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Can Expanding Contraceptive Access Reduce Adverse Infant Health Outcomes?

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Abstract

This paper investigates how expanding access to Long-Acting Reversible Contraceptives (LARCs) leads to positive selection in the health of the cohorts of children being born. I exploit the staggered timing of three privately funded programs which widely distributed LARCs at no cost to mostly lower income women in Colorado, Iowa and St. Louis. I implement an event-study design which compares trends in treated counties with other U.S. counties which had similar family planning clinics offering LARCs but which did not receive additional funding to widely distribute them at no cost to the patient. I find that expanded LARC access led to reductions of approximately 1.0 'extremely preterm' births and 1.1 infant deaths per 1,000 live births. I find significant reductions in infant deaths due to birth defects, maternal pregnancy complications, Sudden Infant Death Syndrome (SIDS), and homicide. These effects only appear in counties with Title X clinics through which the programs were implemented, ruling out the possibility that statewide changes in Iowa and Colorado could be behind them. These results suggest that giving lower income women the autonomy to choose when and if to have children has the potential to reduce adverse infant health outcomes and could decrease the infant mortality gap between the US and other leading economies.

JEL codes: J13, I18, I12
Keywords: Contraceptive access; Infant mortality; Preterm birth; Family planning

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1. Introduction

In 2019, the United States had a higher infant mortality rate (IMR) than 32 of the 37 other OECD countries. At 5.8 deaths per 1,000 live births, over three times as many US infants die in their first year of life compared with Japan, which has an IMR of 1.9 (OECD (2019)). One possible explanation is the high rate of unintended pregnancies in the US, which are linked with delayed initiation of prenatal care, (Kost et al. (2015)) low birthweight (Slemming et al. (2016), Sable et al. (1997), Flower et al. (2013)), and neonatal mortality (Bustan et al. (1994)). While other countries with much lower rates of infant mortality have similar rates of unintended pregnancy,[1] the US may be an outlier in which groups are experiencing unintended pregnancies. According to Sonfield et al. (2014), poor women in the United States are more than five times as likely to have an unintended pregnancy than higher income women, with barriers to contraceptive access playing an important role.

Long-acting reversible contraceptives (LARCs), such as intrauterine devices (IUDs) and subdermal hormonal implants, are the most effective form of reversible contraception available today (Curtis et al. (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)). 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. Finding an answer to this question is complicated, however, by the fact that states and counties with higher levels of contraceptive access also differ in many other important ways that are related to maternal and infant health outcomes.

In this paper, I get around this endogeneity by using the rollout of three privately-funded family planning programs as a natural experiment. These programs each gave thousands of LARCs to mostly low-income women at no cost in Colorado, Iowa and St. Louis. Using restricted-access natality data from the National Vital Statistics System (NVSS), which links birth certificates to infant death records, I

[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.

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 this additional funding.

I find that expanded LARC access led to a reduction of just over 1.0 extremely preterm births (EPBs), which are births before 28 weeks of gestation, and 1.1 infant deaths per 1,000 live births across the three treated regions. I supplement the main analysis by estimating synthetic control specifications on each treated area separately, showing that large reductions in both adverse outcomes appear in each treated region approximately one to two years after the LARC interventions were initiated. I find significant reductions in infant mortalities due to birth defects, maternal pregnancy complications, Sudden Infant Death Syndrome (SIDS), and homicide, all of which are correlated with socioeconomic status. Additionally, I show that in Colorado and Iowa, these reductions only appear in counties with a nearby Title X clinic, where the LARC interventions were implemented, ruling out the possibility that statewide policy changes could be behind the reductions I find. Using a back of the envelope calculation, I find that the program in Colorado[2] cost approximately $108,000 per infant death avoided, which suggests that there is considerable scope for the United States to close the infant mortality gap with the rest of the OECD by expanding access to LARCs to low-income women throughout the US.

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[3] find evidence that access to abortion reduces infant mortality, with Grossman et al. (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). 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 suggests that without LARC access, a substantial number of women with an unintended pregnancy would have chosen not to terminate their pregnancy and eventually would have suffered an infant death or an extremely preterm birth. This paper also

[2]The program in Colorado is the only one for which cost data is available

[3]Pabayo et al. (2020), Corman et al. (1985), Joyce (1987) and Gruber et al. (1999)

builds on a growing literature which documents the effects of expanding access to LARCs specifically. The programs I study in this paper have been shown to reduce unintended pregnancies (McNicholas et al. (2014)), abortions (Biggs et al. (2015), Ricketts et al. (2014)) and the teen birth rate (Lindo et al. (2017), Kelly et al. (2020)), while increasing female educational attainment (Stevenson et al. (2021)). I build on these findings by demonstrating that increased LARC access also led to a reduction in adverse infant health outcomes.

