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ABSTRACT

Can Expanding Contraceptive Access Reduce Adverse Infant Health Outcomes?*

This paper uses the implementation of a privately funded family planning program in Colorado to demonstrate that expanding access to long-acting reversible contraceptives to lower income women creates positive selection in the health of the children being born, reducing the rates of extremely preterm births and infant mortality. My most conservative estimates suggest reductions of 1.1 extremely preterm births and 0.9 infant deaths per 1,000 live births, with the largest reduction in deaths due to Sudden Infant Death Syndrome. This suggests that expanding contraceptive access could help close the infant mortality gap between the U.S. and other leading economies.

JEL Classification: J13, I18, I12

Keywords: contraceptive access, infant mortality, preterm birth, family planning

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1. Introduction

In 2019, 5.8 out of every 1,000 infants born in the United States died before their first birthday, a rate that is over three times higher than Japan, and worse than 32 of the other 37 OECD countries (OECD (2019)). Although this disparity is widely known, there is a relative dearth of evidence as to exactly why the US lags so far behind other leading economies. One plausible explanation is the high rate of unintended pregnancies in the US, which are associated with delayed initiation of prenatal care, (Kost and Lindberg (2015)) low birthweight (Slemming et al. (2016), Sable et al. (1997), Flower et al. (2013)), and neonatal mortality (Bustan and Coker (1994)). Unintended pregnancies alone cannot explain this gap, however, as the US has a similar rate of unintended pregnancies as many countries with much lower rates of infant mortality.[1] The US is somewhat unique, however, in which groups are experiencing unintended pregnancies. Lower-income women in the United States are more than five times as likely to have an unintended pregnancy as higher income women, with barriers to contraceptive access playing an important role (Sonfield et al. (2014)).

Long-acting reversible contraceptives (LARCs), such as intrauterine devices (IUDs) and subdermal hormonal implants, are the most effective form of reversible contraception available today, and are virtually immune to user error (Curtis and Peipert (2017)). Although they are cost effective in the long run, they come with high upfront cost of around $800, which low-income women are typically unable to afford (CDPHE (2017)). They are also several times more expensive in the US than in much of Europe (Buhling et al. (2014)), and multiple studies cite cost as a major barrier to LARC access among low-income American women (Henke et al. (2020), Burke et al. (2020), Bailey et al. (2023)). This leads many low-income women in the US to opt for more expensive, less effective means of contraception, even when they would prefer a LARC. Tragically, low-income women are also disproportionately likely to experience an infant death (Larson (2007)). This raises the question, then, of whether expanding LARC access to lower-income women has the potential to lower the infant mortality rate and reduce other adverse infant health outcomes.

[1] According to Bearak et al. (2022), 46% of all pregnancies from 2015-2019 in the US were unintended, while the three countries with the lowest IMRs in the OECD, Japan, Finland, and Slovenia, had rates of unintended pregnancy of 41%, 51%, and 51%, respectively.

In this paper, I address this question by using the implementation of the Colorado Family Planning Initiative (CFPI) as a source of plausibly exogenous variation in LARC access. This privately funded family planning program gave thousands of LARCs for free to mostly low-income women in Colorado from 2009 to 2015. Using restricted-access natality data from the National Vital Statistics System (NVSS), which links birth certificates to infant death records, I implement an event-study design which compares trends in counties where private funding was received to expand LARC access with trends in other US counties which have similar family planning clinics, but which did not receive additional funding specifically for LARC access.

My most conservative estimates find that expanded LARC access led to a reduction of just over 1.1 extremely preterm births (EPBs), which are births before 28 weeks of gestation, and 0.9 infant deaths per 1,000 live births in Colorado. These represent reductions of between 14-18% off of the base rates of these two outcomes.[2] I supplement the main analysis by estimating a randomization inference at both the county and state level, and I demonstrate that my results are robust to a number of different model specifications, including synthetic control and synthetic difference-in-differences. I rule out the possibility that statewide policy changes within Colorado could be behind the reductions I find by demonstrating that they only appear in counties which have Title X family planning clinics, which are where the program was implemented. Looking into specific causes of death, I find the largest reduction in infant mortalities due to Sudden Infant Death Syndrome, or SIDS. Prior to the this program, births in Colorado to mothers without a college degree were over four times as likely to result in a SIDS death as births to a mother with a college degree,[3] so it makes sense that an intervention designed to enable lower income women to avoid unwanted pregnancies could reduce SIDS.

I also perform three back-of-the-envelope calculations. First, I use estimates of the costs of ventilation care from Hayman et al. (2015) to explore how much ventilation care was avoided because of the reduction in extremely preterm births. I find that the CFPI, which was funded by a $23 million donation from an anonymous donor, led to reductions in medical ventilation costs for extremely preterm

[2] A nationwide reduction of 0.9 infant deaths per 1,000 live births would be enough to close the gap between the US and Japan by 23%.

[3] There were approximately 0.7 SIDS deaths per 1,000 births to mothers without a college degree in Colorado prior to the CFPI, but only 0.17 SIDS deaths to Colorado mothers with a college degree.

births of approximately \$15.6 million over the course of the program. Second, I calculate that if the entire goal of the CFPI was to prevent infant mortalities, it would have cost approximately \$102,700 per infant death avoided. Finally, in order to understand the degree of selection which would be required in order for my estimates to be causal, I calculate the number of LARCs which were needed to avoid each infant death in each year. The year with the highest implied degree of selection was 2012, where there were 286 LARCs distributed per infant death avoided. While this suggests a high degree of selection, I demonstrate that this is plausible given the great deal of variation which exists in infant mortality risk across socioeconomic and demographic subgroups.

This paper builds on a large literature which demonstrates how family planning access can lead to selection which impacts the health outcomes of the cohorts of children being born. A number of studies[4] find evidence that access to abortion reduces infant mortality, with Grossman and Jacobowitz (1981) going so far as to say 'the increase in the legal abortion rate is the single most important factor in reductions in both white and nonwhite neonatal mortality rates' (pg. 695). Grossman and Jacobowitz (1981) estimates a cross-sectional regression comparing social programs and infant mortality rates across U.S. counties in 1971 and extrapolates their coefficient estimates to explain national trends, while the more recent papers exploring this question have come to similar conclusions using more modern empirical methods.

This paper builds on these findings by demonstrating that LARC access also has the ability to reduce infant mortality, even in a setting where abortion is already legal. This is particularly important in light of the recent Supreme Court decision in Dobbs vs. Jackson, which overturned Roe vs. Wade and opened the door for restrictions in abortion access across the country. Since abortion and LARCs have complementary impacts on infant mortality, LARC access will become even more important as abortion access is restricted.

This paper also builds on a growing literature which documents the effects of expanding access to LARCs specifically. Programs that expand LARC access to low-income women have been shown to reduce unintended pregnancies (McNicholas et al. (2014)), abortion (Biggs et al. (2015), Ricketts et al. (2014)) and the teen birth rate (Lindo and Packham (2017), Kelly et al. (2020)), while increasing female

[4] Pabayo et al. (2020), Gruber et al. (1999), Joyce (1987)

educational attainment (Stevenson et al. (2021), Yeatman et al. (2022)). Gallen et al. (2023) use LARC failures as a source of exogenous variation in childbearing and find that an unplanned pregnancy leads to income losses of 20% in the five years after the contraceptive failure. In contrast, successful fertilization procedures do not cause disruptions in women's careers, suggesting that the ability to choose when to have a child is an important determinant of women's economic outcomes. I build on these findings by demonstrating that increased LARC access can also affect the next generation of children being born by leading to a reduction in adverse infant health outcomes.[5]

The rest of the paper is organized as follows. Section two provides background on how family planning programs have impacted maternal and infant outcomes, as well as on LARCs and the three programs I study. Section three describes my data and empirical strategy, while section four presents my results on the effect of LARC access on the rates of extremely preterm births and infant mortality. Section five performs some back-of-the-envelope calculations and concludes.

2. Background

2.1. *Effects of Family Planning on Maternal and Infant Outcomes*

Access to family planning services has been shown to have far-reaching impacts on health and economic outcomes for both mothers and their children, with these effects varying considerably across different forms of contraception. In addition to the many studies which find evidence that access to abortion reduces infant mortality, Clarke and Mühlrad (2021) find that abortion legalization in Mexico caused a sharp decline in maternal morbidity, while Myers (2017) finds that the liberalized access to abortion in the US in the 1970s gave agency to many young women in deciding if and when to get married and have children.[6] A substantial litera-

[5] Goldthwaite et al. (2015) also do this by comparing treated and untreated counties in Colorado and find that births earlier than 37 weeks went down in treated counties following the Colorado Family Planning Initiative. I build on their findings by comparing treated Colorado counties to more similar counties in other states and by analyzing the more dangerous outcomes of extremely preterm births and infant mortality.

[6] Family planning access has also been found to have important effects in developing countries. For example, Ashraf et al. (2014) find that receiving access to concealable long-acting contraceptive methods increased women in Zambia's intra-household bargaining power and reduced unwanted pregnancies, and Miller (2010) find that family planning access in Colombia enabled women to acquire more education and live more independently.

ture, starting with Donohue and Levitt (2001),[7] tracks births which were subject to legal abortion in utero into adulthood, finding that criminality was substantially less than that of cohorts not exposed to legal abortion, though critics have disputed these findings.[8] Other research has found that cohorts exposed to abortion in utero were less likely to get pregnant as a teenager (Donohue, Grogger, et al. (2009)), while also being less likely to become a single parent and more likely to graduate college (Ananat, Gruber, et al. (2009)). In this paper, I document a similar selection effect, whereby allowing lower-income women to opt out of unwanted or unplanned pregnancies reduces the likelihood of adverse infant health outcomes.

Another literature investigates the consequences of the emergence of the birth control pill, finding some similarities and many differences in the effects of access to the pill versus abortion. In their seminal "Power of the Pill" paper, Goldin and Katz (2002) exploit timing variation in state laws granting access to the pill to young women to show that it empowered women to delay the age of first marriage and lowered the cost of human capital acccumulation, though Myers (2017) provided evidence that it was actually liberalized access to abortion which empowered women to delay marriage and motherhood. Bailey (2006) finds that legal access to the pill before age 21 increased the number of women in the labor force, increased their total number of hours worked and decreased the likelihood of a birth before age 22, while Bailey (2012) demonstrates that access to the pill at a younger age conferred an eight percent wage premium to young women and substantially reduced the gender wage gap in both the 1980s and 1990s.

Focusing instead on the children born to women exposed to pill access, Ananat and Hungerman (2012) find that, in contrast to the effect of abortion access, the pill actually increased the share of children born with low birthweight and the share born to poor households in the short run. This effect appears to be driven by upwardly-mobile women delaying child-bearing while poorer women were not able to do so. These effects balanced out in the long run as these women be-

[7] Donohue and Levitt (2008), Donohue, Grogger, et al. (2009), Donohue and Levitt (2020), and François et al. (2014)

[8] Foote and Goetz (2008) point to a coding error in the original paper which weakens the results, though Donohue and Levitt (2020) demonstrate that the preferred specification from Foote and Goetz (2008) returns highly significant results when more years of data are added. Kahane et al. (2008) failed to find evidence of an abortion-crime link in England and Wales through 2001, as did Buonanno et al. (2014) looking at seven European countries, but François et al. (2014) found compelling evidence of an abortion-crime link by exploiting the staggered legalization of abortion across sixteen European countries.

gan having children later. This paper highlights the importance of understanding which groups of women receive access to a specific contraceptive technology. While the birth control pill was a revolutionary breakthrough, it was not cheap and was rarely covered by insurance, meaning it was not available to all women who wished to use it.

This raises the question, then, of how expanding access to LARCs will shift the composition of births and whether the children born to women with this access will be healthier than their counterparts. Because of the high upfront costs, LARCs are difficult for low-income women to afford, even though they are cost effective in the long run. This suggests that, absent some type of intervention, the effects of LARC access are likely to be similar to the effects of access to the birth control pill. The program I study in this paper, however, focused on expanding LARC access to low-income women specifically, so there is potential for it to give these women the same economic freedom that more upwardly mobile women attained with the emergence of the pill. This means programs which expand LARC access to low income women have the potential to improve infant health both in the short run, by shifting the composition of pregnancies towards potentially healthier ones, and in the long run by giving young women the power to delay pregnancies until they are economically more capable of investing resources in their children.

2.2. Long-Acting Reversible Contraceptives

Long-Acting Reversible Contraceptives (LARCs), namely intrauterine devices and subdermal hormonal implants, are the most effective reversible contraceptive methods available, approximately 20 times more effective than pills, patches, and rings (Curtis and Peipert (2017)). LARCs are greater than 99% effective and can prevent pregnancy for anywhere from three to 10 years (CDPHE (2017)). As Stoddard et al. (2011) point out, "they are not dependent on compliance with a pill-taking regimen, remembering to change a patch or ring, or coming back to the clinician for an injection."

LARCs are just as effective as sterilization (Kumari (2016)), with the added benefit of not being permanent, and because they require no further action from the user after insertion, they are almost immune to user error. Oral contraceptives, the patch, and condoms are less effective than LARCs even when used perfectly, and they have much higher rates of user error (Trussell (2004)). According to the Centers for Disease Control and Prevention (CDC), typical use failure rates for LARCs are as low as 0.1% for hormonal IUDs and implants, compared with 7% for oral contraceptives and patches, 13% for male condoms, and 21% for female condoms and spermicides. Even the least effective LARC, the copper IUD, has a typical use failure rate of only 0.8%, almost 90% lower than the rate for oral contraceptives (CDC (2023)). The risk of contraceptive failure is particularly high among low-income women (Sundaram et al. (2017)), suggesting that making LARCs more available to low-income women has the potential to substantially improve the effectiveness of the contraception these women are using.

Despite the many benefits to LARC usage, only 8.5% of women who were using a contraceptive in 2009 were using a LARC (Kavanaugh et al. (2015)). Multiple explanations account for this disconnect, including misinformation (Russo et al. (2013)), supply side issues (Bornstein et al. (2018)), and the high upfront cost of LARCs (CDPHE (2017)). The insertion procedure can also be painful (Callahan et al. (2019)). According to Narayan et al. (2018), adolescents who received IUDs reported higher pain than they anticipated during the insertion, though 78% of users still recommended the IUD after the procedure. Cost is another important barrier to LARC use. Even though LARCs are cost effective for most users in the long-run,[9] many women cannot afford to pay the upfront costs and end up using more expensive, less effective methods.

Multiple studies document an unmet demand for LARCs, with cost being the most frequently cited barrier to adoption and use (Henke et al. (2020), Burke et al. (2020), Potter et al. (2014), Bailey et al. (2023)). Because of this unsatisfied demand, there is potential for programs which improve LARC access to generate substantial improvements in public health, both for the women using LARCs and the children ultimately born to them. I now briefly describe the large-scale program in Colorado which was implemented with the express intention of addressing this unmet demand for LARCs.

