The Impacts of Reduced Access to Abortion and Family Planning Services on Abortion, Births, and Contraceptive Purchases *

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Abstract

Between 2011 and 2014, Texas enacted three pieces of legislation that significantly reduced funding for family planning services and increased restrictions on abortion clinic operations. Together this legislation creates cross-county variation in access to abortion and family planning services, which we leverage to understand the impact of family planning and abortion clinic access on abortions, births, and contraceptive purchases. In response to these policies, abortions to Texas residents fell 16.7% and births rose 1.3% in counties that no longer had an abortion provider within 50 miles. Changes in the family planning market induced a 1.2% increase in births for counties that no longer had a publicly funded family planning clinic within 25 miles. Meanwhile, responses of retail purchases of condoms and emergency contraceptives to both abortion and family planning service changes were minimal.

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1 Introduction

Access to abortion and family planning services has declined precipitously over the past decade. Between 2008 and 2014, the number of facilities providing abortions in the United States fell 6.8%, continuing a long decline since the early 1980s. In some states, including Texas, this drop has been even more dramatic: the number of abortion-providing clinics shrunk by at least 25% in 10 states over the 2008 to 2014 period (Jones and Jerman, 2014, 2017).[1] Coinciding with this, the abortion rate is at its lowest level since the adoption of Roe v. Wade.[2]

In parallel, the funding of family planning services, which primarily include the dispensary of contraceptives, pregnancy testing, sexually transmitted infections (STIs) testing and treatment, primary care, cancer screenings, and preconception and prenatal care, has similarly decreased (Zolna and Frost, 2016). Per capita funding levels of Title X, the federal program devoted solely to the provision of family planning services and targeted to low-income women, hit their peak in 2010 and have fallen subsequently.[3] At its apex of funding, one in four women (and nearly half of poor women) who received contraceptive services did so at a publicly funded clinic.[4] Funding cuts to family planning services, including Title X, are likely to continue given the current health care discussions. In 2017, President Trump signed legislation allowing states to withhold Title X funds from family planning clinics that are affiliated with abortion providers.[5]

In this study, we exploit three recent policy changes in Texas to separately understand the effects of reductions in access to abortion and family planning services. Over the 2011 to 2014 period, the Texas legislature implemented legislation that both limited the ability of non-abortion family planning providers to receive government funding and placed more stringent requirements on the operation of abortion clinics. In the aftermath of these policies, over half of abortion clinics closed by 2015, family planning providers experienced funding cuts of 66% and one-quarter

[1]Note, while the change in Texas is large, it is not an outlier. There are seven states with at least as large of a decline in abortion-providing clinics over this time period.

[2]See http://www.latimes.com/nation/la-na-abortion-rate-2017-story.html.

[3]See https://www.hhs.gov/opa/title-x-family-planning/about-title-x-grants/funding-history/index.html.

[4]Source: https://www.guttmacher.org/fact-sheet/publicly-funded-family-planning-services-united-states.

[5]Title X funding has never been available for abortion services.

of publicly funded family planning clinics closed (White et al., 2015). The first two pieces of legislation concerned family planning funding and the last impacted abortion clinics.

As access to these services may affect fertility decisions on multiple margins, we focus on three sets of outcomes to better understand how they affect fertility behavior: abortions, births, and contraceptive purchases. Our analysis leverages spatial and temporal variation in access to reproductive services across counties in Texas using a difference-in-difference design with county fixed effects. Using data on the location of abortion providers and publicly funded family planning clinics over time, we operationalize the changes in access by focusing on changes in distance to the nearest abortion or publicly funded family planning provider. We define a publicly funded family planning clinic as one that receives state or federal funding. For abortion providers, our measure of access exploits closures whereas for family planning clinics, it leverages both closures and changes in the source of funding (e.g., from public funding to non-public funding). Overall, due to the reduced funding, the number of family planning clinics fell and, for many of those that remained open, so did their ability to serve their customer base. As the impacts of distance are unlikely to be linear, our measures of access are dichotomous - whether or not there is an abortion or family planning clinic within a pre-specified driving distance. For abortion access, much of the action operates on whether or not there is an abortion provider within 50 or 100 miles. For family planning, not surprisingly, the most impactful distance is shorter: 25 miles. In 2015, 24% of the Texas population had no abortion clinic within 50 miles and 11% had no publicly funded family planning clinic closer than 25 miles.

How might the reductions in abortion and family planning access impact fertility outcomes such as abortions and births? A priori, the effects are ambiguous. Reduced access to abortion clinics could cause a woman to have a child when she otherwise would not have, leading to fewer abortions and an increase in births. Alternatively, forward-looking individuals may practice safer sex or abstain, resulting in fewer abortions and potentially lower fertility rates.[6] If the increased distance is not prohibitive, one might expect no alteration in either births or abortions. Similarly,

[6]This is the basic finding of Kane and Staiger (1996) for teenagers in the response to the closing of abortion clinics and declines in Medicaid funding.

the effect of reduced access to family planning services may also be ambiguous. Reduced access may lessen the frequency of contraceptive use, such as IUDs and condoms, which are often dispensed for free or reduced cost at such clinics. As a result, the incidence of unintended pregnancy may rise, possibly leading to either increased abortions, increased births, or both. The impact of family planning services may also operate through sexual education and family planning practice knowledge. In this case, it would be reasonable to expect fertility rates to increase with more restricted access to family planning services.

Several features make Texas an interesting and useful setting for studying access to abortion and family planning. First, the policies examined here are reflective of those currently on the policy agenda nationwide. Second, estimated effects in Texas are likely more informative about the effects of nationwide policy changes compared to the analysis of other states. Because of Texas' size, travel across state lines to other states is less feasible for most residents. Third, unlike in most other states, by law, family planning services are administered separately from abortion services, and thus, we can separately estimate effects of changes in access to abortion and family planning services.[7] Fourth, Texas maintains a consistent and high-quality set of data on abortions by county and age. National abortion data are limited and the quality (i.e., completeness) of state-level data vary significantly (Jacobson and Royer, 2011).

At first glance, the effects of this legislation look dramatic as seen in Figure 1. This figure displays the time-series patterns of births in Texas alongside a synthetic control for Texas. The three vertical bars represent the three pieces of legislation we exploit - first, the Texas Department of State Health Services (TDSHS) cuts in 2011 reduced funding for family planning clinics by 67%; second, the Women's Health Program (WHP) effectively eliminated Medicaid fee-for-service reimbursement of family planning services for Planned Parenthood affiliates in early 2013; and third, later that year, House Bill 2 (HB2) imposed significant regulations on the operation of abortion providers. The fertility rates for Texas and its synthetic control begin to diverge slightly after the enactment of the TDSHS cuts and the pace of separation accelerates with the WHP legislation and

[7]The other states with similar policies include Arizona, Arkansas, Colorado, Indiana, Ohio, and Wisconsin. Source: https://www.guttmacher.org/state-policy/explore/state-family-planning-funding-restrictions.

HB2.[8]

Our primary identification strategy exploits quasi-experimental variation in access across counties within Texas rather than statewide variation as in Figure 1. First, we find that having no abortion provider within 50 miles reduces the observed number of abortions by 16.7%. This estimate may not capture the true effect on abortions as women could travel to other states not covered by our data to receive an abortion or could self-administer an abortion.[9] For this reason, the impact of the reduction in abortion access on births, a 1.3% increase, is more informative of the total effect on fertility-related behaviors. The effect of reduced family planning access on births, as measured by whether or not there is a funded clinic within 25 miles, is similar. Overall, not having a funded clinic within 25 miles increases births by 1.2%. The effects are heterogenous across different demographic groups, and the groups most impacted by reduced access to family planning services are distinct from those most affected by reduced abortion access which highlights the importance of separately estimating the effect of access to each type of clinic.

While it is standard in the abortion and family planning literature to focus on the outcomes of abortions and births, such analyses miss impacts on precautionary behaviors (e.g., contraceptive use). This is mainly due to data limitations rather than a lack of interest. Most utilized data (e.g., National Survey of Youth Women as used in Goldin and Katz (2002) or the National Survey of Family Growth) are retrospective and measured at low frequencies. Earlier papers by Akerlof et al. (1996) and Kane and Staiger (1996) develop theoretical models showing that fertility-impacting policies could influence the use of contraceptives. We use a new source of data on contraceptives based on weekly retail purchases of condoms and emergency contraceptives from the Nielsen

[8]The TDSHS cuts impact births with a delay. In our later analysis, the effects of the changes in family planning services act with a 1-year delay. There are two possible explanations for this. First, given the length of time between conception and birth of 40 weeks, there is a delay between the policy's enactment and the observed effect of the policy. Second, one of the most common services of family planning providers is the insertion of intrauterine birth control devices (IUDs), which have lifespans of several years. Thus, while a reduced ability to provide IUDs will affect the flow of women receiving IUDs, the effect on the stock of women with IUDs, the relevant at-risk group, takes longer to manifest.

[9]The Texas Policy Evaluation Project at The University of Texas at Austin estimates that at least 100,000 women in Texas have attempted a self-induced abortion. This statistic is likely higher in Texas than in other states due to the close proximity of Texas with Mexico where misoprostol, an abortion-inducing drug, is available at pharmacies without a prescription. See http://liberalarts.utexas.edu/txpep/news/article.php?id=10043.

Retail Scanner database. Contraceptive purchases exhibit little response to the changes in reproductive services.

We provide several specification checks to ensure that we are identifying the effect of access rather than potentially coinciding factors. Our exploited quasi-experimental variation occurs on the cusp of the Great Recession when fertility rates were falling. To understand whether our results are biased by trends caused by the Great Recession or other factors, we conduct several tests such as limiting the time period of our analysis or including region-specific time trends, and we obtain similar results. We also attempt to predict changes in clinic access using pre-policy changes in fertility rates and find no statistically significant relationship, further suggesting that differential pre-trends in the outcome are unlikely to be biasing our results.[10] One caveat to our work is that we use cross-county and cross-time variation within Texas, effectively contrasting more affected with less affected counties in Texas. This contrast, of course, will miss the overall effect of the legislative changes on reproductive services in Texas. To ascertain how this affects the conclusions of our analysis, we compare the time trends in Texas with a synthetic control as shown earlier in Figure 1. While one might argue that such analysis is not as credible as those produced from our main identification approach, the estimated effects on births are of similar magnitude - a 2.8% increase using the synthetic control approach, compared to 1.3% and 1.2% increases for abortion and family planning access, respectively (a combined effect of 2.5%).

This study complements the extensive previous work on family planning and abortion services in three important ways. First, we focus on a substantial and significant contraction in family planning and abortion services. Much of the existing literature focuses on early expansions in family planning and abortion access (e.g., Roe v. Wade and the adoption of the birth control pill).[11] Exceptions include the implementation of parental consent and notification laws (Sabia and Ander-

[10] This indirect test for parallel trends in the pre-policy period is similar to a procedure used in Lahey (2014).

[11] For example, many studies have examined the expansion of oral contraceptives and show that it led to delayed childbearing, reduced fertility, increased career investment for women, and better child outcomes (Myers, 2017; Bailey, 2013; Ananat and Hungerman, 2012; Bailey, 2012, 2010; Kearney and Levine, 2009; Bailey, 2006; Goldin and Katz, 2002). Using more recent variation in contraceptives, Gross et al. (2014) show that expanding access to emergency contraceptives has little effect on birth or abortion rates, while Lindo and Packham (2015) conclude that expanded access to long-acting reversible contraceptives through the Colorado's Family Planning Initiative reduced teenage fertility rates.

son, 2016; Colman et al., 2013; Guldi, 2008; Joyce et al., 2006; Levine, 2003; Averett et al., 2002; Levine, 2001; Blank et al., 1996; Joyce and Kaestner, 1996) which only affect teenagers, waiting periods (Bitler and Zavodny, 2001) and acts of violence at abortion clinics (Jacobson and Royer, 2011). Given the current policy environment and the fact that contractions in coverage may incur different impacts than expansions in coverage, our study is relevant for understanding the effects of policies under debate today. Second, we focus on contemporary variation in access (i.e., changes within the last decade). During the 2000's, contraceptive technologies (e.g., IUDs, hormonal patch, vaginal ring, and female condom) improved in terms of their effectiveness and safety, and emergency contraceptives entered the market.[12] Moreover, over time, female labor force participation has markedly increased, making a woman's decision to bear a child more complicated. Thus, fertility-related policies today might affect behaviors differently from the past. Third, the distinct and separate quasi-experimental variation in abortion clinic access and family planning services (the correlation in the variation is 0.16) combined with the legislative environment in Texas (i.e., funded family planning clinics are prohibited from providing abortion services) allows us to isolate the impact of changes in family planning services from the effect of changes to abortion services.

More directly, our paper adds to the literature evaluating the impacts of recent changes to reproductive services in Texas. Several findings from recent and concurrent work emerge: 1) within-county changes in distance to the nearest abortion provider strongly correlate with within-county changes in abortion rates – varying from 10 to 50% depending on the change in distance (Quast et al., 2017; Grossman et al., 2017; Cunningham et al., 2018), 2) by the start of 2013, the closure of clinics within one large network of family planning providers led to an increase of birth rates of 1.2% for every 100 mile increase in the distance to the nearest facility (Lu and Slusky, 2017), 3) teenagers were particularly susceptible to the 2011 family planning cuts with birth rates increasing 3.4% as a result (Packham, 2017), and 4) the exclusion of Planned Parenthood affiliates from Medicaid led to 30% declines in long-acting reversible contraceptives (LARCs) and injectable contraceptives among Medicaid recipients in areas with Planned Parenthood affiliates (Stevenson

[12] See http://www.ourbodiesourselves.org/health-info/a-brief-history-of-birth-control/.

et al., 2016). While this literature confirms that the policies impacted reproductive decisions and outcomes, it is difficult to characterize the complete effect of the Texas legislation because of the prior literature's focus on subpopulations, some but not all of the legislative changes, and a limited set of outcomes.

The goal of our paper is to provide a more comprehensive look at the impact of the Texas legislative changes - examining both the effects of abortion and family planning access. Using data through 2015 (and data on births through 2016), we study a broader set of fertility outcomes in an attempt to gain a better understanding of the different ways in which Texas women are changing their fertility behavior. These outcomes include abortions, births and over-the-counter contraceptive purchases. Impacts of abortion access on births have largely been overlooked with the exception of the concurrent working paper Cunningham et al. (2018). Cunningham et al. (2018) examine the effects of abortion access on abortions and births; while they find similar impacts of abortion access on abortions, they conclude that there are no detectable birth effects. This difference with our findings is discussed in more detail in Section 5.2. Effects on retail contraceptive purchases are unknown even though the results of Stevenson et al. (2016) open up the possibility of compensatory behavior.

2 Background

In Subsection 2.1 we describe the policy setting in the U.S. and Texas. Subsection 2.2 includes a detailed discussion of the three pieces of Texas legislation leveraged in this study.

2.1 Policy Setting

In 2014, there were 1,795,160 women in need of free or subsidized reproductive care in Texas. The Guttmacher Institute characterizes a woman in need if she is sexually active, is able to conceive, wishes not to become pregnant, and is an adult with a family income below 250% of the federal poverty level or is younger than 20 years of age (regardless of income). Publicly funded family planning clinics serve these women and they encompass a diverse set of health providers including public health departments, Federally Qualified Health Centers (FQHC), Planned Parenthood affiliates, hospital outpatient clinics, and other independent non-profit health centers.[13] Services provided by publicly funded clinics include free or subsidized contraceptives, screenings for STIs, Pap tests, vaccination for human papilloma virus (HPV), other key preventive care services, and sexual education.

Publicly funded clinics may receive a variety of federal and state grants. Title X, one of the main funding sources for family planning clinics, is a federal program dedicated to family planning.[14] Congress introduced Title X in 1970 as part of the Public Health Service Act. The goal of this legislation was to make family planning services available to women who wanted them but could not afford them. Today, Title X clinics still play a critical role in ensuring all women have access to family planning services. It remains the only federally funded program dedicated solely to providing reproductive care to low income and uninsured individuals. Receipt of Title X funds is also tied to other federal programs. Clinics receiving Title X funds are eligible for the federal 340B Drug Pricing Program, which provides discounts on pharmaceuticals (including contraceptives) of up to 50%. Clinics receiving Title X funds are exempt from state-level parental consent laws (including Texas) that require teenage women to gain parental consent before obtaining prescription contraceptives.[15]

Despite this large demand for subsidized reproductive services, public funding for these clinics remains controversial among policymakers and the general public. Those that seek to limit funding often view these clinics as closely tied to abortion clinics. Federal law does permit publicly funded family planning clinics to provide abortions, but it is unlawful for federal dollars to fund such procedures. Certain states including Texas, however, go a step further and disallow family planning

[13]There also exists in the U.S. a small number of non-profit family planning clinics that are funded exclusively by private contributions and receive no tax dollars, though in Texas these are quite rare.

[14]Publicly funded family planning clinics are also funded by Medicaid (the largest source of funding), Title V (maternal and child health), and Title XX (social services).

[15]Note that laws requiring parental consent for prescription contraceptives are distinct from those that require parental consent for abortion; only Texas and Utah have laws requiring parental consent for prescription contraceptives.

clinics that receive any public funding from providing abortions. This law has been in effect in Texas since 2003.[16] Proponents of the law believe that in the absence of such restrictions, abortion services are indirectly funded as clinics can use public funds for eligible services, thereby freeing up non-public funds for abortions.

Because of these federal and state laws, in Texas there are two distinct types of clinics that are relevant to our study: stand-alone abortion clinics that exclusively provide abortion services and are privately funded, and publicly funded family planning clinics which provide contraceptive care and other services, but *not* abortions.[17]

2.2 Legislative Background

We leverage three state-level reproductive policies that create large unanticipated shocks to the supply of abortion and non-abortion family planning services in Texas: (1) the 2011 cut to TDSHS for publicly funded family planning services, (2) the 2011 change in the Women's Health Program which was rolled out in 2013 and disallowed certain clinics from receiving Medicaid reimbursements, and (3) House Bill 2 which took effect at the end of 2013 and the beginning of 2014 and greatly reduced access to abortion clinics.

2.2.1 Cuts to TDSHS Funding

In 2011, the Texas government enacted two pieces of legislation which drastically cut funds to family planning clinics in the state, and in particular reduced funding to Planned Parenthood affiliates. The first funding cut reduced the TDSHS budget for family planning services. Previously, TDSHS funded clinics through federal and state grants including Title V (maternal and child health), Title X (family planning) and Title XX (social services).

The budget cut to TDSHS reduced funding by about 67% – a cut from $111 million per bi-

[16] See Seventy-eighth Legislature, Regular Session General Appropriations Act.

[17] Women may also obtain abortions from a hospital or a general practitioner, but in Texas in 2012 – according to a report by TDSHS – only 0.3% of all abortions took place in these types of facilities, where most of them involved an extenuating circumstance (i.e., ectopic pregnancy). By and large, Texas women obtain these services in stand-alone clinics.

ennium to $37.9 million for the 2012 - 2013 biennium budget. Importantly, this piece of legislation also reallocated the remaining funds according to a newly-implemented tiered system which granted funding priority to clinics providing comprehensive primary care over those who specialized in providing family planning services. Planned Parenthood clinics were lowest in funding priority.

These reductions in funding greatly reduced access to family planning clinics. Of the roughly 200,000 women receiving care through these programs, 40% received services from Planned Parenthood and other clinics specializing in family planning (White et al., 2015). According to White et al. (2015), during 2012 25% of clinics shut down and many of the ones that remained open were forced to reduce hours of operation and/or downsize staff. Of the remaining clinics, on average, they only served 54% of the patients that they served in the pre-period. Moreover, due to the funding cuts, in many cases women were now required to pay fees for services and prescriptions that were once free of charge.