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 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 et al. (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. A substantial literature, starting with Donohue and Levitt (2001)[4], tracks birth cohorts subject to legal abortion into adulthood, finding that criminality among these groups was substantially less than that of cohorts not exposed to legal abortion and that women exposed to abortion in utero were less likely to get pregnant as a teenager themselves (Donohue, Grogger, et al. (2009)). In this paper, I document a similar compositional impact, whereby allowing lower-income women to opt out of unwanted or unplanned pregnancies reduces the likelihood of adverse outcomes like preterm births and

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

infant deaths.

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 et al. (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. 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 et al. (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 began having children later. This paper highlights the importance of which types of women a contraceptive technology is made available to. 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 shifts the composition of births and whether the children born to women with this access will be healthier than their counterparts. LARC methods have become increasingly popular in recent years, with the American College of Obstetricians and Gynecologists recommending them as a first-line option for most women seeking to avoid pregnancy (ACOG (2009)). Because of the high upfront costs, LARCs are difficult for low-income women to afford, even if they are cost effective in the long run. The programs I study in this paper, however, focus on expanding LARC access to low-income women specifically, so there is potential for them to give these women the same economic freedom that more upwardly mobile women attained with the emergence of the pill.

This means these programs 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 better off and more capable of investing in their children. These programs have been shown to have many important benefits for the women using them, but relatively little is known about their effect on the health of the infants born to women with expanded LARC access. Because births to young, low-income women are more likely to result in a preterm birth or an infant death (Larson (2007), Finlay et al. (2011)), it seems plausible that these programs could reduce these outcomes, but to my knowledge no current work has drawn a causal link between programs that expand access to LARCs to low-income women and infant health outcomes.

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 et al. (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)). This 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 prevent many unwanted pregnancies. LARC users are also generally satisfied with their choice of contraception, with Peipert et al. (2011) finding that LARC users have higher rates of satisfaction than oral contraceptive users (80% compared with 54%) and are more likely to continue using them beyond a year (86% compared with 55%). Similarly, Dinerman et al. (1995) find that LARC users are more likely to continue use after six months than users of oral contraceptives, and are at no greater risk of sexually transmitted infections.

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. One reason is information. Russo et al. (2013) documents a series of pervasive myths about LARC use, including that they cause disease, infertility, menstrual irregularities, weight gain, acne and hair loss. Blumenthal et al. (2010) and McNicholas et al. (2014) both demonstrate that when women receive information about LARCs from a doctor, they become more likely to request them. There are also supply side issues, where not all healthcare providers have the equipment or training necessary to insert LARCs. Bornstein et al. (2018) document that 21% of health centers have no staff trained in LARC methods, and that almost half (48%) of health centers do not offer IUDs or implants onsite, with these clinics concentrated in rural areas. Arguably the biggest impediment to LARC use, however, is the high upfront cost of up to $800 (CDPHE (2017)). Even though LARCs are cost effective for most users in the long-run[5], many women, particularly low-income 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. Henke et al. (2020) find that cost is the most frequently cited barrier to LARC usage among low-income women, with 53% of low-income women surveyed claiming it was a barrier, compared to only 32% of higher income women. Burke et al. (2020) shows that 22% of women in 2015-2017 had unsatisfied preferences for contraceptive methods, with Black and Hispanic women and lower income women being the mostly likely to desire a different method and cost being listed as the most common barrier to access. Potter, Hubert, et al. (2016) find that among women interviewed six months postpartum, two thirds had experienced a barrier to accessing their preferred method of contraception, while Potter, Hopkins, et al. (2014) find that 34% of postpartum women using less effective methods would prefer to be using LARCs. 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 born to women with this improved access. I now describe the three large-scale programs which were rolled out with

[5] 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.