2.3. The Colorado Family Planning Initiative

In 2009, the Colorado Department of Public Health and Environment (CDPHE) implemented the Colorado Family Planning Initiative with the help of an anony-

[9] Oral contraceptives can cost up to $50 a month, which means that LARC methods can be cheaper as long as they are used for more than 16 months.

mous donor, who funded the program with a $23 million dollar donation. The goal of the program was to reduce unintended pregnancies in Colorado by increasing the number of family planning clients served and by increasing access to LARC methods.[10] The CFPI was implemented through Title X family planning clinics, which were operated in 38 of Colorado's 64 counties.[11] Title X clinics are federally funded family planning clinics which provide comprehensive family planning and preventative health services. These clinics charge clients on a sliding scale based on their income levels. All contraceptive services for clients at or below 100 percent of the federal poverty line are provided at no cost at Title X clinics in Colorado. Individuals between 100-250% of the federal poverty level pay a discount, while those above 250% pay full price. Prior to the CFPI, the high upfront costs of LARC methods combined with a shortage of doctors trained in inserting LARCs and limited clinic capacity made it difficult for Title X clinics to keep up with demand, and it was common for waitlists to form for these methods. The money from the CFPI went directly to fixing these bottlenecks, and made it possible for clinics to dramatically increase LARC insertions in Colorado.

While very few LARC's had been inserted in Colorado Title X clinics prior to the CFPI,[12] by the end of 2009 there had been almost 2,000 new insertions, and this number grew in each subsequent year. Receiving a LARC typically involves an initial visit followed by a relatively simple outpatient procedure. While one of the missions of the CFPI was to educate patients about LARCs and combat prevailing misinformation about them, the CFPI required patient-centered care and the choice of what contraceptive to use ultimately rested with the patient (CDPHE (2017)). In each year from 2010-2015 between 4,000-7,000 LARCs were inserted at Title X clinics in Colorado, so that over 36,700 had been given out by 2015, which translates to approximately one LARC per 20 women aged 15-35 in Colorado. By 2015, 31% of Colorado Title X clients were LARC users, by far the highest rate of any state in the US. At the time, no other state had a rate over 25%, and only four states had rates over 20%, according the the Title X Family Planning Annual

[10] My discussion of the implementation of the CFPI draws on the detailed descriptions provided by CDPHE (2017), Lindo and Packham (2017), and Ricketts et al. (2014)

[11] Althought there is no requirement that a patient live in the county where the clinic is operated, research has shown that distance is an important impediment to receiving family planning services (Lindo, Myers, et al. (2019))

[12] The CDPHE reports that approximately 2,000 Title X clients in Colorado were LARC users in 2008

Report of 2015 (NFPRHA (2015)).[13] In response to the CFPI, teen pregnancy rates declined in Colorado counties with Title X clinics, with the largest impacts occurring in counties with high poverty rates (Lindo and Packham (2017)), indicating that the CFPI made a significant difference for young, low-income women in Colorado.

Prior to the CFPI, women using family planning in Colorado were most commonly using oral contraceptives. Appendix Figure 1 displays the percent of Title X clients using LARCs compared with oral contraceptives in Colorado and the rest of the United States from 2008-2015. In 2008, about 47% of women using family planning in Colorado were using birth control pills, compared with only 37% nationwide (Humphreys et al. (Forthcoming), NFPRHA (2008)). As the CFPI went into effect, LARC use increased in Colorado from roughly 8% to about 37%, while the use of birth control pills steadily declined to from 47% to 26%. Humphreys et al. (Forthcoming) document changes in method mix for Title X clients in Colorado in response to the CFPI using patient records from 2007-2016. While they find some degree of substitution from oral contraceptives to LARCs, roughly 80% of new LARC users had not previously visited a Title X clinic at all. In response to the CFPI, there was an overall increase in both the total number of Title X patients and in the proportion of Title X clients using LARCs. As the CFPI led to a greater relative cost reduction for LARCs than for oral contraceptives, this suggests that the program both enticed new clients to opt for a LARC when they would otherwise have chosen an oral contraceptive and may also have brought in additional clients who would never have visited a Title X clinic absent the intervention. As Colorado teens were using more effective contraception on average than teens in the rest of the US prior to the CFPI, this suggests that the effects of a similar program could be even larger outside of Colorado.

3. Empirical Approach

This section details the data used in my analysis as well as my strategy for estimating the causal effects of expanded LARC access on infant health outcomes.

[13] These states include Alaska at 25%, New Hampshire at 21%, Oregon at 22%, and Vermont at 22%

3.1. Data

This paper uses data from several sources. Data on both extremely preterm births (EPBs) and infant mortality come from restricted-access linked birth and infant death data from the National Vital Statistics System (NVSS). This data includes information from birth records for all live births which took place in the United States from 2003-2015. This includes the number of weeks of gestation, from which I calculate whether the birth was deemed extremely preterm, and also the county of residence of the mother, which I use to infer whether or not she lived in a treated county when the child was born. It also includes an indicator for whether that birth resulted in an infant death, and if so, it includes information from the death record including how old the infant was when they died and what the primary cause of death was. Infant deaths are only recorded if they occurred after a live birth, so stillbirths and miscarriages are not included in the NVSS mortality data.

This data allows me to calculate county-wide rates for both infant mortality and EPBs for each year. EPBs are important to measure independently of infant mortality, because although roughly 75% of EPBs will survive (Patel et al. (2015)), these children are much more likely to suffer from serious cognitive and developmental disabilities (Serenius et al. (2016), Pierrat et al. (2021)). In one sense, we can consider the infant mortality rate to measure the extensive margin of whether a child survives, while the rate of EPB measures the potential quality of life a child faces on the intensive margin.

To control for time-varying county characteristics, I use population data from the National Cancer Institute's Surveillance, Epidemiology and End Results Program (SEER) to construct demographic measures for the percent of the population which are teenagers (15-19 years old), the percent of the population which is Black, and the percent which is Hispanic. To control for time-varying economic conditions, I use county-level unemployment and poverty rates from the Bureau of Labor Statistics. I include two additional indicator variables which control for state-level policies. The first is whether emergency contraceptives are available over-the-counter, while the second controls for whether private insurance plans covering prescription drugs are required to cover any FDA-approved contraceptive. These variables were initially constructed by Lindo and Packham (2017) using data collected from the National Conference of State Legislatures (2012), the

National Women's Law Center (2012), and Zuppann (2011). Finally, I use clinic location data from Lindo and Packham (2017) to designate treatment and control counties.

3.2. Methodology

I estimate the effects of expanding LARC access by estimating event-study specifications of the form:

$$Y_{ct} = \sum_{k \in [-4,..,0,...6]} \theta_k LARC_{c,t-k} + \beta X_{ct} + \alpha_c + \gamma_t + \psi_c * t + \epsilon_{ct} \tag{1}$$

where Y_{ct} measures the outcome of interest for county c in year t. $LARC_c$ is an indicator for a county being treated with a LARC intervention at some point during the sample period, which means the county is both in Colorado and has a Title X clinic. k measures the years before and after the intervention took place. Therefore, θ_{-4} through θ_{-1} estimate differences in trends between treated and control counties before the CFPI went into effect and θ_1 through θ_6 measure the impact of the LARC access on the outcome of interest. If the equal counterfactual trends assumption holds and if the CFPI had a causal impact on infant health outcomes, we should expect θ_{-4} through θ_{-1} to be close to zero and statistically insignificant, while θ_1 through θ_6 should be negative and significant. X_{ct} includes a vector of time-varying, county-level control variables which could impact infant health outcomes. α_c are county fixed effects, which control for time-invariant characteristics of each county which impact infant health, while γ_t are year fixed effects which control for nationwide trends in infant health across time. $\psi_c * t$ is a county-specific linear time trend, which I include to prevent pre-existing differences in trends between treated and control counties from being picked up as a treatment effect. I show my results both with and without the linear trends to demonstrate that my findings do not hinge upon their inclusion. I estimate this specification using weighted-least-squares, where the weights are determined by the total number of births in a county-year cell, so that the results are not driven by idiosyncratic changes in small counties.

I include all 38 counties in Colorado with Title X clinics as treated. In choosing control counties, I begin with all counties which also have a Title X clinic but which did not receive additional funding for LARCs. I then exclude all counties in the 12 states[14] which did not expand Medicaid following the passage of the Affordable Care Act of 2010, as Bhatt and Beck-Sagué (2018) demonstrate that infant mortality went up in these states relative to expanding states. I also drop Iowa and St. Louis, as they both had smaller-scale LARC interventions in the years just before the CFPI was implemented which could bias my results.

I drop all counties in Colorado without a Title X clinic as well as all counties in neighboring states which border a treated county because of concerns over potential spillover effects.[15] Overall, this approach results in 38 treated counties and 1,325 control counties. Figure 1 displays treated counties in red and control counties in grey. Counties which are dropped from the regressions are white. Importantly, I demonstrate in the appendix that my results are not sensitive to the choice of control group, and that they are robust to using both synthetic control and synthetic difference-in-differences.

4. Results

4.1. First Stage - How did the CFPI change the composition of who was giving birth?

In order for the CFPI to have caused a decrease in adverse infant health outcomes, it must first be the case that it caused a shift in the composition of who was giving birth in Colorado. The counterfactual births that were avoided should have observable characteristics which make them more likely to have led to the outcomes in question. Figure 2 displays four event study estimates, without covariates or linear trends, for various factors associated with infant health outcomes. The top left figure looks at how the percentage of mothers who are teenagers shifted in Colorado in the years following the CFPI. I focus on the percentage of births to teenagers as opposed to the teen birth rate in order to demonstrate that not only are teen births going down overall, but they are going down more rapidly than other births in Colorado. If, for example, births at all ages were declining in Colorado relative

[14] Florida, Georgia, South Carolina, North Carolina, Tennessee, Alabama, Mississippi, Texas, Kansas, South Dakota and Wyoming

[15] Since women could travel from neighboring counties to ones with a Title X clinic, these counties can be considered partially treated. Including them in the treated group could bias my estimates downward as the effects are almost certainly smaller for counties where it is more difficult to obtain LARCs. Including them as control counties could also bias my estimates downward by including counties which received a partial treatment in my control group, violating the Stable Unit Treatment Value Assumption (SUTVA). The easiest way to avoid these issues is by dropping these counties entirely (Butts (2021))

to other states, we could still see a decline in the teen birth rate as documented by Lindo and Packham (2017) without seeing a shift in the composition of who is giving birth.

This graph shows that not only is the teen birth rate declining, the share of births to teenagers is as well, which suggests that we may expect to see reductions in adverse infant health outcomes. The top right graph in Figure 2 shows the change in repeat teen births, which are even more closely associated with adverse infant health outcomes. It is particularly telling that the magnitude on repeat teen births is over half the size of the coefficient on teen births. Prior to the CFPI, there were between 6,000-7,000 teen births per year and 1,000-1,500 repeat teen births per year in Colorado. Since the coefficient on repeat teen births is over half the size of the coefficient on all teen births, the reduction in teen births must have been especially concentrated in repeat teen births.

The bottom left graph in Figure 2 tracks the percentage of births which are covered by Medicaid. Unfortunately, this variable only shows up in the NVSS data beginning in 2009, so there is no way to track trends leading up to the CFPI, but there is a clear decline in Colorado relative to the comparison counties after 2009. Finally, the bottom right graph looks at the percent of births to mothers without a high school degree. Incompleteness in the maternal education variable is also a limiting factor. Typically, births in Colorado have maternal education reported for between 98-99% of all observations in each year. The exception to this is in 2007 and 2008, where it is missing 99.7% and 99.9% of observations, respectively, due to a transition in the way the education variable was recorded. For this reason, I drop 2007 and 2008 from the event-study. Somewhat reassuringly, the percent of mothers without a high school degree does not change dramatically from 2006 to 2009.[16] Once again, there is a large and statistically significant decline in the years after the CFPI. It is worth noting that the magnitude of this reduction is almost ten times the size of the reduction in teen births, which suggests that fertility declined among lower educated mothers across the age spectrum in Colorado, and that this result is not driven by the reduction in teen births alone. On multiple dimensions, the composition of who is giving birth in Colorado shifted following the CFPI in ways which could lead to a reduced risk of adverse infant health outcomes.

[16]In 2006, 79.2% of mothers had at least a high school degree, while in 2009 this number was up to 80.4%, though it would then rise to 83.8% in 2011, 86.6% in 2013, and 87.8% in 2015

4.2. The Effect of the CFPI on Extremely Preterm Births and Infant Deaths

This section details my estimates of the effect of expanded LARC access on both EPBs and overall infant mortality. Figure 3 displays overall trends in each of these outcomes in Colorado counties with a Title X clinic, compared with the annual number of LARCs inserted through the CFPI. For both outcomes, the rates hover between 5.7 and 6.5 occurrences per 1,000 live births from 2003 to 2009 with some noise but no apparent trend. As LARCs begin to be given away via the CFPI in 2009, both rates are at local maxima near 6.5, but begin to decline shortly thereafter. Both fall slightly in 2010 but then more aggressively in 2011 and 2012 as more and more LARCs are inserted. These staggered declines make sense as it would take time after each insertion for a birth that would have happened in the counterfactual world to be avoided. Both rates settle after 2012 to values mostly between 4.5 and 5.5 occurrences per 1,000 births, with reductions of greater than one occurrence per 1,000 births each. It is also worth pointing out that births in Colorado are predominantly white (91%), which limits my ability to look at heterogeneous treatments by race. On average, Black mothers have higher rates of unintended pregnancies (Guzman et al. (2010)), and are more susceptible to suffer an infant death (Matthews and MacDorman (2013)), so an intervention that removes barriers to LARC access for Black women could have even larger effects.

Figure 4 displays trends in both outcomes for the treated and control groups from 2003 to 2015. The top row displays the raw rates, while the bottom row displays a three-year moving average in order to minimize the noisiness of the data. In all four cases, trends are evolving similarly from 2003 to 2009, though with some noise. Trends in the control group continue on roughly the same path after 2009, but there are large declines in treated counties which persist all the way through 2015.

Table 1 displays estimates of the event-study specification outlined in equation (1), with coefficients detailing the changing rates of EPB across in Colorado for three years before and four years after the CFPI was initiated. The top panel of Table 1 displays the estimates on the pre-treatment leads while the bottom panel displays estimates for the post-treatment lags. The first thing to notice is that while all of the estimates in the top panel are insignificant at the 10% level, all of the post-treatment lag coefficients for years two and three are negative and significant at

5%, and years four through six are significant at 5% in every specification except one.

Column one displays the standard two-way fixed effects specification and does not include any controls beyond county and year fixed effects. The pre-treatment leads are all insignificant and mostly small in magnitude. After the intervention, there is a small and insignificant decline in the first year, followed by declines of between 0.8 and 1.4 EPBs per 1,000 live births for years two through six, with each of these estimates significant at 5%. In column two I add controls for county demographic makeup and economic conditions, and the reductions are slightly larger. In column three I add the two policy controls and the results grow in magnitude. Finally, in column four I add county linear trends and the effect is attenuated slightly, but still with large, significant reductions of around 1.4-1.5 EPBs per 1,000 live births in years two and three.

