ESSAYS IN HEALTH ECONOMICS

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Ausmita Ghosh

ESSAYS IN HEALTH ECONOMICS

My dissertation is a collection of three essays on the design of public health insurance in the United States. Each essay examines the responsiveness of health behavior and healthcare utilization to insurance-related incentives and draws implications for health policy in addressing the needs of disadvantaged populations. The first two essays evaluate the impact of Medicaid expansions under the Affordable Care Act (ACA) on health and healthcare utilization. The Medicaid expansions that included full coverage of preconception care, led to a decline in childbirths, particularly those that are unintended. In addition, these fertility reductions are attributable to higher utilization of Medicaid-financed prescription contraceptives. The second essay documents patterns of aggregate prescription drug utilization in response to the Medicaid expansions. Within the first 15 months following the policy change, Medicaid prescriptions increased, with relatively larger increases for chronic drugs such as diabetes and cardio-vascular medications, suggesting improvements in access to medical care. There is no evidence of reductions in uninsured or privately-insured prescriptions, suggesting that Medicaid did not simply substitute for other forms of payment, and that net utilization increased. The effects on utilization are relatively higher in areas with larger minority and disadvantaged populations, suggesting reduction in disparities in access to care.

Finally, the third essay considers the effect of Medicaid coverage loss on hospitalizations and uncompensated care use among non-elderly adults. The results show that coverage loss led to higher uninsured hospitalizations, suggesting higher uncompensated care use. Most of the increase in uninsured hospitalizations are driven by visits originating in the ED - a pattern consistent with losing access to regular place of care. These results indicate that policies that reduce Medicaid funding could be particularly harmful for patients with chronic conditions.

Anne Beeson Royalty, PhD, Chair

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1 Public Health Insurance, Fertility and Contraception

1.1. Introduction

Women's healthcare access has featured prominently in the recent health reforms, with the Affordable Care Act (ACA) promoting greater insurance coverage of contraception (Becker and Polsky, 2015; Carlin et al., 2016), and prioritizing women's preventive care as one of the ten essential health benefits (Cuellar et al., 2012). Several of these policy features were motivated as important for reducing unplanned births. However, there are as yet no studies that have examined the effect of the ACA 2014 coverage expansions on fertility and prescription birth control, and this study fills that gap.

According to recent estimates, nearly 4 out of 10 pregnancies in the United States are unintended (Finer and Zolna, 2016). The Institute of Medicine has emphasized the need to increase access to FDA-approved contraceptives as part of preventive services in order to reduce unintended births (Institute of Medicine, 2011). Generally, there is evidence that timing of childbearing has effects on several aspects of women's lives including marriage, and human capital accumulation through education and employment (Bailey, 2006; Goldin and Katz, 2002). Improved control over fertility also has spillovers to the next generation (Ananat and Hungerman, 2012; Bailey et al., 2014).

Although the ACA raised Medicaid eligibility level for all medical services to 138% of the federal poverty level (FPL) regardless of categorical eligibility for the non-elderly population in expansion states, Medicaid already paid for pregnancy-related care, childbirth services and medical costs of the child for low-income pregnant women in all states beginning in 1984 (Zavodny and Bitler, 2010). Thus, the ACA Medicaid expansions constitute new coverage only for the preconception period and may therefore have important implications for childbearing behaviors and outcomes in this population.

Even though there was no nationwide coverage under Medicaid for the preconception period for low-income women, since 1994, several states have established programs that provide Medicaid-funded family planning services to the near-poor. Therefore, it is possible that the ACA Medicaid expansions may have no detectable effects on preconception care, such as the use of contraceptive healthcare, and consequently, there may not be any effect on fertility rates. On the other hand, take-up of these state programs may have been low because of lack of knowledge about these programs and contrastingly, the ACA Medicaid expansions were very visible. The literature shows that insured women are more likely to use contraceptives (Culwell and Feinglass, 2007); and low income and low educational attainment are both associated with gaps in contraceptive use (Frost et al., 2007). This is further highlighted by the fact that Medicaid-paid prescription contraceptives increased substantially following the ACA Medicaid expansions (Ghosh et al., 2017). Thus, the ACA Medicaid expansion provides a valuable opportunity to examine how insurance expansions that fully cover contraceptive coverage and continuous healthcare coverage, not just during the prenatal period, affects fertility.

Although there is a large empirical literature on the relationship between insurance coverage and fertility, theory offers competing hypotheses on how insurance-related incentives affect fertility decisions when coverage is comprehensive for the entire continuum of reproductive and perinatal health (preconception, during pregnancy, childbirth, interconception and covering the newborn child as well). There are numerous pathways that could link health insurance expansions to fertility. Health insurance reduces the cost of medical care related to pregnancy, childbirth and resulting offspring, which could lead to an increase in fertility. Conversely, reduction in the cost of pregnancy prevention will have a negative impact on fertility when insurance coverage benefit includes contraception without cost-sharing. In addition, health insurance can lead to changes in other behaviors such as employment and marriage which have their own effects on demand for children by altering the costs of parenthood. Under the assumption that children are normal goods, health insurance can also affect demand for children through its direct influence on financial well-being. The net effect on aggregate fertility will depend on the responsiveness along these margins.

Recent research evaluating the ACA Medicaid expansions have found significant decline in uninsurance rates since early 2014 (Courtemanche et al., 2017; Frean et al., 2017; Kaestner et al., 2017; Sommers et al., 2015), improvements in financial health (Hu et al., 2016), but little or no effect on labor market outcomes (Gooptu et al., 2016; Kaestner et al., 2017). While the ACA Medicaid expansions seem to have modestly affected healthcare use like hospitalizations and ED visits (Freedman et al., 2017; Wherry and Miller, 2016), the impact on prescription drug use has been substantial (Ghosh et al., 2017; Maclean et al., 2017; Wen et al., 2016). Ghosh et al. (2017) show that Medicaid prescription drug utilization increased 19% in the first 15 months following the ACA Medicaid expansions, and that contraceptives increased 22%. Their results indicate that there was a net increase in utilization as there was no evidence of Medicaid crowding out other payment sources in their claims data. No study of the ACA 2014 Medicaid expansion effects on contraceptive use have examined whether effects differ for states already with Medicaid family planning waivers prior to the ACA, and no previous research has examined effects for fertility in any expansion states.

As previous research has shown, state policies that extended income-based eligibility for Medicaid family planning services to women aged 20 to 44 led to a fall in the birth rate, with the bulk of this decrease being attributable to an increase in the use of contraception (Kearney and Levine, 2009). Nevertheless, the growth in contraceptive utilization following the ACA expansions is suggestive evidence that family planning waivers may have been inadequate and conditional on higher contraceptive use, one might expect attendant effects on childbearing decisions.

In this study, I use the quasi-natural experiment that resulted from the 2012 Supreme Court ruling that led the 2014 insurance expansions under the ACA Medicaid provision to be implemented only in some states and not in others, to examine the causal effect of health insurance on fertility and contraception patterns among low-income populations. I find that the ACA Medicaid expansions led to a decline in fertility among less-educated first-time mothers. For first-time mothers without a high school diploma, a group that is most likely to be affected by the Medicaid eligibility extensions, fertility decreased 4.5 to 6.6 %. Fertility fell by 4.1 to 5.7% among unmarried women in this subgroup, suggesting a reduction in unintended births as a result of the expansions. I also document a similar pattern of decline in childbirths for younger mothers between ages 18-24. As Medicaid participation is negatively associated with educational attainment, in a falsification test, I show that there is no evidence of change in fertility among highly educated mothers.

To explore the mechanism behind this observed impact on childbearing, I provide complementary evidence using administrative Medicaid prescription drugs data that these fertility reductions are attributable to a contemporaneous 24% increase in Medicaid-financed contraceptive utilization indicating greater access to subsidized contraception. Finally, the effect on Medicaid-funded birth control differed across states based on availability of Medicaid-funded family planning services through preexisting state programs, which is consistent with the expansions increasing access to contraceptives in areas with lesser prior access to subsidized contraceptives.

This paper contributes to the literature in several ways. First, in contrast to prior policy shifts in Medicaid and commercial insurance that extended piecemeal coverage for family planning services or for pregnancy and resulting offspring, the results from this study provide evidence on how the complementarities between zero cost-sharing contraception and subsidized physician access afforded by comprehensive coverage affect fertility and contraceptive use among disadvantaged women through a nationwide analysis. Second, I leverage the large sample size and rich detail in the vital statistics data to explore heterogeneity in the impact of the policy by birth parity and maternal demographic characteristics. Third, to explore the mechanism behind fertility responses I document complementary evidence of the policy impact on aggregate Medicaid contraception patterns by using national-level administrative data on Medicaid-financed prescription drugs. In doing so, I confirm findings from the earlier literature and provide new evidence on the evolution of aggregate Medicaid contraceptive utilization in the 3 years following the expansions. Finally, utilizing pre-expansion variation in Medicaid family planning programs across states, I document a differential effect of the expansions on Medicaid-financed contraceptive use in states with and without waiver programs. This provides suggestive evidence of coverage gaps in existing Medicaid-funded family planning services, and how the ACA Medicaid expansions are closing these gaps in unmet needs in women's reproductive healthcare services.

1.2. Background

1.2.1. Institutional Setting

Medicaid is a means-tested health insurance program for low-income populations (pregnant women, children, parents and the disabled) that is jointly administered by the federal and state governments. The ACA provided additional federal financing to states in 2014 for extending Medicaid coverage to non-elderly adults with incomes below 133% FPL (138%, with the inclusion of a 5% income disregard), regardless of categorical eligibility. As modified by a 2012 Supreme Court ruling, participating in Medicaid expansion under the ACA was a voluntary decision for states. As of January 1, 2017, 32 states including the District of Columbia have adopted the expansions, while the remaining 19 states have not.

Prior to the 2014 policy change, Medicaid eligibility for pregnant women and parents was already more generous than the ACA level (138% FPL) in most states. For instance, 42 states plus Washington DC had expanded Medicaid eligibility to include pregnant women with incomes higher than 138% FPL in 2013 (Kaiser Commission on Medicaid and the Uninsured, 2013). In this respect, the ACA extends Medicaid eligibility broadly to cover all low-income adults under age 65, unlike previous Medicaid expansions that were conditional on parental and pregnancy status. Additionally, under federal rules, Medicaid programs in expansion states must provide the new enrollees preventive services, including FDA-approved prescription birth control without cost-sharing, as part of the ten "essential health benefits" (Ranji et al., 2016; Sonfield, 2016). Thus, the ACA Medicaid expansions may enhance access and early use of preconception care which may lead to a fall in fertility. However, fertility may rise as result of reduced costs of prenatal care and childbirth-related care through Medicaid. The literature on the ACA has now examined impacts on many outcomes, but comprehensive nationwide health impacts of this health reform remain understudied. Changes in childbearing decisions are an important pathway through which insurance coverage expansions may affect both health and non-health outcomes of women.

1.2.2. **Related Literature**

Theory is less clear on how comprehensive insurance coverage would affect fertility. If insurance coverage is extended to pregnancy-related healthcare services and care for newborn, then expansions could increase fertility. The earliest studies evaluating Medicaid expansions for pregnant women and children in the 1980s and 1990s that reduced the cost of childbearing find fertility to increase among certain demographic subsamples. Using data from 15 states, Joyce et al. (1998) find a rise in childbirths among low educated white women between ages 19 to 27. Similarly, Zavodny and Bitler (2010) using nationwide data document higher fertility among low educated women, while DeLeire et al. (2011) do not find any fertility impacts. At the same time, experimental evidence from the RAND Health Insurance Experiment (HIE) demonstrated an increase in fertility among women with free health insurance relative to a control group assigned to health plans with patient cost-sharing (Leibowitz, 1990).

In contrast to the ACA Medicaid expansions, several insurance expansions in the past few decades have been targeted towards the preconception period specifically such as mandated insurance coverage of contraceptives for those with employer sponsored health insurance and state expansions of Medicaid-funded family planning services. If mandated coverage is only for preconception period, then theoretically fertility may decline due to increased access to contraceptives. State health insurance mandates that required employer-based health insurance plans to provide contraceptive coverage were associated with an increase in contraception use (Mulligan, 2016; Raissian and Lopoo, 2015) and abortion rate but no corresponding fall in fertility for reproductive-age women (Mulligan, 2016). However, recent research demonstrates that these mandates had intended effects of reducing childbirths among younger women (Trudeau and Conway, 2017). More recently, the ACA contraceptive mandate required private health insurance plans to cover prescription contraceptives without any patient cost-sharing, starting in August 2012 and was implemented nationally (US Department of Health and Human Services, 2014). This mandate was associated with substantial reductions in out-of-pocket spending and higher contraceptive use among privately-insured women (Becker and Polsky, 2015; Carlin et al., 2016; Pace et al., 2016). Similarly, beginning in 1994, several states expanded eligibility for Medicaid-covered family planning services to women who were otherwise ineligible for traditional Medicaid. These limited expansion waivers reduced gaps in contraceptive use by 5.3% and childbirths declined by 2% among women aged 20-44 (Kearney and Levine, 2009). Similarly, Lindrooth and McCullough (2007) also document that Medicaid family planning expansions led to a 2 percentage point fall in fertility.

Recent studies have shown that the 2010 ACA provision that extended "dependent coverage" to young adults up to age 26 reduced fertility (Heim et al., 2017) through increased access to contraceptives (Abramowitz, 2017). Fertility could also decline if there is a reduction in family formation; Heim et al. (2017)'s analysis using tax data in the context of the ACA young adult mandate finds evidence in support of this mechanism. However, the behavior of young adults of higher socio-economic status parents (those with access to employer health insurance) may not mirror the behavior of the population newly eligible for adult Medicaid expansions. In contrast to prior research on insurance and fertility, I examine a policy setting in which a recent and large public health insurance expansion affected a low-income population.

In a closely related study, Apostolova-Mihaylova and Yelowitz (2015) consider fertility impacts of coverage expansion for low-income populations that includes preconception care along with the benefits of traditional Medicaid, but their analysis is limited to the experience of a single state (Massachusetts). The authors find that the Massachusetts health reform in 2006 did not affect overall fertility, but increased the likelihood of childbirths among married women aged 20-34, while reducing childbirths among their unmarried counterparts. However, there is no specific emphasis on birth parity or on an exploration of the mechanism driving these results.

Unlike the ACA Medicaid expansions, most previous coverage expansions for reproductive-age women have been piecemeal—for example, state policies that extend contraceptive coverage or maternity coverage through private insurance mandates – that only cover certain aspects of the reproductive/perinatal health continuum and not others. Additionally, unlike policies that solely mandate contraceptive coverage for the already insured, these Medicaid expansions are more comprehensive in coverage because in addition to pregnancy related health care services, the benefits include physician visits, preventive care and family planning services as well. Taken together, the combination of lowered cost for physician visits as well as subsidized contraceptives may be stronger than the effect of mandated coverage of contraceptives alone. Furthermore, greater awareness of the ACA Medicaid expansions than of state contraceptive mandates may further amplify the potential impact. This policy setting is thus closely related to Medicaid family planning waivers and state plan amendments (SPAs) of the 1990s and 2000s, although the 2014 Medicaid expansions were arguably more broadly advertised than state Medicaid family planning programs. Thus, Medicaid expansions under the ACA offer a unique large-scale policy experiment through which the effects of comprehensive subsidized public health insurance on childbearing decisions and healthcare utilization can be studied. This paper combines detailed data from several sources to address these questions.

1.3. Estimating the Impact of 2014 Medicaid Expansions on Fertility

1.3.1. Data

I use detailed micro data on all births from Natality Detail restricted-use files produced as part of the National Vital Statistics System by the Centers for Disease Control and Prevention to examine whether Medicaid expansions affected fertility in general, and specifically among low-educated mothers. The data I obtained are repeated cross-sections covering the years 2012 to 2015, and contain information gathered from birth certificates for the census of all births occurring in the United States during these calendar years with restricted geographic identifiers for mother's state of residence. Detailed demographic information such as mother's age, educational attainment and pregnancy history is provided, allowing me to stratify fertility rates by mother's demographic characteristics (such as by age and education) and by parity of birth.

To isolate the effect of coverage on childbearing decisions, I date births in the data to the year and quarter of conception by utilizing a hypothetical 39-week gestation which is roughly equivalent to 3 quarters. I aggregate the universe of births into demographic subgroups by maternal education/birth parity/age/conception year and quarter, with the actual time series running from Q2 of 2011 to Q1 of 2015, in an approach similar to DeLeire et al. (2011). Using the geographic identifiers and data on month and year of birth, I map this microdata with publicly available information related to state's Medicaid expansion status, unemployment rates and poverty rates that correspond to the time of conception. I obtain state-level unemployment rates from the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS) program, measured monthly. State poverty rates are obtained from Small Area Income and Poverty Estimates (SAIPE) produced by the Census Bureau. These data dimensions are important because identification rests on policy change at the state level, and I expect heterogeneous response to the insurance expansions across demographic sub-groups and state characteristics.

For this analysis I focus on mothers between ages 18 and 44. In an ideal dataset, family income would be reported for each birth record. However, as the Vital Statistics data do not report income, I examine effects among mothers with low education, as well as mothers whom I expect to respond more to the policy change due to other demographic characteristics such as birth parity and marital status. The standard birth certificate items in the US during the study time period were changed in 1989 and 2003. As states implemented the 2003 birth certificate revisions over a staggered timeline, 38 states and the District of Columbia complied in 2012, while in 2015 (the last year in our study period) only two out the 50 states were non-compliant. While data elements from both 1989 and 2003 versions of birth certificates were included in the files, unrevised variables that were not comparable with 2003 revisions were not recorded (National Center for Health Statistics, 2010). Among the Vital Statistics variables used in this study, only birth counts, mother's age and parity of birth are comparable across birth certificate revisions of 1989 and 2003. Maternal educational attainment is only reported in states adopting the 2003 revisions over all or part of our sample period. As a result, for models stratified by maternal educational attainment, to obtain a balanced panel of states with data available 8 quarters before and after the policy change, I trade off geographic coverage against panel length.[1] I also consider fertility responses for samples stratified by mother's age, as a proxy for pregnancy intendedness, to uncover heterogeneous policy impact on childbearing behavior.

[1] The unrevised states excluded from models stratified by mother's education in the Vital Statistics data are AL, AK, AR, AZ, CT, HI, ME, MS, NJ, RI, VA and WV.

1.3.2. **Estimation strategy**

To empirically evaluate the relationship between expanded Medicaid eligibility and fertility, I will exploit the quasi-experimental variation in states' decisions to implement Medicaid expansions across states and over time. Specifically, I estimate the following differences-in-differences (DD) regression of the following form:

$$\ln(births_{st}) = \beta_0 + \beta_1 Post_t * Expansion_s + \beta_2 Post_t + \beta_3 Expansion_s + \beta_4 \boldsymbol{X}_{st} + \beta_5 \ln(pop_{st}) + \theta_s + \mu_t + \varepsilon_{st} \qquad (1\text{-}1)$$

The primary outcome is $\ln(births_{st})$, representing the natural logarithm of the total birth count measured for women between the ages of 18 and 44, where s indexes state and t indexes time, expressed as a year/quarter combination. While most states implemented the Medicaid eligibility changes beginning January 2014, several states expanded later in our study period, namely: Indiana (2/2015), Michigan (4/2014), New Hampshire (8/2014) and Pennsylvania (1/2015). To account for this staggered state-level implementation, I assign $Post_t * Expansion_s = 1$ if Medicaid expansion is in place in state s at the expected time of conception t.

To control for differences in local labor market conditions that may influence women's childbearing decisions, I include conception year state unemployment and poverty rates in vector $\boldsymbol{X}_{st}$. Additional demographic covariates in $\boldsymbol{X}_{st}$ include fraction of births in a cell that is white, black, Hispanic, other and average maternal age. Equation (1) also controls for state fixed effects to control for unobserved time-invariant state characteristics, θ_s, and fixed effects for each quarter in the data, μ_t, to control for seasonality of births and to remove unobserved time-varying factors that are common to all mothers in each state. The regression includes the covariate $\ln(pop_{st})$ which denotes the natural logarithm of the state population of women aged 18-44. I estimate equation (1) separately for women with low educational attainment and for first-time mothers as coverage gains due to the ACA Medicaid eligibility changes have been larger for childless adults compared to parents (Kaestner et al., 2017). Additionally, as a falsification test, I examine fertility among highly educated women in the sample – those with a master's degree and higher educational attainment. I hypothesize that broadening of Medicaid eligibility for low-income individuals under the ACA are unlikely to affect childbirths among highly educated mothers as Medicaid eligibility is negatively correlated with mother's educational attainment.

To identify the impact of Medicaid expansions on fertility, I assume that absent the policy change, fertility would have evolved similarly across the treated and non-treated states over time. While the counterfactual cannot be tested directly, I evaluate the existence of differential pre-treatment trends in the outcome using the following equation:

$$\ln(births_{st}) = \beta_0 + \beta_1 \sum_t I(Year, Qtr_t) * Expansion_s + \beta_2 Post_t + \beta_3 Expansion_s + \beta_5 \ln(pop_{st}) + \beta_4 \boldsymbol{X}_{st} + \theta_s + \mu_t + \varepsilon_{st} \quad (1\text{-}2)$$

If the change in fertility is due to the Medicaid coverage expansions, the coefficient on the interaction terms $I(Year, Qtr_t) * Expansion_s$ should be close to zero prior to the policy change. In all models the standard errors are adjusted for clustering at the state level. The vectors $\boldsymbol{X}_{st}$, θ_s and μ_t are defined as before. I obtained state female population between ages 18 to 44 from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) Program to use as weights for all reported regressions with fertility as the dependent variable.

1.3.3. **Results**

Before reporting the results from empirical analysis of fertility, I begin with Figure 1-9 which depicts how the use of Medicaid-paid prescription contraceptives have evolved since 2012 through 2016 in the expansion and non-expansion states, using the administrative Medicaid data. Specifically, I use data from the Centers for Medicaid and Medicare Services' (CMS) State Drug Utilization Database (SDUD), which records total Medicaid funded prescriptions in each state. These data have been used in prior research on Medicaid policy (Alpert et al., 2013; Simon et al., 2009) and the recent health reform (Wen et al., 2016). These data show that the pre-expansion trends follow a similar pattern across both groups with a sharp trend break in the expansion states after the policy implementation in the first quarter of 2014. This suggests that the ACA Medicaid expansions were associated with substantial increases in Medicaid birth control utilization and is consistent with findings in the prior literature. With these trends in prescription contraceptive access as background, I now turn to the results of my empirical specification to examine women's childbearing decisions.

Panel A of Table 1-1 presents the results of estimating equation (1) for a high-impact group – the sample of women without a high school diploma. Tabulations from the American Community Survey suggest that 35% of reproductive-age women without a high school diploma and 19% of those without a college degree are covered by Medicaid, as shown in Figure 1-8. In contrast, Medicaid covers only 3% of women with a master's degree or higher education. As Medicaid participation is negatively correlated with educational attainment, I explore fertility responses among high school dropouts to proxy for low-income. The estimates in Panel A indicate that following Medicaid expansion, fertility among this group decreased significantly. For the sample of high school dropouts, total fertility (log total births) decreased by 3.4%, and by 6.6% among those giving birth for the first time. In addition, fertility decreased 2.3% among parous women, and this effect is smaller in magnitude compared to that for first time mothers.

Recent empirical evidence from the ACA Medicaid expansions indicate that Medicaid coverage increased 24% among parents, compared to a 53% increase for childless adults, in the less-educated populations (Kaestner et al., 2017). Furthermore, unintended pregnancies are far more common among less-educated women than those with higher educational attainment (Finer and Zolna, 2016). Consistent with these findings in the literature, the results in Panel A demonstrate that the Medicaid expansions had a negative impact on childbirths among high school dropouts and that the effects were concentrated among those giving birth for the first time. In addition to maternal education, using marital status as a proxy for pregnancy wantedness, I find a 5.7% decline in fertility among single women who are giving birth for the first time, as shown in column (4) of Table 1-1. This indicates that unintended births among less-educated mothers declined as a result of the ACA Medicaid expansions.

As an extension and as an alternative mechanism for identifying women who are more likely to be participating in Medicaid, I examine fertility responses among women without a college degree. As stated previously, similar to high school dropouts, women with a high school diploma or with some college experience but did not complete college are more likely to be on Medicaid than their counterparts with higher educational attainment. If estimated effects are due to changes in Medicaid coverage, the effects on fertility are expected to be more pronounced for the disadvantaged subgroups. The results for mothers without a college degree are displayed in Panel B of Table 1-1. The point estimates suggest that fertility decreased overall in this subsample and within subgroups by parity and marital status. However, the decrease is statistically significant only for high parity births. Notably, as predicted by the implicit negative correlation between Medicaid eligibility and educational attainment, the treatment effects for this group of mothers without a college degree are in general smaller in magnitude and statistically insignificant compared to the corresponding estimates for their counterparts who are high school dropouts.

To ensure that contemporaneous factors are not driving these results, I present a falsification test. I examine fertility among highly educated women in the spirit of a placebo test as this group is expected to be least affected by the policy change. I proceed by estimating equation (1) for the sample of women with a master's degree or higher education. The underlying hypothesis here is that the Medicaid expansions for low-income populations are far less likely to affect coverage among highly educated mothers. Hence, the Medicaid eligibility expansions should not affect childbirths among this group. These results are presented in Table 1-2, and while they are negatively signed, these estimates are statistically insignificant. This falsification test shows that there is no effect of the policy change on childbearing among highly educated women, which provides implicit support for the identification strategy.

Having detected effects on fertility among subgroups that are most likely to gain coverage through the Medicaid expansions (extensive margin), I next consider whether there is a differential impact on various maternal age subgroups. I stratify the sample in to 4 mutually exclusive categories based on mother's age at the time of birth: age 18-24, 25-30, 31-35, and 36-44. These results are displayed in Table 1-3. Overall fertility fell significantly across each age group by 2-4%. There is also a statistically significant decline in first births among the younger groups, and among those who are unmarried. High parity births decreased across all the categories. The negative impact among younger first-time mothers who are unmarried suggests that there is a reduction in unintended pregnancies, as marital status proxies for intendedness of pregnancies. As Medicaid eligibility for parents was already more generous before the ACA, younger, near-poor women are more likely to have gained coverage through the expansions. Thus, higher contraceptive access through coverage gains may lead to a negative impact on childbearing within this subsample. This is broadly consistent with studies that find Medicaid contraceptive utilization to have risen following the 2014 expansions.

1.3.4. **Robustness and Specification Checks**

Identification in the difference-in-differences framework relies on the assumption of parallel trends – that absent the 2014 Medicaid expansion, trends in outcomes would not have differed significantly across expansion and non-expansion states. While this assumption is not directly testable, I compare trends in fertility among less-educated women across the treatment and comparison states prior to the policy change. This approach allows an explicit examination of the validity of the control group by testing for differences in pre-treatment trends in outcomes across the two groups. To test whether the observed decrease in fertility in the main analysis is a true effect of the policy change, I run event-time regressions using the specification represented by equation (2), where Q3 of 2011 is the reference period. For the parallel trends assumption to be valid, the interaction terms in 2011, 2012 and 2013 should be statistically indistinguishable from 0.

The point estimates for less-educated women are presented in Appendix Tables A-3 and A-4 and are displayed graphically in Figures 1-2 and 1-3. The results in these tables show that there is some evidence of statistically significant declines in fertility among high school dropouts following the implementation of the reform consistent with the regression results in Table 1-2. However, there are also some statistically significant differential pre-trends for both education groups. While majority of the coefficient estimates are negatively signed in the post policy period with a few significant at conventional levels, there is, however, no systematic pattern of either growth or decline in treatment effects over time.

To address this issue of differences in pre-treatment trends across states that expanded Medicaid eligibility and states that did not, I examine the policy impact on fertility using a synthetic control methods approach (Abadie et al., 2010). This method involves creating a control group, from the pool of untreated states, that more closely resembles the characteristics of the treated states in period preceding the policy change. This control group is chosen by selecting a set of weights that minimizes the pre-treatment differences between the treatment and the control states in outcome and the observed demographic characteristics of the mother, including race/ethnicity and maternal age, and state unemployment and poverty rates. The pool of control states comprises of those that did not expand Medicaid during our study period. For constructing the synthetic control unit, I estimate a set of weights by matching the pre-treatment values of the outcome variable averaged over each conception year, and covariates averaged over the entire pre-treatment period. Figure 1-4 illustrates the results after applying this method to the sample of high school dropouts and provides a clear visual confirmation of the close match in pre-policy trends of the outcome in the treatment and synthetic control group before the policy change, which provides additional confidence in the validity of this approach. In the post-treatment period, the graphs reveal a discernible fall in first births overall and among single mothers.

The resulting synthetic control matching estimates are displayed in Table 1-4 for low-educated women and show the average treatment effect in the expansion states relative to the synthetic control states. For high school dropouts, Panel A displays the average treatment effect, and the corresponding p-values in row (2) are computed through a permutation method based on repeated randomized treatment assignment to states in the "donor pool" (randomized inference). For comparison, I also present the corresponding DD estimates from Table 1 in the rows below. There is a 4.5% and 4.1% reduction in childbirths to first time mothers and among first time mothers who are unmarried. These estimates are statistically significant as implied by the p-values of 0.065 and 0.085, and are similar in magnitude to the DD estimates in magnitude and direction. The estimates for total births and high parity births in this subgroup are however not significant. However, the similarity in direction, magnitude of the DD and synthetic control estimates lend support to a causal interpretation of the impact of the ACA Medicaid expansions on fertility.

I also conduct a similar synthetic control matching analysis for births to women without a college degree. While the estimates in Panel B of Table 1-4 are signed similarly as the DD estimates from Table 1-1 (except for high parity births), they are imprecisely estimated suggesting that the effects of Medicaid expansion on fertility of women with higher than high school education are less robust than among those with less than high school education. Results from a similar analysis examining childbirths among women with master's degree or higher education are presented in Panel C. As stated previously, conceptually this is akin to conducting a placebo test on a group that is not expected to be affected by the policy change. Consistent with our DD analysis, the synthetic control matching estimates do not provide any evidence of a policy impact in this highly educated demographic subgroup.

Next, I examine childbirths by maternal age and the corresponding synthetic control matching estimates are shown in Table 1-5. These results are quite similar and reinforce the main findings from the DD model in Table 1-3, specifically for women aged 18-24. Among this group, overall fertility decreased by 2.2%, by 3.3% for first births and by 6.1% for unmarried first-time mothers. The corresponding DD estimates for these subsamples are 3.3%, 3.1% and 3.9% respectively. While the estimate for high parity births is negative, it is statistically insignificant. For the older subgroups, synthetic control estimates are all statistically insignificant, and in some cases opposite in sign relative to the DD estimates. These results confirm that the impact of the coverage expansion and subsequent broader access to contraceptives was concentrated among younger women, that were likely unintended pregnancies. Because the effects from synthetic matching are comparable to the results from the DD models, and because the DD method is more transparent even though susceptible on grounds of parallel trends assumptions, I use those in my main analysis tables.

Taken together, these results provide evidence that the ACA Medicaid expansions led to a decline in fertility among low educated mothers. Specifically, for the sample of mothers who are giving birth for the first time and did not complete high school, a group that is most likely to be affected by the Medicaid eligibility extensions, fertility decreased 4.5 – 6.6 %, and by 4.1 – 5.7% among those who are single mothers in this subgroup. There is no clear evidence of change in childbirths among women without a college degree. As a placebo test, I find no evidence of any change in fertility among highly educated mothers. These results are consistent with research that finds the Medicaid expansions under the ACA to have increased coverage and access to care among low SES groups. Overall these findings on fertility responses also support the idea that there is more "bite" from these ACA Medicaid expansions which provides more comprehensive coverage prior to pregnancy compared to previous Medicaid expansions that specifically targeted income-eligible parents and pregnant women. One possible reason is that complementarity between subsidized physician visits and zero cost-sharing contraception in the preconception period, which lowers the cost of pregnancy prevention is manifested through the reductions in childbirth.

1.3.5. **Heterogeneous Effects on Fertility**

The findings of Kearney and Levine (2009) and Lindrooth and McCullough (2007), which show that state-level policies that extend Medicaid family planning services to low-income populations have a positive impact on contraceptive use and a negative impact on fertility, would predict that incremental coverage through ACA Medicaid would have little or no impact on fertility in expansion states with pre-existing Medicaid family planning policies. Studies evaluating state and federal policies allowing access to over-the-counter (OTC) emergency contraceptives finds no significant effect on fertility (Gross et al., 2014; Mulligan, 2016). However, findings of Ghosh et al. (2017) suggests that there may have been gaps in coverage through Medicaid family planning services and one may expect Medicaid-paid contraceptives to increase in response to the 2014 policy change. Thus, to test between these two competing hypotheses, I categorize states into 4 groups that vary in "bite" of the expansion based on prior state Medicaid family planning policies.

