes for all births in the United States. We create a denominator using data on the female population by age from the National Population Estimates file from the U.S. Census Bureau-Population Division. Unemployment data by race for age groups 18-19, 20-24, and 25-29 are obtained from the Labor Force Statistics of the Current Population Survey.

The sample period 2009 to 2015 is ideal for this study for several reasons. Birth certificates underwent a systematic update during this period: states began adopting the 2003 revision of the U.S. Standard Certificate of Live Birth in 2003, but implementation was phased in over more than a decade; data from the 2003 certificate started to be available in 2009. The number of states and territories that had adopted the new certificate rose from 28 in 2009 to 49 (including D.C.) in 2015 (see Table 2A for the implementation schedule). Among variables used in this study, only marital status, race, birth weight, and birth counts are comparable across the 1989 and 2003 birth certificates.[2] Other outcome variables are only available through the 2003 certificate. To ensure data consistency, we only include observations from the updated 2003 certificate for the main analyses. The results are similar if we limit the sample to only the 28 states that adopted the certificate in 2009.

[2] Maternal smoking and prenatal care, though included in both 1989 and 2003 versions of birth certificates, were not comparable because of changes in how questions were specified and the sources from which information was obtained. For example, for the first year revised certificates are implemented, the percentage of women reported to begin care in the first trimester typically falls in a state by at least 10 percentage points (National Center for Health Statistics 2009).

Table 1 reports summary statistics of maternal characteristics and birth outcomes for the treatment and control groups. Unsurprisingly, the control group of mothers between the ages of 27-28 had a higher overall fertility rate. Compared to the treatment group, the older mothers were unsurprisingly from more advantaged social-economic backgrounds: they were more likely to be married and white, and less likely to receive the WIC benefits. They were also less likely to be smokers. In terms of pre-pregnancy health, the treatment group appeared to have lower rates of pre-pregnancy diabetes and hypertension, possibly due to their younger age.

We examine three sets of outcomes: prenatal care utilization, maternal behaviors and health, and infant health. We measure prenatal care from two aspects: the total number of prenatal visits and when prenatal care is initiated. The number of prenatal visits measures the quantity of prenatal care and early prenatal care initiation measures the quality of the prenatal care. For maternal health, we focus on maternal smoking which has been show in prior literature to have a strong effect on infant health. We use birth weight and gestational age to measure infant health. We also report neonatal ICU use for extreme adverse outcomes. The control group displays an earlier initiation of prenatal care and more prenatal visits. Mothers in the treatment group had lower rates of gestational diabetes and hypertension. However, both groups were very similar in terms of infant birth weight, gestational age, and neonatal ICU utilization.

IV. RESULTS

<u>Fertility and Maternal Composition</u>

We first examine changes in fertility due to the young adult provision using Natality data, to compare with estimates in the literature produced with other data sources. Figure 1 shows the regression-adjusted difference in fertility trends between the treatment group of 24-25-year-old mothers and the control group of 27-28-year-old mothers. Prior to October 2010, the difference remained relatively stable: the level difference was negative, indicating that the control group had a higher fertility rate. After October 2011, when the effect of the young adult provision was likely to be fully realized, there was an increase in the difference in fertility between the control and treatment groups. This graph clearly suggests a reduction in fertility among the treatment group following the young adult provision, confirming previous studies.

Table 2 reports regression results for the fertility models. Using log birth counts as our measure, we show that as compared to the control group, the fertility of the treatment group dropped by 2.57%. The mean birthrate for the treatment group in the sample is 84 per 1,000 women, suggesting that the young adult provision led to about 2 fewer births for every 1,000 women. The estimated insurance gain for the 23-25 age group is about 0.0414 (Y. Antwi, Moriya, and Simon 2013), so the implied elasticity of fertility to health insurance is about 0.62. Using fertility rate or log fertility rate as measures, the estimates are also negative and significant, with a slightly larger magnitude.

Our estimate of a -2.57 percent fertility change is larger than those presented in two other studies of the effect of the young adult provision on fertility. Abramowitz (2018) estimates a fertility reduction of -1.17 percent using a sample of 20-to-30-year-old mothers; Heim, Lurie, and Simon (2018) estimate a reduction of -0.5 percent using a sample of 24-to-29-year olds whose parents have employer-based retirement plans. Considering the large variation in sample inclusion criteria, the variation in these estimates is not surprising. More importantly, however, both studies find that the young adult provision has had a negative effect on fertility and builds consensus on this question.

To examine whether changes in fertility translated into changes in the type of parents to whom children are born during our period of study, we apply the DD framework to the composition of mothers. Panel A of Table 3 reports results on the fraction of children born to unmarried mothers and mothers of different racial groups. The coefficient of Column 1 suggests that there was a reduction in the fraction of children born to married mothers. The fraction of children born to white mothers decreased as well, while the fraction of children with African American mothers increased.

Panel B of Table 3 reports changes in pre-pregnancy smoking, diabetes, and hypertension. Pre-pregnancy smoking is higher among mothers in the treatment group following the provision. The increase in pre-pregnancy smoking likely reflects changing maternal composition as those mothers who gained access to private insurance and avoided childbearing were from higher socioeconomic backgrounds. This reduction in fertility among women of higher socioeconomic backgrounds in turn reduced the average socioeconomic status of those who gave birth. Mothers from lower socioeconomic backgrounds, meanwhile, were more likely to smoke.[3] We find a significant reduction in the proportion of mothers with pre-pregnancy diabetes, and the coefficient for hypertension is also negative. The pattern for diabetes and hypertension was less consistent across socioeconomic conditions so changes in mother's composition might have resulted in improvement in diabetes and hypertension.

Turning now to data on infants, those born to disadvantaged women tend to have lower health measures than those born to advantaged mothers, thus these results above on maternal composition have implications for the study of birth outcomes. For example, infant birth weight

[3] We calculate using Natality data that the pre-pregnancy smoking rates were 9.55% for married mothers and 22.25% for unmarried mothers in 2009.

is higher for white mothers relative to African American mothers.[4] The shift in maternal composition to be more African American, less educated, for example, also implies infant health measures may appear lower on average, holding other factors constant.