Table 2 presents estimates of equation (1) with the infant mortality rate (IMR) replacing 'extremely preterm' births on the left-hand side. As with EPB's, there does not appear to be much movement in the four years before the intervention, and then there are large declines in years three through six following treatment which are all significant at 5% in every specification. In this setting, including county linear trends both reduces the average pretreatment difference and increases the size and significance of the posttreatment lags.

Since treatment is not staggered in this setting, this paper is free from the majority of concerns in the recent difference-in-differences with staggered treatment literature (Goodman-Bacon (2021), Sun and Abraham (2021), Callaway and Sant'Anna (2021) to name a few), but another important potential criticism in the TWFE literature comes from Gardner (2021), who points out that the treatment effect can actually contaminate the group and period fixed effects. Figure 5 displays estimates on both outcomes of interest, both with and without county-specific linear time trends, re-estimated using Gardner (2021)'s two-stage difference-in-difference estimator, which is robust to this concern as well as heterogeneous treatment effects with staggered timing. In each case, the parallel trends assumption looks plausible, and the intervention is followed by a large, statistically significant reduction in both outcomes in years two through six. Although some of the individual coefficients are insignificant, a test of the joint significance of the estimates for years two through five returns a p-value of 0 in all four cases.

In addition to the event study estimates presented in this section, in Appendix B I conduct a randomization inference, which randomly selects groups of 38 control counties and runs placebo specifications to determine whether the reduction in extremely preterm births and infant mortality I find in Colorado is greater than the random fluctuations which occur in other areas. I find that in both raw and percentage terms, the reductions in Colorado are greater than over 99% of placebo specifications in both outcomes, consistent with a p-value of less than .01. I also conduct a state-level randomization inference, where each state is assigned a placebo treatment and the relative effect sizes are compared in order to calculate a p-value. In both cases, the declines in Colorado are significant at 5%. This method has the added benefit of not relying on standard errors to calculate the p-value, which suggests that the earlier findings in this section are not due to artificially small standard errors.

4.3. Where are These Improvements Happening?

So far, it has been established that large, statistically significant declines in EPBs and infant mortality occurred in Colorado following the CFPI. In order to establish a causal impact of LARC access on this outcome, however, it is important that the treatment effects are concentrated near the Title X clinics through which the program was implemented. In this section, I compare counts of EPBs and infant mortalities in treated versus untreated counties in Colorado in order to address whether state-specific policies or shocks could have impacted infant health across the entire state. Since non Title X counties were not used as controls, it is not important that they satisfy the equal counterfactual trends assumption, but it should be the case that any treatment effect which shows up should predominantly occur in counties with Title X clinics.

To that end, Figure 6 displays the raw number of annual EPB cases and infant deaths for Colorado counties, broken out by whether or not they were born to a resident of a county with a Title X clinic. The top left graph displays the EPB count over time for counties with a Title X clinic in Colorado. From 2003 to 2009, the count hovers between 400 and 450. The rate drops slightly in 2010 before declining sharply in 2011 and then remaining between 300 and 350 for the remainder of the sample. The story is very different in counties without a Title X clinic, as the counts fluctuate apparently at random between 10 and 30 occurrences throughout much of the sample period and actually rise from 2009-2012 when EPBs were falling in the treated counties.

The bottom panel of Figure 6 repeats this process for infant mortality counts. Counts for Colorado Title X counties hover around 400 from 2003 to 2009, before declining each subsequent year until 2012, where the count settles around 300 per year. In non-Title X counties, infant deaths reach a minimum of 12 in 2009, before rebounding back to pre-CFPI levels between 15-25. There appears to be a clear treatment effect in Colorado Title X counties, but none in non-Title X counties. This suggests that it is unlikely that the main results of this paper are due to any statewide policy changes, as we would expect these to also impact the counties without a Title X clinic.

Interestingly, this also suggests that there were no spillover effects onto untreated counties. This is perhaps surprising, as there is no requirement that a patient must live in the county where the clinic is located. Other research has shown, however, that travel distance to a clinic is an impediment to receiving an abortion (Lindo, Myers, et al. (2019)), so it makes sense that it would also make it more difficult to receive a LARC, particularly for low-income women who may not have access to an automobile.

4.4. Reductions by Cause of Death

Figure 7 displays a breakdown of the reduction in infant deaths from 2006-2009 to 2011-2014 by cause of death. Overall, infant deaths went down from 425 per year in 2006-2009 to 323 in 2011-2014, a reduction of 102 deaths per year, or 24%. Figure 7 shows the change in each of the most common causes of infant death, both in the actual number of deaths and in percentage terms. By far the largest percentage reduction is in Sudden Infant Death Syndrome, or SIDS, which dropped from an average of 40.5 per year in the pre period to only 15.5 in 2011-2014. SIDS is closely associated with socioeconomic status, so it does make sense that an intervention which enabled lower income women to avoid unwanted pregnancies could reduce SIDS. For example, prior to the CFPI, there were approximately 0.7 SIDS deaths per 1,000 live births to Colorado women without a college degree, but only 0.17 SIDS deaths per 1,000 live births among Colorado women with a college degree. There were also large percentage reductions in deaths due to hemorrhage, bacterial sepsis, and homicide, but these are relatively rare outcomes and could be attributable to noise. There were also large reductions in deaths due to being preterm or low birth weight, birth defects, and deaths due to other causes.

4.5. *Robustness Checks*

I now describe results from a series of robustness checkon my main results. Online Appendix Figure 2 recreates the estimates from column four of Table 1 and 2 with a series of minor alterations. First, the main estimates reported standard errors clustered at the county level, but the treatment was arguably at the state level, even though there was variation from clinic to clinic and county to county in the number of LARCs inserted. Recent papers, including Abadie, Athey, et al. (2022), point to the importance of clustering standard errors at the level at which treatment was administered. The graphs on the left of Appendix Figure 2 cluster standard errors at the state level instead of the county level. The county-level estimates are actually more conservative, as the standard errors are smaller when clustered at the state level.

Another concern is that since the LARC initiatives studied in this paper impacted fertility rates, then both the numerator and denominator in the main specifications are being treated. The main point being made in this analysis is that selection effects lead the numerator (the number of adverse outcomes) to be more heavily influenced than the denominator (the total number of births), but it is also useful to demonstrate that the effects still show up when the denominator is untreated by the intervention. To do this, I replace the total number of births in the denominator with the total population of women aged 15-45 in each county. It is possible that some women were induced to migrate into these counties in order to take part in the initiatives, but this seems unlikely as the cost of moving is generally much larger than the cost of a LARC. The middle graphs in Appendix Figure 2 display the results from these specifications. Because only a small fraction of women aged 15-45 will give birth in a year, the coefficients are rescaled to represent the rates of each outcome per 10,000 women aged 15-45 in the population, instead of per 1,000 live births. In each case, the interpretation is similar to the main specifications, with jointly significant reductions in each outcome. Finally, the right graphs in Appendix Figure 2 recreate the main specifications with state-specific linear time trends. The main results are similar regardless of whether there are no time trends at all, or whether time trends are estimated at the state or county level.

In order to demonstrate that the results above do not depend on the selection of control groups, Appendix Tables 4 and 5 recreate column 1 from Tables 1 and 2 using a series of different control groups. First, I use all untreated counties in the U.S., regardless of whether there is a Title X clinic or whether the state expanded Medicaid. Next, I restrict the sample to all Title X counties but continue to include states which did not expand Medicaid. Then I use Title X counties in each of the four main regions of the U.S. (Northeast, South, Midwest, and West) separately. In all specifications, there are no significant estimates prior to the intervention followed by large, statistically significant declines for multiple years after.

As the treatment is concentrated geographically in Colorado, there is also concern that there might be spatial correlation in the residuals driven by some unobservable characteristics which drive risky sexual behaviors. To address this, Appendix Table 6 reestimates column 1 from Tables 1 and 2 using Colella et al. (2019)'s method which calculates standard errors allowing for arbitrary correlation of the residuals. I estimate specifications allowing for correlations of the residuals for counties within 25, 50, and 75 miles of one another and the results are consistent with main findings of the paper.

Finally, in Appendix C I reestimate my main specifications using both the synthetic control method of Abadie, Diamond, et al. (2010) and the synthetic difference-in-differences method of Arkhangelsky et al. (2021). Once again, results from these strategies are consistent with my main findings.

5. Back-of-the-Envelope Calculations/Conclusion

Back-of-the-Envelope Calculations

In this section I perform three back-of-the-envelope calculations in order to better understand the costs and benefits of the CFPI. I begin with an estimate of the cost reduction due to avoided ventilation care for EPBs in Colorado.

Avoided Ventilation Costs for EPBs

EPBs are a tragic and traumatic event, but they are also incredibly costly as the procedures used to treat EPBs are expensive. In order to understand the cost savings in care for EPBs in Colorado, I use a series of estimates from Hayman et al. (2015). Using the estimates from this paper, I to distribute the total reduction of 236 EPBs between 2011 and 2015 which are implied by my most conservative estimate from section 4 into their likely gestational age category and whether they were likely to survive or not. I then use the likelihood of whether they would have received ventilation care and the average cost for a patient of that gestational age and survival cell to arrive at an implied cost-savings based on my estimates. The details of this calculation are laid out in Appendix D, but they imply a cost savings in avoided ventilation care of approximately $15.6 million over the course of the program, which is 67% of the total cost of the CFPI.

Cost per Infant Death Avoided

My most conservative estimates of the effect of the CFPI on infant mortality come from two-stage difference-in-difference estimates not including linear trends. Multiplying these reductions by the average number of births in Colorado prior to the CFPI[17] allows me to estimate the yearly number of infant deaths avoided due to the program. This calculation suggests a reduction of 31 deaths in 2011, 61 in 2012, 50 in 2013, 52 in 2014 and 30 in 2015, for a total reduction of approximately 224 deaths. If avoiding infant deaths were the entire goal of the CFPI, it would have cost approximately $102,700 per infant death avoided.

How Many LARCs Were Needed to Prevent Each Infant Death

In order to understand the degree of selection which must have taken place for the CFPI to cause these reductions, I can also calculate the number of LARCs needed to prevent a single infant death in each year. By 2011, there had been 11,502 LARCs inserted via the CFPI. The estimated reduction of 31 infant deaths implies a reduction of one death for every 371 LARCs inserted. The corresponding number of LARCs inserted per infant death avoided are 286 for 2012, 467 for 2013, 576 for 2014, and 1,225 for 2015.[18] The fact that the reduction was larger in the earlier years despite there being fewer LARCs in circulation at that point is consistent

[17] There were between 69,000 and 71,000 births each year from 2003-2008 and there was no particular trend, so I use the average of 70,000

[18] According to CDPHE (2017), there were a cumulative total of 17,470 LARCs in 2012, 23,235 in 2013, 29.956 in 2014, and 36,762 in 2015.

with the fact that there were large waiting lists to get LARCs when the CFPI began, and the women on waiting lists may have been especially selected on factors correlated with both fertility and infant mortality risk.

It is worth exploring whether these reductions could plausibly be attributed to the expansion of LARC access in Colorado. Put differently, this exercise allows us to calculate how selected the young women who received LARCs via the CFPI must have been in order for these estimates to be causal. In 2008, there were roughly 88 births per 1,000 women aged 15-35 in Colorado. There were also approximately 6.5 infant deaths per 1,000 live births statewide. I will focus on the estimate from 2012, which implies the largest degree of selection was necessary. Assuming that LARCs were randomly distributed to women in Colorado and that they worked perfectly, we would expect to see a reduction of 1,537 births and 10 infant deaths in 2012.

If, however, the women who received LARCs were roughly twice as likely to give birth and three times as likely to suffer an infant death conditional on giving birth than the average 18-35 year old woman in Colorado, we would expect a reduction of 61 infant deaths in 2012, which is precisely the reduction implied by my estimates. This level of selection is certainly plausible, as there is a great deal of variation across women in both the likelihood of childbirth and risk of infant mortality.

In 2009, women below the federal poverty line had roughly double the fertility as women above 200% of the federal poverty line (Statista.com (2024)). Low income women in the U.S. are also 1.8 times as likely to suffer an infant death as high-income women (Turner et al. (2020)), while women without a high school degree in the U.S. experienced a 2.4 times higher risk of infant mortality in 2016 than mothers with a college degree (Singh and Yu (2019)). In addition, there are important gradients in infant mortality risk related to lifestyle factors such as smoking (Johansson et al. (2009)), alcohol use (O'Leary et al. (2013)), and drug use (Wolfe et al. (2005)). There is also a great deal of variation in infant mortality across race, with Black mothers suffering infant deaths at over twice the rate of white mothers in 2010 (Matthews and MacDorman (2013)). Because of the large degree of variation in infant mortality which exists across demographic groups, even the relatively high degree of selection which is implied by the 2012 estimate is plausible.

Conclusion

This paper uses the implementation of a privately funded family planning programs to investigate whether expanding access to long-acting reversible contraceptives to low-income women can reduce adverse infant health outcomes. Because these women are the most likely to experience an extremely preterm birth or an infant death, improving their ability to avoid unwanted pregnancies has the potential to create positive selection in the health of the cohorts of children being born. By comparing trends in treated counties with trends in other counties across the United States with similar family planning clinics which did not receive additional funding to improve LARC access, I demonstrate that expanded LARC access led to large reductions in both the rates of extremely preterm births and overall infant mortality. These results are not sensitive to model specification and only show up in the counties which have the family planning clinics where the programs were implemented, ruling out the possibility that statewide policy shifts or economic shocks could be confounding my estimates.

The programs I study in this paper have been shown to have many other important benefits, including reducing the teen birth rate and increasing female human capital accumulation. I demonstrate an important unintended consequence of expanding LARC access to low-income women, in that it creates positive selection in the health of the cohorts of children being born. These results are particularly important in light of the recent Supreme Court decision in Dobbs vs. Jackson, as legalizing abortion and expanding LARC access to low-income women both appear to reduce adverse infant health outcomes. As abortion becomes more restrictive in many states, effective contraceptive access will become even more important.