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 (CFPI) with the goal of reducing unintended pregnancies in Colorado by increasing the number of family planning clients served and by increasing access to LARC methods[6]. Although Colorado already had an extensive network of Title X clinics, which receive federal funding to provide individuals with family planning and preventative health services, in 2007 it was reported that 40% of all pregnancies and 60% of pregnancies to 15-24 year old mothers were unintended. To combat this alarming trend, an anonymous donor gave over $27 million dollars to provide free LARCs to low-income women through Title X clinics in Colorado. The money went directly to local clinics and was mainly spent on purchasing LARCs, contracting with local providers for LARC placements, hiring and training new staff, improving clinic infrastructure, and increasing clinic hours.

The guidelines for Title X clinics require that all contraceptive methods, counseling, education and exam fees be incorporated into a sliding scale which charges clients based on their income levels. All contraceptive services for clients at or below 200 percent of the federal poverty line are provided at no cost. Since most Title X clients have incomes below this threshold, the majority of contraceptive services are provided at no cost. Prior to the CFPI, the high upfront costs of LARC methods made it difficult for Title X clinics to keep up with demand, and many clinics had long waiting lists for LARCs.

While very few LARC's had been inserted in Colorado Title X clinics prior to the CFPI, by the end of 2009 there had been almost 2,000 insertions, and this number grew in each subsequent year. In each year from 2010-2014 between 4,000-6,000 LARCs were inserted at Title X clinics in Colorado, so that just under 30,000 had been given out by 2014, which translates to approximately one LARC per 24 women aged 15-35 in Colorado. In 2013, over 24% of Colorado teens visiting Title X clinics were LARC users, the highest rate of any state in the U.S. At the time, over 40 states had less than 10% of their Title X clients using LARCs, according the the

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

Title X Family Planning Annual Report of 2013 (NFPRHA (2013)). 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 et al. (2017)), indicating that the CFPI made a significant difference for young, low-income women in Colorado.

2.4. *Iowa Initiative to Reduce Unintended Pregnancies*

In 2007, The Iowa Initiative to Reduce Unintended Pregnancies (IIRUP) was launched. The IIRUP was a privately funded campaign aimed at reducing unintended pregnancies among women aged 18 to 30[7]. Similar to the CFPI, the IIRUP was implemented through Title X family planning agencies, which operated 81 clinical sites across 46 of Iowa's 99 counties. The funding was used to expand hours and locations, train clinic staff on how to talk about LARCs with patients, and purchase IUDs and implants which the clinics had previously been unable to afford. After receiving funding, all of Iowa's Title X agencies began offering both the hormonal implant and two forms of IUDs (hormonal and copper).

LARC takeup increased dramatically in response to the Initiative. While only 1,047 Title X clients were using a LARC method in 2006, that number ballooned up to 10,092 by 2009 as 15% of all Title X clients were LARC users in 2009. Estimating a causal impact of the IIRUP on health outcomes is complicated by the fact that abortion access also increased in Iowa at the same time, with medication abortion via telemedicine becoming available in 2008. Biggs et al. (2015) demonstrate, however, that abortion in Iowa actually declined from a rate of 8.7 per 1,000 reproductive-age women in 2005 to 6.7 in 2012, so it seems more likely that any effects we see in this period are due to the IIRUP as opposed to the increased access to abortion.

2.5. *St. Louis Contraceptive CHOICE Project*

Also in 2007, researchers based at Washington University in St. Louis launched the St. Louis Contraceptive CHOICE Project (SLCCP) in order to study the contraceptive choices women make when cost and access barriers are removed and they

[7] My discussion of the Iowa Initiative to Reduce Unintended Pregnancies and the St. Louis Contraceptive CHOICE Project draw heavily from Strasser et al. (2016), McNicholas et al. (2014), Birgisson et al. (2015) and Biggs et al. (2015)

are educated about the benefits of different contraceptive methods. The privately-funded study enrolled over 9,000 women aged 14-45 in the St. Louis metropolitan area who had been sexually active in the past six months or planned to be sexually active in the next six months, wanted to avoid pregnancy for at least a year and were interested in trying a new form of contraception. The women were all read a script describing LARC methods, were counseled on the full range of contraceptive methods available and were screened for STIs. Once the participant chose a contraceptive method and it was approved by a phyisican, she received it at no cost for up to three years, and was allowed to change methods at any point. 75% of the participants chose a LARC method, which means that approximately 7,000 LARCs were inserted between 2007-2011, and the rates of teen pregnancy and abortion for women in the study were both four times lower than the national average.