Title X funds are a particularly important funding source for family planning, and by reallocating Title X funds according to the new priority-based funding scheme, many clinics whose emphasis was on family planning ceased to receive Title X funds beginning in 2011. In response, the federal agency in charge of awarding Title X grants chose not to award the sole Title X grant for the state of Texas to the state government, and instead awarded the grant to the Women's Health and Family Planning Association of Texas (WHFPT); an organization unaffiliated with the state government. The change in grantee took effect at the beginning of 2013. The new grantee did not have to abide by the state's priority-based funding scheme, and was able to restore Title X funds to some clinics specializing in family planning.

2.2.2 Medicaid Fee-for-Service Family Planning Program

In 2011 the Texas government also passed legislation that excluded family planning clinics that were affiliated with abortion providers (e.g., Planned Parenthood affiliates) from receiving reimbursements through the Women's Health Program (WHP).

The WHP was a fee-for-service family planning program with the goal of subsidizing family planning services for low income women. Through this program women could obtain, either for free or at a subsidized rate, contraceptives, cancer screenings, STI testing and treatments, pregnancy tests, Pap tests, and pelvic exams. From 2007 to 2011, Texas operated this program through a Medicaid waiver program meaning that it was almost entirely funded by federal dollars; approximately $30 million per year comprising 90% of the annual budget for WHP (White et al., 2015).[18]

However, in 2011 when the Texas government passed this legislation, the federal government terminated their contribution stating that the legislation violated federal law by discriminating against qualified federal providers. The state of Texas responded by replacing the WHP with a state-funded program called the Texas Women's Health Program (TWHP), which was implemented on January 1, 2013. This program was identical to the previous federally funded program but was funded solely by state general revenue and excluded all clinics in a network affiliated with an abortion provider.

2.2.3 House Bill 2

On July 18, 2013 Texas House Bill 2 (HB2) was signed into law. In contrast to the previous two pieces of legislation that reduced funding for non-abortion family planning clinics, HB2 was aimed more directly at abortion providers. Broadly, the bill imposed expensive and difficult-to-implement requirements on abortion facilities. Such bills are often know as Targeted Regulation of Abortion Provider (TRAP) laws. The bill required the following: (1) physicians administering abortions must have admitting privileges at a hospital within 30 miles of the abortion clinic, (2) abortions after 20 weeks post-fertilization are prohibited, unless there is severe fetal abnormality or risk of irreversible physical impairment of a major bodily function (not including psychological) to the mother, (3) in accordance with Food and Drug Administration regulation, women must visit the doctor for each of the two doses of the abortion pill and, after taking the pill, the patient must

[18] Medicaid is a health care safety net program in the United States typically funded jointly by federal and state governments.

be seen in a follow-up appointment within 14 days,[19] and (4) all abortions must be performed in a clinic that meets the requirements of an ambulatory surgical center (Texas Policy Evaluation Project, 2015).

The first three provisions of the bill went into effect on November 1, 2013, causing the first wave of abortion clinic closures. The fourth provision of HB2, which required all abortion facilities to meet the requirements of ambulatory surgical centers (ASC), took effect October 3, 2014 but was only enforced for two weeks before the U.S. Supreme Court overruled the provision.[20] Converting a clinic to meet these standards is costly both financially and in time as there is a detailed licensing process, and clinics have to meet physical requirements such as certain room dimensions and corridor widths. This regulation impacted the ability of several additional clinics to provide abortions, but only temporarily. In summary, by 2015 over half of the abortion clinics from the pre-HB2 period were no longer in operation.

3 Data

3.1 Abortion and Family Planning Access

Our measures of access to abortion are constructed using the driving distance from each county to the nearest clinic providing abortions; this requires information on the location and dates of operation for all clinics offering abortions in both Texas and neighboring states. Our measures of access to family planning are constructed using the driving distance from each county to the nearest clinic currently receiving public funding; this requires information on funding status by year for all clinics in Texas and neighboring states. Additional details on clinic locations, dates of operation, funding status, and measuring distances are discussed in Appendix B.

Since the effects of distance are unlikely to be linear, we use binary measures of access. For

[19]This is in addition to the existing 24 hour waiting law that requires women who are within 100 miles of a clinic to wait 24 hours between the initial visit and the actual abortion, including those seeking a medical abortion (the pill).

[20]For more on the timing of the Supreme Court and Fifth Circuit Court of Appeals rulings regarding the provisions of HB2, see the following Texas Tribune article https://www.texastribune.org/2016/06/27/us-supreme-court-rules-texas-abortion-case/.

abortion access, our measures are: no clinic within 25 miles, no clinic within 50 miles, and no clinic within 100 miles. To provide context, in 2015 the share of the Texas population that lived in an area with no abortion clinic within 25 miles, 50 miles and 100 miles was 37%, 24%, and 12%, respectively. In our regression analysis, we exploit clinic closures which generate across time, within-county variation in abortion clinic access.[21]

For family planning services, our measure of access is slightly different. As family planning clinics are more prevalent than abortion clinics, we leverage shorter changes to distance. We use indicators of no publicly funded clinic within 10 miles (21% of the population in 2015) and no publicly funded clinic within 25 miles (11% of the population in 2015).[22] The family planning clinic access measure captures changes to the funding status of the clinic and not necessarily just clinic closures. As such, this access variable encapsulates variation along both the intensive and extensive margins. We view this as a preferable measure as the funding cuts to family planning clinics affected the ability of many clinics to provide family planning services, while not necessarily resulting in a closure. For instance, if a county in a given year no longer has a publicly funded family planning clinic within 25 miles, then it could be that the clinic closed *or* that it remained open and now operates on restricted hours, with reduced staff, and/or has substituted toward relatively lower cost procedures and drugs. Whether the clinic closed or remained open without public funding, access has been restricted in either scenario.

The variation in access to abortion that we utilize is summarized in Figure 2, which visually displays the change in our measure of abortion access for each county in Texas across the last six years of our sample. Starting in 2013, consistent with the timing of the Texas policies, we see considerable changes in the distances to the nearest abortion provider with many counties

[21] In principal, our measure of access will vary in response to a clinic opening too, but the majority of clinic openings over our period occur in the beginning of the sample frame and our estimates are not sensitive to the exclusion of these earlier years. Furthermore, clinic openings that occur at the end of the sample frame were in response to HB2 (i.e., clinic re-openings, or clinics that opened to replace a closed clinic).

[22] We focus on the 25 mile measure for family planning as there is high potential for measurement error with the 10-mile measure given that we are only able to observe county of residence. Because county centroids are population-weighted we are essentially measuring the distance between a county's metropolitan center and the address of the nearest clinic; as such, the 10-mile measure is intended to reflect the absence of publicly funded family planning clinics from a county's population center.

experiencing a change of 100 miles or more.

Similarly, the variation in access to publicly funded family planning is displayed in Figure 3 and demonstrates the change in our measure of family planning access for each county in Texas across the last six years of our sample. This map reinforces that the changes in distance to family planning providers that we exploit is shorter than the changes in abortion access. Contrasting Figure 2 and Figure 3 highlights that the variation in abortion access and family planning access is largely uncorrelated.

3.2 Other Controls

To mitigate worries about confounding factors such as economic and population change, we collected county-by-year unemployment rates and per capita income from the Bureau of Labor Statistics's Local Area Unemployment Statistics and the Regional Economic Information System. We also use age-specific population counts from the Census Bureau and race/ethnicity-specific population counts from the CDC Wonder system. All control variables are measured at the county-by-year level, so we interpolate across months within the year in our county-by-month level regressions to avoid large jumps at the start of the year.

3.3 Outcome Data

3.3.1 Abortion Data

We obtained data on all legal abortions performed in the state of Texas by age, county of patient's residence, and year for 2006-2015 from TDSHS.[23] These data cover abortions performed in Texas and in a limited number of other states but do not indicate the county of occurrence.[24] While abortions occurring within Texas are well-documented, states outside of Texas do not consistently report abortions to Texas residents to TDSHS. For example, New Mexico reported in 2015 but not

[23] These data are publicly available and can be found in Table 34 of the Vital Statistics Annual Reports (2006-2015).

[24] TDSHS receives most of the out of state counts from the State and Territorial Exchange of Vital Statistics (STEVE) system, which is an electronic interstate exchange for vital statistics data including induced termination of pregnancy (ITOP). Reporting ITOP records to STEVE is voluntary and varies by state and year.

in 2014.[25] Data on abortion rates (per 1,000 women age 15-44) are described in Table 1. The overall abortion rate is nearly 13, similar to the 2013 national average of 12.5 (Jatlaoui, 2016).

To better understand the degree to which women travel to other states for abortion services and, thus, are unobserved in our data, we also collect abortion counts in this same time period for each of the states near Texas, including all border states – Arkansas, Colorado, Kansas, Louisiana, New Mexico, and Oklahoma – from each state's health department. These data are more limited than the Texas data in the sense that we can calculate the number of abortions to non-resident women in these outlying states but we do not know from which state or county they come from. Nevertheless, we can provide some descriptive evidence of travel behavior using cross-time and cross-state variation.

3.3.2 Natality Data

Birth data come from the restricted version of the National Vital Statistics System (NVSS) natality files. These files contain information on all U.S. births including information on the county of mother's residence, the year and month of birth, and mother characteristics (e.g., age, education, ethnicity, marital status, and parity). We utilize data on all births occurring between 2006-2016 to mothers residing in Texas.

The timing of access to abortion and family planning relative to the timing of births is an important consideration. Specifically, births should be matched to abortion and family planning access at the time when access to these services is most relevant to fertility decisions. As such, we match births to abortion clinic access at the 13th week of gestation (i.e., the end of the first trimester), and to family planning clinic access at the time of conception.[26] Although we have data on births that occur in all months of 2016, the process of assigning births to abortion access at the 13th week of gestation means that a birth occurring in December would have been in its 13th week of gestation mid-2016. Consequently, the only observed pregnancies leading to birth during

[25] Note, clinics in Albuquerque, New Mexico provide a non-trivial number of abortions to out-of-state residents throughout our sample period, as discussed in Section 5.1.

[26] This matching is discussed further in Appendix B.

much of 2016, particularly the latter half of the year, would be very early births. In addition, our measure of access to family planning is currently available through 2015. As such, we use data on pregnancies leading to birth through December 2015. The final sample represents data on 3,951,350 pregnancies leading to birth.

We utilize other features of these data to explore the heterogeneity of our estimates – in particular, we examine the number of births by age (15-19, 20-29, 30-39, 40-44), ethnicity (Hispanic, non-Hispanic), education (high school or less, some college or more), marital status (married, unmarried) and live-birth parity (first, second, third, fourth or more). Summary statistics for these data are provided in Table 1. The overall fertility rate is elevated relative to the 2015 national average of 62.5.[27]

3.3.3 Contraceptive Purchases

An earlier literature (Akerlof et al., 1996; Kane and Staiger, 1996) highlights a potential interaction between reproductive policies and contraceptive use. However, testing the effect of reproductive policies on contraceptive use has proven to be challenging - mainly due to limited data. For example, the National Survey of Family Growth, the best source of modern data on contraceptive use, covers a relatively small sample of individuals and relies on retrospective recall data, making difference-in-difference analyses like this one nearly impossible.

In this paper, we bring to bear new data on contraceptive use from the Nielsen Retail Scanner database. This large database includes more than 35,000 grocery, drug and mass merchandise retailers across the U.S. and accounts for more than half of the total sales volume in grocery and drug outlets and approximately one third of sales volume for mass merchandise outlets. These data provide weekly sales volumes and prices for nearly every item sold in participating outlets.[28]

There are approximately 2.4 million items included in the data (items are identified by their

[27] Source: https://www.cdc.gov/nchs/nvss/births.htm

[28] Exceptions include prescription drugs, for instance. This measure of contraceptive purchases will undercount total contraceptive use as family planning clinics, doctors' offices, and other health care providers also dispense contraceptives. However, one of the goals with these data is to understand the degree to which individuals substitute over-the-counter contraceptives for those offered by health care providers.

UPC code). Our focus is on contraceptives. Nielsen groups items into approximately 1,100 categories at the finest level, including two categories for contraceptives: Male Contraceptives and Female Contraceptives. All products in the Male Contraceptives category are condoms, and we refer to these as such from here forward. Products in the Female Contraceptives category represent a wider range of products including ovulation tests and emergency contraceptives (ECs). Because some of the products in this category may be used to *improve* the likelihood of pregnancy rather than prevent pregnancy (e.g., ovulation tests), we do not examine all products in this category and instead focus only on ECs.

We study two types of outcomes, expenditures on contraceptives and the number of units sold. For each product, we observe the number of individual units included in a package; for example, we observe that a 12-pack of condoms represents 12 individual condoms. There is wide variation in the size of multi-packs for condoms, but ECs are only available in either one or two dose packs.

All outcomes are constructed at the store-by-month level.[29] The finest geographical identifier for each store is the county, allowing us to merge the store-level data with our measures of access to abortion and family planning. In total, the data include 2,780 stores in Texas that were operating between 2006-2015. The distribution of sales and expenditures varies widely across the three store types covered by these data (i.e., drug stores, grocery stores, and mass merchandise stores), with 83% of contraceptive expenditures occurring in drug stores. The distribution of total expenditures, condom expenditures, and EC doses are presented in Figure D1. To get a sense of the variation in distributions across store types, mean total contraceptive expenditures per week are approximately $563, $64, and $35 in drug stores, grocery stores, and mass merchandise stores, respectively. To ensure the distribution of the outcome is not multi-modal, our preferred sample includes only drug stores. There are 711 drug stores in our sample operating in 81 counties in Texas.

[29]Because the finest temporal scale in the data is the weekly level, where weeks are identified as a Saturday and refer to the previous seven days, the number of weeks assigned to each month depend on the number of Saturdays (i.e., some months will include four weeks and others five). To account for this, our outcomes are constructed to represent monthly sales volume and expenditures *per week*.

4 Empirical Framework

We exploit quasi-experimental variation in access to family planning clinics and abortion clinics across counties and over time in Texas to separately identify the causal relationship between access to each type of clinic and the number of abortions, the number of births, and contraceptive purchases.[30] Essentially, we compare changes in outcomes between areas where clinic access became more restricted to areas that experienced relatively little change. The general form of our difference-in-difference model is as follows:

$$Y_{ct} = \beta_0 + \beta_1 Access_{ct} + \beta_2 X_{ct} + \gamma_c + \alpha_t + \delta_{HHS} \times t + \varepsilon_{ct} \tag{1}$$

Y_{ct} is the outcome in a given county c and time t. In the analysis of abortions, t represents years as the data are at the county-year level; in the analysis of births, t represents year-months as the analysis is at the county-year-month level. Because the abortion and birth data include cells with zero counts, we estimate these models using a Fixed Effects Poisson Quasi-Maximum Likelihood estimator. This estimator allows for the inclusion of county fixed effects but is free of the common incidental parameters problem often present in other non-linear models (Cameron and Trivedi, 2013). In all count models the relevant population – i.e., women of childbearing age (15-44) – is included as the exposure variable to account for the fact that counties vary widely in size and therefore have a different potential for births and abortions.

$Access_{ct}$ is either a measure of abortion clinic access or publicly funded family planning clinic access. As described in the data section, abortion access is characterized by either (1) no abortion clinic within 25 miles, (2) no abortion clinic within 50 miles, or (3) no abortion clinic within 100 miles. The family planning access measure is either (1) no publicly funded clinic within 10 miles or (2) no publicly funded clinic within 25 miles. In each regression, we only include a single indicator for clinic access (e.g., no clinic within 25 miles) rather than the full set of clinic access indicators. As such, the coefficient on 25-mile measure is the effect of having no abortion clinic

[30] A similar identification strategy is used in the literature that documents the effect of distance or proximity to health care providers on individual outcomes, i.e., Buchmueller et al. (2006).

within 25 miles relative to having an abortion clinic within 25 miles, as opposed to the effect of no abortion clinic within 25 miles holding constant the 50-mile and 100-mile measures.

In all specifications we control linearly for family planning access (in the case of abortion access regressions) and abortion access (in the case of family planning access regressions) since a primary objective of the study is to understand the effects of abortion access separately from that of family planning access. One feature of the data that allows for this is the low correlation between changes in abortion access and changes in family planning access; the correlation coefficient is 0.16.

Finally, in Eq. (1), X_{ct} represents a vector of time-varying county-level controls which includes the unemployment rate, per capita income, race/ethnicity-specific populations (Hispanic, White non-Hispanic, Black non-Hispanic, and Other), and female age-specific populations (5-year groups between 15 and 44). γ_c and α_t are county and time fixed effects, respectively.[31] $\delta_{HHS} \times t$ represent region-specific linear time trends, which are included in the preferred specification. Regions are Texas Health and Human Services Regions (HHS), which are groupings of counties. There are 11 such regions in the state. Instead of HHS region trends, an alternative is to include county-specific linear trends, however given that there are 254 counties in Texas (many of which are quite small), including a large number of additional parameters in a Poisson model raises concerns over an incidental parameters problem. Indeed, in attempting to control for county-specific linear time trends, these models often fail to converge. Finally, ε_{ct} is the error term and is clustered at the county level. Clustering accounts for both within county serial correlation in the outcome and overdispersion (Wooldridge, 1999).

In our analysis of contraceptive purchasing behavior we use several specifications, but all are estimated at the store level and can be described generally by the following equation:

$$Y_{sct} = \beta_0 + \beta_1 Access_{ct} + \beta_2 X_{ct} + \gamma_s + \alpha_t + \delta_{HHS} \times t + \varepsilon_{sct} \tag{2}$$

[31] We include year fixed effects in the analyses using annual data and year-month fixed effects in the analyses using monthly data.

Y_{sct} represents either expenditures or the number of units sold for a particular set of products at store s, in county c, in month-year t.[32] The exact specification of the model depends on the outcome analyzed, as the distributions of the outcomes vary substantially across product and store type. We allow these distributions to inform the choice of the specification. The three outcome variables of interest are total contraceptive expenditures, condom expenditures, and the number of emergency contraceptive (EC) doses. The distributions of total contraceptive expenditures and condom expenditures each approximate a normal distribution (with a long right tail), and within drug stores there are very few store-by-month observations with zero expenditures in either category.[33] As such, our preferred specification for these two outcomes is ordinary least squares with the log of expenditures as the outcome. The few observations that equal zero are replaced with expenditures equal to one, though other methods of dealing with this issue produce equivalent results.

ECs are analyzed as a count (number of doses) as opposed to expenditures.[34] The preferred specification for ECs is a Poisson model, similar to our analyses of abortions and births. A potential concern with ECs is that, at the national level, they first became available over-the-counter (OTC) in August of 2006, and their OTC availability has subsequently expanded.[35] The inclusion of HHS-specific linear trends guards against the possibility that this expansion of availability induces differential trends in the outcome.

Identifying Assumption: The identifying assumption for estimating Eqs. (1) and (2) is that

[32] While the exact specification varies by outcome and store type, all regressions are conceptually equivalent to Eq. (1) as the treatment variables are still measured at the county level. Because the stores included in the Nielsen sample do not represent the universe of contraceptive purchases, and because the sample of stores is not representative at the county level, it would be inappropriate to aggregate these measures to the county level. Keeping the unit of analysis at the store-level allows for the inclusion of store fixed effects such that the estimates are identified off of deviations from store-level averages. We follow an approach similar to that used in Cawley and Frisvold (2017).