Table 1 — Event-Study Specifications Measuring the Effect of LARC Access on the Rate of Extremely Preterm Births - 2003-2015

	(1)	(2)	(3)	(4)
	EPB	EPB	EPB	EPB
4 Years Before	-0.483	-0.576	-0.658	-0.836
	(0.498)	(0.476)	(0.480)	(0.619)
3 Years Before	-0.587	-0.661	-0.699	-0.902
	(0.445)	(0.442)	(0.442)	(0.495)
2 Years Before	-0.354	-0.388	-0.389	-0.533
	(0.327)	(0.323)	(0.323)	(0.382)
1 Year Before	-0.296	-0.314	-0.315	-0.399
	(0.466)	(0.462)	(0.463)	(0.499)
Avg pretreatment effect	-.430	-.484	-.515	-.668
p-value (avg effect = 0)	.2340	.1664	.1430	.1155
1 Year After	-0.229	-0.237	-0.240	-0.190
	(0.514)	(0.513)	(0.513)	(0.538)
2 Years After	-1.424^{***}	-1.446^{***}	-2.169^{***}	-1.489^{***}
	(0.257)	(0.262)	(0.312)	(0.410)
3 Years After	-1.391^{**}	-1.416^{*}	-2.143^{***}	-1.399^{*}
	(0.531)	(0.550)	(0.577)	(0.659)
4 Years After	-0.792^{**}	-0.814^{**}	-1.543^{***}	-0.720
	(0.274)	(0.301)	(0.345)	(0.461)
5 Years After	-1.080^{*}	-1.111^{*}	-1.843^{***}	-0.923
	(0.467)	(0.495)	(0.524)	(0.634)
6 Years After	-0.798^{*}	-0.830^{*}	-1.564^{***}	-0.568
	(0.371)	(0.402)	(0.438)	(0.608)
Avg effect years 2-6	-1.097	-1.123	-1.852	-1.020
p-value (avg effect = 0)	.0003	.0006	.0000	.0365
County and year FE's	Y	Y	Y	Y
Main controls	N	Y	Y	Y
Policy controls	N	N	Y	Y
County linear trends	N	N	N	Y
Observations	15169	15157	15157	15157

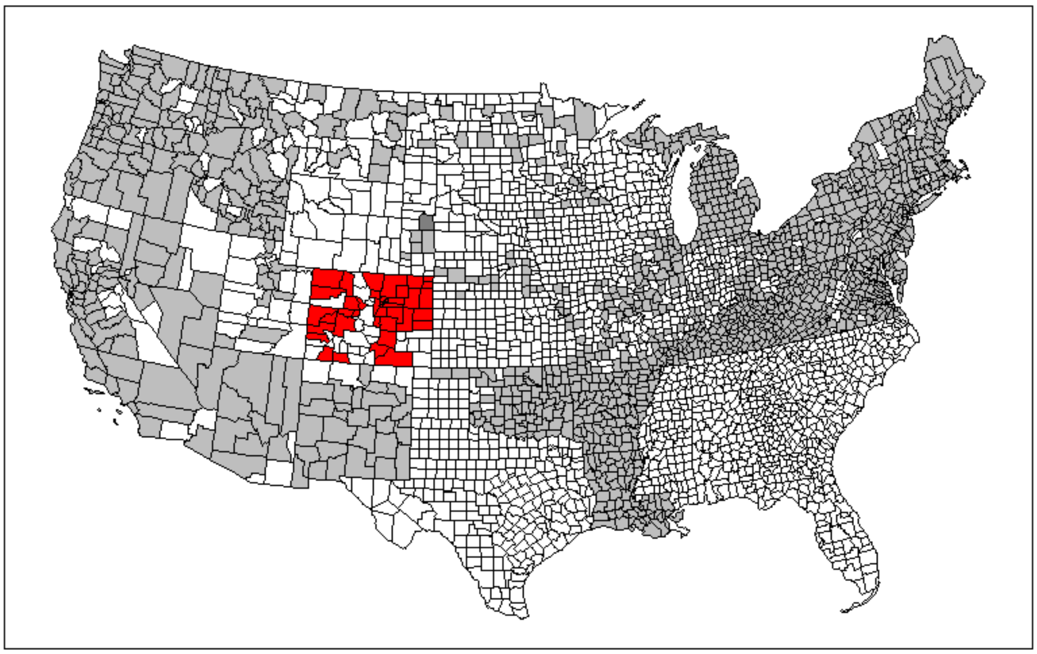
Note: Standard errors in parentheses, clustered at the county level. This table displays estimates of the effect of the Colorado Family Planning Initiative on the number of infant deaths per 1,000 live births. Column one estimates the standard two-way fixed effects (TWFE) specification. Column two adds demographic and economic controls. Column three adds policy controls for whether emergency contraceptives were available over the counter and whether private insurance plans were required to cover any FDA-approved contraceptive. Column four adds county-specific linear time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2 — Event-Study Specifications Measuring the Effect of LARC Access on the Rate of Infant Mortality - 2003-2015

	(1)	(2)	(3)	(4)
	IMR	IMR	IMR	IMR
4 Years Before	-0.293	-0.398	-0.451	0.952
	(0.517)	(0.509)	(0.514)	(0.586)
3 Years Before	-0.957	-1.023	-1.045	-0.0569
	(0.613)	(0.608)	(0.608)	(0.595)
2 Years Before	-0.574	-0.611	-0.611	0.0329
	(0.367)	(0.368)	(0.368)	(0.378)
1 Year Before	-0.394	-0.425	-0.425	-0.101
	(0.503)	(0.503)	(0.504)	(0.529)
Avg pretreatment effect	-.554	-.614	-.633	.206
p-value (avg effect = 0)	.1505	.1079	.0989	.5995
1 Year After	-0.583	-0.611	-0.612	-0.935
	(0.465)	(0.467)	(0.467)	(0.495)
2 Years After	-0.899	-0.943	-1.363**	-1.640**
	(0.495)	(0.492)	(0.515)	(0.595)
3 Years After	-1.330**	-1.391**	-1.813***	-2.399***
	(0.463)	(0.479)	(0.504)	(0.548)
4 Years After	-1.173*	-1.231*	-1.655**	-2.547***
	(0.469)	(0.493)	(0.517)	(0.622)
5 Years After	-1.203**	-1.261***	-1.687***	-2.890***
	(0.373)	(0.375)	(0.407)	(0.491)
6 Years After	-0.887*	-0.945**	-1.372***	-2.884***
	(0.348)	(0.344)	(0.378)	(0.532)
Avg effect years 2-6	-1.098	-1.154	-1.578	-2.472
p-value (avg effect = 0)	.0006	.0004	.0000	.0000
County and year FE's	Y	Y	Y	Y
Main controls	N	Y	Y	Y
Policy controls	N	N	Y	Y
County linear trends	N	N	N	Y
Observations	15015	15003	15003	15003

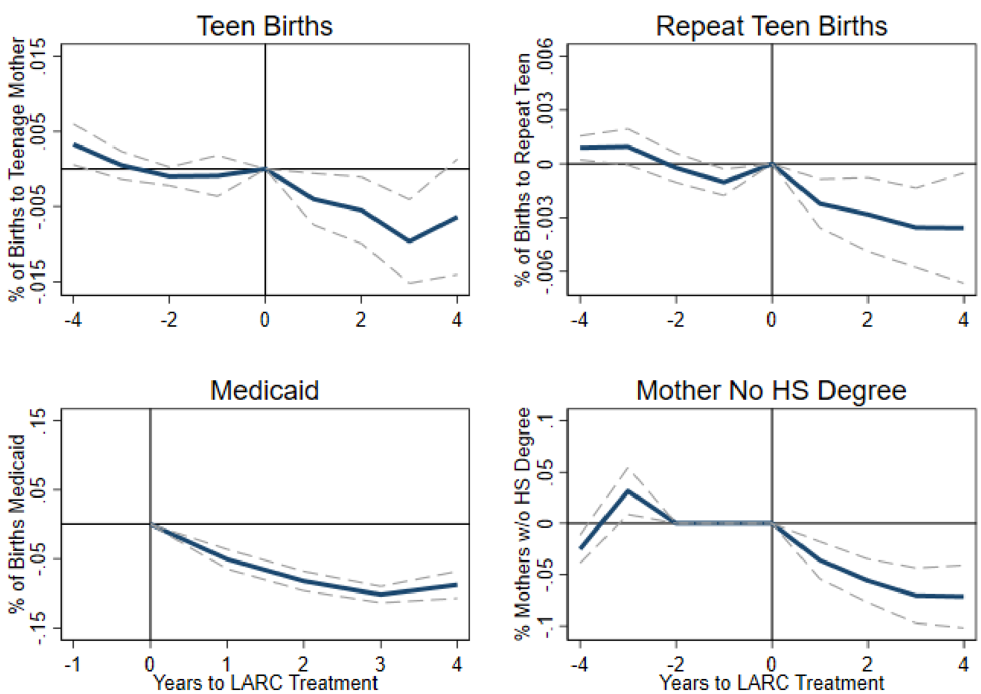
Note: Standard errors in parentheses, clustered at the county level. This table displays estimates of the effect of the Colorado Family Planning Initiative on the number of infant deaths per 1,000 live births. Column one estimates the standard two-way fixed effects (TWFE) specification. Column two adds demographic and economic controls. Column three adds policy controls for whether emergency contraceptives were available over the counter and whether private insurance plans were required to cover any FDA-approved contraceptive. Column four adds county-specific linear time trends. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 1 — Treatment and Control Assignment



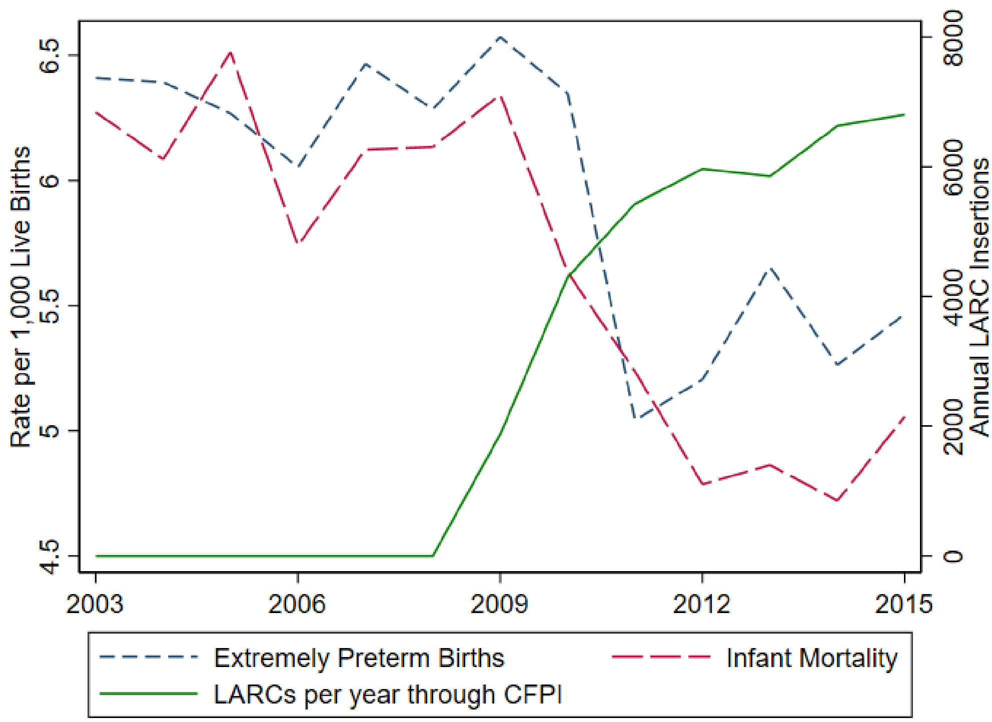
Note: This figure displays (treated) counties which have a Title X family planning clinic and which received specific funding to expand LARC access to low-income women in red. Control counties, which are other US counties which have a Title X clinic but which did not received specific funding to expand LARC access are denoted in grey. Counties which do not have a Title X clinic, or which are located in a state which did not expand Medicaid with the Affordable Care Act of 2010 are omitted from all subsequent regressions, as are all counties which border a treated county for concerns about potential spillovers which would violate the Stable Unit Treatment Value Assumption (SUTVA).

Figure 2 — The Effect of the Colorado Family Planning Initiative on the Composition of Who is Giving Birth in Colorado



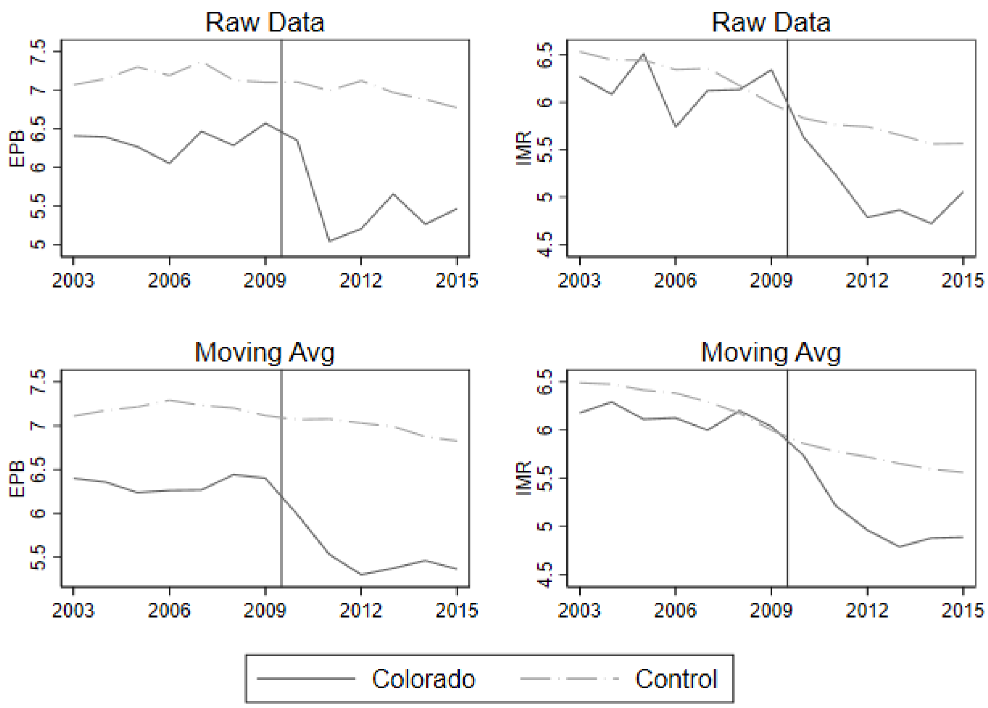
Note: This figure displays event study estimates demonstrating how the composition of women giving birth changed in Colorado following the implementation of the Colorado Family Planning Initiative. The top left graph estimates the effect of the program on the percent of births which were to a teenage mother. The top right graph estimates the effect of the program on births to a repeat teen mother. The bottom left graph estimates the effect on the percent of births covered by Medicaid, while the bottom right graph estimates the effect on the percent of births to mothers without a high school diploma. All specifications use restricted-access natality data from the National Vital Statistics System.

