ESSAYS IN WOMEN'S AND REPRODUCTIVE HEALTH ECONOMICS

A Dissertation

by

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ABSTRACT

The impacts of access to health care and protective services on health and wellness outcomes are important to understand, yet difficult to measure. To establish these causal relationships, researchers need an exogenous shock to access. Since there are a lot of factors that play in to the local networks—particularly in regard to women's health and safety issues—finding these exogenous shocks can be difficult. Some questions of particular relevance to women's health and safety may be the following: How does access to highly effective contraception impact childbearing behavior? Can unintended pregnancies be reduced through pre-existing local health care networks? Does a reduction in local capacity for abortions actually impact abortion behavior, or do women still obtain abortions at the same time and rate that they would in a capacity-unconstrained environment? Are there any direct consequences to leaving sexual assault kits untested, or are untested kits just the rational response by agencies to a lack of evidence? Can testing old, backlogged evidence kits have a meaningful effect on DNA databases?

In this dissertation, I use natural experiments in which policy or funding changes created a shock to the provision of health care or protective services *unrelated to demand for these services*. My combined findings suggest that reducing barriers to reproductive control technology can improve outcomes for women: providing free, long-acting reversible contraception through Title X clinics reduced both teen births and the teen abortion rate by approximately 20-30 percent each. Similarly, reducing local abortion clinic capacity may have reduced the overall abortion rate by as much as 11 percent. Reduced local clinic capacity also delayed the timing of abortions—shifting women from obtaining abortions in the first 8 weeks of gestation into weeks 9-12 and beyond—and increased the birth rate by approximately 3 percent. Finally, increased funding for testing backlogged sexual assault kits increased the rate of profiles entered into state DNA databases by approximately 18 new profiles per 100,000 residents. This increase in profiles could impact victim (or potential

victim) health through increased arrests, deterrence of future assaults, or improved mental health from knowing the perpetrator is behind bars.

DEDICATION

To my parents, Amy and Jeff Kelly, and my sisters Jess Crist and Jayme Kelly. I love you all immensely and would not have made it here without you.

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The impacts of access to health care and protective services on health and wellness outcomes are important to understand, yet difficult to measure. To establish these causal relationships, researchers need an exogenous shock to access. Since there are a lot of factors that play in to the local networks—particularly in regard to women's health and safety issues—finding these exogenous shocks can be difficult. Some questions of particular relevance to women's health and safety may be the following: How does access to highly effective contraception impact childbearing behavior? Can unintended pregnancies be reduced through pre-existing local health care networks? Does a reduction in local capacity for abortions actually impact abortion behavior, or do women still obtain abortions at the same time and rate that they would in a capacity-unconstrained environment? Are there any direct consequences to leaving sexual assault kits untested, or are untested kits just the rational response by agencies to a lack of evidence? Can testing old, backlogged evidence kits have a meaningful effect on DNA databases?

In this dissertation, I use natural experiments in which policy or funding changes created a shock to the provision of health care or protective services *unrelated to demand for these services*. My combined findings suggest that reducing barriers to reproductive control technology can improve outcomes for women: providing free, long-acting reversible contraception through Title X clinics reduced both teen births and the teen abortion rate by approximately 20-30 percent each. Similarly, reducing local abortion clinic capacity may have reduced the overall abortion rate by as much as 11 percent. Reduced local clinic capacity also delayed the timing of abortions—shifting women from obtaining abortions in the first 8 weeks of gestation into weeks 9-12 and beyond—and increased the birth rate by approximately 3 percent. Finally, increased funding for testing backlogged sexual assault kits increased the rate of profiles entered into state DNA databases by approximately 18 new profiles per 100,000 residents. This increase in profiles could impact victim (or potential victim) health through increased arrests, deterrence of future assaults, or improved mental health from knowing the perpetrator is behind bars.

This dissertation has three sections. In each, I use quasi-experimental methods to determine the effects of policies specifically targeted toward women's health and wellness: first I study a policy change designed to improve women's access to reproductive control technologies; next I study a policy change designed to improve the safety of abortions—though this policy ultimately limited access to abortion, and therefore reduced women's access to some types of reproductive control technology; and finally I study a policy change designed to provide better resources for victims of sexual assault, a crime which disproportionately affects women and which has a dramatic backlog of evidence in at least some law enforcement agencies.

My findings overall suggest that policies geared toward improving access to technology and increased funds for women reduces negative outcomes for women: increased access to effective contraception reduces unintended pregnancies, measured through both reductions in births (particularly for teenagers) and abortions; increased funding for sexual assault initiatives increased the number of forensic profiles entered into states' DNA databases, suggesting that arrest and deterrence of future sexual assaults may occur. On the other hand, reducing access to reproductive control technology (through the channel of reduced abortion access) pushed women into riskier behaviors. Women obtained abortions later than they would have preferred, potentially faced higher costs and higher risks of complications, and even obtained fewer abortions—resulting in higher numbers of unintended births. Having an unintended birth can have lasting negative consequences on economic, labor force, educational attainment, and health outcomes for women. Results from Section 2 are also particularly relevant to other health settings in which tempo and timing of treatment have a meaningful impact on outcomes, such as chronic care or cancer screening and treatment. The results from this section can also contribute to our understanding of the consequences of not expanding care in settings in which local capacity is falling—like in areas facing hospital closures—or demand is increasing dramatically—such as care for the elderly as baby boomers age or care for pandemics, like the recent COVID-19 outbreak.

2 THE POWER OF THE IUD: EFFECTS OF EXPANDING ACCESS TO CONTRACEPTION THROUGH TITLE X CLINICS

2.1 Introduction

In February 2019, the Trump administration issued a new rule—scheduled to go into effect 60 days thereafter—that will deny Title X funding to any facility that provides abortion in addition to performing Title X activities, which includes contraception provision in addition to other forms of reproductive health care. Given the historical importance of Title X for low-income women's childbearing (Bailey, 2012), this naturally raises the questions: how important are Title X clinics in today's context and to what degree does it matter how well they are funded?

Underscoring the importance of these questions, a large body of research has demonstrated the remarkable ways in which improving women's ability to control childbearing can affect their outcomes. In particular, it's broadly accepted that expanding access to birth control pills and abortion during the 1960s and 1970s reduced childbearing and increased women's educational attainment, wages, and labor force participation, while reducing dependence on public assistance and improving resources in households with children.[1] We know much less about the effects of the importance of access to contraception in today's context.

Even though more US women are using contraception today than at any other point in history, there is reason to believe that barriers to access may still be important. Nearly half of today's pregnancies are unintended (Finer and Zolna, 2016) and one-third of today's births are from unintended pregnancies (Buckles et al., 2019). Moreover, the fact that one-third of today's unintended pregnancies are to women using some form of contraception (National Center for Health Statistics, 2014) highlights that a relatively small share of women

[1]See Goldin and Katz (2002), Bailey (2006b), Guldi (2008), Bailey (2009), Bailey (2012), Bailey et al. (2013), Myers (2017), Bailey et al. (2019), and Beauchamp and Pakaluk (2019). Also see Bailey and Lindo (2018) for a review.

are using most effective forms of contraception. For these reasons, there is a great deal of enthusiasm for expanding access to long-acting reversible contraceptives (LARCs), a category comprised of intrauterine devices and subdermal implants, which have failure rates less than 1% (versus birth control pills, injectables, patches, rings, and condoms, which have failure rates of 6–18%).[2] Though several medical organizations have called on LARCs to be more widely promoted—including the American College of Obstetricians, Gynecologists' Committee on Adolescent Health Care, the American Academy of Pediatrics, and the Center for Disease Control and Prevention—only 12% of women, and only 5% of teenagers, chose a LARC in 2012 (Bailey and Lindo, 2018). Such low take-up is attributed to the fact that women often lack awareness of LARC availability or effectiveness, have misconceptions about safety, or cannot afford the high up-front fixed costs. Notably, 49% of residents in obstetrics/gynecology training programs use LARCs (Zigler et al., 2017), and 65 percent of participants in a contraception counseling program chose a LARC when it was offered at no cost (Secura et al., 2010), implying that information and financial barriers impede higher usage rates.

In this section we examine the impact of an initiative that provided funds to Title X clinics so they could make LARCs available to low-income women for free or at reduced costs. Specifically, we analyze the effects of the Colorado Family Planning Initiative (CFPI), a program that allowed Title X clinics in Colorado to substantially expand the provision of LARCs to their low-income clients beginning in 2009. Whereas earlier work has shown that the initiative reduced birth rates for 15–19 year olds residing in counties with Title X clinics (Lindo and Packham, 2017), in this study we leverage more granular data and more recent years of data to answer several new questions, including: What kind of "reach" can we expect from a clinic-based initiative like this one? Do the effects extend to women who live a moderate distance away from a clinic? Are there effects on high-school-aged teenagers? On older teenagers and women in their twenties? Do the effects vary across race and ethnicity?

[2]LARCs are inserted by a doctor and do not require anything of the user, thus eliminating user-compliance errors.

Did the extensive media coverage that began in 2014 alter the reach of the program? Is there any evidence of impacts on other outcomes associated with women's or newborns' health?

We answer most of these questions using two separate administrative datasets on contraception use and births. After providing evidence that the initiative led to increases in LARC use, we estimate the effects on births using a difference-in-differences design that compares changes in births for women in zip codes closest to Title X clinics to changes in births for women in zip codes farther away. We use a non-parametric approach to identify which zip codes were most affected by the initiative and find that the effects are largely concentrated among women in zip codes within 7 miles of Title X clinics. We find that the initiative reduced births by approximately 20 percent for 15–17 year olds and 18–19 year olds living in such zip codes. Averaging across all years, we find no evidence of significant effects on births among older women, or women living in more distant zip codes. However, when focusing on the period of time after the initiative received extensive media attention, we observe a trend break in the number of LARC insertions; that the effects on births among 15–17 year olds extend out those living a greater distance from clinics; and that there are significant effects on births among 20–24 and 25–29 year olds.

In other analyses, we consider whether these birth effects vary across income, race and ethnicity, and explore how the initiative affected births that typically involve relatively intensive hospital care. We also conduct an analysis of abortion, the results of which are often imprecise, but which provide suggestive evidence that the initiative also reduced unintended pregnancies among teenagers that otherwise would have ended in abortion.

These results have several implications for policy. First, the magnitude of the effects indicates that expanding free and low-cost access to LARCs through Title X clinics can reduce unintended pregnancy for a large share of young women who live close to such clinics.[3] Second, reducing barriers to highly effective contraceptives can help women from a broad range of ages—from 15–29—avoid unintended pregnancy. Quite notably, the effects are especially

[3]Reductions in unintended pregnancy are implied by any reduction in births that results from women voluntarily opting for more effective methods of contraception.

prominent for 15–17 year olds and also 18–19 year olds, which suggests that the initiative may have improved women's ability to invest in their high school and post-high-school education and thus have important implications for their economic circumstances. Third, our results indicate that estimates of the effect for all women living in a county with a clinic (e.g., in Lindo and Packham (2017)) obscure much larger effects for women who live closest to a clinic. Finally, since 58.9 percent of Colorado women live in zip codes where we do not find any statistically significant effect of the initiative, our results highlight that there is more scope for Title X clinics to provide services to a broader share of the population. These findings have especially important policy implications in light of recent federal guidelines that will cut funding to clinics offering abortion in addition to providing Title X services.

2.2 Background

This section reviews the effectiveness and uptake of long-acting reversible contraceptives before providing background information on the Colorado Family Planning Initiative. To do so, we borrow heavily from the discussion in Lindo and Packham (2017).

2.2.1 Long-Acting Reversible Contraceptives (LARCs)

LARC methods include intrauterine devices (IUDs) and subdermal implants. IUDs are small, T-shaped devices inserted into the uterus to prevent pregnancy, while implants are a thin, matchstick-sized plastic rod inserted under the skin of the non-dominant upper arm. The two most common IUDs are Mirena and ParaGard. Mirena is a hormonal IUD that releases progestin to prevent sperm motility and lasts for up to 5 years. ParaGard, a copper IUD, contains no hormones and can last up to 12 years. Nexplanon, the most popular implant, contains etonogestrel and can last up to 4 years.

During the first year of typical use, fewer than 1 in 1,000 women using an IUD or implant become pregnant, due to the fact that LARCs eliminate the potential for user-compliance error as they require nothing of the user after insertion.[4] Accordingly, professional health

[4]For comparison, oral contraceptives and condoms and have typical-use effectiveness rates of only 91 percent and 82 percent, respectively, and for oral contraceptives, the risk of contraceptive failure is 55 percent

organizations such as the American Congress of Obstetricians and Gynecologists and the American Academy of Pediatrics have endorsed LARCs as preferred methods for young, sexually active women.

Despite the relative ease and effectiveness of LARCs, take-up is lower than nearly every other type of contraceptive device. Only 5.8 percent of adolescents and women aged 15–19 have ever used an implant or IUD, and only 14 percent of US women chose a LARC in 2014 (Guttmacher Institute, 2018). Substantial barriers to use include patients' lack of familiarity, lack of access, and/or misconceptions about safety.[5] For young women especially, who may face social stigma or large transportation costs, scheduling and attending the procedure itself may also serve as a deterrent. Insertion is uncomfortable and sometimes painful, and women can experience side effects, such as menstrual pain and bleeding, spotting, headaches, nausea, and mood changes (Grimes, 2007).[6,7] Moreover, for women wishing to become pregnant in the near future, alternative methods of contraception that do not require a visit to the doctor (for removal) to restore their ability to become pregnant may be more attractive.

Importantly, the high upfront costs of the devices may also create large barriers to uptake. LARCs can cost upwards of $500 out-of-pocket, and even insured patients may pay up to a $160 copayment (Trussell et al., 2009; Planned Parenthood, 2017). And while the Affordable Care Act, which requires insurers to cover all FDA-approved contraceptives, has reduced or eliminated concerns about costs for some women since 2011, there is an exemption for employers with a religious or moral objection that was recently expanded in 2017. Moreover,

higher among women younger than 20 years old (Dinerman et al., 2012; Grady et al., 1986).

[5]Such safety concerns may be a result of the reputation of the Dalkon Shield. The Dalkon Shield, an IUD introduced in the 1970s, increased women's risk for pelvic inflammatory disease and caused an array of severe injuries, leading the US Food and Drug Administration to ban the device in the 1974. Although Congress has since passed laws requiring new oversight for IUD approval, and current alternatives do not suffer from these design flaws, the decades-old controversy is still relevant in shaping attitudes towards IUDs today (Bailey and Lindo, 2018).

[6]Based on their clinical trials, the five IUDs available on the U.S. market (Mirena, Paragard, Sklya, Liletta, and Kyleena) have discontinuation rates due to adverse reactions between 12 percent and 22 percent. As a point of comparison, clinical trials for the commonly prescribed birth control pill, Ortho Tri Cyclen, have had discontinuation rates due to adverse reactions between 11 and 21 percent.

[7]The more common side effects of LARCs include missed periods, bleeding, and headaches, which are common side effects for any type of hormonal contraceptive. More serious risks of IUDs include uterus perforation and ectopic pregnancies, which occur in less than 1 in 1,000 patients (Grimes, 2007).

these contraceptives mandates may not be as impactful for women that rely on their parents' health insurance if they are worried about confidentiality.[8] As a result, the costs of LARCs continue to be highly salient for many women.[9]

In addition to these demand-side concerns, from the provider's perspective there also exist barriers that contribute to the low rate of LARC use among U.S. women. First, doctors and nurses may themselves be unaware or misinformed about LARC technology, and they must be trained on proper LARC insertion/removal to provide them to patients. Second, health clinics that provide free and low-cost contraceptives often cannot afford to offer LARCs to every client. As a result, a large proportion of Title X clinics do not offer LARCs at all, and those that do usually have to offer them to clients selectively. For example, just 61 percent of Title X clinics offered implants, 65 percent offered the copper IUD and 67 percent offered a hormonal IUD in 2015 (Zolna and Frost, 2016).

By allocating funds and technical assistance for LARCs, the CFPI allowed Colorado Title X clinics to offer these devices to thousands of women for the first time. Below, we discuss in greater detail the extent of the LARC-focused program and provide details on its implementation.

2.2.2 The Colorado Family Planning Initiative (CFPI)

In 2009 the Colorado Department of Public Health and Environment (DPHE) established the CFPI in an effort to lower the state's unintended pregnancy rate through family planning.[10] Over the course of the initiative, the Colorado DPHE received $23 million in provisional funding from the Susan Thompson Buffett Foundation to expand family planning services, and, in particular, provide free LARC methods to low-income women in Title

[8]According to a recent survey, 68 percent of teens stated that their primary reason for not using birth control is because they are afraid that their parents might find out (The National Campaign to Prevent Teen and Unplanned Pregnancy, 2015).

[9]Notably, Mestad et al. (2011) find that 70 percent of women aged 14–20 choose a LARC when it is offered at no cost.

[10]Our description of the implementation of the Colorado Family Planning Initiative draws heavily from conversations with the Colorado Department of Public Health and Environment and the detailed discussion provided in Ricketts et al. (2014).

X clinics. All of Colorado's 28 agencies applied for and received funding, which was distributed to 68 Title X clinics across the state from 2009–2015.[11,12]

While the specifics of each agency's implementation strategy varied by clinic capacity and client population, Title X clinics generally agreed to spend their funds on the following objectives: supplying free IUDs and contraceptive implants to low-income women; equipping staff and providers with more knowledge about LARC insertion, promotion, and counseling; and providing technical assistance for billing, coding, and clinic management. Additionally, the CFPI offered general assistance to Title X agencies to increase LARC usage and supported the provision of vaginal rings, tubal ligations, and vasectomies. However, the use of the ring remained fairly constant among clients after the CFPI was implemented, and tubal ligations and vasectomies are extremely rare among young women.[13]

The foundation allocated funding directly to Title X agencies, which receive federal and state funds to provide free or low-cost family planning counseling, sexually transmitted disease screening, and contraceptives. The expansion within the existing network of Title X family planning clinics offered a number of advantages. Due to existing contracts with agencies within the network, the Colorado DPHE was able to expedite the funding process and make use of clinic networks and infrastructure in place to better facilitate collaboration, while current Title X regulations and protocols support high-quality health care and the ongoing collection of data. Since many Title X clinics already had waiting lists of women seeking these expensive devices, the goal of the CFPI was to focus efforts on the unmet demand for highly effective contraceptives and expand access more broadly.

Moreover, Title X clinics are specifically aimed at providing services to low-income women, who may face the largest hurdles to obtain LARCs. Colorado Title X guidelines require all contraceptive methods and exam fees be incorporated into a schedule of discounts,

[11]Due to the declared success of the program, additional public funding for LARCs was appropriated in 2016 after the depletion of private funds.

[12]Money was allocated proportionally to agencies based on their number of clients and the predicted number of LARC insertions in the following year.

[13]Take-up of rings among Colorado Title X clients aged 15–29 is about 7 percent, while take-up of ligations and vasectomies is less than 0.5 percent.

or sliding fee schedule. Anyone reporting that their income is at or below 100 percent of the poverty level pays nothing for any service, and no client is denied services because of an inability to pay. In Colorado, 90 percent of Title X clients report incomes below this level, meaning that nearly all clients pay nothing for contraceptives and doctor visits.[14] Prior to the CFPI, the high upfront costs of LARCs paired with the fact that clinics provided their services for free meant that widespread provision of LARCs was prohibited. In the first year of the program, 20 out of 28 Colorado Title X agencies offered IUDs for the first time, and 16 agencies offered the implant for the first time. In the following year, all Colorado agencies offered IUDs and all but one agency offered subdermal implants, suggesting that the CFPI funding was crucial in allowing clinics to offer LARCs as a contraception option to all women.

2.3 Empirical Approach

We use data on contraceptive use and childbearing from two separate administrative datasets. These data allow us to observe the number of women visiting Colorado Title X clinics and their contraception choices before and after the CFPI, and to analyze trends in births by age, race, and ethnicity. Below, we provide a detailed description of the data used in our analysis as well as our strategies for estimating the causal effects of the CFPI.

2.3.1 Data

To understand how Title X clients' contraception choices changed in response to the CFPI, we use administrative family planning agency-level data from the Colorado Department of Public Health and Environment (DPHE). These data include aggregated, client-level information on contraceptive methods at the time clients left the clinic (for various age groups), as well as the total number of LARC insertions per year. The data span from 2008 to 2015, which allow us to see client's contraceptive choices, including whether or not they chose a LARC, from one year prior to the CFPI through the seventh year of the program.

[14] Agencies must accept verbal communication of income and no verification is required.

Additionally, we use data from the Colorado DPHE on LARC insertions from 2008–2015, which contains the total number of new insertions of IUDs and implants for all Title X clients.

To estimate the effect of the initiative on births for various groups defined by age, race, and ethnicity, we use restricted-access birth data from the Colorado DPHE. These data include a record of every birth to Colorado residents from 2003 to 2016. Critically, these data provide mother's zip code of residence, which allows us to conduct a richer and more granular analysis than (Lindo and Packham, 2017). These data also include information on mother's race, ethnicity, and age in addition to measures of infant health. For our analysis we assign births to the year of conception based on the mother's last menstrual period to construct a measure of births conceived in a particular year.

While nearly all of our analyses focus outcomes measured from birth records, we also consider effects on abortions using county-level data on abortions by age group collected by the Colorado Department of Public Health and Environment for 2004–2016.[15,16]

Because all Colorado Title X agencies accepted CFPI funding, our primary identification strategy uses distance from a zip code's population centroid to the nearest Title X clinic to establish treatment status. This approach is motivated by several recent studies documenting the significant effects of distance to women's health clinics on preventative health care utilization, abortion, contraception use and births (Lu and Slusky, 2016; Lindo et al., 2019; Packham, 2017; Quast et al., 2017; Fischer et al., 2018; Lu and Slusky, 2019). Namely, as distance to a clinic increases, the likelihood that a woman will seek family planning and health services decreases.[17] To measure the distance from a zip code centroid to the nearest

[15]These data do not separate counts by 15–17 year olds and 18–19 year olds; therefore any analysis of abortions by age group will consider 15–19, 20–24, and 25–29 year olds.

[16]In the event that an observation is censored, we assume that the number of abortions is zero.

[17]Specifically, Lu and Slusky (2016) and Lu and Slusky (2019) use a zip-code-level analyses to estimate how much Title X clinic closures affect women's take-up of breast exams, mammograms, and pap tests. They find that an increase of 100 miles to the nearest clinic leads to a decrease in these preventative care tests by 11–18 percent, and increases birth rates by 1.2 percent. Lindo et al. (2019) analyzes the effect of abortion clinic closures on clinic access and finds that closures affect women living within 200 miles of a clinic, although effects are largest for women living nearest to a clinic. In particular, increasing the travel distance by 25 miles reduces abortion by 10 percent. Packham (2017) studies the 2011 Texas Title X funding cuts resulting in 82

Title X clinic, we geocoded the addresses of each Title X clinic in the state using archived directories of Colorado clinics from 2009–2012. Using GPS coordinates, we calculate total travel distance from a centroid to a clinic using the *geonear* package for Stata, which draws on Google Maps API data (Picard, 2010). Importantly, these data allow us to observe locations of Title X clinics over time, to account for any openings or closings during this time period.

To control for time-varying zip-code-level economic conditions, we use data from the American Community Survey (ACS), which contains yearly estimates of poverty rates and unemployment. Because population data by race and age is unavailable by zip code, we additionally include population counts from the National Cancer Institute's Surveillance, Epidemiology, and End Results Program (SEER) to construct county-level measures of demographics, including the fraction of 15–17, 18–19, 20–24, and 25–29 year olds, the fraction of each age group that are black, the fraction Hispanic and demographic fractions by single age.

2.3.2 Identification Strategies

Our primary approach for estimating the effects of the Colorado Family Planning Initiative is a difference-in-differences design that uses zip codes that are farther from Title X clinics as the comparison group for zip codes closest to clinics receiving funding. In our preferred specifications, we define our treated zip codes as those within 7 miles of a Title X clinic, although we perform additional tests to provide evidence that our results are not sensitive to this choice.[18] The identifying assumption underlying this approach is that the proportional changes in births in the comparison zip codes provide a good counterfactual for the proportional changes that would have been observed in the treated zip codes in the

clinic closures and finds that contraception use by Title X clients decreased by 32 percent and teen birth rates increased by 3.4 percent as a result of reduced access to family planning services. Quast et al. (2017) and Lindo et al. (2019) use variation in abortion facility closings in Texas and finds that increases in distance to the nearest abortion facility reduces abortion rates. Fischer et al. (2018) similarly finds that increasing distance to an abortion provider reduces abortions, while increasing distance to a family planning clinic increases births.

[18] Results from a range of distances are discussed below in Section 2.4.2.

absence of the CFPI.

We begin our analysis by estimating Ordinary Least Squares (OLS) models of the following form:

$$Births_{zt} = \alpha_z + \alpha_t + \beta X_{zt} + \theta CFPI_{zt} + \epsilon_{zt} \tag{2.1}$$

where $Births_{zt}$ measures births in zip code z in year t using the inverse hyperbolic sine (IHS) of the count[19], $CFPI_{zt}$ is an indicator variable that takes a value of one in all years after the CFPI began for zip codes defined to be "near" a Title X clinic and zero otherwise, α_z are zip code fixed effects to control for systematic differences across zip codes, and α_t are year fixed effects to control for shocks to birth counts that are common to all zip codes in each year. Given that the outcome variable is the IHS of birth counts, θ can be interpreted as an elasticity and, unlike a standard log transformation, the analysis utilizes observations that have zero counts.[20] All analyses allow errors to be correlated within zip codes over time when constructing standard-error estimates.

We also report estimates that consider how the effects evolve over time with OLS models of the following form:

$$Births_{zt} = \alpha_z + \alpha_t + \beta X_{zt} + \sum_{k=0}^{7} \theta_k CFPI_{z,t-k} + \epsilon_{zt} \tag{2.2}$$

where $CFPI_{z,t-k}$ is an indicator variable that takes a value of one k years after the CFPI began for zip codes defined to be "near" a Title X clinic and zero otherwise. All other terms are unchanged from Equation 1.

We consider how the effects to vary over time for several reasons. First, the nature of contraceptive choice, sexual activity, and childbearing all would suggest that effect may grow over time after the program's implementation, even when we assign births to their year of

[19]This transformation takes on the form $sinh_z^{-1} = ln(z + \sqrt{1 + z^2})$.

[20]We have similarly considered an approach that uses birth rates based on annual population counts for each of our age subgroups based on zip code-level population counts taken decennially. While this leads to estimates that are qualitatively similar to those we present, it leads to estimates that are larger in magnitude but much less precise.

conception. In particular, the share of sexually active women using LARCs is expected to increase over time as they visit clinics and, more generally, become increasingly aware of this option. Second, the program was rolled out at the state level starting in fiscal year 2008 (i.e. July 2008), with money distributed to agencies each year.[21] Although the donated funds ceased after the summer of 2015, the state decided to continue funding the program on its own starting in 2016. Accordingly, we may expect that throughout the program's duration, effects accumulate as teens and older women continue to use LARCs and/or as information about the program spreads. At the same time, the effects on specific age groups may vary over time as women naturally age into other age groups. Finally, while the CFPI was not promoted publicly by the state during its rollout, in 2014 the Colorado DPHE released an internal report on the achievements of the initiative, which generated national news attention from 2014–2015 and which may have altered its effects.

As with any difference-in-differences design, the validity of our approach requires common trends in the outcome over time for the treatment and control groups (those near Title X clinics and those farther away, respectively). We provide support for this assumption with evidence that outcomes for these groups are not diverging from one another prior to the CFPI. We also examine data from the American Community Survey to test whether population flows constitute a threat to the the validity of the identifying assumption.

2.4 Results

2.4.1 Effects of the CFPI on LARC Use

Before presenting our estimated effects on births, we first analyze how the CFPI affected LARC uptake and client caseload. The top panel of Figure A.1 shows the per capita increase in LARC insertions by age group for women visiting Title X clinics in Colorado. While LARC insertions are increasing for women of all ages over time following the CFPI, the most striking increases are for women under age 25, with some increases to women aged

[21]The Colorado DPHE disbursed funds to local agencies starting in July 2008; however, the first year of implementation of the CFPI did not begin until January 2009.

25–29.

In the bottom panel of Figure A.1 we also address the notion that the impact of LARCs on childbearing is likely to depend on both LARC usage and on the rates of childbearing. Towards this end, the bottom panel of Figure A.1 displays the number of Title X clients choosing a LARC per the number of births by age group in 2008. When accounting for differences in births across age groups, take-up rates are highest among the youngest women.

To compare this pattern of LARC use with the use of other contraceptives, Figure A.2 shows how the primary method of contraception used by women visiting Colorado Title X clinics evolved over time. In 2008, the year before the initiative began, LARCs had a usage rate for teenagers and 20–29 year olds of less than 3 percent and 7 percent, respectively, which was lower than usage rates for condoms, injections, rings, and birth control pills. By 2015, LARC take-up among women under age 30 had risen to nearly 26 percent, surpassing all methods except oral contraceptives.

This increase in LARC use is mirrored by a decline in the use of oral contraceptives, indicating that the initiative led to a substitution of LARCs for oral contraceptives.[22] That the substitution appears to be along this margin has important implications for the effects on pregnancy. Most obviously, we would expect this sort of substitution to reduce unintended pregnancy, because LARCs are more effective than oral contraceptives. It also suggests that we would likely expect the effects to be smaller than if the program instead caused substitution away from condoms (as the primary form of contraception), because condoms are less effective than oral contraceptives.

Importantly, we have data only on Title X clinic visitors.[23] Given that IUDs can last up to 12 years, increases in LARC take-up may also reduce the likelihood that such clients visit annually, compared to those clients choosing oral contraceptives, who must visit a clinic

[22] This evidence is consistent with earlier work on the effects of the CFPI on teenagers (Lindo and Packham, 2017) and evidence on LARC takeup among teenagers across the United States when longer-acting methods—Norplant and Depo Provera—became available (Levine, 2001).

[23] If a patient visits a clinic more than once in a given year, we observe only the contraceptives chosen during the first visit.

yearly. When comparing LARC insertions versus reported LARC usage, over 81,000 more clients had a LARC inserted between 2009–2015 than those reporting using a LARC in 2015, implying that Colorado Title X clinics were responsible for approximately 28,000 insertions to women aged 15–29 between 2009 and 2015.[24]

To further demonstrate that the increases in LARC use in Colorado depart from trends in take-up across the rest of the United States, Figure A.3 depicts the difference between LARC usage among Colorado's Title X clinic clients and LARC usage among Title X clinic clients across the United States as a whole. Despite starting at a similarly low rate in 2008, LARC usage among women aged 15–29 visiting Title X clinics across the United States only grew to approximately 13 percent by 2015 versus nearly 30 percent for women aged 15–29 visiting Title X clinics in Colorado. Overall, these findings indicate that the CFPI increased the number of women seeking highly effective contraceptives.[25]

2.4.2 Estimates of the Geographic Reach of the Initiative

As described in Section 2.3.2, to estimate the effects of the CFPI on births we rely on an estimation strategy that compares changes in births for women residing in zip codes near Title X clinics, who we expect to be affected by expanded access to LARCS, to changes in births for women residing in more distant zip codes who are less likely to be affected. Because it is not clear *a priori* which zip codes should be considered "near" to Title X clinics in this context, we use a non-parametric data-driven approach to answer this question in a similar spirit to Muralidharan and Prakash (2017).

Specifically, we use the difference-in-differences model (Equation 1) to estimate the effects using a rolling distance-window to define the treated group while maintaining a constant comparison group. We consider "treated" zip codes h to $h + 5$ miles from a Title X

[24]Data on insertions is unavailable by age. Here we are assuming that the same ratio of visitors using LARCs to cumulative LARC insertions holds over time.

[25]The CFPI appears to have had a much smaller impact on contraception use among older women. For female Title X clients in Colorado over the age of 29, LARC use increased to 24.4 percent in 2015 versus 12.3 percent for clients across the US. It is for this reason that we focus our remaining analyses on women aged 15–29.

clinic for $h = 0, 1, ...15$ and use a comparison group comprised of zip codes greater than 20 miles from a Title X clinic. In so doing, we generate 16 separate difference-in-differences estimates, each intended to measure the effect on birth rates for a different set of zip codes defined based on their distance from a Title X clinic. The results are shown in Figure A.4, with separate panels for the estimated effects on births for different age groups.

As expected, we see significant estimated effects for the specifications in which the treatment group is comprised of zip codes that are especially near Title X clinics. For example, the very first estimate plotted in Panel A indicates that after the CFPI births to 15–17 year olds fell approximately 10 percent more for zip codes 0–5 miles from a clinic than they fell for zip codes greater than 20 miles from a clinic. The estimated effect becomes larger when we consider women in zip codes 1–6 miles but then shrinks towards zero as we evaluate the effects on women farther and farther away. The estimated effect on births to 15–17 year olds is no longer statistically significant at the five-percent level when we consider those 7–12 miles from a clinic. The pattern of estimates is similar when we instead consider births to 18–19 year olds (Panel B). There is relatively little evidence from this analysis that births to 20–24 year olds or births to 25–29 year olds are affected (Panels C and D).

Overall, these estimates imply that the geographical reach of the CFPI—in terms of having a measurable impact on birth rates for some groups of women—was 7 miles. These results determine our definition for treatment and control groups in all subsequent analyses. In particular, in our subsequent analyses, we define our treatment group as those zip codes within 7 miles of a Colorado Title X clinic, and compare changes in births in these zip codes to changes in births in zip codes further than 7 miles from a clinic. Because clinics did serve some women living in more-distant zip codes, this approach can be viewed as estimating a lower bound of the true treatment effects. We also note that any estimates based on this research design will represent intent-to-treat estimates, because only a small share of the population is treated; thus, our estimates will understate the effects of the program on the women it actually served.

To better understand how these groups of zip codes compare to one another with respect to observable characteristics, we present summary statistics for variables used in our zip code-level analysis in Table A.1.[26] Specifically, Table A.1 compares average birth counts, demographic variables, poverty rates, and unemployment rates for zip codes within 7 miles of a Title X clinic and zip codes more than 7 miles from a Title X clinic. Notably, the treatment group—zip codes closest to a Title X clinic—has higher average birth counts in all years (for example, 12.97 versus 3.29 for 18–19 year olds), which in large part reflects the fact that these zip codes also are more highly populated. This table additionally reports travel distance information and travel time information, although these variables do not change over the sample period.[27] While the zip codes in the treatment group may be up to 7 miles from a clinic, on average they are 3.6 miles to their nearest clinic which corresponds to a 9 minute drive. Zip codes further than 7 miles are an average of 22.2 miles from the nearest clinic, corresponding to 53 minutes of driving time.

2.4.3 Graphical Evidence of Trends

Before discussing our preferred estimates of the effects of the CFPI on childbearing, we first present graphical evidence to support our main results and the validity of our research design. In Figure A.6 we present an event-study-styled graph showing difference-in-differences estimates of the effects on births over time, including periods of time prior to its implementation. The outcome variable in each panel includes the inverse hyperbolic sine of births. In each panel, the black circles represent the estimated effects at different points in time from a baseline model controlling for zip-code and year fixed effects.[28] The comparison group includes all Colorado zip codes farther than 7 miles from a clinic. The figure also shows estimates based on a model that additionally controls for time-varying measures of demo-

[26] We also provide a geographical visualization of Title X clinic locations and zip code distance in Figure A.5.

[27] Driving time is calculated using the *geonear* command in Stata 15, which is based on information from Google Maps API.