[33] Out of 69,033 store-by-month observations, 16 and 29 observations equal zero for total contraceptive and condom expenditures, respectively.

[34] There are two reasons for this. First, there is a much higher percentage of observations equal to zero for drug stores (over 2%). Second, ECs are a more homogenous product compared to condoms; the only dimension of variation in ECs is whether they are sold as a 1-pack or 2-pack, and prices per dose are far more uniform compared to condoms.

[35] In 2006, ECs became available for those 18 and older; in 2009 this expanded to those 17 and older; in April of 2013 this expanded to those 15 and older *and* it became available in the aisle rather than behind the pharmaceutical counter; in June of 2013 the age restriction was eliminated altogether. See Trussell et al. (2014) for more detail. Because Nielsen data do not report prescription drug purchases, EC purchases are first observed in mid-2006, thus we start our sample in 2007. We observe 159,432 EC doses sold in 2007 and 402,405 doses sold in 2015; nearly a 150% increase over the period.

the variation in clinic access (family planning and abortion) is uncorrelated with other unobserved time-varying determinants of abortions, births, and contraceptive purchases. Stated differently, to interpret the estimated coefficients as causal it must be that in the absence of changes in access to family planning and abortion services, the outcomes would have continued on a similar trajectory in all counties. The econometric specification does a lot to mitigate endogeneity concerns. The inclusion of county and time fixed effects control for all time-invariant county-level factors and overall time effects that might influence the outcomes. In addition, controlling for the unemployment rate, log per capita income and demographic-specific populations reduce concerns that county level time-varying characteristics that explain the outcomes are also correlated with clinic access.

While the identifying assumption is not directly testable, there is evidence supporting its plausibility. To begin, the timing of the post-2011 changes in clinic access coincide with the timing of the state-level legislative changes suggesting that the legislation caused isolated and unanticipated shocks to the supply of clinics.[36]

We take two approaches to further examine trends in fertility in the period prior to any changes in access to either family planning or abortion services. First, Figure 4 plots the trends in fertility rates over time disaggregated by highly and non highly affected counties. Highly affected counties are those that experienced either a 25 mile increase in driving distance to the nearest abortion clinic or a 10 mile increase in driving distance to the nearest publicly funded family planning clinic between January 2006 and May 2015. Non highly affected counties are the complement. Importantly, in the pre-policy period (2006-2011) the two types of counties appear to be trending similarly, and then as the three policies roll out between 2011 and 2014, the trends begin to diverge. Figure 4 also emphasizes the fact that fertility rates fell substantially in all regions during the Great Recession. To evaluate whether this poses a threat to the validity of the estimates, we test the

[36]Because our measure of access is the distance to the nearest clinic of each type at each period in time, it is possible that the changes in access we observe are not directly related to the legislation that we are studying. Indeed, Figure 4 shows a small percentage of the population is defined as affected prior to 2011. That said, the vast majority of the variation occurs in the post-legislation period, and the insensitivity of the estimates to the exclusion of these earlier years assuages this concern (see, Table C1).

sensitivity of the natality results to different starting years, 2006-2010, and find very similar results across all specifications, see Table C1. This exercise also provides another indirect test of the parallel trends assumption; if there are differential pre-treatment trends in the outcome, then one would expect the point estimates to substantively change as pre-treatment years are successively omitted.

Second, we employ a procedure similar to that used in Lahey (2014) to test for differential trends. Collapsing to the county level, we predict the change in access to each type of clinic occurring between 2011 and 2015 (the roll-out and post-period) using the change in fertility rates from the pre-period, 2006-2010. The endogeneity concern is changes in fertility rates in the pre-period are correlated with changes in clinic access. If this is the case, it will be impossible to distinguish the true impact of clinic access on fertility from changes in fertility due to other unobserved factors. As presented in Table 2, there is no detectable evidence of a relationship between pre-period changes in fertility rates and subsequent changes in abortion or family planning clinic access, further supporting the identifying assumption.

So far, there is little evidence of differential trends prior to the first changes in access in 2011; the first changes were changes in access to family planning. The majority of changes in abortion access, however, began in late 2013 so it is possible given this longer pre-period, differential trends remain a concern. To probe this, we present regression-based event studies which provide further insight into the existence of differential pre-treatment trends. With an event study we are able to more precisely define the pre-period as all months prior to the changes in abortion access and control for access to family planning. Before discussing the results, it is worth noting several factors that make event study estimation difficult in this context. In a typical event study, the treatment is permanent. In our context, the treatment may turn on and then turn off in a subsequent period – that is, clinics re-open in some instances. Second, some of the data are low-frequency; the abortion counts and the family planning access treatment are at the annual level. Consequently, some years are coded as fully treated but in reality are only partially treated – i.e., when treatment status changes mid-year. The problem of low-frequency data is compounded by the possibility that a clinic may close and reopen within a year. With these caveats in mind, estimating an event study is useful for evaluating pre-trends. In particular, we focus on the effects of abortion access on births, in part because these estimates are of primary interest in the paper, but also because both the treatment and outcome are high-frequency (monthly).

Event studies are presented in Figure 5. They are estimated at the monthly level; the period six months prior to the event is excluded as the reference group. The event is defined as the first month in which treatment status switches. We present an event study for each of our three definitions of abortion access (no clinic within 25, 50, and 100 miles). The blue lines represent trends in the coefficient estimates and are allowed to differ pre- and post-event. The trend lines act as a visual smoothing aid – a way to smooth out patterns in the data – and can be used to evaluate whether there are differential pre-trends in births.

The plots in the top row represent the full sample of data (2006-2015). The main takeaway from estimating these full-sample event studies is that over the six-year (72 month) period prior to the event, there appears to be differential trends in the outcome in affected versus unaffected counties, at least for two of the treatment definitions (25 and 50 miles). These figures suggest that we likely overstate any increase in births using the full pre-period. What is also evident is that any differential trends existed well before the changes in access, and that the differential trends in births are flat or decreasing in the period leading up to the changes in access.

In the bottom row, we restrict the sample to a 12 month period on either side of the event. These figures confirm there is little evidence of differential trends in births in the shorter period leading up to changes in access. In the 25-mile measure, there is no visual evidence of an increase in births. In the 50-mile and 100-mile measures, the trends in births are similar before and after the event, but there is a noticeable increase in the number of births in the post-event period. We use the intuition developed here to inform our main regression estimates. In particular, we note that the first major changes in access to abortion occurred in November 2013, and we restrict the sample to a much shorter period prior to this change in access. Results for various samples, including samples analogous to the 12 month window, are discussed in the following section.

Finally, another potential confound is if the state of Texas enacted other policies that changed abortion and fertility rates at the same time, in the same way, and for the same counties as the budget cuts to family planning clinics or the more stringent abortion clinic regulations. We have found no evidence supporting this concern.[37] The Affordable Care Act (the ACA) was implemented during the sample frame; while the ACA did affect reproductive services, Texas opted out of the Medicaid expansions, including the Medicaid family planning expansion.[38] To account for the potential effects of the ACA, we have estimated our models including county-level yearly controls for the fraction of women under 65 who are uninsured (not shown – available upon request). Our results with these controls are virtually unchanged.

5 Results

5.1 Outcome: Abortions

Table 3 presents estimates of the effect of clinic access on abortions. Each estimate in the table comes from a separate regression; Panel A reports results for abortion clinic access and Panel B for family planning clinic access. Within a panel, each row presents a different binary measure of clinic access, where clinic access measures become more extreme moving down the columns. Moving across the table, each successive column includes additional controls. Column 2 adds time-varying controls: economic controls, demographic controls, and a linear control for access of the other type of clinic (controlling for family planning in the case of abortion regressions).[39]

[37] On September 1, 2005 Texas implemented an emergency contraceptive access law which required hospitals to inform victims of sexual assault about emergency contraceptives (e.g., Plan B). This predates our analysis period, which is 2006-2015. Also, greater access to Plan B would only dampen our results. Additionally, on August 24, 2006 a federal law was passed granting all individuals 18 and older access to emergency contraceptives in pharmacies without prescriptions. This law was expanded to include 17-year-olds on April 22, 2009 (Trussell et al., 2014).

[38] Source: http://www.kff.org/report-section/medicaid-and-family-planning-the-aca-medicaid-expansion-and-family-planning/.

[39] We test the sensitivity of our estimates to non-linear controls for access to the other type of clinic, and the estimates remain virtually unchanged for both abortion and family planning (results available upon request).

The preferred specification, column 3, includes HHS region-specific linear time trends.[40] We present estimates for the abortion outcome, which has previously been investigated by Grossman et al. (2017); Quast et al. (2017); Cunningham et al. (2018), in part as a validation check of our approach.

Access to Abortion Clinics

Starting with Panel A column 1, the coefficient of -0.219 on the 25-mile measure implies that abortions fell by 21.9% when a county moves from having a clinic within 25 miles to not having a clinic within 25 miles. As controls are added, the estimates tend to decrease slightly in magnitude and gain precision. Our preferred estimate (column 3) delivers an effect size of 16.6% for the 25-mile measure.[41] As expected, moving from the 25-mile measure to the more extreme measures, the effect sizes tend to grow in magnitude: no clinic within 100 miles is associated with a 22.1% reduction in abortions relative to having at least one clinic within 100 miles.

While our regression specification is different and thus not directly comparable because they categorize distance into bins, Cunningham et al. (2018) estimate that having no abortion clinic within 100 to 200 miles is associated with an increase in abortions of 32% relative to having a clinic within 25 miles. In a short paper, Grossman et al. (2017) estimate that abortions fell 35.7% for counties that became 50 to 99 miles away from an abortion clinic between 2012 and 2014; an effect larger than our own but within the confidence interval of the estimates from our basic specification. Overall, our estimates are of slightly smaller magnitude, primarily due to our inclusion of HHS-region time trends, which are not included in these other studies.

Recall that our estimates only represent the impacts of access to abortion clinics on abortions occurring within Texas or in a subset of other states. We do not observe mother's county of residence for Texas women receiving abortions in *all* states or countries and therefore cannot determine

[40] The sample size in these regressions is 2,530, representing 253 counties over 10 years. We use 253 counties in all regressions because there is one very small county for which age-specific population counts are equal to zero. As it is not possible to estimate a Poisson model with an exposure variable equal to zero, in certain age-specific regressions this county was omitted. For consistency, we chose to omit this county from all regressions.

[41] Note that the average driving distance to an abortion clinic is about 26 miles.

if decreased access to abortion in Texas leads to a total net change in the number of abortions.

Figure 6 provides evidence that Texas women are indeed traveling to nearby states to obtain abortions when access becomes restricted in Texas. Although this figure is descriptive in nature, it shows an increase in the share of abortions provided to non-residents in states that surround Texas, roughly coinciding with the legislation in Texas. In particular, Figure 6 shows a sharp increase in the share of abortions to out-of-state residents in New Mexico and Arkansas, suggesting that Texas women are traveling to these states. In summary, it appears that the reduction in within-Texas abortions is at least in part offset by women traveling to nearby states. Even with more comprehensive data (i.e., data covering abortions to Texas residents in all states outside of Texas by their county of residence), we would surely miss counting some abortions. For example in Mexico, misoprostol, a drug used to induce an abortion, is available without a prescription.

Due to the difficulties of measuring the number of abortions, we place more focus on the birth results. The effects of changes to the abortion clinic market on abortions, however, demonstrate that the intended mechanisms behind our birth results are at play.

Access to Family Planning Clinics

Panel B of Table 3 reports results for the effect of family planning clinic access on within-Texas abortions. Ex ante it is unclear whether one should expect a relationship – and if there is, whether it is likely to be positive or negative – since family planning clinics do not provide abortions. On one hand, reduced access to family planning could lead to a reduction in abortions because there are fewer informal referrals to abortion clinics. However, if reduced access to family planning leads to more unintended pregnancies (as we expect), then abortions may increase. Overall, we find no consistent pattern in the impacts of reduced access to family planning on abortions.

5.2 Outcome: Births

We turn to birth data and ask the following question: Does the reduction in the number of abortions in Texas translate to a proportional increase in births, or are Texas women finding alternative ways to avoid unplanned births?

The natality files have several advantages. First, they are quite complete as they report nearly all births that result in a birth certificate. Second, they code mother's county of residence regardless of where the birth occurs. Finally, these files report month of birth rather than year of birth, which allows us to exploit more of the variation in access.

Access to Abortion Clinics

The main results for the effects of abortion access on births are presented in Table 4. Columns 1-3 of this table use the full sample of birth data, and present estimates in a way that is structured similar to Table 3. Columns 1-3 show that the estimates are insensitive to the inclusion of time-varying controls and the inclusion of HHS-specific trends. Using the full sample and all controls, the 25-mile, 50-mile, and 100-mile estimates indicate a 1.6%, 2.8%, and 1.7% increase in births, respectively.

As described in Section 4, estimates from the full sample may be overstated, at least for the 25-mile and 50-mile measures. Columns 4-6 report estimates from the fully-controlled specification for three limited samples: Jan. 2009+, Jan. 2012+, and Nov. 2012+. The estimates in column 6 limit the sample to a period 12 months prior to the first major changes in abortion access that occurred in November of 2013, and are analogous to the 12-month event studies presented in Figure 5. The magnitudes of the 25-mile and 50-mile estimates are somewhat smaller relative to the full sample, though still statistically significant and we cannot reject that estimates from the full sample and limited samples are the same. The most conservative estimates – and our preferred estimates – use a sample beginning in 2012, and indicate that the 25-mile, 50-mile, and 100-mile measures are associated with 0.7%, 1.3%, and 1.7% increase in births, respectively. The estimates for the 50-mile and 100-mile measures are both statistically significant at the 5% level.[42]

Using similar policy variation, Cunningham et al. (2018) conclude that the induced changes to abortion services did not impact births – a finding at odds with our own. Their conclusion is based on two findings. First, because their Poisson model produces different estimates than the inverse hyperbolic sine model, they are doubtful of the reliability of their estimates. In this context, with counties exhibiting heterogeneity in the number of births, appropriate interpretation of the coefficients obtained from the inverse hyperbolic sine model is difficult. Specifically, if the number of births is small, the coefficient estimates can be interpreted as level changes whereas when the opposite is true, the coefficient estimates have a percentage change interpretation (Burbidge et al., 1988). When there is a mixture of small and large numbers of births, it is not clear how to gauge the size of the coefficient estimates and this fact may explain the differences between the Poisson and inverse hyperbolic sine estimates. Since the Poisson model is explicitly set out to deal with count data, the Poisson estimates are likely more credible in this context. Second, the authors conduct a simulation exercise by adding aborted conceptions based on their abortion estimates to the birth data. Their policy estimates with the simulated data match those with the non-simulated data. Then, because of this similarity, they argue that the estimates with the real, non-simulated data are consistent with a null effect. However, they do not take into account the noise of the abortion effects in their simulations. This is important because the birth estimates are small, so small variants in the added births could affect their conclusions.[43]

Although Cunningham et al. (2018) claim that the abortion effects are too small to distinguish an effect on births, the size of the birth effects that we estimate are consistent with simple back-of-

[42]We also present estimates from alternative limited samples in Figure C1. These figures present coefficient estimates for each measure of access. The left-most estimate within each plot is the full-sample, and each subsequent estimate omits an additional three months from the sample. The right-most estimate uses data only from July 2013 and later (dramatically limiting the pre-period). Focusing on the 50-mile measure, this figure confirms that the estimates fall as additional months in the pre-period are omitted until approximately January 2012, at which point the estimates either level out or begin increasing as more months are omitted from the sample. This figure also demonstrates the stability of the 100-mile measure.

[43]In the October 2017 version of their paper, Cunningham et al. (2018) propose that our results contrast with theirs due to a difference in the way we code whether an abortion clinic is closed or open. However, when we follow their coding, our estimates are virtually unchanged, see Table A1 and Table A2. Furthermore, discrepancies between our coding of dates and the coding used in Cunningham et al. (2018) are discussed in detail in Appendix A; Table A3 displays the dates used in our paper and makes note of any discrepancy.

envelope calculations assuming that the entire abortion effect is transmitted to a birth effect. More specifically, if we use our estimated abortion impacts (16.7% decrease for the 50 mile measure) and assume that all of these unrealized or unobserved abortions would have resulted in births, a back-of-the-envelope calculation using the abortion-to-birth ratio (approximately 0.173 in our data) implies an increase in births of approximately 2.9%. This predicted increase represents an upper-bound on the expected birth impacts because in reality some Texas residents obtain abortion services in states that do not report back to Texas, some may travel to Mexico for misoprostol, and others may take up precautionary behavior. Our analogous estimate for births is a 1.3% increase (approximately 45% of the predicted upper bound effect), implying that a substantial proportion (55%) of the predicted additional births were avoided through unobserved travel or precautionary behavior.

Access to Family Planning Clinics

The impacts of access to family planning services on births are presented in Table 5. Panel A presents estimates analogous to the first three columns of the preceding tables. In the first column, with no controls or trends, there is evidence that having no clinic within 25 miles leads to an increase in births. These estimates, however, do not hold up across the different specifications and many of the point estimates are imprecisely measured. Panel A reports the contemporaneous effects as family planning access is assigned to births at the time of conception. Although a natural place to begin, this assignment is somewhat arbitrary. There is little reason to believe that reduced clinic access should affect births immediately. It is possible if a woman is using short-acting hormonal contraceptives such as the pill, patch or ring, she may have a stock as one can obtain up to three months supply at a time. It is also the case that in order to obtain (or renew) a prescription for contraceptives, one must undergo an annual exam from a licensed physician. Finally, LARCs such as implants, injections or IUDs, last from 3 to 10 years depending on the brand and type. Consequently, there is no obvious *a priori* expectation about the specific dynamic structure of the relationship between access to family planning and births.

To probe the possibility that lagged family planning access has important impacts, we employ the 25-mile measure and modify Eq. (1) to include various lags. We focus on the 25-mile treatment in part because we found suggestive evidence of birth impacts for this treatment when access was measured contemporaneously, and in part because we believe this measure is less subject to measurement error in comparison to the 10-mile measure.[44] These results, which include time-varying controls and HHS trends, are presented in Panel B (columns 4-6). Each successive column adds an additional 12-month lag such that column 6 includes the contemporaneous effect, and a one, two and three year lag in the same regression.[45] This exercise reveals there is consistently a larger effect on the 12 month lag that is statistically different from zero at the 5% level. Having no publicly funded family planning clinic within 25 miles is associated with a 1.2% increase in births that are conceived 12 months from the time at which access is measured.[46] In all subsequent tables we report the contemporaneous effect and the 12 month lag; the specification that corresponds to column 4, as this is where the impacts of reduced access to family planning are concentrated.[47]

5.3 Birth Heterogeneity

To understand whether different subpopulations are affected differently by the changing landscape for reproductive services, we estimate how the effects of access vary across maternal age, parity, mother's ethnicity, mother's education, and mother's marital status. Tables 6 to 8 present estimates for these groups. In each table, Panel A reports the effects of access to abortion on births, and uses the sample restricted to the period January 2012 and beyond. Panel B examines the effects

[44] To be concrete, because distances are measured from population-weighted county centroids to clinic addresses, a measure indicating that there are no clinics within a short distance (i.e., 10 miles) is more likely to mis-assign access to family planning for women who do not live near the population center of a county.

[45] Recall that our measure of access to family planning is only available at the annual level, it is not possible to include lags for periods shorter than 12 months.

[46] For the outcomes involving abortion counts, we also experimented with different lags of family planning access, but the results did not qualitatively change. That is, there continued to be no consistent pattern of publicly funded family planning access on abortions.