Figure 3 — Time Series of Infant Health Outcomes and LARCs Inserted through the Colorado Family Planning Initiative - 2003-2015



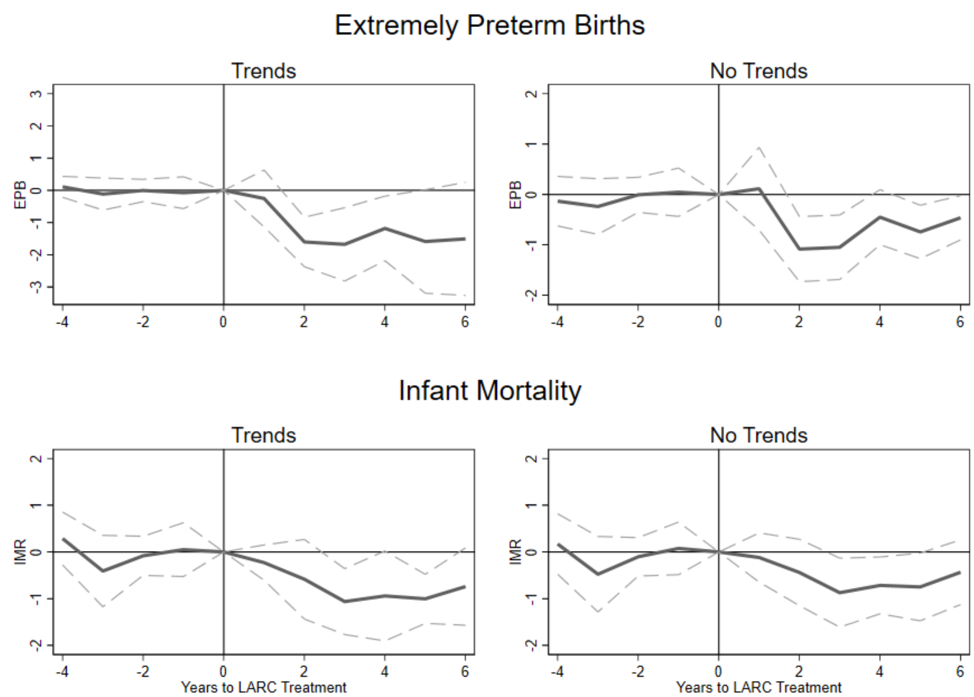
Note: This figure displays the annual number of LARCs inserted through the Colorado Family Planning Initiative compared with the rates of extremely preterm births (births before 28 weeks gestation) as well as the infant mortality rate in Title X counties in Colorado, both calculated using restricted-access data from the National Vital Statistics System.

Figure 4 — Comparison of Infant Health Outcomes Between Treated and Control Groups - 2003-2015



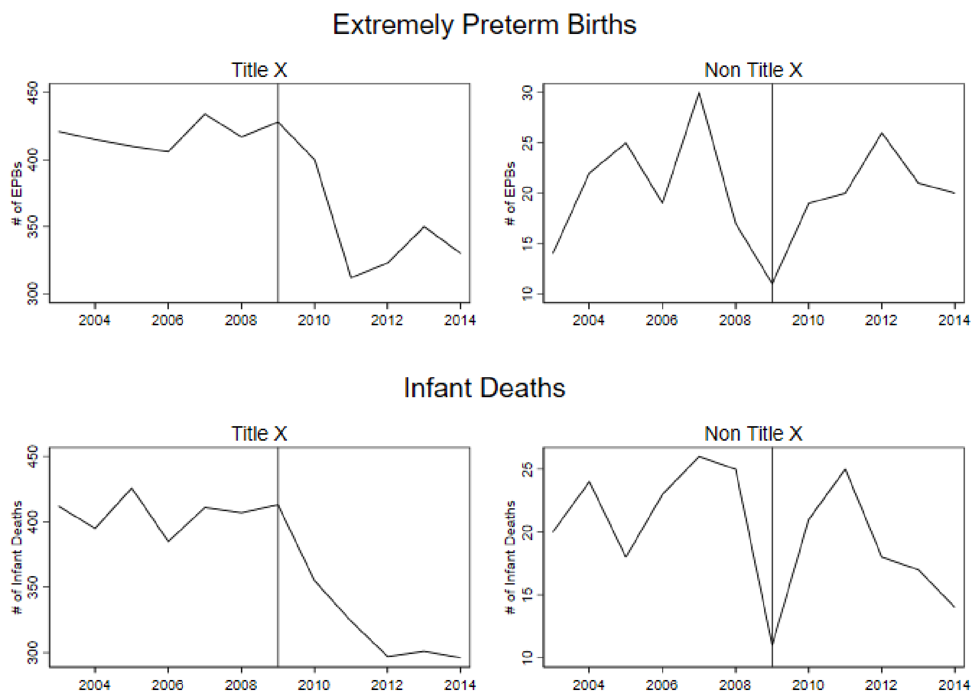
Note: This figure displays the rates of infant mortality and extremely preterm births in the treated and control counties over time. The treated counties are Colorado counties which have a Title X family planning clinic, while the control counties are other counties across the U.S. which have a Title X family planning clinic, but which did not receive specific funding to increase LARC access to low-income women. Additionally, counties in states which did not expand Medicaid with the Affordable Care Act of 2010 are excluded, along with any counties which border a treated county. Rates are calculated using restricted-access data from the National Vital Statistics System.

Figure 5 — Event-Study Graphs Using Two Stage DiD (Gardner (2021)) to Estimate the Effect of LARC Access on the Rate of Extremely Preterm Births and Infant Mortality in Colorado



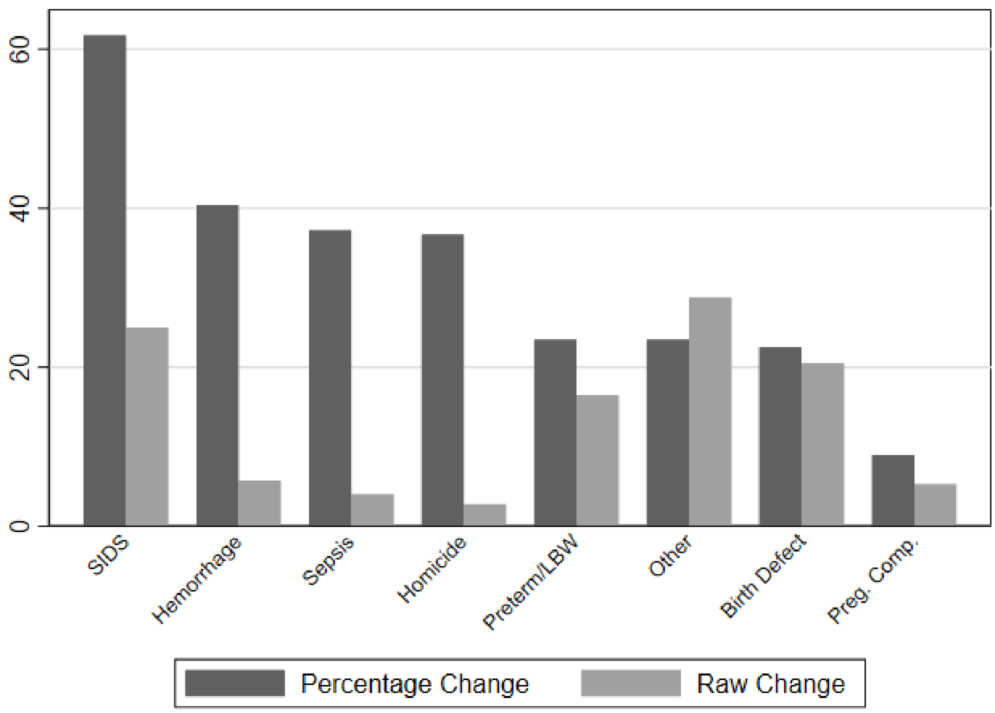
Note: This figure uses natality data from the National Vital Statistics System to plot coefficients from the event-study specification utilizing the two-stage difference-in-difference method of Gardner (2021), comparing extremely preterm birth and infant mortality rates in Colorado with control counties. The top panel estimates the effect of the intervention on extremely preterm births, while the bottom estimates the effect on infant mortality. The graphs on the left include county-specific linear time trends, while the graphs on the right do not.

Figure 6 — Comparison of Adverse Infant Health Outcomes in Title X vs. Non-Title X Counties in Colorado



Note: This figure displays the raw number of extremely preterm births and infant deaths in Colorado counties with and without a Title X clinic. Graphs on the left display the outcome for Title X counties, while graphs on the right display the non-Title X counties. The top row displays extremely preterm births, while the bottom row displays infant deaths, using data from the National Vital Statistics System

Figure 7 — Breakdown of Colorado Infant Mortality Reduction by Cause of Death



Note: This figure displays the change in infant deaths in Colorado by cause of death from 2006-2009 to 2011-2014. This is displayed both in the raw change in the number of deaths, and in percentage terms compared with the base rate in the pre-CFPI period. This is calculated using restricted-access data from the National Vital Statistics System.

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A. Online Appendix - Not For Publication

Table 1 — Comparison of Gestational Ages for Extremely Preterm Births in Colorado: Before and After Colorado Family Planning Initiative

Gestational Age	(1) 2005-09	(2) 2010-15	(3) P-value
<24 Weeks	.326	.322	.809
24 Weeks	.138	.132	.556
25 Weeks	.144	.135	.358
26 Weeks	.186	.184	.855
27 Weeks	.207	.227	.086

Note: This table compares the proportion of extremely preterm births in Colorado which fall under each gestational age category, both before and after the Colorado Family Planning Initiative. Column 1 displays the proportion of EPBs in 2005-2009 (pre-intervention) which were in each gestational age category. Column 2 displays the proportion of EPBs in 2010-2015 (post-intervention) in each gestational age category. Column 3 displays a p-value on a test for equality of the proportions before and after the Colorado Family Planning Initiative.

Table 2 — Annual Cost Calculation for Extremely Preterm Birth Reduction for Infants who Would be Predicted to Survive

Gest. Age	(1) Pred. N	(2) Surv. Odds	(3) N Survived	(4) P(Vent|A,S)	(5) (Cost|A,S)	(6) Pred. Costs Avoided
<24 Wks	76.9	.091	7.1	.94	205,000	1,349,000
24 Wks	31.9	.505	16.1	.91	200,000	2,928,000
25 Wks	35.6	.883	31.5	.74	130,000	3,027,000
26 Wks	42.2	.883	37.3	.74	130,000	3,588,000
27 Wks	49.1	.883	43.3	.74	130,000	4,170,000
Total	236.0		135.2			15,063,000

Note: This table calculates the predicted annual cost savings due to avoided ventilation care among infants who would have been predicted to survive. Column 1 distributes the estimate of a reduction of 236 extremely preterm births across gestational age categories, based on the proportional occurrence of each age in Colorado from 2006-2013. Column 2 displays the likelihood of survival for a birth of that gestational age, taken from Hayman et al. (2015). Column 3 calculates the number of births at each gestational age which would be predicted to survive, based on the likelihood in column 2. Columns 4 and 5 display the probability of being ventilated and the average cost of ventilation, conditional on gestational age and survival, respectively. Finally, column 6 calculates the total predicted avoided ventilation costs from surviving infants for each gestational age group.

Table 3 — Annual Cost Calculation for Extremely Preterm Birth Reduction for Infants who Would be Predicted Not to Survive

	(1)	(2)	(3)	(4)	(5)	(6)
Gest. Age	Pred. N	NS Odds	N NS	P(Vent|A,NS)	(Cost|A,NS)	Pred. Costs Avoided
<24 Wks	76.9	.919	70.7	.18	10,000	127,300
24 Wks	31.9	.495	15.8	.74	15,000	175,000
25 Wks	35.6	.117	4.2	.77	20,000	64,200
26 Wks	42.2	.117	4.9	.77	20,000	76,100
7 Wks	49.1	.117	5.7	.77	20,000	88,400
Sum	236.0		101.3			531,000

Note: This table calculates the predicted annual cost savings due to avoided ventilation care among infants who would have been predicted not to survive. Column 1 distributes the estimate of a reduction of 85 extremely preterm births across gestational age categories, based on the proportional occurrence of each age in Colorado from 2006-2013. Column 2 displays the likelihood of death for a birth of that gestational age, taken from Hayman et al. (2015). Column 3 calculates the number of births at each gestational age which would be predicted not to survive, based on the likelihood in column 2. Columns 4 and 5 display the probability of being ventilated and the average cost of ventilation, conditional on gestational age and non-survival, respectively. Finally, column 6 calculates the total predicted avoided ventilation costs from non-surviving infants for each gestational age group.

Table 4 — Event-Study - LARC Treated vs. Control - Testing Alternate Control Groups

	(1)	(2)	(3)	(4)	(5)	(6)
	EPB	EPB	EPB	EPB	EPB	EPB
4 Years Before	-0.523	-0.533	-0.469	-0.744	-0.505	-0.221
	(0.500)	(0.500)	(0.534)	(0.513)	(0.542)	(0.509)
3 Years Before	-0.601	-0.629	-0.693	-0.781	-0.688	-0.271
	(0.445)	(0.446)	(0.483)	(0.456)	(0.483)	(0.461)
2 Years Before	-0.404	-0.396	-0.0619	-0.642	-0.472	-0.118
	(0.326)	(0.326)	(0.358)	(0.344)	(0.368)	(0.346)
1 Year Before	-0.293	-0.312	-0.231	-0.307	-0.439	-0.230
	(0.468)	(0.468)	(0.501)	(0.481)	(0.505)	(0.482)
Avg effect leads 1-3	-.432	-.445	-.329	-.577	-.533	-.206
p-value (avg effect = 0)	.2027	.1901	.3720	.1001	.1538	.5578
1 Year After	-0.218	-0.229	-0.0288	-0.272	-0.327	-0.204
	(0.517)	(0.517)	(0.539)	(0.528)	(0.547)	(0.529)
2 Years After	-1.432***	-1.445***	-1.117***	-1.545***	-1.658***	-1.347***
	(0.253)	(0.254)	(0.298)	(0.274)	(0.319)	(0.283)
3 Years After	-1.341*	-1.347*	-1.262*	-1.341*	-1.601**	-1.246*
	(0.534)	(0.534)	(0.557)	(0.545)	(0.560)	(0.542)
4 Years After	-0.787**	-0.784**	-0.288	-0.777**	-0.997**	-0.983***
	(0.273)	(0.274)	(0.318)	(0.297)	(0.318)	(0.294)
Avg effect years 2-4	-1.187	-1.192	-.889	-1.221	-1.418	-1.192
p-value (avg effect = 0)	.0000	.0000	.0030	.0000	.0000	.0000
Control Counties	All	Title X	Northeast	South	Midwest	West
Observations	27350	19953	2187	11475	4518	2790

Note: Standard errors in parentheses, clustered at the county level. This table displays estimates of the effect of the Colorado Family Planning Initiative on the number of extremely preterm births per 1,000 live births using a variety of different control groups. Column 1 uses all US counties as potential controls. Column 2 restricts this group to only US counties with a Title X clinic. Column 3 uses only Title X counties in the Northeast (CT, ME, MA, NH, NJ, NY, PA, RI, VT). Column 4 uses only Title X counties in the South (AL, AR, DE, FL, GA, LA, MD, MS, NC, OK, SC, TN, TX, VA, WV). Column 5 uses only Title X counties in the Midwest (IL, IN, IA, KS, MI, MN, MO, NE, ND, OH, SD, WI). Column 6 uses only Title X counties in the West (AK, AZ, CA, HI, ID, MT, NV, NM, OR, UT, WA, WY). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5 — Event-Study - LARC Treated vs. Control - Testing Alternate Control Groups