[28] In estimating the effects over time, the year prior to the implementation of the CFPI, 2008, serves as the omitted category.

graphics and economic conditions (gray squares).[29] Additionally, in an attempt to compare zip codes that are most alike, we also show estimates from a third specification that limits the analysis to zip codes within 15 miles of a clinic (blue diamonds). Effectively, this approach involves a comparison of zip codes within 7 miles of a clinic to zip codes 7–15 miles from a clinic. Finally, the figure also shows estimates from a specification that includes year, zip and county-by-year fixed effects to allow for zip codes in separate counties to experience differential shocks by county over time (green triangles). Essentially, this specification uses other zip codes *within the same county* as the comparison group for each zip code within 7 miles of a clinic.

Although the exact magnitudes for each estimate in Figure A.6 are difficult to discern, the results as a whole reveal some clear patterns. Specifically, the sets of estimates indicate that births in treated and control zip codes followed a similar trajectory prior to the adoption of the CFPI, which provides support for our common trends assumption. This evidence is particularly strong for the 15–17 and 18–19 year old age groups, which is where we find strongest and clearest evidence of impacts on births. The estimated effects in the pre-period are more volatile and less precisely estimated for the older age groups.[30] The pattern of estimates also provides evidence that the CFPI reduced births, particularly for 15–17 and 18–19 year old women. There is suggestive evidence of effects for older women during the sixth and seventh years of the CFPI, something we explore in greater detail below.

2.4.4 Main Results

In Table A.2 we present our main results. They are based on the difference-in-differences model specified by Equation 2.2 and, motivated by our analysis of the "geographic reach" of the CFPI, they use women in zip codes more than 7 miles from a clinic as the comparison group for women in zip codes within 7 miles of a clinic. Specifically, all estimates are based

[29] These controls include zip-code-level unemployment rates and poverty rates and county-level fractions of individuals aged 15–29 by age, ethnicity, and race.

[30] Confidence intervals are not shown in the figure so the estimates from different specifications can be seen more clearly.

on a model that includes year and zip code fixed effects; the estimates shown in even columns are based on a model that additionally controls for time-varying demographic and economic characteristics. As before, we separately estimate the effects for women aged 15–17, 18–19, 20–24, and 25–29.

Columns 1–2 and 3–4 present results for 15–17 year olds and 18–19 year olds, respectively. In Columns 1–2, estimates are negative and statistically significant for all years, and indicate that the CFPI reduced childbearing for women aged 15–17 living within 7 miles of a clinic by 20 percent over 7 years. Estimates shown in Columns 3–4 indicate a similar, albeit slightly smaller, effect for women aged 18–19. Specifically, they indicate that the CFPI decreased births for women aged 18–19 living within 7 miles of a clinic by 18 percent. As a point of comparison, (Lindo and Packham, 2017) documented effects of approximately 6 percent for 15–19 year olds residing in counties with Title X clinics. These earlier estimates obviously masked substantial heterogeneity in the effects, which depend on how far teenagers live from clinics.

We also find that the effects become larger throughout the duration of the program, suggesting that as more women receive LARCs, more unintended pregnancies are prevented over time. Overall, our estimated effects correspond to over 300 fewer births to 15–17 year olds per year and 586 fewer births to 18–19 year olds per year across the state of Colorado as a result of the program–or 4,400 births over 7 years.[31]

In Columns 5–8, we present estimates for women in their twenties. Consistent with the graphical evidence from Figure A.6, there is not any clear evidence of effects for women women aged 20–24 (Columns 5 and 6) or women aged 25–29 in the short run. However, we do see evidence of significant effects (ranging from 12–16 percent) on 20–24 year olds 6–7 years after the implementation of the CFPI. Similarly, there is also evidence of significant effects (ranging from 8–10 percent) on 25–29 year olds 6–7 years after the implementation of the CFPI. Overall, estimates in Table A.2 demonstrate that the CFPI had large and immediate

[31]This is based on the fact that the average annual number of births to 15–17 year olds and 18–19 year olds per zip code is 2.547 and 5.60, respectively, and there are 588 zip codes in our sample.

impacts on births to high-school aged teenagers and older teenagers. While the CFPI also appears to have reduced births to women in their twenties, these effects are not evident until many years after the CFPI was implemented. We explore this finding in greater detail in the next section.

2.4.5 Exploring the Role of Advertising and Awareness

Though the CFPI was implemented in 2009 and eventually received international news coverage, it was not covered by any local, national, or international media outlets for many years.[32] The program's exposure changed after the Colorado DPHE released an internal report on the achievements of the initiative and published an academic paper in *Perspectives on Sexual and Reproductive Health* in 2014, one year before the initiative was set to run out of funding.[33] This sparked media attention to the program beginning in July 2014 and including a front-page story in the New York Times with the headline "Colorado Finds Startling Success in Effort to Curb Teenage Births" (Walker, 2015).[34] This coverage was sustained through 2015 and 2016 as policy-makers vigorously debated whether to provide funding to continue the program.[35,36]

Our main results, shown in Table A.2, provide suggestive evidence that media attention heightened the effects of the initiative. Specifically, the widespread coverage of the program

[32]The initiative was not marketed by the state and we have only been able to find a single media mention of the initiative prior to 2014, which was in the context of an article published 5/6/2013 in *Windsor Now!* headlined "Plan B still banned from county clinics." The Colorado Family Planning Initiative was simply mentioned in this article as having been approved by Weld county commissioners at the same hearing where the commissioners said they would stand by their prior decision to keep Plan B out of county health clinics.

[33]The Susan Thompson Buffett Foundation provided funding for the CFPI from July 2008–June 2015, with all funds expiring in the summer of 2015.

[34]Other outlets covering the program included CNN, Denver Post, NPR, The Guardian, and Vox (Schmidt, 2014; Lopez, 2014; Frank, 2015; Popovich, 2015; Horsley, 2015).

[35]In May 2015, the Colorado state senate voted to kill a bill allocating $5 million in funding for the initiative. After a push for continued funding by the DPHE, in August 2015, state officials announced that a group of organizations pledged $2 million to fund the program through the summer of 2016, with no promise of renewed funding in subsequent years (Paul, 2015). Following the end of these private funds, in 2016 the Colorado Senate voted 30-5 to pass a budget bill that included $2.5 million in Title X funding for LARC provision. The Colorado DPHE has since stated that this pledge of new funds was partially due to the reported benefits of LARC access and public awareness of the successes of the CFPI (Colorado Department of Public Health and Environment, 2017).

[36]Politicians and other advocates gained significant media attention by wearing IUD-shaped earrings at the proceedings (Vagianos, 2015).

aligns with our finding that the CFPI had significant effects on the number of births to women in their twenties only in its sixth and seventh years, and not before.

To further assess the role of advertising, we explore the share of clients having a LARC inserted at Title X clinics in Colorado. Specifically, in Figure A.7 we display LARC insertions per female Title X client for each year, 2009–2015.[37] Notably, “insertion" in this context is different from “usage" (as reported in Figure A.1) because usage statistics reflect both new and existing LARCs at time a woman leaves a clinic. As shown in Figure A.7, the number of LARC insertions per Title X client initially grew slowly (by 3.1 percent from 2009–2010), then grew steadily by 7.3–9.8 percent annually from 2010–2014 before it spiked by nearly 16 percent from 2014 to 2015. This jump is consistent with the idea that the significant media attention in 2015 caused a spike in women's interest in visiting Colorado's Title X clinics to obtain a LARC. It is also important to note, however, that the CFPI *was* covered by some media outlets in the second half of 2014 and we do not see any evidence of a trend break between 2013 and 2014. As such, these results suggest that there was a delayed effect of the 2014 coverage and/or that the 2015 reporting was more impactful. Indeed, the 2015 spike may have been in part (or entirely) due to increased awareness of the possibility that clinics would have to stop providing LARCs without additional funding.

As a third and final approach to assessing whether the effect of the CFPI was different after extensive media coverage, we re-examine the geographic reach of the initiative with a focus on that period of time. This analysis is similar to that discussed in Section 2.4.2, where we showed that the initiative reduced births for women up to 7 miles from clinics when evaluating all years after the CFPI was implemented; here we instead focus on the estimated effects for 2014 and 2015, separately. In particular, we estimate Equation 2.1 using a dataset containing all pre-CFPI years and only the relevant post-initiative year (i.e. 2014 or 2015) in an effort to parse out the year-by-year reach of clinics.

These results, shown in Figure A.8, offer several additional insights. First, they indicate

[37] Unfortunately, these data are not available after 2015. They also do not include separate counts by age group; thus, we focus on the numbers overall.

that the geographic reach, in terms of reducing births for 15–17 year olds, was larger in 2015 than in 2014. In 2014, the estimates are statistically significant for each 5-mile treatment group that we consider until we evaluate women in zip codes 5–10 miles from a clinic. In 2015, the estimated effects are larger in magnitude and continue to be statistically significant until we evaluate women in zip codes 12–17 miles from a clinic. The results shown in Figure A.8 also provide stronger evidence of effects on births of 20–29 year olds in 2015 than in 2014, which mirrors our results shown in Table A.2.

As a whole, these analyses provide several new pieces of evidence to suggest that media coverage of the initiative increased women's interest in obtaining LARCs through Title X clinics and expanded the impact of the program to women in their 20s and also to high-school-aged teenagers living relatively far away from clinics. Taken together with our main results, these findings can inform policy questions on how the spread of information can encourage highly effective contraception use and how far women are willing to travel for low-cost contraceptives.

2.4.6 Additional Subgroup Analyses

Across all of our analyses, we find consistent evidence that the CFPI reduced births for women under the age of 29. We have also shown that the effects are greatest for teenagers (both high-school and post-high-school aged) and that they become significant for women in their 20s after the initiative received widespread media attention. In this section and the following section we further explore the degree to which there were heterogeneous treatment effects across different subgroups of women and zip codes, whether there were effects on abortion, and whether there were effects on infant health.

Given that the CFPI provided funding for Title X clinics, we might expect birth rates to be more responsive in areas with a relatively large share of individuals living in poverty. That said, given that these clinics provide services based on self-reported income and because young women typically have low incomes, the effects might be similar across areas with different levels of poverty. We explore this issue in Table A.3 by separately evaluating subgroups of high- and low-poverty zip codes. We define high-poverty zip codes as those having more than the median 2010 poverty rate and define low-poverty zip codes as having rates below this median. The estimated effects 3–7 years after implementation are nearly all statistically significant across Columns 1–4, implying that the CFPI reduced births to teens living in both high- and low-poverty areas. Average effects for 15–17 year olds and 18–19 year olds are statistically similar across these subgroups, and indicate a reduction in births of 15–21 percent. For women in their 20s, however, the effects on births appear to be driven by low-poverty zip codes, with estimates indicating reductions caused by the CFPI from 12–18 percent. Effects for high-poverty zip codes for these age groups are statistically insignificant for all years, and are much smaller in magnitude (1–2 percent). We note that these findings may be explained by the composition of individuals living in these lower income zip codes. Although high-poverty and low-poverty groups have similar average distance and travel time to clinics, high-poverty zip codes have have higher concentrations of Black and Hispanic women. Below, we explore to what degree estimated effects for Black and Hispanic women drive our main findings.

In Table A.4 we analyze how much the effects of the CFPI vary across race and ethnicity. In Columns 1, 4, 7, and 10 we present estimates of the effects of the CFPI on births to White mothers, in Columns 2, 5, 8, and 11, we present estimates for Black mothers, and in the remaining columns, we show estimates for Hispanic mothers. Across all age groups, the estimated effects for Hispanics are larger than average. Estimates for white mothers are similar to our main results. Estimated effects for black mothers are similar to Hispanics for those 15–17 years old but we find no evidence of effects for the 18–19 year-old age group. And while we do find evidence of effects for the 20–24 year-old group of black women, it is smaller than the estimated effects for white and Hispanic women. Likewise, we find no evidence of effects for black women in the 25–29 year-old age group, although we note that only about 7 percent of Colorado Title X clients are Black, compared to 30 percent and 45 percent for White and Hispanic clients, respectively.

Finally, since Title X clinics are most likely to locate in urban areas, we investigate whether effects are driven by women in urban or rural zip codes. We use the United States Department of Agriculture's Rural-Urban Commuting Area (RUCA) codes to classify each zip code as either urban or rural and estimate the effects separately for rural and urban zip codes.[38] The results of these analyses are presented in Table A.7. Columns 1 and 2 show estimates from our preferred specification for 15–17 year olds at rural and urban zip codes, respectively. Columns 3–8 repeat this process for 18–19 year olds, 20–24 year olds, and 25–29 year olds. While the estimates are typically imprecise for women living in rural zip codes, as a whole these results indicate that the effects are largely similar for women in rural and urban areas.

2.4.7 Estimated Effects on Abortion

Thus far we have shown that the CFPI decreased childbearing for women aged 15–29. These findings indicate that access to highly effective contraceptives decreased unintended pregnancies that otherwise would have resulted in births. It is important to note that these estimated effects may understate the effects on unintended pregnancy overall, particularly if the program also reduced unintended pregnancies that otherwise would have resulted in abortions.[39] This is not a trivial issue given that 29 percent of pregnancies to teenagers—and 26 percent of pregnancies to women aged 20–24—end in abortion (Kost et al., 2017).

To investigate whether the CFPI also reduced unintended pregnancies that would have been terminated, in this section we present difference-in-differences-type estimates for abortion rates by age group (15–19, 20–24, 25–29).[40] For this analysis we use annual county-level abortion counts to construct an inverse hyperbolic sine measure of abortion rates.[41]

[38]Zip code-level classifications can be found at https://ruralhealth.und.edu/ruca and were created from USDA census tract data, by the University of North Dakota's Center for Rural Health. We classify a zip code as 'rural' if its RUCA code is 4.0 or higher.

[39]We also consider the extent to which clinics that provide abortions affect our main results. Only one Title X clinic in Colorado is affiliated with an abortion provider—Boulder Women's Health. When dropping this clinic and the surrounding zip codes from the main births analysis, estimates are statistically similar at the 1 percent level.

[40]Separate breakdowns for younger (15–17) and older (18–19) are not available for this analysis.

[41]Specifically, this measure is constructed as $IHS(count) - IHS(population)$ as we could typically do if

Moreover, we measure a county's exposure to the CFPI based on the percent of its population living within 7 miles of a Title X clinic.[42] Our empirical model includes both county fixed effects and year fixed effects and clusters standard error estimates at the county level.

Estimated effects for all years leading up to—and following—the implementation of the CFPI are presented in Figure A.9, while estimates from our preferred model (excluding lead terms) are presented in Table A.5. In general, the estimates are too imprecise to draw any strong conclusions. As we would expect, the estimates are more precise in Table A.5 where they indicate a statistically significant effect on teenagers. However, to some degree the event-study-type estimates in Figure A.9 raises the possibility that this may be in part due to somewhat differential trends prior to the CFPI. As a whole, we interpret the results of this analysis as providing suggestive evidence of effects on teenage abortion rates. A more cautious interpretation is that we cannot rule out very large or very small effects on abortion rates.[43]

2.4.8 Estimated Effects on Infant Health

To provide an even more comprehensive picture of the health effects of the CFPI, we extend our analysis to study whether the CFPI affected births that tend to involve relatively high hospital costs. Specifically, we use the same empirical strategy as our main results, but focus on births involving low birth weight (less than 2500 grams), very low birth weight (i.e. less than 1500 grams), or low Apgar scores (less than 9).[44] The results of this analysis are shown in Table A.6. Column 1 shows the estimated effects of the CFPI on all births to women aged 15–29, while Columns 2-4 show the estimated effects on subsets of these births involving low birthweight births, very low birthweight births, and births in which the child

evaluating the natural log of a rate variable.

[42] This county-level measure of exposure is constructed as the fraction of people in a county that live in a zip code whose population centroid is within 7 miles of a clinic.

[43] We have similarly estimated analogous to the procedure described above to analyze effects on county-level chlamydia and gonorrhea rates to evaluate whether lowering the cost of obtaining contraceptives increases more risky sexual behavior. Estimates are too imprecise to be meaningful.

[44] The Apgar is a test score scaled from 1-10 and serves as a measure of the status of the newborn immediately after birth. Nearly 82 percent of infants in our sample score either a nine or ten. Scores between four and seven indicate that some assistance for breathing and/or resuscitation might be required.

scored less than a 9 out of 10 on the 5-minute Apgar test, respectively.

Estimates in Column 1 largely reinforce our main findings—that the CFPI reduced births to women aged 15–29 by 7.9 percent, with larger effects in later years. Estimates in Columns 2–4 indicate that the CFPI also had effects on births that typically involve more-than-average hospital care. In particular, the CFPI reduced the number of low birthweight infants by 11.9 percent, and very low birthweight infants by 9.0 percent. This corresponds to approximately 482 fewer infants that may require extra care in the hospital per year.[45] Similarly, as shown in Column 4, we find that the CFPI reduced the number of infants with low 5-minute Apgar scores. Estimates in Column 4 indicate that the CFPI reduced the number of infants scoring 1-8 on the test by 23.5 percent 6-7 years after the program's initiation, suggesting that the CFPI led to an improvement in the overall health of infants born to women aged 15–29. As a whole, the estimates in Table A.6 demonstrate that the CFPI reduced births that tend to involve relatively high hospital costs.[46]

2.5 Validity and Robustness

In this section we present a set of sensitivity checks to provide additional support for our main identifying assumption. We begin by addressing the possibility that differential trends in population flows and/or zip code demographics might be confounding our estimated effects. However, before presenting results from additional analyses, we note the evidence we provided for the common trends assumption (Figure A.6) suggests that it is unlikely that pre-existing trends in population flows and/or demographics are driving our results. To provide further evidence along these lines, we use data from the two most recent decennial censuses to show that treated and comparison zip codes did not change differently between 2000 and

[45]This is based on the fact that approximately 6.3 births to women aged 15–29 are considered low birthweight, while 0.9 births to women aged 15–29 are considered very low birthweight, per zip code, per year, on average.

[46]Though the point estimates typically suggest the effect is larger for these types of births than births overall, the standard errors are too large to reject that the effects are the same at conventional levels of statistical significance.

2010, by evaluating the difference-in-differences in demographics across these years.[47] We additionally show a measure of predicted births based on these population estimates, as a way to gauge whether our estimates are large in comparison to what we would expect in the absence of the intervention. We present these estimates in Table A.8.[48] We find no statistically significant effects on the number of women 15-29 years old or the share of 15-29 year old women who are white, black, or Hispanic. We also find no statistically significant effects on the predicted number of births based on these variables.[49] That said, we recognize that a lack of power means that we cannot rule out economically significant effects at conventional levels of statistical significance.

Second, we test how robust our analyses are to the definition of distance. In Table A.9, we redefine treatment using additional distance measures: “as the crow flies" distance and driving time, in minutes, according to Google map data. To do so, we first consider our treatment group as those zip codes within 7 miles of a clinic, based on crow flies distance, and display these difference-in-differences coefficients in Panel A of Table A.9. Across specifications for all age groups, estimates are consistent with our baseline results in Table A.2. In Panel B, we define our treatment group to be zip codes within 10 minutes driving time of a clinic, which most closely resembles a 7 mile travel distance range. Estimates are similar in magnitude and direction as our main results.

Next, we show how redefining our treatment group by the number of miles affects our estimates. Specifically, in Table A.10 we show what the estimated effects are if we instead defined the treatment group as zip codes within 0–5 miles or as zip codes 0–10 miles of a Title X clinic. Estimates for all treatment definitions are statistically similar to our main results, and indicate reductions in births ranging from 16–20 percent for 15–17 year olds and

[47] Ideally we would examine the same set of years that we examine in our main results; however, this analysis is constrained by the availability of Census data.

[48] The coefficients are transformed to represent the change expected over one year.

[49] Specifically, we estimate predicted births using a two-step procedure. First, we use 2010 data to evaluate the inverse-hyperbolic-sine of the number of births in each zip code as a function of the aforementioned variables. Second, we estimate the *predicted* inverse-hyperbolic-sine of the number of births for each zip code in 2000 and 2010 based on the coefficient estimates from step one combined with the observed demographics.

15 percent for 18–19 year olds.

Furthermore, because we measure treatment effects based on distance, any openings or closings of Title X clinics during our sample period could bias the estimated effects. This is especially relevant given recent literature documenting that changes in clinic access affects birth and abortion outcomes (Lu and Slusky, 2019; Packham, 2017; Lindo et al., 2019; Fischer et al., 2018). Therefore, in Table A.11 we provide estimates from models that drop zip codes which experienced Title X clinic openings and closures during this time period. Specifically, the Colorado Department of Public Health and Environment reports 7 openings from 2009–2015, while two clinics closed.[50,51] Columns 1, 4, 7, and 10 provide the baseline estimates for comparison, using all zip codes across the state of Colorado. Columns 2, 5, 8 and 11 omit zip codes which experienced Title X clinic openings from 2009–2015, and Columns 3, 6, 9 and 12 omit zip codes which experienced Title X closures. Estimates are similar across groups, indicating that changes in clinic openings are not driving our estimated effects on births.

Finally, we provide weighted least squares (WLS) estimates in Table A.12 in an effort to get a better sense of how the CFPI differentially affected zip codes with dissimilar population sizes. In Columns 2, 4, 6, and 8, each cell is weighted by the zip code's female population according to the 2010 ACS. Notably, these alternative estimates are less precise than our main results. They are also smaller in magnitude than the OLS estimates, which suggests that there are relatively large effects for zip codes with relatively small populations for the reasons described in Solon et al. (2015). We explore this potential heterogeneity directly in Table A.13 by presenting estimates for zip codes with less than 2,000 total females.[52]

[50] Openings include the Denver Health affiliated opening in 2009, a Tri-County Health Department opening in 2011, 3 openings across the city of Denver affiliated with the Denver health clinic in 2012, and 2 Denver openings affiliated with the Colorado Coalition for the Homeless in 2012. Closures include Title X clinics in Stratton and Crested Butte.

[51] Given that 6 out of 7 clinic openings occur in Denver, we also consider a specification that omits all Denver zip codes, which will provide more conservative estimates of the initiative. Estimates are statistically similar across all age groups, indicating that these zip codes are not solely responsible for the main results.

[52] We choose this cutoff for two reasons. First, the median female population is about 1,900 and second, in Table A.7 we find differential effects for rural areas, which have, on average, about 1,500 females. Effects for specifications that limit our sample to 1,500 individuals are qualitatively similar.

Importantly, restricting attention to less-populated zip codes limits our number of treatment zip codes to only 26, as compared to 163 in the full sample. Therefore, it may be unsurprising that estimates in some columns of Table A.13 are fairly imprecise. However, we note that these estimates do provide some suggestive evidence that estimates are comparatively large for less-populated zip codes for all age groups except 20–24 year olds, which sheds light on why the OLS estimates are relatively large in magnitude as compared to the WLS estimates.

2.6 Conclusion

In this section, we document the effects of expanding access to highly effective contraceptives through the lens of the Colorado Family Planning Initiative, which provided free LARCs to low-income women at Title X clinics. Using zip-code-level Natality data, we show that the initiative reduced birth rates for women living in within 7 miles of a Title X clinic until the initiative received extensive media coverage. Afterwards, the effects extended to women living farther away from clinics and to non-teenagers. Despite the fact that any Title X client was eligible to participate in the program, we see little to no effects for women living farther than 12 miles from a clinic.

As a whole, our estimated effects correspond to nearly 6,800 fewer births to women aged 15–29 over 7 years. Moreover, we find that the effects on births are largest for women between ages 15–17 and 18–19. Along similar lines, we find suggestive evidence of impacts on abortion for women in these age groups, which suggests that the effects on births likely understate the effects on unintended pregnancy. Given that we observe significant effects on both high-school-aged and post-high-school-aged women, we believe an important next step may be to investigate whether these effects translate into impacts on women's educational and economic outcomes.

Our findings highlight that the effects of funding Title X clinics can be substantial, and that advertising may help them extend their reach, particularly when women may not be aware of the full range of services and contraceptives that the clinics offer. As such, our findings complement recent work documenting statistically significant but imprecise evidence that LARC use can be increased through social media (Byker et al., 2019). Our work also complements earlier work demonstrating that expanding low-income women's access to family planning services can significantly reduce childbearing. In particular, researchers have demonstrated that Medicaid waivers in the early 1990s to mid-2000s had significant effects on teen childbearing (Kearney and Levine, 2015) and non-teen childbearing (Kearney and Levine, 2009), and also that the county-level rollout of Title X reduced and delayed childbearing (Bailey, 2012).[53]

This line of research is especially relevant in light of recent federal policy changes which will cut family planning funding and allow more employers to deny contraception in their health insurance plans. Our results, which demonstrate how access to highly effective contraception affects unintended pregnancy in the modern context, suggests that such policies are likely to increase childbearing and perhaps abortion. Given the well-established link between childbearing and women's long-run outcomes, it will become increasingly important for future research to evaluate the effects of these policies on a wide range of outcomes.

[53] Along similar lines, research investigating the effects of *reduced* funding to family planning clinics in Texas, which caused many clinics to close, finds significant increases in birth rates caused by the funding cuts (Packham, 2017; Lu and Slusky, 2019).

3 WHEN CAPACITY CONSTRAINTS BIND: EVIDENCE FROM REPRODUCTIVE HEALTH CLINIC CLOSURES

3.1 Introduction

Health care spending makes up approximately 18% of American GDP (Martin et al. (2018)), yet it is difficult to measure the degree to which health care services affect health outcomes. Access to health care is largely endogenous: healthy (or unhealthy) people may differentially select into areas with better local health care systems; wealthier people may spend more on health care but may also have better underlying health than the less wealthy; communities with better health care facilities may have invested in these facilities because their population was already sicker than average populations, etc. In any of these cases, comparisons of those with better access to health care services to those with worse access will not reflect the causal effects of access to health care. To establish a causal relationship between health care and health outcomes, researchers need a source of exogenous variation in access to health care. Toward this end, researchers have used changes in insurance (Anderson et al., 2012; Courtemanche et al., 2017; Finkelstein et al., 2012; Kolstad and Kowalski, 2012), shocks to local hospital quality (Doyle, 2011), and exogenous closures to local health care facilities (Countouris et al., 2014; Fischer et al., 2018; Lindo et al., 2019; Venator and Fletcher, 2019; Quast et al., 2017). This work follows in the footsteps of the facility closures literature: I use exogenous closures to study the impact of reduced access to health care services on health outcomes.

Health care facility closures have been used to study the impacts of health care on various outcomes, including mortality (Countouris et al., 2014) and reproductive health (Colman and Joyce, 2011; Fischer et al., 2018; Lindo et al., 2019; Venator and Fletcher, 2019; Quast et al., 2017). In each of their settings, authors argue that the closure of a facility provides an exogenous shock to health care access.[1] Closure of any facility results in two potential

[1] Sometimes these closures occur due to lack of profitability (Countouris et al., 2014), while new govern-

effects: increases in distance for some clients to reach a provider, and a higher number of potential clients at the facilities that remain open—yet it is difficult to separately identify the effects of these two mechanisms. Both mechanisms could be important: traveling further for health care can be costly and could prohibit use of services, but a congested facility may not be able to service demand and may be forced to turn away patients. Research has documented the importance of distance in access to health care services (Colman and Joyce, 2011; Countouris et al., 2014; Fischer et al., 2018; Lindo et al., 2019; Venator and Fletcher, 2019; Quast et al., 2017), but does a change in the number of potential clients per clinic alone impact health care use?

In this project, I exploit a natural experiment in Pennsylvania in which all new and existing abortion clinics were required to meet the same standards as ambulatory surgical facilities. These regulations were primarily related to construction of the building and staffing requirements. The new standards were costly to implement and caused the closure of 9 of the state's existing 22 abortion providers.[2] Notably, these closures all occurred in urban areas where other clinics remained open. As such, they provide a setting in which a region's total clinic capacity changed while distance to the nearest clinic did not. I use a difference-in-differences approach to estimate the causal effect of reduced clinic capacity on abortion and fertility outcomes by comparing counties in Pennsylvania with few or no clinic closures (and therefore little to no change in clinic capacity) to those with major clinic closures. I then verify these results using a synthetic control method, and find the results are very similar across both approaches.

Results suggest that this reduction in clinic capacity reduced abortion access to women in Pennsylvania—in the second year the laws took effect, the overall abortion rate was approximately 13% lower than would have been expected in the absence of the closures, though

mental regulations force closures in other settings (Colman and Joyce, 2011; Fischer et al., 2018; Lindo et al., 2019; Venator and Fletcher, 2019; Quast et al., 2017).

[2]While these types of regulations are increasingly popular among the United States, Pennsylvania's legislation was pushed forward after the discovery of an illegally-operating clinic in Philadelphia. The clinic was not meeting the standards in place at the time, yet the stories that came from this particular rogue clinic gave legislators the public support they needed to pass these laws.

this result is not statistically significant. Effects estimated by the synthetic control method indicate that reduced clinic capacity reduced overall abortion rates by an average of 9.3% per year in each of the years after the closures, but this estimate is not statistically significant. Both the difference-in-difference and the synthetic control method results provide strong evidence that reduced clinic capacity delayed the timing of abortions. Difference-in-difference estimates suggest reduced clinic capacity reduced the rate of abortions occurring within the first 8 weeks of gestation by 39.2% and increased the rate of abortions occurring in weeks 9–10 by 49.1%, weeks 11–12 by 31%. These effects are statistically significant and consistent across various robustness checks, such as including control variables, adjusting functional form, and redefining the comparison group, among others. Synthetic control estimates are similar in direction, statistical significance, and magnitude. Using the difference-in-difference estimates, this amounts to a reduction of approximately 3,600 abortions taking place in the first 8 weeks of gestation and an increase of 1,900 and 580 in weeks 9–10 and 11–12, respectively. In addition, I test for effects of reduced clinic capacity on birth rates by mother's race. Using the difference-in-differences approach, I find 3% increase in the overall birth rate which seems to be driven by births to white mothers, though synthetic control estimates for these outcomes are too noisy to provide conclusive evidence.

Obtaining an abortion later in the pregnancy could have serious implications for a few different reasons: first, the choice set for abortion procedures falls as gestational age increases—medical abortions are only available in Pennsylvania through the 10th week of gestation, and some of the state's providers will not provide surgical abortions past week 18. Second, abortion services get more expensive for pregnant women as time goes on.[3] Third, as gestation continues, the risk of serious complications from abortion procedures increases (Buehler et al., 1985).

This work most directly contributes to literature and discussion around concerns regard-

[3]Planned Parenthood of Western Pennsylvania currently reports fees increasing from $435 in the first 11 weeks of gestation to $540 in weeks 12-13, over $815 in weeks 14-16, and over $915 after the start of week 17.

ing health care facility capacity in light of growing demand and increasingly tight budgets. While the context of abortion is specific, the results from this study can be extrapolated to other health settings in which the timing and tempo of treatment are important. If local health care networks face reduced capacity, all health care services could be impacted, but some impacts will be felt more strongly than others. First, consider reduced local capacity for annual wellness exams. For the healthy adult population for whom an annual wellness exam would not have uncovered underlying conditions, delaying the timing of an annual exam may not have large impacts on overall health and wellbeing. On the other end of the spectrum, consider a patient suffering appendicitis: reduced local capacity to the point of delayed treatment could be deadly. In many cases, hospitals would be able to prioritize treatment of life-threatening conditions, but the consequences of delays in these settings are tangible and economically meaningful. Somewhere between the two extremes lie health outcomes like care for chronic conditions, cancer testing and treatment, and treatment for mild spells of viruses such as influenza or COVID-19. I argue that these are conditions—like abortion—for which the timing and tempo of treatment can have large impacts on patients, both through the health risks and through financial concerns. Delaying abortion can lead to higher costs and higher health risks; delaying cancer testing may lead to higher complication rates or more expensive treatment procedures; delaying testing of and treatment for viruses like the flu or COVID-19 may lead to more severe conditions or higher rates of infection among the population surrounding the patient. The salience of the consequences of delayed treatment may vary for each condition, but in each case delaying timing of treatment could put patients at higher health risk or could increase costs to the patient for treatment later on.

This project also contributes to the literature on access to reproductive control technology in the modern landscape. Access to reproductive control technology improves women's ability to avoid unintended births, which has been documented to improve economic conditions for women (Bailey, 2006a; Bailey et al., 2012, 2019; Goldin and Katz, 2002; Myers, 2017).

Abortion is also one of the safest medical procedures to obtain,[4] yet abortion providers have become an increasingly regulated medical body. According to the Guttmacher Institute (2019), 24 states currently have "laws or policies that regulate abortion providers and go beyond what is necessary to ensure patients' safety." These regulations have caused clinics to close their doors (Colman and Joyce, 2011; Fischer et al., 2018; Lindo et al., 2019; Venator and Fletcher, 2019; Quast et al., 2017), and the closures have reduced access to both abortion and family planning services—all with no evidence that these regulations actually improve the safety of abortions (Roberts et al., 2018).[5] These new barriers to abortion access—in addition to better knowledge and use of effective contraception—imply that changes in access to abortion services in the modern landscape may generate different effects than changes in earlier decades.

Supply-side abortion regulations, or regulations that target the abortion-providing facilities rather than individuals seeking abortions, have become increasingly popular over time. Work has shown that distance to the nearest abortion clinic is a crucial factor in access to abortion services (Colman and Joyce, 2011; Fischer et al., 2018; Lindo et al., 2019; Venator and Fletcher, 2019; Quast et al., 2017). However, to date there is little evidence of the importance of clinic congestion, and the evidence that exists is somewhat conflicting. Lindo et al. (2019) find that clinic congestion reduced abortion rates, but Venator and Fletcher (2019) find no effect of increased clinic congestion on abortion rates. In a setting in which distance remains unchanged, does an increase in the number of potential clients per open clinic impact abortion rates, the timing of abortions, or birth rates?

The remainder of this section is organized as follows. In the next section I provide back-

[4] According to National Academies of Sciences, Engineering, and Medicine (2018), abortions are safe and effective, and complications are rare. All four of the main abortion methods (medication, aspiration, dilation and evacuation, and induction) were studied. Additionally, according to the Pennsylvania Department of Health's Annual Abortion Reports, the total complication rate in any given year of this study ranged from 0.001 to 0.005. This means that in Pennsylvania, in a given year, only $1/10$th of a percent to $1/2$ of a percent of all abortions had some kind of complication.

[5] To my knowledge, there is no causal work that shows the impact of ambulatory surgical facility standards on complication rates or other adverse outcomes related to abortion. Roberts et al. is a correlational study that finds that differences in abortion-related adverse events for women who obtained abortions in ambulatory surgical centers relative to women who obtained abortions in office-based settings are not statistically significant.

ground information on abortion provider regulation guidelines and the natural experiment setting in Pennsylvania. Then I discuss the data and methods I use to analyze the causal effects of reduced clinic capacity on abortion and fertility outcomes and present the main results of the analysis. I next show heterogeneous treatment effects for abortion, effects on birth rates, and robustness tests. I then discuss other possible mechanisms, including out-of-state travel. Lastly, I conclude and discuss the implications of this and similar policies.