[47] We contrast our results with Lu and Slusky (2017) who use variation in access to one single large provider of family planning services through the year 2013. Using a linear family planning access measure without controls for abortion clinic access, they find that an increase of 100 miles to the nearest clinic translates into a 1.2% increase in the birth rate.

of access to family planning on births, and reports the contemporaneous effect (row 1) and the 12-month lag (row 2). Table 6 reports results by age of mother (four groups), Table 7 by birth parity (four groups), and Table 8 by ethnicity, education, and marital status (six groups). Because we examine 14 sub-groups, we want to rule out the possibility that any heterogeneous impacts are driven by a spurious finding stemming from testing many hypotheses (Lee and Shaikh, 2014; List et al., 2016). We provide corrected p-values using the method of Bonferroni (1935). This correction is an extremely conservative approach, and as such we have a high degree of confidence in any estimates that remain significant after the correction. We discuss the heterogeneity patterns for each type of clinic in turn.

Access to Abortion Clinics

As evident by Table 6 Panel A, we find that the significant impacts using corrected p-values are concentrated among relatively older mothers. Having no clinic within 50 miles is associated with a 2.8% increase in births for mothers in their 30s and an 8.5% increase in abortions for mothers ages 40-44. While the impacts for mothers ages 40-44 are particularly large, keep in mind these estimates are identified off of a relatively small number of births – in our sample, births to women ages 40-44 only represent 2.2% of all births.

Table 7 Panel A reveals no strong patterns by birth parity, though the estimates tend to be somewhat larger for mothers who already have children. The only coefficient that is marginally significant after correcting for multiple hypothesis testing is mothers who already have at least three children (using the 50-mile measure).

Results by three additional mother characteristics - ethnicity, education and marital status - are reported in Table 8 Panel A. There are two groups for whom the estimates are significant using corrected p-values: Hispanic women (50-mile measure) and married women (50-mile and 100-mile measures). The 100-mile estimate for married women, which suggests a 4.2% increase in births, is the most significant of all the reported point estimates (corrected p-value$<$0.001). The strength of this finding warrants further investigation. To ensure that this estimate is not driven by differential fertility trends for married women, we estimate 12-month event studies using births to married mothers as the outcome. Results are presented in Figure C2. The event study assuages the concern that the result for married women is driven by pre-existing trends, and provides striking visual evidence of an increase in births to this group immediately following the changes in access to abortion. It is also possible that the increase in births to married women results from an increase in marriages for women who have an unplanned pregnancy (a "shotgun" marriage), though we cannot distinguish between this channel and others in our data.

Together, these results paint a picture of the types of mothers who change their childbearing behavior in response to decreased access to abortion. There are several explanations for these findings. Relatively older, Hispanic, and married mothers are not necessarily more affected by the reduction in access to abortion clinics. That is, there is no reason to believe the cost of seeking an abortion is greater for these women compared to others. In fact, one could argue that older married women may be more likely to have the resources to travel to seek an abortion. However, it is possible that these women are more likely to be on the margin of the decision to have an abortion. If individuals view having a child as costly, then it is reasonable to argue that the marginal cost of a child is smaller for women who have a stable partner and may already have at least one child. It is also possible that the results are particularly strong for Hispanic mothers because abortion continues to be more taboo in this culture. According to a 2017 public opinion poll by the Pew Research Center, 50% of Hispanics hold the view that abortion should be illegal in most cases compared to 40% for non-Hispanic Whites and 34% for non-Hispanic Blacks.[48] When the driving distance to an abortion clinic increases, obtaining an abortion becomes more likely to be noticed by the woman's partner, friends, or family as it now may require a day or more away from one's home. Obtaining an abortion in secret may be particularly prevalent among women who are both Hispanic (because abortion is more taboo on average) and married (because being away from home is likely to be noticed). Supporting this notion is the fact that we find particularly large impacts

[48]http://www.pewforum.org/fact-sheet/public-opinion-on-abortion/

among married women who are Hispanic compared to married women who are not Hispanic.[49]

As an additional dimension of heterogeneity in the effects of access to abortion, we also consider counties near the border to either Mexico or other states. Results are presented in Table C2. In this table we examine three outcomes (abortions, births, and Hispanic births), where each measure of abortion access is also interacted with a variable indicating whether the county is on the Mexico border (columns 1-3) or the border of another state (columns 4-6). For counties near the Mexico border, the effects on both abortions and births are substantially stronger, though the birth effects are not statistically different from non-border counties. There are likely a number of factors that contribute to this finding. Regarding the effects on abortion, many of these women may have traveled to Mexico for misoprostol (an abortifacient available without prescription in Mexico); a behavior that may result in an abortion but is unobserved in our data. It is also the case that in the most populous affected border region (the Rio Grande Valley), the reduction in access was dramatic: the distance to the nearest clinic in Hidalgo county changed from 7 miles to 229 miles at the maximum. Moreover, traveling to the nearest abortion provider during the closures often required traveling through U.S. border patrol checkpoints. Given that border counties are predominantly Hispanic (88%), the cost of travel is greater for this group to the extent that a portion of this population is undocumented.

We also examine births among Hispanic women. The estimates in column 3, albeit imprecisely measured, show that the effects on Hispanic births are more similar in border versus non-border counties relative to the effects for all births. This suggests that the all-birth border result (column 2) may be driven by the fact that the population in border counties is overwhelmingly Hispanic. This finding is in line with the notion that the result is driven, at least in part, by a cultural norm among married Hispanic women and the added cost of travel for undocumented women.

For counties that border other states (columns 4-6), the effects on both abortions and births are slightly attenuated, though the differences are not statistically significant. This is consistent

[49]Using the 50-mile measure, our estimates indicate a 3.9% increase (p-value<0.001) among married Hispanic women and a 0.8% increase (p-value=0.390) among married non-Hispanic women. Using the 100 mile-measure, our estimates indicate a 4.1% increase (p-value<0.001) among married Hispanic women and a 2.8% increase (p-value=0.031) among non-Hispanic Married women.

with the idea that women living in counties that border other states are less affected by changes in access as they were already more likely to travel to other states for an abortion prior to any changes. When considering the Mexico border and other state border results, we find it reassuring that in locations where abortions are affected more (or less), births are also affected more (or less). This suggests that changes in the number of abortions are indeed the mechanism that leads to changes in the number of births.

Access to Family Planning Clinics

Similar to the main estimates for access to family planning, there is no statistically significant contemporaneous effect on births for any subgroup. As such, we focus on the 12-month lagged measure of access to family planning for the remainder of this section. We find no statistically significant impacts on any subgroup using the conservative Bonferroni (1935) corrected p-values, although some estimates are significant using traditional standard errors.

The age-specific estimates presented in Panel B of Table 6 reveal no statistically significant effects by age although the increase in births among teen mothers is substantially larger than the other groups at 2.7%. Packham (2017) estimates that the 2011 Texas family planning cuts led to teenage birth rates rising by 3.4% where the results are most concentrated 2 to 3 years after the cuts. Her estimates are not directly comparable to our own since her estimates capture the effect of the cuts rather than the effect of increased distance to a publicly funded provider.[50] However, it is reaffirming that our estimates for teenagers are of similar magnitude.

Similar to the abortion clinic results, Panel B of Table 7 presents evidence that reduced access to family planning services increases the number of births to mothers who already have children. However, in contrast to the heterogeneity associated with the abortion clinic results, Table 8 Panel B shows that the family planning results are most pronounced for non-Hispanic, unmarried, and low education mothers –these three estimates are significant at the 5% level using the uncorrected standard errors. The overall findings confirm what is known about access to publicly funded family

[50] Packham (2017) compares counties in Texas with at least one publicly funded clinic with counties outside of Texas.

planning services during this period. First, these clinics aim to serve young women (especially teens) and low income women (for which low education is a proxy), and thus it is unsurprising that the estimates are larger for teen and low education mothers. There is also reason to believe that women who already have children would be particularly impacted by these restrictions in access: the funding cuts in question dramatically hampered the ability of these clinics to offer LARCs.[51] Childless women are much less likely to use LARCs than women with children (Branum and Jones, 2015). As such, the decreased access to subsidized LARCs may have fallen heavily on women with children.

5.4 Outcome: Contraceptive Purchases

Increases to the cost of an abortion in the form of longer driving distances could raise emergency contraceptive demand to the extent that abortions and emergency contraceptives are close substitutes. However, given that emergency contraception is used after sexual activity and before a pregnancy is confirmed whereas an abortion occurs after pregnancy confirmation, the degree of substitutability of these two is arguable. If women respond to the changing environment by using contraceptives more regularly, condom and emergency contraceptive purchases could increase. Reducing sexual activity would be an even more forward-looking and cautious response, and in such case, one would expect to see drops in condom and emergency contraceptive purchases. Empirical support for such forward-looking behavior is limited.

The effects of changes in abortion access on condom or emergency contraceptive purchases are small at best. As presented in Panel A of Table 9, across all three outcomes and all three measures of abortion access, there are no statistically significant estimates. The most precisely estimated coefficients are those on condom expenditures, and for this outcome we can rule out positive effects exceeding 2.0%, 4.9% and 1.2% for the 25-, 50- and 100-mile measures, respectively. For emergency contraceptives, the upper bound of the 95% confidence intervals are 4.9%, 6.0%, and

[51] Stevenson et al. (2016) estimate that just one of the pieces of legislation in Texas that we examine (the changes to the Medicaid fee-for-service program) result in a 35.5% reduction in Medicaid claims for LARCs, and a 31.1% reduction in Medicaid claims for injectable contraceptives.

2.2% for the 25-, 50-, and 100-mile measures, respectively. Overall these results suggest little interaction between the market of abortion services and this set of contraceptive purchases, at least for the affected population.

In the remainder of Table 9 (Panel B), we consider the effects of family planning access. The estimates of Stevenson et al. (2016) highlight the clinical response to changes in family planning access. Using Medicaid claims data, Stevenson et al. (2016) estimate that the removal of funding for Planned Parenthood from the Texas Medicaid family planning program reduced claims for both LARCs and injectable contraceptives by 30% in areas with Planned Parenthood affiliates relative to areas without such affiliates. Thus, to the extent that condoms and emergency contraceptives are substitutes – albeit imperfect and perhaps second best for those customers who lose access – it is reasonable to expect a rise in condom or emergency contraceptive purchases. Income constraints might inhibit such a response but relative to LARCs, condoms and emergency contraceptives are less costly. Alternatively, if women are forward-looking and take into account the reduced access to reproductive services, conceptions may fall, decreasing the demand for emergency contraceptives. Declines in conceptions attributable to reduced sexual activity would lessen the demand for condoms.

Despite the substantial drop in more long-term contraceptives, our estimates suggest little compensatory behavior in the form of condom and emergency contraceptive purchases. Specifically, across all three outcomes, we find no evidence that either contemporaneous or lagged access to family planning leads to a change in OTC contraceptive use. For condom purchases, our effects rule out responses exceeding 1.1% and 8.1% for contemporaneous and lagged family planning access, respectively. For emergency contraceptive purchases, the upper bounds of the 95% confidence intervals are 8.3% and 7.2%, respectively. In Appendix D, Panels C and D of Table D1 show that these estimates hold for different store types and specifications. Given the Stevenson et al. (2016) results, we would expect to find larger responses on the margin of condom and emergency contraceptive purchases if a large cross-price elasticity of demand between our contraceptive measure and long-acting contraceptives exists.

6 Discussion and Conclusion

In recent years, there has been much debate about access and public funding of reproductive services. For example, under the Affordable Care Act (the ACA), states have the option of expanding their state's Medicaid coverage including family planning coverage. However, one of the main tenets of support for repealing the ACA involves halting these expansions and restricting the funding of reproductive services. In this paper, we attempt to understand how restrictions in access to both abortion and family planning providers affect abortions, births, and contraceptive purchases. We leverage changes in the market for reproductive services in Texas, which are similar to those being discussed nationally. In response to an abortion clinic closure within 50 miles, births rose 1.3% whereas the birth effects of no longer having a publicly funded family planning clinic within 25 miles were 1.2%. A 1% increase in births is comparable to the effects of an abortion clinic act of violence on births (Jacobson and Royer, 2011). Contraceptive purchases exhibited little compensatory behavior - suggesting that retail contraceptive purchases were not a good substitute for clinical reproductive services. Back-of-the-envelope calculations imply that by the end of 2015, the abortion clinic restrictions led to 1,570 additional births and the changes in funding to family planning clinics increased births by 929. These calculations, however, miss other important costs of reduced access - most importantly, the increased travel cost for women seeking abortion or family planning services.

Our analysis is a case study of one state. Thus, generalizing these findings to other settings is challenging. However, given the resemblance of current debated legislation at the federal level with the policies already implemented in Texas, we view our analysis as providing a useful benchmark of the potential impacts of recent proposed policies.

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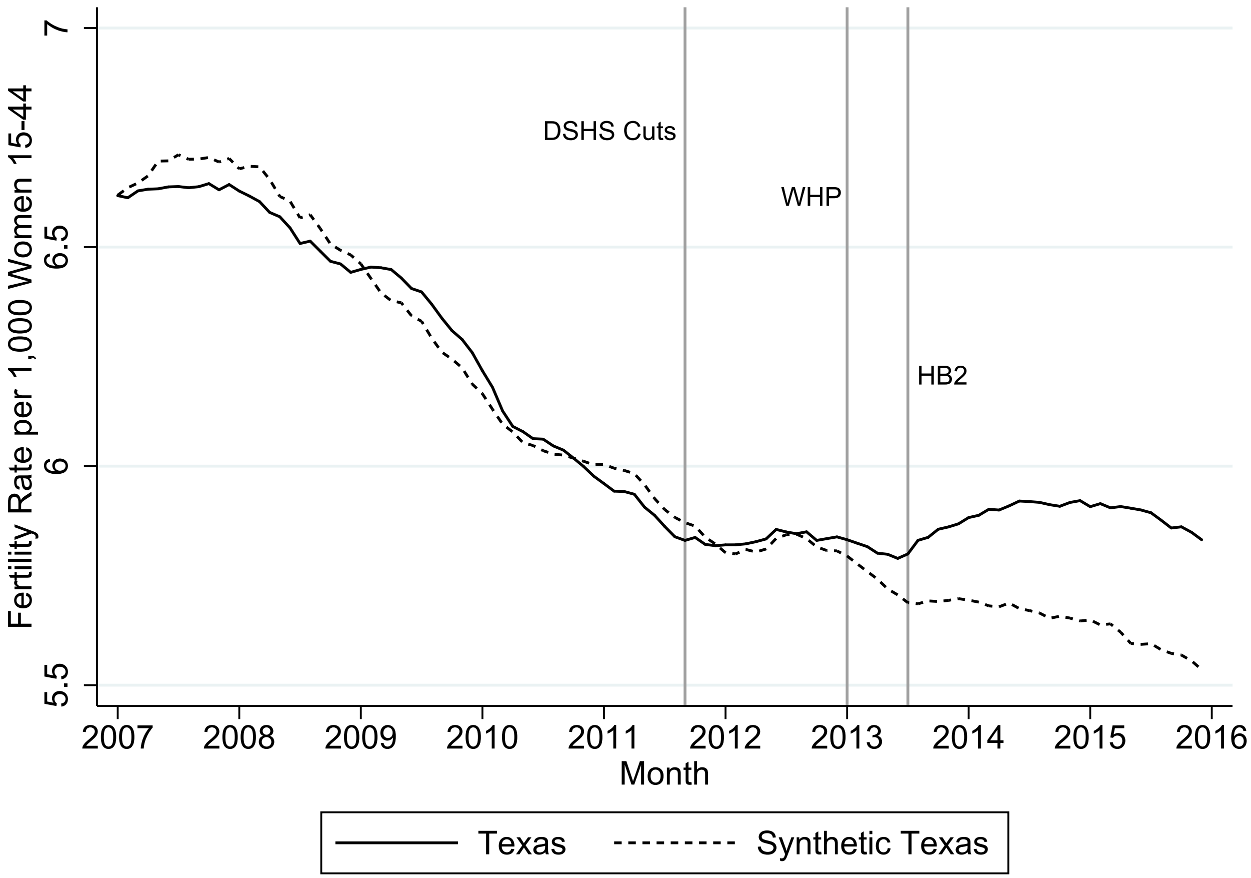
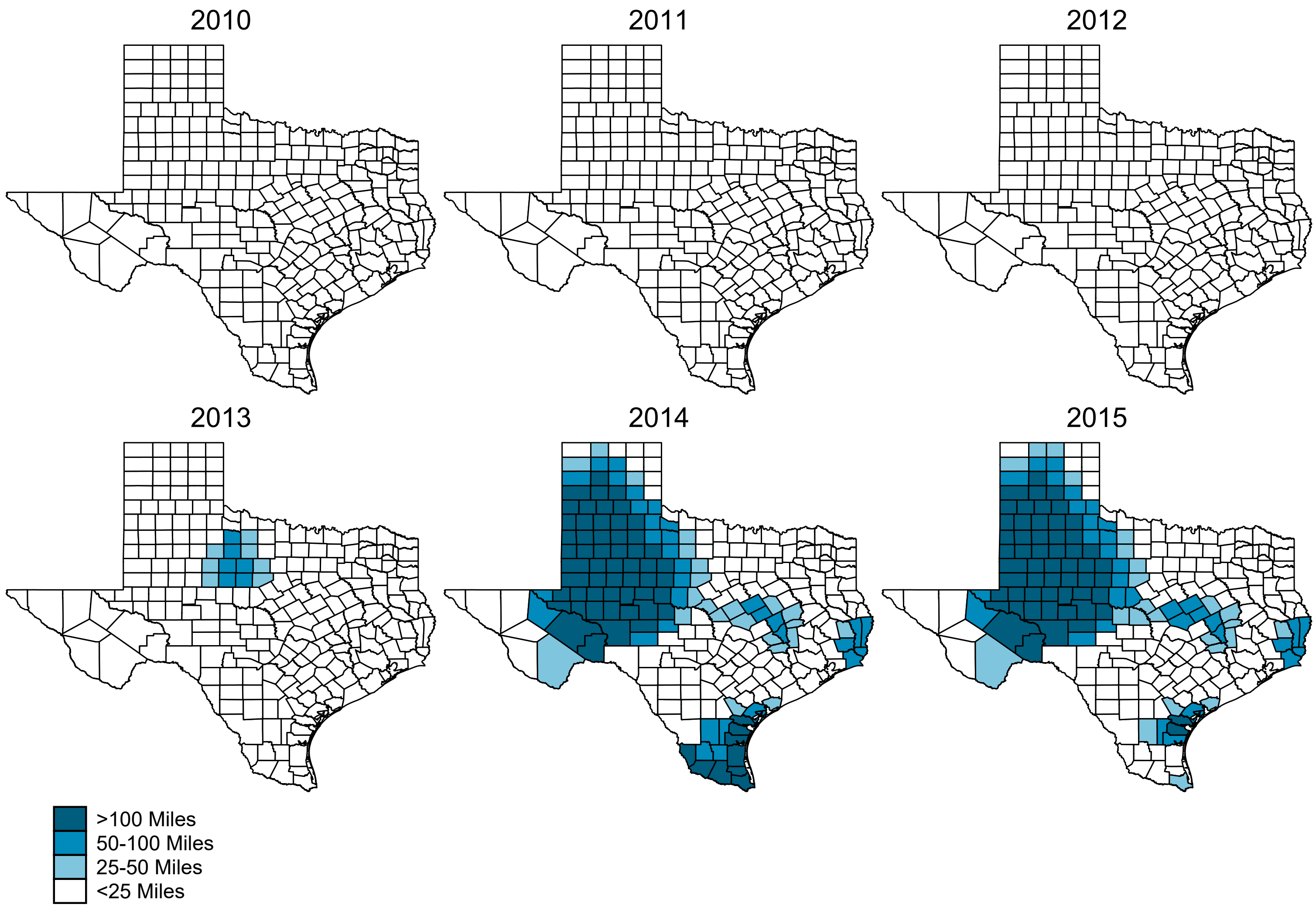
Figure 1: Statewide – Synthetic Control