	(1)	(2)	(3)	(4)	(5)	(6)
	IMR	IMR	IMR	IMR	IMR	IMR
4 Years Before	-0.344	-0.371	-0.190	-0.509	-0.370	-0.259
	(0.520)	(0.520)	(0.543)	(0.530)	(0.538)	(0.531)
3 Years Before	-1.003	-1.008	-0.964	-1.166	-0.883	-0.876
	(0.617)	(0.617)	(0.637)	(0.624)	(0.635)	(0.629)
2 Years Before	-0.636	-0.624	-0.519	-0.677	-0.635	-0.580
	(0.368)	(0.368)	(0.388)	(0.382)	(0.387)	(0.389)
1 Year Before	-0.443	-0.424	-0.367	-0.438	-0.476	-0.399
	(0.507)	(0.508)	(0.525)	(0.515)	(0.530)	(0.518)
Avg effect leads 1-3	-.694	-.685	-.616	-.761	-.665	-.618
p-value (avg effect = 0)	.0840	.0881	.1396	.0635	.1124	.1334
1 Year After	-0.540	-0.533	-0.609	-0.473	-0.511	-0.619
	(0.467)	(0.467)	(0.490)	(0.475)	(0.486)	(0.481)
2 Years After	-0.859	-0.834	-0.766	-0.735	-0.924	-0.993
	(0.497)	(0.498)	(0.524)	(0.504)	(0.514)	(0.511)
3 Years After	-1.280**	-1.256**	-1.230*	-1.170*	-1.241*	-1.444**
	(0.467)	(0.467)	(0.494)	(0.475)	(0.485)	(0.478)
4 Years After	-1.172*	-1.171*	-1.030*	-1.164*	-1.125*	-1.356**
	(0.472)	(0.472)	(0.493)	(0.482)	(0.494)	(0.489)
Avg effect years 2-4	-1.104	-1.087	-1.009	-1.023	-1.097	-1.264
p-value (avg effect = 0)	.0027	.0032	.0107	.0065	.0043	.0010
Control Counties	All	Title X	Northeast	South	Midwest	West
Observations	28224	20376	2187	11475	4941	2790

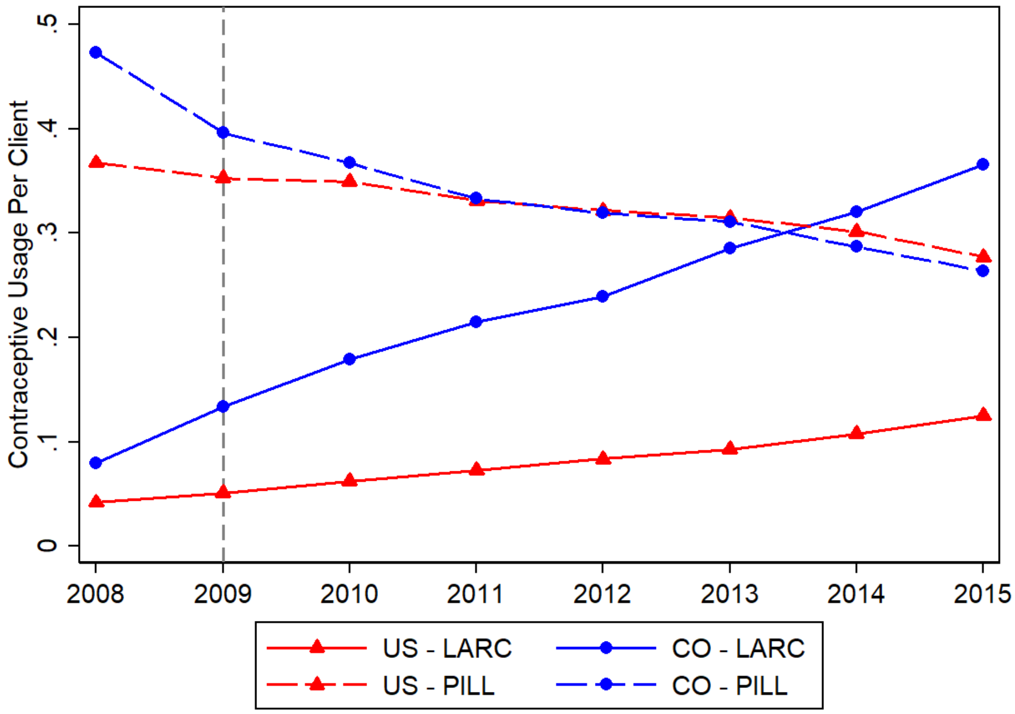
Note: Standard errors in parentheses, clustered at the county level. This table displays estimates of the effect of the Colorado Family Planning Initiative on the number of infant deaths per 1,000 live births using a variety of different control groups. Column 1 uses all US counties as potential controls. Column 2 restricts this group to only US counties with a Title X clinic. Column 3 uses only Title X counties in the Northeast (CT, ME, MA, NH, NJ, NY, PA, RI, VT). Column 4 uses only Title X counties in the South (AL, AR, DE, FL, GA, LA, MD, MS, NC, OK, SC, TN, TX, VA, WV). Column 5 uses only Title X counties in the Midwest (IL, IN, IA, KS, MI, MN, MO, NE, ND, OH, SD, WI). Column 6 uses only Title X counties in the West (AK, AZ, CA, HI, ID, MT, NV, NM, OR, UT, WA, WY). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6 — Event-Study - LARC Treated vs. Control - Allowing for Spatial Autocorrelation

	(1) EPB	(2) EPB	(3) EPB	(4) IMR	(5) IMR	(6) IMR
3 Years Before	-0.594	-0.594	-0.594*	-0.960	-0.960	-0.960*
	(0.411)	(0.360)	(0.253)	(0.635)	(0.734)	(0.439)
2 Years Before	-0.347	-0.347	-0.347	-0.575	-0.575	-0.575**
	(0.266)	(0.212)	(0.190)	(0.367)	(0.298)	(0.212)
1 Year Before	-0.302	-0.302	-0.302	-0.394	-0.394	-0.394
	(0.397)	(0.261)	(.)	(0.286)	(0.373)	(0.328)
Avg preteated outcome	-.414	-.414	-.414	-.643	-.643	-.643
1 Year After	-0.232	-0.232	-0.232	-0.587	-0.587	-0.587**
	(0.383)	(0.519)	(0.318)	(0.540)	(0.399)	(0.203)
2 Years After	-1.433***	-1.433***	-1.433***	-0.904*	-0.904	-0.904
	(0.226)	(0.221)	(0.0995)	(0.449)	(0.519)	(.)
3 Years After	-1.410**	-1.410***	-1.410***	-1.334***	-1.334	-1.334***
	(0.476)	(0.400)	(0.347)	(0.278)	(.)	(0.332)
4 Years After	-0.801**	-0.801**	-0.801**	-1.180*	-1.180***	-1.180***
	(0.257)	(0.306)	(0.292)	(0.466)	(0.114)	(0.180)
Avg effect years 2-4	-1.215	-1.215	-1.215	-1.140	-1.140	-1.140
Autocorrelation distance	25	50	75	25	50	75
Observations	12267	12267	12267	12303	12303	12303

Note: Standard errors in parentheses, clustered at the county level. This table displays estimates of the effect of the Colorado Family Planning Initiative on the number of infant deaths and number of extremely preterm births per 1,000 live births. Columns 1-3 estimate the effect on extremely preterm births, allowing for arbitrary correlation in the residuals of counties within 25, 50, and 75 miles of one another, respectively. Columns 4-6 repeat this exercise with the infant mortality rate replacing extremely preterm births on the left-hand side. Column * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 1 — Comparison of LARC vs. Oral Contraceptive Use in Colorado vs. the United States, 2008-2015



Note: This figure displays the annual percent of Title X clients using either a long-acting reversible contraceptive (LARC) or oral contraceptive from 2008 to 2015, for both Colorado and the United States as a whole. The Colorado specific data come from Humphreys et al. (Forthcoming), while the nationwide data come from NFPRHA (2008), NFPRHA (2009), NFPRHA (2010), NFPRHA (2011), NFPRHA (2012), NFPRHA (2013), NFPRHA (2014), NFPRHA (2015)

Figure 2 — Robustness Checks on Effect of Colorado Family Planning Initiative on Adverse Infant Health Outcomes

Note: This figure displays estimates of the effect of the Colorado Family Planning Initiative (CFPI) on the rates of extremely preterm birth (top row) and infant mortality (bottom row). The left graph has standard errors clustered at the state level. The middle graph replaces the number of total births in the denominator of the outcome of interest with the population of women aged 15-45. The right graph estimates the model with state linear trends instead of county linear trends.

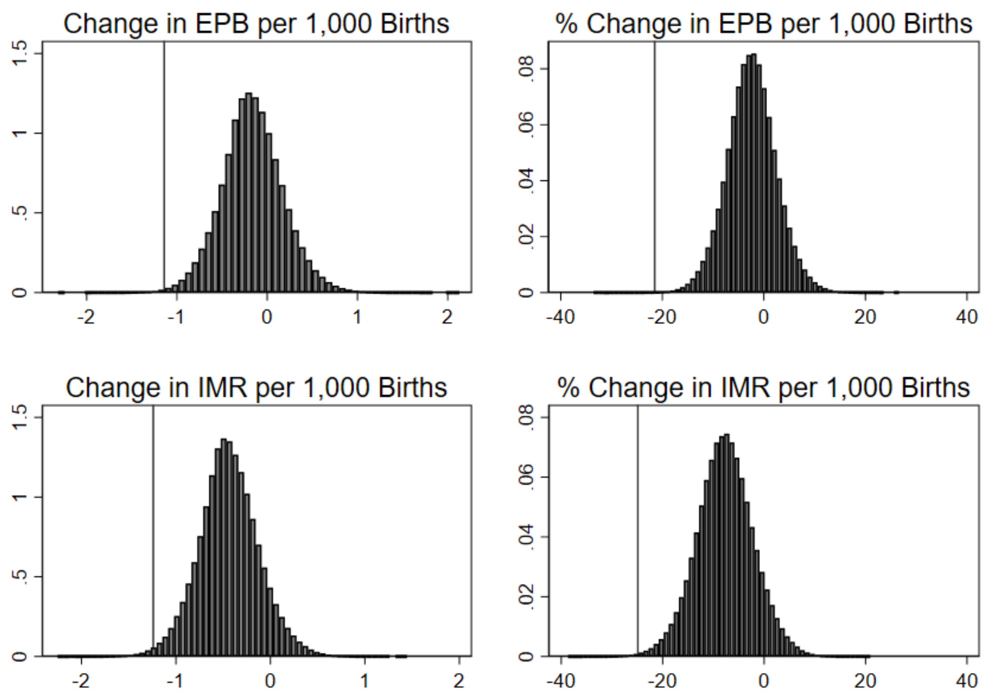
B. Randomization Inference

In addition to the event study, time series, and synthetic control evidence presented in the paper and appendix, I have also conducted two new randomization inference analyses in order to determine whether the declines which occurred in Colorado in extremely preterm births and infant mortality were too large to be due to normal fluctuations in these outcomes. First, as there were 38 treated counties in Colorado and 1,325 control counties with Title X clinics outside of the treated areas, there are $4.9 * 10^{73}$ potential combinations of 38 control counties to use as placebos. As this is computationally infeasible, I randomly selected 1,000,000 different combinations of 38 control counties to see how many would display similar reductions to Colorado. For each of them, I looked at the change in the average number of EPBs and infant mortalities from 2007-2009 to 2011-2013. I computed both the average change and the change as a percentage of the pre-period outcome. For Colorado, there was a 1.14 reduction in EPBs per 1,000 births, which is 17.7% of the pre-period average 6.4 per 1,000, and a 1.24 reduction in infant deaths, which is a 20.0% reduction off of the pre-period average of 6.2 per 1,000. If the CFPI in fact caused these reductions in these outcomes, then it should be unlikely that a randomly drawn group of 38 control counties which did not receive expanded LARC access should see a similar reduction.

Figure 2 below shows the distribution of all four estimates for the 1,000,000 placebo control groups, with a vertical line indicating where the reductions in Colorado fit into this distribution. In all four cases, the resulting p-value is less than .01. For the change in the average number of EPBs from 2007-2009 to 2011-2013, the 38 treated counties in Colorado had a more extreme change than all but 3,515 of the 1,000,000 placebos, for a corresponding p-value of .003515. The corresponding p-values for the percentage change in EPBs as well as the absolute and percentage change in infant mortality are .000892, .00793, and .002228, respectively. In each case, the estimate for Colorado is clearly in the left tail of the distribution, suggesting the decline in Colorado in both outcomes is far greater than would be expected to occur due to the typical fluctuations in these outcomes.

Of course, the 38 counties in Colorado were not randomly selected from across the US, and are all part of a single state, so it is also important to compare the statewide change in Colorado to changes which occurred in other US states. Here, I

Figure 3 — Randomization Inference - Distributions of Placebo Treatment Effects.



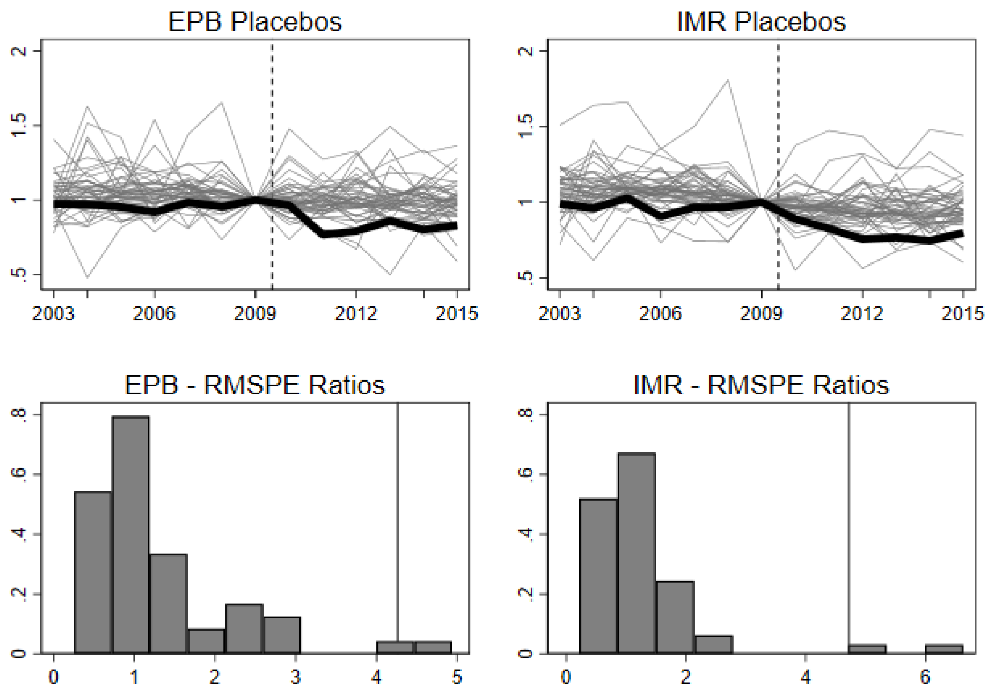
This figure displays distributions of placebo estimates for the treatment effect of the Colorado Family Planning Initiative on extremely preterm births and infant mortality. For each outcome, 1,000,000 different combinations of 38 control counties were randomly selected. For each placebo treatment group, the change in the average rate of both outcomes from 2007-2009 to 2011-2013 was calculated, both in raw terms and as a percentage of the pretreated outcome. The top row of the figure displays the distribution of these estimates for extremely preterm births, while the bottom row displays the estimates for infant mortality. In each case, the treatment effect in Colorado is greater in magnitude than over 99% of the placebos. The p-values, moving from top left to bottom right, are .0035, .0009, .00793, and .0022.

calculate p-values two different ways. First, I calculate them as above and compare the raw and percentage change from 2007-2009 to 2011-2013. For infant mortality, the change in Colorado is the largest in the country in both raw and percentage terms, corresponding to a p-value of .02 (1/50). For extremely preterm births, the change in the average outcome is the third largest behind Vermont and Rhode Island (p=.06) and the second largest in percentage terms (.04) behind Vermont.