As of this writing, low-income women who are otherwise categorically and income ineligible for traditional Medicaid qualify for standalone family planning services through Medicaid in 26 states (Ranji et al., 2016). Using this variation in access to subsidized contraceptives across states, for this analysis, I classify the states as follows, in increasing order of expected treatment intensity: light (no Medicaid expansion/ no waiver), moderate (expansion/ pre-ACA waiver) and heavy (expansion/ no waiver). For this analysis, the base group comprises a pool of states where the policy impact of expanded access to contraceptives is expected to be negligible – those not expanding Medicaid and with pre-existing waivers or state plan amendments (SPA) for the provision of Medicaid family planning services to low-income populations. Appendix Table 2 presents the grouping of states by their pre-ACA status of Medicaid family planning programs and Medicaid expansion status under the ACA in 2014.

There is considerable variation in coverage of family planning services across states, which provides the basis for this identification. To isolate the effect of treatment intensity on fertility I estimate the following equation:

$$\ln(births_{st}) = \beta_0 + \beta_1 Post_t * Heavy_s + \beta_2 Post_t * Moderate_s + \beta_3 Post_t * Light_s + \beta_4 Heavy_s + \beta_5 Moderate_s + \beta_6 Light_s + \beta_7 Post_t + \beta_8 \boldsymbol{X}_{st} + \beta_9 \ln(pop_{st}) + \theta_s + \mu_t + \varepsilon_{st} \quad (1\text{-}3)$$

Here the omitted category consists of the states in the base group. The terms $Heavy_s$, $Moderate_s$ and $Light_s$ are perfectly collinear with state fixed effects and drop out of the equation. The vectors $\boldsymbol{X}_{st}$,θ_s and μ_t carry the usual connotation as defined previously in equation (1-1). Because the ACA Medicaid expansions represent a larger policy change facilitating coverage gains and access to care relative to the states in the base group with waivers, the coefficients for the interaction terms corresponding to the heavy and moderate states are expected to be negative for fertility responses, indicating higher treatment intensity. Even though states in the $Light$ treatment group did not expand Medicaid in 2014, absent any pre-ACA family planning services' programs, one may expect contraceptive utilization to rise due to latent demand for subsidized contraceptives as a result of unmet family planning needs (August et al., 2016) and woodwork/welcome mat effect of increased enrollment among those who were previously Medicaid-eligible but did not enroll (Frean et al., 2017). As a result, childbirths in this group may decline.

Results from the estimation of equation (3) can be found Tables 1-6 and 1-7, for the subsamples stratified by maternal education and by mother's age at birth. Panel A of Table 1-6 presents the estimates for high school dropouts. The estimated coefficients for fertility are negatively signed which is consistent with the notion that coverage expansions would primarily reduce childbearing through increased access to contraceptives. This negative effect on fertility across the three groups of states also accords with the DD results presented before. In addition, the effects are generally larger for the subgroups representing first-births. Splitting the states by expansion status and family planning services' programs allows me to tease out the effect of policy-induced changes in access to contraception on fertility, indicating that there were heterogeneous treatment affects. However, the estimates for the three treatment groups are not statistically different from each other, and the estimates for heavy treatment states are generally smaller in magnitude relative to the remaining groups.

Table 1-7 shows that fertility among first-time mothers aged 18-44 declined significantly in all three treatment groups. The results for the other groups are similar although not always precisely estimated. The analyses presented here are merely suggestive and without a clear causal interpretation. Nonetheless, these results support an interpretation that the policy effect on childbirths exhibited considerable heterogeneity across states based on pre-ACA access to Medicaid-financed family planning services.

In investigating the connections between health insurance and fertility, there are several mechanisms potentially at work. It is quite plausible that Medicaid expansions do not directly influence childbirths other than through an increased access to contraception. This would be violated, however, if pregnant women strategically relocated to states with expanded Medicaid eligibility and better access to care. While not examining pregnant women per se, Goodman (2017) investigates this possibility and finds no empirical support for cross-state migration in response to the Medicaid expansions in 2014. The other obvious mechanism is through increased use of contraceptives. Although I examined this issue in a cursory manner in Fig 1-1 to motivate the fertility analysis, I next examine the regression magnitudes and robustness checks need to now examine magnitude of contra effect in regression to be able to gauge the plausibility of magnitudes and mechanisms.

1.4. Estimating the Impact of 2014 Medicaid Expansions on Prescription Contraceptives

1.4.1. Data

I next test whether Medicaid expansions affect contemporaneous changes in Medicaid contraceptive utilization using administrative data on Medicaid-paid prescription drugs. The primary source for data on Medicaid-financed prescription contraceptives for our analysis is the State Drug Utilization Database (SDUD), compiled by the Centers for Medicaid and Medicare Services (CMS) using data submitted by state Medicaid programs. The SDUD includes aggregate utilization by NDC in each quarter from the universe of covered outpatient prescriptions for which Medicaid serves as a third-party payer (U.S. Department of Health and Human Services, 2012) from all 50 states and the District of Columbia. While the SDUD has included information from fee-for-service (FFS) since its inception, data on prescriptions financed by managed care (MC) plans were added to the SDUD in March 2010 following implementation of the Drug Rebate Equalization Act (2009). I use SDUD data in all quarters from 2012 to 2016 at the state-level, yielding 20 periods of data: 8 and 12 periods of pre- and post-treatment data, respectively.[2]

1.4.2. Estimation strategy

To understand the mechanism behind the observed decline in fertility due to expanded Medicaid coverage, I turn to examine changes in Medicaid-paid contraception use in response to these program expansions. Cross-state variation in state Medicaid expansion status before and after 2014 facilitates identification, and I estimate a regression model of the form, following (Ghosh et al., 2017):

$$\ln(Y_{st}) = \beta_0 + \beta_1 Post_t * Expansion_s + \beta_2 Post_t + \beta_3 Expansion_s + \beta_4 \boldsymbol{X}_{st} + \theta_s + \mu_t + \varepsilon_{st} \qquad (1\text{-}4)$$

The level of analysis here is a state-time cell with s indexing state and t indexing time, expressed as a year/quarter combination. The dependent variable here is the logged Medicaid prescription contraceptives per 100 women aged 18-44, where source for state female population between ages 18 to 44 are the National Cancer Institute's Surveillance,

[2] I exclude Arizona, North Carolina and Rhode Island from all contraceptive utilization analysis due to odd data patterns in at least one quarter during the study period.

Epidemiology, and End Results (SEER) Program data. Note that, in addition to states that implemented Medicaid expansions late in 2014 or 2015, data on contraceptive utilization covers the calendar year 2016, during which MT (1/2016) expanded Medicaid. Hence, $Post_t * Expansion_s$ takes the value of 1 if Medicaid expansion is in place in state s at time t, to account for this staggered policy implementation at the state-level. To control for differences in local labor market conditions that may influence contraception use, the vector $\boldsymbol{X}_{st}$ includes state unemployment and poverty rates. Equation (1-4) also includes state fixed effects and year-quarter fixed effects denoted by the vectors θ_s and μ_t respectively.

To identify the impact of Medicaid expansions on contraceptives, the implicit assumption is that the outcome would have evolved similarly across treated and non-treated states, absent any policy change. As an indirect evaluation of the preceding assumption, I empirically the existence of differential pre-treatment trends in the outcome using the following equation:

$$\ln(Y_{st}) = \beta_0 + \beta_1 \sum_t I(Year, Qtr_t) * Expansion_s + \beta_2 Post_t + \beta_3 Expansion_s + \beta_4 \boldsymbol{X}_{st} + \theta_s + \mu_t + \varepsilon_{st} \quad (1\text{-}5)$$

Here, $Expansion_s$ switches to 1 if a state has expanded Medicaid, which is interacted with year-quarter dummies (the reference period is 2012Q1), instead of the $Post_t$ indicator. If change in utilization is due to the Medicaid coverage expansions, the coefficient on the interaction terms $I(Year, Qtr_t) * Expansion_s$ should be close to zero prior to the policy change. One would also expect that wider access to preconception would increase utilization, and hence post-2014 estimates should be positive. In all models the standard errors are adjusted for clustering at the level of the state.

1.4.3. **Results**

Federal rules require that Medicaid programs in states that have implemented eligibility expansions for low-income populations under the ACA guidelines, to provide the newly eligible Medicaid beneficiaries preventive services, including prescription contraceptives, without any cost-sharing, as part of the ten "essential health benefits" (Centers for Medicare Medicaid Services, 2016).

Table 1-8 presents differences-in-differences results from estimating the specification in equation (1-4) for aggregate Medicaid prescription birth controls. The pre-period is 2012-2013, and the post-period is 2014-2016. The estimate in column (1) shows that the effect of the 2014 ACA Medicaid expansions was 24% increase in contraceptive utilization that was financed by Medicaid. This finding is very similar to the 22% policy impact detected by Ghosh et al. (2017) for Medicaid-paid contraceptives using proprietary pharmacy claims database, although those data only went through Q1 of 2015. Hence, this present analysis adds 7 additional quarters of post-treatment data. The fact that contraceptive utilization increased in response to the policy change provides evidence that expanded ACA Medicaid coverage led to fertility declines through increased access to prescription contraception.

1.4.4. **Robustness and Specification Checks**

Next, I use an event study framework, as outlined by equation (1-4), to understand the impacts of the expansion. In addition to allowing for the examination of the assumption of parallel trends that is critical for identification in a DD study design, this approach offers the added advantage of an examination of the change in policy impact over time. For the parallel trends assumption to be valid, the interaction terms in 2012 and 2013 should be statistically insignificant. Appendix Table A-5 shows the results from this estimation and Figure 6 plots these estimates, where the reference period is Q1 of 2012. The point estimates for the interaction terms in 2012 and 2013 are indeed quite small in magnitude (and not statistically significant) relative to the difference-in-difference estimate in Table 8, indicating that no significant differential trends appear in the pre-expansion period that would otherwise threaten the DD identification strategy. The statistically significant estimates starting in Q3 of 2014 demonstrate that utilization of overall Medicaid prescription contraceptives increased significantly within a few months from the policy implementation and the magnitude of the policy impact increased over time through 2015, as more states implemented expansions in a staggered timeline, and finally levelling off in 2016. These results also demonstrate that the policy impact on Medicaid prescription birth control grew over time which is broadly accords with the fact that prior to consuming contraceptives, new beneficiaries must sign up for Medicaid coverage and schedule a visit to a medical provider to obtain a prescription.

To address this issue of pre-treatment differences in trends across states that expanded Medicaid eligibility and states that did not, I use synthetic control method to examine the policy impact on aggregate Medicaid prescriptions. For this analysis, from the pool of states that did not expand Medicaid, I construct a synthetic control unit that closely matches trends in the expansions states in the pre-treatment period. For each outcome, I estimate a set of weights by matching the pre-treatment values of the outcome variable averaged over each year, and covariates averaged over the entire pre-treatment period.

Figure 1-7 plots trends in aggregate Medicaid contraceptives in expansion states and the synthetic control states. The difference between the solid blue line (outcome in expansion states) and the dashed line (outcome in synthetic control unit) before the treatment period indicates the quality of the fit. The graph demonstrates that aggregate Medicaid prescription utilization in the expansion and synthetic control states followed a similar trend in the pre-policy period, and there is a sharp divergence in trends following the policy change, as one would expect.

The estimated treatment effect on Medicaid contraceptives and the corresponding p-value from this analysis are displayed in column (2) of Table 1-8. There is a statistically significant 30% increase in utilization of birth control financed by Medicaid. This supports the estimates from the main DD analysis. The similarity of the effect size and statistical significance of the estimates obtained through DD analysis and synthetic control method provides additional confirmation that the quasi-experimental identification strategy allows me to derive causal impacts of the effect of Medicaid expansion on contraceptive utilization.

1.4.5. **Heterogeneous Effects on Contraceptive Utilization**

While one would expect the Medicaid expansions to increased access to contraception in all states implementing the policy, the existence of pre-ACA Medicaid family planning programs for the near-poor may attenuate the such effects, relative to states without waivers. The results in the previous section provide suggestive evidence of heterogeneous impact of the expansions on fertility responses across states with and without Medicaid family planning programs. One potential explanation for this heterogeneity is that the impact of expanded Medicaid on use of birth control may have been higher in states without pre-existing programs that provided family planning services to the near-poor compared to states with relatively higher access. To test whether contraceptive utilization patterns evolved similarly, I use an estimating equation similar to equation (1-3), which is of the following form:

$$\ln(Y_{st}) = \beta_0 + \beta_1 Post_t * Heavy_s + \beta_2 Post_t * Moderate_s + \beta_3 Post_t * Light_s + \beta_4 Heavy_s + \beta_5 Moderate_s + \beta_6 Light_s + \beta_7 Post_t + \beta_8 \boldsymbol{X}_{st} + \theta_s + \mu_t + \varepsilon_{st}$$

(1-6)

Table 1-9 presents results from the estimation of equation (1-6). The main estimates suggest that a significant increase in contraceptive utilization is present in all three categories of treatment intensity, namely $Heavy_s$, $Moderate_s$ and $Light_s$. While the corresponding estimates for each of these categories is positive, the effect is statistically significant for only the heavy and moderate expansion groups, with the heavy treatment states experiencing the greatest increase in aggregate Medicaid contraceptives. The policy effect somewhat smaller for moderate treatment states, while the corresponding estimate for light treatment states is positive but smaller in magnitude and statistically insignificant. Specifically, having an expansion in effect in a state along with no previous family planning services' coverage increased contraceptive usage by 35% and this estimate is statistically significant. In expansion states with these programs, contraception increased significantly by 26%. Although there is a 24% rise in contraceptives in states classified as light states, it is statistically insignificant. This positive effect on contraceptive utilization in light treatment states is consistent with the "woodwork effect" of increased Medicaid coverage following the expansions in 2014, even in states that did not expand Medicaid (Frean et al., 2017).

Overall, these results demonstrate that the effect of the Medicaid expansions on contraceptives is the strongest for the heavy treatment states – those with ACA Medicaid expansions but no Medicaid family planning programs. The results also show that rise on contraceptive utilization is correlated with the treatment intensity of the expanded coverage. In other words, the policy effect on use of contraceptives was larger in expansion states where pre-ACA access to subsidized family planning services was relatively lower. The fact that I detect significant increases even in expansion states where prior to 2014

Medicaid-financed family planning services were available for low-income populations, suggests that the ACA expansions are closing gaps in access to family planning services, and that there may be unmet preconception care needs in non-expansion states despite the presence of these programs for low-income populations.

1.5. Discussion and Conclusion

This study exploits policy-driven variation in Medicaid expansion across states to identify the causal effect of expanded coverage on fertility and contraceptive use. The contribution of this paper rests in not only examining the linkage between insurance access and fertility for a disadvantaged population that has not been studied previously, but more importantly, it departs from previous literature by providing evidence from a nationwide analysis that extending comprehensive Medicaid coverage, that includes preconception care in addition to pregnancy-related care, reduces childbirths through an increase in contraceptive utilization. I find that the ACA Medicaid expansions led to reductions in fertility among less-educated and younger women. Specifically, for first-time mothers without a high school diploma, a group that is most likely to be affected by the Medicaid eligibility extensions, fertility decreased 4.5 to 6.6 %. For unmarried women in this subgroup, fertility fell by 4.1 to 5.7. I also document a similar pattern of decline in childbirths for younger mothers. Overall fertility decreased by 2.2 – 3.3% for women between ages 18-24. The policy impact was generally stronger for those giving birth for the first time (3.1 - 3.3%), and for unmarried first-time mothers (3.9 - 6.1%) within this subgroup. These findings for first-time births to unmarried mothers suggest that unintended births fell following the expansions. In a falsification test, I show that there is no evidence of change in fertility among highly educated mothers, as Medicaid participation is known to be negatively correlated with educational attainment,

Health insurance may reduce childbirths through increased utilization of contraception, which would be otherwise unaffordable. Considering this potential mechanism, using administrative Medicaid prescription drugs data, I show that a contemporaneous 24% increase in Medicaid-financed contraceptive utilization indicating greater access to subsidized contraception. I also document suggestive evidence of heterogeneity in the policy impact on fertility and contraceptive use. Specifically, the effect on these outcomes differed across states based on availability of Medicaid-funded family planning services through state programs, which suggests that the expansions increased access to contraceptives in areas with lesser pre-ACA access to subsidized contraceptives. An important policy implication of my finding is that, in contrast to previous Medicaid expansions for pregnant women and children led fertility to increase among a few subgroups, Medicaid coverage that includes preconception care can reduce unplanned births, which is an intended policy effect. Also, in the short-run, preconception coverage shifts utilization from more intensive child birth related care to less expensive pregnancy prevention care.

Apart from the public health insurance expansions, under the ACA, private health insurance plans were mandated to provide all FDA-approved prescription contraceptives without cost-sharing beginning August 1, 2012. One concern is that this policy change overlaps with the period of analysis in this study. However, there is a key difference between the contraceptive mandate and the Medicaid expansions I study in this paper. In contrast to the spatial and temporal variation in state Medicaid expansion decisions, the contraceptive mandate was implemented simultaneously in all the states starting August 2012. These policies may also have different effects as the populations eligible for Medicaid and those that are commercially insured exhibit different health and fertility preferences. Subject to the caveat that while information on insurance status at the time of conception is unavailable, future work with more comprehensive individual-level surveys may address whether other features of the ACA could have affected contraception and fertility, in addition to the Medicaid expansions. Nevertheless, the results from this study are informative about the potential mechanism behind the decline in childbirths due to the law.

There is high interest in understanding the extent to which coverage gains have affected health and well-being among low-income and vulnerable populations. This study contributes to the growing body of evidence on the recent healthcare reforms. While the results of this paper show that the expansions reduced fertility, it is also possible that higher contraceptive access does not affect total births over the lifetime of a woman and that the policy change simply delayed births. However, studies show that allowing more control over fertility decisions have a positive effect on women's labor market outcomes (Bailey, 2006; Goldin and Katz, 2002). Hence, there might be downstream non-health effects associated with the reduction in unintended births in the long-run among cohorts exposed to this policy change.

Improving access to contraception and reducing unplanned births was a stated policy goal for the recent healthcare reforms. The results from this study provide evidence that comprehensive Medicaid coverage which includes preconception care reduces unintended births through improvements in prescription-based contraception. Yet, current debates surrounding health policy have increasingly focused attention on reducing insurance coverage, both public and private, for family planning services. As states continue to debate the merits of the expansions to the Medicaid program under the ACA and terms of coverage for beneficiaries, it is also important to consider the implications of these changes in childbearing and timing of birth for infant health outcomes.

1.6. **Tables and Figures**

Table 1-1: **DD estimates of the effect of 2014 ACA Medicaid expansions on fertility among low educated women**

Dependent variable: Ln (Births)				
	(1)	(2)	(3)	(4)
	Total births	First births	High parity	First births, Single
		Panel A: High school dropout		
Post x Expansion	-0.034**	-0.066**	-0.023*	-0.057**
	(0.014)	(0.028)	(0.013)	(0.028)
Observations	624	624	624	624
		Panel B: Less than college degree		
Post x Expansion	-0.015	-0.004	-0.020**	-0.008
	(0.009)	(0.015)	(0.009)	(0.012)
Observations	624	624	624	624

Notes: Each column is from a separate difference-in-difference regression using NCHS 2012-2015, with natural log of total births as the dependent variable. Observations are at the at state/conception year-quarter/cell level. Sample includes births to women aged 18-44. All models include state fixed effects, fixed effects for each quarter in the data, poverty rate and unemployment rate. Standard errors are clustered at state and presented in parentheses. Estimates are weighted using state by year population of women aged 18-44. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

Table 1-2: **DD estimates of the effect of 2014 ACA Medicaid expansions on fertility among highly educated women (Placebo)**

Dependent variable: Ln (Births)				
	(1)	(2)	(3)	(4)
	Total births	First births	High parity	First births, Single
Post x Expansion	-0.019	-0.006	-0.027	-0.011
	(0.019)	(0.020)	(0.019)	(0.039)
Observations	624	624	624	624

Notes: Each column is from a separate difference-in-difference regression using NCHS 2012-2015, with natural log of total births as the dependent variable. Observations are at the at state/conception year-quarter/cell level. Sample includes births to women aged 18-44. All models include state fixed effects, fixed effects for each quarter in the data, poverty rate and unemployment rate. Standard errors are clustered at state and presented in parentheses. Estimates are weighted using state by year population of women aged 18-44. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

Table 1-3: **DD estimates of the effect of 2014 ACA Medicaid expansions on fertility, by age**

Dependent variable: Ln (Births)				
	(1)	(2)	(3)	(4)
	Total births	First births	High parity	First births, Single
		Panel A: Age 18-24		
Post x Expansion	-0.033***	-0.031***	-0.036***	-0.039***
	(0.009)	(0.010)	(0.009)	(0.012)
Observations	816	816	816	816
		Panel B: Age 25-30		
Post x Expansion	-0.037***	-0.026**	-0.043***	-0.040***
	(0.008)	(0.012)	(0.009)	(0.013)
Observations	816	816	816	816
		Panel C: Age 31-35		
Post x Expansion	-0.030***	-0.021	-0.035***	-0.014
	(0.006)	(0.014)	(0.007)	(0.020)
Observations	816	816	816	816
		Panel D: Age 36-44		
Post x Expansion	-0.017***	-0.009	-0.019**	-0.023
	(0.006)	(0.018)	(0.007)	(0.023)
Observations	816	816	816	816

Notes: Each column is from a separate difference-in-difference regression using NCHS 2012-2015, with natural log of total births as the dependent variable. Observations are at the at state/conception year-quarter/cell level. All models include state fixed effects, fixed effects for each quarter in the data, poverty rate and unemployment rate. Standard errors are clustered at state and presented in parentheses. Estimates are weighted using state by year population of women in the corresponding age group for each panel. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

Table 1-4: **Fertility by educational attainment (Expansion states versus synthetic control states)**

Dependent variable: Ln (Births)				
	(1)	(2)	(3)	(4)
	Total births	First births	High parity	First births, Single
		Panel A: High school dropout		
Treatment effect	-0.007	-0.045*	0.003	-0.041*
P-value	[0.456]	[0.065]	[0.929]	[0.085]
Observations	624	624	624	624
		Panel B: Less than college degree		
Treatment effect	0.0004	-0.006	0.002	-0.022
P-value	[0.571]	[0.530]	[0.847]	[0.341]
Observations	624	624	624	624

Panel C: Highly educated women

Dependent variable: Ln (Births)				
	(1)	(2)	(3)	(4)
	Total births	First births	High parity	First births, Single
Treatment effect	-0.018	0.019	-0.035	0.259
P-value	[0.817]	[0.964]	[0.708]	[0.845]
Observations	624	624	624	624

Notes: Data from NCHS 2012-2015, with natural log of total births as the dependent variable. Observations are at the at state/conception year-quarter/cell level. Sample includes births to women aged 18-44. Synthetic controls are constructed as the weighted average of control states that most closely resemble characteristics of the Medicaid expansion states in the pre-policy change period. The pool of control states comprises of those that did not expand Medicaid during our study period. For the synthetic control estimates, p-values are computed through Fisher permutation tests and presented in brackets. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

Table 1-5: **Fertility by age group (Expansion states versus synthetic control states)**

Dependent variable: Ln (Births)				
	(1)	(2)	(3)	(4)
	Total births	First births	High parity	First births, Single
Panel A: Age 18-24				
Treatment effect	-0.022**	-0.033**	-0.020	-0.061**
P-value	[0.042]	[0.028]	[0.051]	[0.028]
Observations	816	816	816	816
Panel B: Age 25-30				
Treatment effect	-0.013	-0.002	-0.030	0.090
P-value	[0.488]	[0.994]	[0.128]	[0.441]
Observations	816	816	816	816
Panel C: Age 31-35				
Treatment effect	0.004	0.042	-0.019	0.341
P-value	[0.805]	[0.532]	[0.175]	[0.463]
Observations	816	816	816	816
Panel D: Age 36-44				
Treatment effect	0.013	0.163	-0.008	0.348
P-value	[0.901]	[0.475]	[0.707]	[0.721]
Observations	816	816	816	816

Notes: Data from NCHS 2012-2015, with natural log of total births as the dependent variable. Observations are at the at state/conception year-quarter/cell level. Sample includes births to women aged 18-44. Synthetic controls are constructed as the weighted average of control states that most closely resemble characteristics of the Medicaid expansion states in the pre-policy change period. The pool of control states comprises of those that did not expand Medicaid during our study period. For the synthetic control estimates, p-values are computed through Fisher permutation tests and presented in brackets. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

Table 1-6: **Heterogeneous effects of 2014 ACA Medicaid expansions on fertility, by state Medicaid family planning waiver/SPA status**

Dependent variable: Ln (Births)				
	(1)	(2)	(3)	(4)
	Total births	First births	High parity	First births, Single
		Panel A: High school dropout		
Post x Heavy	-0.027	-0.073**	-0.009	-0.049
	(0.021)	(0.033)	(0.019)	(0.038)
Post x Moderate	-0.057***	-0.108***	-0.043**	-0.098***
	(0.019)	(0.026)	(0.018)	(0.027)
Post x Light	-0.034**	-0.087***	-0.020*	-0.094**
	(0.014)	(0.030)	(0.011)	(0.039)
		Panel B: Less than college		
Post x Heavy	-0.026**	-0.018	-0.030***	-0.018
	(0.010)	(0.017)	(0.007)	(0.019)
Post x Moderate	-0.031***	-0.036***	-0.029***	-0.046***
	(0.007)	(0.011)	(0.006)	(0.013)
Post x Light	-0.044**	-0.051***	-0.041*	-0.061***
	(0.017)	(0.015)	(0.021)	(0.015)

Notes: Each column is from a separate difference-in-difference regression using NCHS 2012-2015, with natural log of total births as the dependent variable. Sample includes births to women aged 18-44. Observations are at the at state/conception year-quarter/cell level. All models include state fixed effects, fixed effects for each quarter in the data, poverty rate and unemployment rate. Standard errors are clustered at state and presented in parentheses. Estimates are weighted using state by year population of women aged 18-44. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

Table 1-7: **Heterogeneous effects of 2014 ACA Medicaid expansions on fertility, by state Medicaid family planning waiver/SPA status**

Dependent variable: Ln (Births)				
	(1)	(2)	(3)	(4)
	Total births	First births	High parity	First births, Single
		Panel A: Age 18-24		
Post x Heavy	-0.030***	-0.034**	-0.027**	-0.037**
	(0.011)	(0.014)	(0.010)	(0.016)
Post x Moderate	-0.043***	-0.054***	-0.030***	-0.065***
	(0.009)	(0.011)	(0.009)	(0.014)
Post x Light	-0.020	-0.039**	0.004	-0.048**
	(0.016)	(0.018)	(0.015)	(0.018)
		Panel B: Age 25-30		
Post x Heavy	-0.034***	-0.040***	-0.030***	-0.050***
	(0.008)	(0.012)	(0.009)	(0.012)
Post x Moderate	-0.045***	-0.048***	-0.043***	-0.061***
	(0.012)	(0.014)	(0.012)	(0.014)
Post x Light	-0.022	-0.011	-0.027	-0.031*
	(0.014)	(0.012)	(0.019)	(0.016)
		Panel C: Age 31-35		
Post x Heavy	-0.030***	-0.005	-0.040***	0.005
	(0.008)	(0.017)	(0.009)	(0.026)
Post x Moderate	-0.039***	-0.027*	-0.044***	-0.008
	(0.009)	(0.016)	(0.010)	(0.020)
Post x Light	-0.015	-0.013	-0.015	-0.032
	(0.014)	(0.018)	(0.015)	(0.029)
		Panel D: Age 36-44		
Post x Heavy	-0.034***	-0.023	-0.037***	-0.005
	(0.008)	(0.023)	(0.007)	(0.030)
Post x Moderate	-0.026**	-0.009	-0.030**	0.023
	(0.011)	(0.019)	(0.012)	(0.030)
Post x Light	-0.008	-0.015	-0.005	0.004
	(0.012)	(0.025)	(0.015)	(0.053)

Notes: Each column is from a separate difference-in-difference regression using NCHS 2012-2015, with natural log of total births as the dependent variable. Observations are at the at state/conception year-quarter/cell level. Sample includes births to women aged 18-44. All models include state fixed effects, fixed effects for each quarter in the data, poverty rate and unemployment rate. Standard errors are clustered at state and presented in parentheses. Estimates are weighted using state by year population of women in the corresponding age group for each panel. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

Table 1-8: **DD estimates of the effect of 2014 ACA Medicaid expansions on Medicaid contraceptives**

Dependent variable: Ln (Medicaid prescriptions per 100 population)		
	(1)	(2)
	DD	Synthetic control matching
Estimate	0.24***	0.30***
	(0.07)	[0.001]
Observations	960	960

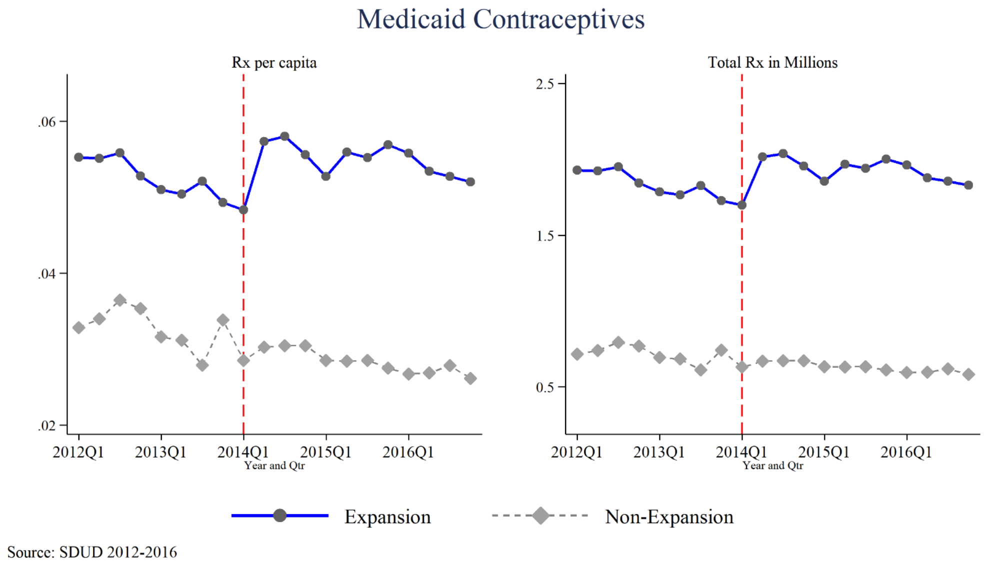
Note: Column (1) is from a separate difference-in-difference regression using SDUD 2012-2016, with natural log of total Medicaid contraceptives per 100 population as the dependent variable. Observations are at the at state/year/quarter level. Model includes state fixed effects, fixed effects for each quarter in the data, poverty rate and unemployment rate. Robust standard errors clustered by state are reported in parentheses.
Column (2) presents synthetic control matching results. Synthetic controls are constructed as the weighted average of control states that most closely resemble characteristics of the Medicaid expansion states in the pre-policy change period. The pool of control states comprises of those that did not expand Medicaid during our study period. For the synthetic control estimates, p-values are computed through Fisher permutation tests and presented in brackets. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

Table 1-9: **DD estimates of effect of the 2014 ACA Medicaid expansions on Medicaid contraceptives, by state Medicaid family planning waiver/SPA status**

Dependent variable: Ln (Medicaid prescriptions per 100 population)	
Post x Heavy	0.35***
	(0.09)
Post x Moderate	0.26***
	(0.07)
Post x Light	0.24
	(0.18)
Observations	960

Note: Reported estimates come from a difference-in-difference regression using SDUD 2012-2016, with natural log of total Medicaid contraceptives per 100 population as the dependent variable. Observations are at the at state/year/quarter level. All models include state fixed effects, fixed effects for each quarter in the data, poverty rate and unemployment rate. Robust standard errors clustered by state are reported in parentheses. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