3.2 Background

3.2.1 Abortion Provider Regulations and Their Effects

Abortion facility regulations have been growing in popularity in the United States. While different states have passed slightly different packages of regulations, they often include staffing requirements, hospital admitting privileges, and building requirements. The implementation of such regulations has been studied in health economics literature: for example, Lindo et al. (2019), Fischer et al. (2018), and Quast et al. (2017) use a major legislative change in Texas as a natural experiment to estimate the effects of supply-side restrictions on abortion access and find that these restrictions reduce abortion rates.[6] These studies all examine the effect of Texas' House Bill 2 (HB2), a bill that required all doctors who provided abortions to have admitting privileges at nearby hospitals (no further than 30 miles from the abortion clinic), required abortion facilities to meet surgical facility standards, and banned abortions after 20 weeks of gestation. The bill caused the closure of 22 of the state's existing 41 clinics to close their doors, and eventually was overturned by the United States Supreme Court in 2016, stating that the 'provisions constituted an undue burden and are therefore unconstitutional' (Domonoske, 2016). Fischer et al. (2018), Lindo et al. (2019), and Quast et al. (2017) all study the impact of clinic closures on abortion rates, focusing on the impact

[6] Colman and Joyce (2011) also studies a law change in Texas called the Women's Right to Know Act, which had a new requirement of information to be provided to women considering an abortion in addition to a new requirement that abortions after 16 weeks gestation be obtained in ambulatory surgical facilities. This change reduced the number of abortions occurring after 16 weeks gestational age, but increased out-of-state travel for abortions and the number of abortions obtained at 15 weeks of gestation. Effects were persistent: even after new facilities opened that were qualified to perform abortions after the 16th week of gestation, the number of abortions obtained in Texas at this gestational age remained well below pre-Right-to-Know levels.

of increases in distance to the nearest clinic on abortion behavior. Each study finds substantial reductions in abortion rates: the estimated reductions in abortion rates range from 10-20 percent, with variation in estimate size coming from different estimation strategies.[7] Given a pre-regulation abortion rate of approximately 12 abortions per 1,000 women of childbearing age in Texas, these estimated effects imply a reduction in abortions of 1-2 abortions per 1,000 women of childbearing age.[8]

Additionally, Venator and Fletcher (2019) study a similar setting in Wisconsin, in which new regulations forced the closure of two of the state's five existing clinics. Venator and Fletcher (2019) document an average increase in distance to the nearest clinic of 55 miles, with some women experiencing significantly larger increases. The increase in distance in Wisconson cased a reduction in the abortion rate of 25% and an increase in the birth rate of 4%. Both Venator and Fletcher (2019) and Lindo et al. (2019) attempt to separate effects of changes in distance from changes in congestion: Venator and Fletcher (2019) find no effect of clinic congestion on abortion or birth rates, and Lindo et al. (2019) find that both distance and congestion reduce abortions, but that increased congestion may account for 59 percent of the effects of clinic closures on abortion and find that an increase in the average number of women per open clinic in an area of 100,000 reduces abortion rates by 5 percent. The setting I study is unique: distance to the nearest clinic is largely unchanged, so I am able to separately identify the effects of reduced local clinic capacity. Additionally, the new regulations in Texas massively cut funding for non-abortion providing family planning clinics, which may be interacting with abortion clinic closures and could be contributing to the effects of clinic closures alone. In Wisconsin, both distance and congestion change simultaneously, and the state has lower pre-regulation access to abortion than Pennsylvania, which could contribute to the detected effects of clinic closures.

I also contribute to this literature by studying a new population: Pennsylvania is different

[7] Fischer et al. (2018) assume a linear relationship between distance and effects; Lindo et al. (2019) allow for non-linearities; and Quast et al. (2017) use a linear regression but have fewer post-HB2 datapoints.

[8] Lindo et al. (2019) estimate a reduction of 11,900 abortions in the two years of the law's enactment.

from Texas both geographically and demographically. Texas shares a border with Mexico; Pennsylvania is almost entirely bordered by other states in the US (with the small exception of the Erie area, which borders Lake Erie). Texas is also much larger than Pennsylvania, geographically. Driving from El Paso to Dallas or El Paso to Houston (representing West to East travel across the state) takes approximately 9 or 10.5 hours, respectively, and the public transportation options increase potential travel time significantly. Driving from Pittsburgh to Philadelphia (again representing West to East travel across the state) can take less than 4 hours, and public transportation options can take as little as one hour longer than driving. Texas also has a large Hispanic population—falling second in the nation with a Hispanic population of over 36 percent—while only approximately 5 percent of Pennsylvanians are Hispanic.[9] Fertility behavior for Hispanic women has been declining much more dramatically than fertility behavior for other ethnic groups over the past decade (Tavernise, 2019), so abortion responses in Texas may not be representative of abortion responses in less-Hispanic states. For each of these reasons, evidence from Pennsylvania helps to inform how similar policy changes may impact other states. Pennsylvania and Wisconsin are somewhat more similar, with more similar demographic and geographic profiles. However, Wisconsin had fewer abortion clinics per population of childbearing-aged women prior to the closures than Pennsylvania, which could impact the effects of the closures in either direction: perhaps women in Wisconsin were already adjusted to limited access to abortion services, which would predict smaller effects of clinic closures; perhaps closures in Wisconsin are more binding than in Pennsylvania due to the scarcity of services, which would predict larger effects of clinic closures.

3.2.2 Pennsylvania SB732

In December of 2011, Pennsylvania SB732 (also known as Act 122 of 2011) was enacted into law, though clinics were given a “grace period" to meet the new standards. This law had

[9] Wee (2016) shows that this places Texas in second for largest share of the population that is Hispanic; in the tenth-place state, Illinois, only 17 percent of the state’s population is Hispanic. This means that Texas has more than double the 90th percentile of the US’s share of population that is Hispanic.

several components, all of which had the stated goal of improving the safety of abortion services. First, the law redefined “abortion facilities" to include any public or private hospital, clinic, center, medical school, medical training institution, physicians office, infirmary, or other institution which provides surgical services meant to terminate the clinically disposable pregnancy of a woman.[10] Second, abortion facilities were required to meet the same fire and safety standards, personnel and equipment requirements, and quality assurance checks as ambulatory surgical facilities. These standards included increased hallway width, increased operating room size, increased staffing requirements, each facility had to have transfer priviliges to a hospital, and elevators had to meet certain size guidelines. Third, this legislation also enacted annual and random inspections of abortion facilities in order to ensure facilities were meeting the requirements. Prior to the passage of this law, annual inspections were not standard.

Before the law was passed, Pennsylvania had 22 open abortion clinics. Between April and December of 2012, 9 abortion facilities permanently closed their doors, and still others closed temporarily to make the necessarily construction changes. Most of these closures occurred in urban areas, resulting in changes in the number of women of childbearing age per open clinic, while the distance from each county's population centroid to the nearest open clinic remained constant. In fact, 5 of the 9 clinic closures that occurred in 2012 were within the city of Pittsburgh, leaving only 2 open clinics in the entire Pittsburgh service region.[11] This setting therefore provides a unique opportunity to understand the effects of reduced clinic capacity, rather than distance from the county's population centroid to the nearest open clinic, on abortion rates, abortion timing, and birth rates.

While I cannot directly measure the congestion in a given clinic, anecdotal evidence sug-

[10]Facilities that only provided medical abortion services were exempt, although prior to the law's passage there were no facilities that only provided medical abortions.

[11]Three of the other clinic closures were in Philadelphia, leaving 9 open clinics in the Philadelphia region; 1 clinic closure was in Allentown, leaving 2 clinics open in the Allentown region. Because these closures did not reduce their respective region's clinic supply as dramatically as the Pittsburgh closures, Allentown and Philadelphia will be a part of the comparison group. However, since Philadelphia and Allentown experienced some closures, I consider my results to be a lower bound for the true effects of increased clinic congestion.

gests that clinic congestion did increase after the closures for the clinics that remained open. I spoke with Planned Parenthood of Western Pennsylvania's CEO and President, Kimberlee Evert, who said that the Planned Parenthood in Pittsburgh, which was one of the two clinics in the Pittsburgh area that remained open, had to close for some time in 2012 to meet the new standards. Despite their temporary closure, the percentage of abortions in the Pittsburgh area that were performed by Planned Parenthood grew from 31% in 2011 to 42% in 2013, although the number of abortions fell over time. Additionally, wait times for abortion services increased dramatically after the closures began. Some of the increase in wait times is persistent to this day: women calling to request abortion services typically have to wait one week for a medical abortion, or two weeks for a surgical one. Ms. Evert says this is an improvement—at one point after the law's passage, the wait times were at least double that for each type of abortion procedure. Given this anecdotal evidence, I argue that the mechanism for any observed effects is clinic congestion (Evert (2019)).

3.3 Empirical Approach

This section describes the data and empirical approach I use to estimate the causal effects of reduced clinic capacity caused by the passage of SB732 and the resulting clinic closures.

3.3.1 Data

To define treatment and comparison groups, I first define a county's treatment-defining abortion service region as the nearest city in 2006 that had an abortion clinic open.[12] Any counties for which the original, treatment-defining service region experienced an endogenous closure prior to the passage of SB732 are dropped from the analysis.[13] Because Pittsburgh is the abortion service region most affected by SB732, I use counties that were first observed in

[12]All distance calculations are based on geolocations for abortion-providing facilities and county population centroids. I calculate distance to the nearest provider using the Stata *georoute* program (Weber and Peclat (2017)). This program estimates the travel distance from the population centroid of each county (United States Census Bureau, 2018) to the geocoded address of the nearest in-state operating abortion clinic. Figure A.17 shows that the population-weighted average distance to the nearest clinic changes very minimally over the time under observation, so I argue that observed effects are caused by the dramatic increases in the average number of women of childbearing age per open clinic.

[13]The abortion service regions being dropped are East Stroudsburg, Erie, Huntingdon, and State College.

the Pittsburgh abortion service region as my 'treated' counties. I use all other counties as my 'comparison' counties.[14] Figure A.14 shows the counties defined as treated in blue, those defined as comparison counties in orange, and omitted counties in gray.[15] Panel A shows the open clinics in 2011, prior to the law's passage, and Panel B shows the open clinics in 2013, after the 2012 closures which resulted from the law's passage. Figure A.15 shows the clinic locations in Pittsburgh, in 2011 and 2013.[16]

In order to estimate the effects of clinic congestion caused by reduced clinic capacity, I create a measure of the "abortion service population," following Lindo et al. (2019). While this measure does not perfectly capture actual clinic congestion, it does capture the expected increase in patient loads faced by the reduced number of clinics in operation.[17] To construct the average service population, I first assign each county c in time period t to an "abortion service region" r according to the location of the closest city with an abortion clinic. The average service population is the ratio of the population of women aged 15–44 in the service region to the number of clinics in the service region:

$$ASP_{c,r,t} = \frac{\sum_{c \in r} population_{c,t}}{number\ of\ clinics_{r,t}} \tag{3.1}$$

To create abortion rates, I use data from the Pennsylvania Department of Health, which tracks various abortion statistics over time. Importantly, these data contain the number of abortions obtained per county per year by age group, as well as the number of abortions obtained per county per year by gestational age at the time of the abortion. I will use both measures, as well as population denominators from to construct my outcomes of interest, namely county-level abortion rates by age group as well as by gestational age at the time of the abortion. In

[14]Omitting counties with endogenous closures still keeps 36 counties in the analysis, and most of the excluded counties are rural. Additionally, Table A.24 tests the robustness of the results to the inclusion of omitted counties in the comparison group. Results from this robustness test are further discussed in Section 3.5.

[15]Counties are omitted if their nearest clinic in 2006 closed prior to the passage of the law. These closures cannot be seen as exogenous.

[16]Four of the 7 clinics in Pittsburgh were within the same suite of offices: the top right dot in 2011 actually represents 4 unique abortion facilities; in 2013 only one facility remained open in that location.

[17]However, if clinics react in such a way that they reduce their wait times, estimated effects will be an underestimate of the effects of clinic congestion.

future work, I plan to also estimate the effects on birth rates by age group and race, both for women living in Pennsylvania and neighboring states. To do so, I will use natality data from the National Vital Statistics System from the Center for Disease Control.

Table A.15 summarizes the variables used in my analysis: abortion rates by age and gestational age by mother's county of residence, average service population (the number of women of childbearing age per open clinic in the region), abortion rates, and variables measuring county demographics: age and racial composition (SEER, 2018), poverty rate (Census Bureau, 2018) and unemployment (BLS, 2018). Data in this table are broken down into the period before the law was enacted (2006–2011) and the period after the law was enacted (2012–2016). Notably, both groups have similar pre-period poverty and unemployment rates, and both are predominantly white.

3.3.2 Identification Strategy

I use a generalized difference-in-differences approach to estimate the causal effects of reduced clinic capacity. This approach exploits within-county variation over time and controls for aggregate time shocks, as well as fixed differences across counties over time and differences in pre-regulation trends. In order for this approach to be valid, it must be true that changes in abortion rates for comparison counties provide a good counterfactual for the changes in abortion rates that would have been observed for reated counties, if clinic capacity had remained unchanged. My approach to estimating the effects of changes in average service population on the abortion rates corresponds to the following equation:

$$E[y_{ct}|capacity_{c,t-k}, \alpha_c, \alpha_t, X_{ct}] = \sum_{k=1}^{5} \theta_k capacity_{c,t-k} + \alpha_c + \alpha_t \tag{3.2}$$

where y_{ct} is the outcome of interest for residents of county c in year t; $capacity_{c,t-k}$ is an indicator for whether county c experienced reduced capacity in year $t - k$; α_c are county fixed effects; α_t are year fixed effects. All reported standard-error estimates are clustered on the county to account for correlation within counties over time. I use this model to estimate effects on the natural log of abortion rates for women of various age groups, abortion rates for various gestational ages, the share of abortions occurring at a given gestational age, and birth rates by mother's race.

To further test the robustness of my results, and to improve the match of the comparison group to the treated group in the pre-period, I use the synthetic control method. I use this method to estimate the effect of reduced clinic capacity on logged abortion rates and birth rates, comparing the outcomes for the Pittsburgh area to the outcomes of a "Synthetic Pittsburgh Area" (Abadie et al. (2010)). First, I create a "Pittsburgh Area" observation: I collapse outcomes for treated counties by a population-weight.[18] I then use data on abortion and fertility behavior from comparison counties. I identify the weighted average of comparison counties that best matches the outcome of interest observed in the Pittsburgh area prior to the closures. Here the identification assumption is that the synthetic Pittsburgh area provides a good counterfactual for abortion and fertility outcomes that would have been observed in the Pittsburgh area, absent the new regulations. If the assumption holds, the difference between outcomes for the Pittsburgh area and the synthetic control will provide an unbiased estimate of the causal effect of reduced clinic capacity. In order to execute this strategy, I select non-negative weights for each potential "donor county" to minimize the function:

$$(X_{Pitt} - X_{SC}W)'V(X_{Pitt} - X_{SC}W) \tag{3.3}$$

where X_{Pitt} is a (K$\times$1) vector of variables measuring abortion or fertility outcomes from 2008-2011, X_{SC} is a (K$\times$J) matrix containing the outcome variables for other counties in Pennsylvania, W is a (J$\times$1) vector of weights summing to one, and the diagonal matrix V contains the "importance weights" assigned to each variable in X. I include the outcome of interest (abortion rate, rate of abortion at a given gestational age, share of abortions in a given gestational age, or birth rate) observed in each pre-regulation year in X, allowing the

[18] Results from the synthetic control approach are robust to using Allegheny County (the home of Pittsburgh) as the only treated unit and omitting other 'treated counties' from the analysis.

program to assign weights in order to find the best-fit.[19]

To conduct inference, I estimate the distribution of estimated treatment effects under the null hypothesis of a zero treatment effect and reassign treatment separately to each county in the donor pool to estimate a placebo effect for each county. I then construct p-values for the estimated effect for the Pittsburgh area, given the ratio of the post-period root mean squared error to the pre-period root mean squared error. I use this approach for each outcome of interest: abortion rates by age group, abortion rates by gestational age, share of abortions occurring at each gestational age, and birth rates by race.

3.4 Results

3.4.1 Graphical Evidence for the Proposed Mechanism

First, to demonstrate the increase in average service population, I plot the number of childbearing-aged women per open clinic in an abortion service region. Figure A.16 shows the average service population by treatment status. This figure demonstrates that average service population was relatively constant prior to the passage of the new regulations, and that the treated and comparison counties' average service populations tracked prior to the regulations. However, after the regulations were passed and clinics closed, we see both treated and comparion counties' average service population increase, but the treated counties' average service population increases much more dramatically.[20] Meanwhile, Figure A.17 shows that distance to the nearest clinic did not change for the treated or the comparison counties over time—meaning that any observed effects should be a result of reduced clinic capacity rather than changes in distance to the nearest clinic. Next, I show the natural log of the abortion rate over time (minus the natural log in the first year) in Figure A.18. This figure demonstrates that treated and comparison counties follow a similar trend in the pre-2011 period, and there appears to be a much more dramatic decrease in the treated group after the legislation was

[19] Splitting the weights evenly among all pre-period years created convexity issues that made the code unable to run. My results are also robust to different weighting of pre-period outcomes.

[20] There is a jump in average service population in both the treated and comparison counties in 2010. Figures A.32 shows that the main results are robust to excluding this 'odd' pre-period year.

passed and local clinic capacity fell. This provides some evidence that the comparison counties do in fact provide a good counterfactual for the changes in abortion rates that would have occurred in the treated counties, had the changes in average service population—my proxy for local clinic capacity—been similar across the groups.

3.4.2 Effects on Abortion Rates

3.4.2.1 Difference-in-Difference Estimates

3.4.2.1.1 Overall Abortion Rate Before discussing my preferred estimates of the effects of clinic congestion on abortion rates, abortion timing, and births, I first present graphical evidence to support my main results and the validity of my research design. In Figure A.20 I present results graphically for the overall abortion rate, as well as the abortion rate for various gestational ages. The results shown in these figures are from my baseline specification, which includes county and year fixed effects. Estimates prior to the new regulations provide a placebo test for my model, and the model passes these tests since the estimates are not statistically different from zero.

Panel A of Figure A.20 shows the estimated effects on abortion rates overall: this figure shows that estimated effects are negative, but statistically insignificant. Estimated reductions across the three post-period years range from 0–10%, and the reduction is visually compelling despite lacking statistical significance. Results from the baseline specification for the overall abortion rate are shown in Column 1 of Table A.16. The results from Table A.16 indicate that the the estimated effects of reduced clinic capacity on the overall abortion rate was -7.1% over the entire post-period, but that estimated effects are not statistically different from zero.

3.4.2.1.2 Abortion Timing Next, I look at abortions occurring at various gestational ages. Panels B–D of Figure A.20 show estimated effects using the same baseline regression (Equation 3.2). These figures demonstrate that the model passes the pre-regulation placebo tests in most cases, though it does fail in some of the later gestational age outcomes. The estimated effect is negative for the rate of abortions occurring in the first 8 weeks of gestation, and is statistically significant and economically meaningful. The average estimated effect in the first three years is approximately -44 percent. The estimated effects are positive for the rate of abortions occurring in weeks 9–10 and 11-12.

Table A.16 shows the estimated effects by gestational age. Effects are large and statistically significant in the first three years for most gestational ages: abortion rates for the first 8 weeks of gestational age fall, while abortion rates in weeks 9–12 rise. The average effect over the entire post-period is also significant in most gestational ages: the average estimated effect on abortion rates in the first 8 weeks of gestational age is almost -20 percent, which corresponds to over 4,500 fewer abortions occurring in this gestational age group over the entire post-period. These results, combined with the results from the overall abortion rate, suggest that reduced clinic capacity *may* reduce overall abortion access, but is likely causing women who would otherwise have obtained very early-term abortions to have abortions after the first 8 weeks of gestation.

3.4.2.2 Synthetic Control Estimates

3.4.2.2.1 Overall Abortion Rate In order to test whether estimated effects are persistent across other models, I next present graphical evidence from a synthetic control approach. First, I look at abortion rates overall and by gestational age. Figures A.21 and A.22 presents synthetic control estimates for abortion rates on the left-hand side, with the corresponding randomization inference figures on the right-hand side. Using the synthetic control approach, I am able to compare the Pittsburgh area to a synthetically created comparison group, using the same comparison counties included in the difference-in-differences approach. The Synthetic Control provides a close match to the Pittsburgh area in the pre-regulation period, and the divergence between the Pittsburgh area and the Synthetic Control provides the estimated effect of reduced clinic capacity in the Pittsburgh area. Panels A and B of Figure A.21 present the results for the overall abortion rate. Estimated effects generally follow the same pattern as the difference-in-differences estimates.

Table A.19 shows the estimated effect and corresponding p-values (calculated using the ratio of Root Mean Squared Errors in the post-period to the Root Mean Squared Errors in the pre-period). The results for the overall abortion rate are shown in Panel A: using the synthetic control approach, the estimated effects of reduced clinic capacity range from -5.5 to -11.8 percent, with an average estimated effect for the entire post-period of -9.3 percent. Using pre-regulation abortion rates, this reduction corresponds to nearly 1,700 fewer abortions. Taken with the results from the difference-in-differences approach, this suggests that reduced clinic capacity may have caused a reduction in abortion rates of 0–11 percent. In either approach, the estimated effects are not positive—they are negative or not statistically differentiable from zero.

3.4.2.2.2 Abortion Timing Panels C through F of Figure A.21 and all panels of Figure A.22 present the synthetic control estimates for abortion rates by gestational age group on the left-hand side, with the corresponding randomization inference figures on the right-hand side. Again, the synthetic control largely supports the difference-in-differences findings: early-term abortion rates drop dramatically as a result of reduced clinic congestion. Results for increases in later abortions, though following the same pattern as DiD results, are not statistically significant. These results suggest that any abortions taking place after the clinic closures did so at a later gestational age than they would have if the clinics had remained open.

Table A.19 shows the estimated effect and corresponding p-values (calculated using the ratio of Root Mean Squared Errors in the post-period to the Root Mean Squared Errors in the pre-period). The results for abortion rates by gestational age are shown in Panels B–D: using the synthetic control approach, the estimated effects of reduced clinic capacity on the rate of abortions occurring in weeks 1–8 of gestational age range from -22 to -37 percent, with an average estimated effect for the entire post-period of -31.3 percent. Using pre-regulation abortion rates, this reduction corresponds to nearly 3,600 fewer abortions occurring within the first 8 weeks of gestation. Effects for the rate of abortions occurring in weeks 9–12 also follow the same pattern as the difference-in-difference results, though p-values are, in some cases, too large to reject the possibility that the true effect is zero.

3.4.3 Additional Results

3.4.3.1 Heterogeneous Effects on Abortion

3.4.3.1.1 Difference-in-Differences Estimates Figure A.23 shows the results from Equation 3.2 on abortion rates by age. Panel A shows the estimated effects for teens, while Panel B shows the estimated effects for non-teenaged women. The estimated effects for teens demonstrate a slight reduction in abortions for teens in 2012, but this effect is imprecise and the confidence intervals do not rule out the possibility of the true effect being a zero. Estimated effects for non-teens, however, display a similar pattern to the overall abortion rate. The estimated effects for non-teens are negative in 2013 and 2014, with the estimated effect in 2013 being statistically significant at the 5% confidence level. The estimated effect in 2013 is -16.8 percent, which corresponds to approximately 3,500 fewer abortions obtained by non-teen women in the Pittsburgh area as a result of reduced clinic capacity. Table A.17 shows the estimated effects on abortion rates for teens and non-teens. The results follow what we would expect from the figures, and suggest that any reduction in the overall abortion rate is driven by non-teenaged women.

3.4.3.1.2 Synthetic Control Estimates Effects broken down by age group (teen and non-teen) are shown in Figure A.24. The results shown using the synthetic control method are slightly more visually convincing than the results shown in Figure A.23, and the results shown in Table A.20 demonstrate that the estimated effects are statistically significant for non-teenaged women in 2013 and 2014. Reductions in abortion rates are persistent across the entire post-period.

3.4.3.2 Effects on Birth Rates

3.4.3.2.1 Difference-in-Differences Estimates Given the documented disparities in access to reproductive health care based on race and ethnicity, I would ideally run a similar analysis for abortion rates by race and ethnicity. Unfortunately, these data do not exist at the county-by-year level for the state of Pennsylvania. Instead, I estimate effects for birth rates occurring by mother's race and ethnicity. Figure A.25 shows the estimated results using the same specification as was used in the abortion figures, and passes the pre-period placebo tests for the rate of births occurring to the total population, white mothers, black mothers, or Hispanic mothers.[21] These results demonstrate an increase in birth rates for women overal, which appears to be driven by white women. This suggests that clinic closures impact women of different races differentially. Table A.18 shows the results for the estimation of effects on birth rates by race. Results suggest an increase in the overall birth rate of approximately 3.4 percent over the entire post-period. This corresponds to approximately 3,800 additional births—these results could be true if nearly every abortion that would have otherwise occurred resulted in a birth.

3.4.3.2.2 Synthetic Control Estimates Finally, Figure A.26 presents synthetic control estimates for birth rates on the left-hand side, with the corresponding randomization inference figures on the right-hand side. Estimates generated by the synthetic control method are noisy, but follow the same general pattern as estimates from the difference-in-differences approach. These results suggest that reduced abortion clinic capacity had little to no effect on birth rates, with some suggestive evidence of an increase in birth rates overall and for black women. Table A.21 shows the estimated effects and associated p-values: estimated effects are near zero and insignificant for all groups.

[21] Data on births by mother's race/ethnicity are somewhat uninformative: there is no indicator for children born to women who identify as more than one race/ethnicity category. Future work will use better birth data which do include such an indicator.

3.5 Validity and Robustness

In this section, I present a set of robustness checks to provide additional support for my identifying assumption. First, one might be concerned that the counties that remain in the sample are somehow different from the counties that are omitted in fertility and abortion behavior. This could create a problem for external validity. To test this, I include the previously excluded counties in the comparison. That is, I keep the treated group the same, but add the counties that experienced endogenous closures of their nearest abortion facility (closures prior to 2011) into the comparison group. Point estimates for this analysis can be found in Table A.24. The point estimates in this table are quite similar to those shown in Table A.22. Similarly, estimated effects for abortion rates at various gestational ages remain robust: Table A.24 presents the point estimates. All results are similar to those shown in the previous section.

Next, I provide further support that the mechanism for the effects is, in fact, clinic congestion. One may be concerned that small changes in distance in urban areas have a meaningful impact on abortion access for women living in those urban areas. To address this concern, I re-run the main analysis, dropping Allegheny County (home of Pittsburgh). If all of my effects were due to women in the Pittsburgh losing access to these facilities (perhaps via increased difficulty in using public transport), this analysis would show no effects from the closures. Table A.25 presents the point estimates from this analysis. Both the direction and the magnitude of the estimates are quite similar to those presented in the main analysis. The takeaways from these tables are largely the same as those from the full sample, which supports the idea that effects are driven by clinic congestion rather than changes in distance. However, these results suggest that the reduction in abortion rates is driven by Pittsburgh women, although the delay in timing is consistent to the exclusion of Pittsburgh

Finally, I consider the possibility of inter-state travel for women wishing to obtain abortions. Due to the gravity of the potential outcomes of not obtaining an abortion when desired, women may travel to nearby states to obtain an abortion. If this is the case, then women traveling into Pennsylvania for abortions could also be impacted by reduced clinic capacity. This would be especially true for women traveling to the Pittsburgh area rather than other parts of the state. To test for effects on out-of-state women obtaining abortion in Pennsylvania, I plot the natural log of the rate of abortion for women traveling to Pennsylvania for abortions, for each of Pennsylvania's six neighboring states.[22] Figure A.33 shows the natural log of the abortion rates for each of these six states, with a vertical line drawn in at the passage of the new regulations. There do appear to be some declines in abortion rates for some states and some age groups: teenagers in all neighboring states seem to experience a reduction after 2011; West Virginia also appears to demonstrate a reduction for almost all age groups. The figures for abortion rates by gestational age overall do not exhibit evidence of delays in abortion timing. This suggests that closures within Pennsylvania may impact abortion or fertility behaviors for women in neighboring states, particularly in states with limited access to abortion services.

The other possibility for inter-state travel is Pennsyvlania women traveling to other states for abortion services. If closures in Pennsylvania force women in treated counties to obtain abortions out-of-state rather than in Pennsylvania, their abortions would not be collected in the Pennsylvania Department of Health data. This means that any negative estimated effects in abortion rates *could* be a result of women traveling out-of-state for abortions, rather than abortions actually falling. In order to test for effects on this behavior, I rely on the CDC Abortion Surveillance Data, which is available from 2009-2015. These data do not provide information on the age of the woman obtaining the abortion, or on the gestational age at the time of the abortion, so figures can only show the total abortion rate for women living in Pennsylvania obtaining abortions out of state. Figure A.34 shows the natural log of

[22]The reason I use these six states is because the Pennsylvania Department of Health reports the number of abortions occurring within Pennsylvania per age group and per gestational age for each of its six neighboring states (Delaware, Maryland, New Jersey, New York, Ohio, and West Virginia)—but not for any other states. CDC Abortion Surveillance data also show the number of women from other states obtaining abortions in Pennsylvania, and include more than just the six neighboring states. However, I am choosing to look that the PA Department of Health data in order to have the age of the women obtaining abortions, as well as the gestational age at the time of the abortion.

the abortion rate of women in Pennsylvania obtaining abortion in other states, grouping the neighboring states based on which of Pennsylvania's borders they share. Since Pittsburgh is the treated city and is near the West border of the state, I expect any changes resulting from the new regulations to appear in the West Border States group.[23] Abortion rates for Pennsylvania residents traveling to North, South, and East border states remain relatively constant. Abortion rates for Pennsylvania residents traveling to West border states was falling sharply before the clinic closures, then rose in the first two years after Pennsylvania clinics closed. This suggests that some Pittsburgh-area women are responding to the closures by traveling out-of-state when they otherwise may have obtained an abortion in Pittsburgh.

3.6 Conclusion and Discussion

While it is important to understand the impacts of access to health care on health outcomes, it is difficult to untangle this causal relationship due to endogenous selection into health service areas. It is also difficult to measure what mechanisms of access to health care matter most: when there is an exogenous shock to access to health care, there are often multiple mechanisms changing at once. For example, closures caused by unexpected policy changes or budgetary struggles may provide a good opportunity to understand the causal effects of access to health care, yet these closures may create several mechanisms (such as increased distance to the nearest clinic as well as increased congestion at clinics that remain open) that could impact outcomes. Using a unique setting in which clinic closures cause local clinic capacity to fall while distance to the nearest clinic remains constant, I am able to uniquely identify the effects of reduced access to health care through the channel of reduced clinic capacity. Results show that reduced local clinic capacity can have important impacts: in areas where abortion clinic capacity was reduced while distance remained unchanged, abortion rates fell by up to 8%, abortions were delayed from the first 8 weeks of gestation to

[23] Since I do not know the county of residence for women obtaining out-of-state abortions, the thought process here is that women are likely to travel to the nearest out-of-state clinic if they choose to leave the state. This means that West border states are treated, East are not, and the predicted effects for North and South border states is ambiguous.

weeks 9–14, and evidence suggests possible increases in birth rates.

A delay in abortion timing is important for three reasons: first, the choice set for abortion procedures falls as gestational age increases. In the state of Pennsylvania, women can obtain a medical abortion through their tenth week of gestation. After that point, they are only able to access surgical abortion, which is a much more invasive procedure—and after 18 weeks of gestation, some facilities in the state no longer provide any type of abortion.[24] Second, abortion services get more expensive as gestational age increases. In July of 2019, Planned Parenthood of Western Pennsylvania listed prices for abortion services by gestational age. For a surgical abortion with local anesthetic (the cheapest surgical abortion option), the cost of an abortion was $435 up through week 11 of gestation, then jumped to $540 in weeks 12–13, $815 in weeks 14–16, and $915 in weeks 17–18. This particular clinic also does not offer abortion services after week 18 of gestation, though the state legally allows abortions through week 24. This increase in the cost of abortion is particularly concerning since nearly half of all women obtaining abortions in the United States have an income below the federal poverty level. This cost increase is not considering any other potential costs a woman may incur due to delaying her abortion, such as reduced productivity or lost hours of work, increased childcare costs, increased medical or travel costs, or the costs to mental health of continuing an unwanted pregnancy (Jones and Jerman (2017)). Third, while abortion is overall a very safe procedure, the risk of dangerous complications grows as gestation goes on. Typically, abortions are safest early in the pregnancy, and grow less safe as the pregnancy goes on. Figure A.27 shows Pennsylvania's average state-wide complication rate by gestational age at the time of abortion and type of complication, for the years of 2006-2011. Retained products of conception refers to a complication in which the abortion was unsuccessful and a second 'abortion' must take place. This complication is most common with medical abortions, so seeing this type of complication rate fall as gestational age increases (as the medical abortion

[24] Although some facilities stop providing abortions after 18 weeks of gestation, abortion is legal in Pennsylvania until the 24th week of gestation. After 24 weeks of gestation, abortion may only legally be obtained if the mother's life is in danger.

is no longer an option) is unsurprising. However, the risk of complications like infection or bleeding increases with gestational age. While women obtaining abortions at any gestational age are quite unlikely to experience any complications, the increase in the risk of these dangerous complications is a concern. Additionally, while the regulations were passed under a stated purpose of improving the safety of abortions in the state of Pennsylvania, statewide complication rates actually *increased* over time. Figure A.28 plots complications over time by type of complication. Since complications are only available at the state-by-year level, which means that I cannot identify whether the increases are a causal effect of reduced clinic capacity caused by these new regulations, this evidence suggests that regulations did not improve the safety of abortions.

This line of research is relevant to discussions regarding access to reproductive health care given the growing popularity of regulations of this nature. While the stated aim of these regulations—and similar ones in other states—is to improve the safety of abortions obtained, results show that they actually force the closure of many existing clinics. Clinic closures may increase distance to the nearest clinic, which has been documented to be important to health outcomes, and may increase clinic congestion, which I show also has significant effects on access to services obtained at the clinics that remain open. Evidence on the effects of clinic congestion is relevant to discussions about health care access: being geographically near an open clinic is only part of the issue. Additionally, complications from abortions are quite low, so policies such as these may not have enough of an impact on safety to move the needle on abortion complication rates—meaning the closures may cost more (in terms of access to abortion and other reproductive health care services) than they're worth.

These results are also relevant to other types of health care. Increased clinic congestion—or reduced local clinic capacity—could impact the timing and takeup of other health care services—which could be important for care of chronic conditions, such as cancer or diabetes. If clinics become more congested, patients may have to delay the timing of treatment, which could be costly to the health care providers and to patients, and could increase risks of complications if timing of treatment is important. Researchers, policymakers, and health care providers must be aware of the potential effects of reduced clinic capacity on timing and takeup of health care services, particularly for populations for which timing of services could mean the difference between life and death.

4 ENDING THE BACKLOG: THE EFFECTS OF RAPE KIT TESTING ON FORENSIC PROFILE REGISTRATION

4.1 Introduction

While research has documented the prevalence and the emotional and financial significance of surviving sexual assault, we still know very little about how to improve reporting rates, whether testing kits actually improves arrest or conviction rates, or how criminal justice agencies should best prioritize their limited funds (Peterson et al., 2017; McCollister et al., 2010; Carey et al., 2015; Smith et al., 2019). However, we do know that many criminal justice agencies have left untested evidence for cases related to sexual assault. While different agencies have left sexual assault kits untested for various reasons—a belief cases lacked evidence, a shortage of funds, or an officer prioritization of other types of cases with potential DNA evidence—the result is a massive nation-wide backlog of untested sexual assault kits. In 2015, the Bureau of Justice Assistance identified over 200,000 untested sexual assault kits across 54 jurisdictions in 35 states, which suggests the true number of untested kits in the United States at that time was significantly higher. Failing to test sexual assault kits can have several potential consequences, not the least of which are (1) serial offenders getting away with their crimes even after evidence is collected against them, and (2) victims may lose faith in the system to handle their case with respect and dignity—which could ultimately lead a victim to choose not to report the crime committed against them. Improving testing rates of sexual assault evidence kits could lead to higher arrest rates, deterrence of future criminal behavior, or increases in victim reporting. Unfortunately, to date we have no source of exogenous variation in the funds designated to sexual assault kit testing, or the number of kits tested themselves, and have therefore been unable to establish a causal relationship between funds or testing and victim or criminal behavior.