Next, because this method includes many states with low populations which have very noisy rates for these outcomes, I look for states which were relatively stable prior to 2009, which then saw large changes around the time of the implementation of the CFPI. Similar to what is done with a synthetic control, I compare the root mean squared predicted error in the pre period versus the post period compared with the 2009 value. Figure 3 displays the evolution of Colorado and the other 49 US states. For both outcomes, Colorado is stable up until 2009, but then displays a large decline which looks more like a treatment effect than any other state. In both cases, the RMSPE ratio for Colorado is the second largest, for a p-value of .04.[19] Regardless of how the randomization inference is conducted, it is clear that the changes which occurred in Colorado around the time of the CFPI were larger than would be expected to have occurred by chance.

[19] For EPB, the only state with a larger ratio was Wyoming, while the only state with a larger ratio for infant deaths was Vermont. Notably, Vermont and Wyoming have the two smallest populations in the country, and thus have rates of infant health outcomes that vary substantially.

Figure 4 — Randomization Inference - Colorado Compared with Other States

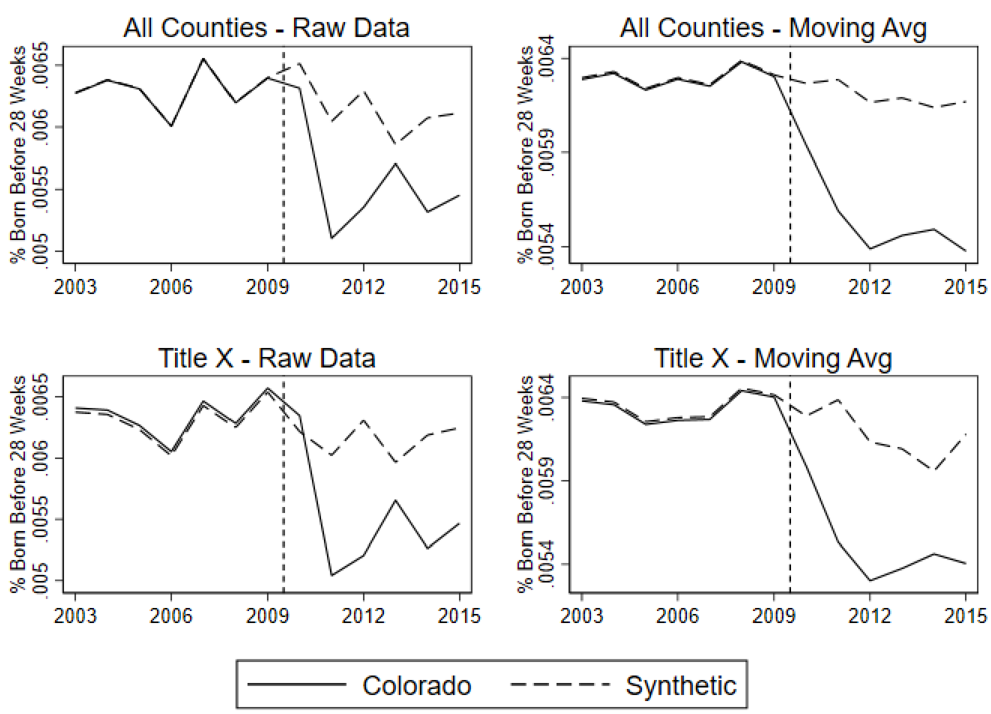


This figure displays results of a randomization inference comparing the change in extremely preterm births and infant mortality in Colorado with every other US state. The top row displays the evolution in each outcome in Colorado relative to its 2009 value in bold, with the same evolution for each other state in grey. The bottom row of the figure displays the distribution of the ratio of root mean squared predicted error from the post period compared with the pre period for each state. In both outcomes, Colorado has the second largest ratio, which translates to a p-value of .04.

C. Synthetic Control and Synthetic Difference-in-Differences

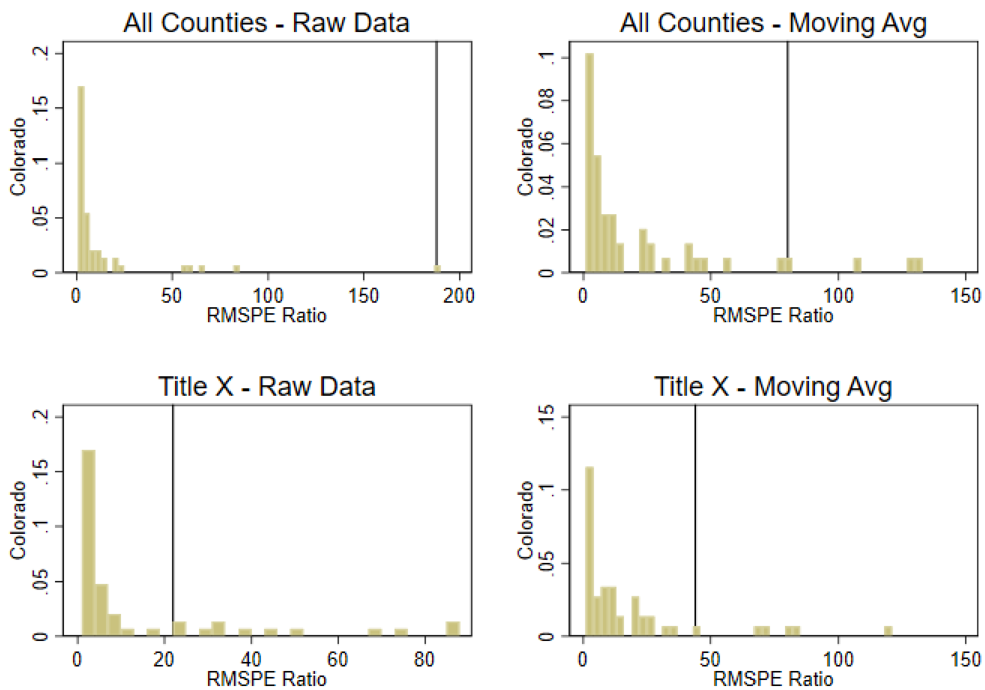
I now reestimate the effect of these LARC interventions using the synthetic control method pioneered by Abadie and Gardeazabal (2003) and Abadie, Diamond, et al. (2010). I estimate the synthetic control specification both on the entire state and on the entire state that remains after dropping all of the counties which do not have a Title X clinic. Additionally, for each treated region, I estimate synthetic controls for both the raw EPB data as well as on three-year moving averages of the rates of EPB. I do this because these rates are inherently noisy, and this can cause synthetic controls to match on idiosyncratic noise rather than on the latent variables which are causing differences in trends.

Figure 5 — Synthetic Control - Colorado - EPB



Note: This figure displays outcomes from applying the SCM to Colorado, estimating the effect of the CFPI on extremely preterm births. The top row estimates the SCM on all counties, while the bottom row only includes counties with a Title X clinic. The left column estimates the SCM on the raw data, while the right column uses a three-year moving average to reduce noise. p-values (moving from top left to bottom right) = .02, .08, .22, .12

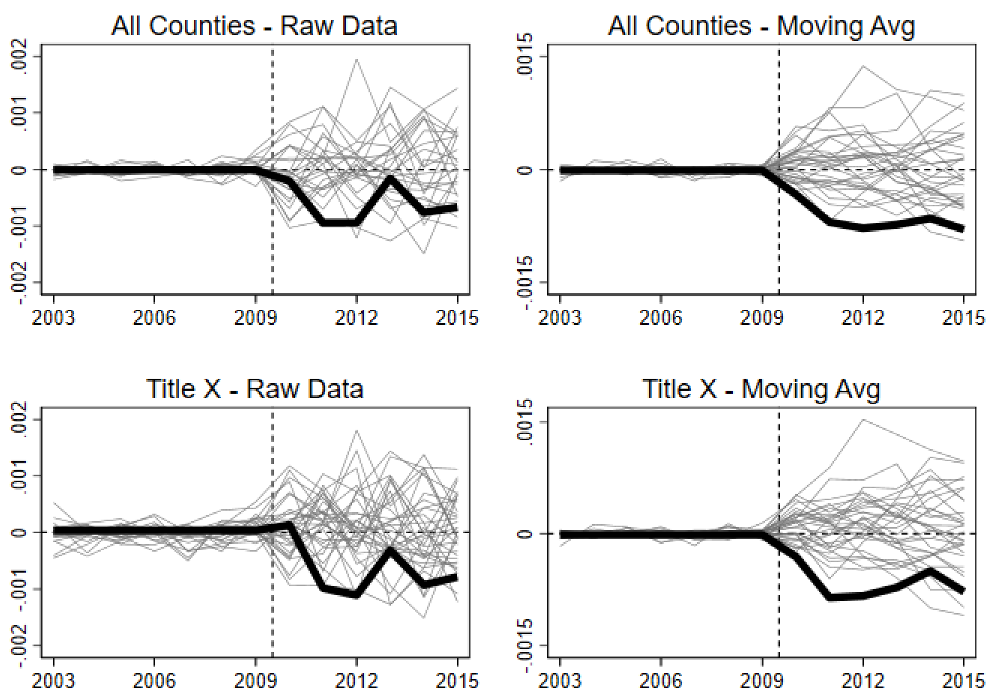
Figure 6 — Synthetic Controls - RMSPE Distributions - Colorado - EPB



Note: This figure displays the distribution of root mean squared predicted error (RMSPE) ratios for Colorado compared with placebo ratios for each of the other 49 states. The top row estimates the SCM on all counties, while the bottom row only includes counties with a Title X clinic.

Appendix Figure 5 displays all four specifications for Colorado, while Appendix Figures 6 and 7 display the standard figures used for inference with the synthetic control method. Beginning with the top left, which estimates the model using all counties in Colorado and raw data instead of moving averages, there is a close match prior to the CFPI. After 2009, there is a slight drop in the first year, but then a large decline in 2011, down 1.3 EPBs per 1,000 births from 2009. There is a slight rebound, but overall there still appears to be a large change in levels of between .5 and 1.0 EPBs per 1,000 births. When compared to the 49 placebo specifications, the ratio of post versus pretreatment root mean squared error for Colorado is the largest, more than double the next highest. Because of the noise that occurs in relatively rare outcomes like this one, the top right panel of Appendix Figure 5 reestimates the same specification on a three-year moving average of the rate of

Figure 7 — Synthetic Controls - RMSPE Distributions - Colorado - EPB

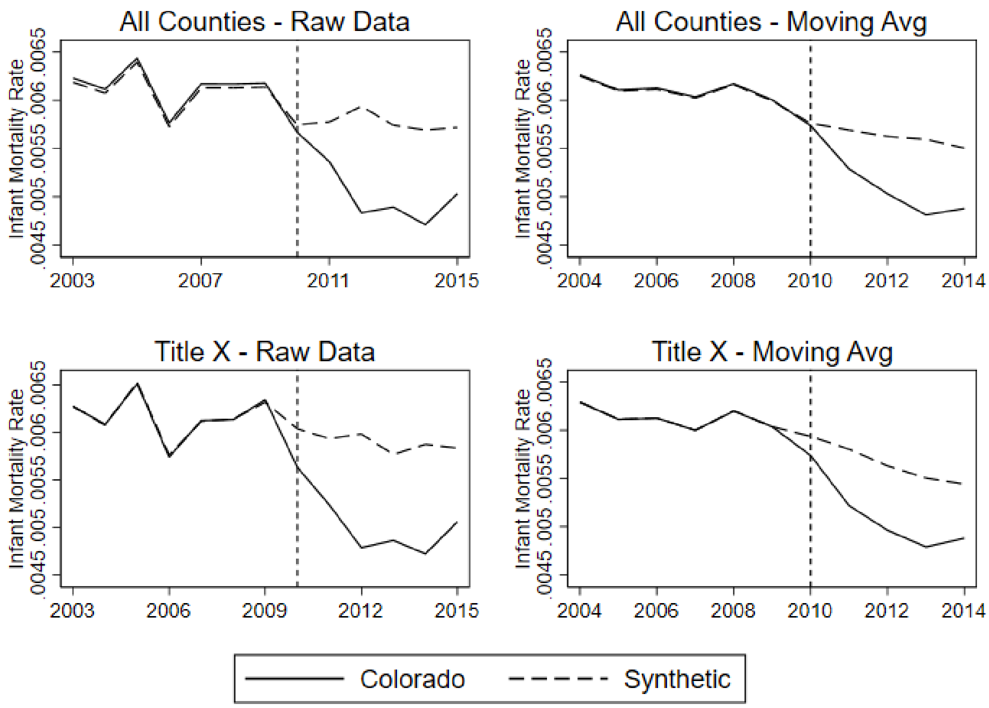


Note: This figure displays the difference between Colorado and synthetic Colorado in black compared with placebo treatment effects for each of the other 49 states. The top row estimates the SCM on all counties, while the bottom row only includes counties with a Title X clinic.

EPB. Now, the change in levels is far more obvious, as there is a decline of about .9 EPBs per 1,000 live births by 2012 which shrinks slightly in the later years as the levels drop in the synthetic control as well.