The results controlling for maternal composition changes are reported in Panel C of Table 3. These regressions include the proportion of white mothers, African American mothers, and Hispanic mothers and the proportion of married mothers in the model. When we compare the estimates in Panel B with those in Panel C, we see that all changes in pre-pregnancy health can be explained by shifting maternal composition: after controlling for composition, the coefficients for pre-pregnancy smoking, diabetes, and hypertension become very close to zero and are no longer significant. This suggests that controlling for the proportions effectively accounts for composition changes and that composition shifts resulting from improved family planning might not have been significant enough to cause changes in pre-pregnancy health outcomes.

Prenatal Care and Birth Outcomes

In this section, we discuss the effect of the young adult provision on prenatal care utilization and birth outcomes. The provision affected health insurance during pregnancy by increasing the share of pregnant women with private insurance, which was more generous than Medicaid and was thus expected to improve access to prenatal care. Prenatal care and birth outcomes were also expected to be affected by shifting maternal composition: a higher proportion of babies born to more disadvantaged mothers would result in decreases in prenatal and infant health without

[4] Our calculation suggests that the average birth weights were 3324.986 g for white mothers and 3085.926 g for African American mothers in 2009.

changes in pregnancy wantedness or prenatal care. We report results from the basic model in Table 4 and estimates controlling for composition changes in Table 5.

Panel A of Table 4 suggests that before controlling for race and marital status, there appears significant improvement in prenatal care in the form of earlier prenatal care initiation. The proportion of mothers who started prenatal care in the first trimester increased by 1.16 percentage points. The increase was accompanied by a reduction in those starting prenatal care in the second (0.753 percentage points) and third trimesters (0.337 percentage points), rather than an increase in the overall proportion of mothers getting any prenatal care. In 2009, before the provision, 67.77% of mothers in the treatment group started prenatal care in the first trimester, so the increase was about 2 percent of the prior average.

Earlier initiation of prenatal care has been shown to improve maternal health as well as infant birth outcomes, and in potentially cost-effective ways, since it could be accomplished while keeping the total number of prenatal visits the same. Earlier initiation, rather than an increase in the total number of prenatal visits, is considered beneficial to pregnancies without incurring additional costs. Healthy People 2020 sets the goal for the proportion of pregnant women who receive prenatal care beginning in the first trimester at 77.9%. The young adult provision moved the treatment group marginally closer to that goal.

In addition to private insurance being more generous than Medicaid and thus providing better access to prenatal care (insofar as it makes it easier to find a provider and results in less waiting time), having any insurance before pregnancy rather than obtaining Medicaid after becoming pregnant seems also to encourage earlier initiation of prenatal care. Rosenberg et al. (2007) find that pre-pregnancy Medicaid coverage appears to be associated with earlier initiation of prenatal care. Our results also suggest that providing health insurance before pregnancy may be an effective way to improve prenatal care initiation.

Panel B of Table 4 reports the effects on maternal behaviors and health. Without controls for race and marital status, we observe a reduction in gestational diabetes and hypertension. The effects are rather large: a 7.5 percent reduction (over a base of the prior mean of 2.8 percentage points) in gestational diabetes and a 4.2 percent reduction in gestational hypertension (prior mean of 3.9 percentage points). For maternal smoking, the coefficient suggests that there were no significant changes. Recall that in Table 3 Panel B, pre-pregnancy smoking appeared to have increased as a result of the provision, but only before race and marital status were added. We attempt now to directly examine smoking cessation behaviors by constructing two smoking cessation measures in panel B of table 4: smoking cessation before pregnancy (the proportion of mothers not smoking at all during pregnancy among those who were smoking within 3 months of pregnancy) and cessation during pregnancy (the proportion of mothers not smoking in the third trimester among those who were smoking during the first trimester). These regressions without race and marital status controls indicate a significant increase in smoking cession during pregnancy of about 2 percentage points, which was about 10 percent of the prior average.

Infant health, on the other hand, displayed an extremely small (less than one tenth of a percent) though statistically significant decrease in two cases of the four displayed in Panel C of Table 4. The lack of response in birth outcomes to the provision may result from the opposing effect of improved prenatal care and maternal health, and from changes in the fraction of children born to mothers of lower socioeconomic backgrounds.

To separate the composition effect from the effect of improved private insurance access, we use a regression model that adjusts for demographic characteristics, just as we do in lower Panel B of Table 3. We report the results in Table 5. Comparing the results in Table 5 to those in Table 4, we find that the improvement in prenatal care initiation is still significant after accounting for compositional changes and if anything, are slightly stronger results, suggesting that this change was due to improved access to private insurance rather than changes in observed maternal composition. It also appears that the improvements in gestational diabetes and hypertension were not entirely due to compositional changes but benefited from insurance improvements. The effect on smoking cessation behaviors, however, changes once we control for composition, becoming statistically insignificant. Last but not least, the measures in Table 5 Panel C suggest that there were no significant changes in infant health. After accounting for the potential negative effect of compositional changes, it appears that infant health is not sensitive to the improvement in the prenatal care and maternal health brought by the provision. We also report the estimates by race and marital status for selected outcomes in Table 3A in the appendix. The subgroup results are consistent with the direct effects in Table 5.

The results in Table 5 suggest that the private insurance access afforded by the young adult provision had positive effects on prenatal care and maternal health, but no detectable effect on infant health. Thus, the overall take-away regarding the impact of the provision on birth and prenatal care is consistent with Daw and Sommers (2018), who also find improvements in prenatal health but no strong evidence on childbirth outcomes.