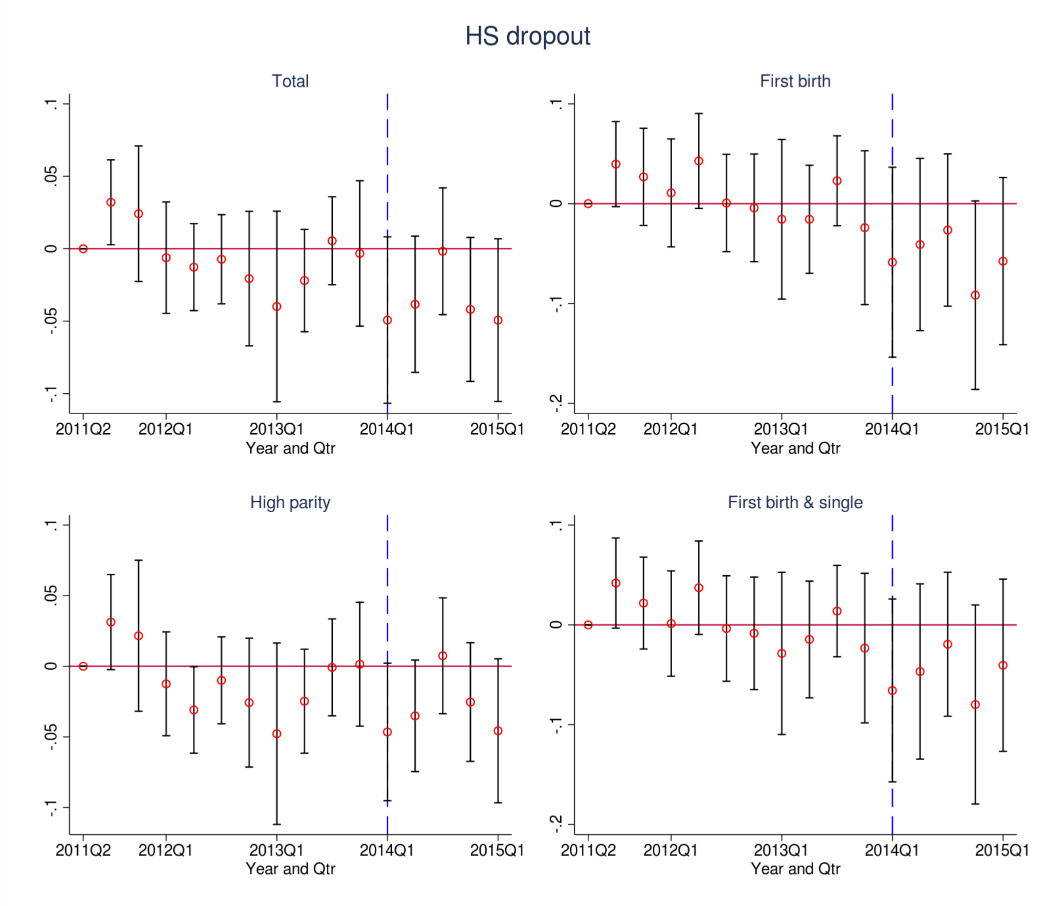
Figure 1-1: **Descriptive trends in Medicaid contraceptive utilization, 2012-2016**



Notes:

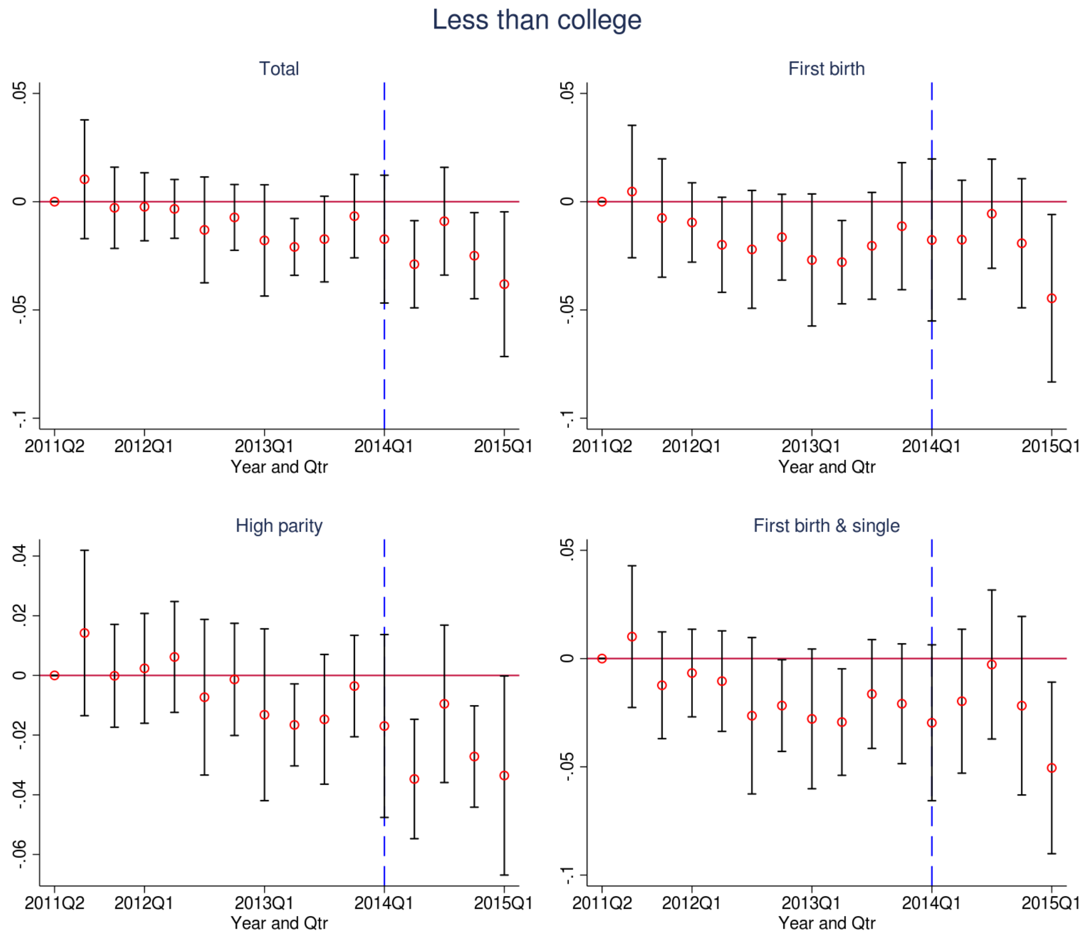
1. Each figure plots unadjusted trends in Medicaid prescription contraceptives. Data is aggregated to treatment/year/quarter level.
2. January 2014 expansion states include: AK, AR, AZ, CA, CO, CT, DC, DE, IN, HI, IL, IA, IL, KY, LA, MA, MD, MI, MN, NH, NV, NY, NJ, NM, ND, OH, OR, PA, RI, VT, WA, WV.
3. Non-expansion states include: AL, AK, FL, GA, ID, KS, LA, ME, MI, MO, MT, NE, NC, OK, SC, SD, TN, TX, UT, VA, WI, WY.
4. The red vertical line is drawn at the 1st quarter of 2014, indicating implementation of the 2014 ACA Medicaid expansions.

Figure 1-2: **Event study estimates of the effect of the ACA Medicaid expansions on fertility among women without a high school diploma**



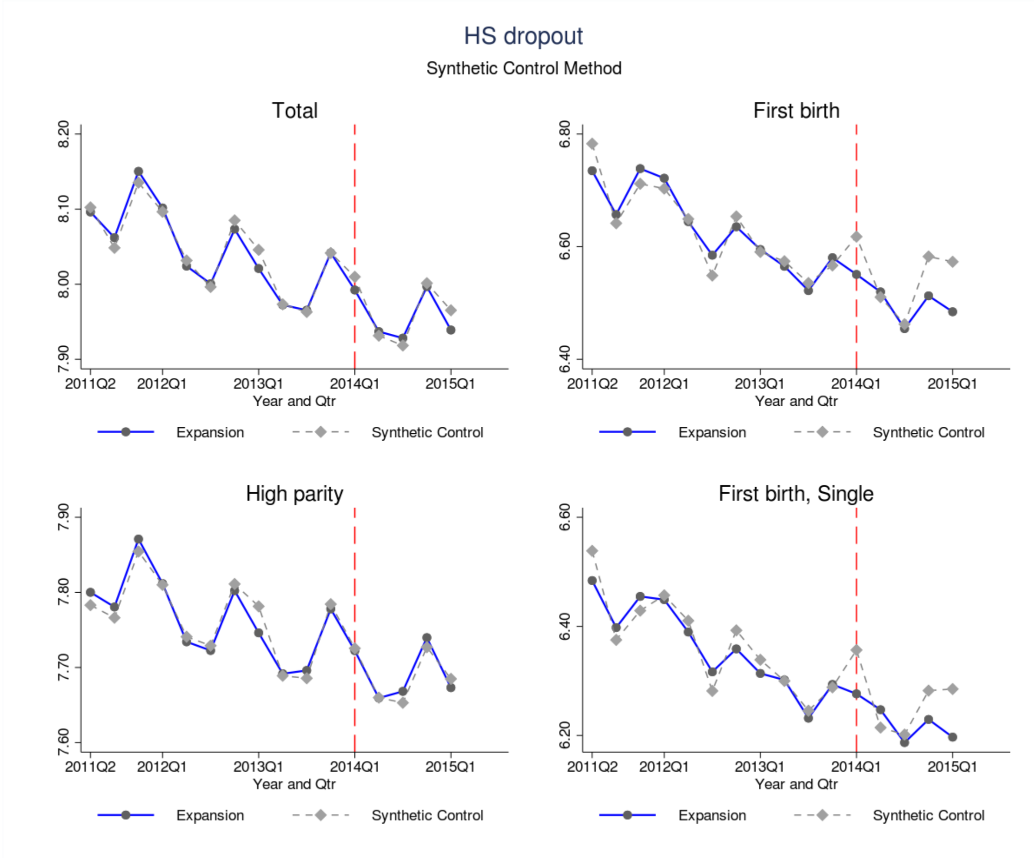
Notes: Each figure plots estimates from an event study analysis using NCHS 2012-2015, with natural log of total births as the dependent variable. Observations are at the at state/conception year-quarter/cell level. Sample includes births to women aged 18-44. Model includes state unemployment rate and poverty rate, state fixed effects, and fixed effects for each quarter in the data. Standard errors are clustered at state and presented in parentheses. Estimates are weighted using the state by year population of women aged 18-44. For each period, the red circle represents the coefficient estimate on the interaction term of expansion indicator and an indicator for the year-quarter of conception. The solid black vertical line represents the 95 percent confidence interval. The vertical dashed line in blue is drawn at the 1st quarter of 2014, with 2012Q1 being the reference period. See Appendix Table 3, columns 1-4, for corresponding estimates.

Figure 1-3: **Event study estimates of the effect of the ACA Medicaid expansions on fertility among women without a college degree**



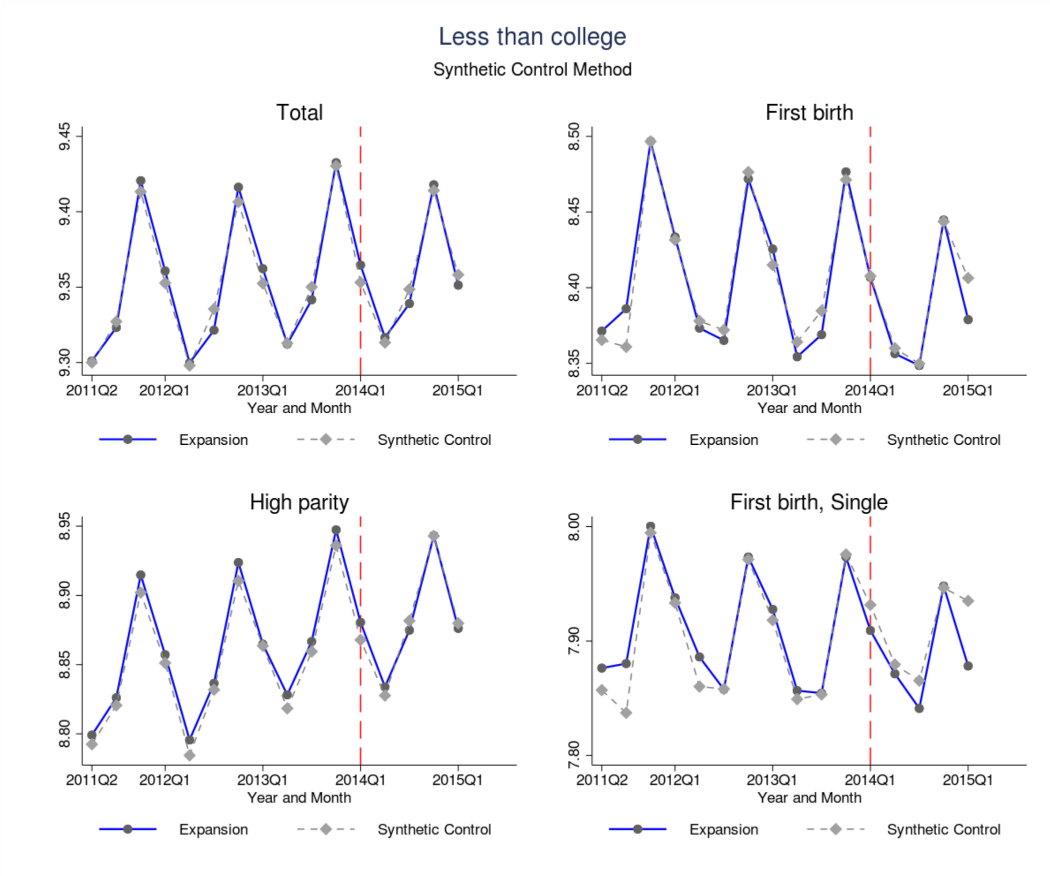
Notes: Each figure plots estimates from an event study analysis using NCHS 2012-2015, with natural log of total births as the dependent variable. Observations are at the at state/conception year-quarter/cell level. Sample includes births to women aged 18-44. Model includes state unemployment rate and poverty rate, state fixed effects, and fixed effects for each quarter in the data. Standard errors are clustered at state and presented in parentheses. Estimates are weighted using the state by year population of women aged 18-44. For each period, the red circle represents the coefficient estimate on the interaction term of expansion indicator and an indicator for the year-quarter of conception. The solid black vertical line represents the 95 percent confidence interval. The vertical dashed line in blue is drawn at the 1st quarter of 2014, with 2012Q1 being the reference period. See Appendix Table 3, columns 1-4, for corresponding estimates.

Figure 1-4: **Fertility among women without a high school diploma (Expansion states versus synthetic control states)**



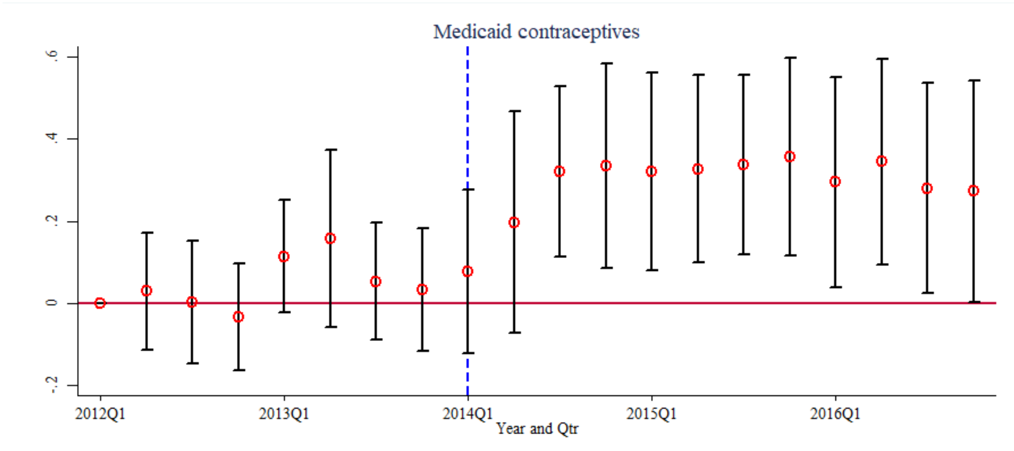
Notes: Data from NCHS 2012-2015, with natural log of total births as the dependent variable. Observations are at the at state/conception year-quarter/cell level. Sample includes births to women aged 18-44. Synthetic controls are constructed as the weighted average of control states that most closely resemble characteristics of the Medicaid expansion states in the pre-policy change period.

Figure 1-5: **Fertility among women without a college degree (Expansion states versus synthetic control states)**



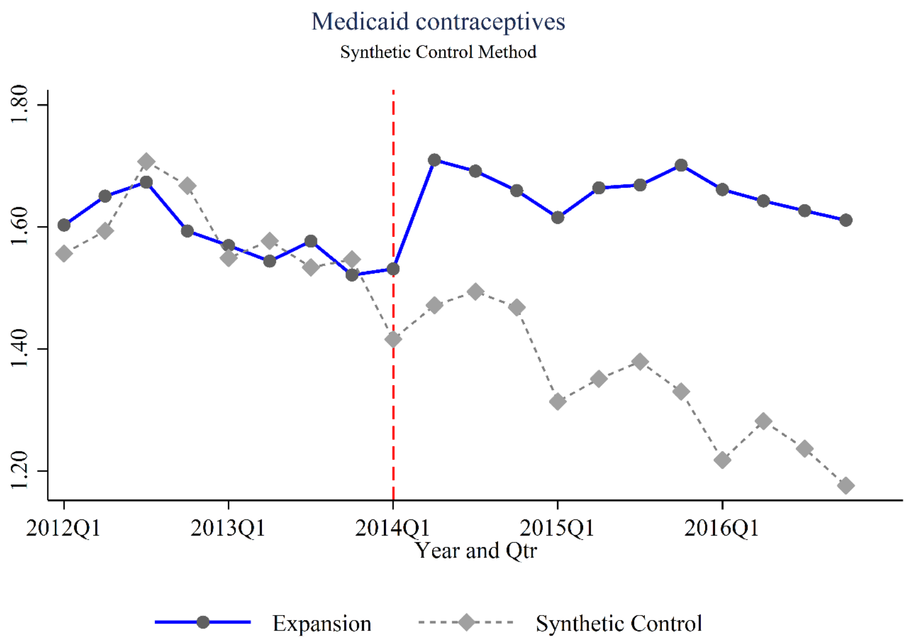
Notes: Data from NCHS 2012-2015, with natural log of total births as the dependent variable. Observations are at the at state/conception year-quarter/cell level. Sample includes births to women aged 18-44. Synthetic controls are constructed as the weighted average of control states that most closely resemble characteristics of the Medicaid expansion states in the pre-policy change period.

Figure 1-6: **Event study estimates of the effect of the ACA Medicaid expansions on aggregate contraceptive utilization**



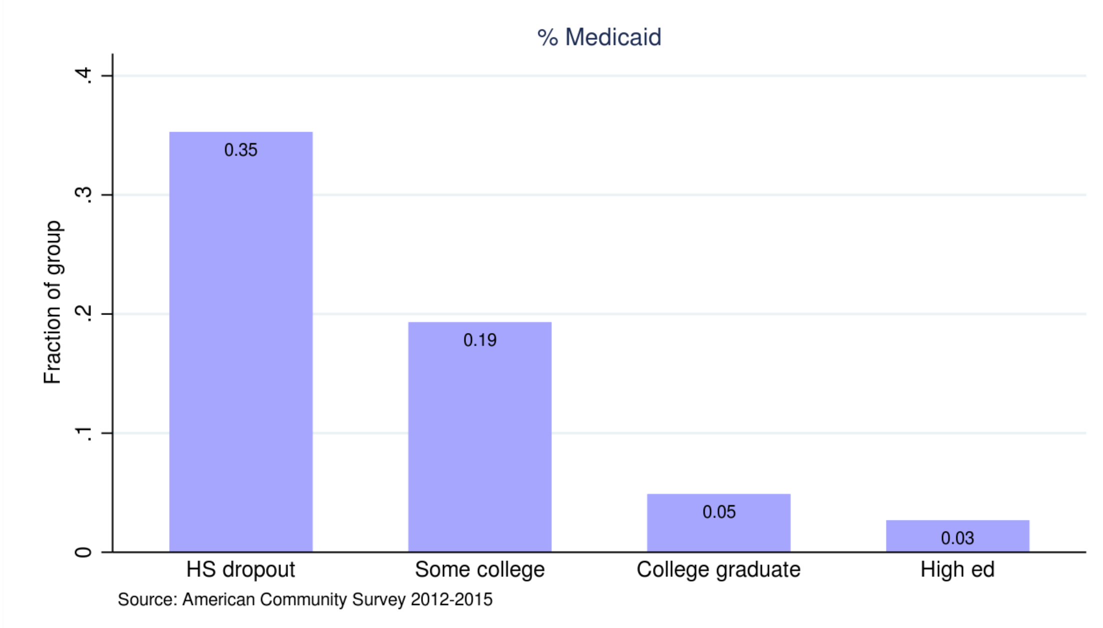
Notes: Each figure plots estimates from an event study analysis using SDUD 2012-2016, with natural log of total Medicaid contraceptives per 100 population as the dependent variable. Observations are at the state/year/quarter level. Model includes state unemployment rate and poverty rate, state fixed effects, and fixed effects for each quarter in the data. Standard errors are clustered at state and presented in parentheses. For each period, the red circle represents the coefficient estimate on the interaction term of expansion indicator and an indicator for the year-quarter in the data. The solid black vertical line represents the 95 percent confidence interval. The vertical dashed line in blue is drawn at the 1st quarter of 2014, with 2012Q1 being the reference period. See Appendix Table 4, columns 1-3, for corresponding estimates.

Figure 1-7: **Medicaid contraceptive utilization: Expansion states versus synthetic control states**



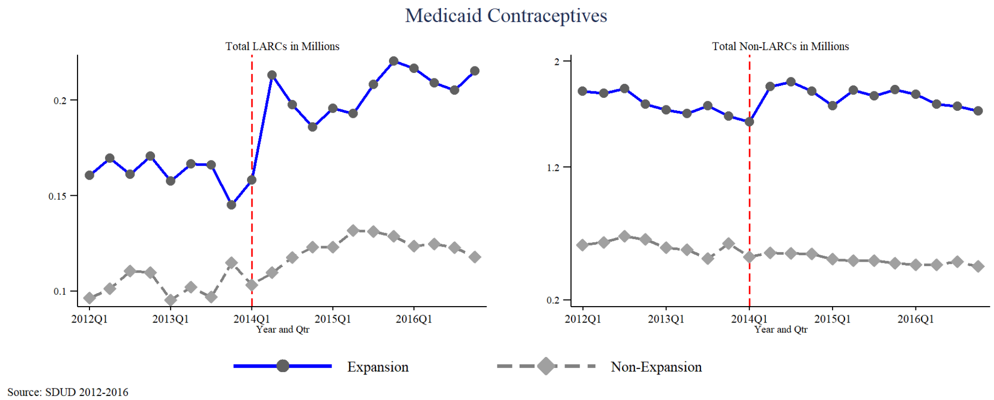
Notes: Data from SDUD 2012-2016, with natural log of total Medicaid contraceptives per 100 population as the dependent variable. Observations are at the at state/year/quarter level. Synthetic controls are constructed as the weighted average of control states that most closely resemble characteristics of the Medicaid expansion states in the pre-policy change period.

Figure 1-8: **Medicaid coverage rates among women, by educational attainment**



Notes: Sample drawn from ACS 2012-2015, and is based on women aged 18-44.

Figure 1-9: **Unadjusted trends in Medicaid contraceptive utilization**



Notes:

1. Each figure plots unadjusted trends in Medicaid prescription contraceptives. Data is aggregated to treatment/year level.
2. January 2014 expansion states include: AK, AR, AZ, CA, CO, CT, DC, DE, IN, HI, IL, IA, IL, KY, LA, MA, MD, MI, MN, NH, NV, NY, NJ, NM, ND, OH, OR, PA, RI, VT, WA, WV.
3. Non-expansion states include: AL, AK, FL, GA, ID, KS, LA, ME, MI, MO, MT, NE, NC, OK, SC, SD, TN, TX, UT, VA, WI, WY.
4. The red vertical line is drawn at the 1st quarter of 2014, indicating implementation of the 2014 ACA Medicaid expansions

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2 The Effect of Public Subsidies on Prescription Drug Use: Evidence from Recent Medicaid Expansions

2.1. Introduction

Prescription drugs represent one of the fastest-growing areas of healthcare spending (Martin et al., 2016) and remain a mainstay of effective treatment for costly chronic conditions. Numerous studies have examined the effects of health insurance coverage on drug utilization among the elderly, particularly around the creation of the Medicare Part D program (Kaestner and Khan, 2012; Ketcham and Simon, 2008; Lichtenberg and Sun, 2007; Yin et al., 2008). However, much less is known about the effects of health insurance on prescription drug use among low-income adults, who are primarily insured through Medicaid or lack health insurance entirely. The passage of the Affordable Care Act (ACA) in 2010 and the subsequent Supreme Court ruling making Medicaid expansion optional provided a valuable opportunity for evaluating the responsiveness of low-income individuals to insurance expansions in terms of prescription drug usage. Thus far, 31 states plus the District of Columbia have adopted the expansions. We leverage this natural experiment to examine changes in utilization across drug classes, as well as brand and generic forms of prescriptions; in addition, we use state-level variation in Medicaid drug copayments to estimate the price elasticity of demand in this population.

An extensive literature using experimental and quasi-experimental research designs establishes that the demand for prescription drugs is responsive to out-of-pocket prices (Chandra et al., 2010; Goldman et al., 2007; Newhouse, 1993), and the complete lack of insurance is in essence an extreme version of price variation. In all 50 states, Medicaid covers most major categories of medical intervention, including pharmacological therapy. Meanwhile, non-elderly adults without health insurance are four times more likely than their insured counterparts to report foregoing needed prescription drugs due to cost, and most of the uninsured are non-elderly adults (National Center for Health Statistics, 2016). The extent to which expanded Medicaid coverage induces greater use of prescription drugs in this population is a worthwhile question. Utilization of prescription medications can provide indirect evidence of how insurance expansions affect access to providers, because prescriptions can only be obtained through consultation with a medical practitioner with prescriptive authority. In turn, studying the effect of insurance coverage on healthcare utilization may also be important for understanding potential changes in health outcomes.

Much of what is known about the coverage impacts on prescription drugs comes from studying the elderly population, but there has been far less research on the responsiveness of prescription medication use to insurance coverage among non-elderly adults. There are several reasons why the ACA Medicaid-eligible younger population may not respond in the same way as the well-studied elderly population affected by Medicare Part D. Common health conditions and available therapies are likely to be different at younger ages. The low-income population in Medicaid may also be particularly affected by cost-sharing requirements, which vary by state. Another key difference is that the population gaining Part D coverage was already insured for other types of care beforehand – Medicare Part D added prescription drug benefit to existing otherwise comprehensive coverage. The response may differ when a population is gaining access to subsidized pharmaceuticals at the same time as insurance for provider visits. Opposing complementarity and substitutability between provider visits and medications imply that the net effect of coverage on prescription drug utilization in the case of the ACA is theoretically ambiguous, and in any event likely differs from the experience in Medicare Part D.

The Oregon Health Insurance Experiment (OHIE) represents the only other study we are aware of that assesses changes in prescription drug utilization among low-income non-elderly adults gaining health insurance. Evidence from Oregon showed that acquiring Medicaid led to significant increases in the likelihood of using any medication (Finkelstein et al., 2012b) and in diabetes medications utilization (Baicker et al., 2013). However, the subsidized public health insurance in the OHIE did not include any cost-sharing for prescription drugs. In contrast, of the 28 states that expanded Medicaid under the ACA within our study period, 20 states have prescription drug co-pays for the expansion population (Brooks et al., 2015). This variation allows us to estimate a price elasticity for drugs among Medicaid beneficiaries, which is an economically important consideration as states increasingly propose more cost-sharing for this population. More broadly, using national data, we extend the scope of previous research to provide the first natural-experimental evidence on how subsidized public health insurance coverage impacts prescription utilization on the Medicaid population. Taking advantage of a rich claims-based data source that is nationwide in coverage, we also assess the relative effects of insurance coverage on utilization across therapeutic classes and drug types (generic or branded), adding rich detail to our understanding of consumer demand for pharmaceuticals.

Our work also fits into the broader emerging literature on the effects of the Affordable Care Act. Multiple studies have found that uninsurance rates have declined substantially since early 2014 (Courtemanche et al., 2017; Frean et al., 2017; Kaestner et al., 2017; Sommers et al., 2015); others have found improvements in self-reported access to primary care and prescription medications, particularly in Medicaid-expansion states (Sommers et al., 2016b; Sommers et al., 2015). Although changes in insurance coverage resulting from the ACA Medicaid expansions have already received substantial attention from researchers and policy makers, far less is known about its impact on the use of specific types of medical services including prescription drugs.

Our study design focuses on the 2014 state Medicaid expansions under the ACA to examine the impact of expanded coverage on prescription drug use. We employ a difference-in-difference approach similar to that used in recent studies assessing the impact of expanded health insurance on uncompensated hospital-care costs (Dranove et al., 2016; Nikpay et al., 2015; Nikpay et al., 2016) and on coverage, access to care, and labor market outcomes (Gooptu et al., 2016; Kaestner et al., 2017). We assess aggregate effects on overall Medicaid pharmaceutical utilization rates, examine usage patterns across specific therapeutic classes and branded vs. generic drugs, and test for heterogeneity of effects in areas of the country that have high rates of baseline poverty, uninsurance, and concentration of racial/ethnic minority populations. In addition, we explore the differential effect of Medicaid drug cost-sharing in order to assess price elasticity of prescription medications for low-income non-elderly adults. Our research also provides an analysis of whether public coverage expansion simply substitutes away from utilization under uninsured or private payment sources. This helps in gauging the extent of new drug use, as opposed to a simple substitution of payment source; the issue of Medicaid 'crowd out' of private insurance has been a substantial concern in prior literature (Congressional Budget Office, 2007; *New York Times*, 2015b).

Our key findings are as follows. We find a significant increase in prescription drug utilization in response to insurance expansions aimed at low-income adults. Medicaid prescriptions increased by 19 percent in states that expanded program eligibility in the first 15 months following the 2014 policy change, relative to states that did not adopt Medicaid expansion. Moreover, we observe heterogeneity in utilization by therapeutic category, with a general pattern of larger increases for maintenance drugs used for chronic conditions and smaller increases for acute condition medications. The largest increase of 24 percent occurred among drugs associated with treating diabetes; the next largest were a 22 percent increase for contraception, and a 21 percent increase for drugs associated with cardiovascular disease. Increases in respiratory/allergy medications and antibiotics were significantly smaller.

We observe no significant effect of Medicaid expansion on Medicare, privately insured prescription utilization, or on uninsured prescriptions paid by cash or assistance programs. These coefficients are not statistically significant and are small in magnitude, suggesting a lack of substantial crowd out of private prescriptions following Medicaid expansion. In other words, the increased Medicaid drug utilization appears to represent a net increase in utilization among low-income adults.

We find evidence that the rise in Medicaid prescriptions was driven by a relative increase in generic drug usage that was nearly twice as large as that for branded drugs, which suggests that the Medicaid expansions steered patients towards lower-cost prescription drugs and may have therefore helped control program costs. This has important implications for states concerned with the budget implications of drug spending in the Medicaid program, as well as for the efficiency of health insurance expansions. This pattern of utilization also matters for the pharmaceutical industry's assessments of the impacts of ACA policies on future revenues.

Unlike the Oregon Medicaid expansion studied in the past, most of the states that expanded Medicaid in our study required some cost-sharing for prescription drugs. Our results reveal smaller increases in utilization in expansion states imposing higher cost-sharing for prescription drugs, than in expansion states with low or no cost-sharing. We use these estimates to derive an implied price elasticity of -0.06, in the case of medications for respiratory illnesses, which is the only therapeutic class where we detect a statistically significant effect in the continuous copay specification. While this estimate is smaller than the -0.23 found by Chandra et al. (2014) in their regression discontinuity study of demand elasticities for prescription medications among low-income non-elderly adults with subsidized public insurance in Massachusetts, it is closer to the estimate of -0.098 for those with chronic illnesses in their analysis sample. Also, our use of aggregate data and weighted-average statewide copay likely introduces some measurement error, which would presumably bias our estimated elasticity towards zero.

To investigate the robustness of our results and to understand the heterogeneity of policy effects, we test whether effects are larger in areas we would expect the insurance expansions to have had more substantial reach. Within expansion states, we find that increases in prescription drug utilization were larger in geographical areas with higher baseline uninsured rates in 2013, where the ACA likely produced the largest coverage changes. We also document suggestive evidence that increases in Medicaid prescriptions were greater in markets with higher pre-2014 rates of poverty and rates of minority (Hispanic and black) populations, indicating that Medicaid expansion under the ACA may have reduced ethnic/racial disparities in access to medications. Our findings are robust to various alternative specifications, comparison of pre-policy trends and event study specifications, as well as placebo testing, suggesting the results can be causally attributed to the impact of expanded public health insurance to low-income populations.

2.2. **Background**

Multiple experimental and natural-experimental studies have demonstrated that health insurance increases the use of medical care, including prescription drug use. The vast quasi-experimental evidence from Medicare Part D implementation suggests that among the elderly, health insurance drug coverage reduced out-of-pocket (OOP) spending and led to higher use of pharmaceuticals among the elderly (Kaestner and Khan, 2012; Ketcham and Simon, 2008; Lichtenberg and Sun, 2007) and improved health status (Afendulis et al., 2011; Ayyagari and Shane, 2015; Kaestner et al., 2014a). There is also evidence that prescription medications for chronic conditions such as high cholesterol and diabetes can reduce the use of more expensive forms of healthcare among the near-elderly (Borrescio-Higa, 2015).