While these three different initiatives had many differences in the populations they were serving and the scale and scope of their operations, they had several important characteristics in common that are useful for the purposes of this study. First, they all reduced the cost barrier of LARC methods to low-income women by providing LARC insertions free of charge. In response to each program there was a dramatic uptick in the number of LARCs being used. The CFPI was the largest and most successful of the three initiatives, so we may expect to find larger effect sizes in Colorado, but if LARC access has a causal impact on infant health we should expect to see improvements in all three areas.

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.

3.1. Data

This paper uses data from several sources. Data on both preterm births 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 2002-2015. 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.

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.

Because not all counties in Colorado and Iowa have Title X clinics through which the LARC interventions were implemented, I define a county in these two states as treated if it had a Title X clinic in 2008. This clinic assignment was gathered by Lindo et al. (2017), with Colorado counties identified based on clinic addresses in the Colorado Department of Public Health and Environment's Directory of Family Planning Services. Clinics in other states were identified by geocoding the addresses of Title X clinics listed in the US Department of Health and Human Service's 340B Database. My event-study specifications will thus compare trends in infant health in treated counties (counties in Colorado and Iowa with Title X clinics as well as St. Louis county) with other counties in the U.S. which have similar Title X family planning clinics but which were not given additional funding specifically for a LARC program. Additionally, because infant mortality declined in states which expanded Medicaid after the passage of the Affordable Care Act of 2010 (Bhatt et al. (2018)) relative to states which did not, I only include counties in the 39 states which expanded Medicaid as possible control counties since Colorado, Iowa and Missouri all expanded Medicaid.

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 that 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 unemployment rates from the Bureau of Labor Statistics. Finally, 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 et al. (2017) using data collected from the National Conference of State Legislatures (2012), the National Women's Law Center (2012), and Zuppann (2011).

3.2. Methodology

I estimate the effect of expanding LARC access on infant health outcomes through two primary methodologies. First, I use event-study specifications of the form:

$$Y_{ct} = \sum_{k=-4}^{4} \theta_k LARC_{c,t+k} + \beta X_{ct} + \alpha_c + \gamma_t + \psi_i * t + \epsilon_{ct} \tag{1}$$

in order to estimate the joint effect of all three programs. Here, Y_{ct} measures the rate of a specific health outcome, either the rate of EPBs or the infant mortality rate, for county c in year t. $LARC$ is an indicator for a county being treated with a LARC intervention at some point during the sample period, while k measures the years before and after the intervention took place. Therefore, $\theta_{-4} - \theta_{-1}$ estimate differences in trends between treated and control counties before the LARC interventions went into effect and $\theta_1 - \theta_4$ measure the impact of the policies. If the LARC interventions had a causal impact on infant health outcomes, we should expect $\theta_{-4} - \theta_{-1}$ to be close to zero and statistically insignificant, while $\theta_1 - \theta_4$ should be negative and significant. X_{ct} includes a vector of control variables that 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 ntaionwide trends in infant health across time. ψ_{c*t} is a county-specific linear time trend, which I include to prevent preexisting differences in trends between treated and control counties from being picked up as a treatment effect. I estimate this specification using weighted-least-squares, where the weights are determined by the total number of births in a county-year cell.

There are two reasons why I expect the effect of LARC access to increase over time. First, the policies continued over a period of several years, and we would expect their effects to be cumulative. Taking Colorado as an example, there were only about 2,000 LARCs inserted via the CFPI in 2009, but an additional 4,200 were inserted in 2010 and then between 5,000-7,000 were inserted in each subsequent year until 2015. Since women who received a LARC in 2010 were able to keep it for up to 10 years, the total stock of women protected by a LARC was increasing over time. Additionally, because of the unpredictable timing of sexual activity, even after LARCs are inserted we would not expect to see an immediate change in the number of unwanted births. In the counterfactual world without expanded LARC access, many unprotected women would still not get pregnant and even those that do would be unlikely to get pregnant right away. Therefore, the number of births that were avoided in each year would be increasing over time[8], as would any effects this has on infant health outcomes.

I include all counties which had a clinic where free LARCs were distributed as treated. This includes 38 counties in Colorado and 46 counties in Iowa which had Title X clinics through which the CFPI and IIRUP were implemented, as well as St. Louis county. I drop all counties in Iowa and 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. 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)).