V. ROBUSTNESS CHECK

The key assumption underlying the DD framework is that in the absence of the policy change, the treatment and control groups would have experienced parallel trends. In our context, if there were changes in the size and composition of our cohorts, the DD estimates would capture these changes but not cohort decisions in response to the young adult provision. We control for the size of the female population, where the most disaggregated level of data we can obtain is the yearly-race level. If there were changes in the proportion of cohorts in other dimensions, the estimation may be biased. It is thus important to check the validity of the common trend assumption. Figure 1 provides visual evidence that the difference between the treatment and control groups stayed constant prior to the provision.

One challenge, as pointed out in Slusky (2015), is that potentially different trends in the treatment and control groups caused by a shifting labor market after the 2007 recession pose a threat to the identification strategy used in the young adult provision literature. The narrow treatment group and the unemployment interaction term in our model help control for these differences. Our conservative model uses aggregate data and flexible time trends, which adds to the confidence of our main findings.

To formally test the pre-trend difference between the treatment and control groups, we regress outcome variables on a linear time trend and its interaction with the treatment status, using data prior to November 2010 for fertility and mother's composition and prior to March 2010 for prenatal care and birth outcomes. The coefficient of the interaction term is expected to catch the pre-trend difference in a linear fashion. For consistency with the main model, we control for age fixed effects, month-year fixed effects, and the unemployment rate and its interaction with the treatment status. We weigh the fertility and mother's composition regressions by the female population and the prenatal care and birth outcome models by the number of births in each cell.

Table 6 reports pre-trends for fertility, maternal composition, and pre-pregnancy health. All coefficients are small and not significant at conventional levels. Table 7 reports the pre-trend tests for prenatal care, maternal health, and infant health. Most coefficients are small compared to the estimates in the analyses and far from statistical significance except for "quit smoking during pregnancy" and "neonatal ICU". Overall, these results indicate that differing pre-policy trends do not appear to substantially detract from assumptions underlying the DD framework.

To further address the possibility of differential time trends for the treatment and control groups, we perform placebo tests with pseudo implementation dates using data prior to March 2010. In these tests, the implementation dates are set as 12 pseudo dates from February 2009 to January 2010.[5] We report the results of these pseudo experiments for prenatal care initiation and maternal health in Table 8. Each pair represents a separate regression of the outcome using the specified date as the pseudo policy date. Overall, the coefficients are small and not statistically significant, indicating that our results are not driven by differential underlying trends. Results for other outcomes are available upon request.

<u>Using State Level Data</u>

In Table 9, we use 2009 to 2013 state-level data to explore the heterogeneity among states interacting with previous policies such as Medicaid standalone contraceptive coverage. For the state-level analyses, we collapse the data at the state-month-age level and control for race,

[5] The prenatal care and maternal health variables shown in Table 8 are not available prior to 2009 so we cannot conduct a longer-term pre-trend test.

marital and education composition, as well as the unemployment rate and women's population size. We also include state, year, and month fixed effects and the interaction between the unemployment rate and treatment status.

We explore the heterogeneity in interaction with the state Medicaid family planning waivers. A Medicaid family planning waiver allows states to provide contraceptive coverage to women with an income level too high to qualify for Medicaid with federal reimbursement for 90% of the cost of these services and supplies. In 2011, 22 states had expanded eligibility for family planning services (Sonfield, Frost, and Gold 2011). Theoretically, the young adult provision should have a stronger effect in states without family planning waivers. Column 2 and 3 in Table 9 report the fertility results by waiver status. It suggests that the effects are very similar, which might be due to the fact that the young adult provision targeted women whose parents have health insurance and can be part of a different population than the Medicaid waiver.

The last two columns of Table 9 show the effects by prior state level young adult provision status. 30 states have expanded dependent coverage to some extent to young adults beyond age 18 before 2010 and we hypothesize that the effect of the federal provision would be more substantial in states without prior provision. However, the magnitudes of the effect are similar with or without prior provision. Depew (2015) examines the effect of state young adult provision on fertility using CPS data and found the effect to be not significant. Trudeau and Conway (2018) also find no changes in birth rates due to the state young adult mandate. Given these findings, it is likely that the federal provision has similar effects regardless of prior state provisions.

VI. CONCLUSION

This paper examines the role of maternal selection in how the ACA young adult provision, which allows young adults to stay on their parents' health insurance plan until the age of 26, affects fertility, prenatal care utilization, and pregnancy and birth outcomes. The evidence suggests that the short-term effects of the provision are a reduction in fertility and an increase in the relative proportion of infants who are born to less advantaged mothers. It also suggests that the provision improves prenatal care and maternal health even after we account for maternal composition changes, though there is no detectable evidence that this translates into improvements in birth outcomes.

Our result suggests that the young adult provision causes women from advantaged socioeconomic backgrounds to disproportionally withdraw from motherhood (because they gained coverage through the provision that raises their use of contraception) and raises an inequality concern. This represents an additional reason for society wishing to prioritize public policy that provides insurance coverage for women from disadvantaged backgrounds.

We also find very small improvements in earlier initiation of prenatal care, which relates to Healthy People 2020's goal of "increasing the proportion of pregnant women who receive prenatal care beginning in the first trimester to 77.9 percent". Currently, the rate is around 70% for 24-25 yr old mothers, and the ACA increased this by about 1.3 percentage points (Table 5). Although the benefit of more prenatal visits has been contested (McDuffie et al. 1996), early prenatal care is largely considered cost effective and beneficial to infant and maternal health in policy discussions (Healthy People 2020). In our context, it appears however that birth outcomes such as gestation and birth weight are not sensitive to the small magnitude of improvement in prenatal care caused by the provision, thus further studies are needed to identify policy actions that improve birth outcomes in statistically detectable ways.

The study has several limitations. The most important challenge is that composition shifts might not be fully captured by the variables available to us, such as pre-pregnancy health status and health behaviors, race/ethnicity, and marital status. Thus, while our study is the first to highlight heterogeneous fertility responses as a potentially important consideration when examining birth outcomes, there is room in future studies to improve ways to capture compositional changes.