In this paper, we use a natural experiment in which various agencies across the United

States received funding with the purpose of addressing their backlogged sexual assault kits. Not all agencies in the US received funds, and those that did received different amounts of funding, and received funds at varying times. Some agencies never received grants; some received as many as four unique grants between 2015 and 2018. Using this variation, we uncover the causal effect of providing funding for the testing of sexual assault kits on the rate of forensic profiles entered into the state's DNA database. In future work, we will use this exogenous shock in rape kit testing to understand how rape kit testing impacts reporting and arrests.

4.2 Background

4.2.1 Sexual Assault Kits and CODIS

Sexual assault kits (SAKs)—also known as 'rape kits'—contain evidence collected by a nurse during a sexual assault evidence exam. A primary goal of such exams is to collect DNA evidence from the assault (from the perpetrator's semen or saliva, for example), to assist in identifying the perpetrator. Victims are advised to have an exam completed within 72 hours of the assault, and not to shower or otherwise clean affected areas before the exam (RAINN, 2020).[1] It is unclear what share of victims have SAKs completed, or what share of completed SAKs contain usable DNA. In one study, performed at the only hospital in Ottawa that provides sexual assault kits in 2015, 77 percent of the survivors that visited the emergency room due to a sexual assault were eligible to have a sexual assault kit taken—yet only 49 percent (or 64 percent of those who were eligible for the kit) had the kit taken (Muldoon et al., 2018). Further, only 22.9 percent of the survivors who visited the hospital for sexual assault ultimately agreed to turn over forensic evidence to the police. While the study sample was small, and the results come from Canada, they are the only numbers documented to date to give a perspective on the lack of sexual assault kit testing. Additionally, this study only represents the victims who had already selected into visitng a hospital after their assault—we do not know how many victims never even make the trip to the hospital, suggesting that the

[1]See RAINN for more information: `https://www.rainn.org/articles/rape-kit`.

percentage of *all* victims willing to submit a sexual assault kit to the police is significantly lower.

Once an exam is completed, SAKs are sent to a crime lab for analysis. If a usable DNA profile is identified, it can be used in a criminal case against an accused perpetrator. The DNA evidence would provide evidence that sexual contact took place, and so is useful when identity is the issue in the case. DNA evidence may not be useful when the alleged perpetrator acknowledges having sexual contact with the victim, but argues that this contact was consensual. The exception might be if the DNA profile links the case to other cases where consent was the issue; if the same person is repeatedly accused of sexual assault by different victims, this might help establish a pattern of behavior that makes all of the cases stronger.

SAKs are arguably most useful when the identify of the perpetrator is unknown, or when linking to other allegations could help bolster a case against a serial offender. To realize these benefits, the DNA profile from an SAK must be uploaded to the forensic profile portion of the state DNA database, which is then linked to other states' databases via CODIS (the Combined DNA Index System, maintained by the FBI). Forensic profiles in CODIS are regularly compared with one another, and with known offender profiles (that is, DNA profiles from people previously convicted of or arrested for eligible crimes). If a forensic profile matches another forensic profile, or a known offender profile, such a match is called a 'hit' and the relevant information is sent to the jurisdictions that originally uploaded the profiles. The purpose of such matches is to provide leads in cases that might not have been identified another way.

4.2.2 SAK Backlogs and Federal Funding

Over the past decade, numerous police departments have announced that they have large backlogs of SAKs that had not been sent to a lab for testing. Public outrage about these announcements pushed many cities and states to prioritize testing these SAKs (Bettinger-Lopez, 2016; Reilly, 2015; Aiken Standard, 2014). The federal government facilitated this process by providing funding to agencies, through the Sexual Assault Kit Initiative funding program, sponsored by the Bureau of Justice Assistance and the District Attorney of New York. Between 2015 and 2018, 75 agencies received an average of $1,370,707 each to help clear their backlogs of untested kits (BJA, 2020; DANY, 2020).

4.3 Data

Data for this project come from a variety of sources.

4.3.1 Grant Funding

For information on funding amounts and receipt dates, we use information from the Sexual Assault Kit Initiative, housed by the Bureau of Justice Assistance.[2] This website contains information on which agencies received grants from either the BJA or the District Attorney of New York (DANY), how much they received, and when they received the funds. The years of grant-receipt range from 2015 through 2018. Treatment for a given state begins in the first year in which any agency in the state received funding from either the BJA or DANY.

4.3.2 CODIS

The FBI posts the following information on its website, for each state, roughly each month: (1) the number of convicted offender profiles in the database, (2) the number of arrestee profiles in the database, (3) the number of forensic profiles in the database, (4) the number of participating forensic labs that are eligible to upload DNA profiles to the database, and (5) the number of investigations aided (that is, the number of 'hits' that have occurred between profiles). The data posted on the website are for snapshots in time; the FBI does not provide historical data, and such data are typically not available from states either. We have collected this information from the FBI website for several years. This gives us a unique data set containing state-level monthly counts of profiles available in CODIS, for most months between 2010 and the present.

[2]Their website can be found at https://www.sakitta.org/

We focus on the number of forensic profiles in CODIS from each state, as of December of each year. Note that this number is the total of all forensic profiles across all case types; we are not able to focus just on forensic profiles from sexual assault cases. We convert the number of forensic profiles to rates, per 100,000 state residents.

4.3.3 Demographic Controls

To control for other time-varying state characteristics, we use the Bureau of Labor Statistics' state-level unemployment rates, state-level poverty rate from the Census Bureau, and population information from SEER. We convert our outcome variable to rates: the rate is forensic profiles per 100,000 state residents, based on the SEER population data.

4.3.4 Analysis Sample

Table A.26 shows summary statistics for our final analysis sample. Our sample includes 450 state-year observations spanning the years 2010-2018.

4.4 Empirical Strategy

We use a generalized difference-in-differences approach to estimate the causal effects of increased funding for testing backlogged sexual assault kits. This approach exploits variation across states over time and controls for aggregate time shocks, as well as fixed differences across states and differences in pre-treatment trends. In order for this approach to be valid, it must be true that trends in forensic profile rates, crime rates, and reporting rates for comparison states provide a good counterfactual for the changes in corresponding rates that would have been observed for treated states, if funding for this initiative had remained unchanged.

To measure the effects of SAK funding on crime and reporting rates, we estimate the following equation:

$$ForensicProfileRate_{s,t} = \theta treated_{s,t} + \alpha_s + \gamma_t + X_{s,t} + \alpha_s * t + \epsilon_{s,t} \tag{4.1}$$

$ForensicProfileRate_{s,t}$ is the rate of forensic profiles entered into CODIS per 100,000 state residents for state s in year t. $treated_{s,t}$ is a binary indicator for whether state s experienced funding in or before year t. α_s are state fixed effects. $X_{s,t}$ are time-varying, state-level controls: the unemployment rate, the poverty rate, and the shares of the population that are civilian, U.S. citizen, male and married, female and married, male and never married, and female and never married.[3] α_t are year fixed effects. $\alpha_s * t$ are state-specific linear time trends. All reported standard-error estimates are clustered on the state to account for serial correlation over time.

To show how the effects of testing SAKs evolve over time, we plot coefficients from the following regression:

$$ForensicProfileRate_{s,t} = \sum_{k=-4}^{2} \theta_k treated_{s,t+k} + \alpha_s + \gamma_t + X_{s,t} + \alpha_s * t + \epsilon_{s,t} \quad (4.2)$$

Observations from more than 4 years before treatment are included in k = -4, and observations from more than 2 years after treatment are included in k = 2. If our specification is isolating the causal effect of the funding, we should see flat pre-trends and then a change in the outcomes after funding is granted.

4.5 Results

We now consider the effect of funding on the number of forensic profiles uploaded to CODIS. It is not obvious that providing funding to local jurisdictions to analysis SAKs would increase the number of forensic profiles, for at least two reasons: (1) money is fungible, and places might have found a way to analyze these kits even without the federal grants, and (2) the backlogged SAKs might not have yielded usable DNA samples that are necessary to upload a profile. It was possible that police officers were choosing which SAKs to send to the lab based on the likelihood that they would yield useful evidence; if this was the case and their intuitions were correct, we might not see an increase in forensic profiles, even if funding did increase the number of SAKs analyzed.

[3] The share married/never married leaves as the excluded category the share of males and females who had been but are no longer married (such as divorcees or widowers).

Figure A.35 shows how the number of forensic profiles per 100,000 residents contained in the state DNA database, each year relative to the receipt of grant funding. The funding has no ‘effect’ before the grant is received; pre-trends are flat, as they should be if our specification is identifying the causal effect of the treatment. After grant funds are received at time 0, we see an immediate increase in the number of forensic profiles in the state database.

Column 4 of Table A.27 shows the overall impact: receiving grant funding causes an increase of 18 forensic profiles in CODIS, per 100,000 residents ($p < 0.10$).

4.5.1 Validity and Robustness

Finally, we show that the estimates found are not just a function of the specification we used. Columns 1-3 of Table A.27, adding in various controls. While the exact point estimates do change across columns, the general direction and significance remains consistent.[4]

4.6 Conclusion and Discussion

Increasing funding to test backlogged sexual assault kits increased the rate of forensic profiles entered into states’ DNA databases by approximately 18 profiles per state per year. Given that it costs an average of $1,000-$1,500 to test each kit, we would expect the cost of testing kits alone to be between $18,000 and $27,000 per state per year of funding per 100,000 people (EndTheBacklog, 2020). Since each state that did receive funds received an average of $44,000 per 100,000 people, back-of-the-envelope calculations suggest that roughly half of the funds states received went to testing kits which successfully created a forensic profile in the DNA database.[5]

The funds granted to agencies across the United States by both the BJA and DANY give us a unique opportunity to understand the impacts of analyzing backlogged sexual assault kits. Our work so far demonstrates the importance of providing these funds on entering

[4]Note that Column 1 includes a point estimate for 4+ years pre-treatment. Since treatment may impact the trend in outcomes, we need to construct the linear time trends using only pre-period outcomes (see Wolfers (2006)). This means that when we do not include linear trends, we can observe more pre-period data; therefore Column 1 includes all pre-period years, and Columns 2-4 have the 4+ years pre-period omitted.

[5]States that received funding received approximately $0.44 per person per year of funding, although the average funds per population varied widely across state-years.

forensic profiles into state DNA databases; our future work will uncover the impact of these new forensic profiles on other outcomes such as clearance rates and reports of rape offenses.

5 CONCLUSION

In this dissertation, I document the effects of improved access to health care on health outcomes and increased testing of DNA kits on forensic profiles. I find that, generally speaking, increased access to care or justice improves outcomes: increased access to long-acting reversible contraception reduces unintended pregnancies, potentially improving long-term outcomes for women; reducing access to abortion services—by reducing local clinic capacity—reduces abortion rates, delays the timing of abortions, and increases birth rates; and increasing funding for backlogged sexual assault kits increases the number of forensic profiles, which may impact other outcomes (such as arrest rates or reporting rates) down the line.

Each of these sections can provide insight to a variety of other settings. Section 2 demonstrates that even when health care providers (or other experts) may know that a given approach is safe, effective, and overall a great value, people may not take up the approach unless it is provided to them for free at a facility they already know and trust. Improving access to these types of approaches may require expansion of practices by pre-existing facilities. These results could be relevant to reproductive health settings like the one studied in this section, to other medical interventions where up-front costs are a major barrier, or to health settings where people simply do not know that a given approach is safe and effective.

Section 3 demonstrates that reduced local capacity for health care facilities—which *may* create congestion or over-demanded facilities—can reduce takeup of services provided by these facilities, can delay the timing of treatment, and can push people into riskier outcomes through either of these mechanisms. These results could be relevant to settings in which health care facilities are being faced with extreme budgetary constraints and are trying to determine a best course of action. They could also be relevant to other health settings in which timing and tempo of treatment matters, like chronic care or cancer screening and treatment. They could also be relevant to health markets that are seeing exponential increases in demand with little to no accommodating expansion—perhaps like services geared toward the aging baby boomer population, or services connected to pandemics, like the recent COVID-19 outbreak.

Finally, Section 4 demonstrates that funding the testing of backlogged sexual assault kits does, in fact, lead to an increase in forensic profiles registered with the state's DNA database. This is very relevant to agencies that are facing tough decisions and worry that testing backlogged kits—for sexual assault or for other types of crime—may not lead to any useful forensic evidence. The potential effects down the line from these new forensic profiles could also be important to understanding how people respond to increased likelihood of arrests—both on the perpetrator side and on the victim reporting side. The results could speak more broadly to understanding how to improve reporting rates for underreported crime, or how to increase the likelihood of stopping serial offenders.

Sections 2-4 shed light on the effects of increasing (or reducing) access to health care and criminal justice support on outcomes, and contribute to three separate but related literatures regarding the impacts of having access to vital services. My work thus far provides clear policy implications for legislation addressing women's health and safety.

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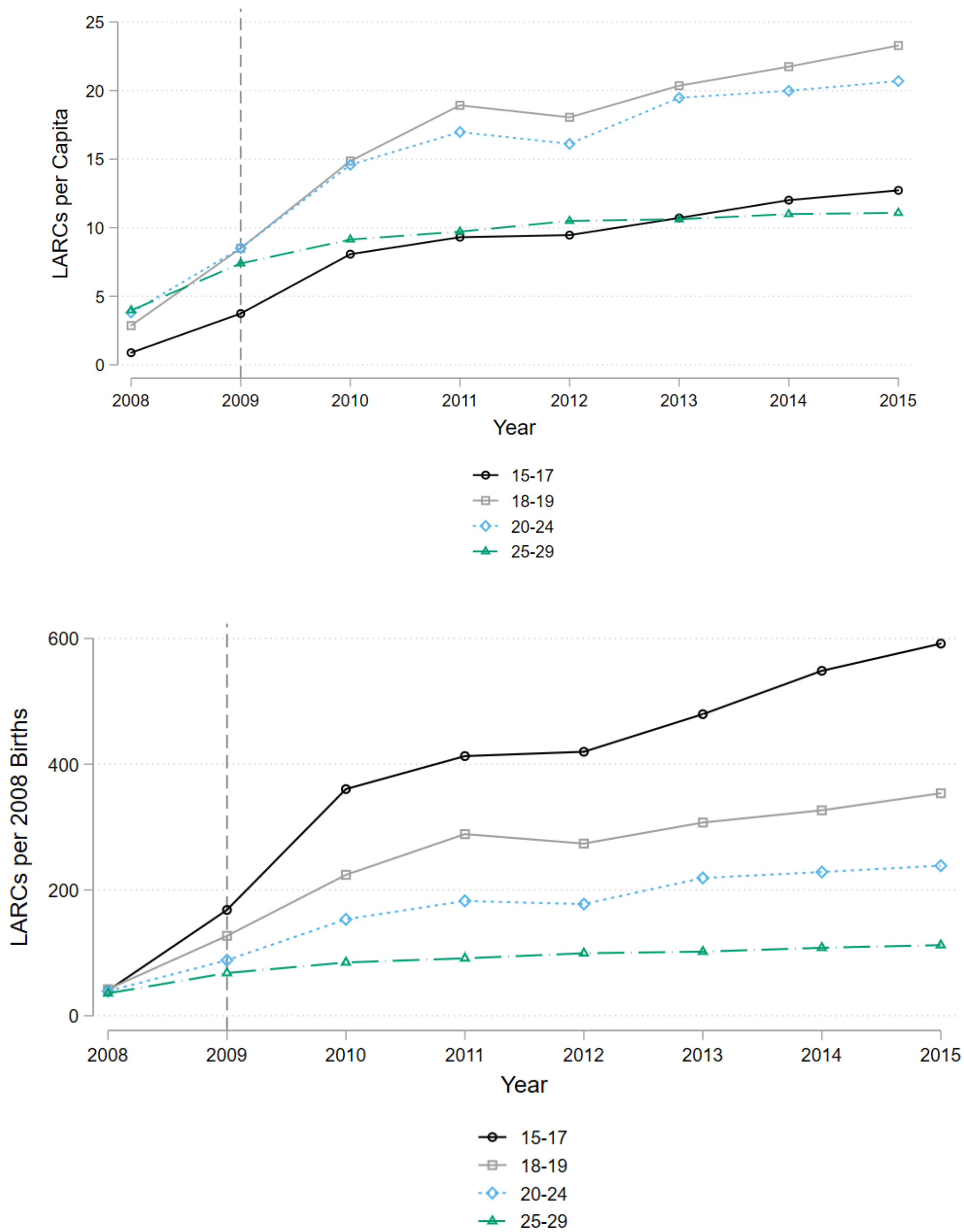
APPENDIX

Figures and Tables for Section 2

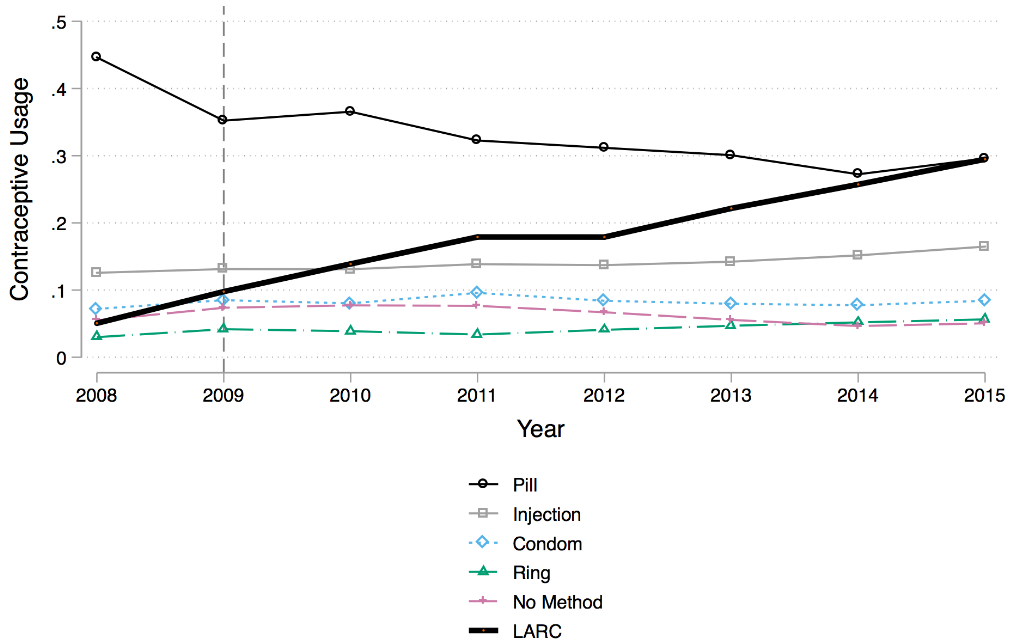
Figures

Figure A.1: Number of Female Title X Clients Choosing a LARC, By Age



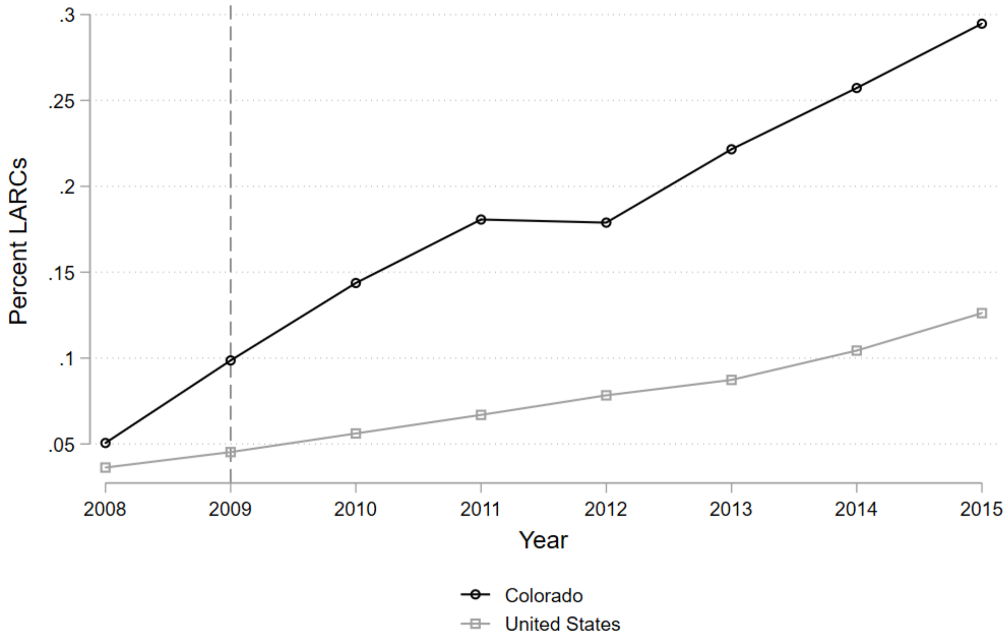
Notes: Authors' calculation based on annual data on Colorado Title X contraception usage by age and method provided by the Colorado Department of Public Health and Environment (DPHE). Zip-code-level population data are from the 2010 American Community Survey. The vertical line, drawn at 2009, represents the year Colorado's Family Planning Initiative was implemented. The top panel displays the number of LARCs chosen by Colorado Title X clients per capita, by age group, from 2008–2015, while the bottom panel displays the number of LARCs chosen by Colorado Title X clients per births in 2008, according to natality data from the Colorado DPHE.

Figure A.2: Primary Form of Contraceptive Used by Females Aged 15–29 Visiting Title X Clinics in Colorado



Notes: Authors' calculation based on annual data on Colorado Title X contraception usage by age and method provided by the Colorado Department of Public Health and Environment. The vertical line, drawn at 2009, represents the year Colorado's Family Planning Initiative was implemented.

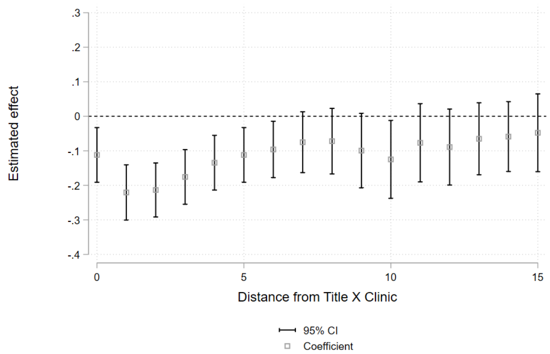
Figure A.3: Percent Female Clients Aged 15–29 Visiting Title X Clinics Choosing a LARC, Colorado versus United States



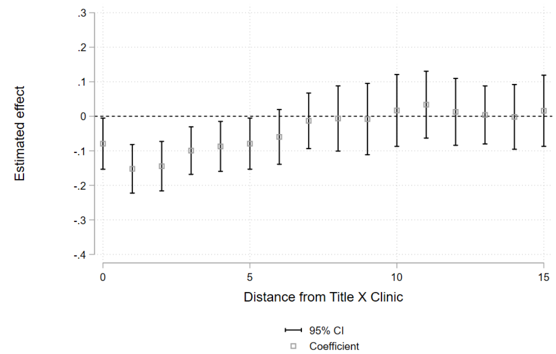
Notes: Numbers for Colorado are based on annual data on Colorado Title X contraception usage by age and method provided by the Colorado Department of Public Health and Environment. Numbers for the United States overall are from the Department of Health and Human Services Title X Family Planning Annual Reports, United States 2008–2015. The vertical line, drawn at 2009, represents the year Colorado's Family Planning Initiative was implemented.

Figure A.4: Estimated Effects of the CFPI on Births by Rolling 5-Mile Distance Bins

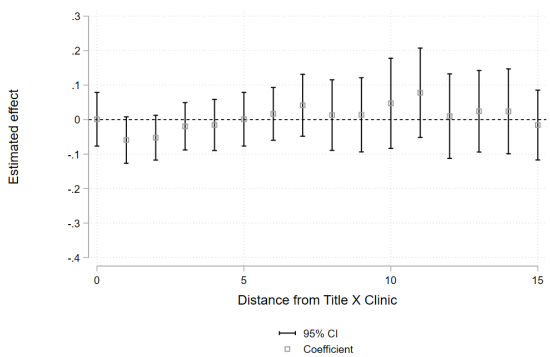
Panel A. 15–17 Year Olds



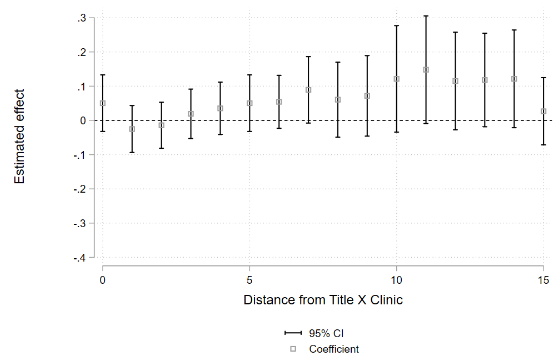
Panel B. 18–19 Year Olds



Panel C. 20–24 Year Olds

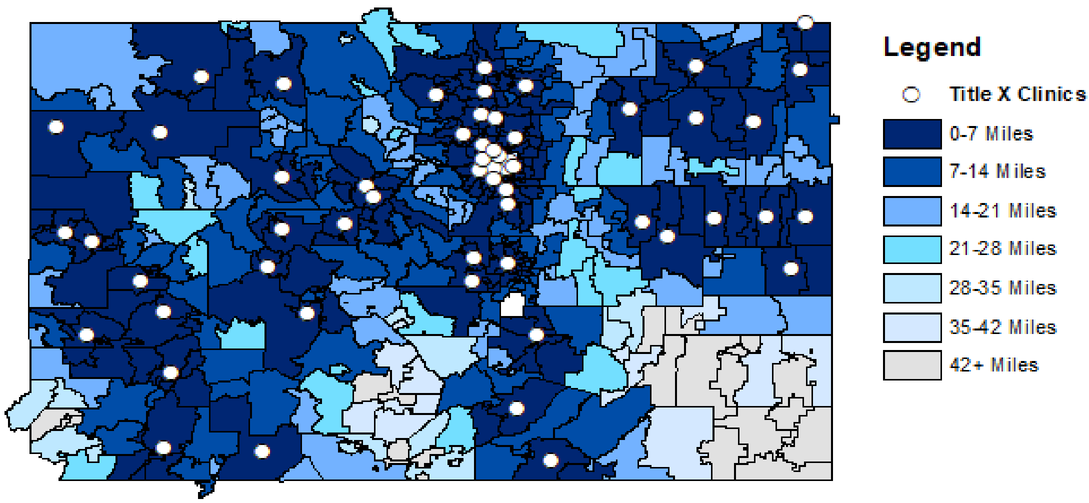


Panel D. 25–29 Year Olds



Notes: Coefficients and their respective 95% confidence intervals are generated from a regression estimated using OLS-IHS transformation, as specified in Equation 2.1, using rolling 5-mile distance bins to define treatment. A x-axis value of "i" where $i = 0, 1, ...15$ indicates an estimate from a difference-in-differences analysis comparing changes in births in zip codes within i and $(i + 5)$ miles of a Title X clinic to changes in zip codes between $(i + 5)$ and 20 miles from a clinic. Zip codes greater than 20 miles from a Colorado Title X clinic are omitted from this analysis. Standard errors are clustered at the zip-code level.

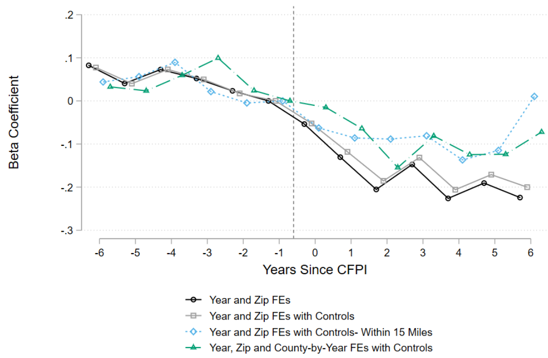
Figure A.5: Distance from Population Centroid to Nearest Title X Clinic



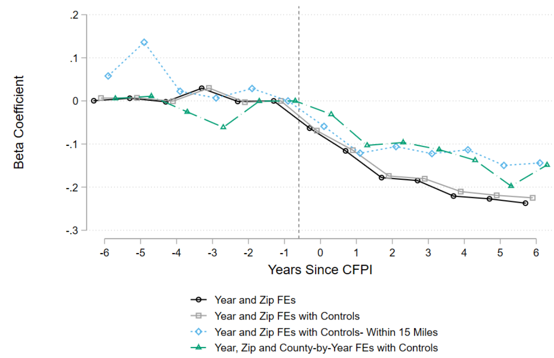
Notes: “Distance" indicates distance, in miles. Authors’ calculation of zip-code centroid distance to the nearest clinic is based on geocoded data of Title X clinics from the Colorado Department of Public Health and Environment directory.

Figure A.6: Difference-in-Differences Estimates of the Effects of the CFPI on Births

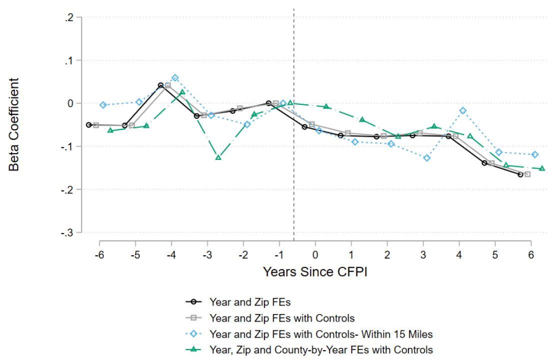
Panel A. 15–17 Year Olds



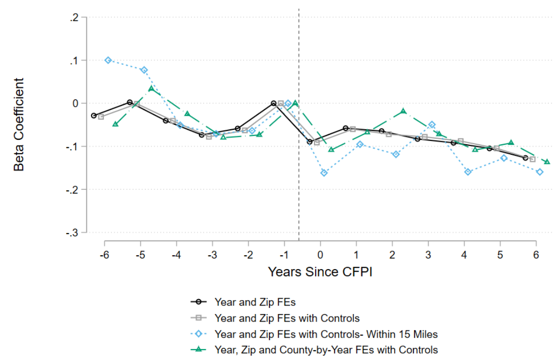
Panel B. 18–19 Year Olds



Panel C. 20–24 Year Olds

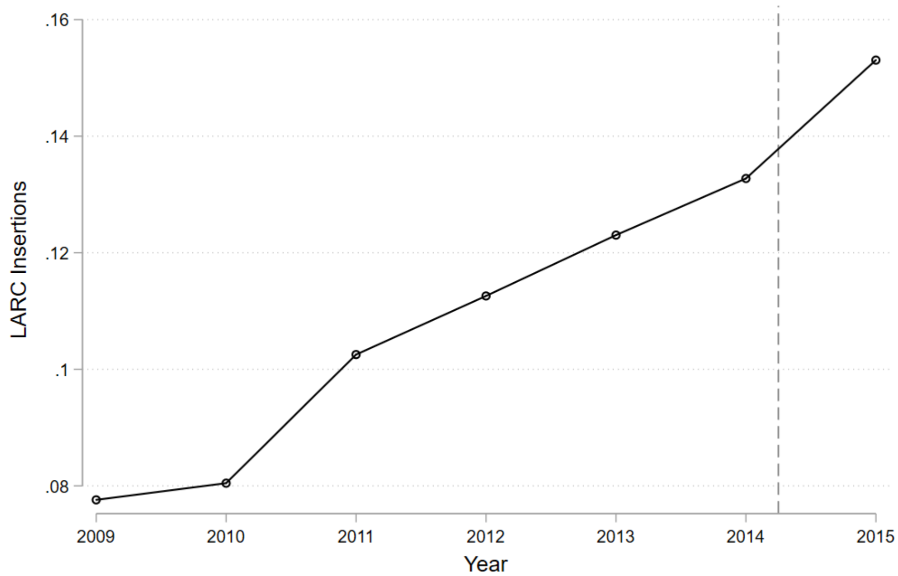


Panel D. 25–29 Year Olds



Notes: Coefficients are generated from estimating our main difference-in-differences model, as specified in Equation 2.2. The vertical line, drawn before 2009, represents the year Colorado's Family Planning Initiative was implemented. The treatment group includes zip codes within 0–7 miles of a Title X clinic. The control group includes zip codes further than 7 miles from a clinic. Estimates are relative to 2008.

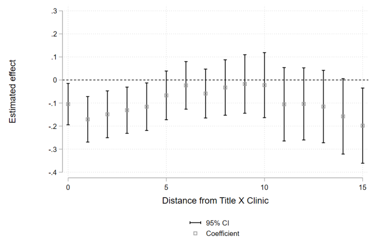
Figure A.7: LARC Insertions per Client



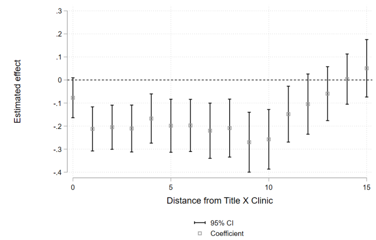
Notes: Authors' calculations based on LARC insertion data from the Colorado Department of Public Health and Environment. The vertical line, drawn before 2015, represents the initiation of media coverage.

Figure A.8: Estimated Effects of the CFPI on Births by Rolling 5-Mile Distance Bins, 2014–2015

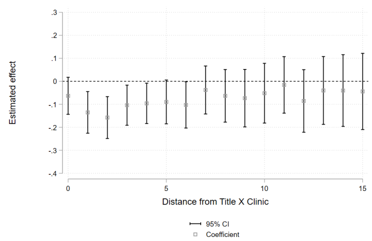
Panel A. 15–17 Year Olds, 2014



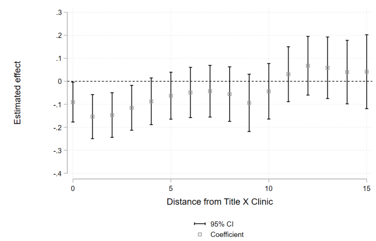
Panel B. 15–17 Year Olds, 2015



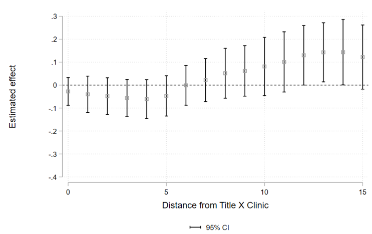
Panel C. 18–19 Year Olds, 2014



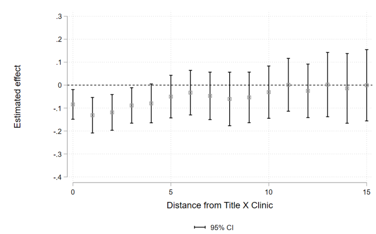
Panel D. 18–19 Year Olds, 2015



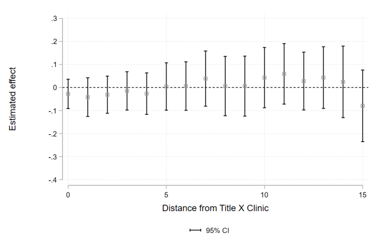
Panel E. 20–24 Year Olds, 2014



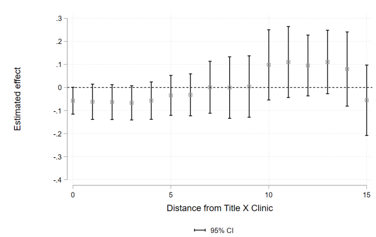
Panel F. 20–24 Year Olds, 2015



Panel G. 25–29 Year Olds, 2014



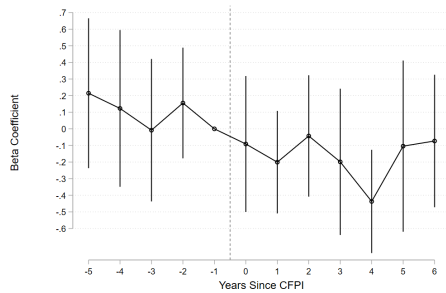
Panel H. 25–29 Year Olds, 2015



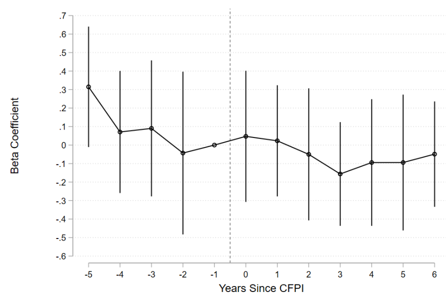
Notes: Coefficients and their respective 95% confidence intervals are generated from a regression estimated using OLS-IHS transformation, as specified in Equation 2.1, using rolling 5-mile distance bins to define treatment. A x-axis value of "i" where $i = 0, 1, \ldots 15$ indicates an estimate from a difference-in-differences analysis comparing changes in births in zip codes within i and $(i + 5)$ miles of a Title X clinic to changes in zip codes between $(i + 5)$ and 20 miles from a clinic. Zip codes greater than 20 miles from a Colorado Title X clinic are omitted from this analysis. Standard errors are clustered at the zip-code level.