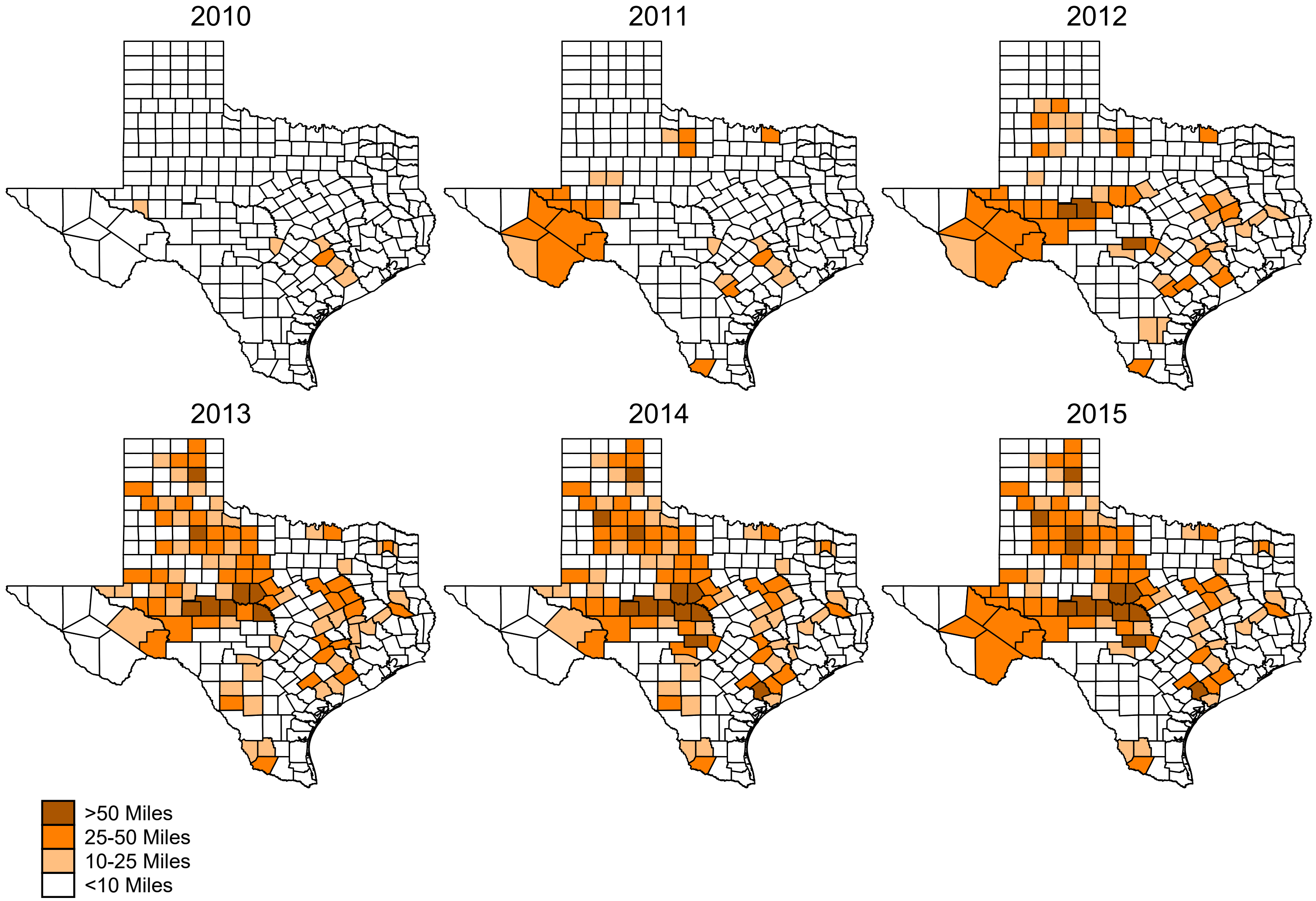
Notes: Synthetic Texas is constructed by matching on the following: fertility rates (the outcome) prior to September 2011 (i.e., the date of TDSHS cuts), the proportion of the population Hispanic and the proportion of the population of childbearing age (15-4T4) both measured at the 2010 census. The potential donor pool consists of all other U.S. states, and the matching procedure results in the selection of three donor states (California: 28.4%; New Mexico: 47.9%; Utah: 23.7%). The synthetic control and all resulting estimates are constructed using monthly data; the plot uses 12-month moving averages to smooth seasonal variation in fertility rates. A regression of treatment-control differences on a post treatment indicator reveals an increase in the monthly fertility rate of 0.170 (Donald and Lang (2007) standard error of 0.040). This represents a 2.8% effect relative to the mean monthly fertility rate in Texas of 6.12. Following Abadie et al. (2010), we also estimate placebo synthetic control estimates for all 50 other U.S. states (including D.C.), and find that only two states (North Dakota and South Dakota) return positive estimates larger than the Texas estimate. The large estimates for the Dakotas result largely because of poor fit on the pre-treatment trends in the outcome due to the fact that these were among the only states with increasing fertility rates over the sample period.

Figure 2: Change in Access to Abortion Clinics



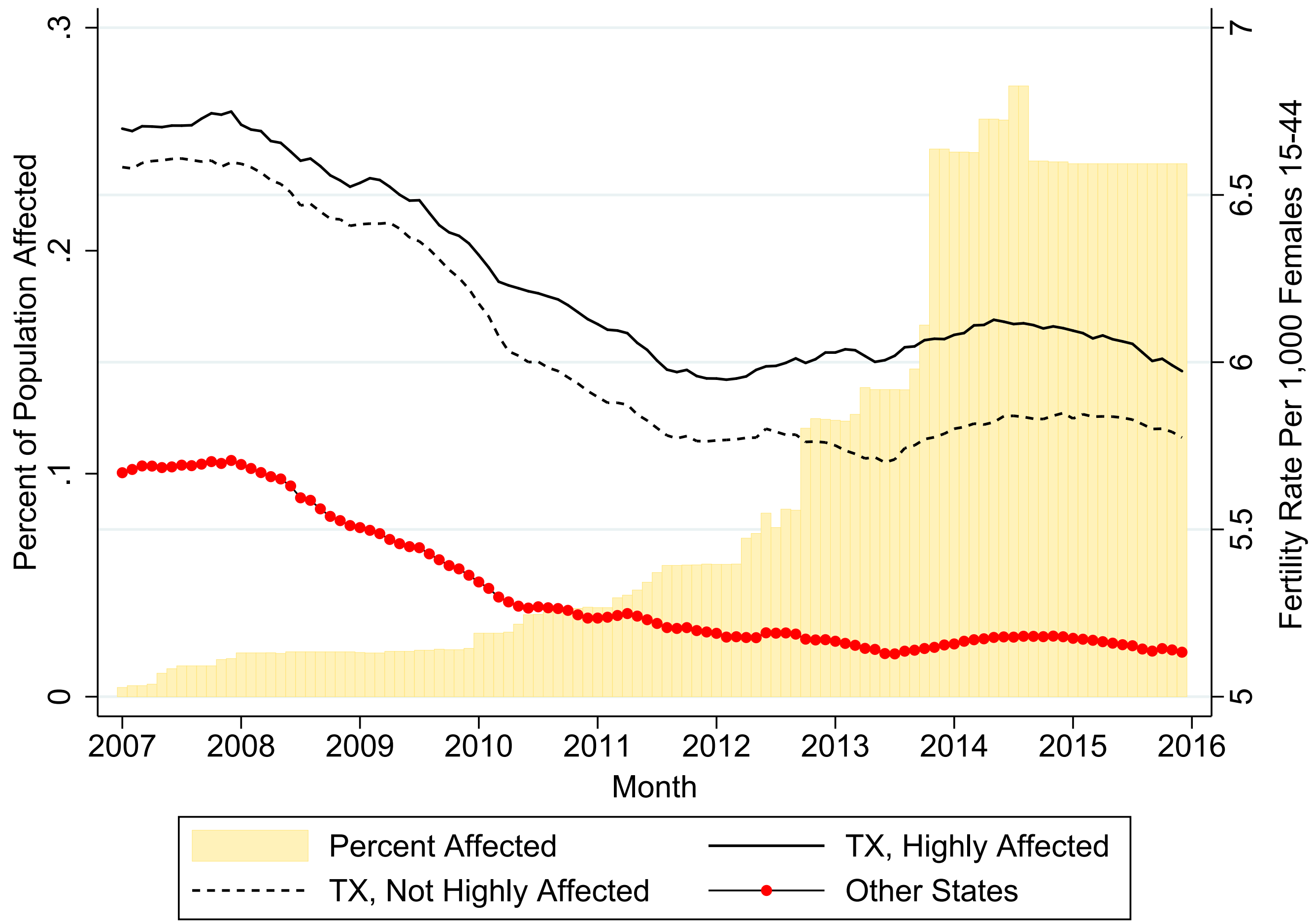
Notes: Change in access is defined as the change in distance between July of the year in question and July of 2010. Distances are measured as miles driving from each county's population-weighted centroid to the nearest clinic that provides abortion services.

Figure 3: Change in Access to publicly funded Family Planning Clinics



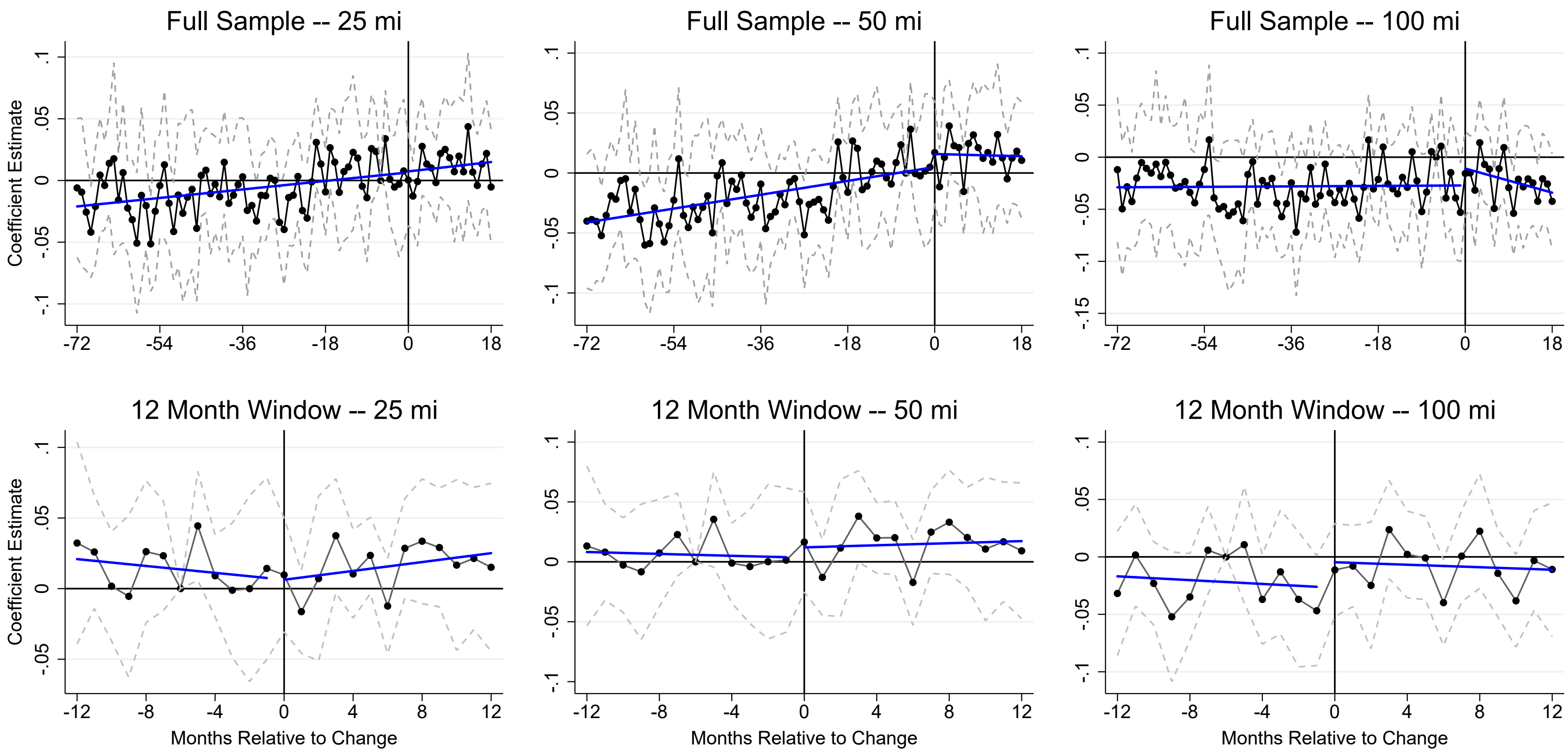
Notes: Change in access is defined as the change in distance between the year in question and 2010. Distances are measured as miles driving from each county's population-weighted centroid to the nearest publicly funded clinic that provides family planning services.

Figure 4: Within State – Natality Trends



Notes: Highly Affected counties in Texas are defined as those that ever experienced either a 25 mile increase in driving distance to the nearest abortion clinic or a 10 mile increase in driving distance to the nearest publicly funded family planning clinic between the start of the sample in January 2006 and the end of the sample in December 2015. Percent affected represents the proportion of the Texas population in each period that experienced either such increase in driving distance between the start of the sample and the corresponding period. The plot uses 12-month moving averages to smooth seasonal variation in fertility rates.

Figure 5: Event Studies – Effect of Abortion Access on Births



Notes: The model used to estimate the event studies is a Fixed Effects Poisson which is equivalent to the model used to produce the main estimates, except that instead of a single treatment variable, there are multiple treatment variables corresponding to months relative to the event. The event is defined as the first month in which a county switched from having a clinic to not having a clinic within the corresponding distance. In other words, $\beta_1 Access_{ct}$ from Equation 1 is replaced with $\sum_{k=-6}^{-T_{pre}} \theta_k A_{ctk} + \sum_{k=1}^{T_{post}} \theta_k A_{ctk}$. A_{ctk} is an indicator equal to 1 for k months from the event for county c, where the estimates of θ_k are the coefficients of interest. The period six months prior the event is omitted as it is the reference group. The blue lines represent linear trends through the estimates and are intended to provided a visual aid illustrating the existence of any differential trends in the period prior to the event.

Figure 6: Out of State Abortion Rates by State

Notes: The data come from each state's Department of Health. Abortion rates in Texas are constructed as rates per 1,000 women aged 15-44. We have also collected data on out-of-state abortions for the state of Louisiana, however these data are preliminary and are not consistently reported throughout this time period. That said, the data we received from Louisiana for 2012-2015 includes the exact state of residence, and indicate a 31.5% increase in the number of abortions in Louisiana for Texas residents between 2012 and the average over 2013-2015.

Table 1: Summary Statistics

	Mean	Std. Dev.
Panel A: Abortion Data		
Abortion rate (all ages)	12.60	(5.50)
Panel B: Natality Data		
Fertility rate (all ages)	72.8	(10.5)
Fertility rate 15-19	47.7	(18.4)
Fertility rate 20-29	116.3	(25.1)
Fertility rate 30-39	71.3	(11.1)
Fertility rate 40-44	9.85	(2.87)
Mothers Hispanic (%)	0.46	(0.21)
Mothers high school or less (%)	0.50	(0.12)
Mothers unmarried (%)	0.41	(0.08)
First child (%)	0.39	(0.03)
Second child (%)	0.31	(0.02)
Third child (%)	0.18	(0.02)
Panel C: Nielsen Data (store level)		
Total contraceptive expenditures ($)	28,816	(21,517)
Condom expenditures ($)	10,924	(7,372)
Emergency contraceptive doses	518.8	(443.1)
Panel D: Treatments		
Driving dist. nearest abortion clinic (mi)	26.2	(44.7)
2006 pop. with no abortion clinic in 25mi (%)	0.26	
2006 pop. with no abortion clinic in 50mi (%)	0.15	
2006 pop. with no abortion clinic in 100mi (%)	0.07	
2015 pop. with no abortion clinic in 25mi (%)	0.37	
2015 pop. with no abortion clinic in 50mi (%)	0.24	
2015 pop. with no abortion clinic in 100mi (%)	0.12	
Driving dist. nearest funded FP clinic (mi)	7.7	(11.5)
2006 pop. with no funded FP clinic in 10mi (%)	0.12	
2006 pop. with no funded FP clinic in 25mi (%)	0.05	
2015 pop. with no funded FP clinic in 10mi (%)	0.21	
2015 pop. with no funded FP clinic in 25mi (%)	0.11	

Notes: All summary statistics are aggregated to the annual level (e.g., annual fertility rates and annual condom expenditures). There are 2,530 observations for the abortion and natality data, except for the birth composition variables which only have 2,528 observations due to missing data. Abortion rates and fertility rates are calculated as per 1,000 in the relevant age group (the relevant age group for the all-age rate is 15-44). Panel C reports summary statistics for contraceptive purchases from drug stores, and there are 5,855 store-by-year observations. In Panel D, 2015 pop. with no clinic reports the percentage of the population that has no clinic of either type within the corresponding driving distance. All county-level means are weighted by population; the store-level means for the Nielsen data are unweighted.

Table 2: Pre-Trends Test: Δ Past Fertility Rate (2006-2011) on Δ Future Clinic Access (2011+)

Panel A: Abortion Access				
	Max. Δ Dist. (Jan. 2011+)	Δ-No Clinics 25 mi	Δ-No Clinics 50 mi	Δ-No Clinics 100 mi
Δ Fertility Rate	-0.003	-0.002	-0.009	-0.004
	(4.248)	(0.002)	(0.008)	(0.013)

Panel B: Family Planning Access			
	Max. Δ Dist. (Jan. 2011+)	Δ-No Clinics 10 mi	Δ-No Clinics 25 mi
Δ Fertility Rate	-0.690	0.006	-0.005
	(1.331)	(0.008)	(0.010)

Notes: This table tests whether pre-treatment changes in the outcome predict subsequent changes in treatment status. The analysis is at the county level and includes 253 observations. Each estimate comes from a separate regression. The regressor in all specifications is the change in the mean annual all-age fertility rate between 2006 and 2010. The standard deviation for the regressor is 1.1, meaning that the marginal effects presented here can be roughly interpreted as the impacts of a one standard deviation increase in the regressor. The outcomes are various measures of the treatment. In the first column of each panel, the outcome is the change in driving distance between January 2011 and the maximum observed driving distance for that county post-January 2011. In the following columns, the outcomes represent whether our measures of access changed between Jan. 2011 and the end of the sample. For example, Δ-No Clinics 50 mi is an indicator equal to one if a county changed from having at least one clinic within 50 miles to having none. Robust standard errors are reported in parentheses.