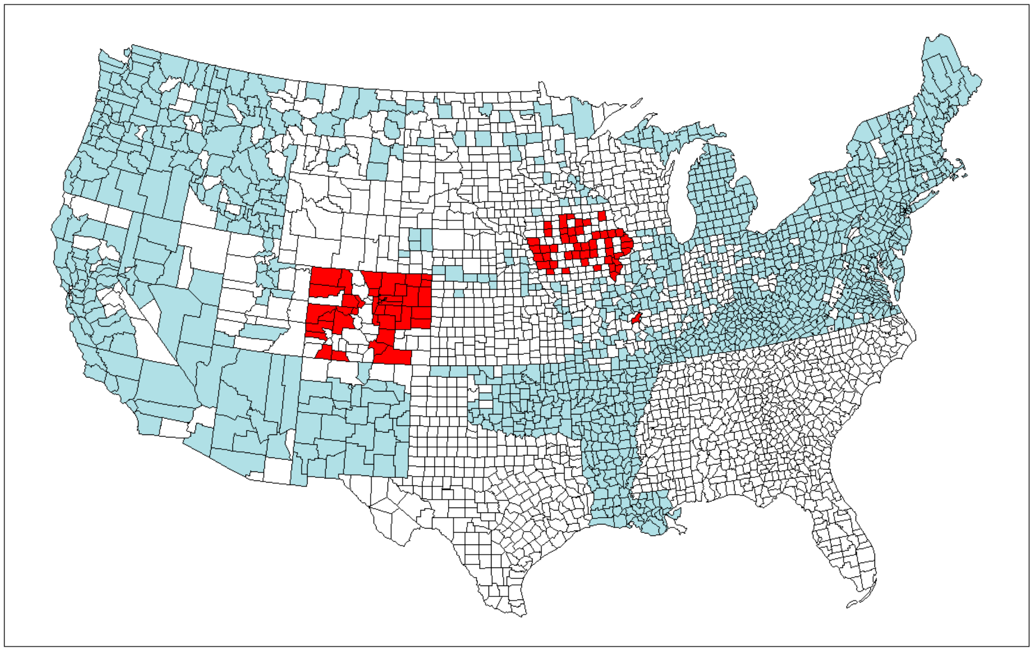
In choosing control counties, I begin with all counties which also have a Title X clinic but which did not receive additional funding for LARCs. I then exclude all counties in the 12 states[9] which did not expand medicaid following the passage of the Affordable Care Act of 2010. Overall, this approach results in 85 treated counties and 1,291 control counties. Figure 1 displays treated counties in red and

[8] This is consistent with the findings of Lindo et al. (2017) and Kelly et al. (2020) which find virtually no impact of the CFPI in 2009, and then a small decrease in 2010 which increases in 2011 and 2012

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

control counties in light blue.

Figure 1 — County Map - Treated and Control Counties



Note: This figure displays treated counties (red) and other counties with Title XI counties which are in states that expanded medicaid and do not border treated counties (blue).

I supplement the event-study specifications by separately estimating the synthetic control method (SCM) of Abadie and Gardeazabal (2003) and Abadie, Diamond, et al. (2010) separately on each treated region in the Appendix. The SCM constructs a control group which is a weighted average of all the possible controls, where the non-negative weights are determined by minimizing the sum of squared pretreatment differences between the treated group and the synthetic control. This approach has both benefits and drawbacks which, overall, complement the event-study specifications nicely. The biggest benefit is that the synthetic control approach chooses a specific control group which most closely matches the treatment group on the pre-treatment outcome, instead of including all Title X counties which are in medicaid expansion states and which are not bordering a treated county. This allows for the construction of a more plausible counterfactual against which to compare the treated group. I estimate state-level synthetic controls on both Colorado and Iowa and a county-level synthetic control on St.

Louis.