Figure A.9: Estimated Effects on Abortion Rates by the Fraction of the Population Living within 7 Miles of a Title X Clinic

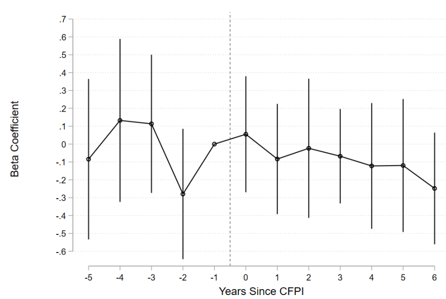
Panel A. 15–19 Year Olds



Panel B. 20–24 Year Olds

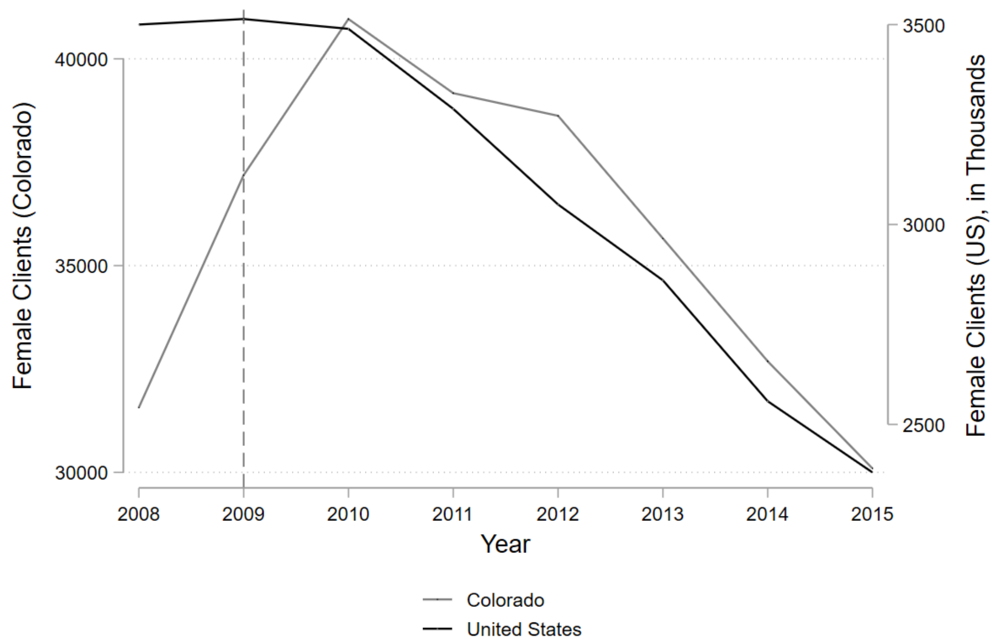


Panel C. 25–29 Year Olds



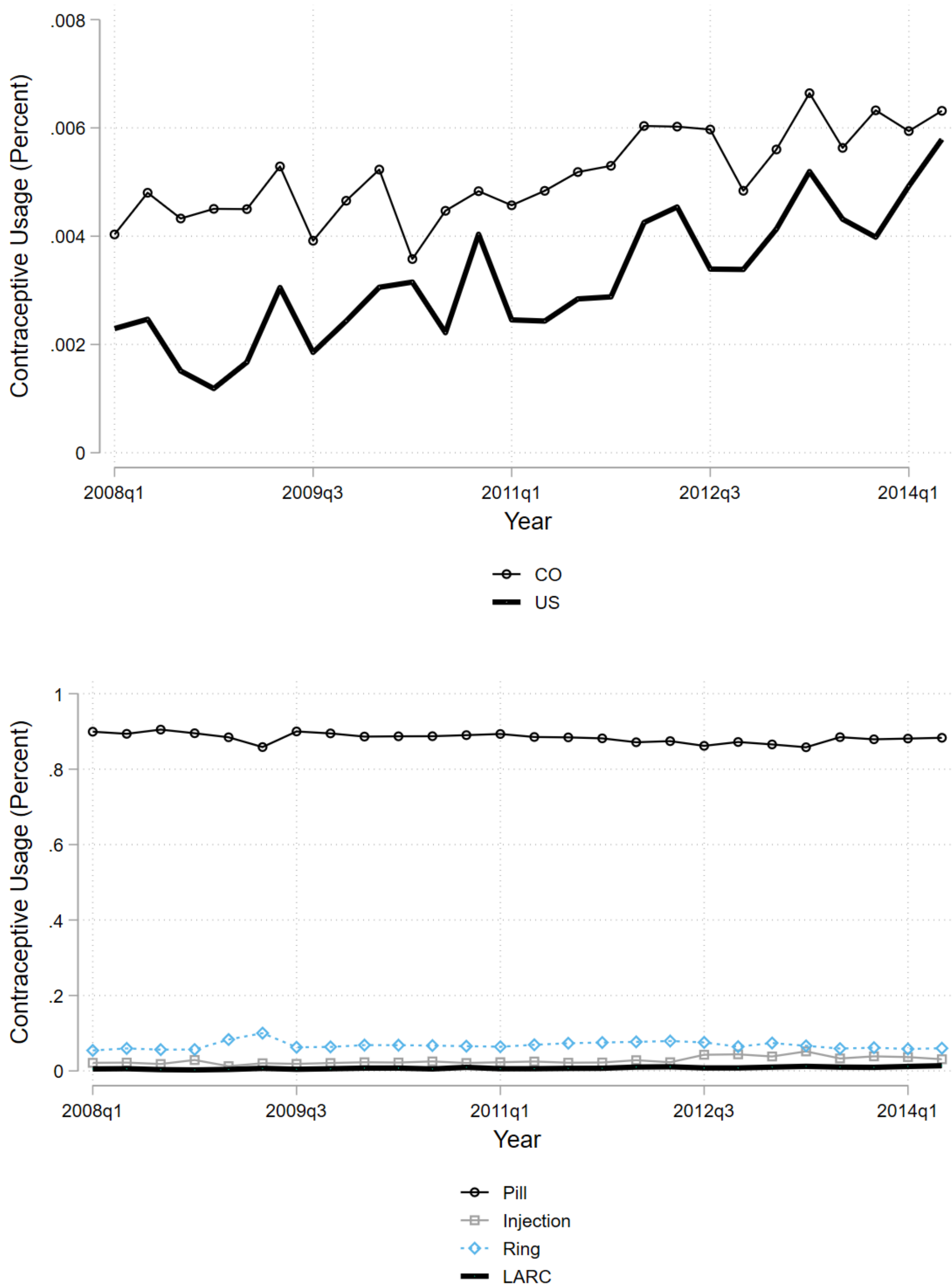
Notes: County-level abortion data are from the Colorado Department of Public Health and Environment. The outcome variable is the difference between the inverse hyperbolic sine transformations of abortions by age group and female population for the relevant age group. Coefficients are generated from estimating our main difference-in-differences model, as specified in Equation 2.2, using a continuous measure—the fraction of population in zip codes within 7 miles of a Title X clinic—to measure exposure to the CFPI. Controls include county-level unemployment rates, poverty rates, fractions of individuals aged 15–29 by age, ethnicity, and race, the percent of each age group who are Hispanic, and the percent of each age group who are black. The vertical line, drawn before 2009, represents the year Colorado's Family Planning Initiative was implemented. Estimates are relative to 2008.

Figure A.10: Female Clients Aged 15–29 Visiting Title X Clinics, Colorado versus United States



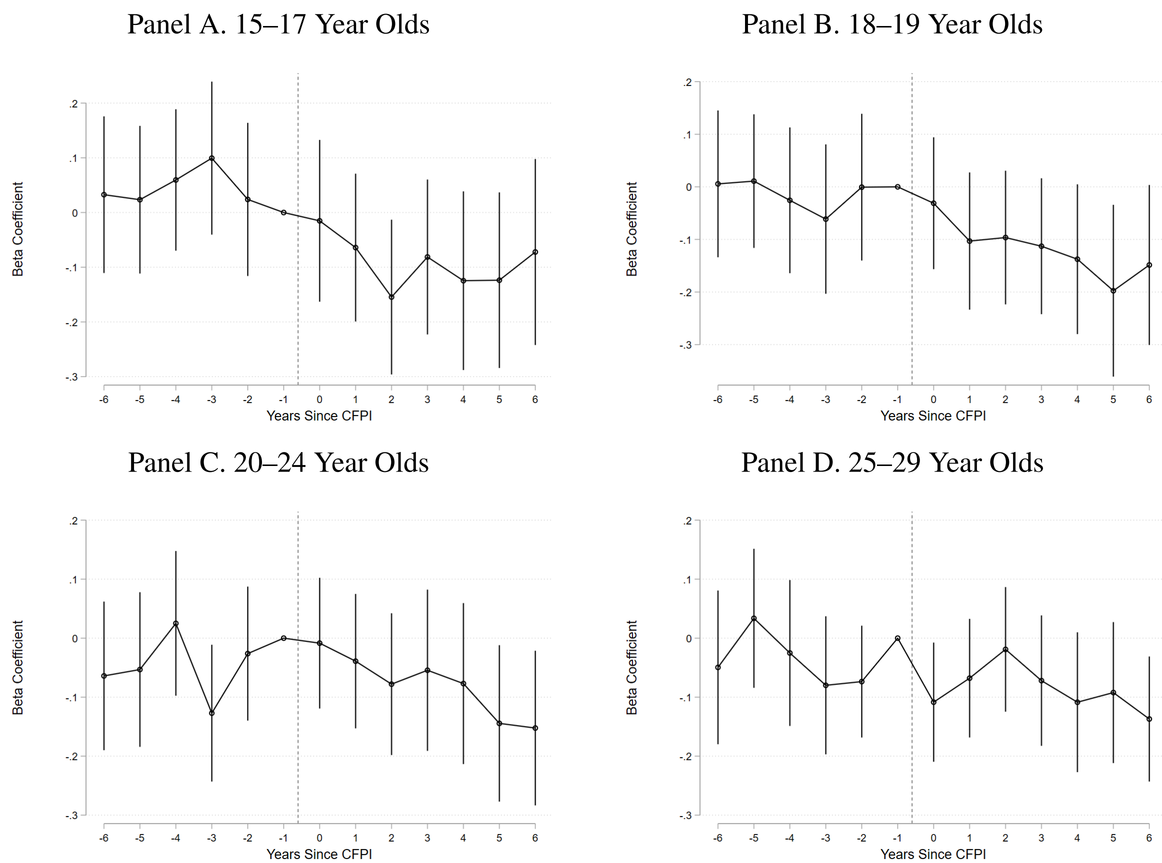
Notes: Numbers for Colorado are based on annual data on Colorado Title X contraception usage by age and method provided by the Colorado Department of Public Health and Environment. Numbers for the United States overall are from the Department of Health and Human Services Title X Family Planning Annual Reports, United States 2008–2015. The vertical line, drawn at 2009, represents the year Colorado's Family Planning Initiative was implemented.

Figure A.11: Prescription Contraceptives Sales



Notes: Numbers for Colorado are based on annual data on Colorado Title X contraception usage by age and method provided by the Colorado Department of Public Health and Environment. Numbers for the United States overall are from the Department of Health and Human Services Title X Family Planning Annual Reports, United States 2008–2015. The vertical line, drawn at 2009, represents the year Colorado's Family Planning Initiative was implemented.

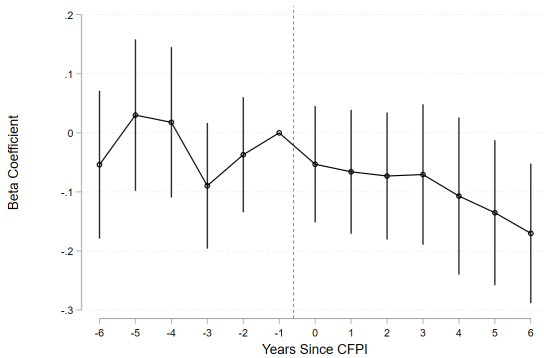
Figure A.12: Difference-in-Differences Estimates of the Effects of the CFPI on Births, with 95% Confidence Intervals



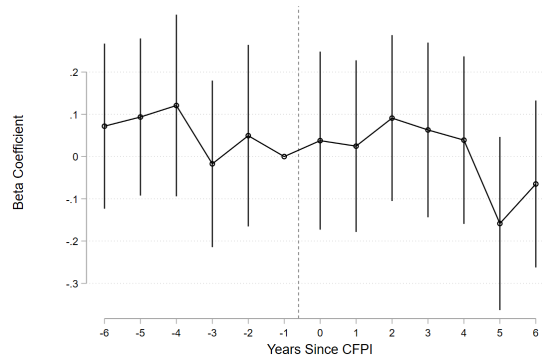
Notes: Coefficients and their respective 95% confidence intervals are generated from estimating our main difference-in-differences model, as specified in Equation 2.2. The vertical line, drawn before 2009, represents the year Colorado's Family Planning Initiative was implemented. The treatment group includes zip codes within 0–7 miles of a Title X clinic. The control group includes zip codes further than 7 miles from a clinic. All specifications include year and zip code fixed effects, county linear time trends, and demographic and economic controls. Estimates are relative to 2008.

Figure A.13: Estimated Effects of the CFPI on Births by Rolling 5-Mile Distance Bins

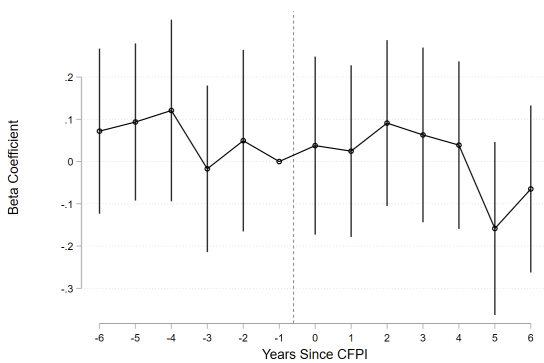
Panel A. Births 15–29 Year Olds



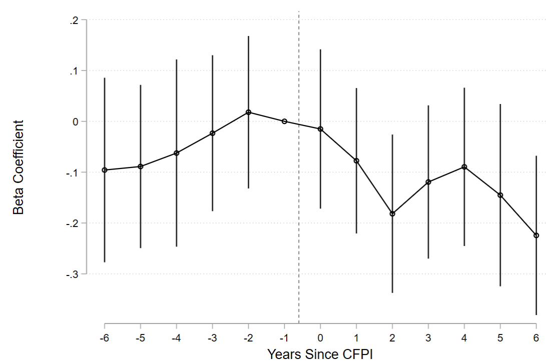
Panel B. Low Birthweight



Panel C. Very Low Birthweight



Panel D. 5-Min. Apgar Score < 9



Notes: Coefficients and their respective 95% confidence intervals are generated from estimating our main difference-in-differences model, as specified in Equation 2.1. The vertical line, drawn before 2009, represents the year Colorado's Family Planning Initiative was implemented. The treatment group includes zip codes within 0–7 miles of a Title X clinic. The control group includes zip codes further than 7 miles from a clinic. All specifications include year and zip code fixed effects, county linear time trends, and demographic and economic controls. Estimates are relative to 2008.

Tables

Table A.1: Summary Statistics - CFPI's Treated vs Comparison Zip Codes

	Within 7 Miles	Over 7 Miles
Pre-Treatment (2003–2008)		
Births to Females aged 15-17	7.74	1.79
Births to Females aged 18-19	14.37	3.99
Births to Females aged 20-24	51.61	15.55
Births to Females aged 25-29	58.72	19.50
Percent Poverty Rate	11.56	12.06
Unemployment Rate	0.05	0.05
Population (County)	41888	15678
Percent Hispanic (County)	21.24	18.15
Percent Black (County)	4.80	2.24
Percent White (County)	70.96	77.50
Travel Distance to Nearest Title X Clinic	3.62	34.87
Driving Time to Nearest Title X Clinic	9.24	53.80
Post-Treatment (2009–2015)		
Births to Females aged 15-17	3.89	0.98
Births to Females aged 18-19	9.02	2.79
Births to Females aged 20-24	39.56	13.25
Births to Females aged 25-29	55.48	19.18
Percent Poverty Rate	13.41	14.13
Unemployment Rate	0.07	0.07
Population	53140	20531
Percent Hispanic (County)	22.50	19.63
Percent Black (County)	5.07	2.65
Percent White (County)	69.25	75.52
Travel Distance to Nearest Title X Clinic	3.62	34.87
Driving Time to Nearest Title X Clinic	9.24	53.80

Notes: Birth data are from the Colorado Department of Public Health and Environment. Unemployment rates are from the BLS. Zip-Code-level population data are from the 2010 ACS. Column 1 shows the means for treated zip codes in our sample, i.e., Colorado zip codes within 7 miles of a Title X clinic. Column 2 displays the means for the comparison zip codes, i.e., zip codes in Colorado further than 7 miles from a Title X clinic.

Table A.2: The Effect of CFPI on Births

	15–17 Year Olds		18–19 Year Olds		20–24 Year Olds		25–29 Year Olds	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect of Initiative in First Year	-0.099**	-0.091**	-0.069	-0.074*	-0.037	-0.029	-0.056	-0.052
	(0.044)	(0.044)	(0.043)	(0.044)	(0.038)	(0.039)	(0.037)	(0.037)
Effect of Initiative in Second Year	-0.176***	-0.165***	-0.121***	-0.127***	-0.057	-0.052	-0.025	-0.020
	(0.047)	(0.048)	(0.045)	(0.045)	(0.039)	(0.040)	(0.040)	(0.040)
Effect of Initiative in Third Year	-0.251***	-0.233***	-0.183***	-0.183***	-0.060	-0.063	-0.031	-0.038
	(0.049)	(0.049)	(0.044)	(0.044)	(0.039)	(0.039)	(0.037)	(0.037)
Effect of Initiative in Fourth Year	-0.193***	-0.182***	-0.190***	-0.188***	-0.057	-0.059	-0.050	-0.047
	(0.052)	(0.052)	(0.046)	(0.046)	(0.045)	(0.045)	(0.040)	(0.040)
Effect of Initiative in Fifth Year	-0.271***	-0.259***	-0.226***	-0.220***	-0.059	-0.067	-0.059	-0.058
	(0.061)	(0.061)	(0.052)	(0.053)	(0.046)	(0.046)	(0.045)	(0.045)
Effect of Initiative in Sixth Year	-0.236***	-0.225***	-0.233***	-0.225***	-0.121***	-0.134***	-0.072	-0.079*
	(0.060)	(0.061)	(0.051)	(0.052)	(0.042)	(0.043)	(0.045)	(0.047)
Effect of Initiative in Seventh Year	-0.269***	-0.252***	-0.243***	-0.229***	-0.148***	-0.159***	-0.094**	-0.104**
	(0.058)	(0.058)	(0.053)	(0.054)	(0.043)	(0.044)	(0.041)	(0.042)
Average effect	-0.213	-0.201	-0.181	-0.178	-0.077	-0.080	-0.055	-0.057
P-value (test average effect = 0)	0.000	0.000	0.000	0.000	0.012	0.009	0.075	0.070
Average effect in years 6-7	-0.253	-0.238	-0.238	-0.227	-0.134	-0.146	-0.083	-0.091
P-value (test average effect in years 6-7 = 0)	0.000	0.000	0.000	0.000	0.000	0.000	0.030	0.021
Observations	7644	7644	7644	7642	7644	7644	7644	7644
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Estimates are based on restricted Natality files by zip code for the state of Colorado from 2003–2015. All specifications include year and zip code fixed effects. Controls include zip-code-level unemployment rates and poverty rates and county-level fractions of individuals aged 15–29 by age, ethnicity, and race, the percent of each age group who are Hispanic, and the percent of each age group who are black. Standard errors are clustered at the zip-code level.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.3: The Effect of CFPI on Births by Zip Code Poverty Rate

	15–17 Year Olds		18–19 Year Olds		20–24 Year Olds		25–29 Year Olds	
	Low Pov. (1)	High Pov. (2)	Low Pov. (3)	High Pov. (4)	Low Pov. (5)	High Pov. (6)	Low Pov. (7)	High Pov. (8)
Effect of Initiative in First Year	-0.115	-0.062	-0.078	-0.050	-0.033	-0.036	-0.082	-0.032
	(0.081)	(0.056)	(0.080)	(0.053)	(0.059)	(0.052)	(0.070)	(0.041)
Effect of Initiative in Second Year	-0.126	-0.166***	-0.199**	-0.065	-0.135**	-0.004	-0.145**	0.083*
	(0.090)	(0.055)	(0.088)	(0.052)	(0.061)	(0.052)	(0.066)	(0.049)
Effect of Initiative in Third Year	-0.184**	-0.252***	-0.219***	-0.149***	-0.131*	-0.021	-0.033	-0.056
	(0.076)	(0.067)	(0.079)	(0.054)	(0.067)	(0.046)	(0.065)	(0.046)
Effect of Initiative in Fourth Year	-0.134	-0.185***	-0.134*	-0.230***	-0.188**	0.045	-0.077	-0.040
	(0.083)	(0.067)	(0.074)	(0.058)	(0.074)	(0.058)	(0.072)	(0.046)
Effect of Initiative in Fifth Year	-0.226**	-0.255***	-0.139	-0.261***	-0.195**	0.021	-0.140*	-0.014
	(0.114)	(0.073)	(0.089)	(0.067)	(0.082)	(0.057)	(0.084)	(0.052)
Effect of Initiative in Sixth Year	-0.152	-0.256***	-0.339***	-0.155***	-0.270***	-0.024	-0.161*	-0.046
	(0.106)	(0.078)	(0.098)	(0.059)	(0.078)	(0.050)	(0.087)	(0.051)
Effect of Initiative in Seventh Year	-0.275**	-0.225***	-0.335***	-0.159**	-0.280***	-0.075	-0.194***	-0.031
	(0.110)	(0.069)	(0.091)	(0.066)	(0.079)	(0.052)	(0.069)	(0.052)
Average effect	-0.173	-0.200	-0.206	-0.153	-0.176	-0.013	-0.119	-0.019
P-value (test average effect = 0)	0.010	0.000	0.000	0.000	0.001	0.701	0.052	0.528
Average effect in years 6-7	-0.214	-0.241	-0.337	-0.157	-0.275	-0.050	-0.178	-0.038
P-value (test average effect in years 6-7 = 0)	0.025	0.000	0.000	0.005	0.000	0.249	0.013	0.373
Observations	3796	3848	3796	3846	3796	3848	3796	3848

Notes: Estimates are based on restricted Natality files by zip code for the state of Colorado from 2003–2015. All specifications include year and zip code fixed effects, and demographic and economic controls. “Low Pov." zip codes are zip codes with poverty rates at or below the 2010 Colorado median poverty rate. “High Pov." zip codes are defined as zip codes with poverty rates above the 2010 Colorado median poverty rate. Standard errors are clustered at the zip-code level.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.4: The Effect of CFPI on Births by Race and Ethnicity

	15–17 Year Olds			18–19 Year Olds			20–24 Year Olds			25–29 Year Olds		
	White (1)	Black (2)	Hispanic (3)	White (4)	Black (5)	Hispanic (6)	White (7)	Black (8)	Hispanic (9)	White (10)	Black (11)	Hispanic (12)
Effect of Initiative in First Year	-0.087**	-0.091**	-0.097**	0.040	0.006	-0.089**	-0.008	0.034	-0.065*	-0.002	0.005	-0.076**
	(0.043)	(0.044)	(0.043)	(0.042)	(0.004)	(0.042)	(0.039)	(0.039)	(0.039)	(0.036)	(0.039)	(0.038)
Effect of Initiative in Second Year	-0.134***	-0.165***	-0.168***	-0.127***	-0.002	-0.097**	-0.073*	-0.057	-0.046	0.010	0.023	-0.021
	(0.050)	(0.048)	(0.046)	(0.047)	(0.007)	(0.047)	(0.041)	(0.038)	(0.042)	(0.040)	(0.040)	(0.037)
Effect of Initiative in Third Year	-0.163***	-0.233***	-0.235***	-0.184***	0.006*	-0.146***	-0.063	-0.086**	-0.057	0.012	-0.011	-0.054
	(0.045)	(0.049)	(0.050)	(0.049)	(0.004)	(0.045)	(0.043)	(0.040)	(0.036)	(0.038)	(0.045)	(0.039)
Effect of Initiative in Fourth Year	-0.138***	-0.182***	-0.223***	-0.188***	0.006	-0.227***	-0.078*	-0.118***	-0.110**	-0.025	-0.018	-0.066
	(0.051)	(0.052)	(0.049)	(0.052)	(0.004)	(0.045)	(0.044)	(0.040)	(0.043)	(0.039)	(0.043)	(0.043)
Effect of Initiative in Fifth Year	-0.170***	-0.259***	-0.320***	-0.177***	0.002	-0.253***	-0.131***	0.002	-0.152***	-0.038	0.014	-0.050
	(0.053)	(0.061)	(0.060)	(0.052)	(0.005)	(0.052)	(0.047)	(0.043)	(0.052)	(0.046)	(0.041)	(0.042)
Effect of Initiative in Sixth Year	-0.150***	-0.225***	-0.290***	-0.216***	0.006	-0.234***	-0.119**	-0.072*	-0.181***	-0.079*	0.033	-0.004
	(0.054)	(0.061)	(0.055)	(0.053)	(0.004)	(0.051)	(0.046)	(0.042)	(0.045)	(0.044)	(0.044)	(0.042)
Effect of Initiative in Seventh Year	-0.204***	-0.252***	-0.244***	-0.268***	0.003	-0.241***	-0.157***	-0.085**	-0.214***	-0.105**	0.082*	-0.044
	(0.059)	(0.058)	(0.053)	(0.061)	(0.005)	(0.050)	(0.046)	(0.043)	(0.045)	(0.042)	(0.049)	(0.046)
Average effect	-0.149	-0.201	-0.225	-0.160	0.004	-0.184	-0.090	-0.055	-0.118	-0.033	0.018	-0.045
P-value (test average effect = 0)	0.000	0.000	0.000	0.000	0.369	0.000	0.002	0.043	0.000	0.289	0.509	0.112
Average effect in years 6-7	-0.177	-0.238	-0.267	-0.242	0.004	-0.238	-0.138	-0.079	-0.198	-0.092	0.058	-0.024
P-value (test average effect in years 6-7 = 0)	0.000	0.000	0.000	0.000	0.348	0.000	0.001	0.029	0.000	0.016	0.133	0.525
Observations	7644	7644	7644	7642	3885	7642	7644	7644	7644	7644	7644	7644

Notes: Estimates are based on restricted Natality files by zip code for the state of Colorado from 2003–2015. All specifications include year and zip code fixed effects, and demographic and economic controls. Standard errors are clustered at the zip-code level.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.5: The Effect of CFPI on Abortion Rates
County-level Analysis Based on Share of the Population Within 7 miles of a Title X Clinic

	15–19 Year Olds		20–24 Year Olds		25–29 Year Olds	
	(1)	(2)	(3)	(4)	(5)	(6)
Effect of Initiative in First Year	-0.171	-0.321***	-0.053	-0.095	0.108	0.033
	(0.125)	(0.117)	(0.174)	(0.180)	(0.146)	(0.198)
Effect of Initiative in Second Year	-0.277*	-0.396***	-0.086	-0.124	-0.039	-0.041
	(0.161)	(0.140)	(0.167)	(0.169)	(0.167)	(0.176)
Effect of Initiative in Third Year	-0.138	-0.262	-0.165	-0.144	0.014	0.038
	(0.171)	(0.216)	(0.179)	(0.192)	(0.251)	(0.278)
Effect of Initiative in Fourth Year	-0.281	-0.358*	-0.263	-0.219	-0.039	-0.045
	(0.220)	(0.189)	(0.171)	(0.198)	(0.180)	(0.179)
Effect of Initiative in Fifth Year	-0.523***	-0.532***	-0.201	-0.134	-0.087	-0.090
	(0.176)	(0.164)	(0.168)	(0.173)	(0.218)	(0.189)
Effect of Initiative in Sixth Year	-0.202	-0.220	-0.198	-0.169	-0.079	0.004
	(0.243)	(0.219)	(0.193)	(0.202)	(0.212)	(0.234)
Effect of Initiative in Seventh Year	-0.158	-0.227	-0.163	-0.130	-0.208	-0.133
	(0.227)	(0.266)	(0.182)	(0.211)	(0.204)	(0.222)
Average effect	-0.250	-0.331	-0.161	-0.145	-0.047	-0.033
P-value (test average effect = 0)	0.048	0.006	0.187	0.254	0.769	0.852
Average effect in years 6-7	-0.180	-0.223	-0.181	-0.149	-0.144	-0.064
P-value (test average effect in years 6-7 = 0)	0.362	0.301	0.281	0.428	0.444	0.765
Observations	370	370	463	463	409	409
Controls	No	Yes	No	Yes	No	Yes

Notes: Estimates are based on restricted zip-code-level abortion data from the Colorado Department of Public Health and Environment for the state of Colorado from 2004–2015. The outcome variable is the difference between the inverse hyperbolic sine transformations of abortions by age group and female population for the relevant age group. The fraction treated indicates the percent of the population living in zip codes within 7 miles of a clinic. All specifications include year and county fixed effects. Controls include county-level unemployment rates and poverty rates and county-level fractions of individuals aged 15–29 by age, ethnicity, and race, the percent of each age group who are Hispanic, and the percent of each age group who are black. Standard errors are clustered at the county level.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.6: The Effect of CFPI on Births Typically Involving Relatively High Costs

	All Births	Low Birthweight	Very Low Birthweight	5-Min. Apgar Score < 9
	(1)	(2)	(3)	(4)
Effect of Initiative in First Year	-0.048	-0.058	-0.045	0.032
	(0.035)	(0.051)	(0.064)	(0.055)
Effect of Initiative in Second Year	-0.056	-0.066	-0.091	0.086
	(0.039)	(0.058)	(0.064)	(0.055)
Effect of Initiative in Third Year	-0.078**	-0.155***	-0.034	-0.048
	(0.039)	(0.050)	(0.052)	(0.063)
Effect of Initiative in Fourth Year	-0.051	-0.135**	-0.104	0.000
	(0.044)	(0.057)	(0.067)	(0.058)
Effect of Initiative in Fifth Year	-0.073	-0.096*	-0.011	-0.054
	(0.048)	(0.052)	(0.061)	(0.057)
Effect of Initiative in Sixth Year	-0.115**	-0.142**	-0.158**	-0.164***
	(0.046)	(0.060)	(0.064)	(0.062)
Effect of Initiative in Seventh Year	-0.132***	-0.182***	-0.185***	-0.306***
	(0.042)	(0.051)	(0.058)	(0.060)
Average effect	-0.079	-0.119	-0.090	-0.065
P-value (test average effect = 0)	0.020	0.000	0.004	0.123
Average effect in years 6-7	-0.124	-0.162	-0.172	-0.235
P-value (test average effect in years 6-7 = 0)	0.002	0.000	0.000	0.000
Observations	7644	6355	6355	6355

Notes: Estimates are based on restricted Natality files by zip code for the state of Colorado from 2003–2015. All specifications include year and zip code fixed effects, and demographic and economic controls. "Total Births" include all births to women aged 15-29. "Low Birthweight" indicates the number of infants within a zip code born under 2500 grams. "Very Low Birthweight" indicates the number of infants within a zip code born under 1500 grams. "5-Min. Apgar Score < 9" measures the number of infants scoring less than a 9 out of 10 on the 5-Minute Apgar test.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively. Standard errors are clustered at the zip-code level.