Another limitation is that we cannot directly identify the “provision-affected” population whose parents had employer-sponsored health insurance because birth certificates do not collect information on mothers’ parents. Our analysis thus cannot directly identify the elasticity of births in response to health insurance access or coverage. However, we do offer an imprecise back-of-envelope calculation for targeted groups using estimates of the young adult provision’s effect on insurance coverage.

Reduced fertility naturally raises questions of how young adults spend the time they would otherwise have spent on child rearing. Although education, work, and leisure would be natural guesses, the evidence only seems to support an increase in time spent on leisure activities (Colman and Dave 2018); it does not support substantial changes in education or labor force participation (Heim, Lurie, and Simon 2014).

Amidst discussions regarding the future of healthcare reform and several attempts to repeal the ACA, the young adult provision is among the more popular pieces of the law and is likely to remain regardless of any other changes in U.S. health policy in the near future. Indeed, the

American Health Care Act and several other recent ACA replacement proposals proposed to keep the young adult provision unaltered. This increases the need to understand the policy's effects beyond those on coverage and on young adult health status. Our findings suggest that the provision has improved maternal health and helped young adults determine their optimal fertility, although more research is needed to uncover the direct effects of health insurance on improved birth outcomes.

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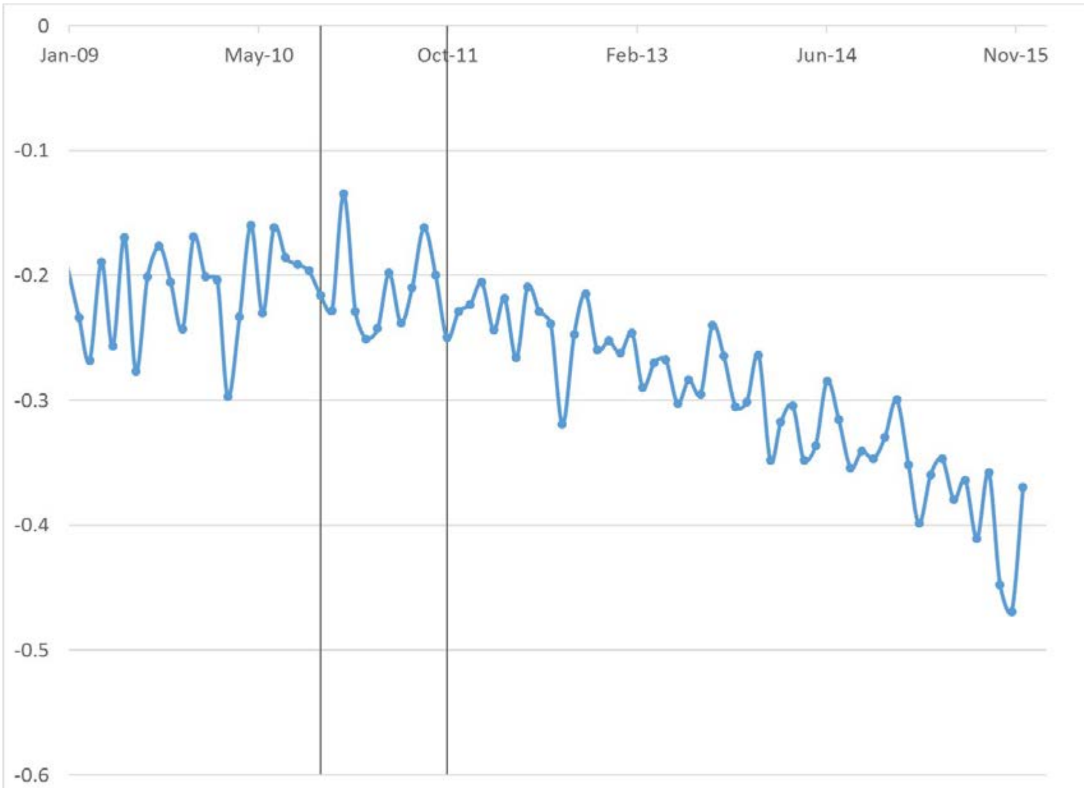
FIGURE 1

Difference in Fertility Trends between Treatment and Control Groups



Note: Using cell level data (age/year/month/race/marital/education), we plot regression-adjusted estimates of the difference between control and treatment group fertility trends, controlling for population size. We run two separate regressions: one for the treatment group and one for the control group. Explanatory variables include year-month fixed effects, age fixed effects, and cell fixed effects at the race-marital-education level. Using the 'margin' command in Stata, we generate predicted regression-adjusted fertility by year-month and plot the difference. The number represents the difference between the control and treatment groups. Lines are drawn at 9 months after March 2010 (the month of the ACA announcement) and 9 months after January 2011 (the month of the law's full implementation).

TABLE 1

Descriptive Statistics by Age Group: Maternal Composition and Birth Outcomes, Pre-period

Outcome Variables	Age 24-25 Mean	St.dev	Age 27-28 Mean	St.dev	T-test Significance
Demographic Characteristics					
Fertility	72.032	1.293	76.795	0.578	#
Married	0.541	0.032	0.688	0.021	***
WIC	0.552	0.028	0.412	0.025	***
Race					
White	0.524	0.011	0.562	0.015	***
Black	0.141	0.008	0.115	0.006	***
Hispanic	0.287	0.012	0.255	0.013	***
Health Pre-pregnancy					
Smoking before Pregnancy	0.155	0.009	0.117	0.007	***
Diabetes before Pregnancy	0.005	0.001	0.006	0.001	***
Hypertension before Pregnancy	0.009	0.001	0.011	0.001	***
Health Behaviors and Birth Outcomes					
Infant Health					
Birth Weight	3263.112	9.606	3293.025	8.329	***
Gestational Age	38.668	0.040	38.659	0.037	
Neonatal ICU	0.067	0.003	0.067	0.003	
Maternal Health					
Maternal Smoking	0.121	0.007	0.089	0.005	***
Gestational Diabetes	0.031	0.002	0.041	0.003	***
Gestational Hypertension	0.040	0.002	0.039	0.003	
Medical Utilization					
Prenatal Visits	10.885	0.079	11.212	0.074	***
Month Prenatal Care Started	1.391	0.021	1.314	0.014	***
Cesarean Delivery	0.301	0.006	0.321	0.006	***

Notes: 1. Summary statistics are calculated using the Natality Public Use Files from January 2009 to September 2010. The fertility estimates are based on 2009 data.
2. Fertility: yearly # of births per 1,000 women. # t-tests are not applicable to fertility rates.
3. The upper panel (demographic characteristics) is analyzed at the age/month level. The numbers reported are calculated using cell averages weighted by the female population.
4. The lower panel (health behaviors and outcomes) is calculated using cell averages weighted by birth counts.
5. T test is to test for significant differences between treatment and control groups. *0.10, **0.05, ***0.01 denote significance levels.