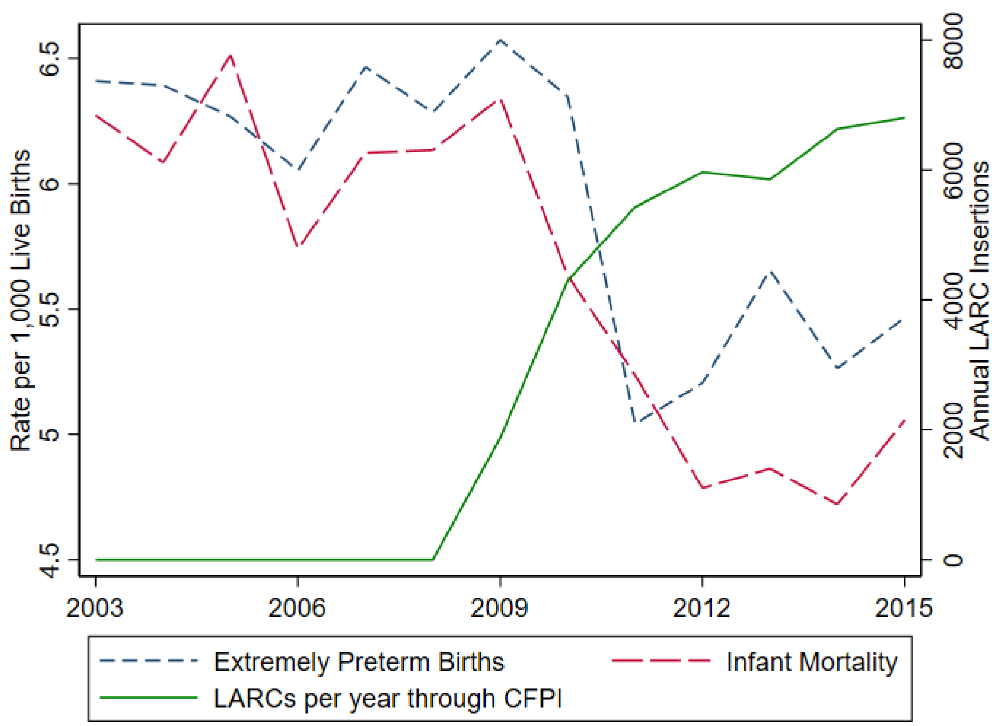
One drawback of the state-level models is that since many counties in both Iowa and Colorado do not have Title X clinics, this approach essentially includes many untreated or only partially treated areas as treated. This will result in an understatement of the overall treatment effect and will bias my estimates toward zero. For both Colorado and Iowa, I correct for this issue by both running the SCM on the entire state and then again by first dropping all non-Title X counties from my sample before estimating. This will remove this bias and also serve as a falsification test, as when the untreated counties are removed, the treatment effects should at least stay as large, if not increase. If they were to decline after this procedure it would raise concerns that the effects I am picking up are from some other factor not related to the LARC interventions. Additionally, because the outcomes I am tracking are rare and somewhat noisy and because it is important for the SCM to match treated and control groups based on underlying trends and not on idiosyncratic noise, I also estimate the SCM on each group using a three-year moving average of the outcome of interest, which removes a substantial amount of noise without compromising the trends occurring in the data. In all cases, the economic inference is similar.

4. Results

This section details my estimates of the effect of expanded LARC access on both EPBs and overall infant mortality. Figure 2 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 happenned 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 each, or around 16% of pre-CFPI levels. In the remainder of this section I will argue that this relationship is a causal impact of the CFPI. First, I will focus on the EPB outcome and show that it occurred not just in Colorado but also in St. Louis and Iowa after similar LARC interventions and that it cannot be explained by changing demographics, economic indicators, policy changes or pre-existing trends.

Figure 2 — CFPI and Infant Health - 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.

4.1. Extremely Preterm Births

Table 1 displays estimates of the event-study specification outlined in equation (1), with coefficients detailing the changing rates of EPB across the three treated regions for three years before and four years after the LARC interventions were initiated. This means that for St. Louis and Iowa, pretreated estimates are displayed for 2004-2006 while postreatment estimates are displayed for 2008-2011. Likewise, for Colorado the pretreated estimates are for 2006-2008 while the postreatment estimates are for 2010-2013. The top panel of Table 1 displays the estimates on the pretreatment leads while the bottom panel displays estimates for the postreatment lags. The first thing to notice is that while all of the estimates in the top panel are insignificant at the 10% level and take on both positive and negative values, all of the posttreamtment lag coefficients are negative and all but two of the estimates for years two through four are significant at 5%.

Column one includes all three treated regions but does not include any controls beyond county and year fixed effects. The pretreatment leads are all negative and insignificant, which suggests EPBs may actually have been rising very slightly in the LARC-treated areas prior to the interventions. Still, the average difference is only 0.24 EPBs per 1,000 births and the p-value from a test that the average effect is zero is .3612. 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 four, with each of these estimates significant at 5%. In column two I add county-specific linear trends to control for pre-existing patterns in the treated counties. If anything, these trends were biasing the estimates in column one towards zero. Each of the pretreated leads is now smaller in magnitude and less significant, with an average effect of just 0.15. Each of the posttreatment lags, on the other hand, is now larger and more significant with an average effect in years two through four of -1.72 EPBs per 1.000 live births. In columns three I add the demographic and economic controls, while in column four I include the two policy controls, and the story is roughly the same.