Yet, this previous literature on the elderly and the near-elderly does not provide direct evidence from which to draw inferences about the non-elderly adult population, particularly lower-income adults targeted for insurance expansion under the ACA. The ACA provides additional federal financing to states for extending Medicaid coverage to non-elderly adults earning less than 138 percent of the federal poverty level (FPL). The expansion decision was later delegated by the Supreme Court to states, and as of January 1, 2017, 31 states plus Washington DC had implemented the ACA Medicaid expansion, while the remaining 19 states had not. Medicaid is a means-tested health insurance program for low-income populations that is jointly administered by the federal and state governments. Since the creation of the Medicaid program in 1965, states have had broad discretion over a range of eligibility rules, program benefits, and provider reimbursement, subject to compliance with federal minimum standards. As a result, there has long been considerable variation in Medicaid eligibility standards and program generosity across states. Even though prescription drug coverage was a state option, all states covered pharmacological treatments prior to the ACA; following the ACA's Medicaid expansion, new enrollees must be offered so-called "benchmark" benefits, including prescription drug coverage.

Thus, the ACA's Medicaid expansion offers a unique large-scale policy experiment through which the effects of health insurance on healthcare utilization, including prescription drugs, can be studied. Only a few prior studies estimate the impact of Medicaid on prescribed medications. The OHIE found that in the first year, Medicaid coverage increased the likelihood of using prescription medications by 15 percent among previously-uninsured low-income adults (Finkelstein et al., 2012b). Using a difference-in-difference study design, Sommers et al. (2016b) estimated a 10 percentage-point reduction in low-income adults reporting skipping prescribed medication in Kentucky and Arkansas following Medicaid expansions under the ACA, relative to non-expansion Texas.

Two recent studies have explored the pharmaceutical utilization implications of the ACA Medicaid expansions in particular. Mulcahy et al. (2016) used prescription transaction data to longitudinally follow a sample of non-elderly adults who reported any prescription drug use during January 2012. They find that adults who gained Medicaid in 2014 increased their prescription drug use by 79 percent. However, their sample was limited to those already using medications, nearly two-thirds of whom reported chronic health conditions such as diabetes, asthma and breast cancer. Thus, their study population had a much higher prevalence of chronic disease than the overall population, such as adults newly eligible for coverage through the ACA Medicaid expansions (Decker et al., 2014). More importantly, their sampling design did not allow them to consider the effects on those who did not use prescription medications prior to the expansions. The second study, by Wen and colleagues (2016), examines aggregate Medicaid medication use as reported in the CMS State Drug Utilization Database (SDUD) through 2014. They find a significant increase in the number of prescriptions per enrollee following expansion, but no significant change in total spending. Notably, none of these aforementioned studies examine heterogeneity in effects by therapeutic class, state cost-sharing requirements, across payer types other than Medicaid or by sub-state geography.

Price elasticity of demand for pharmaceutical services among low-income non-elderly adults has received relatively less attention in the literature, even though the extant literature from other insurance settings suggests that higher consumer cost-sharing reduces utilization of medical services. Several studies have estimated the responsiveness of demand for prescription medications to out-of-pocket costs, with one meta-analysis across all payer types and populations finding that for each 10 percent increase in out-of-pocket costs, total drug spending fell by 2-6 percent (Goldman et al., 2007). To the best of our knowledge, low-income non-elderly adult population-specific elasticities have not been assessed at the national level. Chandra et al. (2014) examine the elasticity of different types of medical spending to cost-sharing among low-income non-elderly adult beneficiaries of subsidized public health insurance in Massachusetts through a regression discontinuity design. Using discontinuous changes in patient cost-sharing around the 100 and 200 percent FPL income thresholds, they estimate a price elasticity of expenditure on prescription drugs to be -0.23, with a lower price elasticity (-0.098) for those with chronic diseases. We add to this literature by exploring this question in a national setting, in the context of a large-scale public insurance expansion, and by providing an analysis of therapeutic class-specific price elasticities by taking advantage of our rich data source.

2.3. **Data**

The database provides a combined view of U.S. pharmaceutical distribution sales to U.S. retail brick-and-mortar and mail-order pharmacy prescription activity, including large pharmacy chains, independent pharmacies and pharmacy benefit managers (PBMs). The data we use in our empirical specifications are aggregated to the level at which variation occurs (for example, state/year/therapeutic class/payer type), but here we describe the micro data from which we aggregate. The micro database contains slightly more than 80% of all U.S. retail prescriptions and 60% of all U.S. mail order prescriptions, including all data from more than 40,000 retail pharmacies. The data are then projected to the national level using weights to adjust for any systematic differences in the sample composition. There were no changes to the reporting frame during the years of our sample (i.e. no substantial changes in which pharmacies contributed data each year, although every year some pharmacies will move in and out of the sample); the analysis weights provided in the database are adjusted to produce nationally and sub-nationally reflective totals. Together these data files offer a rich set of information on every drug claim including the month of each transaction, the core-based statistical area (CBSA) of the pharmacy or facility, Uniform System of Classification (USC) product code for the drug, a quantity measure of package size, and payment type (including commercial plans, Medicare, cash, assistance programs, and Medicaid). The Medicaid category includes both fee-for-service Medicaid and Medicaid managed-care claims and covers all fifty states and DC.

We obtained prescription counts aggregated by unit of geography (all states and DC, as well as data from the 917 Core Based Statistical Areas (CBSA) in the US),[3] time

[3] We use Core Based Statistical Area (CBSA) as our market definition because this is the smallest geographic unit available to us. CBSAs are geographic aggregations produced by the US Office of Management and Budget (OMB); they consist of groupings of geographic areas with a population of at least 10,000 and associated with an urban core. These areas are clusters of adjacent counties with social and economic integration. Approximately 94 percent of the total US population lives within CBSAs. An example of a CBSA is Mobile, Alabama.

(quarterly, from Q1 2013 through Q1 2015), drug class (total, as well as by 9 therapeutic classes), and payer type.

Key advantages of this dataset include its large and nationally-representative sample – far larger than any of the government surveys that examine healthcare utilization, which enables us to conduct state and sub-state analyses; information not only on Medicaid-covered prescriptions but also all other major payer types including uninsured patients; quarterly-level information, rather than just annual data, which bolsters our identification strategy; and rich drug information on therapeutic categories and other factors such as brand-name or generic status. Other rich data sources such as the Medical Expenditure Panel Survey (MEPS) Household Component have very small sample sizes in comparison and lack the geographic detail of our claims data. Meanwhile, other administrative data sources on drug spending –such as the State Drug Utilization Database (SDUD) collected by the federal government lack information on non-Medicaid sources of payment and lack sub-state data for investigating heterogeneity across areas.

Our data set also has important disadvantages. One is that it does not contain the patient-level information on demographic and socioeconomic characteristics that would allow us to estimate specifications separately by age, gender, income, or other important subgroupings of individuals who may differ in their demand for drugs or response to coverage. To control for changes in the economic climate that may independently affect state Medicaid rolls, we merge the pharmaceutical claims data with state and CBSA-level unemployment rates from the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS).[4] We control for possible changes in population by dividing total counts of prescriptions by Census Bureau state and county annual non-elderly adult population estimates.[5] We also incorporate information on uninsurance rates, poverty rates and racial/ethnic composition of the local markets using estimates produced by the Census

[4] To obtain the unemployment rate in a CBSA, we take the average of the unemployment rates for all the counties within a CBSA. County-level unemployment rates come from BLS LAUS, and the listings of counties within CBSAs come from the Mable Geographic Correspondence Engine (http://mcdc.missouri.edu/websas/geocorr12.html).

[5] We obtain these annual county resident population estimates by year from https://www.census.gov/data/datasets/2015/demo/popest/counties-total.html.

Bureau.[6] The resulting dataset is a balanced panel of 51 states (including Washington, DC) observed for a total of 9 quarters (459 =51*9 total cells), and a balanced panel of 781 CBSAs that do not cross state lines (we drop 143 that do) for a total of 7,029 (=781*9) cells.

2.4. Empirical Framework

Our main empirical strategy uses a state-level difference-in-difference model to compare pharmaceutical utilization in all expansion states to that in non-expansion states (first difference) before and after the expansions (second difference). In a specification check, following Wherry and Miller (2016), we drop from our expansion group the five states (DC, DE, MA, NY and VT) that had large expansions of public coverage to nonelderly adults prior to 2014.

Our main outcome of interest is medication utilization under Medicaid, defined as the total number of Medicaid prescription drugs dispensed (new and refills, all therapeutic classes) where Medicaid (including both fee-for-service and managed care) is recorded as the payer. We also examine total prescriptions dispensed for other payer categories. Our data are aggregated, by quarter, to state and CBSA levels. We generate per-100 population utilization rates by dividing the total number of prescriptions by Census Bureau estimates of the non-elderly adult population. The identifying variation in our main analysis comes from cross-state differences in expansion decisions. Our baseline model for the effect of the ACA Medicaid expansions is specified as:

$$Y_{st} = \alpha + \beta Post_t x Expansion_s + \partial UE_{st} + \tau_t + \vartheta_s + e_{st} \qquad (2\text{-}1)$$

The dependent variable is the logged prescriptions per 100 non-elderly adult population in the state, with s indexing state and t indexing each quarter in the data, respectively. The difference-in-difference coefficient, β, measures the change in Medicaid scripts in expansion states net of the change in non-expansion states. While most states

[6] We obtain county-level 2013 uninsured rates for non-elderly adults at or below 138 percent FPL from the Census Bureau's Small Area Health Insurance Estimates (SAHIE) program, racial/ethnic composition data from the Area Health Resource Files (AHRF) and poverty rates from the Census Bureau's Small Area Income and Poverty Estimates (SAIPE).

implemented the Medicaid eligibility changes beginning January 2014, Indiana, Michigan, New Hampshire and Pennsylvania expanded later in 2014 or 2015. To account for this staggered implementation timeline, the difference-in-difference interaction term, $Post_t xExpansion_s$, indicates whether a state has expanded Medicaid at time t. The model includes state fixed effects to account for time-invariant state-specific differences in prescription drug use and time dummies for each quarter and year in the data to capture national time trends. We use state quarterly unemployment rates (UE) to control for changes in economic conditions that may independently influence drug utilization patterns.[7] The model is estimated using ordinary least squares, and throughout we report standard errors clustered at the state level to account for correlated error terms across states over time (Bertrand et al., 2004b).

Identification in the difference-in-differences model is based on the assumption of parallel trends – that absent the 2014 Medicaid expansion, trends in outcomes would not have differed significantly across expansion and non-expansion states. While this assumption is not directly testable, we compare trends in Medicaid prescriptions across the treatment and comparison states prior to the policy change; this comparison offers support for our identifying approach. Because we have a limited number of quarters of data prior to the policy change, we also examine Medicare prescription counts as a placebo test; again, the results (presented below) support our identification strategy.

2.5. **Results**

2.5.1. **Descriptive Trends**

In Figure 2-1, we plot unadjusted time trends for total per-capita (normalized by the number of non-elderly adults in the state) prescriptions paid at the state level by Medicaid, Medicare, private insurance plans, or through cash and assistance programs available to the uninsured during the study period; we plot these trends separately for the expansion and non-expansion states. The blue and grey lines represent ACA Medicaid-expansion states and non-expansion states, respectively.[8] The first vertical line in red

[7] We also estimate our models without including the unemployment rate as a covariate and find they do not materially alter our results.

[8] Four states (i.e., IN, MI, NH and PA) are omitted from the graphs because they expanded after January 2014. They are, however, included in the regressions that allow for more flexibility in

denotes the beginning of the ACA's first open enrollment period in the fourth quarter of 2013; the second denotes the implementation of the ACA Medicaid expansions in the first quarter of 2014.

Figure 2-1 demonstrates that prior to the expansions in early 2014, Medicaid prescriptions followed similar trends across the two groups of states. There is a clear divergence in trends after the first quarter of 2014, when the expansion states experience a very noticeable increase in Medicaid within 6 months of the policy change, relative to the non-expansion states. The remaining panels in Figure 2-1 display trends in pharmaceuticals that were paid by Medicare, private insurance, and were uninsured (cash and assistance programs). These panels demonstrate that trends in aggregate prescriptions did not differ appreciably across the two groups of states for the non-Medicaid payment categories.

2.5.2. **Effect on Medicaid Prescription Drug Utilization**

Table 2-1 displays results from our main difference-in-differences analysis of equation (2-1) and unadjusted sample means of the dependent variable. The dependent variable is the natural logarithm of Medicaid prescriptions per 100 population. The pattern in the first panel of Figure 2-1 is reflected in the difference-in-difference estimate for Medicaid prescriptions in column (1) of Table 2-1. The estimate demonstrates that Medicaid expansions in 2014 led to sizable and statistically significant increases in Medicaid prescription drug use. The coefficient represents a 19-percent increase in Medicaid prescription utilization relative to non-expansion states for all therapeutic classes. Column (2) demonstrates that the estimate increases to 23 percent when we drop the five states that provided publicly subsidized coverage to adults at or below 100 percent of the FPL prior to 2014 (the District of Columbia, Delaware, Massachusetts, New York, and Vermont).

specifying the Medicaid expansion date. The graphs do not change in any meaningful manner when these states are included in the expansion category.

2.5.3. **Effect on Aggregate Prescription Drug Utilization by Payment Source**

One mechanism through which Medicaid prescriptions may increase following expansions is private insurance crowd-out. Indeed, much attention in health economics devoted to this question (Cutler and Gruber, 1996; Gruber and Simon, 2008). Another possibility is that increased Medicaid drug claims simply reflects a substitution of Medicaid coverage for drug claims that were previously purchased in cash by uninsured patients. To explore these mechanisms, we investigate the impact of the expansions on prescriptions from private insurance, as well as cash and other assistance programs available to the uninsured. This analysis allows us to consider whether Medicaid prescriptions increased simply through a substitution of payment source or due to a net increase in utilization.

Table 2-2 below contains these results for other payer sources. Changes in cash/assistance programs (likely representing uninsured individuals) had a statistically insignificant and small point estimate of -0.02 in column (1). Similarly, in the case of private insurance, the statistically insignificant estimated coefficient in column (2) was -0.01. Thus, the point estimates for privately insured and uninsured prescriptions are negative but statistically indistinguishable from zero indicating little to no crowd-out of prescription drug use from payment sources other than Medicaid. This suggests that in the aggregate, the policy impact on use of Medicaid prescription drugs primarily reflects new prescriptions rather than simply a shift in payer-mix from cash to insurance or from private insurance to public insurance.

As a falsification test, we next consider changes in Medicare prescription utilization. We hypothesize that extended Medicaid eligibility for the non-elderly adult population under the ACA is unlikely to affect prescription drug use in the Medicare program. Consistent with our hypothesis, the result from this analysis, which we report in column (3) of Table 2-2, show that there is little evidence that Medicare is affected. The dependent variable is the natural logarithm of uninsured, privately insured, or Medicare prescriptions per 100 population. In Appendix Table B-2, we present event history estimates for aggregate prescriptions paid by non-Medicaid sources. Taken together, these regression estimates indicate that prescriptions from non-Medicaid payment sources did not respond significantly to the policy expanding public health insurance.

2.5.4. **Heterogeneity in Medicaid Prescription Utilization by Drug Class**

We next study whether specific therapeutic classes were differentially affected by the expansion. Prescription medications that treat acute medical conditions may respond differently to coverage expansion than maintenance drugs associated with chronic illnesses, but prior literature has not examined this among low-income populations. It is possible that demand for some short-term medications (e.g. allergy relief) would be more elastic, if those medications are viewed as more discretionary and less critical for health than those associated with chronic illnesses (e.g. diabetes or heart disease). Alternatively, some acute medications may treat conditions requiring immediate treatment (e.g. antibiotics for infection) and might therefore be less price sensitive and less responsive to changes in coverage. Another reason to examine heterogeneity across therapeutic classes is that the newly insured population's healthcare needs may differ from those of the nation as a whole; in particular, chronic conditions are likely to be more common among low-income individuals qualifying for Medicaid than among those with private insurance (Decker et al., 2014) .

Table 2-3 describes the impact on utilization of a range of medication classes. Our results indicate that there were statistically significant increases in utilization across all therapeutic classes, but relatively larger effects for certain classes, particularly those relevant to common chronic medical conditions. Diabetes medications increased 24 percent; accounting for the largest growth among all therapeutic classes. This effect was statistically different from the mean effect on all the remaining classes. The use of cardiovascular medications (those for high blood pressure, high cholesterol and heart disease) increased by 21 percent, while the use of contraceptives increased by 22 percent. Meanwhile, the use of respiratory/allergy medications, antibiotics, and gastrointestinal medications, which are more commonly taken for shorter-term conditions than the other therapeutic classes, increased less than overall drug spending, with growth rates ranging from 15 to 17 percent.

The larger increases in use of medications for chronic conditions such as diabetes detected in our data accord with recent research that finds diagnoses of chronic health conditions to have increased among low-income adults in states that have broadened

Medicaid eligibility under the ACA (Kaufman et al., 2015; Wherry and Miller, 2016) and that hospitalizations for diabetes has decreased (Freedman et al., 2017). Overall, this pattern of results suggests that health insurance expansion was particularly effective at increasing prescription drug utilization for common and potentially costly chronic medical conditions. Our results also indicate that even though state Medicaid family planning waivers existed prior to the ACA expansions, the recent expansions in coverage lead to meaningful effect on access to contraceptive treatments.

In recent years, high prices of life-saving hepatitis C and HIV medications have fueled debate about the role of public policy in providing access to costly but effective pharmacological treatments for vulnerable populations. Our finding of a more modest effect for HIV and Hepatitis C medications is likely due to two factors. First, the existence of federally funded programs such as the Ryan White HIV/AIDS Program (RWP) and the AIDS Drug Assistance Program (ADAP) already facilitated the use of these medications for many patients prior to the expansion of Medicaid in 2014. Second, growth in use of Hepatitis C medications may have been attenuated by limited access in several state Medicaid programs (such as Indiana and Washington) due to cost concerns (New York Times, 2015a; Pear, 2015).

2.5.5. **Differentiating Between Brand-Name and Generic Drug Utilization**

Several facts suggest that we should expect the use of generic medications in Medicaid to rise compared to brand-name drugs. First, patent expirations of several branded prescription drugs such as atorvastatin (Lipitor) (Jackevicius et al., 2012) and the SSRI-class of antidepressants (Huskamp et al., 2008) in recent years have increased the number of prominent generics in the market (Frank, 2007). Second, in response to budgetary constraints, many state Medicaid programs use several policy levers for utilization management – such as higher copayments for brand-name drugs, mandatory generic substitution, and lower reimbursements to pharmacies for brand-names relative to generic – that encourage utilization of generic medications (Simon et al., 2009). A recent study finds that there was a significant increase in Medicaid prescriptions following the ACA Medicaid expansions, with no detectable increase in the program's drug spending (Wen et al., 2016), which supports the notion that most new prescriptions were likely low-cost. These features of the policy environment may impact utilization of medications in ways that can potentially improve health and manage program spending growth.

In Table 2-4, we decompose total Medicaid prescriptions filled into brand-name, generic and other (includes prescription products recorded as medical supplies and bulk chemicals) components. To obtain estimates of the impact of the 2014 Medicaid expansions on composition of Medicaid prescriptions, we estimate equation (1) for each group.[9] While Medicaid prescriptions increased significantly in all three categories, the magnitude of the impact was the largest for generic drugs. Generic drug claims in Medicaid increased by 24 percent, compared to 14 percent for brand drugs. At baseline, generic drugs represented nearly 79 percent of Medicaid claims in our sample, "other" less than 1 percent, and brand-name drugs the remaining 21 percent, based on total prescription counts in 2013. Taken together, this provides suggestive evidence that the Medicaid expansions guided patients towards lower-cost prescription drugs, which may help control program costs.

2.5.6. **Price Elasticity of Medicaid Prescription Drug Utilization**

Our work thus far studies the effect of coverage expansions on prescription drug utilization, capturing the full effect of health insurance, which could operate through reductions in the cost of seeing a prescribing clinician as well as paying for the prescription drug itself. Since expansion states differ in the amount of cost-sharing required for prescription drugs, spatial variation in drug copayments provides us an opportunity to better understand the price elasticity of demand for medications among low-income adults.

We match our data with state level Medicaid drug copayments specific to the expansion population from the Kaiser Family Foundation (Brooks et al., 2015). As of January 2015, of the 28 states that extended Medicaid eligibility under the ACA guidelines, 12 states have \$0 copay for generics, 7 states have a \$1 generic copay, and the remaining

[9] For this analysis, we use CBSA-aggregates, as product information on brand/ generic status is only available at the sub-state level. One concern in examining this specification is whether CBSAs in the sample differ in ways that introduce bias in our estimates. We re-estimate our baseline difference-in-difference model (equation 1) using this sample, comparing CBSAs in expansion states to a group of comparison CBSAs in non-expansion states. The results from this analysis are displayed in Appendix Table 6. These estimates are closely comparable in magnitude, direction and precision, confirming our findings from the main specification.

have average copayments between \$1.50 and \$4. Brand-name drug copayments track generic copayments closely, with \$0 being the copay in 9 of these states, and varying between \$1-\$6 in the rest.[10]

We conducted two analyses to assess the effects of copays on prescriptive drug utilization after health insurance expansions to low-income adults. First, we split expansion states into those with copays above and below the median,[11] and estimated two difference-in-difference models akin to equation (2-1): one model compares high-copay expansion states to all non-expansion states, and the other compares low-copay expansion states to all non-expansion states.

Table 2-5 reports the results of this exercise. As expected, the policy impact on prescription drug utilization was larger in expansion states with low cost-sharing requirements than high cost-sharing. These results show that increased Medicaid drug utilization was 22 percent in states with lower copays and 17 percent in states with higher copays, both compared to non-expansion states. The coefficients are both statistically significant, providing evidence consistent with basic consumer theory. However, the two estimates are not statistically significantly different from each other, thus we interpret this evidence as suggestive rather than definitive regarding a response to cost-sharing

We next explore the effect of prescription drug copays on aggregate Medicaid prescriptions and provide a parameterized estimate of the price elasticity related to Medicaid copays through the following specification:

$$Y_{st} = \alpha_0 + \alpha_1 Post_t x Expansion_s x\ Wtd\ Copay_s + \alpha_2 Post_t x\ Wtd\ Copay_s + \alpha_3 Post_t x Expansion_s + \alpha_4 Expansion_s x\ Wtd\ Copay_s + \delta UE_{cst} + \tau_t + \vartheta_s + \varepsilon_{st} \qquad (2\text{-}2)$$

In the above equation, $Wtd\ Copay_s$ represents a weighted average of Medicaid generic and brand-name drug copayments in each expansion state, with the state's respective pre-period share of total prescriptions that are generic vs brand name used as

[10] In cases where the dollar amount of cost-sharing is reported as a range, we use the midpoint of the range.

[11] We used a weighted average of the generic and brand-name drug copay facing new enrollees in each expansion state, using the state's respective pre-period share of total prescriptions that are generic vs brand name as the weights. This is similar to the approach used by Chandra et al. (2014) for computing weighted copayments.

weights.[12] Here, the effect of consumer cost-sharing is identified by α_1, which allows us to isolate differential policy impact by state drug copayments. The terms $Expansion_s, Wtd\ Copay_s$, and $Expansion_s x\ Wtd\ Copay_s$ are perfectly collinear with state fixed effects; hence these terms all drop out of the equation.

Table 2-6 displays results from the estimation of the above equation. The point estimate of -0.038 in column (1) indicates that the increase in post-expansion utilization was 3.8 percent lower for each added dollar in Medicaid copays per prescription (compared to a mean weighted cost-sharing of $1.24 in expansion states), though this estimate was not statistically significant.

Next, we test for heterogeneous response of utilization to copayments by therapeutic class. Theoretically, cost-sharing may differently affect drug use across therapeutic classes due to factors such as the availability of alternative over-the-counter (OTC) treatments and severity of the medical condition.

The results in columns (2) through (9) of Table 2-6 suggest that utilization in the remaining therapeutic classes appears to respond negatively to greater cost-sharing as expected, with price responsiveness varying appreciably across the classes, though the key $Post_t x\ Expansion_s x\ Wtd\ Copay_s$ interaction term was not statistically significant for most drug classes. We did detect a statistically significant point estimate for respiratory medications in column (8) which indicates that each $1 increase in the average drug copayment was associated with a 4.9 percent relative decline in use of medications for respiratory illnesses and allergies after Medicaid expansion (compared to an overall increase of 18.8 percent in utilization for expansion states with $0 copays, indicated in the 2nd row of the table). In contrast to these drug classes, demand for potentially life-saving medications such as cardiovascular and HIV/Hepatitis C drugs were the least price-responsive.[13] Overall, this pattern is consistent with prior research showing that

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[12] In 2013, the average OOP cost per prescription for a low income non-elderly adult was $78.40 (authors' calculations from the Medical Expenditure Panel Survey NET tool at https://meps.ahrq.gov/mepsweb/data_stats/MEPSnetHC/datasource). We assign copayments for non-expansion states a value of $78.40. The estimates presented in Table 6 are robust to the choice of alternative cost-sharing amounts such as $20 or $100.

[13] Note that under federal rules, state Medicaid programs must provide prescription birth control without cost-sharing. Hence, we exclude contraceptives from this analysis.

medications used to treat acute symptomatic conditions such as asthma are relatively more price-sensitive than chronic treatments for cardiovascular conditions (Goldman et al., 2004; Landsman et al., 2005).

Overall, taking the point estimate from column (1) in Table 2-6, we calculate that each $1 increase in cost-sharing (amounting to an 81 percent increase from the mean copay of $1.24) led to a 3.8 percent relative decline in utilization, yielding a price elasticity of roughly -.05. Using this approach, the copay elasticity for allergy and respiratory medications is -0.06, and is statistically significant. This degree of price elasticity is similar in direction but substantially smaller the -0.23 found by Chandra et al. (2014) for prescription medications among low-income non-elderly adults with subsidized public insurance in Massachusetts. It is worth noting that their elasticity estimate is based on variation in copayments at income thresholds alone. In contrast, we study the effect of higher copayments along with contemporaneous policy-induced gains in insurance coverage, which may bring into treatment a population with relatively inelastic pent-up demand for healthcare services. Perhaps unsurprisingly, Chandra et al. (2014) find that patients with chronic illnesses such as hypertension, high cholesterol, diabetes, asthma, arthritis or gastritis in their analysis sample, are less price sensitive, with an elasticity estimate of -0.098, which is much closer to our price elasticity estimate of -0.06. Our estimated elasticity could presumably be biased downward as aggregate data and weighted-average statewide copay likely introduces some measurement error. Notably, while we find price elasticities to be higher for respiratory and allergy medications and lowest for cardiovascular, diabetes, and other serious illness, this is largely the opposite pattern as what we detected in Table 2-3, where we found that overall coverage gains from Medicaid expansion led to the largest increases in utilization for the latter conditions. One way to reconcile this apparent contradiction is that Table 2-3 reflects both the reduction in out-of-pocket price for medications and the reduction in cost for seeing a physician. It is likely that better access to clinicians - and thus higher rates of diagnosis and then treatment of chronic conditions like diabetes - is just as important in increasing medication use as the reduction in drug-related out-of-pocket costs. This pattern again points to the value in assessing the economic effects of a comprehensive coverage expansion like Medicaid, distinct from just adding drug coverage to existing insurance as in the case of Medicare Part D.

2.5.7. **Robustness Checks**

To investigate the basis of support for the natural-experimental study design, we examine whether there were differential pre-trends in Medicaid prescription drug utilization across states based on treatment status. We do this by using four quarters of data from 2013Q1-2013Q4 and the following empirical specification:

$$Y_{st} = \alpha + \gamma Trend_t + \delta Expansion_s xTrend_t + \beta UE_{st} + \tau_t + \vartheta_s + \varepsilon_{st} \qquad (2\text{-}3)$$

In the above equation, $Trend$ stands for a quarterly linear time trend, $Expansion_s$ indicates the states that expanded after 2014 Q1, and the interaction term $Expansion_s xTrend_t$ identifies differential trends between expansion and non-expansion states during the pre-expansion period. Also included in the regression are the trend main term, the unemployment rate, and state and month-by-year fixed effects.

To further evaluate the parallel trends assumption, we use an event study approach by interacting the expansion dummy with each year-quarter in the data from 2013-2015. For the parallel trends assumption to be valid, we expect the interaction terms in 2013 to be statistically indistinguishable from 0. The point estimates in column (1) of Table 2-7 are indeed quite small in magnitude (and not statistically significant) relative to the difference-in-difference estimate in Table 2-2, indicating that no significant differential trends appear in the pre-expansion period that would otherwise threaten our identification strategy. Column (2) displays the event history estimates. Consistent with the graphical results, we find no evidence of differential trends across the states that expanded and those that did not, prior to the policy change. Starting with 2014 Q2, the estimates are positive and statistically significant as we would expect given the timing of state expansion decisions. The estimates are higher in magnitude every consecutive quarter, which implies that the impact of coverage has amplified over time, consistent with other recent evidence on the ACA's Medicaid expansion (Sommers et al., 2016a). This dynamic pattern in the data also mirrors the staggered timeline of state expansion decisions. In every successive quarter after the first six months of the policy change, the effect on aggregate Medicaid prescriptions exceeds the mean effect of 19 percent (shown in Table 2-2). The evolution of the ACA's impact on prescription drug utilization during this 15-month period suggests some longer-term effects on coverage, access, and utilization occur through diffusion of information about policy changes, as well as potential lags in obtaining care after acquiring coverage.

We test the sensitivity of our results to the addition of group-specific linear time trends. These results are presented in column (3) of Table 2-7. This specification addresses the possibility that expansion and non-expansion states may follow different, unobserved time trends correlated with Medicaid expansion decisions. We detect no difference in the estimates in column (3) compared to the baseline model, emphasizing that underlying group-specific trends are not responsible for the observed effect on Medicaid prescriptions.

2.5.8. **Heterogeneous Effects at the Market Level**

The analysis with aggregated state data presented so far indicates that Medicaid prescription drug utilization increased significantly in response to the ACA Medicaid expansions. We next expand on this reduced-form analysis to probe whether these results are concentrated in geographical areas with higher treatment intensity. Geographic variation in factors that affect demand for healthcare services, such as rates of insurance coverage, level of income, and demographic characteristics of the population that pre-dates policy change, may affect prescription drug utilization differently across markets. Here we consider three key pre-expansion characteristics – uninsurance rate, poverty rate, and share of minority population – to test whether the observed effects on utilization are concentrated among particular markets.

First, we examine the dose-response relationship between prescriptions paid by Medicaid and reductions in uninsurance that occurred after 2014. Using data aggregated to the CBSA level, we explore whether the effects of Medicaid expansion on prescription drugs is larger in CBSAs with higher pre-reform uninsurance levels (where we expect larger gains in coverage), relative both to CBSAs with lower baseline (2013) uninsurance in expansion states and to CBSAs with higher baseline uninsurance rates in non-expansion states. The estimating equation is specified below:

$$Y_{cst} = \alpha_1 Post_t x Expansion_s x\ Pct\ Uninsured\ 2013_c + \alpha_2 Post_t x\ Pct\ Uninsured\ 2013_c + \alpha_3 Post x Expansion_{st} + \alpha_4 Expansion_s x\ Pct\ Uninsured\ 2013_c + \delta UE_{cst} + \tau_t + \vartheta_{cs} + \varepsilon_{cst} \quad (2\text{-}4)$$

The main coefficient of interest in the above equation is α_1, which represents the differential change in Medicaid prescription use in CBSAs with high 2013 uninsurance rates compared to those with low rates in expansion states, with non-expansion states as the difference-in-difference control group. We expect that there would be a greater increase in Medicaid prescription drugs in CBSAs where the baseline fraction of uninsured population was larger (i.e., α_1>0). The coefficient of interest in this regression appears in panel A of Table 8, along with summary statistics of 2013 uninsurance rates. The point estimate in column (1) indicates that a 10 percentage-point increase in exposure to the expansions was associated with a 0.1 percent increase in Medicaid utilization. This implies that, on average, areas with 2013 uninsurance rate of 33.7 percent, experienced a 0.3 percent increase in Medicaid prescription drug utilization, due to the policy change. Column (2) displays results from a model which excludes states that partially expanded Medicaid to low-income non-elderly adults before 2014. The similarity of these results with those in column (1) demonstrate that our main results are not sensitive to this exclusion.

We next consider an alternative economic measure to understand heterogeneous treatment impact at the market level. This analysis is motivated by the nature of the Medicaid program. Under the ACA, states expanded coverage for non-elderly adults at or below 138 percent FPL. Thus, impact on utilization is likely to be of a greater magnitude in markets with relatively higher rates of poverty. The estimating equation is described below:

$$Y_{cst} = \alpha_1 Post_t x Expansion_s x\ Pct\ Poverty\ 2013_c + \alpha_2 Post_t x\ Pct\ Poverty\ 2013_c + \alpha_3 Post_t x Expansion_s + \alpha_4 Expansion_s x\ Pct\ Poverty\ 2013_c + \delta UE_{cst} + \tau_t + \vartheta_{cs} + \varepsilon_{cst} \quad (2\text{-}5)$$

The regression estimates in Panel B suggest that policy impact on utilization is larger in high poverty areas, potentially driven by higher Medicaid coverage gains among the low-income population, though these results are fairly imprecise.