TABLE 2

Fertility Results

	Log Birth Count[1]	Fertility Rate[2]	Log Birth Count Using Data up to 2013
Coefficient	-0.0257***	-8.539***	-0.0249***
	(0.00404)	(0.548)	(0.00390)
Wild bootstrap t p-value	0.0591	0.0541	0.0490
Observations	292	292	196
R-squared	0.991	0.970	0.989

Note: 1. The sample consists of individuals between the ages of 24 and 28 (26 excluded) and is drawn from the 2009 to 2015 Natality Public Use Files. The data is aggregated at the age-month-year level. Log birth count specification controls for the log of the female population. The model includes fixed effects for a given month-year and age fixed effects. It also includes lagged unemployment rates and an interaction between treatment status and lagged unemployment rates.
2. The fertility rate is calculated as the number of newborns per 1,000 women. The last three columns provide the estimate using a sample from 2009 to the year in the column name.
3. The coefficients reported here are the coefficients of the interaction terms between treatment status and post time period.
4. Robust standard errors are reported. We also report the p-value for the standard error clustered at the age level using wild bootstrap t with Webb (2014) weights.
5. *0.10, **0.05, ***0.01 denote significance levels.

TABLE 3

Maternal Composition

Panel A: Marital Status and Race				
	Married	White	African American	WIC Receipt
Coefficient	-0.0223***	-0.0147***	0.0138***	0.00253**
	(0.00122)	(0.00135)	(0.000898)	(0.00121)
Wild bootstrap t p-value	0.0591	0.0581	0.0601	0.0691
Pre-mean	0.540	0.524	0.141	0.582
Observations	292	292	292	292
R-squared	0.997	0.969	0.976	0.997
Panel B: Pre-pregnancy Health				
	Smoking before Pregnancy	Pre-pregnancy Diabetes	Pre-pregnancy Hypertension	
Coefficient	0.00186**	-0.00547***	-0.000351	
	(0.000855)	(0.000617)	(0.000229)	
Wild bootstrap t p-value	0.2543	0.0651	0.2843	
Pre-mean	0.166	0.00503	0.00887	
Observations	292	292	292	
R-squared	0.981	0.985	0.784	
Controlling for Race and Marital Status				
Coefficient	0.0000515	-0.00130	-0.0000993	
	(0.00134)	(0.000889)	(0.000370)	
Wild bootstrap t p-value	0.9249	0.4595	0.1782	
Observations	292	292	292	
R-squared	0.983	0.988	0.781	

Note: 1. The sample consists of individuals between the ages of 24 and 28 (26 excluded) and is drawn from the 2009 to 2015 Natality Public Files. The data are aggregated at the age-month-year level. The model includes fixed effects for a given month-year and age fixed effects, as well as lagged unemployment rates and an interaction between treatment status and lagged unemployment rates. The estimates are weighted by the inverse of the female population.

2. In Panel A, the dependent variables are the proportions of mothers who are married, white, black, or Hispanic. The dependent variables in Panel B are the health conditions of mothers before their pregnancies. Panel C reports the same outcomes as Panel B, which are estimated with additional controls for the proportion of race and marital status composition (the outcome variables in Panel A).3. The coefficients reported here are the coefficients of the interaction terms between treatment status and the post time period.

4. Robust standard errors are reported. We also report the p-value for the standard error clustered at the age level using wild bootstrap t with Webb (2014) weights.

5. *0.10, **0.05, ***0.01 denote significance levels.

TABLE 4

Prenatal Care and Maternal and Infant Health Outcomes without Controlling for Race and Marital Status

Panel A: Prenatal Care					
		Prenatal Care Start			
	Prenatal Visits	1st Trimester	2nd Trimester	3rd Trimester	Caesarean
Coefficient	0.00757	0.0116***	-0.00753***	-0.00337***	0.000372
	(0.00850)	(0.00139)	(0.00118)	(0.000512)	(0.00115)
Wild bootstrap t p-value					
	0.0951	0.0641	0.0561	0.0641	0.8488
Pre-mean	11.066	0.692	0.225	0.054	0.303
Observations	4248305	4248305	4248305	4248305	4248305
R-square	0.982	0.988	0.985	0.921	0.930
Panel B: Maternal Health					
	Maternal Smoking	Gestational Diabetes	Gestational Hypertension	Quit Smoking before Pregnancy	Quit Smoking during Pregnancy
Coefficient	-0.000376	-0.00219***	-0.00163***	0.0121***	0.00976***
	(0.000837)	(0.000400)	(0.000523)	(0.00251)	(0.00297)
Wild bootstrap t p-value					
	0.7628	0.0631	0.0531	0.0591	0.1922
Pre-mean	0.117	0.028	0.039	0.314	0.242
Observations	4248305	4248305	4248305	4248305	4248305
R-square	0.985	0.962	0.909	0.652	0.424
Panel C: Infant Health					
	Birth Weight	Low Birth Weight: <2,500g	Gestation	Preterm: Gestation<37 weeks	Neonatal ICU
Coefficient	-5.498***	0.000749	-0.0212***	-0.000386	0.000164
	(1.226)	(0.000559)	(0.00676)	(0.000901)	(0.000678)
Wild bootstrap t p-value					
	0.0651	0.0561	0.0741	0.6026	0.6667
Pre-mean	3265.755	0.075	38.664	0.114	0.067
Observations	4248305	4248305	4248305	4248305	4248305
R-square	0.967	0.735	0.853	0.807	0.852