Table A.7: The Effect of CFPI on Births by Urbanicity

	15–17 Year Olds		18–19 Year Olds		20–24 Year Olds		25–29 Year Olds	
	Rural (1)	Urban (2)	Rural (3)	Urban (4)	Rural (5)	Urban (6)	Rural (7)	Urban (8)
Effect of Initiative in First Year	-0.128	-0.052	0.026	-0.136**	0.030	-0.007	-0.060	-0.102*
	(0.119)	(0.057)	(0.122)	(0.057)	(0.097)	(0.053)	(0.102)	(0.054)
Effect of Initiative in Second Year	-0.234*	-0.108*	0.006	-0.115**	0.055	-0.087	-0.119	-0.017
	(0.129)	(0.061)	(0.114)	(0.058)	(0.084)	(0.054)	(0.080)	(0.058)
Effect of Initiative in Third Year	-0.277**	-0.148**	-0.186*	-0.131**	-0.042	-0.091	-0.089	-0.032
	(0.126)	(0.067)	(0.097)	(0.058)	(0.087)	(0.056)	(0.091)	(0.058)
Effect of Initiative in Fourth Year	-0.073	-0.150**	-0.118	-0.173***	0.016	-0.091	-0.142	-0.029
	(0.100)	(0.065)	(0.107)	(0.062)	(0.099)	(0.062)	(0.087)	(0.058)
Effect of Initiative in Fifth Year	-0.139	-0.242***	-0.259*	-0.160**	-0.078	-0.037	-0.072	-0.050
	(0.117)	(0.078)	(0.139)	(0.063)	(0.112)	(0.060)	(0.109)	(0.061)
Effect of Initiative in Sixth Year	-0.173	-0.135*	-0.261*	-0.154**	-0.063	-0.167***	-0.139	-0.045
	(0.150)	(0.076)	(0.133)	(0.067)	(0.107)	(0.060)	(0.115)	(0.065)
Effect of Initiative in Seventh Year	-0.245**	-0.120	-0.093	-0.189***	-0.024	-0.190***	-0.060	-0.136**
	(0.113)	(0.078)	(0.126)	(0.068)	(0.112)	(0.058)	(0.096)	(0.061)
Average effect	-0.181	-0.136	-0.127	-0.151	-0.015	-0.096	-0.097	-0.059
P-value (test average effect = 0)	0.036	0.006	0.076	0.000	0.825	0.030	0.138	0.236
Average effect in years 6-7	-0.209	-0.127	-0.177	-0.172	-0.043	-0.179	-0.100	-0.091
P-value (test average effect in years 6-7 = 0)	0.080	0.061	0.108	0.004	0.669	0.001	0.297	0.119
Observations	3757	3887	3755	3887	3757	3887	3757	3887

Notes: Estimates are based on restricted Natality files by zip code for the state of Colorado from 2003–2015. All specifications include year and zip code fixed effects, and demographic and economic controls. Data on urbanicity is from the University of North Dakota's Rural Health Center. "Rural" zip codes include micropolitan areas, small towns, and rural areas, while "Urban" zip codes include metropolitan areas. Standard errors are clustered at the zip-code level.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.8: Difference-in-Difference Estimates for Compositional Changes between Treated and Comparison Zip Codes, 2000–2010

IHS(Number of Women aged 15-29)	0.000
	(0.167)
Percent White (Non-Hispanic Women aged 15-29)	0.033
	(0.031)
Percent Black (Non-Hispanic Women aged 15-29)	-0.004
	(0.006)
Percent Hispanic (Women aged 15-29)	-0.019
	(0.013)
Predicted IHS(Births to Women aged 15-29)	-0.059
	(0.107)

Notes: Estimates are based on Census data for zip code demographics, showing the difference in differences between treated and comparison zip codes from 2000 to 2010. IHS represents the inverse-hyperbolic-sine transformation. The predicted IHS of births is calculated using estimated coefficients from a regression of IHS(birth count) on zip code demographics from 2010, then multiplying those coefficients by the observed demographics in 2000 and 2010. The regression using 2010 data to evaluate how demographics predict births takes the form of $IHS(births)_{zy} = \alpha + \beta_1 * IHS(No.Females15-29)_{zy} + \beta_2 * PctWhite_{zy} + \beta_3 * PctBlack_{zy} + \beta_5 * PctHispanic_{zy}$, where "$PctWhite$" is the percent of females aged 15-29 that are white, etc. This yields an estimated model for $Predicted\ IHS(births)_{zy} = -2.02 + 0.87 * IHS(No.Females15-29)_{zy} + 0.05 * PctWhite_{zy} + 0.21 * PctBlack_{zy} + .27 * PctHispanic_{zy}$.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.9: The Effect of CFPI on Births, Using Alternative Measures of Distance

	15–17 Year Olds		18–19 Year Olds		20–24 Year Olds		25–29 Year Olds	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. "As the Crow Flies" Distance								
Effect of Initiative in First Year	-0.076*	-0.068	-0.062	-0.066	-0.009	-0.001	-0.005	0.000
	(0.041)	(0.041)	(0.040)	(0.041)	(0.039)	(0.040)	(0.038)	(0.039)
Effect of Initiative in Second Year	-0.183***	-0.175***	-0.113***	-0.119***	-0.021	-0.015	-0.003	0.001
	(0.042)	(0.043)	(0.041)	(0.042)	(0.040)	(0.041)	(0.040)	(0.040)
Effect of Initiative in Third Year	-0.258***	-0.246***	-0.160***	-0.159***	-0.037	-0.040	-0.016	-0.023
	(0.046)	(0.046)	(0.042)	(0.042)	(0.041)	(0.041)	(0.040)	(0.040)
Effect of Initiative in Fourth Year	-0.175***	-0.167***	-0.155***	-0.152***	-0.017	-0.016	-0.050	-0.047
	(0.048)	(0.047)	(0.044)	(0.045)	(0.044)	(0.044)	(0.042)	(0.041)
Effect of Initiative in Fifth Year	-0.293***	-0.284***	-0.200***	-0.193***	-0.044	-0.055	-0.024	-0.023
	(0.054)	(0.054)	(0.047)	(0.048)	(0.045)	(0.044)	(0.045)	(0.045)
Effect of Initiative in Sixth Year	-0.259***	-0.252***	-0.203***	-0.194***	-0.110***	-0.121***	-0.029	-0.035
	(0.054)	(0.055)	(0.047)	(0.048)	(0.042)	(0.042)	(0.047)	(0.047)
Effect of Initiative in Seventh Year	-0.305***	-0.291***	-0.195***	-0.181***	-0.115***	-0.126***	-0.079*	-0.091**
	(0.052)	(0.053)	(0.049)	(0.049)	(0.042)	(0.043)	(0.043)	(0.043)
Average effect	-0.221	-0.212	-0.155	-0.152	-0.050	-0.054	-0.030	-0.031
P-value (test average effect = 0)	0.000	0.000	0.000	0.000	0.094	0.074	0.348	0.328
Average effect in years 6-7	-0.282	-0.272	-0.199	-0.188	-0.113	-0.124	-0.054	-0.063
P-value (test average effect in years 6-7 = 0)	0.000	0.000	0.000	0.000	0.002	0.001	0.165	0.113
Observations	7644	7644	7644	7642	7644	7644	7644	7644
Panel B. Driving Time (In Minutes)								
Effect of Initiative in First Year	-0.045	-0.036	-0.115**	-0.124**	-0.009	-0.001	-0.042	-0.038
	(0.051)	(0.052)	(0.048)	(0.048)	(0.045)	(0.045)	(0.036)	(0.036)
Effect of Initiative in Second Year	-0.153***	-0.134**	-0.007	-0.010	-0.036	-0.029	-0.050	-0.045
	(0.056)	(0.057)	(0.045)	(0.045)	(0.044)	(0.045)	(0.042)	(0.042)
Effect of Initiative in Third Year	-0.219***	-0.190***	-0.154***	-0.150***	-0.036	-0.034	-0.043	-0.044
	(0.059)	(0.060)	(0.052)	(0.052)	(0.040)	(0.040)	(0.039)	(0.039)
Effect of Initiative in Fourth Year	-0.161**	-0.138**	-0.162***	-0.154***	-0.060	-0.055	-0.063	-0.053
	(0.064)	(0.063)	(0.058)	(0.058)	(0.051)	(0.051)	(0.040)	(0.040)
Effect of Initiative in Fifth Year	-0.241***	-0.216***	-0.226***	-0.211***	-0.022	-0.022	-0.059	-0.055
	(0.074)	(0.075)	(0.064)	(0.064)	(0.053)	(0.053)	(0.048)	(0.048)
Effect of Initiative in Sixth Year	-0.204***	-0.178**	-0.156**	-0.137**	-0.055	-0.063	-0.045	-0.048
	(0.072)	(0.074)	(0.063)	(0.064)	(0.046)	(0.047)	(0.049)	(0.051)
Effect of Initiative in Seventh Year	-0.252***	-0.226***	-0.147**	-0.125**	-0.098*	-0.100*	-0.056	-0.061
	(0.071)	(0.073)	(0.062)	(0.063)	(0.050)	(0.052)	(0.044)	(0.045)
Average effect	-0.182	-0.160	-0.138	-0.130	-0.045	-0.043	-0.051	-0.049
P-value (test average effect = 0)	0.000	0.001	0.001	0.001	0.195	0.218	0.112	0.130
Average effect in years 6-7	-0.228	-0.202	-0.151	-0.131	-0.077	-0.081	-0.050	-0.055
P-value (test average effect in years 6-7 = 0)	0.001	0.003	0.007	0.020	0.075	0.069	0.222	0.197
Observations	7644	7644	7644	7642	7644	7644	7644	7644
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Estimates are based on restricted Natality files by zip code for the state of Colorado from 2003–2015. All specifications include year and zip code fixed effects. Controls include zip-code-level unemployment rates and poverty rates and county-level fractions of individuals aged 15–29 by age, ethnicity, and race, the percent of each age group who are Hispanic, and the percent of each age group who are black. Estimates in Panel A are from a model that defines treated zip codes as those within 0–7 miles "as the crow flies" of a Title X clinic, and comparison zip codes as those further than 7 crow flies miles from a Title X clinic. Estimates in Panel B are from a model that defines treated zip codes as those within 0–10 minutes driving time of a Title X clinic, and comparison zip codes as those further than 10 minutes from a Title X clinic. Standard errors are clustered at the zip-code level.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.10: The Effect of CFPI on Births, Using Within 5 miles, 7 Miles, and 10 Miles to Define Treated Zip Codes

	15–17 Year Olds			18–19 Year Olds			20–24 Year Olds			25–29 Year Olds		
	0–7 Miles (1)	0–5 Miles (2)	0–10 Miles (3)	0–7 Miles (4)	0–5 Miles (5)	0–10 Miles (6)	0–7 Miles (7)	0–5 Miles (8)	0–10 Miles (9)	0–7 Miles (10)	0–5 Miles (11)	0–10 Miles (12)
Effect of Initiative in First Year	-0.091**	-0.079	-0.054	-0.061	-0.111**	-0.061	-0.010	-0.024	-0.007	0.010	-0.033	0.010
	(0.044)	(0.050)	(0.040)	(0.041)	(0.045)	(0.041)	(0.039)	(0.042)	(0.039)	(0.040)	(0.034)	(0.040)
Effect of Initiative in Second Year	-0.165***	-0.126**	-0.136***	-0.118***	-0.054	-0.118***	-0.028	-0.043	-0.028	0.020	-0.036	0.020
	(0.048)	(0.052)	(0.042)	(0.042)	(0.044)	(0.042)	(0.041)	(0.042)	(0.041)	(0.041)	(0.040)	(0.041)
Effect of Initiative in Third Year	-0.233***	-0.183***	-0.180***	-0.163***	-0.158***	-0.163***	-0.034	-0.040	-0.037	-0.007	-0.047	-0.007
	(0.049)	(0.054)	(0.046)	(0.042)	(0.049)	(0.042)	(0.040)	(0.038)	(0.040)	(0.040)	(0.036)	(0.040)
Effect of Initiative in Fourth Year	-0.182***	-0.159***	-0.095**	-0.153***	-0.154***	-0.153***	-0.008	-0.079*	-0.014	-0.030	-0.034	-0.030
	(0.052)	(0.060)	(0.047)	(0.044)	(0.053)	(0.044)	(0.044)	(0.047)	(0.044)	(0.041)	(0.038)	(0.041)
Effect of Initiative in Fifth Year	-0.259***	-0.247***	-0.224***	-0.185***	-0.218***	-0.185***	-0.037	-0.058	-0.044	-0.002	-0.070	-0.002
	(0.061)	(0.070)	(0.055)	(0.047)	(0.058)	(0.047)	(0.045)	(0.048)	(0.045)	(0.046)	(0.044)	(0.046)
Effect of Initiative in Sixth Year	-0.225***	-0.232***	-0.189***	-0.184***	-0.168***	-0.184***	-0.103**	-0.095**	-0.111***	-0.026	-0.058	-0.026
	(0.061)	(0.068)	(0.054)	(0.048)	(0.058)	(0.048)	(0.042)	(0.045)	(0.042)	(0.048)	(0.047)	(0.048)
Effect of Initiative in Seventh Year	-0.252***	-0.201***	-0.244***	-0.191***	-0.198***	-0.191***	-0.122***	-0.154***	-0.131***	-0.073*	-0.088**	-0.073*
	(0.058)	(0.066)	(0.053)	(0.049)	(0.060)	(0.049)	(0.044)	(0.046)	(0.044)	(0.043)	(0.041)	(0.043)
Average effect	-0.201	-0.175	-0.160	-0.151	-0.152	-0.151	-0.049	-0.070	-0.053	-0.016	-0.052	-0.016
P-value (test average effect = 0)	0.000	0.000	0.000	0.000	0.000	0.000	0.110	0.029	0.080	0.625	0.082	0.625
Average effect in years 6-7	-0.238	-0.217	-0.217	-0.188	-0.183	-0.188	-0.112	-0.124	-0.121	-0.050	-0.073	-0.050
P-value (test average effect in years 6-7 = 0)	0.000	0.000	0.000	0.000	0.001	0.000	0.003	0.003	0.002	0.208	0.061	0.208
Observations	7644	7644	7644	7642	7642	7642	7644	7644	7644	7644	7644	7644
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates are based on restricted Natality files by zip code for the state of Colorado from 2003–2015. All specifications include year and zip code fixed effects. Controls include zip-code-level unemployment rates and poverty rates and county-level fractions of individuals aged 15–29 by age, ethnicity, and race, the percent of each age group who are Hispanic, and the percent of each age group who are black. Standard errors are clustered at the zip-code level.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.11: The Effect of CFPI on Births, Omitting Zip Codes Affected by Colorado Title X Openings and Closures

	15–17 Year Olds			18–19 Year Olds			20–24 Year Olds			25–29 Year Olds		
	All	No Openings	No Closures	All	No Openings	No Closures	All	No Openings	No Closures	All	No Openings	No Closures
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Effect of Initiative in First Year	-0.091**	-0.091**	-0.091**	-0.074*	-0.069	-0.074*	-0.029	-0.028	-0.029	-0.052	-0.052	-0.052
	(0.044)	(0.045)	(0.044)	(0.044)	(0.044)	(0.044)	(0.039)	(0.038)	(0.039)	(0.037)	(0.037)	(0.037)
Effect of Initiative in Second Year	-0.165***	-0.167***	-0.165***	-0.127***	-0.133***	-0.127***	-0.052	-0.057	-0.052	-0.020	-0.020	-0.020
	(0.048)	(0.048)	(0.048)	(0.045)	(0.045)	(0.045)	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)
Effect of Initiative in Third Year	-0.233***	-0.238***	-0.233***	-0.183***	-0.187***	-0.183***	-0.063	-0.064*	-0.063	-0.038	-0.030	-0.038
	(0.049)	(0.050)	(0.049)	(0.044)	(0.045)	(0.044)	(0.039)	(0.038)	(0.039)	(0.037)	(0.037)	(0.037)
Effect of Initiative in Fourth Year	-0.182***	-0.191***	-0.182***	-0.188***	-0.188***	-0.188***	-0.059	-0.055	-0.059	-0.047	-0.037	-0.047
	(0.052)	(0.052)	(0.052)	(0.046)	(0.047)	(0.046)	(0.045)	(0.045)	(0.045)	(0.040)	(0.039)	(0.040)
Effect of Initiative in Fifth Year	-0.259***	-0.271***	-0.259***	-0.220***	-0.221***	-0.220***	-0.067	-0.065	-0.067	-0.058	-0.060	-0.058
	(0.061)	(0.061)	(0.061)	(0.053)	(0.053)	(0.053)	(0.046)	(0.045)	(0.046)	(0.045)	(0.045)	(0.045)
Effect of Initiative in Sixth Year	-0.225***	-0.230***	-0.225***	-0.225***	-0.231***	-0.225***	-0.134***	-0.140***	-0.134***	-0.079*	-0.067	-0.079*
	(0.061)	(0.062)	(0.061)	(0.052)	(0.052)	(0.052)	(0.043)	(0.043)	(0.043)	(0.047)	(0.045)	(0.047)
Effect of Initiative in Seventh Year	-0.252***	-0.258***	-0.252***	-0.229***	-0.237***	-0.229***	-0.159***	-0.161***	-0.159***	-0.104**	-0.101**	-0.104**
	(0.058)	(0.059)	(0.058)	(0.054)	(0.054)	(0.054)	(0.044)	(0.043)	(0.044)	(0.042)	(0.041)	(0.042)
Average effect	-0.201	-0.206	-0.201	-0.178	-0.181	-0.178	-0.080	-0.082	-0.080	-0.057	-0.053	-0.057
P-value (test average effect = 0)	0.000	0.000	0.000	0.000	0.000	0.000	0.009	0.008	0.009	0.070	0.092	0.070
Average effect in years 6-7	-0.238	-0.244	-0.238	-0.227	-0.234	-0.227	-0.146	-0.151	-0.146	-0.091	-0.084	-0.091
P-value (test average effect in years 6-7 = 0)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.021	0.029	0.021
Observations	7644	7618	7644	7642	7616	7642	7644	7618	7644	7644	7618	7644
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates are based on restricted Natality files by zip code for the state of Colorado from 2003–2015. Columns 1, 4, 7, and 10 provide the baseline estimates for comparison, using all zip codes across the state of Colorado. Columns 2, 5, 8 and 11 omit zip codes which experienced Title X clinic openings from 2009–2015, and Columns 3, 6, 9 and 12 omit zip codes which experienced Title X closures. All specifications include year and zip code fixed effects. Controls include zip-code-level unemployment rates and poverty rates and county-level fractions of individuals aged 15–29 by age, ethnicity, and race, the percent of each age group who are Hispanic, and the percent of each age group who are black. Standard errors are clustered at the zip-code level.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.12: The Effect of CFPI on Births, OLS and WLS Estimates

	15–17 Year Olds		18–19 Year Olds		20–24 Year Olds		25–29 Year Olds	
	OLS	WLS	OLS	WLS	OLS	WLS	OLS	WLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect of Initiative in First Year	-0.091**	-0.019	-0.074*	-0.069	-0.029	-0.009	-0.052	-0.063**
	(0.044)	(0.069)	(0.044)	(0.045)	(0.039)	(0.035)	(0.037)	(0.029)
Effect of Initiative in Second Year	-0.165***	-0.023	-0.127***	-0.087*	-0.052	-0.079**	-0.020	-0.016
	(0.048)	(0.070)	(0.045)	(0.049)	(0.040)	(0.037)	(0.040)	(0.032)
Effect of Initiative in Third Year	-0.233***	0.002	-0.183***	-0.076	-0.063	-0.039	-0.038	-0.046
	(0.049)	(0.074)	(0.044)	(0.055)	(0.039)	(0.040)	(0.037)	(0.033)
Effect of Initiative in Fourth Year	-0.182***	-0.018	-0.188***	-0.046	-0.059	-0.042	-0.047	-0.019
	(0.052)	(0.071)	(0.046)	(0.057)	(0.045)	(0.039)	(0.040)	(0.031)
Effect of Initiative in Fifth Year	-0.259***	-0.022	-0.220***	-0.042	-0.067	-0.044	-0.058	-0.035
	(0.061)	(0.086)	(0.053)	(0.059)	(0.046)	(0.038)	(0.045)	(0.032)
Effect of Initiative in Sixth Year	-0.225***	0.083	-0.225***	-0.071	-0.134***	-0.070*	-0.079*	-0.047
	(0.061)	(0.088)	(0.052)	(0.058)	(0.043)	(0.039)	(0.047)	(0.038)
Effect of Initiative in Seventh Year	-0.252***	0.096	-0.229***	-0.105	-0.159***	-0.095**	-0.104**	-0.060*
	(0.058)	(0.090)	(0.054)	(0.069)	(0.044)	(0.046)	(0.042)	(0.035)
Average effect	-0.201	0.014	-0.178	-0.071	-0.080	-0.054	-0.057	-0.041
P-value (test average effect = 0)	0.000	0.807	0.000	0.079	0.009	0.091	0.070	0.150
Average effect in years 6-7	-0.238	0.090	-0.227	-0.088	-0.146	-0.082	-0.091	-0.053
P-value (test average effect in years 6-7 = 0)	0.000	0.255	0.000	0.117	0.000	0.038	0.021	0.117
Observations	7644	7644	7642	7642	7644	7644	7644	7644

Notes: Estimates are based on restricted Natality files by zip code for the state of Colorado from 2003–2015. All specifications include year and zip code fixed effects, and demographic and economic controls. Population weights for weighted least squares specifications in Columns 2, 4, 6, and 8 include the zip code-level female population from 2010, according to the American Community Survey. Standard errors are clustered at the zip-code level.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.13: The Effect of CFPI on Births in Zip Codes with Less Than 2,000 Females

	15–17 Year Olds		18–19 Year Olds		20–24 Year Olds		25–29 Year Olds	
	<2000	≥2000	<2000	≥2000	<2000	≥2000	<2000	≥2000
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect of Initiative in First Year	-0.175	-0.013	0.069	-0.072	0.057	-0.043	-0.121	-0.064**
	(0.115)	(0.060)	(0.155)	(0.056)	(0.157)	(0.038)	(0.183)	(0.032)
Effect of Initiative in Second Year	-0.139	-0.095	-0.093	-0.087	0.195	-0.103**	-0.179	-0.026
	(0.093)	(0.069)	(0.157)	(0.054)	(0.156)	(0.042)	(0.157)	(0.039)
Effect of Initiative in Third Year	-0.204**	-0.065	-0.225**	-0.081	-0.227	-0.030	-0.126	-0.019
	(0.093)	(0.074)	(0.101)	(0.058)	(0.158)	(0.044)	(0.139)	(0.040)
Effect of Initiative in Fourth Year	-0.149	-0.050	-0.140	-0.057	-0.079	-0.053	-0.258	0.016
	(0.095)	(0.074)	(0.126)	(0.066)	(0.202)	(0.047)	(0.171)	(0.039)
Effect of Initiative in Fifth Year	-0.003	-0.056	-0.208	-0.060	-0.058	-0.060	-0.131	-0.033
	(0.112)	(0.084)	(0.140)	(0.065)	(0.209)	(0.046)	(0.207)	(0.045)
Effect of Initiative in Sixth Year	0.008	0.018	-0.206*	-0.069	-0.163	-0.095**	-0.223	-0.057
	(0.126)	(0.082)	(0.124)	(0.070)	(0.176)	(0.047)	(0.212)	(0.048)
Effect of Initiative in Seventh Year	-0.218**	0.031	-0.055	-0.098	-0.048	-0.157***	-0.064	-0.074*
	(0.092)	(0.080)	(0.138)	(0.069)	(0.179)	(0.049)	(0.165)	(0.044)
Average effect	-0.126	-0.033	-0.123	-0.075	-0.046	-0.078	-0.157	-0.037
P-value (test average effect = 0)	0.075	0.531	0.174	0.076	0.754	0.024	0.287	0.283
Average effect in years 6-7	-0.105	0.024	-0.130	-0.083	-0.105	-0.126	-0.144	-0.066
P-value (test average effect in years 6-7 = 0)	0.253	0.728	0.247	0.166	0.534	0.004	0.421	0.125
Observations	3887	3757	3885	3757	3887	3757	3887	3757

Notes: Estimates are based on restricted Natality files by zip code for the state of Colorado from 2003–2015. All specifications include year and zip code fixed effects, and demographic and economic controls. "<2000" represents a subsample of Colorado zip codes with less than 2,000 total females, according to the 2010 American Community Survey, while "≥2000" represents zip codes with more than 2,000 females. Standard errors are clustered at the zip-code level.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.14: The Effect of CFPI on Births, Poisson Estimates

	15–17 Year Olds		18–19 Year Olds		20–24 Year Olds		25–29 Year Olds	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect of Initiative in First Year	-0.046	-0.029	-0.009	-0.006	-0.081**	-0.071**	-0.035	-0.028
	(0.061)	(0.061)	(0.039)	(0.040)	(0.032)	(0.033)	(0.026)	(0.025)
Effect of Initiative in Second Year	-0.058	-0.032	-0.123***	-0.115***	-0.130***	-0.119***	-0.015	0.001
	(0.068)	(0.070)	(0.042)	(0.041)	(0.033)	(0.032)	(0.030)	(0.027)
Effect of Initiative in Third Year	-0.170**	-0.138*	-0.074	-0.077	-0.063*	-0.053	-0.031	-0.011
	(0.068)	(0.071)	(0.050)	(0.048)	(0.035)	(0.033)	(0.033)	(0.030)
Effect of Initiative in Fourth Year	-0.158**	-0.150**	-0.154***	-0.160***	-0.099**	-0.094***	-0.042	-0.018
	(0.073)	(0.072)	(0.048)	(0.048)	(0.039)	(0.035)	(0.032)	(0.028)
Effect of Initiative in Fifth Year	-0.130	-0.121	-0.141**	-0.130**	-0.122***	-0.116***	-0.044	-0.012
	(0.097)	(0.096)	(0.055)	(0.055)	(0.038)	(0.034)	(0.034)	(0.030)
Effect of Initiative in Sixth Year	0.059	0.066	-0.127***	-0.128***	-0.107***	-0.109***	-0.047	-0.013
	(0.084)	(0.082)	(0.046)	(0.048)	(0.041)	(0.039)	(0.037)	(0.032)
Effect of Initiative in Seventh Year	-0.075	-0.063	-0.178***	-0.179***	-0.151***	-0.156***	-0.069**	-0.033
	(0.085)	(0.083)	(0.064)	(0.060)	(0.045)	(0.044)	(0.033)	(0.030)
Average effect	-0.082	-0.067	-0.115	-0.114	-0.107	-0.102	-0.040	-0.016
P-value (test average effect = 0)	0.093	0.163	0.001	0.000	0.001	0.001	0.148	0.499
Average effect in years 3-7	-0.008	0.001	-0.153	-0.153	-0.129	-0.133	-0.058	-0.023
P-value (test average effect in years 3-7 = 0)	0.906	0.986	0.001	0.000	0.002	0.001	0.083	0.429
Observations	5304	5304	6097	6095	6877	6877	7137	7137
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Estimates are based on restricted Natality files by zip code for the state of Colorado from 2003–2015. All specifications include year and zip code fixed effects. Controls include zip-code-level unemployment rates and poverty rates and county-level fractions of individuals aged 15–29 by age, ethnicity, and race, the percent of each age group who are Hispanic, and the percent of each age group who are black. Standard errors are clustered at the zip-code level.

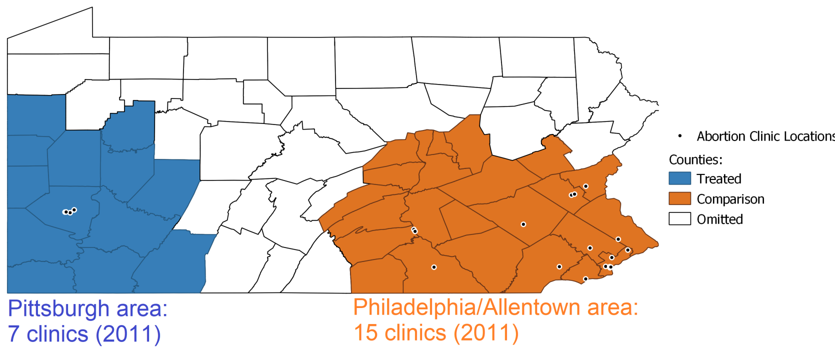
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Figures and Tables for Section 3

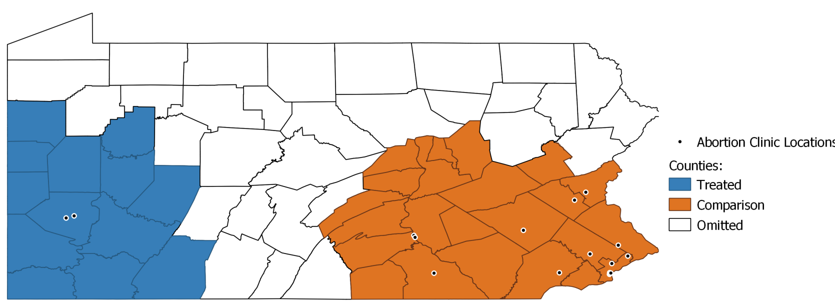
Figures

Figure A.14: Abortion Clinic Locations

2011



2013



Notes: These maps display the abortion clinic locations in 2011, prior to the law's passage, and in 2013, after the law had taken effect and clinics had closed. Counties shaded in blue (on the west side of the state) are the treated counties, while counties shaded in burnt orange (on the east side of the state) are the comparison counties. Counties in white are omitted from the main analysis, as the closest clinic in 2006 (the first year of clinic location data) closed prior to the law change.

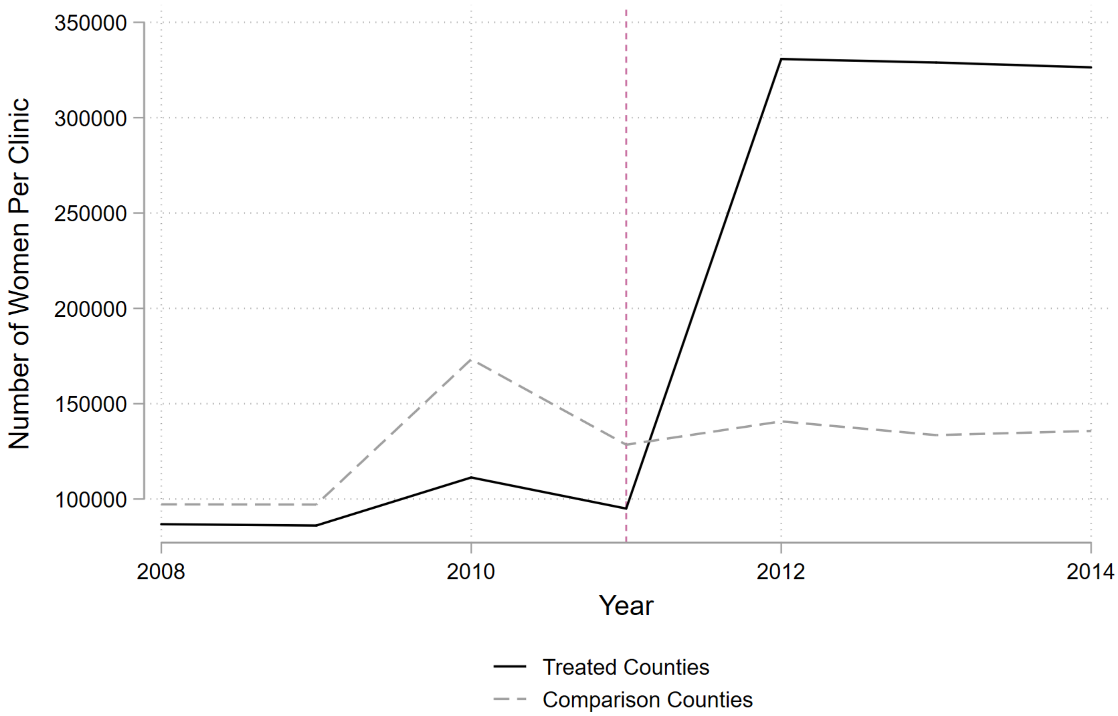
Figure A.15: Abortion Clinic Locations - Pittsburgh

2011

2013

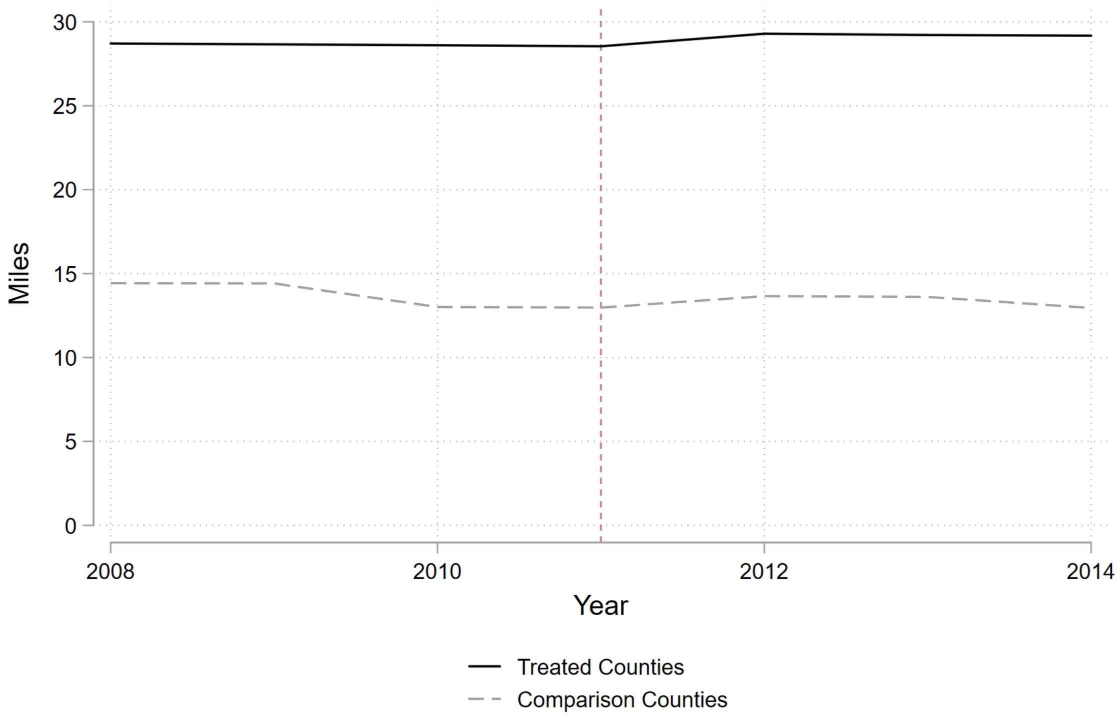
Notes: These maps display the abortion clinic locations in Pittsburgh in 2011, prior to the law's passage, and in 2013, after the law had taken effect and clinics had closed.

Figure A.16: Service Populations Over Time



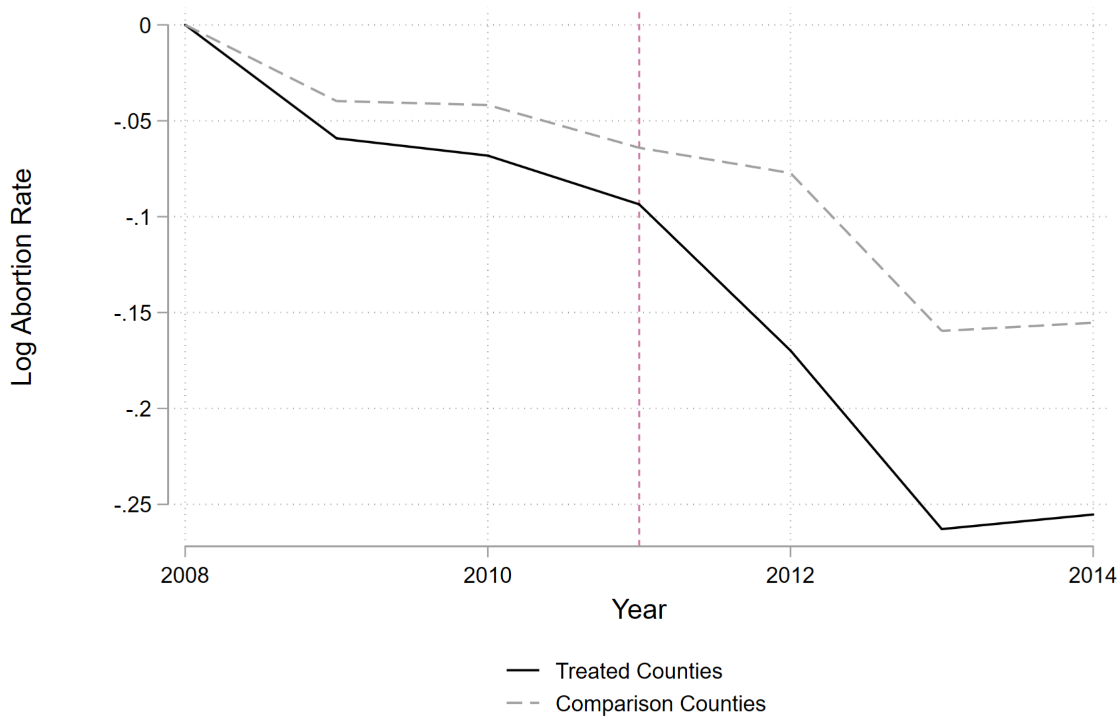
Notes: This figure shows the average service population (number of childbearing aged women divided by number of open abortion clinics) for treated and comparison counties over time. Treated counties are those for which Pittsburgh was the nearest abortion-providing city in the first year of data, comparison counties are those for which Allentown, Harrisburg, Philadelphia, Reading, Upland, Warminster, or West Chester was the nearest abortion-providing city in the first year of data.