The ACA Medicaid expansions are also expected to reduce absolute differences in insurance coverage related to race and ethnicity. Using the American Community Survey, Nikpay et al. (2016) document that in 2014, the largest declines in uninsurance occurred among Hispanics (7.1 percentage points) and blacks (5.1 percentage points), relative to whites (3 percentage points). We exploit geographic variation in racial composition to examine whether the impact on Medicaid prescriptions was comparatively larger in areas with greater Hispanic and black populations. The estimating equation is:

$$Y_{cst} = \alpha_1 Post_t x Expansion_s x\ Pct\ Minority\ 2013_c + \alpha_2 Post_t x\ Pct\ Minority\ 2013_c + \alpha_3 Post_t x Expansion_s + \alpha_4 Expansion_s x\ Pct\ Minority\ 2013_c + \delta UE_{cst} + \tau_t + \vartheta_{cs} + \varepsilon_{cst} \qquad (2\text{-}6)$$

We report this result in panel C. The coefficient on the interaction term $Post_t x Pct\ Minority\ 2013_c x Expansion_s$ of 0.003 indicates that the effect of the Medicaid expansion on Medicaid prescription drug utilization was significantly higher in areas of expansion states with a greater share of Hispanic and Black populations, indicating that the Medicaid expansions reduced racial disparities in access to medications.

Appendix Tables B-3 through B-5 present results from a specification where we replace the post-2014 indicator with a dummy for each quarter to investigate pre-policy trends for these heterogeneity analyses in an event history framework. Appendix Table B-3 shows small and statistically insignificant coefficients for the time periods before Medicaid expansion, which is reassuring. The post expansion effects appear strongest in the 3rd quarter of 2014, although statistically significant and positive effects also appear later in column 2. The results of Appendix Table B-4 & B-5 are less convincing; although most coefficients are statistically insignificant in this table, they are all positive and larger in magnitude after the expansion.

The results from Table 2-8 and Appendix Tables B-3 through B-5 together are suggestive that the growth in utilization of Medicaid prescription medications was more pronounced in geographical areas where the "bite" of expansions was larger because baseline uninsurance and poverty were higher, and in areas where minority populations were more concentrated. While the results from the event history specification in Appendix Table B-4 are broadly consistent with the results from the triple-difference approach in Table 2-7 for baseline uninsurance rates, the corresponding story for is not as strong for the percent poverty and minority analyses.

2.6. Conclusion

This paper provides rich and nuanced evidence on the effects of health insurance coverage for low-income adults on patterns of prescription drug use. Using a national administrative dataset of prescription drug utilization, together with an identification strategy that has been used to look at other healthcare outcomes, we find that non-elderly adult Medicaid prescriptions per capita increased by 19 percent following the ACA Medicaid expansion. When restricting the set of expansion states to the ones without any Medicaid eligibility changes for non-elderly adults pre-dating 2014, we detect a larger effect on Medicaid utilization (23 percent). This pattern is consistent with the likelihood that states with larger potential gains in coverage after Medicaid expansion experienced larger increases in prescription drug use.

We next consider a back-of-the-envelope calculation to gauge the implications of our findings for the use of prescription medications among the newly enrolled population in the extensive margin. Multiplying the quarterly average of 59.15 Medicaid prescriptions per 100 non-elderly adult population in our data (from taking the inverse natural log of 4.08 in Table 2-1) by the total pre-ACA non-elderly adult population in the expansion states (114 million, based on the 2013 American Community Survey) translates the 19 percent increase to 12.8 million additional prescription fills (0.19 *59.15* 114 million/100). This is an estimate of the ACA Medicaid expansion induced increase in prescription fills per quarter, assuming no population growth.[14] In order to back out how many new prescription fills this represents per newly eligible beneficiary, we divide this 12.8 million by an

[14] This estimate is expected to be smaller than the total realized increase in prescriptions under Medicaid from 2013 to 2014, as this estimate does not capture the "welcome mat" effect and changes in prescription use among other Medicaid populations that may be co-occurring.

estimate of the number of individuals who gained Medicaid coverage through ACA, which comes from a difference-in-difference research design that compares expansion to non-expansion states. Centers for Medicare and Medicaid Services (CMS) official enrollment reports through the end of March 2015 indicate that the net enrollment in Medicaid/CHIP in expansion states was 10.5 million more compared to 2013 (Centers for Medicare and Medicaid Services, 2016), while the similar figure for non-expansion states was 1.8 million, yielding an unadjusted difference-in-differences estimate of 8.7 million newly-enrolled individuals by the end of our study period, though this number may include as many as 1 million children (Frean et al., 2017). Subtracting out children yields 7.7 million newly-enrolled adults. Therefore, the "treatment on the treated" estimate of the effect of Medicaid expansion on prescription drug use equates to 1.7 (12.8 million/7.7 million) prescriptions per enrollee per quarter or 6.6 prescription fills per enrollee per year. Related to this, a recent study examining the 2014 policy change, Mulcahy et al. (2016) find that previously uninsured adults who gained Medicaid had 13.3 more prescription fills in 2014 compared to 2013. This was higher (17.8 more prescriptions) for those with chronic conditions, and lower (10.9 additional fills) among the healthier Medicaid enrollees in their sample (aged 20-61). Our results imply a somewhat smaller effect but a similar order of magnitude as their healthier-population estimate.

Our sub-group analysis by drug-class suggests that the increase in utilization was higher for medications used in treating chronic conditions such as diabetes and cardiovascular disease, and for psychotherapeutic medications. Given that lack of appropriate management of chronic diseases through medications is one of the plausible mechanisms for coverage affecting long-term health, and that survey-based analyses of the Medicaid expansion have shown increased rates of care for chronic conditions (Sommers et al., 2016b), these findings are encouraging in terms of their significance for potentially improving health outcomes. Conversely, we find that copays within Medicaid expansion states had a smaller effect on dissuading drug utilization for serious illness like heart disease and HIV, with larger effects on utilization of contraception and allergy/respiratory medications. However, our overall estimates indicate fairly inelastic demand for drugs in this population as a whole, at least at the very small copay levels typically used in Medicaid (average weighted copay of $1.24 per prescription). Whether substantially larger copays would lead to larger proportional changes is not clear and is an important subject for future research, as states experiment with more cost-sharing in Medicaid, including approaches such as health savings accounts.

One of the limitations of this study is that our dataset does not observe individuals longitudinally, so it cannot capture the effect of the expansions on those who were previously uninsured. However, in additional analysis using pre-expansion uninsurance rates at the sub-state level, we examine the effect of change in insurance coverage on utilization. The lack of individual-level data also precludes us from estimating directly the per-person changes in utilization, though our back-of-the-envelope calculations indicate that our estimates correspond to a little more than one additional monthly prescription fill per newly enrolled Medicaid beneficiary.

This study contributes to the set of studies that informs the literature on health economics of insurance coverage, using variation provided by the large recent expansions as part of the ACA. Our findings also provide important new evidence to the literature on Medicaid expansions' effects on the healthcare safety net and improvements in access to and utilization in states that expanded Medicaid. Care management through pharmaceuticals may potentially reduce the use of more resource-intensive medical care such as emergency department visits or other non-drug medical spending (Goldman et al., 2007; Lavetti and Simon, 2016; Roebuck et al., 2015; Stuart et al., 2009). In part, reflecting previous research findings, the ACA classifies prescription drugs as one of the ten categories of "essential health benefits" that all commercial private insurance plans must provide. Future research should consider whether this policy-induced boost in prescription drug utilization is reflected in subsequent health impacts and downstream effects on use of other types of medical care.

2.7. **Tables and Figures**

Table 2-1: **Effect of ACA Medicaid Expansions on Medicaid Prescription Utilization**

Dependent variable: Ln (Medicaid prescriptions per 100 population)		
	(1)	(2)
	All States	Excl. DC, DE, MA, NY, VT
Post x Expansion	0.19***	0.23***
	(0.06)	(0.06)
Year and quarter fixed effects	Y	Y
State fixed effects	Y	Y
Observations	459	414
Dependent variable means		
Expansion, Before	4.08	3.97
Non-expansion, Before	3.94	3.94
Expansion, After	4.35	4.29
Non-expansion, After	4.04	4.04

Notes:

1. Difference-in-differences (DD) estimates are based on aggregated state-quarter data covering 2013Q1 to 2015Q1. The pre-expansion period includes 2013Q1-2013Q4, while 2014Q1-2015Q1 represents the post-expansion period. All models include state fixed effects, fixed effects for each quarter in the data, and state quarterly unemployment rate. Robust standard errors clustered by state reported in parentheses. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.
2. In column (1), specification includes all states being categorized into expansion vs non-expansion states.
    a. Expansion states: AR, AZ, CA, CO, CT, DE, DC, HI, IL, IN, IA, IL, KY, MD, MA, MI, MN, NV, NH, NJ, NM, NY, ND, OH, OR, PA, RI, VT, WA, WV.
    b. Non-expansion states: AL, AK, FL, GA, ID, KS, LA, ME, MI, MO, MT, NE, NC, OK, SC, SD, TN, TX, UT, VA, WI, WY.
3. For the analysis corresponding to column (2), DC, DE, MA, NY, and VT were dropped from the sample; the analysis is otherwise the same as in the first column.

Table 2-2: **Effect on Aggregate Prescription Drug Use by Payer**

Dependent variable: Log (per capita prescriptions)				
	(1)	(2)	(3)	(4)
	Other (Cash and assistance programs)	Private	Medicare	Medicaid
Post x Expansion	-0.02	-0.01	0.003	0.19***
	(0.02)	(0.01)	(0.012)	(0.06)
Year and quarter fixed effects	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y
Observations	459	459	459	459
Dependent variable means				
Expansion, Before	3.96	5.66	4.92	4.08
Non-expansion, Before	4.22	5.70	4.96	3.94
Expansion, After	4.02	5.60	4.94	4.35
Non-expansion, After	4.31	5.65	4.98	4.04

Notes: Each coefficient comes from a separate difference-in-difference regression. Analyses are based on aggregated state-quarter prescription data by payer type and include all states. Data covers the period 2013Q1 to 2015Q1. All models include state fixed effects, fixed effects for each quarter in the data, and unemployment rate. Robust standard errors clustered by state are reported in parentheses. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

Table 2-3: **Heterogeneity by Drug Class**

Dependent variable: Ln (Medicaid prescriptions per 100 population)			
	(1)	(2)	(3)
Drug Class	DD coefficient	Is the effect statistically different from the effect on the remaining classes?	Share among all Medicaid prescriptions
All classes	0.19***		
	(0.06)		
Antibiotics	0.17***	§	0.101
	(0.06)		
Birth Control	0.22***		0.019
	(0.06)		
Cardiovascular medications	0.21***		0.140
	(0.07)		
Diabetes medications	0.24***	#	0.037
	(0.06)		
GI medications	0.17***	#	0.066
	(0.05)		
HIV/ Hepatitis	0.15**		0.011
	(0.07)		
Mental health medications	0.19***		0.149
	(0.07)		
Respiratory and Allergy medications	0.15***	#	0.118
	(0.05)		
Other	0.19***		0.359
	(0.05)		

Notes: Each coefficient is from a separate difference-in-difference regression. Regressions are based on aggregated state-quarter Medicaid prescription data covering 2013Q1 to 2015Q1 by therapeutic class. The pre-expansion period includes 2013Q1-2013Q4, while 2014Q1-2015Q1 represents the post-expansion period. All models include state fixed effects, fixed effects for each quarter in the data, and unemployment rate. Robust standard errors clustered by state are reported in parentheses. This analysis includes all states.
* Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.
# Significant at the 5-percent level. § Significant at the 10 percent level.

Table 2-4: **Effect of ACA Medicaid Expansions on Medicaid Prescription Utilization, by Product Type**

Dependent variable: Log (per capita Medicaid prescriptions)			
	(1)	(2)	(3)
	Brand	Generic	Other
Post x Expansion	0.14***	0.24***	0.20***
	(0.05)	(0.06)	(0.09)
Year and quarter fixed effects	Y	Y	Y
CBSA fixed effects	Y	Y	Y
Observations	7,028	7,029	6,707
Dependent variable means			
Expansion, Before	2.63	4.11	-2.69
Non-expansion, Before	3.00	4.37	-2.44
Expansion, After	2.84	4.45	-2.37
Non-expansion, After	3.07	4.50	-2.28

Notes: Each coefficient comes from a separate difference-in-difference regression. Analyses are based on aggregated CBSA-quarter prescription data by payer type and include all states. Data covers the period 2013Q1 to 2015Q1. All models include CBSA fixed effects, fixed effects for each quarter in the data, and unemployment rate. Robust standard errors clustered by state are reported in parentheses. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

Table 2-5: **Effect on Medicaid Prescription Utilization, by state copayment**

Dependent variable: Log (per capita Medicaid prescriptions)		
	(1)	(2)
	Low	High
Post x Expansion	0.22***	0.17**
	(0.07)	(0.07)
Range of copayments	$0	$0.3-$4.4
Observations	288	369

Notes: Each coefficient is from a separate difference-in-difference regression. Regressions are based on aggregated state-quarter Medicaid prescription data covering 2013Q1 to 2015Q1. The pre-expansion period includes 2013Q1-2013Q4, while 2014Q1-2015Q1 represents the post-expansion period. All models include state fixed effects, fixed effects for each quarter in the data, and unemployment rate. Robust standard errors clustered by state are reported in parentheses.
* Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

Table 2-6: **Effect of Drug Copayments on Medicaid Prescription Utilization**

Dependent variable: Log (per capita Medicaid prescriptions)					
	(1)	(2)	(3)	(4)	
	All classes	Antibiotics	Cardiovascular	Diabetes	
Post x Expansion x Copay	-0.038	-0.046	-0.021	-0.030	
	(0.028)	(0.028)	(0.042)	(0.038)	
Post x Expansion	0.187***	0.208***	0.150*	0.186***	
	(0.063)	(0.065)	(0.079)	(0.069)	
Post x Copay	-0.0004	-0.0003	-0.001	-0.001	
	(0.001)	(0.001)	(0.001)	(0.001)	
	(5)	(6)	(7)	(8)	(9)
	GI Meds	HIV/Hepatitis	Psychotherapeutic	Respiratory	Other
Post x Expansion x Copay	-0.037	-0.013	-0.036	-0.049*	-0.036
	(0.025)	(0.039)	(0.029)	(0.025)	(0.028)
Post x Expansion	0.188***	0.170*	0.186***	0.188***	0.197***
	(0.057)	(0.101)	(0.065)	(0.062)	(0.062)
Post x Copay	-0.0001	-0.001	-0.001	-0.001	-0.0001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)

Notes: Regression estimates are based on aggregated state-quarter data covering 2013Q1 to 2015Q1. The pre-expansion period includes 2013Q1-2013Q4, while 2014Q1-2015Q1 represents the post-expansion period. All models include state fixed effects, fixed effects for each quarter in the data, and state quarterly unemployment rate. Robust standard errors clustered by state reported in parentheses. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

Table 2-7: **Testing for Differential Pre-Expansion Trends in Medicaid Prescription Drug Utilization**

Dependent variable: Log (per capita Medicaid prescriptions)			
	(1)	(2)	(3)
	Pre-treatment trend test	Event study	Event study
Expansion x Trend (2014 Q1-Q4)	-0.002 (0.02)		
Expansion x 2013Q2		0.03 (0.03)	0.03 (0.03)
Expansion x 2013Q3		0.02 (0.05)	0.02 (0.05)
Expansion x 2013Q4		-0.002 (0.061)	-0.002 (0.061)
Expansion x 2014Q1		0.09 (0.07)	0.09 (0.07)
Expansion x 2014Q2		0.18** (0.08)	0.18** (0.08)
Expansion x 2014Q3		0.23*** (0.08)	0.23*** (0.08)
Expansion x 2014Q4		0.24*** (0.09)	0.24*** (0.09)
Expansion x 2015Q1		0.26*** (0.08)	0.26*** (0.08)
Includes *Expansion x linear time trend*	Y	N	Y
Observations	204	459	459

Notes: Analysis is based on aggregated state-quarter Medicaid prescription data. Estimate in column (1) uses data covering 2013Q1-2013Q4 and only includes separate time trends for expansion vs non-expansion states, while those in columns (2) and (3) use data from 2013Q1 to 2015Q1 and an event study approach. All models include state fixed effects, fixed effects for each quarter in the data, and unemployment rate. Robust standard errors clustered by state are reported in parentheses. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

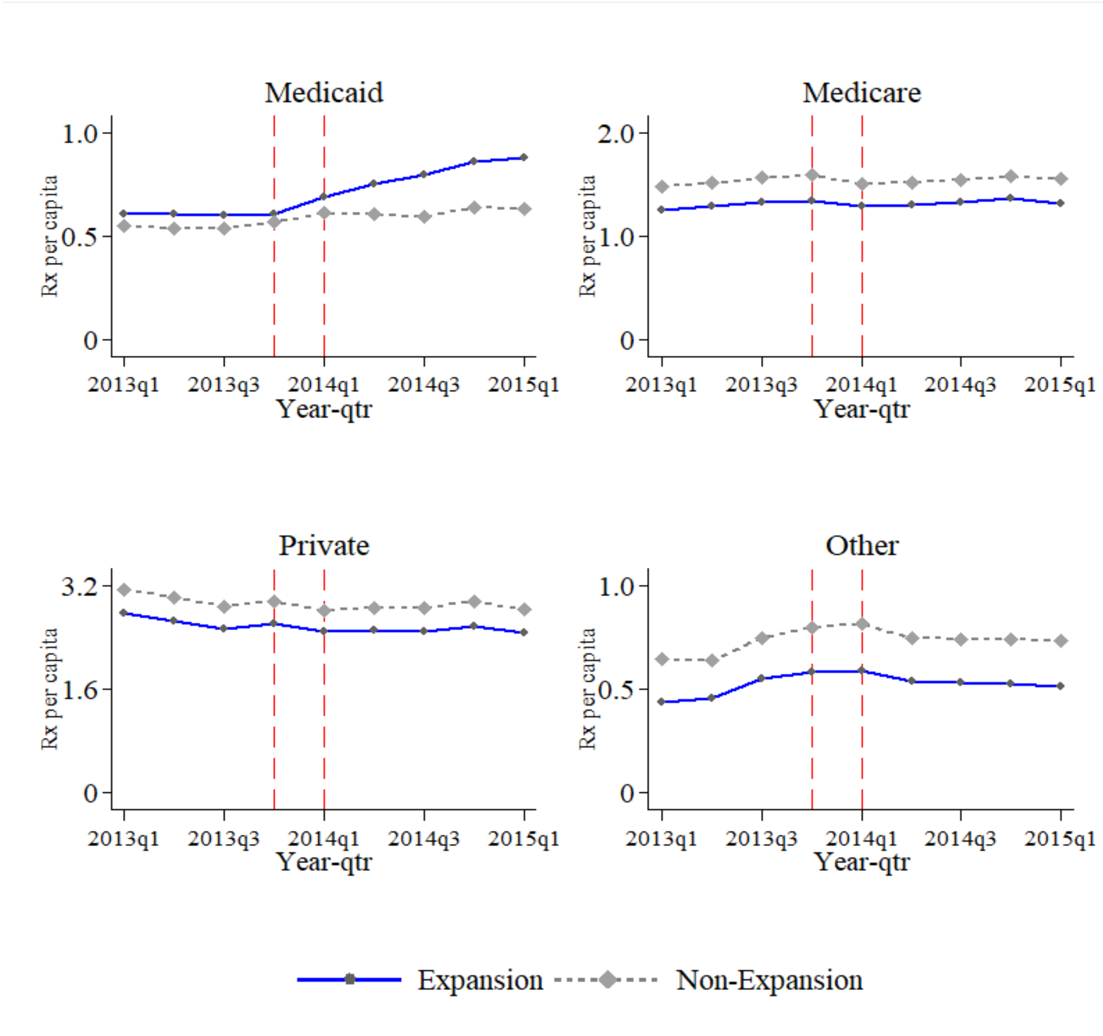
Table 2-8: **Effect of the ACA Medicaid Expansions on Medicaid Prescriptions, Triple Difference (CBSA-level Analysis)**

Dependent variable: Log (per capita Medicaid prescriptions)		
	(1)	(2)
	All States	Excl. DC, DE, MA, NY, VT
Panel A		
Post x Expansion x Pct Uninsured 2013	0.010***	0.011***
	(0.002)	(0.002)
Post x Expansion	-0.047	-0.080
	(0.064)	(0.071)
Post x Pct Uninsured 2013	0.002**	0.002**
	(0.001)	(0.001)
Median 2013 pct uninsured	33.7	34.4
Standard deviation of 2013 pct uninsured	9.5	8.9
Panel B		
Post x Expansion x Pct Poverty 2013	0.003	0.002
	(0.004)	(0.004)
Post x Expansion	0.181***	0.208***
	(0.057)	(0.058)
Post x Pct Poverty 2013	0.005***	0.005***
	(0.002)	(0.002)
Median 2013 pct poverty	16.9	17.1
Standard deviation of 2013 pct poverty	6.1	6.1
Panel C		
Post x Expansion x Pct Minority 2013	0.003**	0.003**
	(0.001)	(0.001)
Post x Expansion	0.185***	0.199***
	(0.054)	(0.023)
Post x Pct Minority 2013	0.002***	0.002***
	(0.0005)	(0.0005)
Median 2013 pct minority	12.6	12.9
Standard deviation of 2013 pct minority	19.1	19.4

Notes:

1. Analysis is based on aggregated CBSA-quarter Medicaid prescription data from 2013Q1 to 2015Q1. All models include CBSA fixed effects, fixed effects for each quarter in the data, and unemployment rate. Robust standard errors clustered by CBSA are reported in parentheses. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.
2. In column (1), estimates are based on all states being categorized into expansion vs non-expansion states.
    a. Expansion states: AR, AZ, CA, CO, CT, DE, DC, HI, IL, IN, IA, IL, KY, MD, MA, MI, MN, NV, NH, NJ, NM, NY, ND, OH, OR, PA, RI, VT, WA, WV.
    b. Non-expansion states: AL, AK, FL, GA, ID, KS, LA, ME, MI, MO, MT, NE, NC, OK, SC, SD, TN, TX, UT, VA, WI, WY.
3. For the analysis corresponding to column (2), the early expansion states of DC, DE, MA, NY, and VT were dropped from the sample.

Figure 2-1: **Unadjusted Trends in Per Capita Prescriptions Filled by Payer, 2013Q1-2015Q1**



Notes:

1. January 2014 expansion states include: AR, AZ, CA, CO, CT, DC, DE, HI, IL, IA, IL, KY, MA, MD, MN, NV, NY, NJ, NM, ND, OH, OR, RI, VT, WA, WV. IN, MI, NH, PA are excluded from this list and from this analysis as they expanded after January 2014 (but are included in the regressions with time-varying expansion definitions).
2. Non-expansion states include: AL, AK, FL, GA, ID, KS, LA, ME, MI, MO, MT, NE, NC, OK, SC, SD, TN, TX, UT, VA, WI, WY.
3. The first vertical line is drawn at 4th quarter of 2013, and the second vertical line is drawn at the 1st quarter of 2014, thus the area in between the two lines indicates the transition into the 2014 ACA Medicaid expansion.

2.8. **References**

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3 The Effect of Medicaid on Adult Hospitalizations: Evidence from Tennessee's Medicaid Contraction

3.1. Introduction

One of the primary ways the Affordable Care Act (ACA) aims to reduce uninsurance is by expanding Medicaid to previously ineligible non-elderly adults. However, as several states have chosen not to implement these expansions, the reduction in uninsurance among the non-elderly has been considerably weaker in non-expansion states (DeLeire et al., 2014). The high rate of uninsurance in the US left hospitals shouldering the burden of almost 60 percent of all uncompensated 2013 health care costs nationwide (Coughlin et al., 2014). Expanded Medicaid coverage is expected to lower hospital uncompensated care costs through reductions in uninsured visits or by decreasing hospital admissions through improved access to preventive or outpatient care. Hospital costs may further decrease if health insurance reduces the role of emergency departments (EDs) as the primary source of care and redirects healthcare consumption towards preventive ambulatory care. Thus, estimates of Medicaid's effects on the payer composition of hospitalizations and the corresponding implications for hospital finances are empirically relevant and of broader interest in current health care reform discussions.

Previous research by Sommers (2017) has posited that state adult Medicaid expansions led to reductions in mortality among non-elderly (under age 65) adults in expansion states relative to non-expansion states. Although their study estimates that the pre-2010 Medicaid expansions reduced mortality by nearly 6 percent, the mechanism by which this reduction occurs is not clearly understood. Because health insurance is an important determinant of access to care, Medicaid may have a protective effect on health through utilization of medical services. Medicaid may also reduce mortality through competing avenues such as its beneficial effect on financial stress related to affordability and access to health care (Baicker et al., 2013; Finkelstein et al., 2012a).

Most of the literature examining the effects of Medicaid has analyzed policies that extend health insurance coverage to low-income children and pregnant women, populations whose experience may not generalize to those targeted by the Medicaid provisions of the Affordable Care Act (ACA) of 2010, specifically non-elderly adults without dependent children. While several papers have examined the impact of Medicaid coverage among non-elderly adults by analyzing the changes in Oregon, Wisconsin and Tennessee, these studies tend to focus on insurance coverage and labor market effects. Little research has investigated how health insurance status affects health care utilization among newly eligible Medicaid populations, and the few studies that have been conducted do not provide consensus on these outcomes (DeLeire et al., 2013; Finkelstein et al., 2012a).

To analyze these implications empirically, we consider a 2005 policy change in Tennessee, which led to over 170,000 Medicaid beneficiaries losing coverage. We estimate the impact of this Medicaid contraction by comparing inpatient utilization in Tennessee to that in states that did not contract or expand Medicaid, before and after Tennessee's policy change. This identification strategy follows prior work on the labor market outcomes of Tennessee's Medicaid contraction (Garthwaite et al., 2014) as well as studies on the impact of health insurance expansions on hospitalization (Kolstad and Kowalski, 2012). Because Tennessee's Medicaid contraction primarily affected non-elderly non-pregnant adults while leaving other population subgroups – children, the elderly and pregnant women – relatively unaffected, we are also able to use a triple-difference study design taking advantage of this variation in exposure.

We begin by studying the impact of Tennessee's 2005 Medicaid contraction on the level of Medicaid and uninsured inpatient admissions among non-elderly adults. We expect to see an unambiguous decrease in Medicaid admissions. To the extent that those losing Medicaid were able to find private or other insurance coverage, we expect little increase in uninsured hospitalizations; if, on the other hand, many of those losing coverage were unable to find coverage, or if those who were at risk of hospitalization were especially likely to remain uninsured, we expect to see substantial increases in uninsured hospitalizations. Under the premise that the uninsured seek less care than the insured (Decker et al., 2013), we also expect a decrease in the aggregate volume of hospitalizations. Additionally, we examine whether the policy influenced the entry point for uninsured hospitalizations (i.e., through the ED or directly to the inpatient unit) and whether the hospitalizations were for preventable conditions. Evidence that hospitalizations were more likely to originate in the ED and that hospitalizations increased for preventable conditions would be consistent with reduced access to primary care or to a regular source of care (medical home) after the policy change.

Restricting Medicaid eligibility in Tennessee led to a 21 percent decrease in Medicaid coverage and a 61 percent increase in uninsurance among non-elderly adult hospitalizations, relative to the baseline and to changes in other states. Such a change has implications for hospital revenue streams, because most uninsured visits result in unpaid bills (Glied and Kronick, 2011). The results from our preferred specification suggest that the volume of Medicaid inpatient hospitalizations decreased by 24 percent, and uninsured inpatient admissions increased by 55 percent, relative to the baseline. These results are consistent with the prior studies on the effect of Tennessee's Medicaid policy change on insurance coverage and hospital uncompensated care costs (Garthwaite, Gross and Notowidigdo, 2014; 2015).

We find that increases in admissions originating in the ED explain 75 percent of the overall increase in uninsured inpatient stays; this is not surprising because those who lose Medicaid coverage and become uninsured face a more difficult process in being admitted directly to inpatient care. Prior evidence showed that losing Medicaid coverage is associated with a higher incidence of preventable hospitalizations (Bindman et al., 2008). However, in our data uninsured hospital visits increased for both preventable and non-preventable conditions by the same magnitude (more than 50 percent, compared to pre-disenrollment levels). While this is consistent with a shift in the expected payment source of hospitalizations from Medicaid to uninsured after TennCare contraction, it does not suggest a loss in access to preventive care per se. When we examine how Medicaid contraction affected the total volume of inpatient hospitalizations among non-elderly adults in Tennessee, we estimate a 24 percent decrease. This suggests that changes in hospital-based care may have played a role in explaining the connection between prior state Medicaid expansions and reduced mortality.

This study contributes to the literature by providing the first empirical evidence on the hospital care utilization impact of one of the largest contractions in state Medicaid policy. In doing so, it adds to existing work on the economic impact of the TennCare disenrollment (Garthwaite et al., 2014, 2018). By providing a new estimate we also contribute to the broader literature on the effect of Medicaid on medical care use among non-elderly adults, an important population for current health policy. In contrast to the several concurrent policy changes occurring under the Affordable Care Act's 2014 Medicaid expansions, Tennessee's Medicaid contraction is more of an isolated policy variation, allowing for a clearer interpretation of changes in Medicaid eligibility alone. Our results indicate the ACA Medicaid expansions can reduce uninsured hospitalizations, thereby decreasing hospital uncompensated care costs. Contraction of Medicaid coverage has received far less attention in the empirical literature than Medicaid expansion. Our study addresses this relatively understudied phenomenon and explores its consequences for access to care and use of uncompensated care. The results from our study provide information important for current policy discussions, given the future possibility of states reversing their ACA Medicaid expansions (Pear, 2017).

This paper is structured as follows. In Section 2, we describe the institutional details of Tennessee's Medicaid policy changes. Section 3 provides a review of the literature related to the causal effect of health insurance coverage on health care utilization. Section 4 explains the data, identification strategy and empirical framework. In Sections 5 we present results and examine the robustness of our empirical specification. Finally, Section 6 concludes and discusses implications for the ACA.

3.2. **Institutional Background**

From its inception in 1965, Medicaid typically provided coverage only to low-income populations that the federal government mandated it serve, such as children, pregnant women, parents and disabled individuals. Prior to the ACA, the federal government did not routinely share the costs of enrollees ineligible for traditional Medicaid (mostly low-income, childless adults), referred to as "optional" or "expansion" populations. Accordingly, most states denied Medicaid coverage to these populations.

One of the ways in which states could extend Medicaid coverage to non-mandatory populations prior to the ACA was through section 1115 demonstration waivers of the Social Security Act. To do so, states had to obtain authorization from the Health Care

Financing Administration (HCFA).[15] Tennessee obtained approval from the HCFA for its statewide Medicaid demonstration project, TennCare, in November 1993. TennCare was created with the objective of reducing uninsurance in Tennessee and reining in healthcare costs. In January 1994, Tennessee placed all of its Medicaid enrollees in managed care organization (MCO) contracted plans, aiming to control costs and use the savings generated to provide subsidized Medicaid coverage to optional populations. Uninsured individuals who qualified for TennCare coverage included those who did not have employer-sponsored insurance but whose annual income was too high to make them eligible for public insurance.[16] The demonstration project resulted in Tennessee achieving the highest Medicaid coverage rate of any state in the country, with 23 percent of its population enrolled in TennCare in 2004 (Farrar et al., 2007).

Despite these efforts, TennCare was unable to sustain its cost-control objective, and the state of Tennessee submitted a waiver amendment proposal to the Centers for Medicare and Medicaid Services (CMS, formerly HCFA) in September 2004. In November 2004, Governor Phil Bredesen announced that TennCare would stop covering the optional population (Chang and Steinberg, 2016). CMS approved the proposal in March 2005, which authorized disenrollment of TennCare beneficiaries over age 19 who were not eligible for the open Medicaid categories.

The disenrollment took place within a span of 3 months beginning late July 2005. In 2004, administrative records showed that there were 1,340,824 beneficiaries of Tennessee Medicaid, of whom 1,079,975 were in mandatory categories and 260,849 were in optional categories. Nearly 160,000 adults belonging to the optional population had been

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[15] States could use section 1115 waivers to expand their Medicaid programs subject to budget-neutrality, such that a demonstration project would not cost the federal government more than the existing Medicaid program. This could be achieved by using existing Medicaid funds or savings/revenue from other state programs and restricting the benefit packages of new enrollees and streamlining service delivery options to limit costs (Holahan et al, 1995).