Note: 1. The sample consists of individuals between the ages of 24 and 28 (26 excluded) and is drawn from the 2009 to 2015 Natality Public Files. The data are aggregated at the age-month-year level. The model includes fixed effects for a given month-year, age fixed effects, unemployment rates, and an interaction between treatment status and unemployment rates. The regression is weighted by total birth counts for that cell.
2. The dependent variables in Panel A are the average prenatal care use of mothers for each cell; Panel B reports the average measures of maternal health in each cell; Panel C reports the average measures of birth outcomes for each cell. The data is aggregated at the age-month-year level.
3. The coefficients reported here are the coefficients of the interaction terms between treatment status and post time period.
4. Robust standard errors are reported. We also report the p-value for the standard error clustered at the age level using wild bootstrap t with Webb (2014) weights.
5. *0.10, **0.05, ***0.01 denote significance levels.

TABLE 5

Prenatal Care and Maternal and Infant Health Outcomes, Controlling for Race and Marital Status

Panel A: Prenatal Care					
			Prenatal Care Start		
	Prenatal Visits	1st Trimester	2nd Trimester	3rd Trimester	Caesarean
Coefficient	0.0210*	0.0126***	-0.00829***	-0.00291***	-0.00210
	(0.0124)	(0.00191)	(0.00178)	(0.000779)	(0.00170)
Wild bootstrap t p-value					
	0.0681	0.0490	0.0470	0.0511	0.6436
Pre-mean	11.066	0.692	0.225	0.054	0.303
Obs	4248305	4248305	4248305	4248305	4248305
R-square	0.983	0.988	0.986	0.922	0.932

Panel B: Maternal Health					
	Maternal Smoking	Gestational Diabetes	Gestational Hypertension	Quit Smoking before Pregnancy	Quit Smoking during Pregnancy
Coefficient	-0.00134	-0.00347***	-0.00269***	0.00122	0.00518
	(0.00107)	(0.000618)	(0.000825)	(0.00397)	(0.00475)
Wild bootstrap t p-value					
	0.3213	0.0661	0.0541	0.7477	0.4735
Pre-mean	0.117	0.028	0.039	0.314	0.242
Obs	4248305	4248305	4248305	4248305	4248305
R-square	0.987	0.963	0.912	0.670	0.431

Panel C: Infant Health					
	Birth Weight	Low Birth Weight: <2,500g	Gestation	Preterm: Gestation<37 Weeks	Neonatal ICU
Coefficient	0.410	0.0000962	0.00445	-0.00131	-0.000891
	(2.008)	(0.000957)	(0.00939)	(0.00136)	(0.00109)
Wild bootstrap t p-value					
	0.8869	0.9389	0.5666	0.4885	0.2963
Pre-mean	3265.755	0.075	38.664	0.114	0.067
Obs	4248305	4248305	4248305	4248305	4248305
R-square	0.972	0.754	0.864	0.809	0.857

Note: 1. The sample consists of individuals between the ages of 24 and 28 (26 excluded) and is drawn from the 2009 to 2015 Natality Public Files. The data are aggregated at the age-month-year level. The model includes fixed effects for a given month-year, age fixed effects, unemployment rates, an interaction between treatment status and unemployment rates, and additional controls for the proportion of race and marital status composition. The regression is weighted by total birth counts for that cell.
2. The dependent variable in Panel A is the average prenatal care use of mothers for each cell; Panel B reports the average measures of maternal health for each cell; Panel C reports the average birth outcome measures for each cell.
3. The coefficients reported here are the coefficients of the interaction terms between treatment status and post time period.
4. Robust standard errors are reported. We also report the p-value for the standard error clustered at the age level using wild bootstrap t with Webb (2014) weights.
5. *0.10, **0.05, ***0.01 denote significance levels.

TABLE 6

Pre-Policy Trends for Fertility and Maternal Composition

	Panel A: Fertility			
	Log Count	Fertility Rate		
Pre-trend	0.000141	-0.0204		
	(0.00101)	(0.0728)		
N	88	88		
adj. R-sq	0.977	0.972		
	Panel B: Marital Status and Race			
	Married	White	Black	WIC Receipt
Pre-trend	0.0000693	0.000533	0.000125	-0.000241
	(0.000306)	(0.000387)	(0.000226)	(0.000421)
Observations	88	88	88	88
R-square	0.997	0.961	0.956	0.995
	Panel C: Pre-pregnancy Health			
	Smoking before Pregnancy	Diabetes before Pregnancy	Hypertension before Pregnancy	
Pre-trend	0.0000770	-0.0000471	-0.0000555	
	(0.000221)	(0.0000370)	(0.0000661)	
Observations	88	88	88	
R-square	0.972	0.591	0.626	

Note: 1. The sample consists of individuals between the ages of 24 and 28 (26 excluded). The upper panel is estimated using the January 2009 to November 2010 Natality Public Files, and the lower panel is estimated using data from January 2005 to November 2010 Natality Public Files. The coefficient reported is for the interaction between a linear time trend and treatment status. The model includes fixed effects for a given month-year and age fixed effects. It also includes unemployment rates and an interaction between treatment status and unemployment rates.
2. Robust standard errors are reported.
3. *0.10, **0.05, ***0.01 denote significance levels.