Table 3: Access to Abortion & Family Planning Clinics on Number of Abortions (Poisson)

	(1)	(2)	(3)
Panel A: Abortion Access			
No Clinics 25 mi	-0.219***	-0.209***	-0.166***
	(0.059)	(0.049)	(0.039)
No Clinics 50 mi	-0.224***	-0.218***	-0.167***
	(0.068)	(0.060)	(0.053)
No Clinics 100 mi	-0.359***	-0.327***	-0.221***
	(0.084)	(0.072)	(0.082)
Panel B: Family Planning Access			
No Clinics 10 mi	0.020	0.035*	0.035*
	(0.019)	(0.020)	(0.018)
No Clinics 25 mi	-0.005	-0.008	-0.003
	(0.028)	(0.029)	(0.027)
Time-Varying Controls	-	X	X
HHS Trends	-	-	X
Observations	2,530	2,530	2,530

Notes: The analysis is at the county-year level, and the coefficients represent estimates from a Fixed Effects Poisson model with the number of abortions in each category as the outcome. The exposure variable is the population of females 15-44 years old. Each estimated coefficient comes from a separate regression and the treatment variables of interest are dummy variables indicating that there were no clinics (abortion or publicly funded family planning) in the relevant driving distance. All regressions include county and year fixed effects. Time-varying controls are the unemployment rate, log per capita income, age- and race-specific populations, the distance to the nearest family planning clinic in the abortion regressions, and distance to the nearest abortion clinic in the family planning regressions. HHS trends represent HHS region-specific linear time trends. Standard errors are reported in parentheses and are clustered at the county level.

Table 4: Access to Abortion on Number of Births (Poisson)

	Full Sample			2009+	2012+	Nov. 2012+
	(1)	(2)	(3)	(4)	(5)	(6)
No Clinics 25 mi	0.011	0.014	0.016**	0.010	0.007	0.007
	(0.012)	(0.009)	(0.007)	(0.007)	(0.008)	(0.009)
No Clinics 50 mi	0.024**	0.027***	0.028***	0.023***	0.013**	0.014**
	(0.009)	(0.008)	(0.007)	(0.006)	(0.007)	(0.007)
No Clinics 100 mi	0.015	0.018**	0.017***	0.018***	0.017**	0.019***
	(0.010)	(0.009)	(0.006)	(0.006)	(0.007)	(0.007)
Time-varying Controls	-	X	X	X	X	X
HHS Trends	-	-	X	X	X	X
Observations	30,360	30,360	30,360	21,252	12,144	9,614

Notes: The analysis is at the county-year-month level, and the coefficients represent estimates from a Fixed Effects Poisson model with the number of births in each category as the outcome. The exposure variable is the population of females 15-44 years old. Each estimated coefficient comes from a separate regression, and the treatment variables of interest are dummy variables indicating that there were no abortion clinics in the relevant driving distance. All regressions include county and year-by-month fixed effects. Time-varying controls are the unemployment rate, log per capita income, age- and race-specific populations, and the distance to the nearest family planning clinic. HHS trends represent HHS region-specific linear time trends. Standard errors are reported in parentheses and are clustered at the county level.

Table 5: Access to Family Planning on Number of Births (Poisson)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Contemporaneous Impacts						
No Clinics 10 mi	0.005	-0.009	-0.008			
	(0.012)	(0.007)	(0.007)			
No Clinics 25 mi	0.017**	-0.002	-0.003			
	(0.008)	(0.008)	(0.008)			
Panel B: Dynamic Impacts						
No Clinics 25 mi (t=0)				-0.011	-0.009	-0.012
				(0.008)	(0.008)	(0.008)
No Clinics 25 mi (t-12)				0.012**	0.013**	0.013**
				(0.006)	(0.006)	(0.007)
No Clinics 25 mi (t-24)					-0.001	-0.001
					(0.006)	(0.007)
No Clinics 25 mi (t-36)						-0.001
						(0.008)
Time-varying Controls	-	X	X	X	X	X
HHS Trends	-	-	X	X	X	X
Observations	30,360	30,360	30,360	27,324	24,288	21,252

Notes: The analysis is at the county-year-month level, and the coefficients represent estimates from a Fixed Effects Poisson model with the number of births in each category as the outcome. The exposure variable is the population of females 15-44 years old. In Panel A, each coefficient comes from a separate regression. In Panel B, each column is a separate regression. The treatment variables are dummy variables indicating that there were no family planning clinics in the relevant driving distance at the time of conception (t=0), a year prior to conception (t-12), and so on. All regressions include county and year-by-month fixed effects. Time-varying controls are the unemployment rate, log per capita income, age- and race-specific populations, and the distance to the nearest abortion clinic. HHS trends represent HHS region-specific linear time trends. Standard errors are reported in parentheses and are clustered at the county level.

Table 6: Impacts on Number of Births by Age (Poisson)

	15-19	20-29	30-39	40-44
Panel A: Abortion Access				
No Clinics 25 mi	-0.005	0.009	0.014	0.040
	(0.013)	(0.009)	(0.010)	(0.030)
	[1.000]	[1.000]	[1.000]	[1.000]
No Clinics 50 mi	0.011	0.009	0.028***	0.085***
	(0.012)	(0.008)	(0.008)	(0.029)
	[1.000]	[1.000]	[0.003]	[0.039]
No Clinics 100 mi	0.013	0.011	0.031***	0.096***
	(0.013)	(0.009)	(0.008)	(0.034)
	[1.000]	[1.000]	[0.003]	[0.074]
Observations	11,952	12,144	12,144	11,808
Panel B: Family Planning Access				
No Clinics 25 mi (t=0)	-0.016	-0.003	-0.003	0.032
	(0.017)	(0.009)	(0.014)	(0.042)
	[1.000]	[1.000]	[1.000]	[1.000]
No Clinics 25 mi (t-12)	0.027	0.001	0.010	0.011
	(0.017)	(0.007)	(0.013)	(0.042)
	[1.000]	[1.000]	[1.000]	[1.000]
Observations	27,324	27,324	27,324	27,108
Time-Varying Controls	X	X	X	X
HHS Trends	X	X	X	X
Mean Fertility Rate	45.8	224.9	132.4	9.2

Notes: The analysis is at the county-year-month level, and the coefficients represent estimates from a Fixed Effects Poisson model with the number of births in each category as the outcome. The exposure variable is the population of females in the corresponding age group. The mean fertility rate is included in this table as an indication of the number of births within each group that are used to identify the impacts; births in the 40-44 age group are relatively rare. In Panel A, each estimate comes from a separate regression; in Panel B, each column is a separate regression. The treatment variables of interest are dummy variables indicating that there were no clinics (abortion or publicly funded family planning) in the relevant driving distance. Sample sizes vary across panels due to the limited sample for abortion access and the lagged measure of family planning access. Sample sizes vary within panels because counties in which the outcome is equal to zero in all periods are dropped from the regression. Standard errors are reported in parentheses and are clustered at the county level. Bonferroni p-values that adjust for multiple hypothesis testing across many subgroups are reported in brackets; for each measure of access, p-values are adjusted for the 14 subgroups analyzed in Tables 6 to 8.

Table 7: Impacts on Number of Births by Parity (Poisson)

	First	Second	Third	Fourth or More
Panel A: Abortion Access				
No Clinics 25 mi	0.001	0.005	0.018*	0.010
	(0.011)	(0.010)	(0.010)	(0.013)
	[1.000]	[1.000]	[1.000]	[1.000]
No Clinics 50 mi	0.001	0.018*	0.017	0.031***
	(0.011)	(0.010)	(0.011)	(0.011)
	[1.000]	[0.968]	[1.000]	[0.076]
No Clinics 100 mi	0.021*	0.010	0.016	0.020*
	(0.011)	(0.008)	(0.012)	(0.012)
	[0.747]	[1.000]	[1.000]	[1.000]
Observations	12,144	12,144	12,144	12,144
Panel B: Family Planning Access				
No Clinics 25 mi (t=0)	-0.008	-0.010	-0.015	-0.016
	(0.013)	(0.011)	(0.016)	(0.019)
	[1.000]	[1.000]	[1.000]	[1.000]
No Clinics 25 mi (t-12)	-0.003	0.007	0.029**	0.039**
	(0.014)	(0.010)	(0.014)	(0.019)
	[1.000]	[1.000]	[0.571]	[0.610]
Observations	27,324	27,324	27,324	27,324
Time-Varying Controls	X	X	X	X
HHS Trends	X	X	X	X

Notes: The analysis is at the county-year-month level, and the coefficients represent estimates from a Fixed Effects Poisson model with the number of births in each category as the outcome. The exposure variable is the population of females 15-44 years old. In Panel A, each estimate comes from a separate regression; in Panel B, each column is a separate regression. The treatment variables of interest are dummy variables indicating that there were no clinics (abortion or publicly funded family planning) in the relevant driving distance. Sample sizes vary across panels due to the limited sample for abortion access and the lagged measure of family planning access. Standard errors are reported in parentheses and are clustered at the county level. Bonferroni p-values that adjust for multiple hypothesis testing across many subgroups are reported in brackets; for each measure of access, p-values are adjusted for the 14 subgroups analyzed in Tables 6 to 8.

Table 8: Impacts on Number of Births by Mother Characteristics (Poisson)

	Non-Hispanic	Hispanic	Low Edu.	High Edu.	Married	Unmarried
Panel A: Abortion Access						
No Clinics 25 mi	-0.017*	0.019**	-0.005	0.013	0.010	0.003
	(0.009)	(0.007)	(0.015)	(0.015)	(0.010)	(0.011)
	[0.921]	[0.163]	[1.000]	[1.000]	[1.000]	[1.000]
No Clinics 50 mi	-0.002	0.019**	0.000	0.022*	0.028***	-0.005
	(0.008)	(0.008)	(0.011)	(0.012)	(0.009)	(0.014)
	[1.000]	[0.035]	[1.000]	[1.000]	[0.014]	[1.000]
No Clinics 100 mi	0.005	0.015*	0.002	0.035**	0.042***	-0.011
	(0.013)	(0.009)	(0.013)	(0.017)	(0.008)	(0.016)
	[1.000]	[1.000]	[1.000]	[0.500]	[0.000]	[1.000]
Observations	12,144	12,144	12,144	12,144	12,144	12,144
Panel B: Family Planning Access						
No Clinics 25 mi (t=0)	-0.011	-0.008	-0.006	-0.010	-0.001	-0.023*
	(0.009)	(0.011)	(0.013)	(0.010)	(0.009)	(0.012)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[0.896]
No Clinics 25 mi (t-12)	0.019**	0.001	0.020**	0.007	0.004	0.023**
	(0.008)	(0.010)	(0.010)	(0.009)	(0.008)	(0.010)
	[0.228]	[1.000]	[0.558]	[1.000]	[1.000]	[0.215]
Observations	27,324	27,324	27,324	27,324	27,324	27,324
Time-Varying Controls	X	X	X	X		
HHS Trends	X	X	X	X	X	X

Notes: The analysis is at the county-year-month level, and the coefficients represent estimates from a Fixed Effects Poisson model with the number of births in each category as the outcome. The exposure variable is the population of females 15-44 years old. In Panel A, each estimate comes from a separate regression; in Panel B, each column is a separate regression. The treatment variables of interest are dummy variables indicating that there were no clinics (abortion or publicly funded family planning) in the relevant driving distance. Low Edu. indicates a high school degree or less; High Edu. indicates some college or more. Sample sizes vary across panels due to the limited sample for abortion access and the lagged measure of family planning access. Standard errors are reported in parentheses and are clustered at the county level. Bonferroni p-values that adjust for multiple hypothesis testing across many subgroups are reported in brackets; for each measure of access, p-values are adjusted for the 14 subgroups analyzed in Tables 6 to 8.

Table 9: Contraceptive Purchases

	ln(Total Cont. Exp.)	ln(Condom Exp.)	Poisson(EC Doses)
Panel A: Abortion Access			
No Clinics 25 mi	-0.032	-0.023	-0.020
	(0.034)	(0.022)	(0.035)
No Clinics 50 mi	0.019	0.004	-0.010
	(0.037)	(0.023)	(0.036)
No Clinics 100 mi	-0.033	-0.039	-0.042
	(0.033)	(0.026)	(0.033)
Observations	69,033	69,033	68,993
Panel B: Family Planning Access			
No Clinics 25 mi (t=0)	-0.001	-0.032	-0.007
	(0.034)	(0.022)	(0.046)
No Clinics 25 mi (t-12)	-0.025	0.032	-0.016
	(0.037)	(0.025)	(0.045)
Observations	69,014	69,014	68,974
Economic Controls	X	X	X
Demographic Controls	X	X	X
Access Controls	X	X	X
HHS Trends	X	X	X

Notes: The analysis is at the store-year-month level, and the sample includes drug stores only. The first two columns represent the natural log of total contraceptive expenditures (condoms plus ECs) and the natural log of condom expenditures, respectively. These regressions are estimated via OLS and the standard errors are clustered at the county level. There are 16 observations with zero total contraceptive expenditures and 29 observations with zero condom expenditures; to ensure these are included in the log specification, these are replaced with $1 in expenditures. The third column represents the number of EC doses purchased (EC pills), and is estimated via Fixed Effects Poisson. The Poisson model is estimated with store fixed effects and standard errors are clustered at the store level. The treatment variables of interest are dummy variables indicating that there were no clinics (abortion or publicly funded family planning) in the relevant driving distance. In Panel A, each estimate comes from a separate regression; in Panel B, each column is a separate regression. Sample sizes are smaller for EC doses because the Poisson specification omits stores if the outcome is equal to zero in all periods; sample sizes vary across panels because the lagged measure of family planning access is missing for new stores.

Appendix A: Abortion Clinic Coding

Discrepancies with Cunningham et al. (2018)

Beyond differences in our empirical approach, which are outlined in Section 5.2, there are several distinctions between the dataset used in our analysis and that used in Cunningham et al. (2018) (henceforth CLMS). First, our sample period begins in 2006 and ends in 2016, compared to CLMS which ends in 2015. Given the already short post period, our additional year of data approximately doubles the post period as it captures conceptions through 2015. Second, we aggregate natality data to the monthly level while CLMS aggregates to the yearly level. Since both clinic access and births are measured monthly, monthly aggregation affords a more precise match between treatment and outcome timing. For instance, this approach allows for the assignment of abortion access at the 13th week of gestation, as that is likely the approximate time when such a decision is made. Another benefit of monthly aggregation is that it captures variation in access driven by closures and reopenings within a year. This is particularly important when assigning access to Whole Women's Health McAllen which was forced to close on November 1, 2013 and resumed services in September 2014 after a ruling by the Fifth Circuit Court of Appeals. During the 10 month closure the nearest clinic for McAllen residents was over 200 miles away.

Finally, there are some discrepancies between our coding of abortion clinic access and the coding of CLMS. We outline these in detail below. As a robustness check, in Table A2 we report our main estimates using our empirical approach and CLMS's coding of abortion access. Comparing this table with Table 4 confirms that our results are not sensitive to the differences in dates coded.

Table A3 lists abortion clinics in Texas and nearby states along with their corresponding dates of operation. All dates come from the following sources: the TDSHS clinic license database, records from the non-profit Fund Texas Choice, directly contacting clinics, and from press releases and newspaper articles.[52] Table A3 also reports any differences between our coding in access and the dates CLMS code, where we flag clinics if there is more than a 30 day difference in the date. Because of the way the treatment is coded, there are only two discrepancies that affect the coding of the treatment (Trust Women South Wind Women's Center in Oklahoma City and the Fort Worth Planned Parenthood). This is because nearly all of the discrepancies occur in cities in which at least one clinic was always operational. In these cases the closure of one clinic in a city does not affect our treatment variables which are defined as distance to the nearest clinic.

[52] All referenced press releases and newspaper articles are available upon request.

Table A1: Access to Abortion on Number of Abortions (Poisson) – CLMS Dates

	(1)	(2)	(3)
No Clinics 25 mi	-0.211***	-0.198***	-0.153***
	(0.060)	(0.049)	(0.044)
No Clinics 50 mi	-0.226***	-0.222***	-0.176***
	(0.067)	(0.058)	(0.053)
No Clinics 100 mi	-0.357***	-0.326***	-0.220***
	(0.084)	(0.071)	(0.082)
Time-Varying Controls	-	X	X
HHS Trends	-	-	X
Observations	2,530	2,530	2,530

Notes: The analysis replicates the analysis presented in Table 3, but uses dates of operation for abortion clinics as defined in Cunningham et al. (2018).

Table A2: Access to Abortion on Number of Births (Poisson) – CLMS Dates

	Full Sample			2009+	2012+	Nov. 2012+
	(1)	(2)	(3)	(4)	(5)	(6)
No Clinics 25 mi	0.009	0.014	0.016**	0.011	0.007	0.006
	(0.012)	(0.010)	(0.007)	(0.007)	(0.008)	(0.008)
No Clinics 50 mi	0.025***	0.027***	0.029***	0.023***	0.014**	0.015**
	(0.009)	(0.008)	(0.007)	(0.006)	(0.007)	(0.007)
No Clinics 100 mi	0.015	0.017**	0.016**	0.017***	0.015**	0.016**
	(0.010)	(0.009)	(0.006)	(0.006)	(0.007)	(0.007)
Time-varying Controls	-	X	X	X	X	X
HHS Trends	-	-	X	X	X	X
Observations	30,360	30,360	30,360	21,252	12,144	9,614

Notes: The analysis replicates the analysis presented in Table 4, but uses dates of operation for abortion clinics as defined in Cunningham et al. (2018).

Table A3: Dates of Abortion Clinic Operations in Texas and Nearby States

Clinic	Location	Dates providing abortion services
Planned Parenthood Choice	Abilene, TX	11/10/2008-11/6/2012
Austin Women's Health Center	Austin, TX	10/1/2002-present
International Healthcare Solutions	Austin, TX	10/19/2006-8/31/2014
Planned Parenthood South Austin Clinic	Austin, TX	6/7/2004-present
Reproductive Services[1]	Austin, TX	3/6/2001-4/17/2008
Whole Woman's Health Austin[2]	Austin, TX	2/12/2003-11/10/2014, 4/28/2017-present
Whole Woman's Health Beaumont	Beaumont, TX	11/10/2004-3/11/2014
Planned Parenthood Bryan	Bryan, TX	3/14/2001-9/24/2013
Coastal Birth Control Center	Corpus Christi, TX	10/17/2001-6/6/2014
Abortion Advantage[3]	Dallas, TX	12/12/2001-11/1/2013, 2/12/2014- 6/12/2015
Fairmount Center[4]	Dallas, TX	5/7/2001-12/7/2009
North Park Medical Group	Dallas, TX	6/28/2001-11/1/2013, 2/2017-present)
Planned Parenthood of Greater Texas (formerly PP South Dallas)	Dallas, TX	8/1/2001-present
Routh Street Women's Clinic	Dallas, TX	6/20/2001-6/12/2015
Southwestern Women's Surgery Center[5]	Dallas, TX	12/7/2009-present
Hilltop Women's Reproductive Center	El Paso, TX	12/4/2001-present
Reproductive Services[6]	El Paso, TX	2/5/2001-11/10/2014, 9/24/2015-present
Planned Parenthood Greater Texas Fort Worth/PP Southwest Fort Worth (formerly on Henderson St.)[7]	Fort Worth, TX	3/31/2000-11/2013, 12/2013-present
Trinity Valley Women's Clinic (reopens as Whole Woman's Health)[8]	Fort Worth, TX	5/31/2001-10/13/2009
West Side Clinic	Fort Worth, TX	3/28/2001-11/4/2013
Whole Woman's Health Fort Worth[9]	Fort Worth, TX	10/13/2009-11/1/2013, 11/27/2013-present
Reproductive Services (Harlingen Reproductive)	Harlingen, TX	3/6/2001-11/1/2013
A Affordable Women's Medical Center	Houston, TX	7/28/2005-2/14/20014
AAA Concerned Women's Center[10]	Houston, TX	4/2/2001-11/12/2014
Aalto Women's Clinic[11]	Houston, TX	6/4/2001-2/1/2014
Aaron's Women's Center/Women's Pavilion	Houston, TX	7/19/2001-8/7/2014
Americas Women's Clinic[12]	Houston, TX	3/20/2001-8/16/2006
Crescent City Women's Center	Houston, TX	12/19/2003-12/30/2011
Houston Women's Clinic	Houston, TX	6/6/2001-present
Planned Parenthood of Gulf Coast (Fannin & Gulf Freeway)[13]	Houston, TX	4/3/2001-present

Notes: Dates come from the following sources: Texas DSHS (TDSHS) clinic license database, records from the non-profit Fund Texas Choice, directly contacting clinics, and from press releases and newspaper articles.