[16] Eligibility in the optional categories of TennCare for the non-elderly required beneficiaries' annual income to be less than 400 percent of the federal poverty level (FPL) and included sliding scale premiums for beneficiaries with incomes above 100 percent FPL. These optional/expansion populations included non-elderly adults who either (1) were "uninsured" on March 1, 1993 and had continued to be without health insurance since or (2) belonged to the "uninsurable" category – individuals who were denied health insurance due to pre-existing health conditions (Moreno and Hoag, 2001).

disenrolled from TennCare by the fourth quarter of 2005, which represented a 12 percent reduction in Medicaid enrollment in the state. By 2006, the total number of adult TennCare beneficiaries disenrolled reached approximately 170,000. Comprising non-elderly adults, this disenrolled population from Tennessee was similar to those gaining coverage under the expanded Medicaid categorical eligibility provisions of the ACA; both groups were predominantly composed of adults without dependent children in the household (Garthwaite et al., 2014).

Because the TennCare disenrollees and those gaining Medicaid eligibility through the ACA expansions share key characteristics, our results can potentially imply connections between Medicaid eligibility and health care utilization among the newly eligible population. However, because the health status of the Tennessee disenrollees could have been worse than those gaining coverage under the ACA Medicaid expansions, our estimates regarding the disenrollment must be interpreted with caution. On the other hand, the disenrollees could be healthier because of the healthcare they received while insured.[17] If the ACA Medicaid expansions induce unhealthier adults to opt out of private health insurance and take up Medicaid (Clemens, 2015), it is possible that our estimates will have greater external validity for extrapolating to the non-elderly adult population gaining coverage as a result of the ACA.

3.2.1. **Literature on Health Insurance and Hospital Care Utilization**

Our paper is closely related to several quasi-experimental studies that examine the effect of health insurance on inpatient hospitalizations. Most of the prior research finds that extending health insurance coverage leads to increases in hospital utilization among children, young adults and the elderly (Anderson et al., 2012; Anderson et al., 2014; Antwi et al., 2015; Card et al., 2008; Dafny and Gruber, 2005). However, low-income non-elderly adults differ from these other populations in their prevalence of health conditions and

[17] Based on interviews with providers and current and former TennCare beneficiaries in the first year following the disenrollment, Farrar et al. (2007) report that 67,000 of the 170,000 Medicaid disenrollees were uninsurable as they did not qualify for any other insurance coverage due to poor health status; in addition, a large fraction of the disenrollees had multiple chronic conditions.

patterns of use (Decker et al., 2013), and thus health insurance may have distinct effects their use of hospital care.

Recent studies on Medicaid expansions in Oregon and Wisconsin provide evidence on Medicaid's hospital utilization effects among low-income adults. Using administrative data on hospital visits in Oregon, Finkelstein et al. (2012a) find that Medicaid coverage gained through random assignment led to a 30 percent increase in the probability of hospitalization among previously uninsured low-income adults who were categorically ineligible for traditional Medicaid. On the other hand, using administrative data from Wisconsin, DeLeire et al. (2013) find that inpatient hospitalizations *decreased* by 59 percent when previously uninsured low-income childless adults automatically gained Medicaid (BadgerCare) coverage. Massachusetts reform that extends private as well as public coverage, however, does not appear to increase hospitalizations (Kolstad and Kowalski, 2012). Hence, the evidence from these studies is inconclusive with respect to Medicaid's impact on inpatient hospital use among non-elderly adults.

3.2.2. **Literature on TennCare Contraction**

A small body of research has examined the TennCare contraction using quasi-experimental methods and shows sizable net increases in uninsurance and uncompensated care despite increases in private coverage. Garthwaite et al. (2014) examine the impact of the Medicaid contraction in Tennessee on insurance and labor market outcomes using within- and across-state variation in the Current Population Survey (CPS). They find that Medicaid coverage among non-elderly adults declined by 4.6 percentage points and that private insurance coverage increased by 1.6 percentage points in the two years following TennCare contraction. In a subsequent article, Garthwaite et al. (2018) examine aggregated administrative hospital-level data from the American Hospital Association and Joint Annual Reports from Tennessee Department of Health and find that uncompensated costs increased in Tennessee by 18 percent after the disenrollment compared to other states, and that the effects were concentrated in hospitals with EDs. The increase in prevalence of uninsurance after the Medicaid policy change in 2005 and the effect on hospital uncompensated care costs, suggests that healthcare utilization in Tennessee may have been affected as well.

Two observational studies report on ED health care use in Tennessee after the Medicaid contraction. Using a census of all Tennessee ED visits between 2004 and 2006 (inclusive), Heavrin et al. (2011) describe a 22 percent decrease in adult Medicaid visits and a 39.5 percent increase in uninsured adult visits in Tennessee after the contraction. They also note a 2 percent increase in the fraction of uninsured ED visits that result in inpatient hospitalization. Emerson et al. (2012) use the census of ED discharges for one Tennessee county (Davidson, which includes the city of Nashville) for 2003-2007 and report increases in both the number of ED visits for ambulatory-sensitive conditions and hospital uncompensated care costs after the disenrollment. Although their data also contained inpatient hospitalizations, the paper focuses only on ED visits; they do, however, note that the number of uninsured inpatient admissions among non-elderly adults increased by 42 percent, but there was only a very minor decline of 0.6 percent in Medicaid hospital admissions. Because these studies only use Tennessee data, it is unclear that we can draw causal lessons from them because some changes may reflect national trends. Additionally, unlike scheduled direct inpatient hospitalizations that are price sensitive, ED visits are less responsive to insurance status. Therefore, while these studies based on data from ED visits are informative, it is also important to examine utilization of hospital-based care, both scheduled and otherwise, using a quasi-experimental study design.

Our study goes beyond previous work as it is the first non-observational study to examine the healthcare-use impacts of Tennessee’s Medicaid contraction, and the first to focus specifically on inpatient hospitalization among non-elderly adults using patient level data. Our research contrasts with prior quasi-experimental studies from other states on adult Medicaid policy because we examine the impact of a loss of Medicaid coverage on healthcare utilization, whereas Finkelstein et al. (2012a) and DeLeire et al. (2013) analyze expansions of Medicaid coverage. Healthcare consumption may respond asymmetrically to Medicaid coverage gain or loss. For example, loss of Medicaid coverage may have less impact on utilization as patients are already familiar with the healthcare system, whereas transitioning from uninsurance to Medicaid may increase use of care but only after a lag due to difficulties in navigating the new and complex healthcare environment. We also extend the methods used in these earlier two studies: we use a cross-state identification strategy as well as within-state controls, whereas the findings from Oregon and Wisconsin were based on within-state control groups only.

The empirical approach comparing one state to several others is closest to that employed by Kolstad and Kowalski (2012) in studying the effect of the Massachusetts health care reform on inpatient hospitalizations, and to that of (Garthwaite et al., 2014, 2018) in analyzing the effect of Tennessee's Medicaid contraction on labor market outcomes. We also include within-state control groups to estimate a triple difference specification, similar to Garthwaite et al. (2014), who compare outcomes among those under 65 to those over 65.

3.3. **Method**

3.3.1. **Data**

Our empirical analysis uses the Nationwide Inpatient Sample (NIS) 2001-2009, which is part of the Healthcare Cost and Utilization Project (HCUP) of the Agency for Healthcare Research and Quality and contains patient-level data on all inpatient stays from a 20-percent national sample of community hospitals.[18] Each year of the data contains patient-level information on age, gender, race, source of admission, payer (including Medicaid, Medicare, private insurance, self-pay or no charge, Tricare, CHAMPUS, etc.), diagnosis and procedures performed for all admissions in a sampled hospital. The NIS includes state identifiers and, for some states, county identifiers for hospitals in the sample.

Administrative hospital data has a number of advantages over survey data. First, administrative data is superior with regard to accuracy of information on payer source due to a higher potential for measurement error in self-reports. Second, these data allow us to use four full years from pre- and post-treatment periods. Unlike the Oregon and the Wisconsin studies which use only one year of post-treatment data, this broader time span

[18] The American Hospital Association (AHA) defines community hospitals as "all non-Federal, short-term, general and other specialty hospitals, excluding hospital units of institutions." The NIS sample includes among community hospitals various specialty hospitals such as obstetrics-gynecology, orthopedic, ear-nose-throat and pediatric institutions as well as academic medical centers and public hospitals and long-term acute care facilities, all since 2005. Short-term rehabilitation hospitals, long-term non-acute care hospitals, psychiatric hospitals and alcoholism/chemical dependency treatment facilities are excluded from the NIS sample. NIS increased the number of states represented each year, from 33 in 2001 to 44 in 2009.

will help us observe changes in the utilization of medical services that may take longer to manifest. One of the limitations of the NIS is the lack of longitudinal patient identifiers, which prevents us from examining hospitalization patterns specifically among formerly Medicaid-insured patients. Furthermore, we are unable to observe utilization of primary care or outpatient care. Although alternative data sets (such as the Medical Expenditure and Panel Survey) contain individual-level information on health insurance coverage as well as socio-demographic characteristics and medical care utilization before and after the policy changes at the state level, the size of these datasets preclude the study of single-state policies for comparatively rare medical events like hospitalizations. Thus, as in prior studies on hospitalization responses to health insurance policy, we utilize cross-sectional administrative data.

3.3.2. **Empirical Strategy**

Difference-in-difference framework

To isolate the causal effect of TennCare contraction, we use several complementary identification strategies. Our main approach is a simple difference-in-difference framework similar to that used by Garthwaite et al. (2014) when studying labor-market outcomes. We compare hospitalizations among non-elderly adults in Tennessee with those in other Southern states (first difference) before and after Medicaid policy changes (second difference).[19] Next, we exclude all birth-related hospitalizations for this specification, because they are arguably less likely to be affected by the disenrollment.[20] Our reduced-form estimating equation, similar to that of Kolstad and Kowalski (2012), uses hospital data aggregated at the *hospital-quarter* level and is as follows:

$$Y_{ht} = \alpha + \gamma Post_t + \mu Treat_h + \delta Treat_h x Post_t + X_{ht}\beta + \theta_h + \tau_t + \varepsilon_{ht} \quad (3\text{-}1)$$

In equation (1) Y_{ht} denotes our outcome variable of interest for hospital *h* and time *t*. The regressor of principal interest here is *TreatxPost*, where (a) $Treat$ is a binary

[19] Of the 17 states that the Census defines as the South region, the NIS does not include 4 of them (Alabama, Delaware, Mississippi and Washington, D.C.) during the years 2001-2009.

[20] Non-birth admissions indicate inpatient stays in the sample with a major diagnostic code (MDC) other than 14. An MDC code of 14 indicates that the principal diagnosis for the date of discharge was pregnancy, childbirth or puerperium.

indicator that takes the value of 1 for Tennessee and 0 for other states; (b) $Post$ is a binary indicator taking the value of 1 for year 2006 and after, 0 otherwise and (c) the parameter δ is the difference-in-difference estimate of the impact of the Medicaid contraction in Tennessee. The vector X_{ht} includes patient-level demographic and clinical characteristics aggregated at the cell level.

The model includes year- and quarter-fixed effects to capture aggregate time trends that are common to both the treatment and the comparison states. We include the unemployment rate, and an interaction between the treatment indicator and the unemployment rate, to control for the effect of business cycles or other macroeconomic factors.[21] As the NIS is an unbalanced panel of hospitals, following Kolstad and Kowalski (2012) and Antwi et al. (2015), we include hospital-fixed effects to account for unmeasured hospital-specific factors that could affect utilization outcomes. We cluster standard errors at the state level to account for arbitrary correlations in error terms at the state level over time (Bertrand et al., 2004a). We exclude data from 2005, the year in which the contraction took place.[22] Based on prior studies on the effect of health insurance expansions on inpatient care utilization (Antwi et al., 2015; Kolstad and Kowalski, 2012; Miller, 2012), we use ordinary least squares to estimate the equation for ease of interpretation.

Our difference-in-difference identification strategy uses plausibly exogenous variation in insurance coverage due to the disenrollment. This approach rests on an assumption that the control states serve as an appropriate counterfactual for Tennessee,

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[21] We merge in unemployment rate data from the Bureau of Labor Statistics' Local Area Unemployment Statistics at the county-by-year level for the NIS states in which a county is identified; in other states, we merge in the statewide average annual unemployment rate. We also merge in county and state population estimates from the Census Bureau in a similar manner, for use in later specifications where outcomes are measured per capita. Because the population of Tennessee grew by roughly 9.7 percent between 2001 and 2009 (authors' calculations based on Census estimates), the use of per-capita measures separates the effect of secular trends in population size from the impact of the disenrollment on the extensive margin.

[22] For cleaner identification, in our main specification we have dropped observations from the year 2005, as disenrollment was announced in November 2004, began in July 2005 and continued through the last quarter of 2005. Following earlier work by Garthwaite et al. (2014), we have defined the post-period as the year 2006 and later. As there may have been anticipatory effects following the announcement of the policy change in the fourth quarter of 2004. We explored an alternative specification by dropping both years 2004 and 2005 from the analysis sample; we found our results to be similar (results available upon request).

absent Medicaid reform. This assumption is more likely to hold if pre-treatment trends between Tennessee and all other Southern states are similar; our empirical strategy tests this condition. We check the sensitivity of our DD results by augmenting this basic specification using two different approaches. First, we identify appropriate comparison states by utilizing (a) all states in the NIS as a comparison group, or (b) a group of NIS states identified through a synthetic control-matching procedure (Abadie et al., 2010). Second, we employ a triple-difference framework.

Triple-difference framework

We use a difference-in-difference-in-difference identification strategy to identify the causal effect of Medicaid on inpatient hospitalizations by using within-state comparison groups that were not directly affected by TennCare disenrollment. We exploit the fact that utilization among those under age 19, the elderly and pregnancy-related hospitalizations among the non-elderly is not likely to be directly affected by TennCare disenrollment.[23] Garthwaite et al. (2014) also use a DDD strategy by comparing labor market outcomes of the non-elderly to the within-state group over age 65.

In equation (2), we compare changes in insurance coverage rates by payer type and utilization among non-elderly adults in Tennessee relative to our three possible within-state control groups, relative to other states before and after the policy change in 2005.

$$Y_{ght} = \alpha + \beta_1 Treat_h + \beta_2 Post_t + \beta_3 WSA_g + \gamma_1 Treat_h x Post_t + \gamma_2 Treat_h x WSA_g + \gamma_3 Post_t x WSA_g + \delta Treat_h x Post_t x WSA_g + X_{ght}\emptyset + \theta_h + \tau_t + \varepsilon_{ght} \quad (3\text{-}2)$$

The variable Y_{ght} is the outcome of interest for inpatient admissions in age group *g* for hospital *h* and time *t*. The indicator variable WSA (within-state affected groups) takes the value of 1 for the targeted individuals between ages 20 and 64 (inclusive); it takes the value of 0 for each of the three alternative control groups in three separate specifications. This specification includes all covariates that were included in (1). Even though each of

[23] We use the elderly (age 66 and older) as a control group only to study total hospitalization volume outcomes. We do not use this control group strategy to study insurance outcomes because of the dominant role of Medicare. We identify the pregnancy group as non-elderly inpatient stays with primary diagnosis recorded as birth-related conditions (where the MDC code takes the value of 14).

the control groups is not directly affected by disenrollment, spillover effects may exist. To address this possibility, in the DDD framework we estimate whether TennCare disenrollment led to proportionally higher changes in the outcomes of interest among the affected group relative to those in the controls, in addition to comparing outcomes in Tennessee to those in other states before and after 2006. The coefficient of interest here again is δ.

Compared to the DD estimation strategy, the DDD method allows us to control for confounding shocks to non-elderly hospitalizations that differ between Tennessee and other states at the time of the TennCare policy change. At a national level, one concern with incorporating data on those over age 65 is that the implementation of Medicare part D in 2006 could undermine the use of elderly hospitalizations in the DDD because of possible spillover effects from drug use to hospitalizations (Kaestner et al., 2014b). As there is no reason to expect these spillover effects to differ across states, we also estimate a DDD using those over 65 and find similar results as the DD. A potential drawback to using pregnant women and children as within-state controls is that in 2005, CMS authorized TennCare to restrict pharmaceutical benefits for continuing non-pregnant Medicaid beneficiaries. However, these changes appear minor, and it is unclear whether they were binding and actually instituted.

3.3.3. **Hypotheses**

Following the Medicaid contraction in Tennessee, we expect to find that fewer hospitalizations in Tennessee were paid through Medicaid, and, to the extent that those losing Medicaid were unable to obtain other coverage, we expect to find that the share of uninsured hospitalizations increased.[24] Given that lack of health insurance increases the cost of obtaining medical care, we expect to find that the total volume of hospitalizations among non-elderly adults decreased after the Medicaid contraction as a result of a price

[24] Our measure of uninsured admissions includes inpatient stays categorized as self-pay (the patient was billed directly by the hospital) and no charge (neither patient nor insurer was billed; likely attributed to charity care). HCUP documentation reports that self-pay categories may not reflect full payment of outstanding charges, and that in the event of non-payment hospitals bear the burden of unpaid costs as uncompensated care (bad debt). On average, uninsured families with incomes less than 200 percent of the federal poverty level have enough assets to pay in full for only 4 percent of their hospitalizations (Glied and Kronick, 2011).

effect. Garthwaite et al. (2014) provide evidence that the Tennessee disenrollment increased private insurance coverage through an employment increase, thus partially offsetting the decrease in public coverage. However, those who are at risk of hospitalization are likely over-represented among those who were not able to find employment after losing Medicaid coverage. To the extent that disenrollees seeking hospital-based care obtained private insurance, we expect to also find an increase in private insurance coverage for hospitalizations.

The uninsured tend to preferentially use the ED, as opposed to office-based care (Anderson et al., 2014), due to legislative provisions such as the Emergency Medical Treatment and Labor Act (EMTALA) requiring hospitals to provide stabilizing care to all patients presenting at emergency rooms regardless of their ability to pay. In the NIS, we are able to observe whether an inpatient admission originated in the ED, although we are unable to observe outpatient ED visits in our data. We decompose uninsured inpatient hospitalizations by source of admission, specifying ED or otherwise. We expect to find a higher number of uninsured hospitalizations resulting from ED following the TennCare contraction.

A higher volume of uninsured hospitalizations could also reflect an adverse effect of coverage loss on access to primary or office-based care due to uninsurance, as the financial disincentives associated with lack of health insurance may induce the uninsured to forego ambulatory care, leading to more hospitalizations among the uninsured for preventable medical conditions. Therefore, we expect to find greater increases in hospitalizations among the uninsured for preventable relative to unpreventable conditions.

3.4. **Results**

3.4.1. **Descriptive Statistics**

Table 3-1 presents sample statistics of inpatient admissions for non-birth-related conditions among those aged 20 to 64 from NIS 2001-2009 for Tennessee and the other Southern states that serve as the comparison group.[25] There are broad similarities between

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[25] The states included here are those that the U.S. Census Bureau defines as the Southern states and are a part of the NIS sample, namely Florida, Georgia, Maryland, North Carolina, South

Tennessee and the comparison states in the age, gender and racial composition of their inpatient stays before TennCare contraction. In addition, the means for clinical characteristics are not very different across the two groups. Unsurprisingly, given the broad scope of Tennessee's Medicaid program prior to 2005, inpatient admissions in Tennessee are substantially more likely to be Medicaid insured and less likely to be uninsured relative to the comparison states in the pre-contraction period. After the TennCare disenrollment in 2005, a smaller fraction of hospitalizations are Medicaid insured and a greater fraction are uninsured in Tennessee compared to before 2005, reaching the same levels as in the comparison states (a little over 16 percent of hospitalizations are Medicaid insured and 13 percent are uninsured). By comparison, private insurance and Medicare move fairly similarly as sources of coverage among hospitalization in Tennessee and in control states over this time period. In the last panel, we present sample means for the population-adjusted volume of hospitalizations. The changes in volume outcomes across insurance types correspond directly with the changes in health insurance composition of the hospitalizations. Medicaid hospitalizations decline from 5.6 per 1,000 population in the state to 2.5 per 1,000, and increase in uninsured hospitalizations from 1.3 per 1,000 to 2.0 per 1,000, in Tennessee. Reflecting the larger decrease in Medicaid volume than the increase in uninsured volume, total volume of hospitalizations in Tennessee appears to have decreased in the post-contraction period. The volume of hospitalizations in total and by insurance type remained relatively stable in the comparison states.

We examine further the changes indicated in Table 3-1 between pre- and post-periods by depicting the exact trends in insurance composition and volume of hospitalizations by insurance type between years 2001-2009 in Figures 3-1 and 3-2. The three vertical lines in Figures 3-1 and 3-2 denote the announcement of TennCare contraction in 2004Q4, policy implementation in 2005Q3, and the beginning of the post-period in 2006Q1. Figure 3-1 shows that there is a sharp decline in Medicaid admissions and an uptick in uninsured hospital admissions in Tennessee immediately after the announcement in the fourth quarter of 2004; the trend continues through 2005 into the post-contraction period beginning in 2006. Figure 3-2 also shows the similarity in pre-policy

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Carolina, Virginia, West Virginia, Kentucky, Tennessee, Arkansas, Louisiana, Oklahoma and Texas.

trends in volume of admissions by payment source between treatment and control states, and the pronounced changes in volume of Medicaid and uninsured admissions 2004Q4 onwards in Tennessee compared to other states.

3.4.2. **Effect on Insurance Coverage Among Hospitalizations**

The results in panel A of Table 3-2 come from the DD regression specification (equation 3-1) and demonstrate the effect of TennCare contraction on the insurance composition of non-elderly adult patients in the inpatient sample. The proportion of inpatient admissions with Medicaid decreased by 6.3 percentage points. This represents a 21 percent decrease relative to the pre-treatment mean showing that 30.4 percent of hospitalizations in Tennessee were Medicaid insured. The proportion of uninsured inpatient admissions in Tennessee increased relative to the comparison states; the coefficient estimate of a 4.1 percentage point increase in column 5 of panel A implies an approximately 61 percent increase in the proportion of uninsured inpatient admissions following the policy change, relative to the pre-contraction mean of 6.7 percent. Taken together, these results on changes in insurance coverage composition suggest that the contraction led to a shift in the patient payment composition for hospitals from Medicaid to uncompensated care. Our estimate of the impact of the policy change on Medicaid coverage among inpatient hospitalizations is consistent with findings of general population level insurance changes in prior literature; Garthwaite et al. (2014) find reduced Medicaid coverage of 5.1 percentage points and an increase in private coverage by 1.7 percentage points, among non-elderly adults in Tennessee. Our results suggest that estimates of increased private insurance in the general population do not generalize to this relatively unhealthy population seeking hospital-based care.

3.4.3. **Effect on Volume of Admissions**

Panel B of Table 3-2 shows the impact of Medicaid contraction on the volume of inpatient admissions by insurance status. The dependent variable in panel B is the population adjusted rate of hospitalizations at the hospital-quarter level (number of admissions divided by county population in 10,000s). The point estimates for Medicaid and uninsured hospitalization in panel B have the same sign as the corresponding results in panel A and are all statistically significant at the 1-percent level. The estimates indicate a decrease in Medicaid visits and an accompanying increase in uninsured visits. Relative to an average population adjusted Medicaid hospitalizations rate of 72.259 in Tennessee, the point estimate of -17.481 in column 1 implies a reduction of nearly 24 percent. The coefficient estimate of 11.066 on uninsured hospitalizations suggests that among the non-elderly hospitalized population there was a 55-percent increase in the volume of uninsured visits post-disenrollment relative to the initial pre-treatment mean of 20.316 uninsured hospital admissions. These estimates are consistent with prior studies that find a decrease in Medicaid and an increase in uninsured inpatient hospitalizations following TennCare contraction (Emerson et al., 2012; Garthwaite et al., 2018; Heavrin et al., 2011). The last column in panel B shows that there a negative but statistically insignificant effect on overall admissions after the disenrollment. In all later specifications in section 5.7 using alternate control groups, this effect is statistically significant. This implies that some of those who lost Medicaid still incur hospitalizations but now with no source of insurance (replacing just over half of all Medicaid hospitalizations that would otherwise have occurred; 11.066 /17.481), while the remaining hospitalizations are less likely to occur at all.[26]

3.4.4. Effect on Source of Uninsured Admissions

To understand the nature of the change in inpatient admissions better, in Table 3-3 we now examine whether contraction affected the source of uninsured admissions. The first 2 columns indicate whether the admission originated in the emergency room. Of the total increase of 11.066 per-capita uninsured admissions in Tennessee (DD estimate from Table 3-2, panel B, column 5), the estimates in columns 1 and 2 show that 8.391 of these took place through the ED, with the remaining 2.686 through non-ED sources. Both these estimates are statistically significant at the 1 percent level. This suggests that nearly 75 percent of the total increase in volume of uninsured admissions in the post-disenrollment period was driven by an increase in those that originated in the ED, which is much larger than the increase in non-ED uninsured admissions. In other words, among uninsured

[26] We also estimate a triple-differences model using the over-age-65 inpatient admissions as the third within-state control group to obtain similar effects on total population adjusted hospital admissions, suggesting that the Medicaid contraction led to a decline in the aggregate volume of inpatient admissions (results are available upon request).

inpatient admissions, rise in admissions through the emergency room outpaced rise in admissions through non-ED sources.

When compared to baseline means of per-capita uninsured visits, the implied treatment effect is 64 percent and 37 percent for ED and non-ED visits, respectively. This finding is consistent with a scenario in which cost-related barriers to care lead the uninsured to seek care through emergency rooms. This result is comparable in direction to Heavrin et al. (2011), who find an increase in uninsured ED visits resulting in inpatient admission after the Medicaid contraction in Tennessee. Likewise, Garthwaite et al. (2018) find that TennCare disenrollment led to higher uncompensated care costs in Tennessee hospitals, and that the increase was more pronounced among hospitals with an ED.

3.4.5. **Effect on Preventable Admissions Among the Uninsured**

Inpatient hospitalizations due to ambulatory-care-sensitive conditions[27] (ACSC) are considered to be potentially preventable through timely and/or good-quality care provided in a less resource-intensive outpatient setting. Hence, the Agency for Healthcare Research and Quality (AHRQ) bases their prevention-quality indicators (PQIs) on inpatient ACSC hospitalizations to measure population-level access to good-quality preventive care in an outpatient or office-based setting. Given this inverse relationship between access to primary care and preventable hospitalizations, we expect the disenrollment to increase uninsured hospitalizations for preventable medical conditions.

Columns 3 and 4 of Table 3-3 report DD estimates of the impact of the Medicaid contraction on uninsured inpatient admissions, decomposed by whether the medical condition is unpreventable in nature. While we expect that as Medicaid contracts, former Medicaid patients may now appear as uninsured patients and thus increasing uninsured hospitalizations, there may also be other implications for uninsured hospitalizations beyond this simple accounting effect. If Medicaid contraction reduces access to ambulatory care for the newly uninsured, they may now appear more often for preventable hospitalizations. The direction of these point estimates in columns 3 and 4 of Table 3-3

[27] The list of ACS conditions used in this study includes but is not limited to medical conditions such as COPD, hypertension, CHF, uncontrolled diabetes, angina without procedure and adult asthma as specified in AHRQ guidelines.

suggest that uninsured inpatient hospitalizations increased for preventable as well as non-preventable conditions after the changes in TennCare eligibility, and by slightly more than 50 percent relative to the baseline pre-disenrollment levels. Thus, although we expect that Medicaid contraction also reduced access to ambulatory care, the pattern of hospitalization change does not provide evidence consistent with a shift in composition towards more preventable hospitalizations by this measure of "preventability." These estimates must, however, be interpreted with caution, as they only provide implicit confirmation of our hypothesis given that our data does not provide information on utilization in other settings.

In prior work, Kolstad and Kowalski (2012) find corresponding declines in preventable admissions among the non-elderly population after the Massachusetts health care expansion. Our results are also similar in direction to Emerson et al. (2012) who find that TennCare contraction was associated with higher uninsured ACSC admissions.

3.4.6. **Effect on Intensity of Hospital Treatment**

The results from Table 3-2 suggest that the relative changes in payer mix may have had an adverse impact on hospital finances due to shifts in the expected source of payment for inpatient admissions; changes in patient health mix, however, could potentially exacerbate such an effect. These second-order effects may capture changes in the characteristics of the patient pool. In particular, it can be argued that the disenrollment may have altered the health mix of the uninsured hospital admissions, as this group now includes individuals with poorer health status who do not qualify for health insurance from sources other than TennCare. An increase in post-2005 intensity of treatment among the uninsured, as measured by the number of procedures performed during an inpatient stay and the length of stay, would provide evidence of such an effect. We display these results in Table 4.[28]

In columns 1-6 of Table 3-4 we report findings on intensity of treatment among all admissions and in the sample of uninsured visits only. Columns 5 and 6 show that length of stay increased among uninsured hospital visits, while the number of procedures appears statistically unaffected (in column 4). This increase in length of stay among the uninsured

[28] The number of procedure codes reported in the NIS varies by state. We record up to a total of 6 procedures, which is the minimum number reported by the states during the period 2001-2009.

provides suggestive evidence that the disenrollment shifted the composition of uninsured patients towards the less healthy. However, the disenrollment did not appear to have any effect on any of the measures of treatment intensity when all admissions were considered together. These effects on volume and treatment intensity together suggest an upward shift in the volume of uncompensated hospitalizations, a likely decrease in total hospitalizations, especially of those that are reimbursed, as well as an increase in resource intensity for treatment of the uninsured, which potentially exacerbates the fiscal pressure on hospitals.

3.4.7. **Sensitivity Checks**

We test the validity of our DD identification strategy by employing a series of sensitivity checks. Although Figures 3-1 and 3-2 show that hospitalization trends in Tennessee seemed to match other Southern states in the period prior to TennCare contraction, here we formally test the assumption of parallel pre-treatment trends. We regress each of our outcome variables on the Tennessee indicator interacted with a linear time-trend (measured in quarters) using NIS data for 2001-2004. The results from this test are displayed in Appendix Table C-1, panels A and B. A statistically significant coefficient on the regressor *TrendxTreat* would indicate that Tennessee and the other Southern states experienced different trends in that outcome prior to the Medicaid contraction, and that a DD estimator might pick up the continuation of this divergence in trends. The key coefficients in panel A, where the outcomes are fractions of admissions by insurance type, are not statistically significant, and support our identification strategy. Among the volume outcomes of interest in panel B, the uninsured visits outcome has statistically significantly different pre-trends as indicated by the interaction terms, although the magnitude is substantially smaller than the corresponding DD estimate.[29] For example, the uninsured column in panel B of Appendix Table C-1 shows a coefficient of 0.620 (p-value<0.01) while the DD coefficient corresponding to this model, in panel B of Table 3-2, shows a magnitude of 11.066 (p-value<0.01). Nevertheless, this raises the concern that our estimates may be biased if we do not account for these pre-existing trends, and so we include state linear time trends in a sensitivity analysis. In Appendix Table C-2 panel D,

[29] Appendix Table C-8 reports estimates from a similar pre-trend analysis where we use all other states in the NIS 2001-2004 as controls and obtain similar results.

we find that this inclusion does not change our estimates in a substantial way. The largest difference in magnitudes of the point estimates between Table 3-2 panel A and Appendix Table C-2 Panel D is in the Medicaid column; the coefficient in Table 3-2 is -0.063 and in Appendix Table C-2 Panel D is it -0.047 (with the state time trend included), and both are statistically significant at the 1 percent level. We also present results using quadratic and cubic state time trends (Appendix Table C-2 and C-4); they too show consistent results.

Our main analysis compared Tennessee to all other Southern states in our data. We estimate the sensitivity of this choice of control groups by using all states in the NIS sample as comparison states and present the coefficient estimates for the proportion and volume outcomes in panel A of Appendix Tables C-2 and C-4, respectively[30]. These results confirm our findings from the baseline model; the coefficients on our main outcomes of interest – Medicaid and uninsured visits – are similar in magnitude and precision. We explore the choice of control states further by using a synthetic control matching technique as outlined by (Abadie et al., 2010). We use both levels and trends of Medicaid hospitalizations and the control variables in the pre-treatment period as criteria to obtain the appropriate subset of control states after aggregating our patient-level data to state-year cells.[31] As clustering at the state level tends to result in unreliable standard errors when the number of states is less than 11 (Angrist and Pischke, 2008), we cluster the standard errors in the synthetically matched DD specification at the state-year level, similar to (Courtemanche and Zapata, 2014) analysis of the effect of the healthcare reform in Massachusetts using control states picked through a synthetic match. In Appendix Tables C-2 and C-4 (for payment

[30] Note that both Missouri and Massachusetts experienced considerable changes in health insurance coverage during the implementation and post-disenrollment periods in Tennessee. Because such contemporaneous changes can bias estimation, these two states are not entered as control states in any specification. Missouri introduced substantial cutbacks in its Medicaid program in 2005, resulting in more than 100,000 beneficiaries losing coverage. Massachusetts, meanwhile, adopted legislation in 2006 with the goal of attaining near-universal health insurance coverage in the state, which included a large-scale Medicaid expansion.