Figure A.17: Distances Over Time



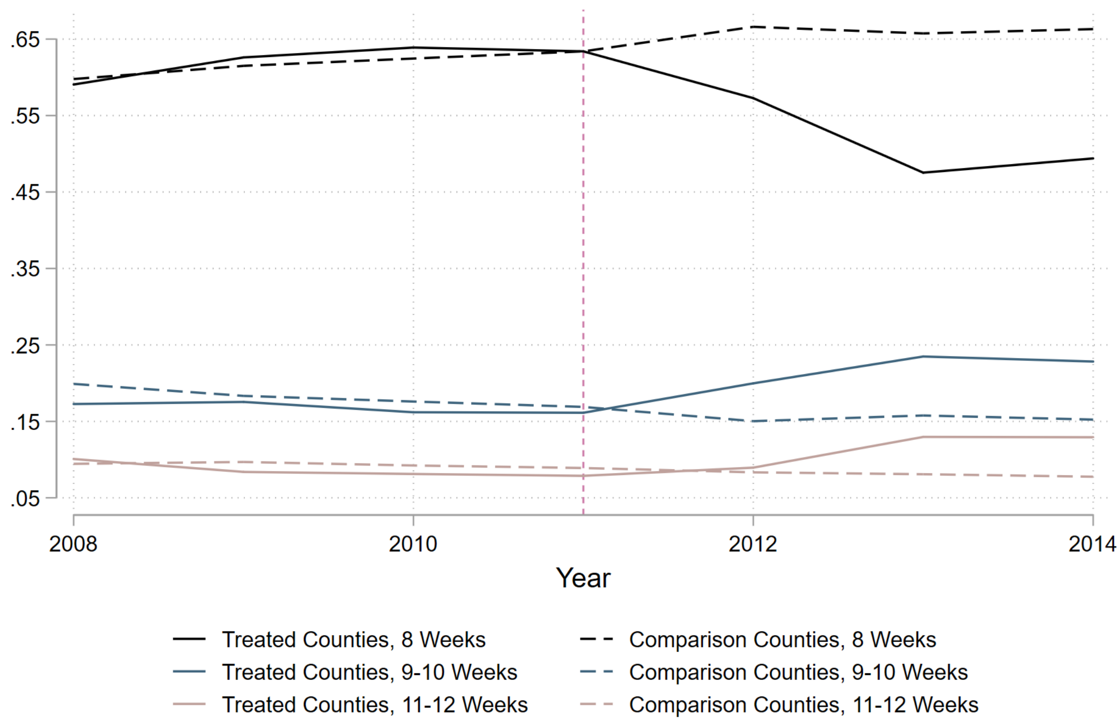
Notes: This figure plots the average distance from the county population centroid to the nearest abortion-providing facility over time, for treated and comparison counties. Treated counties are those for which Pittsburgh was the nearest abortion-providing city in the first year of data, comparison counties are those for which Allentown, Harrisburg, Philadelphia, Reading, Upland, Warminster, or West Chester was the nearest abortion-providing city in the first year of data.

Figure A.18: Abortion Rate



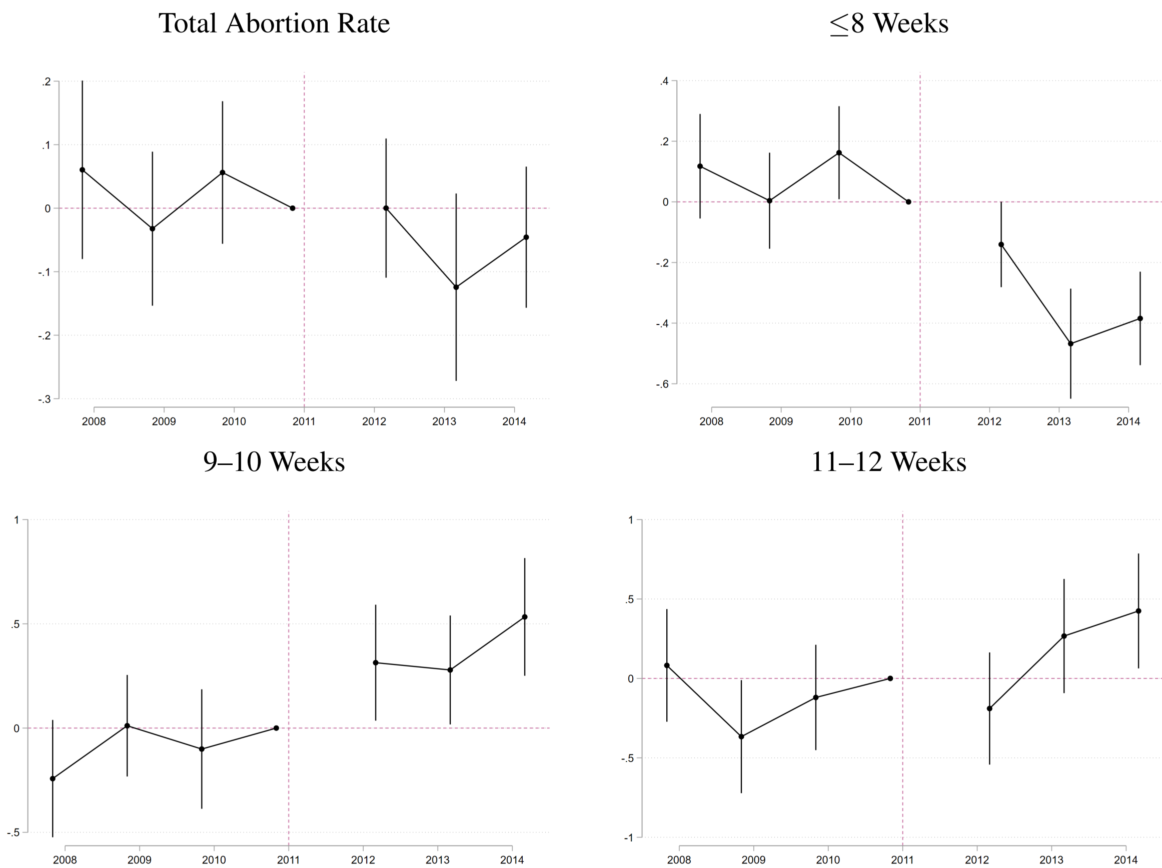
Notes: This figure plots the log abortion rate over time (minus the log abortion rate in 2008), for treated and comparison counties. Treated counties are those for which Pittsburgh was the nearest abortion-providing city in the first year of data, comparison counties are those for which Allentown, Harrisburg, Philadelphia, Reading, Upland, Warminster, or West Chester was the nearest abortion-providing city in the first year of data.

Figure A.19: Share of Abortions Occurring at Various Gestational Ages



Notes: This figure plots the average percentage of abortions occurring within various gestational ages, for treated and comparison counties. Treated counties are those for which Pittsburgh was the nearest abortion-providing city in the first year of data, comparison counties are those for which Allentown, Harrisburg, Philadelphia, Reading, Upland, Warminster, or West Chester was the nearest abortion-providing city in the first year of data.

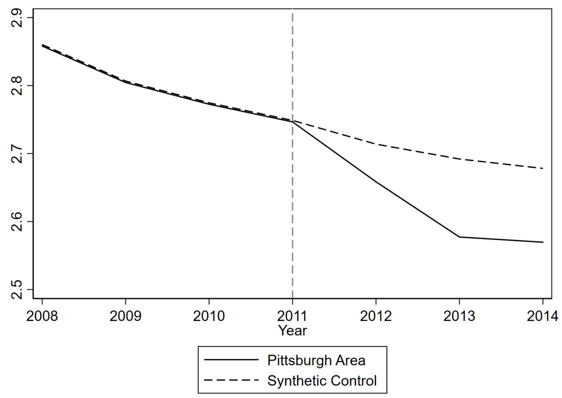
Figure A.20: Effects on Abortion Rates Overall and by Gestational Age - OLS



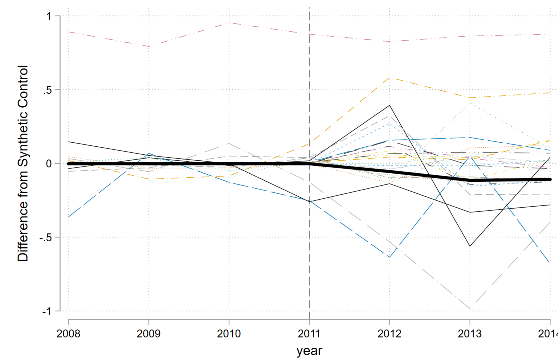
Notes: This figure plots the estimated effect of reduced local clinic capacity on abortion rates overall and by gestational age. Estimates come from a model which controls for county and year fixed effects. Treated counties are those for which Pittsburgh was the nearest abortion-providing city in the first year of data, comparison counties are those for which Allentown, Harrisburg, Philadelphia, Reading, Upland, Warminster, or West Chester was the nearest abortion-providing city in the first year of data.

Figure A.21: Synthetic Control - Main Results

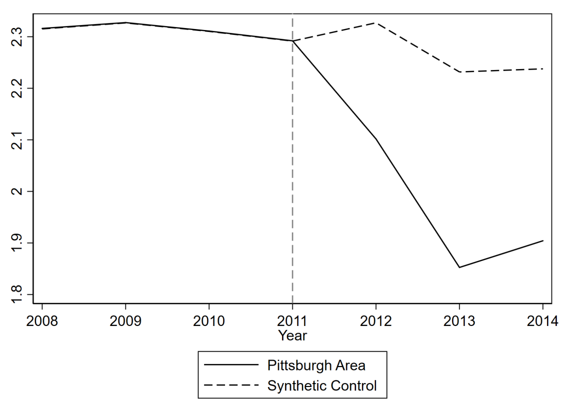
Log Total Abortion Rate



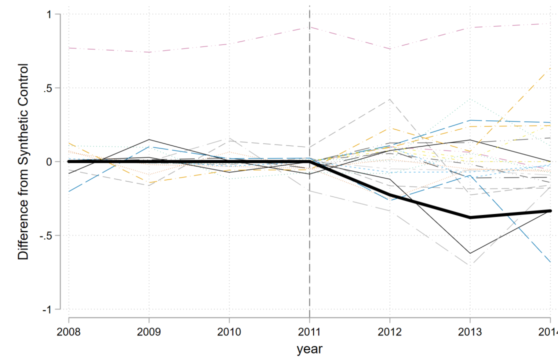
Inference: Log Total Abortion Rate



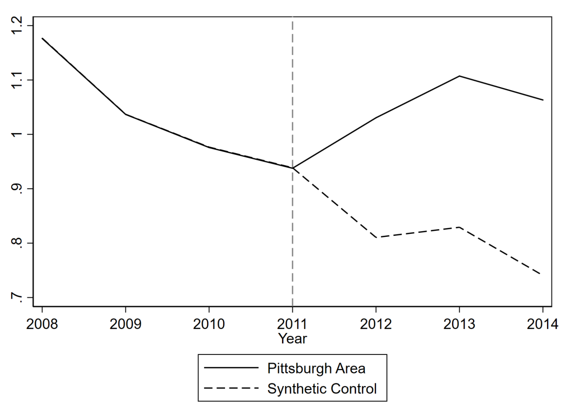
Log Abortion Rate, ≤8 Weeks



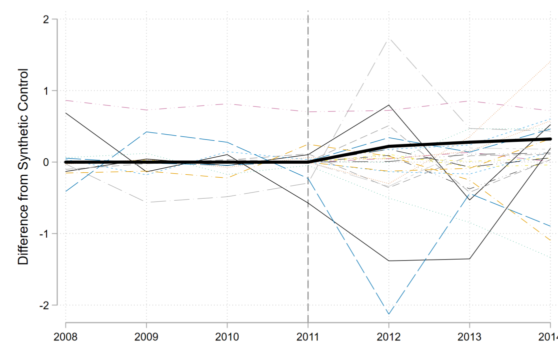
Inference: Log Abortion Rate ≤8 Weeks



Log Abortion Rate, 9–10 Weeks



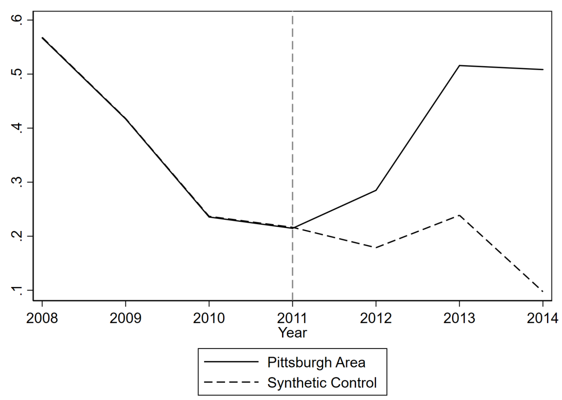
Inference: Log Abortion Rate 9–10 Weeks



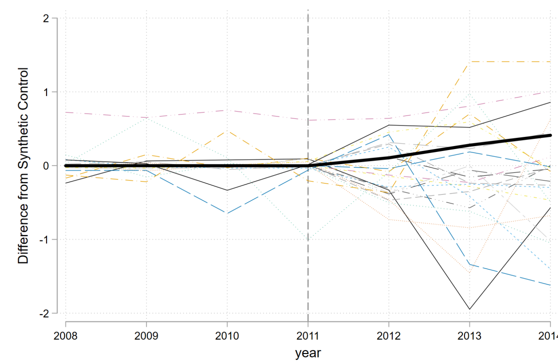
Notes: These figures come from a synthetic control method approach using the log of the respective abortion rate as the outcome. The solid line represents the treated area's log abortion rate over time, while the dashed line represnts the synthetic control's log abortion rate over time. The synthetic control places weights on counties in the donor pool in order to best match the treated area's log abortion rate in the pre-period, then maintains these weights in the post-period to show how we would have expected log abortion rates to move in the absence of abortion clinic closures.

Figure A.22: Synthetic Control - Main Results Continued

Log Abortion Rate, 11–12 Weeks



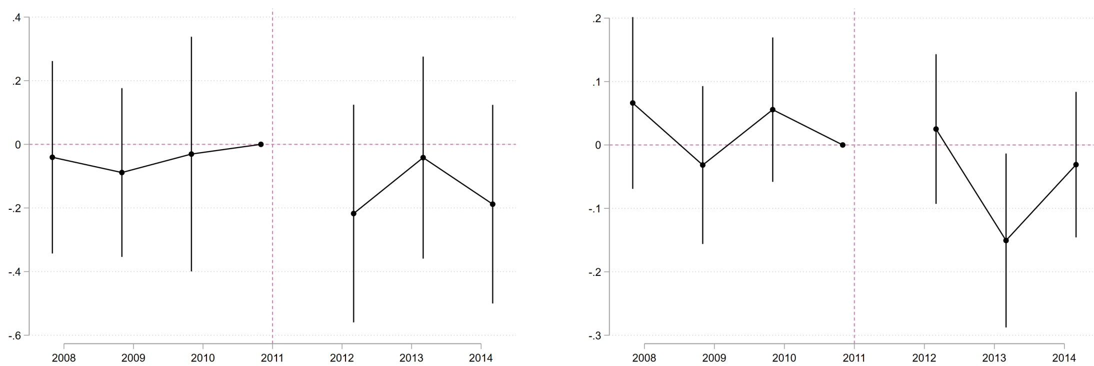
Inference: Log Abortion Rate 11–12 Weeks



Notes: These figures come from a synthetic control method approach using the log of the respective abortion rate as the outcome. The solid line represents the treated area's log abortion rate over time, while the dashed line represnts the synthetic control's log abortion rate over time. The synthetic control places weights on counties in the donor pool in order to best match the treated area's log abortion rate in the pre-period, then maintains these weights in the post-period to show how we would have expected log abortion rates to move in the absence of abortion clinic closures.

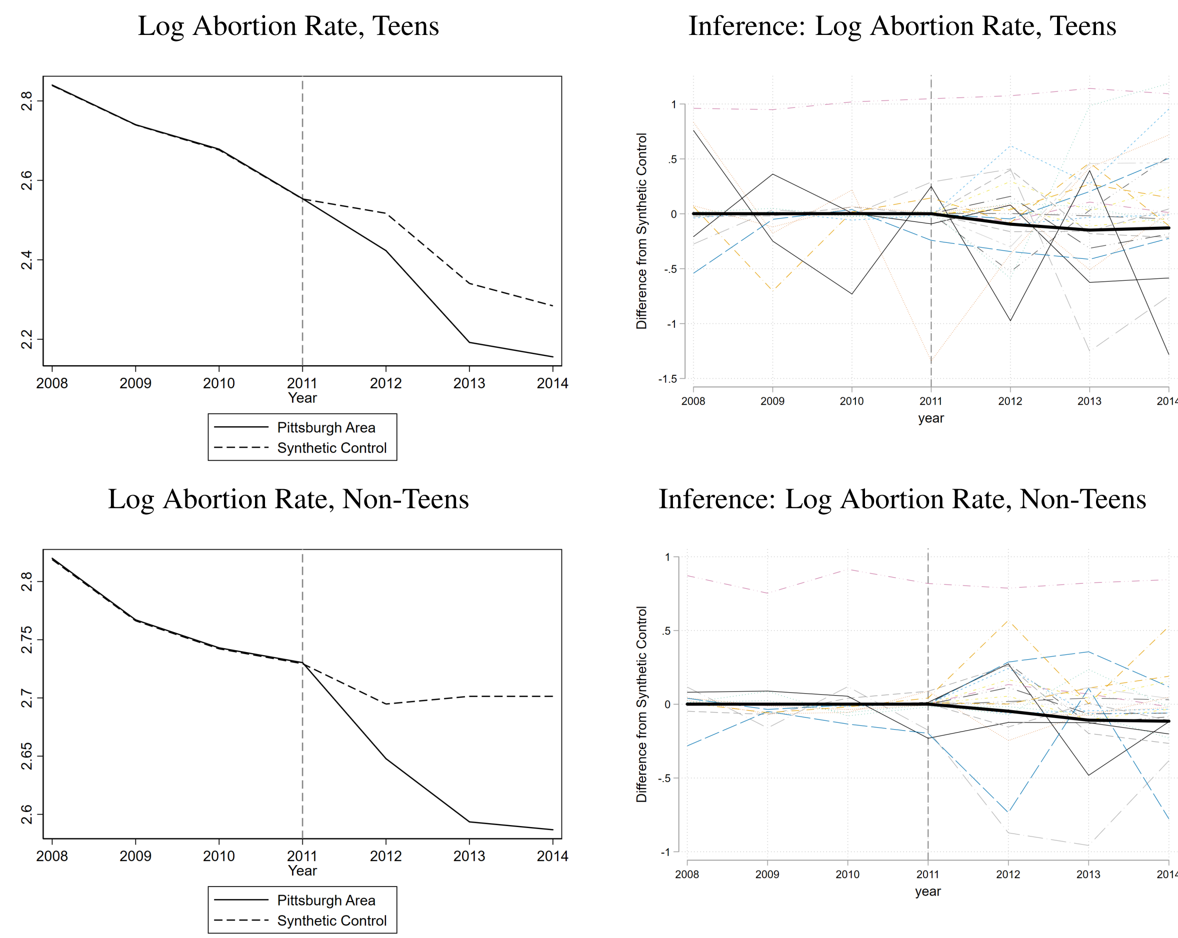
Figure A.23: Effects on Abortion Rate by Age Group

Teens - OLS | Non-Teens - OLS



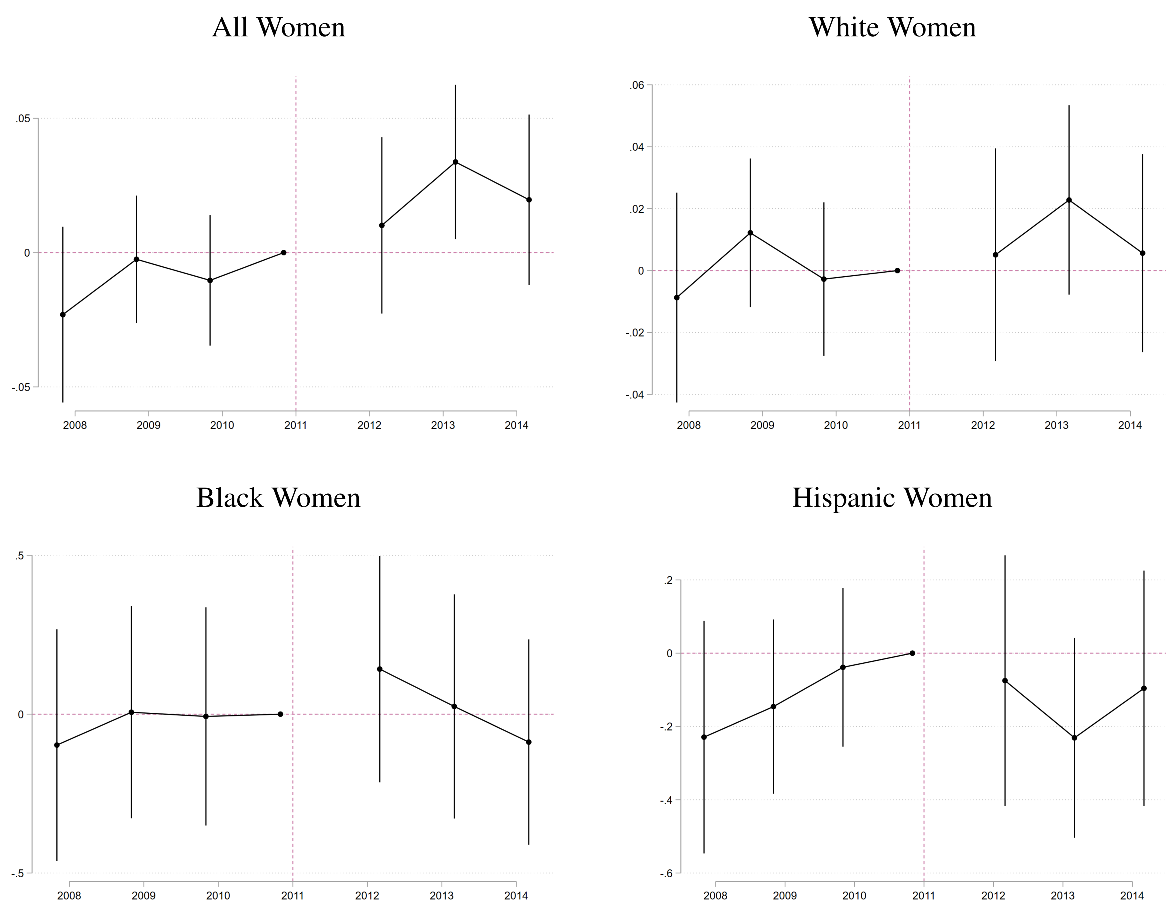
Notes: This figure plots the estimated effect of reduced local clinic capacity on abortion rates for teens and non-teens. Estimates come from a model which controls for county and year fixed effects. Treated counties are those for which Pittsburgh was the nearest abortion-providing city in the first year of data, comparison counties are those for which Allentown, Harrisburg, Philadelphia, Reading, Upland, Warminster, or West Chester was the nearest abortion-providing city in the first year of data.

Figure A.24: Synthetic Control - Abortion Rate by Age



Notes: These figures come from a synthetic control method approach using the log of the respective abortion rate as the outcome. The solid line represents the treated area's log abortion rate over time, while the dashed line represnts the synthetic control's log abortion rate over time. The synthetic control places weights on counties in the donor pool in order to best match the treated area's log abortion rate in the pre-period, then maintains these weights in the post-period to show how we would have expected log abortion rates to move in the absence of abortion clinic closures.

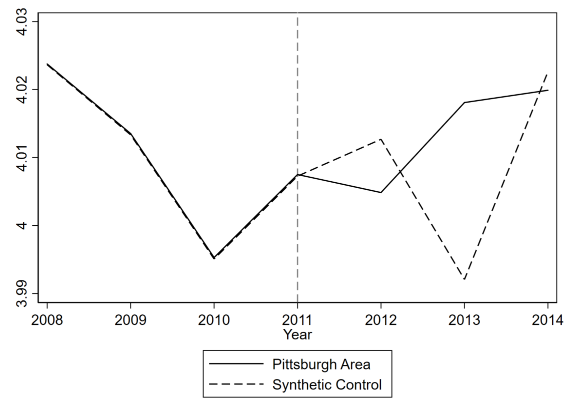
Figure A.25: Effects on Births by Race of Mother



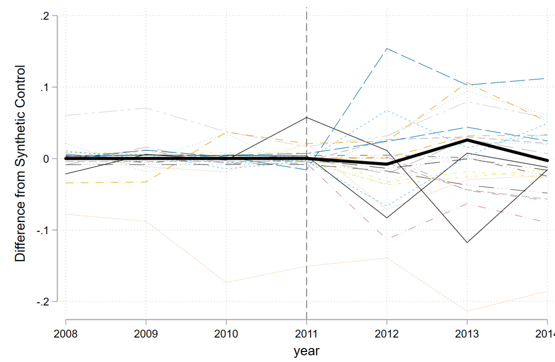
Notes: This figure plots the estimated effect of the passage of the law on births to mothers of various races. Estimates come from a model which controls for county and year fixed effects. Treated counties are those for which Pittsburgh was the nearest abortion-providing city in the first year of data, comparison counties are those for which Allentown, Harrisburg, Philadelphia, Reading, Upland, Warminster, or West Chester was the nearest abortion-providing city in the first year of data.

Figure A.26: Synthetic Control - Birth Rate by Race

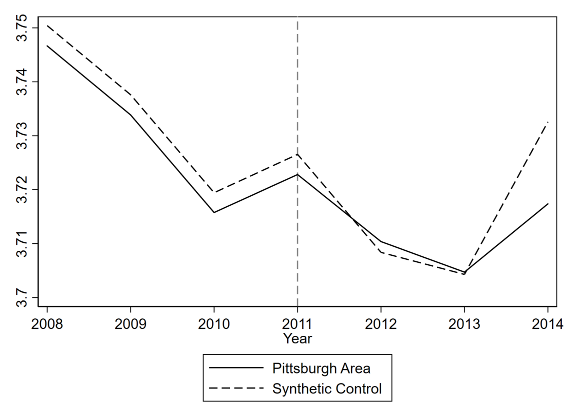
Log Birth Rate, Total Births



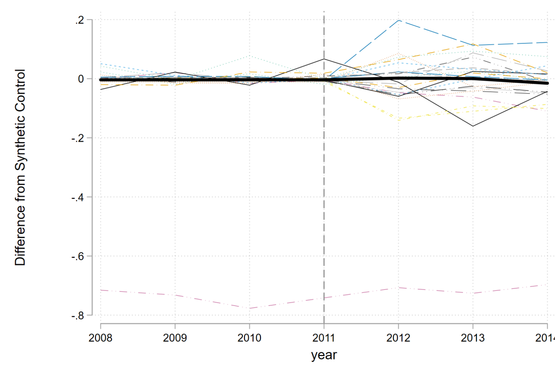
Inference: Log Birth Rate, Total Births



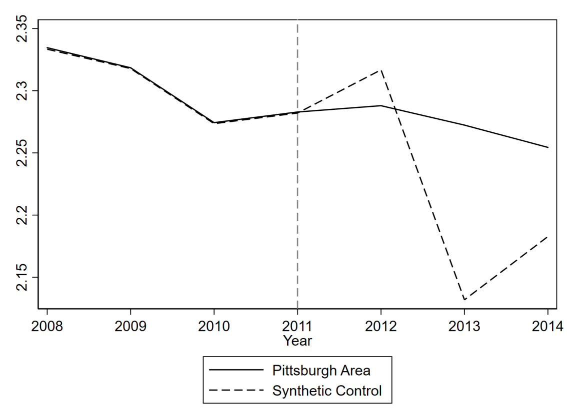
Log Birth Rate, White Births



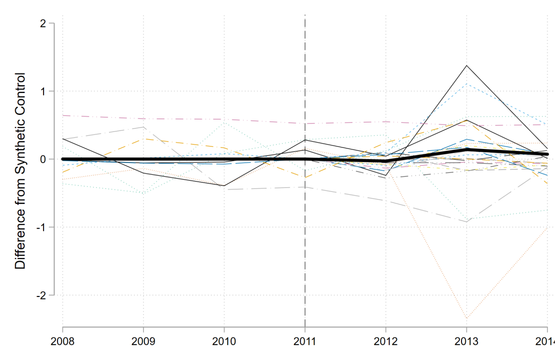
Inference: Log Birth Rate, White Births



Log Birth Rate, Black Births

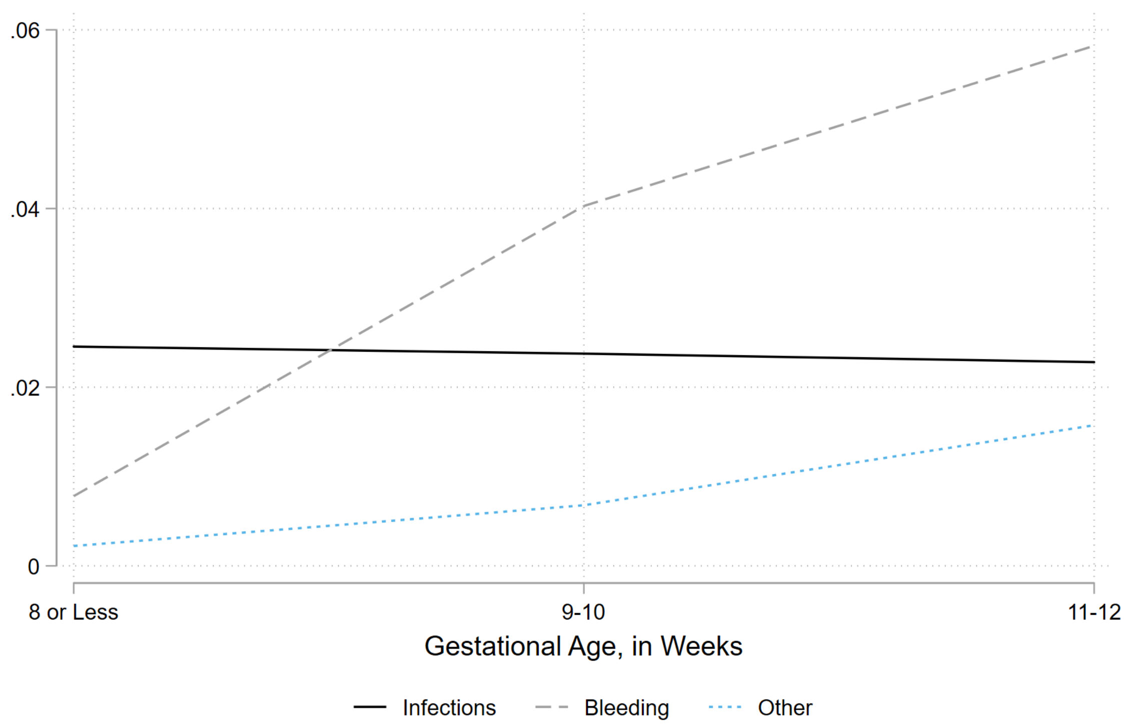


Inference: Log Birth Rate, Black Births



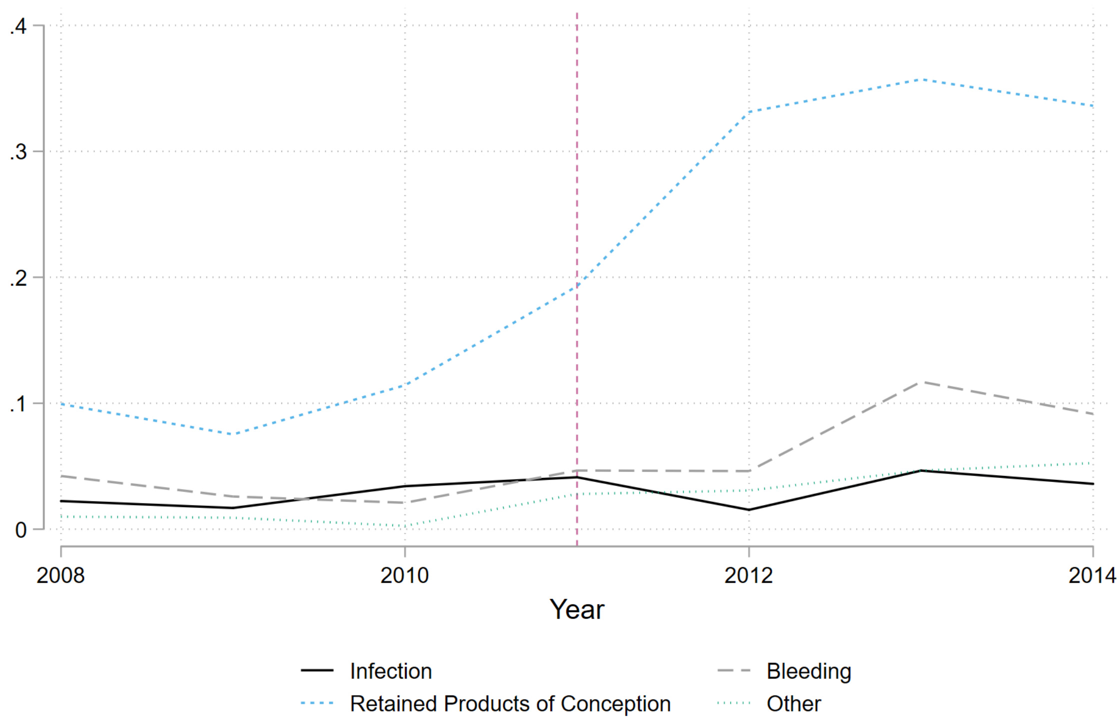
Notes: These figures come from a synthetic control method approach using the log of the respective abortion rate as the outcome. The solid line represents the treated area's log abortion rate over time, while the dashed line represnts the synthetic control's log abortion rate over time. The synthetic control places weights on counties in the donor pool in order to best match the treated area's log abortion rate in the pre-period, then maintains these weights in the post-period to show how we would have expected log abortion rates to move in the absence of abortion clinic closures.

Figure A.27: Complication Rates by Gestational Age at Abortion



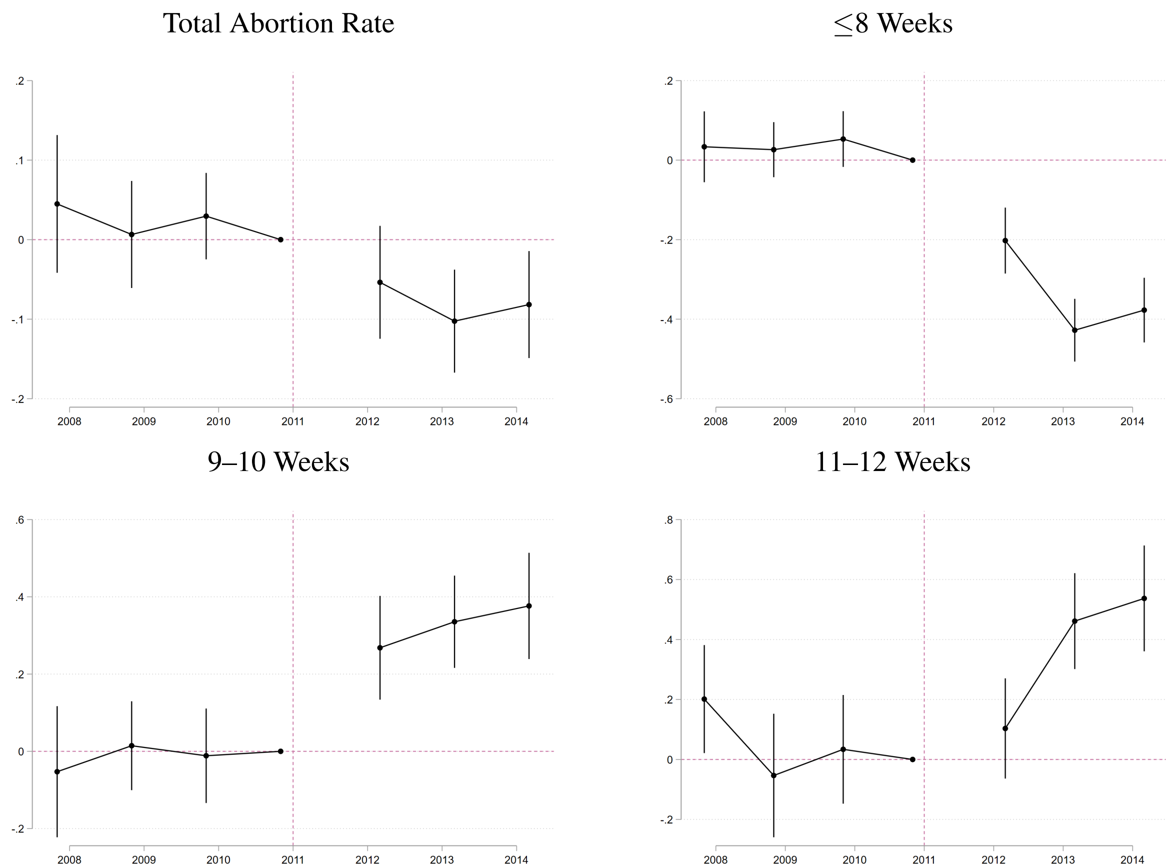
Notes: This figure plots the complication rate by gestational age and type of complication. Data come from the Pennsylvania Department of Health Annual Abortion files from 2008-2011.