The bottom left panel again estimates the same specification, this time dropping data from all counties in the United States which did not have a Title X clinic in 2008. In Colorado, around 92% of births occur in counties with such a clinic, so there is not a large difference between the top left and bottom left panels, but the treatment effect is in fact larger in the bottom panel as the rate of EPB declined by 1.5 per 1,000 live births in the bottom panel (compared to 1.3 in the top). This suggests that the reductions were largest in areas with Title X clinics, which is further evidence that the reduction we see was in fact caused by the CFPI. The story is similar in the bottom right graph, which shows the three-year moving average

Figure 8 — Synthetic Control - Colorado - IMR

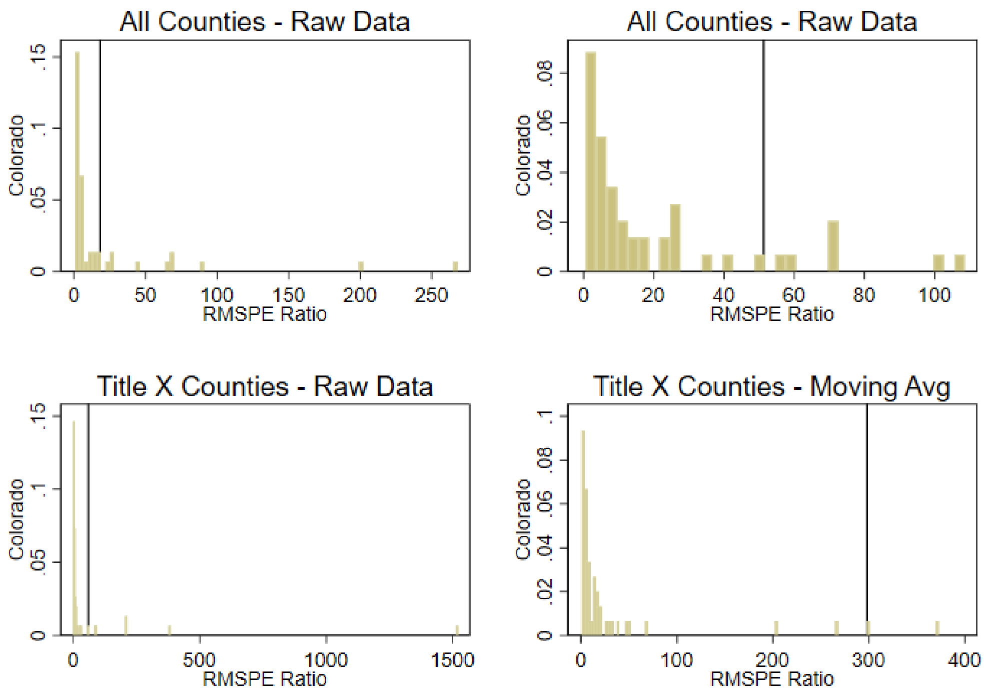


Note: This figure displays outcomes from applying the SCM to Colorado, estimating the effect of the CFPI on the infant mortality rate. The top row estimates the SCM on all counties, while the bottom row only includes counties with a Title X clinic. The left column estimates the SCM on the raw data, while the right column uses a three-year moving average to reduce noise. p-values (moving from top left to bottom right) = .22, .14, .12, .04

for only counties with a Title X clinic. Again, there is a close pretreatment match and then a large decline of over 1.0 EPB between 2009 and 2011. The two main takeaways are that the decline which occurred in Colorado in the years following the CFPI did not occur in other states which had been evolving similarly up to that point, and that the treatment effect is larger in counties with Title X clinics than elsewhere.

Appendix Figure 6 displays histograms of the root mean squared predicted error (RMSPE) ratios for Colorado and the 49 placebos. Although Colorado does not always have the largest ratio, it is always in the tail of the distribution. Finally, Appendix Figure 7 displays the difference over time between Colorado and synthetic

Figure 9 — Synthetic Controls - RMSPE Distributions - Colorado - IMR

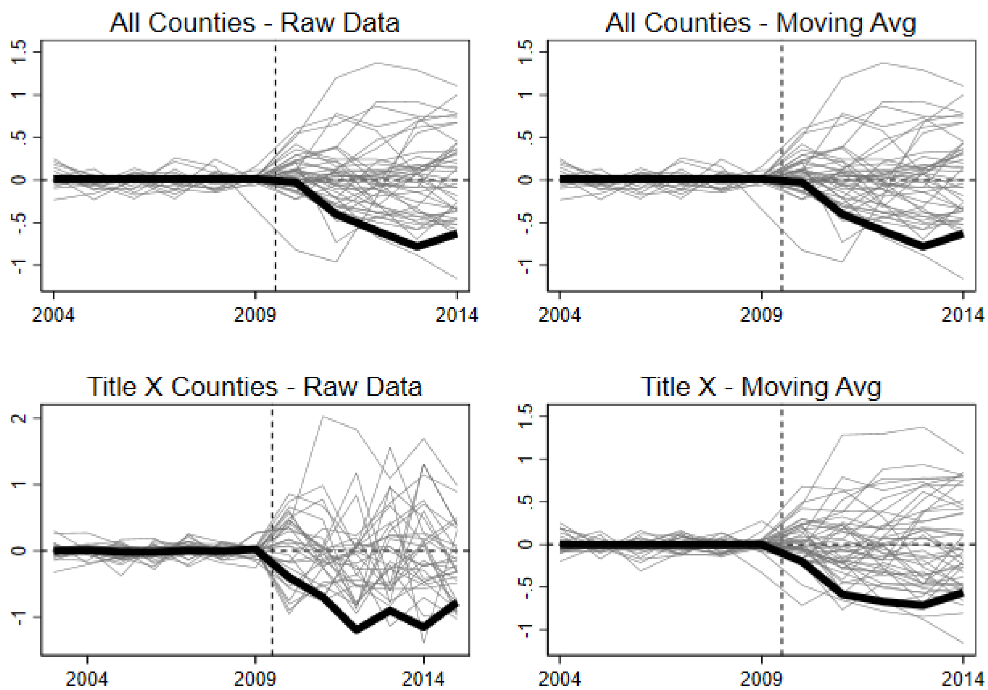


Note: This figure displays the distribution of root mean squared predicted error (RMSPE) ratios for Colorado compared with placebo ratios for each of the other 49 states. The top row estimates the SCM on all counties, while the bottom row only includes counties with a Title X clinic.

Colorado in black, with the difference between each of the 49 placebos and their synthetic version in grey. Although there is the slight rebound in 2013, Colorado consistently shows a larger reduction that almost any other state in this period.

Appendix Figures 8, 9, and 10 repeat this exercise for infant mortality, and the results are similar. In each case, the synthetic version of Colorado closely mirrors the trends in Colorado prior to the CFPI, but then continues along on the previous trend while Colorado drops sharply starting in 2010. Once again, the effect is similar if slightly larger when dropping the non-Title X counties before performing the matching procedure. Although the infant mortality results are slightly less significant than the results for EPBs, this is due more due other states with extremely close pre-treatment matches, which drive up the RMSPE ratio. It is clear from Appendix Figure 9 that Colorado is considerably to the right of the

Figure 10 — Synthetic Controls - Placebo Treatment Effects - EPB



Note: This figure displays the difference over time between Colorado and synthetic Colorado in black compared with placebo treatments effects for each of the other 49 states in grey. The top row estimates the SCM on all counties, while the bottom row only includes counties with a Title X clinic.

main mass of ratios, and Appendix Figure 10 demonstrates that the treatment effect which occurred in Colorado is larger than placebo effects showing up in virtually every other state.

C.1. Synthetic Difference-in-Differences

Finally, because my treated group contains several different counties, I also perform the synthetic difference-in-differences method of Arkhangelsky et al. (2021). Similar to the synthetic control method, this approach creates a control group which is a weighted average of units in the donor pool which most closely matches trends in the treatment group. This method also chooses weight for time periods so that the average posttreatment outcome in the control group differs only by a constant from the outcome before treatment, more closely mirroring the canonical

2x2 difference-in-difference framework.

One drawback of this method, however, is that it does not allow for the use of weighted least squares. This means that counties with only a few births per year carry the same amount of weight as larger counties. While it is important to show that the intervention works outside of the main population centers as well, these smaller counties can have a great deal more variance from year to year. For example, Cheyenne County in Colorado has a population of 1,700 people, with between 20-40 births per year. There were no EPBs in Cheyenne County during the sample except for in 2012, where there were two. This means that the rate of EPBs per 1,000 live births jumped from 0 to 53 in 2012, compared with Denver County, where the rate is between five and nine in each year of the sample. For this reason, I drop counties with less than 50 births per year from the sample before estimating the model. This only includes three counties (Cheyenne, Sedgwick, and Washington Counties), which make up around 100 births per year, or about 0.15% of births in Colorado. Compared to my main specifications, this method now preferentially weights smaller counties in Colorado as long as they average more than 50 births per year. If the intervention only worked in larger counties, the treatment effect would be much smaller, or even nonexistent, in these specifications. The results are reported below in Appendix Table 4, and they are consistent with the main estimates from Section 4. The CFPI is associated with a reduction of 1.262 deaths and 1.106 EPBs per 1,000 live births. Estimates are reported with bootstrapped and placebo standard errors, and in each case the result is significant at least at the 10% level. Finally, Appendix Figure 11 display the evolution of each outcome compared with the synthetic diff-in-diff control group over time. In each case there is a small but consistent difference between the groups leading up the CFPI, followed by a divergence which remains consistently in place throughout the sample period.

D. Avoided Costs from Ventilation Care

Hayman et al. (2015) details the likelihood of receiving medical ventilation as a result of births at various gestational ages, conditioned on whether or not the infant ultimately survives, along with the associated average costs of ventilation for each gestational age/survival cell. The authors use 2009 data from the Agency for Healthcare Research and Quality (AHRQ). My estimates from section 4 imply

Table 7 — Synthetic Difference-in-Differences Estimates

	(1) IMR	(2) IMR	(3) EPB	(4) EPB
LARC Treat	-1.262**	-1.262*	-1.106**	-1.106*
	(0.597)	(0.655)	(0.471)	(0.632)
Standard Errors	Bootstrap	Placebo	Bootstrap	Placebo
Reps	100	100	100	100
Observations	12015	12015	12024	12024

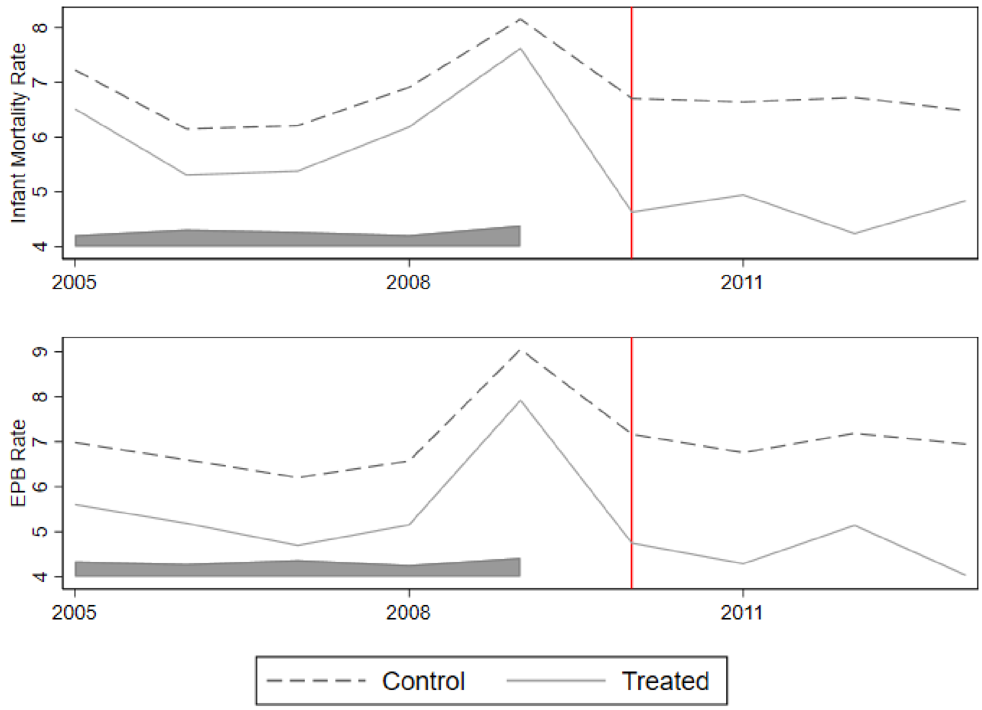
Note: This table displays estimates of the synthetic difference-in-differences method on the rates of both extremely preterm births and infant mortality in treated counties in Colorado. The first column estimates the effect on infant mortality using bootstrapped standard errors, while the second column uses the placebo method. Columns three and four repeat this procedure for extremely preterm births. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

a reduction of around 236 EPBs between 2011 and 2015 in Colorado following the CFPI. Of these EPBs, many would have received ventilation. The likelihood of receiving this care varies both by the gestational age of the birth, and by whether or not the infant ultimately survived.

Because of this, I first demonstrate that the reductions in EPBs associated with the CFPI did not cause large changes in the proportions of EPBs occurring at each gestational age. For example, if the reductions in EPBs were all occurring in births at less than 24 weeks of gestation, this would imply very different cost savings than if they were all occurring at 27 weeks of gestation. Online Appendix Table 1 displays the proportion of EPBs in Colorado which occurred at each gestational length, both before and after the Colorado Family Planning Initiative. Column 3 of this table displays a p-value for whether the proportion of EPBs at each gestational age are the same before and after the CFPI. In each row, equality cannot be rejected at .05, suggesting that the distribution of gestational ages did not change in response to the CFPI. This means that it is reasonable to treat the 236 EPB reduction as if it had the same proportions in each gestational age bracket as all of Colorado.

This assumption allows me to calculate the proportion of the 236 EPBs which would have come from each gestational age group. I then use the estimates from Hayman et al. (2015) to calculate the likelihood of survival, the probability of being ventilated given survival or non-survival, and then the average cost avoided for

Figure 11 — Synthetic Difference-in-Differences Estimates



Note: This figure displays the evolution of infant mortality and extremely preterm births in Colorado counties with a Title X clinic compared with the synthetic difference-in-differences control group, using both bootstrapped and placebo standard errors.

each of the 236 EPBs. Online Appendix Tables 2 and 3 display the estimates of this calculation for the proportion of the 236 EPBs which would be predicted to survive and not survive, respectively. For each gestational age, the total cost avoided, conditional on survival outcome, is equal to:

$$Cost_{age,survival} = N_{age,survival} * P(Vent|age, survival) * VentCost_{age,survival} \tag{2}$$

Where $N_{age,survived}$ is the predicted number of EPBs at that gestational age to survive (or not survive), $P(Vent|age, survival)$ is the probability of being ventilated conditional on being in that gestational age group and survival outcome, and $VentCost_{age,survival}$ is the average cost of ventilation care, also conditional on being in that gestational age group and survival outcome. The total costs avoided due to the CFPI can then be calculated by summing the individual avoided costs across each gestational age/survival cell.

Column 1 of Online Appendix Table 2 distributes the 236 EPB reduction across the gestational age categories based on their proportional occurrence in Colorado from 2005-2009. Column 2 displays the likelihood of survival for an EPB of that gestational age, based on Hayman et al. (2015).[20] Column 3 then calculates the predicted number of counterfactual EPBs which would have survived, given the prevailing survival odds. Column 4 gives the probability of being ventilated, conditional on gestational age and ultimate survival, while column 5 displays the average cost of ventilation for that gestational age, also conditional on survival. Finally, column 6 presents the estimated costs avoided rounded to the nearest hundred, which is the product of columns 3-5, as displayed in equation 2. For surviving EBPs, the predicted cost savings across gestational age categories totals $15,063,000 in total.

Online Appendix Table 3 repeats this exercise for EPBs which would be predicted not to survive. It is notable that the average cost of ventilation is much smaller for an infant who does not survive, as infants who do survive can require ventilation for weeks and even months. The predicted cost savings from EPBs who would be predicted not to survive is only around $531,000, which means the total costs avoided are roughly $15.6 million. Considering the fact that the CFPI only cost $23 million, this means that 67% of the total program cost was made up for in reduced ventilation care of extremely preterm births alone. Of course, this is only one of many potential avoided costs associated with EPBs and infant mortalities. The EPBs who ultimately survived would have likely experienced higher than average medical costs throughout their entire lives, to say nothing of the effect of these traumatic events on the health and well-being of the parents and their friends and family members.

[20] Note that the odds are identical for gestational ages 25-27 because Hayman et al. (2015) group these together in their estimates