TABLE 7

Pre-Policy trends for Prenatal Care and Maternal Health

Panel A: Prenatal Care					
		Prenatal Care Start			
	Prenatal Visits	1st Trimester	2nd Trimester	3rd Trimester	Caesarean
Coefficient	0.00689	0.000430	0.000294	-0.000114	0.000161
	(0.00474)	(0.000729)	(0.000629)	(0.000212)	(0.000575)
N	729991	729991	729991	729991	729991
R^2	0.978	0.984	0.980	0.961	0.901

Panel B: Maternal Health					
	Maternal Smoking	Gestational Diabetes	Gestational Hypertension	Quit Smoking before Pregnancy	Quit Smoking during Pregnancy
Coefficient	-0.000184	-0.000195	-0.0000530	0.000589	-0.00341**
	(0.000379)	(0.000182)	(0.000259)	(0.00137)	(0.00161)
N	729991	729991	729991	729991	729991
R^2	0.982	0.958	0.544	0.673	0.333

Panel C: Infant Health					
	Birth Weight	Low Birth Weight: <2,500g	Gestation	Preterm: Gestation<37 Weeks	Neonatal ICU
Coefficient	0.269	-0.000418	0.000238	-0.0000314	-0.000734**
	(0.563)	(0.000271)	(0.00282)	(0.000383)	(0.000349)
N	729991	729991	729991	729991	729991
R^2	0.959	0.730	0.719	0.722	0.543

Note: 1. The sample consists of individuals between the ages of 24 and 28 (26 excluded) and is drawn from January 2009 to March 2010 Natality Public Files. The coefficient reported is for the interaction between a linear time trend and treatment status. The model includes fixed effects for a given month-year, as well as age fixed effects and cell-level fixed effects. It also includes unemployment rates and an interaction between treatment status and unemployment rates.
2. Robust standard errors are reported.

TABLE 8

Placebo Tests with Pseudo Implementation Dates

	Prenatal Care Start in 1st Trimester	Prenatal Care Start in 2nd Trimester	Prenatal Care Start in 3rd Trimester	Gestational Diabetes	Gestational Hypertension
Estimate	0.0116***	-0.00753***	-0.00337***	-0.00219***	-0.00163***
	(0.00139)	(0.00118)	(0.000512)	(0.0004)	(0.000523)
February 2009	0.00253	-0.00271	-0.000280	-0.000627	0.00105
	(0.00593)	(0.00561)	(0.000763)	(0.000637)	(0.00112)
March 2009	0.00366	-0.00333	-0.000195	-0.000406	-0.000468
	(0.00431)	(0.00402)	(0.00102)	(0.000901)	(0.00179)
April 2009	-0.00163	-0.000576	0.00193**	0.000953	-0.00350*
	(0.00394)	(0.00364)	(0.000873)	(0.000983)	(0.00180)
May 2009	-0.000434	-0.00286	0.00106	-0.000525	-0.000345
	(0.00396)	(0.00371)	(0.00119)	(0.00129)	(0.00176)
June 2009	-0.000270	-0.00118	0.00132	-0.00108	0.000347
	(0.00323)	(0.00308)	(0.000946)	(0.00120)	(0.00126)
July 2009	0.00253	-0.00253	0.000237	-0.00127	0.0000200
	(0.00253)	(0.00226)	(0.000909)	(0.000866)	(0.00117)
August 2009	0.00272	-0.00164	-0.000844	-0.00119*	-0.000390
	(0.00207)	(0.00197)	(0.000915)	(0.000705)	(0.000959)
September 2009	0.00498**	-0.00399**	-0.00105	-0.000682	-0.000607
	(0.00207)	(0.00193)	(0.000759)	(0.000657)	(0.000861)
October 2009	0.00282	-0.00292	-0.000525	0.0000335	-0.000219
	(0.00226)	(0.00195)	(0.000743)	(0.000666)	(0.000809)
November 2009	0.00254	-0.00264	-0.0000337	-0.000623	-0.0000221
	(0.00251)	(0.00215)	(0.000754)	(0.000740)	(0.000850)
December 2009	-0.00217	0.000485	0.000475	-0.000768	0.000124
	(0.00196)	(0.00202)	(0.000822)	(0.000922)	(0.000925)
January 2009	-0.00171	0.000599	-0.000114	0.000656	-0.000439
	(0.00182)	(0.00209)	(0.00111)	(0.000913)	(0.000951)

Note: 1. The sample consists of individuals between the ages of 24 and 28 (26 excluded) and is drawn from January 2009 to March 2010 Natality Public Files. The coefficient reported is for the interaction between a pseudo implementation date as indicated and the treatment status. The model includes fixed effects for a given month-year, age fixed effects, and cell-level fixed effects. It also includes unemployment rates and an interaction between treatment status and unemployment rates.
2. Robust standard errors are reported.
3. *0.10, **0.05, ***0.01 denote significance levels.

TABLE 9

Fertility Effects using State Level Data

	28 States with new certificate	No Waiver	Waiver	No Prior Provision	With Prior Provision
Effect	-0.0619***	-0.0556***	-0.0889***	-0.0435*	-0.0510**
	(0.0124)	(0.0140)	(0.0144)	(0.0188)	(0.0174)
N	9114	8820	5880	6174	8820
adj. R-sq	0.971	0.958	0.967	0.973	0.967

Note: 1. The sample consists of individuals between the ages of 24 and 28 (26 excluded) and is drawn from the 2009 to 2013 Natality Public Use Files with state identifiers. The data is aggregated at the age-month-year-state level. The dependent variable is the log birth count in each cell. The model includes fixed effects for month and year fixed effects, state fixed effects, and age fixed effects. It also includes lagged unemployment rates and an interaction between treatment status and lagged unemployment rates.
2. The coefficients reported here are the coefficients of the interaction terms between treatment status and post time period.
4. Standard errors are clustered at the state level.
5. *0.10, **0.05, ***0.01 denote significance levels.