Dates of Abortion Clinic Operations in Texas and Nearby States (Continued)

Clinic	Location	Dates providing abortion services
Suburban Women's Clinic	Houston, TX	2/18/2007-present
Suburban Women's Medical Center (formerly Women's Surgical Center of NW Houston)	Houston, TX	3/20/2001-8/16/2006, 1/25/2007-present
Texas Ambulatory Surgery Center	Houston, TX	7/19/2001-present
Women's Center of Houston	Houston, TX	10/4/2013-present
Killeen Women's Health Center	Killeen, TX	6/1/2004-11/1/2013
Planned Parenthood Women's Health Center	Lubbock, TX	5/14/2001-11/1/2013
Whole Woman's Health McAllen	McAllen, TX	3/5/2004-11/1/2013, 9/4/2014-present
Planned Parenthood Choice	Midland, TX	7/1/2005-9/19/2013
Planned Parenthood Choice	San Angelo, TX	11/2/2007-9/19/2013
A Woman's Choice Quality Health Center	San Antonio, TX	8/2/2001-10/5/2011
Alamo Women's Clinic[14]	San Antonio, TX	2/5/2001-present
All Women's Medical Center	San Antonio, TX	2/25/2004-8/6/2013
New Women's Clinic	San Antonio, TX	3/28/2001-11/4/2013
Planned Parenthood Babcock Sexual Care/ Medical Center[15]	San Antonio, TX	12/13/2001-present
Planned Parenthood Bandera Road Sexual Healthcare[16]	San Antonio, TX	11/16/2009-11/1/2013
Planned Parenthood Northeast Sexual Healthcare[17]	San Antonio, TX	11/16/2009-11/1/2013
Reproductive Service	San Antonio, TX	6/6/2001-7/17/2012
Whole Woman's Health San Antonio[18]	San Antonio, TX	4/19/2013-present
Whole Woman's Health San Marcos[19]	San Marcos, TX	1/21/2005-9/1/2006
Planned Parenthood Center for Choice Stafford	Stafford, TX	9/4/2007-10/1/2013
KNS Medical PLLC Inc.	Sugar Land, TX	4/9/2004-3/27/2013
Planned Parenthood of Greater Texas Surgical Health Services[20]	Waco, TX	7/22/2005-9/1/2013; 4/2017-present
Planned Parenthood of Waco	Waco, TX	12/6/2001-12/2011
Planned Parenthood Feyetteville	Fayetteville, AR	<2006-present
Little Rock Family Planning Services	Little Rock, AR	<2006-present
Planned Parenthood Little Rock	Little Rock, AR	<2006-present
Alamosa Planned Parenthood[21]	Alamosa, CO	<2006-present
Boulder Abortion Clinic[22]	Boulder, CO	<2006-present
South Wind Women's Center[23]	Wichita, KS	6/7/2013-present

Notes: Dates come from the following sources: TDSHS clinic license database, records from the non-profit Fund Texas Choice, directly contacting clinics, and from press releases and newspaper articles.

Dates of Abortion Clinic Operations in Texas and Nearby States (Continued)

Clinic	Location	Dates providing abortion services
Delta Clinic of Baton Rouge[24]	Baton Rouge, LA	1/1/2006-1/2/2016
Bossier City Medical Suite	Bossier City, LA	<2006-present
Causeway Medical Clinic[25]	Metarie, LA	1/1/2011-1/2/2016
Women's Health Care Center[26]	New Orleans, LA	<2006-present
Hope Medical Group for Women	Shreveport, LA	<2006-present
Planned Parenthood Surgical Center[27]	Albuquerque, NM	<1/1/2010-present
Southwestern Women's Options[28]	Albuquerque, NM	<2006-present
University of New Mexico Center for Reproductive Health	Albuquerque, NM	1/1/2007-3/25/2014, 4/1/2014-present
Whole Woman's Health of New Mexico	Las Cruces, NM	9/15/2014-present
Planned Parenthood: Santa Fe Health Center	Santa Fe, NM	<2006-present
Hilltop Women's Reproductive Clinic Santa Teresa	Santa Teresa, NM	<2006-present
Medical Practice of William H. Richardson, M.D.	Norman, OK	<2006-present
Abortion Surgery Center		
Outpatient Service for Women	Oklahoma City, OK	<2006-12/2014
Trust Women South Wind Women's Center[29]	Oklahoma City, OK	9/2016-present
Reproductive Services of Tulsa[30]	Tulsa, OK	<2006-present

Notes: Dates come from the following sources: TDSHS clinic license database, records from the non-profit Fund Texas Choice, directly contacting clinics, and from press releases and newspaper articles. All documents referenced are available upon request.

1. Reproductive Services (Austin) not included in CLMS data because it pre-dates their sample period.
2. Whole Woman's Health (Austin). CLMS dates: <2009-7/14/2014; 4/2017-present. TDSHS license data reports closure on 7/2014 but Fund Texas Choice reports them operational until 11/10/2014.
3. Abortion Advantage (Dallas). CLMS dates: <2009-11/1/2013; 2/2014-12/2014. TDSHS license date reports closure on 12/10/2014 but Fund Texas Choice reports them operational until 6/12/2015.
4. Fairmount Center (Dallas). CLMS dates: <2009-10/2009. Fairmount Center closed in 12/2009 as shown in the TDSHS license data and immediately reopened as Southwestern Women's Surgery Center. The TDHS license data show Southwestern Women's Surgery Clinic operational as of 12/2009. If you take this into account, our dates for these two clinics match CLMS.
5. Southwestern Women's Surgery Center (Dallas). CLMS dates: 9/2009-present. See Comment 4 above.
6. Reproductive Services (El Paso). CLMS dates: <2009-11/1/2013; 1/2014-4/2014; 9/24/2015-present. Fund Texas Choice reports this clinic as operational until 11/2014. It reopened in 9/2015 and is still operational as of September 2017. The reopening date is found in an El Paso Times article and in the TDSHS license data.
7. During our sample period there were technically two Planned Parenthood branches in Fort Worth (Grater Texas Fort Worth and Southwest Fort Worth) that provided abortion services but only one was operational at any given point in time. CLMS report a break in service from 11/1/2013 to 1/13/2014. We don't observe any break in service until 11/2013 and the clinic resumed services in 12/2013. See article in the Fort Worth Stare-Telegram for documentation.
8. Trinity Valley Women's Clinic in Fort Worth closed and immediately reopened in a different location as Whole Woman's Health Fort Worth. If you combine our dates for Trinity and Whole Woman's Health, the dates match with the dates that CLMS report for Whole Woman's Health. The clinic did have a short three week laps in service from 11/1/2013 to 11/27/2013, see Rewire.News article for documentation.
9. Whole Woman's Health Fort Worth. See Comment 8.
10. AAA Concerned Women's Center (Houston). CLMS dates: <2009-10/1/2014. Our closure date comes from the records of Fund Texas Choice.
11. Aalto Women's Center (Houston). CLMS dates: <2009-3/13/2014. Our closure date comes from the records of Fund Texas Choice.
12. Americas Women's Clinic (Houston) is not included in CLMS's data because it pre-dates their sample period.
13. In May of 2010 Planned Parenthood of Southeast Texas (Houston, Fannin Clinic) became Planned Parenthood Gulf Coast (located on Gulf Freeway) which is still operational as of September 2017. As such, we report them as one, CLMS report them separately. We find no evidence of a gap in services. See Houston Chronicle article for documentation on Gulf Freeway opening in May 2010.
14. Alamo Women's Reproductive Services (San Antonio) became Alamo Women's Clinic. As such, we code them as one. CLMS code them separately. Initially the clinic was located at 8600 Wurzbach Rd. and moved to John Smith drive in 6/2015. We find no evidence of a gap in service.
15. We report Planned Parenthood Babcock Sexual Healthcare (San Antonio) and Planned Parenthood Medical Center together, CLMS report them separately. The Babcock clinic closed 7/31/2015 as documented in TDSHS license data, and the Medical Center opened in the summer of 2015 as documented by the Planned Parenthood South Texas website. We find no evidence of a gap in service.
16. Planned Parenthood Bandera Road Sexual Healthcare (San Antonio). We report this clinic first providing abortion services 11/2009 as documented in the TDSHS license data. CLMS have services beginning prior to 2009.
17. Planned Parenthood Northeast Sexual Healthcare (San Antonio). We report this clinic first providing abortion services 11/2009 as documented in the TDSHS license data. CLMS have services beginning prior to 2009.
18. Whole Woman's Health San Antonio. CLMS dates: 8/2/2010-present. We observe service beginning 4/19/2013 as reported by TDSHS license records. We find no evidence of it being operational before this time.

19. Whole Woman's Health San Marcos is not included in CLMS's data because it pre-dates their sample period.
20. Planned Parenthood of Waco (1927 Columbus St.) closed 12/2011. Planned Parenthood of Greater Texas Surgical Health (1121 Ross Ave.) became the only abortion provider in the city. This clinic had a laps in service between 9/1/2013 and 4/1/2017. Documentation for this can be found in articles from Life.News.com and the Waco Tribune as well as TDSHS license data. CLMS dates: 1/1/2012-8/2013; 5/2017-present and <2009-12/31/2011.
21. Alamosa Planned Parenthood (Alamosa, CO). CLMS dates: <2009-present. Our start of service date comes from calling the clinic.
22. Boulder Abortion Clinic (Boulder, CO). CLMS do not include this clinic in their dataset.
23. South Wind Women's Center (Wichita, KS). CLMS do not include this clinic in their dataset.
24. Delta Clinic of Baton Rouge (Baton Rouge, LA). CLMS do not include this clinic in their dataset.
25. Causeway Medical Clinic (Metarie, LA). CLMS do not include this clinic in their dataset.
26. Women's Health Care Center (New Orleans, LA). CLMS do not include this clinic in their dataset.
27. Planned Parenthood Surgical Center (Albuquerque, NM). CLMS dates: <2009-present. Our start of service date comes from calling the clinic.
28. Southwestern Women's Options (Albuquerque, NM). CLMS dates: 1/2009-present. Our start of service date comes from the clinic's website and from calling the clinic.
29. Trust Women South Wind Women's Center (OKC, OK). CLMS dates: 7/2006-present. They began offering service 9/2016 which can be verified on the clinic website.
30. Reproductive Services of Tulsa (Tulsa, OK). CLMS do not include this clinic in their dataset.

Appendix B: Data and Sample Construction

Abortion Provider Data

The main data source for clinic locations and dates of operation are licensure files from the TDSHS that provide information on all abortion clinics in the state for the years 2006-2015. These data include exact license dates for each clinic (both start and expiration dates) as well as each clinic's address. We determine the exact latitude/longitude coordinates of each clinic using a geocoding service provided by Texas A&M University.

We crosscheck the TDSHS data with information on abortion clinic closures from a Texas non-profit, Fund Texas Choice. The mission of Fund Texas Choice is to help pay for abortion travel-related expenses, particularly for low-income, teen, and rural Texans. As such, they have more up-to-date information on clinic closures, particularly during the roll-out of HB2 in 2013-2014. Fund Texas Choice frequently calls all clinics in Texas and border states to determine which clinics are providing abortions and keeps an ongoing record of clinic closures. In addition to validating the TDSHS abortion data, we have appended the TDSHS license data with data from Fund Texas Choice on the location and operation dates of clinics in neighboring states. Using the Fund Texas Choice data, we amended 13 of the closure dates provided by TDSHS. These were typically minor discrepancies in dates (there were no discrepancies in *which* clinics closed), and in all cases the TDSHS closure dates were later than those from Fund Texas Choice suggesting that these TDSHS licensure dates lagged behind actual closure. To further reduce concerns of measurement error in closure dates and to document any clinics that may have closed and reopened, we also crosscheck clinic operation dates with press reports and news articles documenting clinic closures. A particularly important clinic is Whole Woman's Health McAllen. This clinic was forced to stop providing abortion services on November 1, 2013. From 11/1/2013 to 9/2014 the nearest clinic to McAllen providing abortion services was in San Antonio, over 200 miles away. Whole Woman's Health McAllen resumed abortion services in September 2014 when the U.S. Court of Appeals for the Fifth Circuit overturned the admitting privileges provision of HB2. The dates of operation for this clinic can be verified by the Fifth Circuit Court of Appeals Ruling (Appellee, 2015) and an article from the New York Times.[53] Finally, we contacted each of the clinics that we observe as being operational in September 2016 to verify that they were still providing abortions. Table A3 presents a list of all clinics in Texas and nearby states with their corresponding dates of operation. Table A3 also indicates any discrepancy between the dates of operation used in our analysis and the dates used in the analysis of Cunningham et al. (2018). For all discrepancies, the notes that follow the table discuss our data source.

Family Planning Provider Data

Family planning centers include a diverse set of providers. To maintain consistency over time, we define publicly funded family planning clinics as those clinics receiving funds earmarked for family planning. Data on family planning clinic funding come from three sources. The first file is from TDSHS and indicates in each year the clinics in Texas that received family planning funding through this agency. The second dataset comes from WHFPT, the agency that took over the Title X grant for the state in 2013. These data indicate in each year from 2013 to 2015 the clinics that received Title X funds. This second source is necessary as the data from TDSHS only include clinics that received funding through TDSHS-administered programs and, beginning in 2013, Title X was no longer part of their budget.

Third, we supplement these data with information on publicly funded family planning clinics in other states. For the year 2010, the Guttmacher Institute collected data on the number of publicly funded family planning clinics per county for each state. We use the Guttmacher data to account for the possibility that women near state borders may be traveling out of state to seek family planning services. Unfortunately, these data only cover a cross-section and thus, we cannot account for changes in the number of publicly funded family planning clinics outside of Texas over time. For the purposes of our regression analyses, if we observe a positive number of clinics in any non-Texas county in 2010, we assume that there was an operational clinic in all years of our sample located at the population-weighted county centroid. In practice, as we describe in further detail below, our measure of access to family planning is an indicator for whether there is a publicly funded clinic within a short distance (i.e., 25 miles) therefore accounting for family planning services in nearby states has very little influence. When we exclude border counties (i.e., those areas which may have a family planning clinic within 25 miles but located in a different state) from the analysis, our estimates are qualitatively similar (results available upon request).

[53] URL: https://www.nytimes.com/2014/09/04/us/texas-abortion-clinic-to-reopen-after-court-ruling.html

It should be noted that our measure of publicly funded family planning excludes those receiving money from the WHP, the Medicaid waiver program which became the state-funded TWHP in 2013. This program explicitly targeted clinics affiliated with abortion providers (e.g., Planned Parenthood) and left funding intact for other clinics (Stevenson et al., 2016). However, abortion affiliated clinics had already been excluded from TDSHS funding and thus were already coded as non-funded in our measure of family planning access by the time TWHP went into effect in January 2013.

In our family planning access measure, we do not cover facilities covered under the Expanded Primary Health Care (EPHC) program that took effect in 2014. This state-funded expansion provided additional funding to primary care clinics including FQHCs, public health departments, and hospitals in an effort to repair the reproductive health care safety net (Department of State Health Services, 2015). Clinics considered to be affiliated with abortion providers were not eligible. Despite the goal of the expansion, White et al. (2017) provide evidence suggesting that, at least initially, these clinics lacked the capacity to provide the family planning services previously provided by specialized women's health care organizations. They find that these clinics often lacked trained providers to administer LARCs, that they mostly served existing patients rather than expanding their base to cover those that may have lost access, and that a patient's primary care needs left little or no time in an allotted appointment to address contraceptive needs. We decided to exclude these clinics primarily because while in theory they provide family planning services, they do not focus exclusively on such services. While our measure may not be the most comprehensive (i.e., covering all health care providers who offer family planning services), it is a measure less fraught with error. In a sensitivity analysis presented in Table C3, we exclude the years in which the EPHC program went into effect (2014 and 2015) and find that the results are virtually unaffected. Cunningham et al. (2018) avoid these complications and control for access to family planning using an interaction between a county-level indicator for the presence of a publicly funded clinic prior to 2011 (the year of the TDSHS cuts) and a post-2011 indicator. Although a simpler measure, they are interested solely in the effect of abortion clinic access and thus view family planning access only as a potential confounder, and do not present these estimates.

Measuring Distances

For both abortion and family planning access, we calculate the straight-line distance between the population-weighted county centroid for each county in Texas and the geographical coordinates of each clinic's exact address. For all clinics that are among the five closest in terms of straight-line distance in any time period, the driving distance is calculated, and the minimum is chosen as the county's closest abortion clinic. Driving distance is calculated using the Stata program *Georoute* (Weber et al., 2016). We first calculate the straight-line distance for each of the closest five clinics to avoid using the driving distance API for thousands of coordinate pairs. In most cases, the closest clinic in terms of straight-line distance is also the closest clinic in terms of driving distance. Because Texas does not have significant geographical features (e.g., large mountain ranges), the straight-line distance is a very good proxy for the driving distance. Ultimately, our results are not sensitive to the use of either straight-line distance or driving distance (except in interpretation, as driving distances are longer).

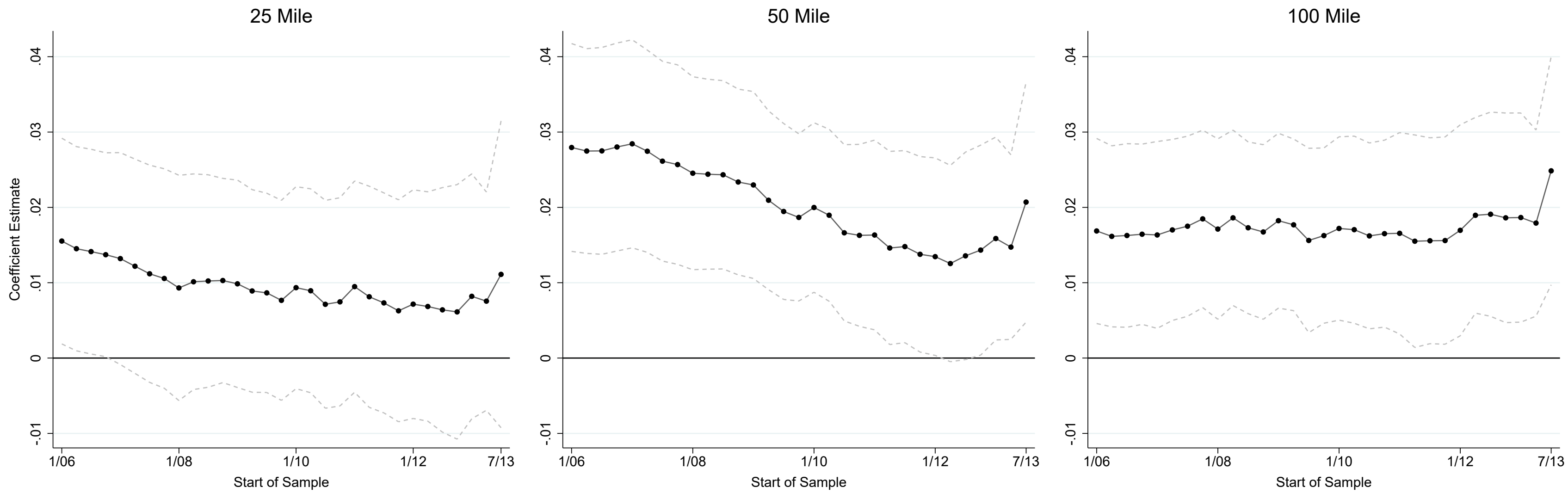
Assigning Access to Births

Abortion access is assigned to births at the 13th week of gestation and family planning access is assigned at conception. For abortion, matching at the time of conception would be inappropriate as there is not yet any knowledge of pregnancy; matching at any point beyond the 20th week of gestation would be inappropriate as Texas law prohibits abortion after the 20th week. The end of the first trimester represents a point at which the decision to receive an abortion is still actionable. For family planning, matching at any time beyond conception would be inappropriate if one believes that the primary purpose of access to family planning is preventing unintended pregnancy. Because access to family planning prior to the time of conception may be even more relevant, we explore the extent to which lags in access to family planning are important.