[31] The synthetic control group chosen by this algorithm when matched on per-capita Medicaid admissions (levels) is 36.7% KY, 24.1% NY and 39.3% TX. By matching on the rate of change in per-capita Medicaid admissions (trends) the resulting group of control states is composed of 13.1% CO, 30.6% MD, 42.2% NJ, 11.5% NY and 2.7% UT. Control variables include age, gender, race, unemployment rate, poverty rate and median income averaged over the 2001 to 2004 sample period. In both cases, we reweight the sample using the synthetic weights to obtain the control group.

composition and volume of hospitalizations, respectively), panel B presents the DD estimates where control states were matched on levels, while for the estimates in panel C the control group was matched on trends, using pre-2005 data on Medicaid admissions. The coefficients on Medicaid and uninsured admissions remain statistically significant across specifications and are largely similar when compared to the estimates in Table 3-2, providing further evidence that our DD estimates are not sensitive to the exact choice of control states. For all these tests described in Appendix Tables C-2 and C-4, we have also included linear, quadratic and cubic time trends (panels D, E and F) and find that, as with our main specification, results are largely insensitive to this addition.

We next present results of our DDD specification (equation 3-2) using (1) the Southern states, and (2) all NIS states as comparison states. Appendix Tables C-3 and C-5 present results in which we use two alternative within-state control groups: those aged 0-19, and non-elderly hospitalizations that are pregnancy related. Our results from this specification point to similar conclusions as those from the DD, although the DDD estimates are generally slightly larger. The largest difference in coefficients is for the volume of Medicaid hospitalizations, where the point estimate in Table 3-2 panel B (DD) is -17.481 and the DDD (Appendix Table C-5, panel B) is -24.222; both are statistically significant at the 1 percent level.

To check the robustness of our results in Table 3-3 examining source of admission for uninsured hospitalizations and whether visits are for conditions considered preventable, we present in Appendix Tables C-6 and C-7 results of each of the specifications conducted for our main analysis. This check includes testing the sensitivity of control group choice, of including state time trends, and of estimating the DDD specifications. Overall, our conclusions remain unchanged, as evident in the similarity of the point estimates as well as statistical precision across specifications. These results confirm the earlier finding that the impact of the Medicaid contraction was disproportionately higher for uninsured admissions taking place through the ED and that both preventable and non-preventable uninsured admissions increased.[32]

[32] In addition, we estimated equation 2 by using those over age 65 as a within-state comparison group. The DDD estimates for per-capita uninsured admissions and each of the four outcomes in

Finally, in Appendix Table C-9, we examine the impact on total volume of hospitalizations using different control groups and the results confirm our initial result from Table 3-2. Using all the NIS states as the control group, we now find that the Medicaid contraction led to a decline in total volume of hospitalizations (statistically significant at the 5 percent level, and similar in terms of magnitude). In the DDD specifications this negative effect remains statistically significant, suggesting that neither non-elderly nor Tennessee-specific factors are confounding these results. Therefore, we interpret these results for total volume of hospitalizations as evidence of a decrease in overall inpatient utilization in response to the Medicaid contraction.

3.5. **Discussion**

Our estimates of the impact of Medicaid coverage changes on the utilization outcomes from Table 2 panel A and B are qualitatively comparable to findings from earlier literature on the impact of health insurance on inpatient hospitalizations among adults. In particular, using a similar difference-in-difference model and the same data source as our study, Kolstad and Kowalski (2012) find that the near-universal health insurance coverage expansion in Massachusetts resulted in a 2.31 percentage-point drop in uninsurance. Compared to the pre-reform uninsurance rate of 6.43 percent, this drop represents a decline of 36 percent. Furthermore, Antwi et al. (2015) find the dependent coverage mandate of the ACA to have decreased uninsured hospitalizations among young adults between ages 19 to 29 in the NIS sample by 12.7 percent. Our results are also consistent with evidence from a randomized controlled trial conducted in Oregon that found a 30 percent increase in inpatient hospitalizations among those provided Medicaid (Finkelstein et al., 2012). However, our estimates differ from results in Wisconsin DeLeire et al. (2013), where insurance expansion is associated with a decrease in inpatient utilization. While the direction of the impact of the Medicaid contraction is similar to the effect of Medicaid expansions studied in prior literature, the effect on magnitude may not be symmetric. Medicaid expansions can potentially increase access and utilization of both inpatient and

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Table 4 are reassuringly similar to our estimates from the baseline specification in terms of direction, magnitude and precision (results are available upon request).

outpatient care that may be complements or substitutes in consumption. Consequently, the net effect on utilization will depend on the relative strengths of these opposing forces.

Unlike our results for Medicaid and uninsurance, the coefficient estimates for Medicare, private and other insurance are generally less consistent across specifications in direction and precision, and are therefore not as informative. Garthwaite et al. (2014) present evidence that as public insurance coverage dropped, private insurance coverage increased, and a crowd-out rate of 34.6 percent resulted among childless adults in the Current Population Survey data in response to TennCare disenrollment. They argue that the disenrollment led to higher labor supply among childless adults, suggesting that the disenrolled beneficiaries who valued health insurance obtained jobs with health insurance. This may have contributed to higher private insurance coverage rates. In contrast, we find no systematic evidence that share of privately insured hospitalizations increased in our data possibly due to differences in the incidence and morbidity of health conditions between the populations covered in the CPS and the NIS.

Given that the underlying health conditions of the population of interest in Garthwaite et al. (2014) are unknown, it is plausible that that their findings are not generalizable to a hospital-care-seeking population that is likely negatively selected in terms of health status. In particular, their estimate of crowd-out for the sub-sample reporting poor health was less than one-third of the size of the estimate for those reporting good health. This finding suggests that the comparatively unhealthy individuals seeking hospital-based care were unlikely to have obtained private insurance coverage after being disenrolled from Medicaid. Notably, in their subsequent analysis using aggregate hospital data, Garthwaite et al. (2018) report a decline in the number of privately insured hospitalizations in 2006. Although their study is limited to one year of post-disenrollment data in contrast to our four years, this result provides indirect evidence in support of our finding that there was no substantial increase in private insurance coverage in the sample of hospital admissions.

One of the advantages of using aggregate hospital data is that it provides a clearer understanding of the spillover effect of TennCare disenrollment on the local healthcare market through its effect on hospital finances. Based on the volume results, not only do we find a reduction in Medicaid admissions after the disenrollment, but we also find an increase in uninsured admissions. To the extent that the direction of this shift from Medicaid to uninsured represents a shift in expected source of payment towards uncompensated care, we conclude that, overall, TennCare disenrollment had a negative impact on hospital finances. Given that the ACA Medicaid expansions aim to reduce uninsurance, the direction of our estimates suggest that hospital uncompensated care costs should decrease in expansion states. This prediction is consistent with recent findings in the literature that the 2010 Medicaid expansion in Connecticut, under the ACA provisions, led to reductions in uncompensated care costs incurred by hospitals in the state (Nikpay et al., 2015).

3.6. **Conclusion**

In this paper we present the first estimates of the impact of Tennessee's Medicaid contraction in 2005 on inpatient hospital care utilization among non-elderly adults. By comparing the insurance composition of inpatient admissions in Tennessee to other Southern states using administrative data on a nationwide sample of inpatient hospital stays, we find that the prevalence of uninsurance among hospital admissions increased by nearly 60 percent after the Medicaid contraction. As expected, the prevalence of Medicaid among hospitalizations decreased by about 20 percent. The volume of hospitalizations with Medicaid decreased by 25 percent and uninsured hospitalizations were 60 percent higher. We also find increases in uninsured inpatient admissions that originated in the ED which is not surprising given that the uninsured have difficulty scheduling direct admission to inpatient care. There is also evidence that the contraction reduced the overall volume of hospitalizations, although this highlights the importance of control group choice, as the effect is statistically insignificant when other Southern states are used as the comparison group. This result is consistent with recent studies that find health insurance increases use of inpatient medical care in other populations (Anderson et al., 2012; Anderson et al., 2014; Antwi et al., 2015; Card et al., 2008; Dafny and Gruber, 2005). This finding also supports the possibility that Medicaid and mortality rates may be connected through increased hospital care utilization (Sommers, 2017).

While prior studies find that pre-ACA state Medicaid expansions reduced mortality among non-elderly adults, the mechanism driving this result was unclear. To date, evidence on whether Medicaid increases inpatient utilization among non-elderly adults is inconclusive. Given that the ACA state Medicaid expansions target non-elderly adults, it is valuable to understand how the newly eligible population utilizes medical care. The results from this study suggest that increased use of hospital-based care due to Medicaid coverage expansions may have been a plausible pathway leading to mortality reductions among the non-elderly population. This evidence also suggests that state Medicaid expansions following the ACA guidelines can potentially improve access and utilization in expansion states. Evidence already exists that early ACA provisions for young adults have reduced out of pocket costs for the uninsured (Busch et al., 2014), and that pre-ACA state Medicaid expansions decreased personal bankruptcies, plausibly through reductions in out-of-pocket medical expenditures (Gross and Notowidigdo, 2011). Correspondingly, the ACA-related Medicaid expansions may lower out-of-pocket spending, particularly for high-cost hospital care.

Our results also shed light on the potential negative spillover effect that Medicaid disenrollment may have on hospitals through increased uninsured visits. By focusing on a policy change that targeted non-elderly adults (who were neither disabled nor pregnant), our results suggest that Medicaid expansions will decrease uninsured hospitalizations, thereby reducing use of hospital uncompensated care. Back-of-the-envelope calculations suggest that a predicted 12-13 million increase in non-elderly Medicaid and CHIP enrollment due to the ACA (Congressional Budget Office, 2014) will reduce uninsured inpatient stays by 2.9 to 3.1 million each year in the expansion states. Under the assumption that the uninsured utilize inpatient care at the same rate as Medicaid beneficiaries, we divide 17.481, the point estimate of per-10,000 Medicaid inpatient visits, by the pre-treatment mean of 72.259, and multiply by the estimated Medicaid enrollment increase of 12-13 million. As of July 2015, 19 states have decided not to implement the ACA Medicaid expansions; if, following ACA guidelines, reductions in Medicaid and Medicare disproportionate-share hospital (DSH) payments outpace the decrease in uncompensated care costs at hospitals, then hospital finances may be adversely affected in non-expansion states. Given that our analysis is based on a single-state study of a Medicaid contraction, these estimates must be interpreted with caution with respect to the ACA.

The income-based-eligibility approach of Medicaid and Medicaid's re-enrollment policies may lead enrollees to involuntarily drop out of Medicaid over time due to fluctuations in income and employment, which can shift enrollees across income eligibility thresholds. Sommers (2009) estimates that nearly 43 percent Medicaid beneficiaries lose coverage within 12 months of enrollment due to transitions in employment, family structure or income, thereby facing uninsurance. Nonetheless, little is known about the implications of loss of Medicaid coverage for use of medical services or its effect on the healthcare system. We find evidence that the post-disenrollment increase in uninsured hospitalizations was primarily due to an increase in inpatient admissions originating in the ED. This finding provides suggestive evidence of the adverse effect of Medicaid disenrollment on access to medical care. Indeed, the increase in preventable hospitalizations among the uninsured lends further support to the adverse impact of loss of Medicaid coverage on access to care and to the potential negative spill-over effects on hospital finances. In light of these results, ACA Medicaid expansions that reduce uninsurance will likely decrease ED use by the uninsured as well as uninsured admissions for ambulatory-case sensitive conditions in expansion states, thereby reducing hospital uncompensated care costs.

3.7. Tables and Figures

Table 3-1: **Summary Statistics for the Treatment and Comparison States**

	Tennessee		Southern States	
	Before	After	Before	After
Demographic characteristics				
Age	47.4	48.1	46.8	47.5
Female	52.7%	51.9%	53.1%	52.2%
White	75.4%	72.5%	48.1%	50.2%
African-American	20.2%	17.8%	16.9%	18.3%
Hispanic	0.6%	1.2%	8.5%	8.7%
Other	2.3%	1.1%	2.9%	3.5%
Clinical characteristics				
Number of diagnosis codes	5.50	7.74	5.45	7.36
Length of Stay (LOS)	4.75	4.81	4.63	4.72
Log(LOS)	2.22	2.23	2.16	2.18
Number of procedure codes	1.60	1.66	1.48	1.62
Health Insurance Status				
Medicaid	24.4%	16.6%	13.8%	15.0%
Uninsured	5.8%	13.0%	12.4%	14.9%
Private	46.6%	43.0%	50.3%	44.0%
Medicare	20.9%	24.9%	16.9%	19.1%
Other Insurance	2.2%	2.6%	6.6%	7.0%
Hospitalization rates by Insurance Type				
Medicaid	5.6	2.5	1.8	1.6
Uninsured	1.3	2.0	1.5	1.5
Private	10.7	6.6	6.6	4.6
Medicare	4.8	3.8	2.2	2.0
Other Insurance	0.5	0.4	0.9	0.8
Total	22.9	15.3	13.1	10.5
Number of Observations	458,664	321,255	5,131,400	4,406,199

Notes: Sample estimates obtained from NIS 2001-2009 for all adults between ages 20-64. We use non-birth admissions only. Demographic characteristics (excluding age) and health insurance status variables are measured as percentages. Hospitalization rates are measured at the state-year level per 1,000 population. Before-period includes years 2000-2004 and after-period represents years 2006 and later. Control states consist of the Southern states in the NIS except Tennessee. See section 4.5.1 for details.

Table 3-2: **Effect on Inpatient Admissions by Source of Coverage (DD Estimates using Southern States as Comparison Group)**

	(1)	(2)	(3)	(4)	(5)
	A. Fraction of Admissions				
	Medicaid	Private	Other	Medicare	Uninsured
PostxTreat	-0.063***	0.007	0.004	0.011***	0.041***
	(0.005)	(0.004)	(0.003)	(0.002)	(0.004)
Dependent Variable Means					
Treatment, Before	0.304	0.349	0.016	0.265	0.067
Control, Before	0.157	0.418	0.065	0.234	0.126
Treatment, After	0.197	0.322	0.021	0.342	0.117
Control, After	0.151	0.373	0.052	0.307	0.118
Observations	11,479	11,479	11,479	11,479	11,479

	(1)	(2)	(3)	(4)	(5)	
	B. Hospitalization Rate					
	Medicaid	Private	Other	Medicare	Uninsured	Total
PostxTreat	-17.481***	-5.454	0.120	-5.012	11.066***	-15.585
	(3.146)	(3.497)	(2.166)	(8.159)	(1.237)	(13.172)
Dependent Variable Means						
Treatment, Before	72.259	139.259	6.911	71.628	20.316	313.692
Control, Before	46.416	145.397	23.148	63.242	34.674	314.018
Treatment, After	56.918	122.881	8.022	104.416	33.399	328.367
Control, After	59.908	125.561	20.079	94.101	35.329	335.777
Observations	11,481	11,481	11,481	11,481	11,481	11,481

Notes: Sample estimates obtained from NIS 2001-2009 for all adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and the Southern states as comparison group. We exclude year 2005 from sample. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. *Significant at 0.10, **significant at 0.05, ***significant at 0.01.

Table 3-3: **Effect on Source and Type of Uninsured Admissions (DD Estimates using Southern States as Comparison Group)**

	(1)	(2)	(3)	(4)
	Dependent Variable: Uninsured Hospitalization Rate			
	Uninsured Admissions through ED	Uninsured Admissions not through ED	Uninsured Admissions for Preventable Conditions	Uninsured Admissions for Non-Preventable Conditions
PostxTreat	8.391***	2.686***	1.993***	9.073***
	(1.066)	(0.357)	(0.242)	(1.027)
Dependent Variable Means				
Treatment, Before	13.030	7.267	3.547	16.769
Control, Before	23.873	10.684	5.788	28.886
Treatment, After	23.992	9.355	4.762	28.637
Control, After	24.512	10.664	5.108	30.222
Observations	11,481	11,481	11,481	11,481

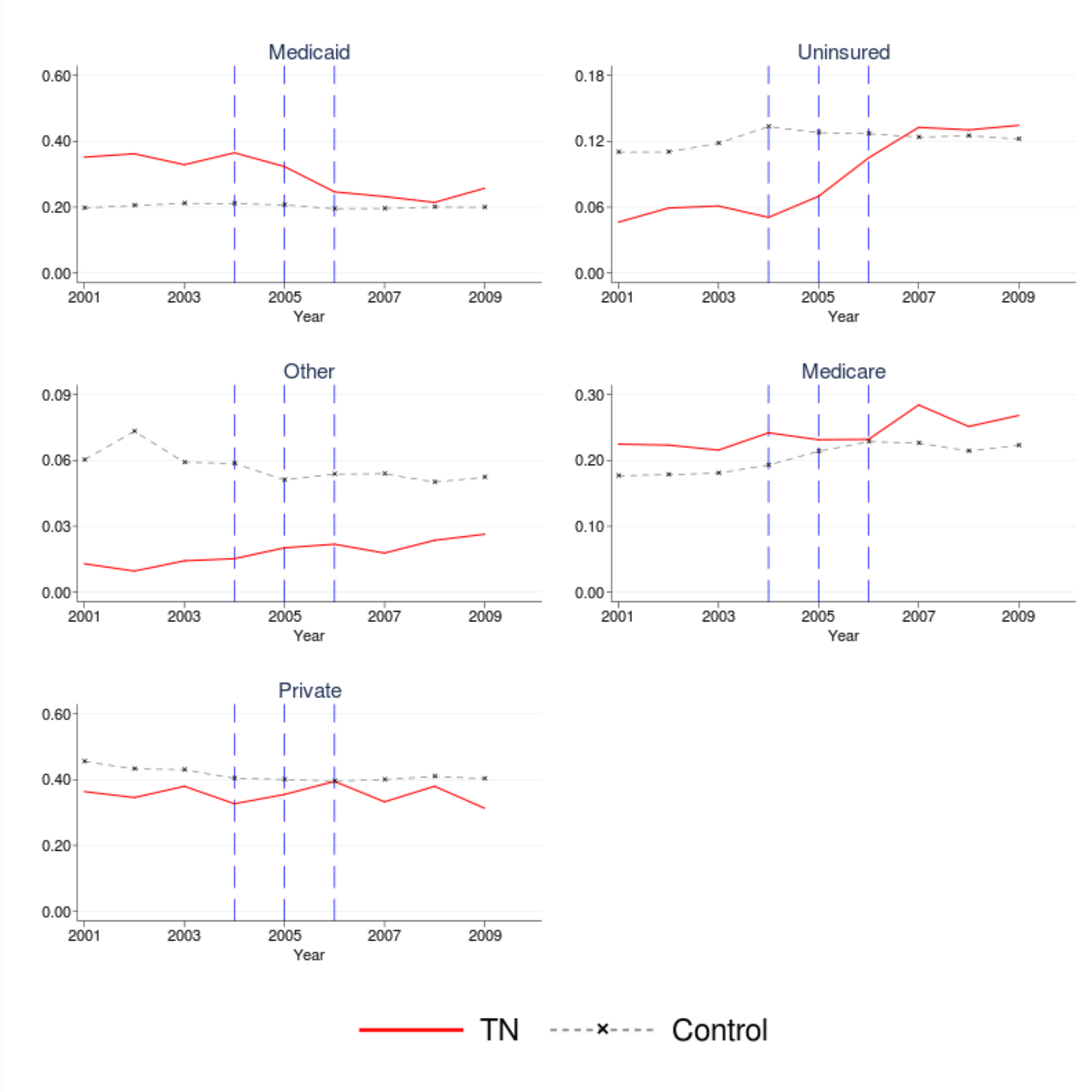
Notes: Sample estimates obtained from NIS 2001-2009 for full sample of adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and the Southern states as comparison group. We exclude year 2005 from sample. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. *Significant at 0.10, **significant at 0.05, ***significant at 0.01

Table 3-4: **Effect on Intensity of Treatment (DD Estimates using Southern States as Comparison Group)**

	(1)	(2)	(3)	(4)	(5)	(6)
	Intensity (All Admissions)			Intensity (Uninsured Admissions Only)		
	Number of Procedures	Length of Stay (LOS)	Log of LOS	Number of Procedures	Length of Stay (LOS)	Log of LOS
PostxTreat	-0.034	-0.036	0.002	-0.009	0.100**	0.019***
	(0.029)	(0.033)	(0.004)	(0.013)	(0.033)	(0.003)
Dependent Variable Means						
Treatment, Before	0.944	3.999	1.385	0.870	3.380	1.260
Control, Before	1.073	4.932	1.449	0.897	3.841	1.309
Treatment, After	1.006	4.123	1.404	0.892	3.449	1.285
Control, After	1.154	5.813	1.518	0.917	3.906	1.305
Observations	11,481	11,481	11,481	9,930	9,930	9,930

Notes: Sample estimates obtained from NIS 2001-2009 for adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and the Southern states as comparison group. We exclude year 2005 from sample. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. *Significant at 0.10, **significant at 0.05, ***significant at 0.01.

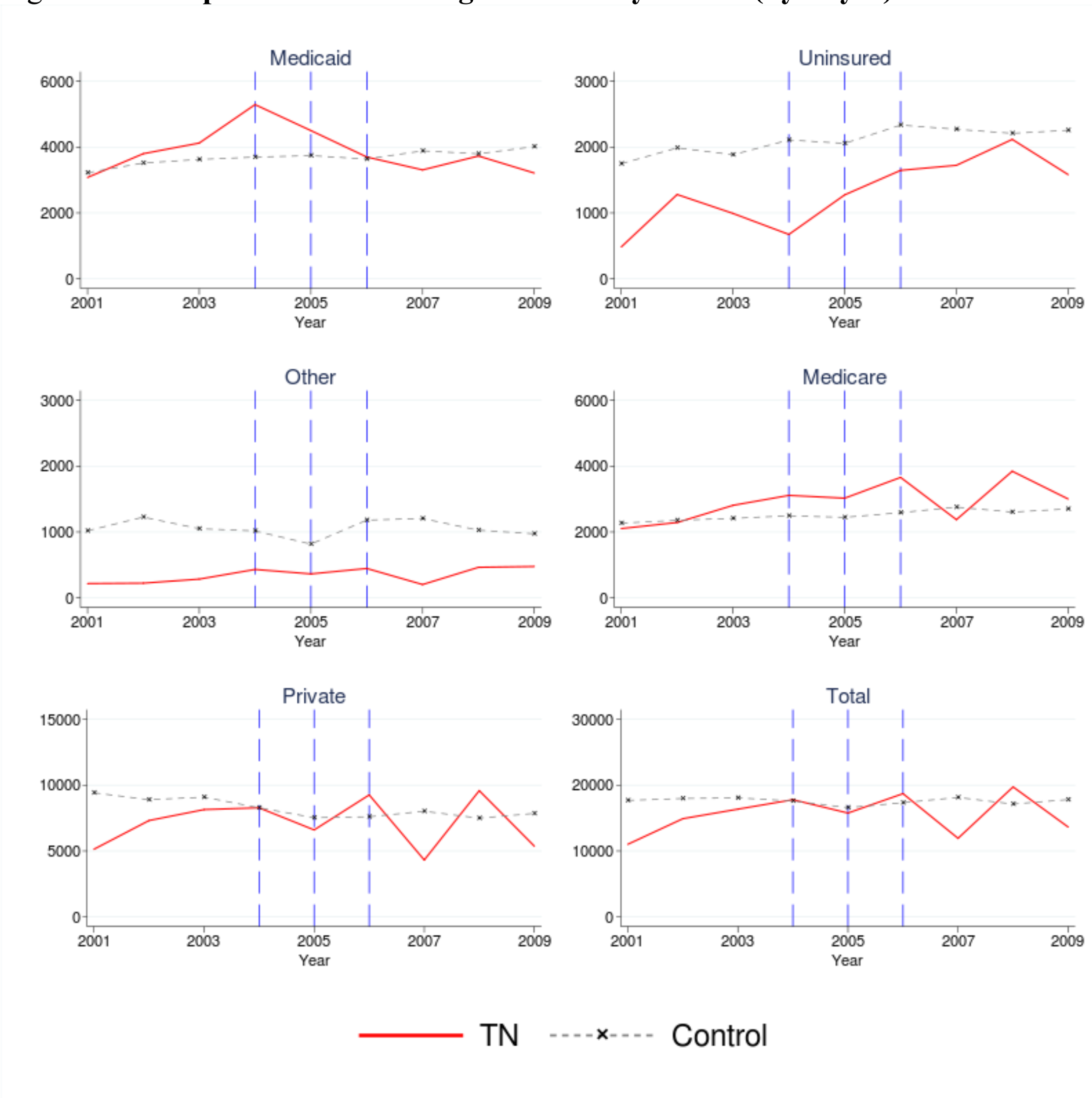
Figure 3-1: **Insurance Status of Inpatient Admissions among Non-Elderly Adults**



Notes:

1. Sample estimates from the NIS 2001-2009.
2. The first vertical line denotes the announcement of TennCare disenrollment in 2004, the second vertical line denotes its implementation in 2005, and the third vertical line represents the beginning of the post-period in our analysis.
3. Sample includes non-pregnancy-related inpatient admissions among 20-64-year-olds.
4. The solid red line and the dashed line indicates share of admissions per hospital in Tennessee and the control states, respectively.
5. Control states consist of the Southern states in the NIS except Tennessee. See section 4.5.1 for details.

Figure 3-2: **Hospitalizations Among Non-Elderly Adults (by Payer)**



Notes:

1. Sample estimates from the NIS 2001-2009.
2. The first vertical line denotes the announcement of TennCare disenrollment in 2004, the second vertical line denotes its implementation in 2005, and the third vertical line represents the beginning of the post-period in our analysis.
3. Sample includes non-pregnancy-related inpatient admissions among 20-64-year-olds.
4. The solid red line and the dashed line indicates number of admissions per hospital in Tennessee and the control states, respectively.
5. Control states consist of the Southern states in the NIS except Tennessee. See section 4.5.1 for details.

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4 Appendices

Appendix A

Table A-1: Categorization of state Medicaid expansion status

2014 Expansion States without substantial prior Medicaid expansion	Expansion States with Substantial prior Medicaid expansion	Non-Expansion States (Control group)
Alaska[1]	Delaware[11]	Alabama
Arkansas[2]	District of Columbia[7]	Florida
Arizona[3]	Massachusetts[12]	Georgia
California[4,7]	New York[13]	Idaho
Colorado[5]	Vermont[14]	Kansas
Connecticut		Maine
Hawaii[9]		Mississippi
Illinois		Missouri
Indiana[1]		Nebraska
Iowa[6]		North Carolina
Kentucky		Oklahoma
Louisiana[1]		South Carolina
Maryland		South Dakota
Michigan[1]		Tennessee
Minnesota[7]		Texas
Montana[1]		Utah
Nevada		Virginia
New Hampshire[1]		Wisconsin[10]
New Jersey[7]		Wyoming
New Mexico		
North Dakota		
Ohio		
Oregon[8]		
Pennsylvania[1]		
Rhode Island		
Washington[7]		
West Virginia		

Notes: This table shows the state classification for Medicaid eligibility used in this paper. These are mutually exclusive lists of states. Source: Reproduced from Table A1 of Ghosh et al. (2017).

1 The Medicaid expansion became effective in January 2014 for all expansion states as of this writing, except for the following: Alaska (September 2015), Indiana (February 2015), Louisiana (July 2016), Michigan (April 2014), Montana (January 2016), New Hampshire (August 2014), and Pennsylvania (January 2015). Since this table records only expansion status as of January 2014, some of the states that later expanded Medicaid appear in the control group column. The remaining notes to this Table explain the categorization of expansion states into columns 1 or 2.

2 Arkansas operated a limited-benefit premium-assistance program for childless adults who worked for small, uninsured employers (ARHealthNetworks waiver) (Kaiser Family Foundation, 2016) prior to the ACA.

3 Since 2000, Arizona offered Medicaid-equivalent benefits to childless adults with income below 100 percent FPL through a Section 1115 waiver program. However, the state closed the program

to new enrollees in July 2011 (Kaiser Family Foundation, 2016) and consequently experienced a significant expansion for childless adults in 2014.

4 Although California expanded Medicaid for childless adults to some degree as part of the state's 1115 "Bridge to Reform" waiver, this was not available in all counties and was not full Medi-Cal benefits (http://kff.org/health-reform/fact-sheet/the-california-health-care-landscape/).

5 Colorado had only very limited eligibility before 2014. Adults with income up to 10 percent FPL were eligible for Medicaid as of May 2012, and enrollment was capped to 10,000 adults (Kaiser Family Foundation, 2016).

6 Under the IowaCare program, childless adults with income below 200 percent FPL were eligible for public health insurance since 2005. However, IowaCare provided limited services in a limited network, and so low-income adults in Iowa effectively underwent substantial expansion in coverage in 2014 (Damiano et al., 2013).

7 California, Connecticut, District of Columbia, Minnesota, New Jersey, and Washington elected to enact the ACA Medicaid expansion in 2010 to 2011. However, New Jersey's early expansion only extended to 23 percent FPL while the other five states extended at least until 50 percent FPL (Sommers et al., 2013). Also, Washington's early expansion was limited to prior state plan enrollees (Sommers et al., 2013). Hence, we treat New Jersey and Washington as full 2014 expansion states.

8 In 2008, Oregon enacted a small Medicaid expansion for low-income adults through lottery drawings from a waitlist. However, less than one-third of the 90,000 people on the waitlist were selected to apply for Medicaid in 2008 (Baicker et al., 2013) and so the 2014 expansion represented a significant increase in eligibility for low-income adults.

9. In Hawaii, childless adults with incomes up to 100 percent FPL were eligible for the state's QUEST Medicaid managed care waiver program (Kaiser Family Foundation, 2016).

10 Although Wisconsin was not an ACA expansion state, the state received federal approval to offer Medicaid to childless adults below 100 percent FPL through the BadgerCare program as of 2009 (Gates and Rudowitz, 2014).

11 In Delaware, childless adults with incomes up to 100 percent FPL were eligible for Medicaid benefits through the Diamond State Health Plan waiver (Kaiser Family Foundation, 2016).

12 Massachusetts implemented reforms to expand insurance coverage to low-income adults in 2006 (Kaiser Family Foundation, 2016).

13 In New York, childless adults up to 78 percent FPL were eligible for the Medicaid (Home Relief) waiver program and childless adults up to 100 percent FPL were eligible for the Family Health Plus waiver program (Heberlein et al., 2011).

14 In Vermont, childless adults up to 150 percent FPL were eligible for Medicaid-equivalent coverage through the Vermont Health Access Plan waiver program (Heberlein et al., 2011).

Table A-2: Categorization of state family planning waivers/SPA adopted prior to 2014 ACA Medicaid expansions

	Base	Light	Moderate	Heavy
	Alabama	Idaho	California	Alaska
	Florida	Kansas	Connecticut	Arizona
	Georgia	Nebraska	Indiana	Arkansas
	Idaho	South Dakota	Iowa	Colorado
	Maine	Tennessee	Louisiana	Delaware
	Mississippi	Utah	Maryland	District of Columbia
	Missouri[1]		Minnesota	Hawaii
	North Carolina		Montana	Illinois
	Oklahoma		New Hampshire	Kentucky
	South Carolina		New Mexico	Massachusetts
	Texas[1]		New York	Michigan
	Virginia		Oregon	Nevada
	Wisconsin		Pennsylvania	New Jersey
	Wyoming		Rhode Island	North Dakota
			Washington	Ohio
			Vermont	West Virginia
Medicaid Family Planning Waiver / SPA Status	Yes	No	Yes	No
ACA Expansion Status	No	No	Yes	Yes

Notes:
This table shows classification of states based on Medicaid expansion status under the ACA and pre-ACA Medicaid coverage of family planning services through family planning waivers or state plan amendments (SPA) used in this paper. These are mutually exclusive lists of states.

1 Both Texas and Missouri provide family planning services to women older than 18 years of age and with incomes up to 185% FPL, through programs that are entirely state-funded.

Sources: Classification of states based on Guttmacher Institute, the Kaiser Family Foundation, and Appendix Table 1.