Appendix

TABLE 1A

Percentage Missing of All Outcome Variables

Outcome Variables	Missing	Total	Percent Missing
Birthweight	14,140	13,346,815	0.11%
Gestational Age	15,341	13,346,815	0.11%
Maternal Smoking	1,069,064	13,346,815	8.01%
Gestational Diabetes	133,561	13,346,815	1.00%
Gestational Hypertension	133,561	13,346,815	1.00%
Prenatal Visits	568,740	13,346,815	4.26%
Month Prenatal Care Started	619,627	13,346,815	4.64%
Cesarean Delivery	12,467	13,346,815	0.09%
Married	0	13,346,815	0.00%
WIC	390,071	13,346,815	2.92%
White	175,273	13,346,815	1.31%
Black	175,273	13,346,815	1.31%
Hispanic	175,273	13,346,815	1.31%
<High School	231,392	13,346,815	1.73%
High School	231,392	13,346,815	1.73%
Some College	231,392	13,346,815	1.73%
College	231,392	13,346,815	1.73%
Graduate Degree	231,392	13,346,815	1.73%
Smoking before Pregnancy	1,068,058	13,346,815	8.00%
Diabetes before Pregnancy	42,332	13,346,815	0.32%
Hypertension before Pregnancy	42,332	13,346,815	0.32%

Note: from Natality Public Files 2009-2015

TABLE 2A

Implementation of the 2003 U.S. Standard Certificate of Live Birth

Year Total	2015 48 States and the D.C	2014 47 States and the D.C	2013 41 States and the D.C	2012 38 States and the D.C	2011 36 States and the D.C	2010 33 States and the D.C	2009 28 States
Alabama	1	1					
Alaska	1	1	1				
Arizona	1	1					
Arkansas	1	1					
California	1	1	1	1	1	1	1
Colorado	1	1	1	1	1	1	1
Connecticut							
Delaware	1	1	1	1	1	1	1
District of Columbia	1	1	1	1	1	1	1*
Florida	1	1	1	1	1	1	1
Georgia	1	1	1	1	1	1	1
Hawaii	1	1					
Idaho	1	1	1	1	1	1	1
Illinois	1	1	1	1	1	1	
Indiana	1	1	1	1	1	1	1
Iowa	1	1	1	1	1	1	1
Kansas	1	1	1	1	1	1	1
Kentucky	1	1	1	1	1	1	1
Louisiana	1	1	1	1	1	1*	
Maine	1	1	1*				
Maryland	1	1	1	1	1	1	
Massachusetts	1	1	1	1	1*		
Michigan	1	1	1	1	1	1	1
Minnesota	1	1	1	1			
Mississippi	1	1	1				
Missouri	1	1	1	1	1	1	
Montana	1	1	1	1	1	1	1
Nebraska	1	1	1	1	1	1	1
Nevada	1	1	1	1	1	1	1*
New Hampshire	1	1	1	1	1	1	1
New Jersey	1	1*					
New Mexico	1	1	1	1	1	1	1
New York (excluding NYC)	1	1	1	1	1	1	1

New York City	1	1	1	1	1	1	1
Year	2015	2014	2013	2012	2011	2010	2009
North Carolina	1	1	1	1	1	1*	
North Dakota	1	1	1	1	1	1	1
Ohio	1	1	1	1	1	1	1
Oklahoma	1	1	1	1	1	1	1*
Oregon	1	1	1	1	1	1	1
Pennsylvania	1	1	1	1	1	1	1
Rhode Island	1	1*					
South Carolina	1	1	1	1	1	1	1
South Dakota	1	1	1	1	1	1	1
Tennessee	1	1	1	1	1	1	1
Texas	1	1	1	1	1	1	1
Utah	1	1	1	1	1	1	1
Vermont	1	1	1	1	1	1	1
Virginia	1	1	1	1*			
Washington	1	1	1	1	1	1	1
West Virginia	1	1					
Wisconsin	1	1	1	1	1		
Wyoming	1	1	1	1	1	1	1

Note: Information obtained from the 2015 Natality Public File. Includes each state and territory, New York City, and the District of Columbia; * states revised certificates after January 1; new birth certificate information is included from the following year. Data excludes reporting areas that revised after January 1.

Table 3A
Subgroup Analyses of the Birth Outcomes

	(1)	(2)	(3)	(4)	(5)
	Prenatal Care Start				
	1st Trimester	2nd Trimester	Gestational Diabetes	Gestational Hypertension	Gestation Age
Full sample	0.0116***	-0.00753***	-0.00219***	-0.00163***	-0.0212***
	(0.00139)	(0.00118)	(0.0004)	(0.000523)	(0.00676)
White-Unmarried	0.00622	-0.00580	-0.00364*	-0.0000410	0.0287
	(0.00508)	(0.00560)	(0.00176)	(0.00196)	(0.0170)
White-Married	0.00212	-0.00299*	-0.00234**	-0.00189**	-0.00787
	(0.00237)	(0.00163)	(0.000954)	(0.000899)	(0.0132)
Black-Unmarried	0.00944**	-0.00746**	-0.00166	-0.00286**	-0.00776
	(0.00447)	(0.00316)	(0.00192)	(0.00122)	(0.0282)
Black-Married	0.0165	-0.00815	0.00348	0.000904	-0.0201
	(0.0106)	(0.00756)	(0.00383)	(0.00227)	(0.0275)
Hispanic-Unmarried	0.00999***	-0.00570*	-0.00373***	0.000556	0.0358*
	(0.00334)	(0.00286)	(0.00101)	(0.00197)	(0.0207)
Hispanic-Married	0.00269	-0.000298	-0.0000975	-0.00160	0.00504
	(0.00606)	(0.00361)	(0.00152)	(0.00144)	(0.0306)

Note: 1. The sample consists of individuals between the ages of 24 and 28 (26 excluded) and is drawn from the 2009 to 2015 Natality Public Use Files. The dependent variables are the average measures of birth outcomes in each cell. The data is aggregated to the age-marital status-race-education-month-year level. The model includes month and year fixed effects, age fixed effects, and education fixed effects. It also includes unemployment rates and an interaction between treatment status and unemployment rates.
2. Standard errors are clustered at the age-marital status-race-education level.
3. *0.10, **0.05, ***0.01 denote significance levels.