Figure A.28: Complication Rates Over Time



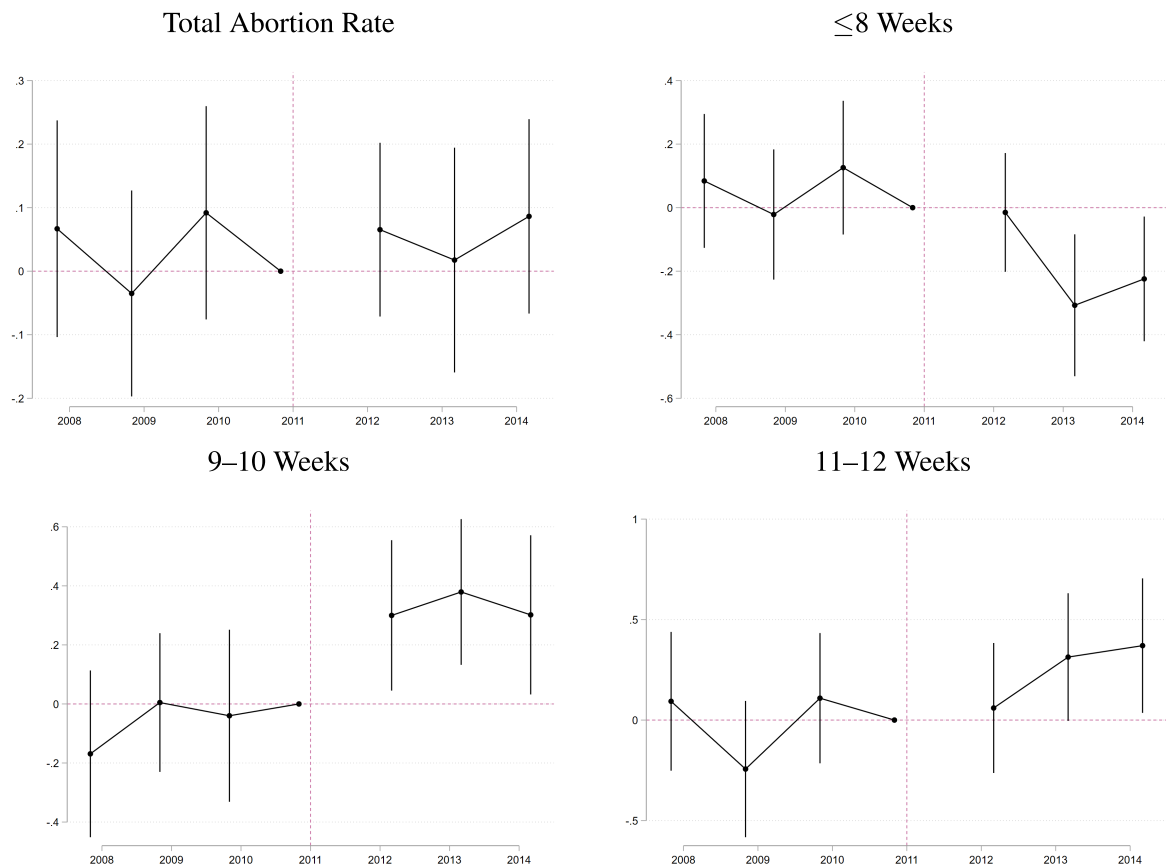
Notes: This figure plots the complication rate type of complication over time. Data come from the Pennsylvania Department of Health Annual Abortion files from 2008-2014.

Figure A.29: Effects on Abortion Rates Overall and by Gestational Age - WLS



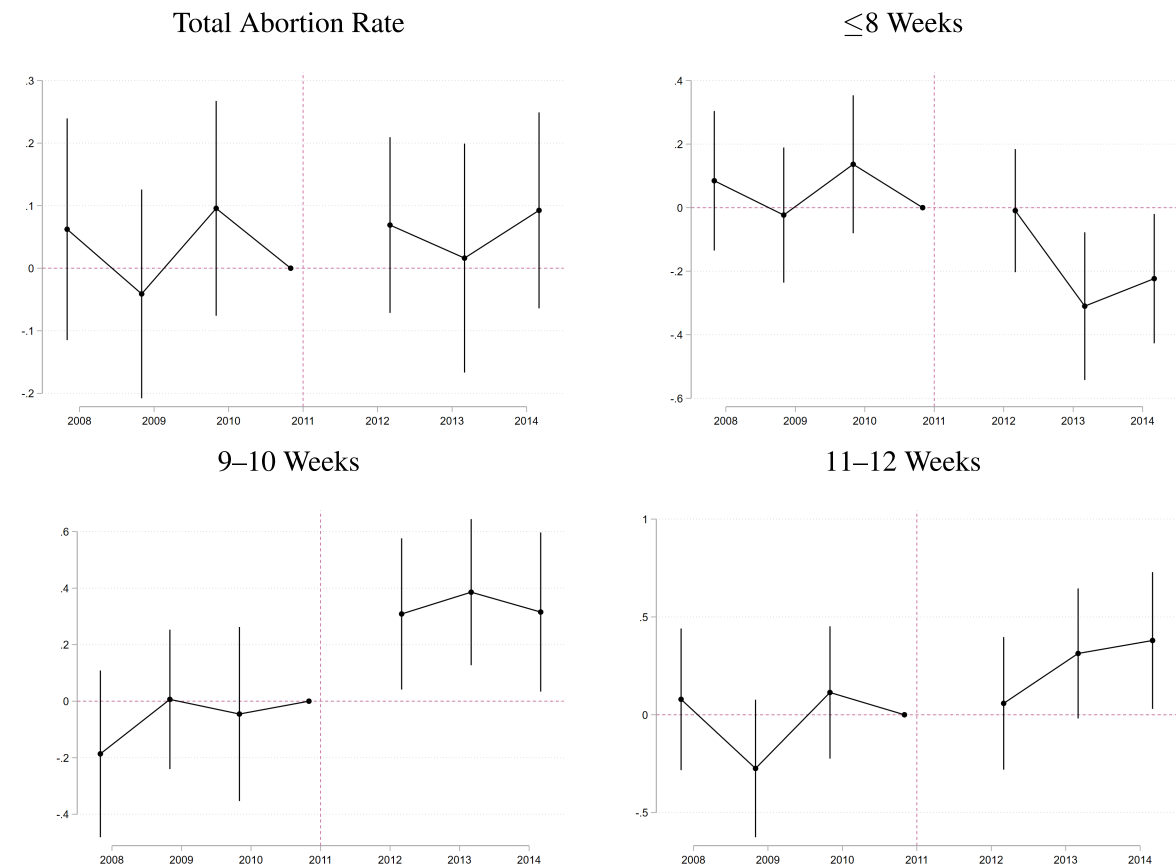
Notes: This figure plots the estimated effect of reduced local clinic capacity on abortion rates overall and by gestational age, using population-weighted least squares. Estimates come from model which controls for county and year fixed effects. Treated counties are those for which Pittsburgh was the nearest abortion-providing city in the first year of data, comparison counties are those for which Allentown, Harrisburg, Philadelphia, Reading, Upland, Warminster, or West Chester was the nearest abortion-providing city in the first year of data.

Figure A.30: Effects on Abortion Rates Overall and by Gestational Age - Including All Counties



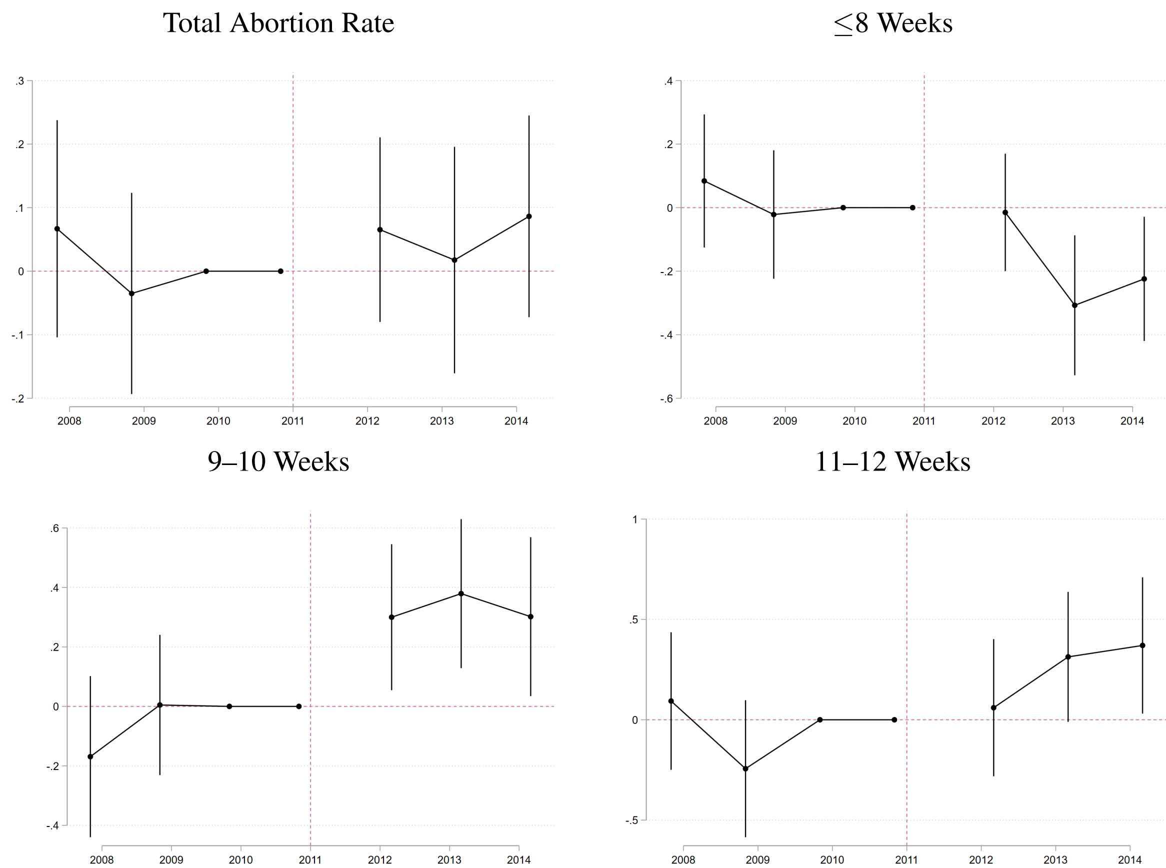
Notes: This figure plots the estimated effect of reduced local clinic capacity on abortion rates overall and by gestational age, including counties that were previously omitted into the comparison group. Estimates come from a model which controls for county and year fixed effects. Treated counties are those for which Pittsburgh was the nearest abortion-providing city in the first year of data, comparison counties are all other counties in Pennsylvania.

Figure A.31: Effects on Abortion Rates Overall and by Gestational Age - Excluding Pittsburgh



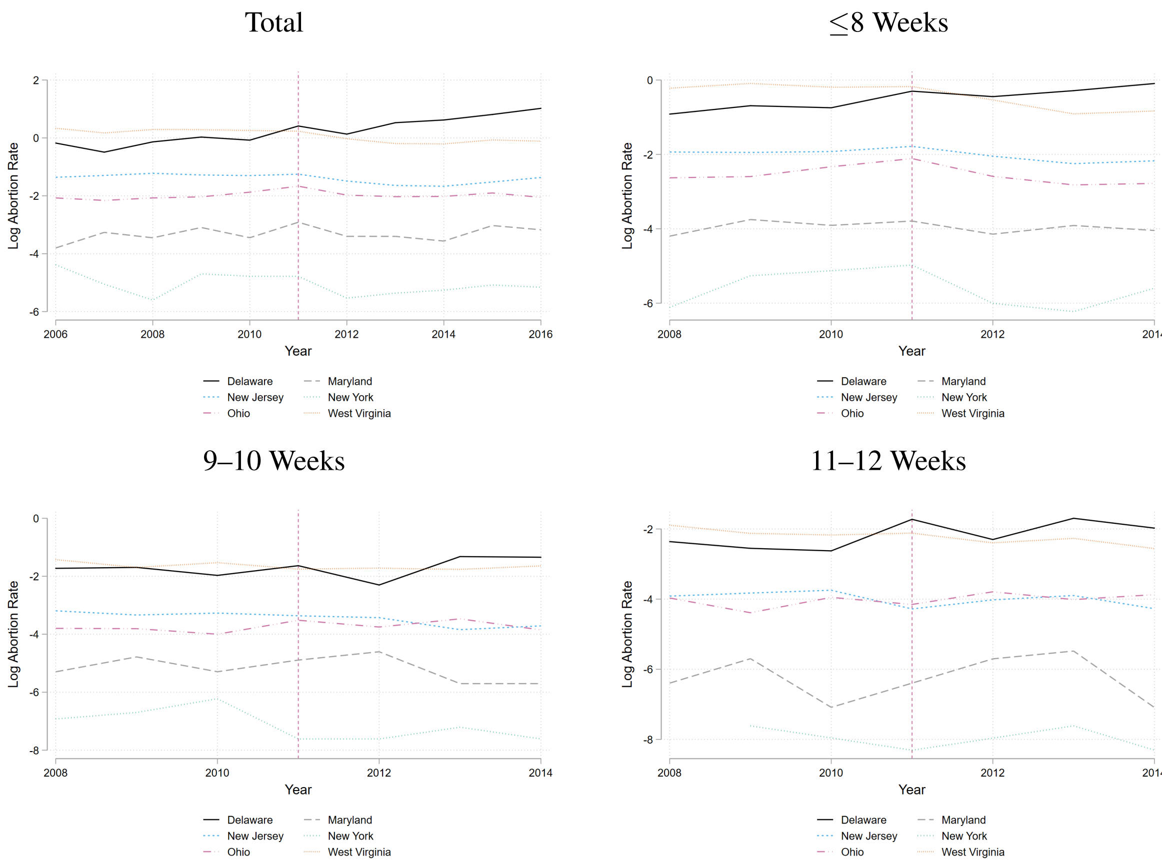
Notes: This figure plots the estimated effect of reduced local clinic capacity on abortion rates overall and by gestational age, excluding Allegheny County (Pittsburgh's county) to remove individuals living inside the city who may be more likely to be impacted by small changes in distance. Estimates come from a model which controls for county and year fixed effects. Treated counties are those for which Pittsburgh was the nearest abortion-providing city in the first year of data, comparison counties are those for which Allentown, Harrisburg, Philadelphia, Reading, Upland, Warminster, or West Chester was the nearest abortion-providing city in the first year of data.

Figure A.32: Effects on Abortion Rates Overall and by Gestational Age - Excluding 2010



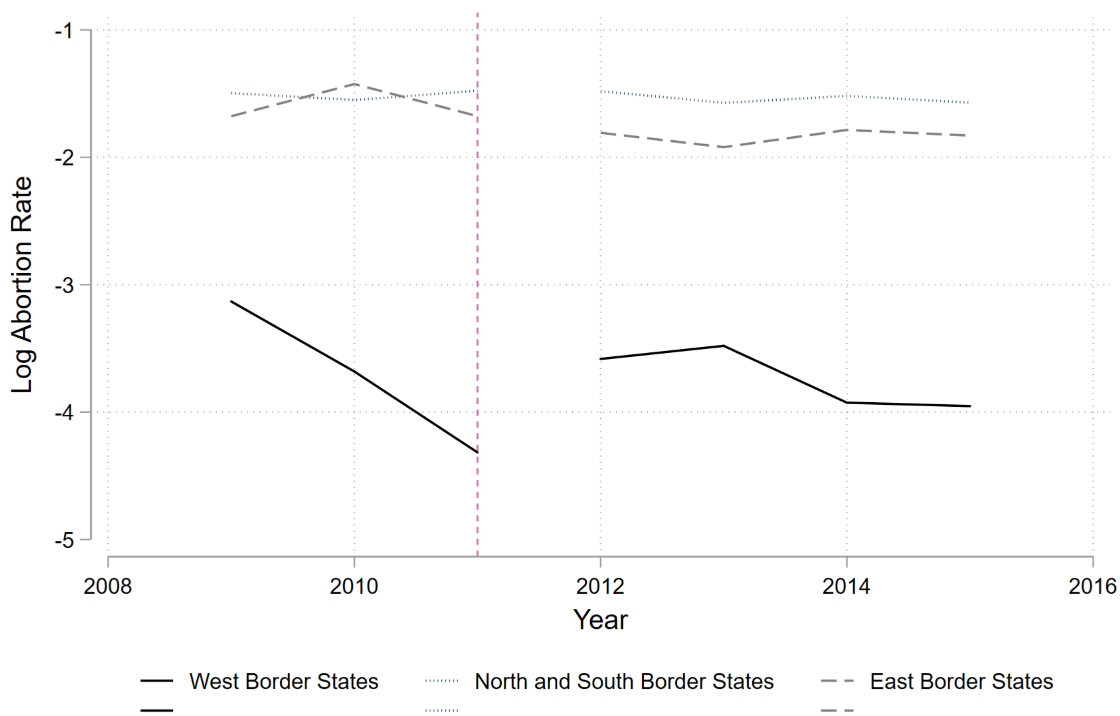
Notes: This figure plots the estimated effect of reduced local clinic capacity on abortion rates overall and by gestational age, excluding the year of 2010 (as this year had endogenous closures in both the treated and comparison areas). Estimates come from a model which controls for county and year fixed effects. Treated counties are those for which Pittsburgh was the nearest abortion-providing city in the first year of data, comparison counties are those for which Allentown, Harrisburg, Philadelphia, Reading, Upland, Warminster, or West Chester was the nearest abortion-providing city in the first year of data.

Figure A.33: Log Abortion Rate, Out-of-State Women



Notes: This figure plots the natural log of the abortion rate for women coming to Pennsylvania from other states to obtain an abortion. The states shown are the only states for which state-specific data is provided in the Pennsylvania Department of Health's Annual Abortion Report.

Figure A.34: Log Abortion Rate for PA Residents Traveling Out of State for Abortions



Notes: This figure plots the natural log of the abortion rate for Pennsylvania women obtaining abortions outside of Pennsylvania. Data are from the CDC Abortion Surveillance dataset.

Tables

Table A.15: Summary Statitstics, Treated vs. Control Counties

	Treated	Comparison
Pre Period (2008-2011)		
Abortion Rate per 1000 Women aged 15-44	12.3	18.3
Abortion Rate per 1000 Women aged 15-19	11.0	16.1
Pct of Population that is aged 15-44	37.1	39.7
Pct White	89.0	73.3
Pct Hispanic	1.2	8.0
Pct Black	8.0	14.6
Poverty Rate	12.3	12.1
Unemployment Rate	7.8	7.3
Post Period (2012-2014)		
Abortion Rate per 1000 Women aged 15-44	10.4	16.6
Abortion Rate per 1000 Women aged 15-19	7.2	11.2
Pct of Population that is aged 15-44	36.8	39.1
Pct White	88.3	71.7
Pct Hispanic	1.5	8.9
Pct Black	8.1	14.8
Poverty Rate	12.4	13.1
Unemployment Rate	7.2	6.7

Notes: This table shows summary statistics for treated and comparison counties, before and after the law's passage. Treated counties are those for which Pittsburgh was the nearest abortion-providing city in the first year of data, comparison counties are those for which Allentown, Harrisburg, Philadelphia, Reading, Upland, Warminster, or West Chester was the nearest abortion-providing city in the first year of data.

Table A.16: Estimated Effects of Reduced Clinic Capacity on Abortion Rates

	Total	≤8 Weeks	9-10 Weeks	11-12 Weeks
First Year of the Law	-0.014	-0.201***	0.429***	-0.047
	(0.055)	(0.068)	(0.123)	(0.150)
Second Year of the Law	-0.138*	-0.528***	0.394***	0.409***
	(0.075)	(0.089)	(0.113)	(0.154)
Third Year of the Law	-0.060	-0.445***	0.649***	0.567***
	(0.056)	(0.075)	(0.126)	(0.156)
1 Year Before Law	-0.014	-0.061	0.116	0.142
	(0.055)	(0.070)	(0.118)	(0.162)
2 Years Before Law	0.042	0.101	0.015	0.022
	(0.057)	(0.075)	(0.128)	(0.137)
Average effect	-0.071	-0.392	0.491	0.310
P-value (test average effect = 0)	0.143	0.000	0.000	0.008
Observations	252	252	252	252

Notes: Results come from a model with county and year fixed effects.
***, **, and * represent p-values less than 0.01, 0.05, and 0.10, respectively.

Table A.17: Estimated Effects of Reduced Clinic Capacity on Abortion Rates by Age Group

	Teen	Non-Teen
First Year of the Law	-0.153	0.008
	(0.152)	(0.060)
Second Year of the Law	0.023	-0.168**
	(0.138)	(0.070)
Third Year of the Law	-0.123	-0.048
	(0.135)	(0.059)
1 Year Before Law	0.065	-0.017
	(0.127)	(0.054)
2 Years Before Law	0.034	0.038
	(0.167)	(0.058)
Average effect	-0.084	-0.069
P-value (test average effect = 0)	0.395	0.152
Observations	252	252

Notes: Results come from a model with county and year fixed effects.
***, **, and * represent p-values less than 0.01, 0.05, and 0.10, respectively.

Table A.18: Estimated Effects of Reduced Clinic Capacity on Birth Rates by Race of Mother

	Total	White	Black	Hispanic
First Year of the Law	0.023	0.003	0.187	0.113
	(0.016)	(0.016)	(0.136)	(0.163)
Second Year of the Law	0.047***	0.021	0.069	-0.044
	(0.014)	(0.014)	(0.135)	(0.126)
Third Year of the Law	0.032**	0.004	-0.043	0.092
	(0.015)	(0.015)	(0.116)	(0.152)
1 Year Before Law	0.013	-0.002	0.045	0.187
	(0.012)	(0.013)	(0.160)	(0.121)
2 Years Before Law	0.002	-0.004	0.038	0.149
	(0.011)	(0.011)	(0.132)	(0.094)
Average effect	0.034	0.009	0.071	0.054
P-value (test average effect = 0)	0.002	0.393	0.465	0.611
Observations	252	252	245	252

Notes: Results come from a model with county and year fixed effects.
***, **, and * represent p-values less than 0.01, 0.05, and 0.10, respectively.

Table A.19: Synthetic Control: Estimated Effects of Reduced Clinic Capacity on Abortion Rates Overall and by Gestational Age

Total Abortion Rate

	2012	2013	2014	Average
Est. Effect	-0.055	-0.118	-0.109	-0.093
P-Value	0.25	0.125	0.083	0.167

≤8 Weeks

	2012	2013	2014	Average
Est. Effect	-0.225	-0.379	-0.333	-0.313
P-Value	0.042	0.042	0.042	0.042

9–10 Weeks

	2012	2013	2014	Average
Est. Effect	0.230	0.278	0.323	0.274
P-Value	0.083	0.083	0.042	0.083

11–12 Weeks

	2012	2013	2014	Average
Est. Effect	0.106	0.277	0.411	0.265
P-Value	0.292	0.292	0.167	0.292

Notes: Results come from the synthetic control method approach, using the ratio of RMSE in the pre-period to RMSE in the post-period to establish p-values.
***, **, and * represent p-values less than 0.01, 0.05, and 0.10, respectively.

Table A.20: Synthetic Control: Estimated Effects of Reduced Clinic Capacity on Abortion Rates for Teens and Non-Teens

Teen Abortion Rate

	2012	2013	2014	Average
Est. Effect	-0.094	-0.148	-0.128	-0.124
P-Value	0.292	0.250	0.208	0.333

Non-Teen Abortion Rate

	2012	2013	2014	Average
Est. Effect	-0.047	-0.108	-0.115	
P-Value	0.167	0.042	0.083	0.125

Notes: Results come from the synthetic control method approach, using the ratio of RMSE in the pre-period to RMSE in the post-period to establish p-values.
***, **, and * represent p-values less than 0.01, 0.05, and 0.10, respectively.

Table A.21: Synthetic Control: Estimated Effects of Reduced Clinic Capacity on Birth Rates by Race

Total Birth Rate

	2012	2013	2014	Average
Est. Effect	-0.008	0.026	-0.003	0.005
P-Value	0.042	0.125	0.208	0.125

White Birth Rate

	2012	2013	2014	Average
Est. Effect	0.002	0.000	-0.015	-0.004
P-Value	0.917	1	0.458	0.792

Black Birth Rate

	2012	2013	2014	Average
Est. Effect	-0.029	0.140	0.072	0.061
P-Value	0.412	0.125	0.208	0.167

Hispanic Birth Rate

	2012	2013	2014	Average
Est. Effect	-0.239	-0.386	-0.245	-0.290
P-Value	0.583	0.625	0.792	0.625

Notes: Results come from the synthetic control method approach, using the ratio of RMSE in the pre-period to RMSE in the post-period to establish p-values.
***, **, and * represent p-values less than 0.01, 0.05, and 0.10, respectively.

Table A.22: Estimated Effects of Reduced Clinic Capacity on Abortion Rates - WLS

	Total	$\leq$8 Weeks	9–10 Weeks	11–12 Weeks
First Year of the Law	-0.079**	-0.232***	0.287***	0.030
	(0.039)	(0.043)	(0.065)	(0.078)
Second Year of the Law	-0.128***	-0.458***	0.355***	0.388***
	(0.037)	(0.041)	(0.057)	(0.075)
Third Year of the Law	-0.107***	-0.407***	0.396***	0.463***
	(0.038)	(0.042)	(0.067)	(0.084)
1 Year Before Law	-0.026	-0.030	0.019	-0.074
	(0.031)	(0.032)	(0.058)	(0.083)
2 Years Before Law	0.004	0.023	0.008	-0.040
	(0.032)	(0.037)	(0.059)	(0.086)
Average effect	-0.105	-0.366	0.346	0.293
P-value (test average effect = 0)	0.001	0.000	0.000	0.000
Observations	252	252	252	252

Notes: Results come from a model with county and year fixed effects.
***, **, and * represent p-values less than 0.01, 0.05, and 0.10, respectively.

Table A.23: Estimated Effects of Reduced Clinic Capacity on Abortion Rates by Age - WLS

	Teen	Non-Teen
First Year of the Law	-0.106	-0.076*
	(0.070)	(0.041)
Second Year of the Law	-0.045	-0.139***
	(0.073)	(0.037)
Third Year of the Law	-0.111	-0.105***
	(0.071)	(0.040)
1 Year Before Law	0.034	-0.033
	(0.056)	(0.031)
2 Years Before Law	0.019	-0.000
	(0.071)	(0.033)
Average effect	-0.087	-0.107
P-value (test average effect = 0)	0.086	0.001
Observations	252	252

Notes: Results come from a model with county and year fixed effects.
***, **, and * represent p-values less than 0.01, 0.05, and 0.10, respectively.

Table A.24: Estimated Effects of Reduced Clinic Capacity on Abortion Rates by Age, Including All Counties in PA

	Total	≤8 Weeks	9–10 Weeks	11–12 Weeks
First Year of the Law	0.049	-0.046	0.382***	0.135
	(0.056)	(0.076)	(0.112)	(0.137)
Second Year of the Law	0.002	-0.339***	0.461***	0.389***
	(0.080)	(0.098)	(0.107)	(0.133)
Third Year of the Law	0.070	-0.256***	0.384***	0.446***
	(0.066)	(0.082)	(0.120)	(0.144)
1 Year Before Law	-0.016	-0.031	0.082	0.075
	(0.074)	(0.093)	(0.115)	(0.155)
2 Years Before Law	0.076	0.095	0.042	0.184
	(0.075)	(0.090)	(0.133)	(0.137)
Average effect	0.041	-0.213	0.409	0.323
P-value (test average effect = 0)	0.442	0.001	0.000	0.003
Observations	469	469	469	469

Notes: Results come from a model with county and year fixed effects.

***, **, and * represent p-values less than 0.01, 0.05, and 0.10, respectively.

Table A.25: Estimated Effects of Reduced Clinic Capacity on Abortion Rates by Age, Excluding Pittsburgh

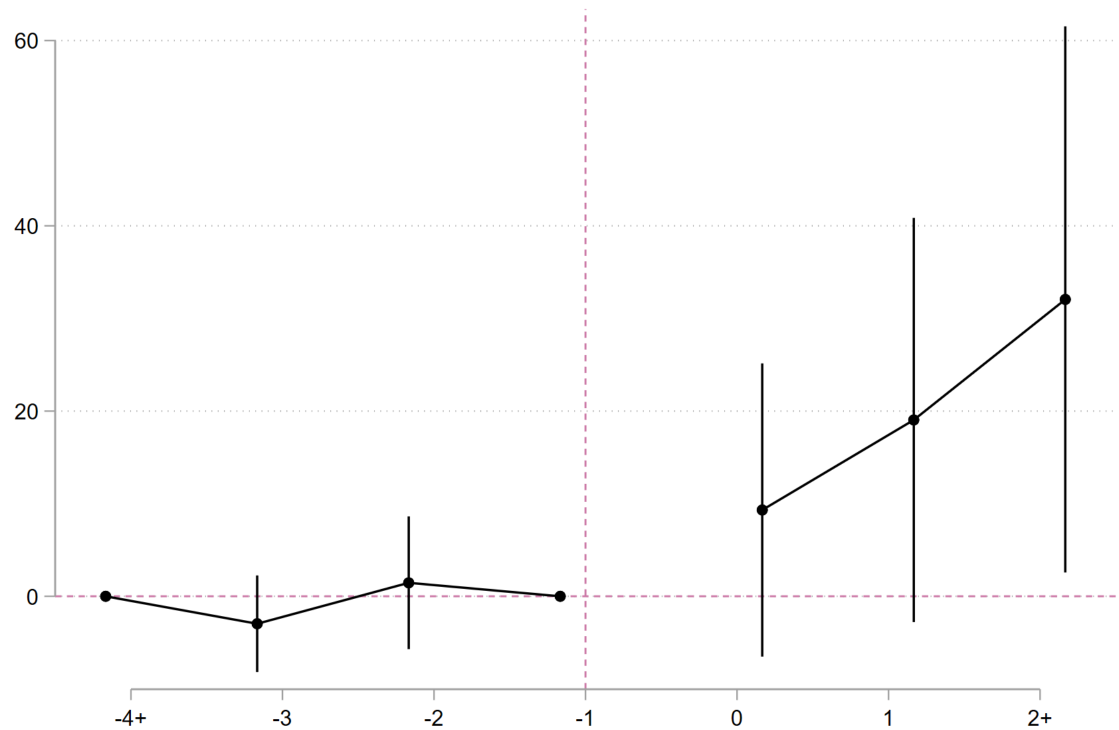
	Total	≤ 8 Weeks	9–10 Weeks	11–12 Weeks
First Year of the Law	-0.005	-0.195***	0.446***	-0.027
	(0.057)	(0.072)	(0.128)	(0.156)
Second Year of the Law	-0.135*	-0.531***	0.409***	0.432***
	(0.078)	(0.095)	(0.118)	(0.159)
Third Year of the Law	-0.048	-0.444***	0.670***	0.599***
	(0.058)	(0.079)	(0.130)	(0.161)
1 Year Before Law	-0.009	-0.060	0.123	0.165
	(0.058)	(0.075)	(0.124)	(0.169)
2 Years Before Law	0.051	0.113	0.017	0.050
	(0.059)	(0.079)	(0.136)	(0.143)
Average effect	-0.063	-0.390	0.508	0.335
P-value (test average effect = 0)	0.215	0.000	0.000	0.006
Observations	245	245	245	245

Notes: Results come from a model with county and year fixed effects.
***, **, and * represent p-values less than 0.01, 0.05, and 0.10, respectively.

Figures and Tables for Section 4

Figures

Figure A.35: Rate of Forensic Profiles



Notes: This figure plots the state-level rate of forensic profiles [(no. profiles/pop)*100000] using years since treatment as the treatment indicator. The model controls for state fixed effects, year fixed effects, new laws related to SAKI (but independent of funding), county-level unemployment rate, and county-level poverty rate, and the share of the male and female population age 16+ that are married as well as the share that are single. The model also includes state-specific linear time trends and standard errors are clustered at the county level. Vertical bars represent 95% confidence intervals.

Tables

Table A.26: Summary Statistics — Treated vs. Never-Treated States

	Treated States	Never-Treated States	All States
Population	7269824	3765071	6358588
Rate of Forensic Profiles	171.426	138.477	162.859
SAKI Funds per Capita	0.303	0.000	0.224
Share of Population that are Civilians	0.703	0.708	0.704
Share of Population that are Native-Born Citizens	0.904	0.925	0.910
Share of Women Over Age 15 who are Married	0.226	0.230	0.227
Share of Men Over Age 15 who are Married	0.232	0.235	0.233
Share of Women Over Age 15 who are Single (Never Married)	0.225	0.224	0.225
Share of Men Over Age 15 who are Single (Never Married)	0.215	0.213	0.214
Year	2014	2014	2014
Observations	324	126	450

Notes: These summary statistics are for all states in the sample over the entire sample period. States that ever receive funds from DANY or BJA are considered treated, and states that never receive these funds are considered never-treated.

Table A.27: Main Results: Effects on Forensic Profile Rates

	Event Study Estimates			
	1	2	3	4
4+ years before receiving funds	-6.409	0.000	0.000	0.000
	(8.859)	(.)	(.)	(.)
3 years before receiving funds	-7.360	-3.004	-3.506	-2.964
	(5.206)	(2.063)	(2.097)	(2.597)
2 years before receiving funds	-4.343	-0.652	0.257	1.459
	(2.873)	(3.159)	(3.232)	(3.565)
1 year before receiving funds	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
1st year on funds	4.735	7.337	6.789	9.318
	(3.306)	(7.522)	(7.688)	(7.877)
2nd year on funds	13.235*	15.224	18.038	19.038*
	(6.919)	(10.768)	(10.937)	(10.858)
3+ years on funds	24.960**	25.918*	30.599**	32.054**
	(11.303)	(14.250)	(14.048)	(14.672)
Observations	450	450	450	450
	Difference-in-Differences Estimates			
Difference-in-Differences Estimate	16.977*	15.806	16.877*	17.980*
	(9.437)	(9.574)	(9.496)	(9.657)
Observations	450	450	450	450
State & Year Fixed Effects	Y	Y	Y	Y
State-Specific Linear Time Trends	N	Y	Y	Y
SAKI-Related Law Change Controls	N	N	Y	Y
Demographic Controls	N	N	N	Y

Notes: These summary statistics are for all states in the sample over the entire sample period. States that ever receive funds from DANY or BJA are considered treated, and states that never receive these funds are considered never-treated. All models control for state and year fixed effects and cluster on the state level to account for serial correlation over time. SAKI-related law change controls include controls for any law changes or funding changes that were not connected to DANY or BJA funding. Demographic controls include the unemployment rate, the poverty rate, and the shares of the population that are civilian, U.S. citizen, male and married, female and married, male and never married, and female and never married.
***, **, and * represent p-values less than 0.01, 0.05, and 0.10, respectively.