Table A-3: Event study estimates of the effect of Medicaid expansion on fertility among women without a high school diploma

Dependent variable: Ln (Births)	(1)	(2)	(3)	(4)
	Total births	First births	High parity	First births, Single
Expansion x y2011q3	0.032**	0.040*	0.031*	0.042*
	(0.014)	(0.021)	(0.017)	(0.022)
Expansion x y2011q4	0.024	0.027	0.022	0.022
	(0.023)	(0.024)	(0.026)	(0.023)
Expansion x y2012q1	-0.006	0.011	-0.012	0.001
	(0.019)	(0.027)	(0.018)	(0.026)
Expansion x y2012q2	-0.013	0.043*	-0.031**	0.037
	(0.015)	(0.023)	(0.015)	(0.023)
Expansion x y2012q3	-0.007	0.001	-0.010	-0.004
	(0.015)	(0.024)	(0.015)	(0.026)
Expansion x y2012q4	-0.021	-0.004	-0.026	-0.008
	(0.023)	(0.027)	(0.023)	(0.028)
Expansion x y2013q1	-0.040	-0.016	-0.048	-0.029
	(0.033)	(0.039)	(0.032)	(0.040)
Expansion x y2013q2	-0.022	-0.016	-0.025	-0.015
	(0.017)	(0.027)	(0.018)	(0.029)
Expansion x y2013q3	0.005	0.023	-0.001	0.014
	(0.015)	(0.022)	(0.017)	(0.023)
Expansion x y2013q4	-0.003	-0.024	0.001	-0.023
	(0.025)	(0.038)	(0.022)	(0.037)
Expansion x y2014q1	-0.049*	-0.059	-0.047*	-0.066
	(0.028)	(0.047)	(0.024)	(0.045)
Expansion x y2014q2	-0.038	-0.041	-0.035*	-0.047
	(0.023)	(0.043)	(0.020)	(0.043)
Expansion x y2014q3	-0.002	-0.026	0.007	-0.019
	(0.022)	(0.038)	(0.020)	(0.036)
Expansion x y2014q4	-0.042*	-0.092*	-0.025	-0.080
	(0.025)	(0.047)	(0.021)	(0.049)
Expansion x y2015q1	-0.049*	-0.058	-0.046*	-0.040
	(0.028)	(0.041)	(0.025)	(0.043)
Observations	624	624	624	624

Notes: Analysis is based on NCHS 2012-2015 and the estimates in each column are obtained from a separate event study analysis, with natural log of total births as the dependent variable. Observations are at the state/year/quarter/cell level. Sample includes births to women aged 18-44. Covariates include cell means of mother's age, race/ethnicity, marital status, state fixed effects, fixed effects for each quarter in the data, and state unemployment rate and poverty rate. Standard errors are clustered at state and presented in parentheses. Estimates are weighted using state by year population of women aged 18-44. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

Table A-4: Event study estimates of the effect of Medicaid expansion on fertility among women without a college degree

Dependent variable: Ln (Births)				
	(1)	(2)	(3)	(4)
	Total births	First births	High parity	First births, Single
Expansion x y2011q3	0.010	0.005	0.014	0.010
	(0.014)	(0.015)	(0.014)	(0.016)
Expansion x y2011q4	-0.003	-0.008	-0.000	-0.012
	(0.009)	(0.014)	(0.009)	(0.012)
Expansion x y2012q1	-0.002	-0.010	0.002	-0.007
	(0.008)	(0.009)	(0.009)	(0.010)
Expansion x y2012q2	-0.003	-0.020*	0.006	-0.010
	(0.007)	(0.011)	(0.009)	(0.011)
Expansion x y2012q3	-0.013	-0.022	-0.007	-0.026
	(0.012)	(0.013)	(0.013)	(0.018)
Expansion x y2012q4	-0.007	-0.016	-0.001	-0.022**
	(0.008)	(0.010)	(0.009)	(0.010)
Expansion x y2013q1	-0.018	-0.027*	-0.013	-0.028*
	(0.013)	(0.015)	(0.014)	(0.016)
Expansion x y2013q2	-0.021***	-0.028***	-0.017**	-0.029**
	(0.006)	(0.010)	(0.007)	(0.012)
Expansion x y2013q3	-0.017*	-0.020	-0.015	-0.016
	(0.010)	(0.012)	(0.011)	(0.012)
Expansion x y2013q4	-0.007	-0.011	-0.004	-0.021
	(0.010)	(0.014)	(0.008)	(0.014)
Expansion x y2014q1	-0.017	-0.018	-0.017	-0.030
	(0.015)	(0.018)	(0.015)	(0.018)
Expansion x y2014q2	-0.029***	-0.018	-0.035***	-0.020
	(0.010)	(0.014)	(0.010)	(0.016)
Expansion x y2014q3	-0.009	-0.006	-0.010	-0.003
	(0.012)	(0.012)	(0.013)	(0.017)
Expansion x y2014q4	-0.025**	-0.019	-0.027***	-0.022
	(0.010)	(0.015)	(0.008)	(0.020)
Expansion x y2015q1	-0.038**	-0.045**	-0.034**	-0.051**
	(0.017)	(0.019)	(0.016)	(0.020)
Observations	624	624	624	624

Notes: Analysis is based on NCHS 2012-2015 and the estimates in each column are obtained from a separate event study analysis, with natural log of total births as the dependent variable. Observations are at the state/year/quarter/cell level. Sample includes births to women aged 18-44. Covariates include cell means of mother's age, race/ethnicity, marital status, state fixed effects, fixed effects for each quarter in the data, and state unemployment rate and poverty rate. Standard errors are clustered at state and presented in parentheses. Estimates are weighted using state by year population of women aged 18-44. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

Table A-5: Event study estimates for Medicaid contraceptive use

Dependent variable: Ln (Medicaid prescriptions per 100 population)	
Expansion x 2012q2	0.03
	(0.07)
Expansion x 2012q3	0.00
	(0.07)
Expansion x 2012q4	-0.03
	(0.07)
Expansion x 2013q1	0.11
	(0.07)
Expansion x 2013q2	0.16
	(0.11)
Expansion x 2013q3	0.05
	(0.07)
Expansion x 2013q4	0.03
	(0.07)
Expansion x 2014q1	0.08
	(0.10)
Expansion x 2014q2	0.20
	(0.13)
Expansion x 2014q3	0.32***
	(0.10)
Expansion x 2014q4	0.33***
	(0.12)
Expansion x 2015q1	0.32**
	(0.12)
Expansion x 2015q2	0.33***
	(0.11)
Expansion x 2015q3	0.34***
	(0.11)
Expansion x 2015q4	0.36***
	(0.12)
Expansion x 2016q1	0.29**
	(0.13)
Expansion x 2016q2	0.35***
	(0.12)
Expansion x 2016q3	0.28**
	(0.13)
Expansion x 2016q4	0.27**
	(0.13)
Observations	960

Note: Analysis is based on aggregated state-quarter Medicaid prescription data and the estimates in each column are obtained from a separate event study analysis, with natural log of total Medicaid contraceptives per 100 population as the dependent variable. Observations are at the at state/year/quarter level. Data covers the period 2012Q1 to 2016Q4. All models include state fixed effects, fixed effects for each quarter in the data, poverty rate and unemployment rate. Robust standard errors clustered by state are reported in parentheses. * Significant at the 10-percent level. ** Significant at the 5-percent level. *** Significant at the 1-percent level.

Table A-6: Summary statistics

	Expansion		Non-expansion		
	Before	After	Before	After	Data Source
Mother's age	28.90	29.14	27.85	28.13	NCHS
Married	0.62	0.62	0.59	0.59	NCHS
Less than high school	0.14	0.13	0.15	0.14	NCHS
Less than college	0.51	0.52	0.57	0.57	NCHS
Master's or more	0.15	0.15	0.10	0.10	NCHS
Female	0.49	0.49	0.49	0.49	NCHS
Live birth order 1-8	2.09	2.11	2.15	2.17	NCHS
White	0.51	0.52	0.58	0.55	NCHS
Black	0.11	0.11	0.17	0.17	NCHS
Hispanic	0.27	0.26	0.20	0.22	NCHS
Other	0.11	0.12	0.05	0.06	NCHS
Unemployment rate	8.63	6.52	7.66	5.73	BLS LAUS
Poverty rate	14.52	14.54	15.64	15.66	SAIPE
Total births	19,173.49	18,726.93	19,333.45	18,992.45	NCHS
Total Medicaid contraceptives	61,983.65	65,708.50	39,338.94	35,296.47	SDUD

Notes: This table displays summary statistics separately for 2014 Medicaid expansion and non-expansion states.

Source: NCHS 2012-2015; BLS 2012-2015; SAIPE 2012-2015; SDUD 2012-2016.

Appendix B

Table B-1: Categorization of State Expansion Status

2014 Expansion States without substantial prior Medicaid expansion	Expansion States with Substantial prior Medicaid expansion	Non-Expansion States (Control group)
Arkansas[1,2]	Delaware[11]	Alabama
Arizona[3]	District of Columbia[7]	Alaska
California[4,7]	Massachusetts[12]	Florida
Colorado[5]	New York[13]	Georgia
Connecticut	Vermont[14]	Idaho
Hawaii[9]		Kansas
Illinois		Louisiana[1]
Indiana[1]		Maine
Iowa[6]		Mississippi
Kentucky		Missouri
Maryland		Montana[1]
Michigan[1]		Nebraska
Minnesota[7]		North Carolina
Nevada		Oklahoma
New Hampshire[1]		South Carolina
New Jersey[7]		South Dakota
New Mexico		Tennessee
North Dakota		Texas
Ohio		Utah
Oregon[8]		Virginia
Pennsylvania[1]		Wisconsin[10]
Rhode Island		Wyoming
Washington[7]		
West Virginia		

Notes: This table shows the state classification for Medicaid eligibility used in this paper. These are mutually exclusive lists of states. We first examine states in the first two columns together, as expansion states. Later specifications separate expansion states into those with and without substantial pre-2014 Medicaid expansions. Source: Reproduced from Table A1 of (Ghosh et al., 2017).

1 The Medicaid expansion became effective in January 2014 for all expansion states as of this writing, except for the following: Alaska (September 2015), Indiana (February 2015), Louisiana (July 2016), Michigan (April 2014), Montana (January 2016), New Hampshire (August 2014), and Pennsylvania (January 2015). Since this table records only expansion status as of January 2014, some of the states that later expanded Medicaid appear in the control group column. However, our regressions categorize those states that expanded after January 2014 but before March 2015 as expansion states only in the quarters after the expansion was implemented. The remaining notes to this Table explain the categorization of expansion states into columns 1 or 2.

2 Arkansas operated a limited-benefit premium-assistance program for childless adults who worked for small, uninsured employers (ARHealthNetworks waiver) (Kaiser Family Foundation, 2016) prior to the ACA.
3 Since 2000, Arizona offered Medicaid-equivalent benefits to childless adults with income below 100 percent FPL through a Section 1115 waiver program. However, the state closed the program to new enrollees in July 2011 (Kaiser Family Foundation, 2016) and consequently experienced a significant expansion for childless adults in 2014.
4 Although California expanded Medicaid for childless adults to some degree as part of the state's 1115 "Bridge to Reform" waiver, this was not available in all counties and was not full Medi-Cal benefits (http://kff.org/health-reform/fact-sheet/the-california-health-care-landscape/).
5 Colorado had only very limited eligibility before 2014. Adults with income up to 10 percent FPL were eligible for Medicaid as of May 2012, and enrollment was capped to 10,000 adults (Kaiser Family Foundation, 2016).
6 Under the IowaCare program, childless adults with income below 200 percent FPL were eligible for public health insurance since 2005. However, IowaCare provided limited services in a limited network, and so low-income adults in Iowa effectively underwent substantial expansion in coverage in 2014 (Damiano et al., 2013).
7 California, Connecticut, District of Columbia, Minnesota, New Jersey, and Washington elected to enact the ACA Medicaid expansion in 2010 to 2011. However, New Jersey's early expansion only extended to 23 percent FPL while the other five states extended at least until 50 percent FPL (Sommers et al., 2013). Also, Washington's early expansion was limited to prior state plan enrollees (Sommers et al., 2013). Hence, we treat New Jersey and Washington as full 2014 expansion states.
8 In 2008, Oregon enacted a small Medicaid expansion for low-income adults through lottery drawings from a waitlist. However, less than one-third of the 90,000 people on the waitlist were selected to apply for Medicaid in 2008 (Baicker et al., 2013) and so the 2014 expansion represented a significant increase in eligibility for low-income adults.
9 In Hawaii, childless adults with incomes up to 100 percent FPL were eligible for the state's QUEST Medicaid managed care waiver program (Kaiser Family Foundation, 2016).
10 Although Wisconsin was not an ACA expansion state, the state received federal approval to offer Medicaid to childless adults below 100 percent FPL through the BadgerCare program as of 2009 (Gates and Rudowitz, 2014).
11 In Delaware, childless adults with incomes up to 100 percent FPL were eligible for Medicaid benefits through the Diamond State Health Plan waiver (Kaiser Family Foundation, 2016).
12 Massachusetts implemented reforms to expand insurance coverage to low-income adults in 2006 (Kaiser Family Foundation, 2016).
13 In New York, childless adults up to 78 percent FPL were eligible for the Medicaid (Home Relief) waiver program and childless adults up to 100 percent FPL were eligible for the Family Health Plus waiver program (Heberlein et al., 2011).
14 In Vermont, childless adults up to 150 percent FPL were eligible for Medicaid-equivalent coverage through the Vermont Health Access Plan waiver program (Heberlein et al., 2011).

Table B-2: Event Study Estimates by Payer

Dependent variable: Ln (prescriptions per 100 population)			
	(1)	(2)	(3)
	Uninsured (Cash and assistance programs)	Commercial	Medicare
Expansion x 2013Q2	0.03*	-0.01	0.01*
	(0.02)	(0.01)	(0.005)
Expansion x 2013Q3	0.03	-0.004	0.01*
	(0.03)	(0.01)	(0.01)
Expansion x 2013Q4	0.03	-0.005	0.01
	(0.03)	(0.02)	(0.01)
Expansion x 2014Q1	0.01	-0.01	0.001
	(0.03)	(0.02)	(0.01)
Expansion x 2014Q2	0.003	-0.01	0.003
	(0.03)	(0.02)	(0.01)
Expansion x 2014Q3	0.005	-0.02	0.01
	(0.03)	(0.02)	(0.02)
Expansion x 2014Q4	0.002	-0.02	0.01
	(0.03)	(0.02)	(0.02)
Expansion x 2015Q1	-0.01	-0.03	0.004
	(0.03)	(0.02)	(0.02)
Observations	459	459	459

Notes: Analysis is based on aggregated state-quarter Medicaid prescription data from 2013Q1 to 2015Q1. All models include state fixed effects, fixed effects for each quarter in the data, and unemployment rate. Robust standard errors clustered by state reported in parentheses. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

Table B-3: Event Study Estimates of Prescription Drug Utilization Based on 2013 CBSA Uninsurance Rates

Dependent variable: Log (per capita Medicaid prescriptions)		
	(1)	(2)
	All States	Excl. DC, DE, MA, NY, VT
Pct Uninsured 2013 x Expansion x 2013Q2	-0.0002	0.001
	(0.001)	(0.002)
Pct Uninsured 2013 x Expansion x 2013Q3	-0.001	0.001
	(0.002)	(0.003)
Pct Uninsured 2013 x Expansion x 2013Q4	-0.003	0.000
	(0.003)	(0.003)
Pct Uninsured 2013 x Expansion x 2014Q1	0.002	0.005
	(0.003)	(0.003)
Pct Uninsured 2013 x Expansion x 2014Q2	0.003	0.006*
	(0.003)	(0.003)
Pct Uninsured 2013 x Expansion x 2014Q3	0.006**	0.008***
	(0.003)	(0.003)
Pct Uninsured 2013 x Expansion x 2014Q4	0.004	0.006*
	(0.003)	(0.003)
Pct Uninsured 2013 x Expansion x 2015Q1	0.003	0.004
	(0.003)	(0.003)
Observations	7,029	6,714

Notes:

1. Analysis is based on aggregated CBSA-quarter Medicaid prescription data from 2013Q1 to 2015Q1. All models include CBSA fixed effects, year by quarter fixed effects, and CBSA unemployment rate. Robust standard errors clustered by CBSA reported in parentheses. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.
2. In column (1), estimates are based on all states being categorized into expansion vs non-expansion states.
3. For the analysis corresponding to column (2), the early expansion states of DC, DE, MA, NY, and VT were dropped from the sample.

Table B-4: Event Study Estimates of Prescription Drug Utilization Based on 2013 CBSA Poverty Proportions

Dependent variable: Log (per capita Medicaid prescriptions)		
	(1)	(2)
	All States	Excl. DC, DE, MA, NY, VT
Pct Poverty 2013 x Expansion x 2013Q2	0.002	0.002
	(0.002)	(0.002)
Pct Poverty 2013 x Expansion x 2013Q3	0.002	0.003
	(0.003)	(0.003)
Pct Poverty 2013 x Expansion x 2013Q4	0.003	0.004
	(0.004)	(0.004)
Pct Poverty 2013 x Expansion x 2014Q1	0.005	0.005
	(0.004)	(0.004)
Pct Poverty 2013 x Expansion x 2014Q2	0.005	0.005
	(0.004)	(0.004)
Pct Poverty 2013 x Expansion x 2014Q3	0.004	0.004
	(0.005)	(0.005)
Pct Poverty 2013 x Expansion x 2014Q4	0.004	0.004
	(0.005)	(0.005)
Pct Poverty 2013 x Expansion x 2015Q1	0.004	0.004
	(0.005)	(0.005)
Observations	7,029	6,714

Notes:

1. Analysis is based on aggregated CBSA-quarter Medicaid prescription data from 2013Q1 to 2015Q1. All models include CBSA fixed effects, fixed effects for each quarter in the data, and unemployment rate. Robust standard errors clustered by CBSA reported in parentheses. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.
2. In column (1), estimates are based on all states being categorized into expansion vs non-expansion states.
3. For the analysis corresponding to column (2), the early expansion states of DC, DE, MA, NY, and VT were dropped from the sample.

Table B-5: Event Study Estimates of Prescription Drug Utilization Based on 2013 CBSA Minority Proportions

Dependent variable: Log (per capita Medicaid prescriptions)		
	(1)	(2)
	All States	Excl. DC, DE, MA, NY, VT
Pct Minority 2013 x Expansion x 2013Q2	-0.0004	-0.0003
	(0.001)	(0.001)
Pct Minority 2013 x Expansion x 2013Q3	-0.001	-0.001
	(0.001)	(0.001)
Pct Minority 2013 x Expansion x 2013Q4	-0.002	-0.002
	(0.001)	(0.001)
Pct Minority 2013 x Expansion x 2014Q1	0.002	0.002
	(0.002)	(0.002)
Pct Minority 2013 x Expansion x 2014Q2	0.002	0.002
	(0.002)	(0.002)
Pct Minority 2013 x Expansion x 2014Q3	0.002	0.003
	(0.002)	(0.002)
Pct Minority 2013x Expansion x 2014Q4	0.001	0.002
	(0.002)	(0.002)
Pct Minority 2013 x Expansion x 2015Q1	0.002	0.003
	(0.002)	(0.002)
Observations	7,029	6,714

Notes:

1. Analysis is based on aggregated CBSA-quarter Medicaid prescription data from 2013Q1 to 2015Q1. All models include CBSA fixed effects, fixed effects for each quarter in the data, and unemployment rate. Robust standard errors clustered by CBSA reported in parentheses. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.
2. In column (1), estimates are based on all states being categorized into expansion vs non-expansion states.
3. For the analysis corresponding to column (2), the early expansion states of DC, DE, MA, NY, and VT were dropped from the sample.

Table B-6: Effect of the ACA Medicaid Expansions on Medicaid Prescription Drugs, State Specification, Restricted to data from CBSAs

Dependent variable: Log (per capita Medicaid prescriptions)		
	(1)	(2)
	All States	Excl. DC, DE, MA, NY, VT
Post x Expansion	0.22***	0.23***
	(0.03)	(0.03)
Year and quarter fixed effects	Y	Y
CBSA fixed effects	Y	Y
Observations	7,029	6,714
Dependent variable means		
Expansion, Before	4.26	4.23
Non-expansion, Before	4.68	4.68
Expansion, After	4.59	4.57
Non-expansion, After	4.79	4.79

Notes:

1. Analysis is based on aggregated CBSA-quarter Medicaid prescription data from 2013Q1 to 2015Q1. All models include unemployment rate. Robust standard errors clustered by state reported in parentheses. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.
2. In column (1), estimates are based on all states being categorized into expansion vs non-expansion states.
    a. Expansion states: AR, AZ, CA, CO, CT, DE, DC, HI, IL, IN, IA, IL, KY, MD, MA, MI, MN, NV, NH, NJ, NM, NY, ND, OH, OR, PA, RI, VT, WA, WV.
    b. Non-expansion states: AL, AK, FL, GA, ID, KS, LA, ME, MI, MO, MT, NE, NC, OK, SC, SD, TN, TX, UT, VA, WI, WY.
3. For the analysis corresponding to column (2), the early expansion states of DC, DE, MA, NY, and VT were dropped from the sample.

Appendix C

Table C-1: Pre-Treatment Trend Test (Southern States as Comparison Group)

	(1)	(2)	(3)	(4)	(5)
	A. Dependent Variable: Fraction of Admissions				
	Medicaid	Private	Other	Medicare	Uninsured
TrendxTreat	0.000	0.000	0.001	0.001	-0.002
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)
Observations	5,250	5,250	5,250	5,250	5,250

	(1)	(2)	(3)	(4)	(5)
	B. Dependent Variable: Hospitalization Rate				
	Medicaid	Private	Other	Medicare	Uninsured
TrendxTreat	-0.347	3.236	1.097*	0.329	0.620***
	(0.454)	(2.191)	(0.499)	(0.474)	(0.175)
Observations	5,252	5,252	5,252	5,252	5,252

Notes: Sample estimates obtained from NIS 2001-2004 for adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and the Southern states as comparison group. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. * Significant at 0.10, ** significant at 0.05, *** significant at 0.01.

Table C-2: Alternative Difference-in-Differences Model Specifications

Dependent Variable: Fraction of Inpatient Admissions

		(1)	(2)	(3)	(4)	(5)	(6)
		Dependent Variable: Fraction of Inpatient Admissions					
	Interaction Term	Medicaid	Private	Other	Medicare	Uninsured	N
A. Difference-in-difference estimates using all states as control	*PostxTreat*	-0.071***	0.012***	0.004*	0.011***	0.044***	29,867
		(0.003)	(0.004)	(0.002)	(0.002)	(0.002)	
B. Difference-in-difference estimates using synthetic control states (level)	*PostxTreat*	-0.046***	0.001	0.007	0.014**	0.025***	6,734
		(0.013)	(0.009)	(0.004)	(0.006)	(0.007)	
C. Difference-in-difference estimates using synthetic control states (growth rate)	*PostxTreat*	-0.051***	0.001	-0.002	0.016**	0.036***	4,687
		(0.018)	(0.009)	(0.004)	(0.008)	(0.010)	
D. Difference-in-difference using Southern states as control, includes state-specific linear time trends	*PostxTreat*	-0.049***	-0.005	0.007**	0.015***	0.031***	11,048
		(0.012)	(0.004)	(0.003)	(0.004)	(0.007)	
E. Difference-in-difference using Southern states as control, includes state-specific squared time trends	*PostxTreat*	-0.047***	-0.004	0.004	0.015***	0.032***	11,048
		(0.013)	(0.004)	(0.003)	(0.004)	(0.009)	
F. Difference-in-difference using Southern states as control, includes state-specific cubic time trends	*PostxTreat*	-0.050***	0.000	0.003	0.014***	0.034***	11,048
		(0.012)	(0.005)	(0.003)	(0.003)	(0.009)	

Notes: Sample estimates obtained from NIS 2001-2009 for adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and exclude year 2005 from sample. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. *Significant at 0.10, **significant at 0.05, ***significant at 0.01.

Table C-3: Alternative Triple-Differences Model Specifications

Dependent Variable: Fraction of Inpatient Admissions

		(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Fraction of Inpatient Admissions						
	Interaction Term	Medicaid	Private	Other	Medicare	Uninsured	N
A. Triple-difference estimates using Southern states	*PostxTreatxOver20*	-0.075***	0.009*	0.005	0.012***	0.049***	21,903
		(0.007)	(0.005)	(0.004)	(0.003)	(0.004)	
B. Triple-difference estimates using Southern states	*PostxTreatxNon-birth*	-0.073***	0.010*	0.004	0.014***	0.045***	19,068
		(0.005)	(0.005)	(0.004)	(0.002)	(0.004)	
C. Triple-difference estimates using all states	*PostxTreatxOver20*	-0.084***	0.020***	0.005**	0.008***	0.050***	57,779
		(0.005)	(0.004)	(0.002)	(0.003)	(0.002)	
D. Triple-difference estimates using all states	*PostxTreatxNon-birth*	-0.080***	0.020***	0.004**	0.009***	0.046***	52,253
		(0.004)	(0.004)	(0.002)	(0.002)	(0.002)	

Notes: Sample estimates obtained from NIS 2001-2009. Each coefficient estimate represents a separate regression. We exclude year 2005 from sample. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. * Significant at 0.10, ** significant at 0.05, *** significant at 0.01.

Table C-4: Alternative Difference-in-Differences Model Specifications
Dependent Variable: Hospitalization Rate

		(1)	(2)	(3)	(4)	(5)	(6)
		Dependent Variable: Hospitalization Rate					
	Interaction Term	Medicaid	Private	Other	Medicare	Uninsured	N
A. Difference-in-difference estimates using all states as control	*PostxTreat*	-17.677***	-8.055***	-0.022	0.612	11.470***	29,881
		(1.881)	(2.175)	(0.768)	(3.722)	(0.747)	
B. Difference-in-difference estimates using synthetic control states (level)	*PostxTreat*	-19.839***	-22.733***	2.016	-14.774**	8.127***	6,736
		(4.820)	(6.156)	(1.559)	(5.701)	(2.120)	
C. Difference-in-difference estimates using synthetic control states (growth rate)	*PostxTreat*	-18.270***	-20.032***	-0.933	-8.966**	7.773***	4,689
		(5.501)	(7.338)	(0.664)	(4.246)	(2.437)	
D. Difference-in-difference using Southern states as control, includes state-specific linear time trends	*PostxTreat*	-13.332*	-8.179	-4.470	3.105	11.194***	11,050
		(6.970)	(8.051)	(3.034)	(12.111)	(3.548)	
E. Difference-in-difference using Southern states as control, includes state-specific squared time trends	*PostxTreat*	-12.326*	-5.738	-4.358	7.647	10.710**	11,050
		(5.993)	(6.656)	(3.882)	(10.522)	(3.473)	
F. Difference-in-difference using Southern states as control, includes state-specific cubic time trends	*PostxTreat*	-12.493**	-3.875	-3.096	10.803	10.196***	11,050
		(4.374)	(5.555)	(3.510)	(8.293)	(2.775)	

Notes: Sample estimates obtained from NIS 2001-2009 for adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and exclude year 2005 from sample. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. *Significant at 0.10, **significant at 0.05, ***significant at 0.01.

Table C-5: Alternative Triple-Differences Model Specifications
Dependent Variable: Hospitalization Rate

		(1)	(2)	(3)	(4)	(5)	(6)
		Dependent Variable: Hospitalization Rate					
	Interaction Term	Medicaid	Private	Other	Medicare	Uninsured	N
A. Triple-difference estimates using Southern states	*PostxTreatxOver20*	-19.895***	-5.788	0.862	-6.055	11.556***	21,910
		(2.005)	(3.736)	(2.474)	(5.768)	(1.627)	
B. Triple-difference estimates using Southern states	*PostxTreatxNon-birth*	-24.222***	-6.630*	0.524	-4.855	11.476***	19,074
		(1.775)	(3.220)	(2.602)	(5.279)	(1.718)	
C. Triple-difference estimates using all states	*PostxTreatxOver20*	-19.684***	-9.735***	0.196	2.027	12.327***	57,779
		(1.005)	(2.654)	(0.915)	(3.442)	(0.867)	
D. Triple-difference estimates using all states	*PostxTreatxNon-birth*	-22.875***	-10.286***	-0.015	2.200	12.584***	52,276
		(0.946)	(2.646)	(0.994)	(3.382)	(0.916)	

Notes: Sample estimates obtained from NIS 2001-2009. Each coefficient estimate represents a separate regression. We exclude year 2005 from sample. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. * Significant at 0.10, ** significant at 0.05, *** significant at 0.01.

Table C-6: Alternative Difference-in-Differences Model Specifications

Dependent Variable: Uninsured Hospitalization Rate

		(1)	(2)	(3)	(4)	(5)
		Dependent Variable: Uninsured Hospitalization Rate				
	Interaction Term	Uninsured ED	Uninsured non-ED	Uninsured ACSC	Uninsured non-ACSC	N
A. Difference-in-difference estimates using all states as control	*PostxTreat*	9.382***	2.127***	1.711***	9.759***	29,881
		(0.581)	(0.288)	(0.137)	(0.652)	
B. Difference-in-difference estimates using synthetic control states (level)	*PostxTreat*	7.192***	0.949***	1.033***	7.094***	6,736
		(1.913)	(0.302)	(0.370)	(1.870)	
C. Difference-in-difference estimates using synthetic control states (growth rate)	*PostxTreat*	6.620***	1.167***	0.760	7.013***	4,689
		(2.126)	(0.423)	(0.462)	(2.067)	
D. Difference-in-difference using Southern states as control, includes state-specific linear time trends	*PostxTreat*	9.221***	1.918*	1.756**	9.438***	11,050
		(2.693)	(0.964)	(0.618)	(2.944)	
E. Difference-in-difference using Southern states as control, includes state-specific squared time trends	*PostxTreat*	8.874***	1.781*	1.712**	8.998***	11,050
		(2.793)	(0.853)	(0.610)	(2.885)	
F. Difference-in-difference using Southern states as control, includes state-specific cubic time trends	*PostxTreat*	8.557***	1.596**	1.725***	8.471***	11,050
		(2.358)	(0.718)	(0.512)	(2.292)	

Notes: Sample estimates obtained from NIS 2001-2009 for adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and exclude year 2005 from sample. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. *Significant at 0.10, **significant at 0.05, ***significant at 0.01.

Table C-7: Alternative Triple-Differences Model Specifications
Dependent Variable: Uninsured Hospitalization Rate

		(1)	(2)	(3)	(4)	(5)
		Dependent Variable: Uninsured Hospitalization Rate				
	Interaction Term	Uninsured ED	Uninsured non-ED	Uninsured ACSC	Uninsured non-ACSC	N
A. Triple-difference estimates using Southern states	*PostxTreatxOver20*	8.966***	2.590***	1.888***	9.667***	21,910
		(1.456)	(0.435)	(0.194)	(1.484)	
B. Triple-difference estimates using Southern states	*PostxTreatxNon-birth*	8.903***	2.588***	1.794***	9.682***	19,074
		(1.615)	(0.351)	(0.237)	(1.514)	
C. Triple-difference estimates using all states	*PostxTreatxOver20*	10.142***	2.169***	1.736***	10.591***	57,779
		(0.692)	(0.319)	(0.136)	(0.760)	
D. Triple-difference estimates using all states	*PostxTreatxNon-birth*	10.281***	2.300***	1.649***	10.935***	52,276
		(0.769)	(0.297)	(0.151)	(0.793)	

Notes: Sample estimates obtained from NIS 2001-2009. Each coefficient estimate represents a separate regression. We exclude year 2005 from sample. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. Covariates include demographic characteristics, number of diagnoses, unemployment rate and unemployment rate interacted with treatment indicator. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. * Significant at 0.10, **significant at 0.05, *** significant at 0.01.

Table C-8: Pre-Treatment Trend Test (All States as Comparison Group)

	(1)	(2)	(3)	(4)	(5)
	A. Dependent Variable: Fraction of Admissions				
	Medicaid	Private	Other	Medicare	Uninsured
TrendxTreat	-0.000	0.001	0.000	-0.000	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Observations	14,284	14,284	14,284	14,284	14,284

	(1)	(2)	(3)	(4)	(5)
	B. Dependent Variable: Hospitalization Rate				
	Medicaid	Private	Other	Medicare	Uninsured
TrendxTreat	-0.784***	1.312	0.551**	-0.443	0.484*
	(0.252)	(1.230)	(0.255)	(0.289)	(0.273)
Observations	14,286	14,286	14,286	14,286	14,286

Notes: Sample estimates obtained from NIS 2001-2004 for full sample of adults between ages 20-64. Each coefficient estimate represents a separate regression. We use non-birth admissions only and all states as comparison group. Hospitalization rates are measured at the hospital-quarter level per 10,000 population. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. * Significant at 0.10, ** significant at 0.05, *** significant at 0.01. Massachusetts and Missouri have been excluded.

Table C-9: Effect on Total Volume of Hospitalizations (DD estimates)

	A. South States - Main specification		
	Total (DD)		Total (Pre-Trend Test)
PostxTreat	-15.585	*TrendxTreat*	4.660
	(13.172)		(3.419)
Observations	11,481		5,252
	B. All StatesA9		
	Total (DD)		Total (Pre-Trend Test)
PostxTreat	-12.704**	*TrendxTreat*	0.808
	(5.473)		(2.021)
Observations	29,881	Observations	14,286
	C. Triple-difference estimates		
	Southern states as control		All states as control
PostxTreatxOver20	-18.704** (8.245)	*PostxTreatxOver20*	-14.523*** (4.505)
Observations	21,910	Observations	57,779
PostxTreatxNon-birth	-23.122** (7.833)	*PostxTreatxNon-birth*	-18.096*** (4.885)
Observations	19,074		52,276

Notes: Sample estimates obtained from NIS 2001-2009. Each coefficient estimate represents a separate regression. Total volume of hospitalizations is measured at the hospital-quarter level per 10,000 population. All specifications are weighted using discharge weights and include hospital-fixed effects and year- and quarter-fixed effects. Standard errors are clustered at the state level. Cluster-robust standard errors are shown in parentheses. * Significant at 0.10, ** significant at 0.05, *** significant at 0.01. Massachusetts and Missouri have been excluded.

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