Matching births to abortion access at the 13th week of gestation requires first identifying the month in which each child is at their 13th week of gestation; this is done using information on the month of each birth and the number of weeks in gestation. The number of weeks in gestation (converted to months by multiplying by 7/30.5) is subtracted from the birth month, and 13 weeks (converted to months) is added. The function used is as follows: Week13 = round((BirthMonth+0.5) - (WeeksGestation*7/30.5) + (13*7/30.5)). Note that 0.5 is added to the birth month to represent the middle of the month rather than the start. Next, the data on births (where the year and month represent the 13th week) are merged with our measures of access. At this point, both abortion access and family planning access are defined at the 13th week of gestation – to ensure family planning access is defined at the time of conception, our measure of access to family planning is lagged by 3 months (approximately 13 weeks). In exploring further lags in access to family planning, we refer to this 3-month lagged version as t=0 denoting the time of conception.

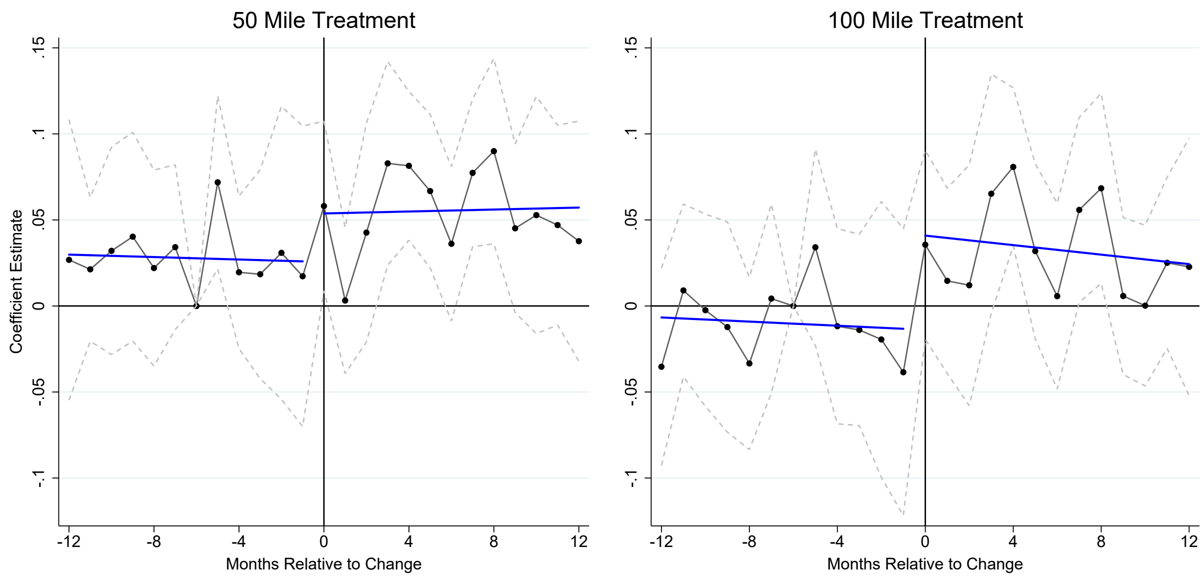
Appendix C: Robustness Analysis of Abortion and Birth Results

Figure C1: Effect of Abortion Access on Births – Varying Samples



Notes: These graphs plot the coefficient estimates for the three measures of access to abortion (i.e., 25-mile, 50-mile, and 100-mile) for a variety of samples that start at different dates. The first point on the left of each plot (1/06) represents the full sample, and each subsequent point omits three months from the sample period (so the second third, fourth, and fifth points represent samples starting in 4/2006, 7/2006, 10/2006, and 1/2007 respectively). Note that the first major changes in access to abortion occurred in November 2013, so the last point on the plots (7/13) represent a sample in which only four months prior to the first major changes in access to abortion are used.

Figure C2: Event Studies for Abortion Access on Births – Married Women



Notes: The model used to estimate the event studies is a Fixed Effects Poisson which is equivalent to the model used to produce the main estimates, except that instead of a single treatment variable, there are multiple treatment variables corresponding to months relative to the event. The event is defined as the first month in which a county switched from having a clinic to not having a clinic within the corresponding distance. In other words, $\beta_1 Access_{ct}$ from Equation 1 is replaced with $\sum_{k=-6}^{-T_{pre}} \theta_k A_{ctk} + \sum_{k=1}^{T_{post}} \theta_k A_{ctk}$. A_{ctk} is an indicator equal to 1 for k months from the event for county c, where the estimates of θ_k are the coefficients of interest. The period six months prior the event is omitted as it is the reference group. The blue lines represent linear trends through the estimates and are intended to provided a visual aid illustrating the existence of any differential trends in the period prior to the event.

Table C1: Number of Births - Sensitivity to Start of Sample

	2006	2007	2008	2009	2010
Panel A: Abortion Access					
No Clinics 25 mi	0.016**	0.013*	0.009	0.010	0.009
	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)
No Clinics 50 mi	0.028***	0.028***	0.025***	0.023***	0.020***
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)
No Clinics 100 mi	0.017***	0.016***	0.017***	0.018***	0.017***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	30,360	27,324	24,288	21,252	18,216
Panel B: Family Planning Access					
No Clinics 25 mi (t=0)		-0.011	-0.009	-0.012	-0.012
		(0.008)	(0.008)	(0.008)	(0.008)
No Clinics 25 mi (t-12)		0.012**	0.013**	0.012**	0.010
		(0.006)	(0.006)	(0.006)	(0.007)
Observations		27,324	24,288	21,252	18,216
Economic Controls	X	X	X	X	X
Demographic Controls	X	X	X	X	X
Access Controls	X	X	X	X	X

Notes: This table tests the sensitivity of the main estimates to the year in which the sample begins (i.e., the length of the pre-period). The analysis is at the county-year-month level, and the coefficients represent estimates from a Fixed Effects Poisson model with the number of births in each category as the outcome. The exposure variable is the population of females 15-44 years old. In Panel A, each estimate comes from a separate regression; in Panel B, each column is a separate regression. Because a one-year lag is included in Panel B, the baseline estimates begin in 2007 (i.e., the lagged measure represents access in 2006). The treatment variables of interest are dummy variables indicating that there were no clinics (abortion or publicly funded family planning) in the relevant driving distance. Sample sizes vary across panels due to the lagged measure of family planning access. Standard errors are reported in parentheses and are clustered at the county level.

Table C2: Effects of Access to Abortion Near Mexico or State Borders

	Mexico Border			Other State Border		
	All Abortions	All Births	Hispanic Births	All Abortions	All Births	Hispanic Births
No Clinics 25 mi	-0.118***	-0.001	0.019*	-0.175***	0.007	0.018**
	(0.032)	(0.007)	(0.011)	(0.041)	(0.008)	(0.008)
No Clinics 25 mi × Border	-0.243***	0.021*	-0.001	0.117	0.001	0.020
	(0.032)	(0.012)	(0.016)	(0.102)	(0.012)	(0.026)
No Clinics 50 mi	-0.103***	0.008	0.019*	-0.178***	0.015**	0.020**
	(0.031)	(0.007)	(0.012)	(0.058)	(0.007)	(0.009)
No Clinics 50 mi × Border	-0.607***	0.015	-0.001	0.113	-0.017	-0.029
	(0.053)	(0.012)	(0.017)	(0.103)	(0.017)	(0.046)
No Clinics 100 mi	-0.092***	0.011	0.011	-0.223***	0.017**	0.016*
	(0.027)	(0.009)	(0.012)	(0.084)	(0.007)	(0.009)
No Clinics 100 mi × Border	-0.625***	0.012	0.007	0.068	-0.009	-0.033
	(0.050)	(0.013)	(0.017)	(0.111)	(0.022)	(0.043)
Observations	2,530	12,144	12,144	2,530	12,144	12,144
Time-Varying Controls	X	X	X	X	X	X
HHS Trends	X	X	X	X	X	X

Notes: The analysis is at the county-year level for abortions and the county-year-month level for births. The coefficients represent estimates from Fixed Effect Poisson models with the number of abortions or births as the outcome. The exposure variable in all regressions is the population of females 15-44 years old. For each measure of abortion access, an interaction with a border county dummy variable is also included. Each pair of rows (the main effect for each measure of access and its interaction) represents estimates from a single regression. Border indicates the county is either on the Mexico border (columns 1-3) or the border with another state (columns 4-6). The estimates are similar if we expand the definition of border include counties that are adjacent to border counties. Standard errors in parentheses are clustered at the county level.

Table C3: Impacts of Lagged Family Planning Access on Births (Poisson) – Excluding 2014-2015

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Contemporaneous Impacts						
No Clinics 10 mi	0.012	-0.001	-0.001			
	(0.011)	(0.006)	(0.007)			
No Clinics 25 mi	0.024***	0.008	0.006			
	(0.008)	(0.008)	(0.008)			
Panel B: Dynamic Impacts						
No Clinics 25 mi (t=0)				0.000	0.003	-0.003
				(0.009)	(0.010)	(0.010)
No Clinics 25 mi (t-12)				0.013*	0.015*	0.012
				(0.007)	(0.008)	(0.009)
No Clinics 25 mi (t-24)					-0.001	0.003
					(0.009)	(0.010)
No Clinics 25 mi (t-36)						-0.005
						(0.010)
Time-varying Controls	-	X	X	X	X	X
HHS Trends	-	-	X	X	X	X
Observations	24,288	24,288	24,288	21,252	18,216	15,180

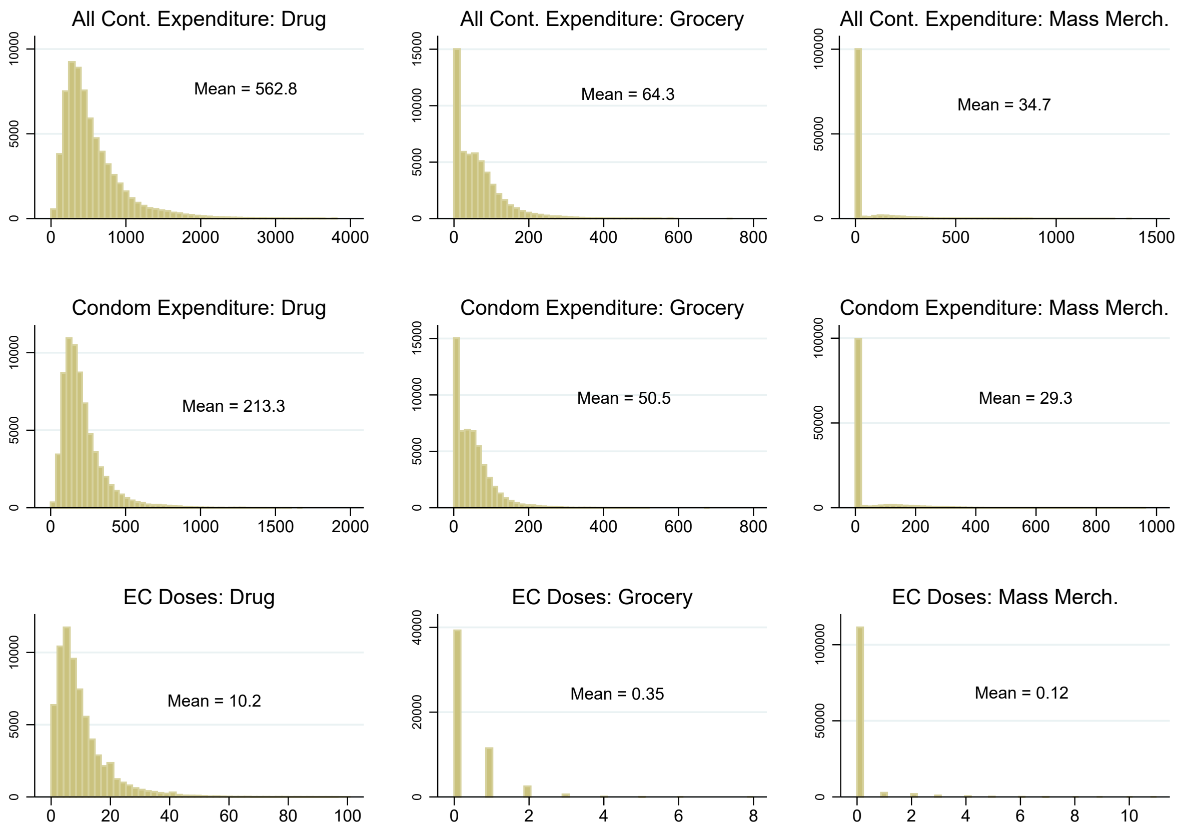
Notes: This table replicates the estimates in Table 5, but excludes 2014 and 2015 to test whether the omission of funding from the Expanded Primary Health Care program affects the results. The analysis is at the county-year-month level and the coefficients represent estimates from a Fixed Effects Poisson model with the number of births in each category as the outcome. The exposure variable is the population of females 15-44 years old. In Panel A, each coefficient comes from a separate regression. In Panel B, each column is a separate regression. The treatment variables are dummy variables indicating that there were no family planning clinics in the relevant driving distance at the time of conception (t=0), a year prior to conception (t-12), and so on. All regressions include county and year-by-month fixed effects. Time-varying controls are the unemployment rate, log per capita income, age- and race-specific populations, and the distance to the nearest abortion clinic. HHS trends represent HHS region-specific linear time trends. Standard errors are reported in parentheses and are clustered at the county level.

Appendix D: Robustness Analysis of Contraceptive Purchasing Results

In a previous version of this paper, we included all store types and estimated OLS models where the outcome was the inverse hyperbolic sine of condom expenditures. We found that both access to abortion and family planning led to an increase in condom purchases. Upon further investigation, we discovered a primary factor contributing to this result was the combination of a multi-modal distribution of the outcome, caused by heterogeneity in store type, with the inverse hyperbolic sine (IHS) transformation. The IHS has a linear interpretation for observations of the outcome near zero and a log interpretation for large observations, as such, when the distribution of the outcome is multi-modal point estimates tend to be unreliable, see Burbidge et al. (1988) for a more detailed discussion of this issue. In response, in the current version of the paper we have made several updates which include adding a new year of data and expanding our analysis to emergency contraceptives. Most importantly we have chosen to analyze different store types separately, which ensures that store level observations within a given regression are comparable and that the distributions of the outcomes are not multi-modal. Beyond this, we have been careful to allow the distributions of the data to inform our empirical specifications.

To evaluate whether these estimated impacts are unique to drug stores or to the specification, additional estimates are provided in Table D1. The four columns in this table represent samples that include all stores, drug stores only, grocery stores only, and mass merchandise stores only. Because grocery and mass merchandise stores have many observations with zero sales of either condoms or ECs, the log specification is inappropriate for regressions that include these stores. We instead report results using a Poisson model for both condoms and ECs, where the outcome is the number of condoms sold or the number of EC doses sold. In both cases, we can identify the actual number of units sold; that is, a 12-pack of condoms counts as 12 condoms rather than a single unit sold. Panel A reports the impacts of abortion access on the number of condoms sold and Panel B reports the impacts of abortion access on the number of EC doses sold. Panels C and D report the equivalent for family planning access. Consistent with the previous results, these estimates indicate no significant impact of reduced access to abortion or family planning on contraceptive purchases.

Figure D1: Contraceptive Purchase Distributions



Notes: In all histograms, the y-axis represents the frequency of store-month observations. In all cases, the variable of interest represents weekly expenditures or quantities; we use weekly figures because this is the finest level of aggregation in the Nielsen data, and months differ in the number of weeks represented depending on the number of Saturdays in a particular month. The first and second rows display the distributions of expenditures on all contraceptives (condoms plus emergency contraceptives) and condoms alone, respectively. The third row displays the distribution of emergency contraceptive (EC) doses purchased; one dose is equivalent to one EC pill.

Table D1: Contraceptive Purchases By Store Type

	All	Drug	Grocery	Mass
Panel A: Abortion Access; Condoms				
No Clinics 50 mi	0.000	-0.021	-0.021	0.046
	(0.022)	(0.021)	(0.085)	(0.031)
Observations	239,995	69,033	53,067	117,895
Mean Dep. Var.	95.7	182.1	70.0	56.9
Panel B: Abortion Access; EC				
No Clinics 50 mi	-0.020	-0.010	0.133	-0.539
	(0.038)	(0.036)	(0.151)	(0.356)
Observations	120,062	68,993	33,907	17,162
Mean Dep. Var.	3.1	10.2	0.3	0.1
Panel C: Family Planning Access; Condoms				
No Clinics 25 mi (t=0)	-0.017	-0.021	0.009	-0.071
	(0.022)	(0.027)	(0.052)	(0.043)
No Clinics 25 mi (t-12)	0.023	0.013	-0.034	0.060
	(0.026)	(0.030)	(0.051)	(0.056)
Observations	239,592	69,014	53,007	117,571
Mean Dep. Var.	95.7	182.1	70.0	56.9
Panel D: Family Planning Access; EC				
No Clinics 25 mi (t=0)	0.005	-0.007	-0.241	1.186*
	(0.046)	(0.046)	(0.361)	(0.632)
No Clinics 25 mi (t-12)	-0.023	-0.016	-0.193	-0.548
	(0.043)	(0.045)	(0.310)	(0.607)
Observations	120,043	68,974	33,907	17,162
Mean Dep. Var.	3.1	10.2	0.3	0.1

Notes: Panels represent different treatments (abortion access or family planning access) and different outcomes (condoms or emergency contraceptives). All regressions are estimated via Fixed Effects Poisson, and the outcome is the number of units sold. Note that the definition of a unit is a single contraceptive (a single EC pill or a single condom) such that a 12-pack of condoms will count as 12 units sold. The Poisson accounts for the fact that there are potentially a large number of observations equal to zero for some products and store types, and for the fact that the distribution of sales is highly skewed, especially in regressions with multiple store types. The mean dependent variable represents the mean weekly number of contraceptives of a given type. Sample sizes are smaller for EC doses because the Poisson specification omits stores if the outcome is equal to zero in all periods, and ECs are not carried by as many stores (especially mass merchandise retailers). Sample sizes vary across panels because the lagged measure of family planning access is missing for new stores.