Of course, this standard two-way fixed effects (TWFE) model has come under scrutiny recently when treatments are staggered (Goodman-Bacon (2021), Callaway et al. (2021), Sun et al. (2021), Borusyak et al. (2021)), especially when there is potential for heterogeneous treatment effects over time. One of the main concerns is that later treated observations will be compared with earlier treated units whose treatment effects have been growing over time. This can even cause the parameter of interest to flip signs in certain situations leading to flawed inference. Although the treatment effects in this specification are staggered, these concerns do not present a serious threat to identification here because there are 1,331 untreated counties and only 85 treated counties, meaning the vast majority of 2x2

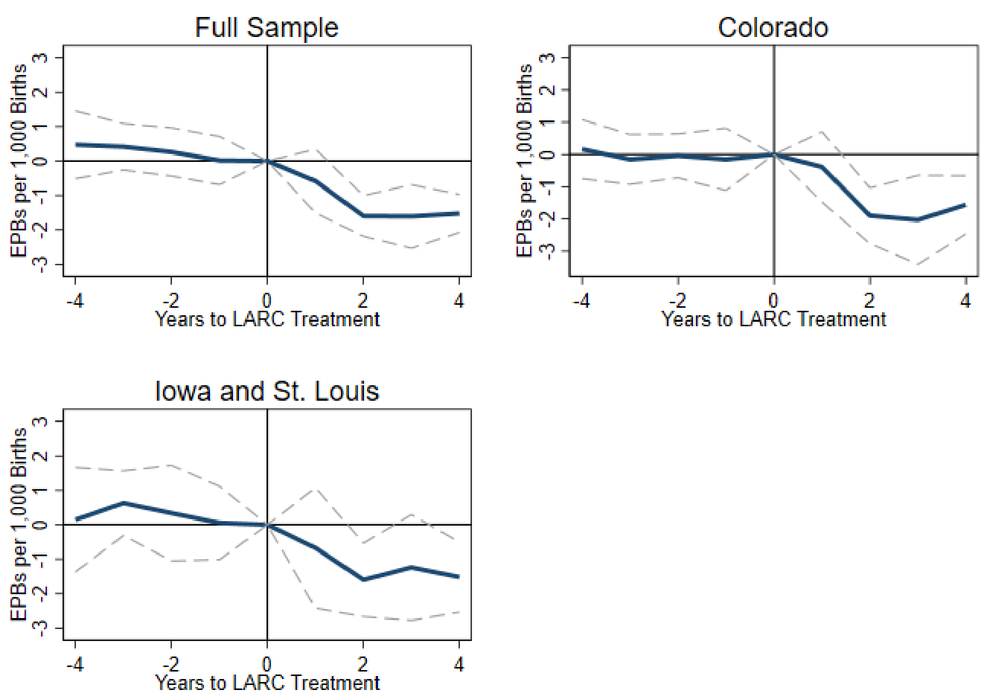
Table 1 — Event-Study Specifications - LARC Treated vs. Control

	(1) EPB	(2) EPB	(3) EPB	(4) EPB	(5) EPB	(6) EPB
3 Years Before	-0.305	0.285	0.504	0.652	-0.101	0.748
	(0.344)	(0.335)	(0.385)	(0.393)	(0.504)	(0.681)
2 Years Before	-0.203	0.193	0.374	0.466	0.0377	0.463
	(0.307)	(0.348)	(0.370)	(0.376)	(0.416)	(0.780)
1 Year Before	-0.219	-0.0235	0.0618	0.101	-0.147	0.0364
	(0.345)	(0.355)	(0.361)	(0.362)	(0.491)	(0.571)
Avg pretreated effect	-.242	.151	.313	.406	-.070	.416
p-value (avg effect = 0)	.3620	.5820	.3004	.1875	.8580	.4684
1 Year After	-0.351	-0.551	-0.616	-0.652	-0.393	-0.754
	(0.470)	(0.475)	(0.488)	(0.485)	(0.580)	(0.905)
2 Years After	-1.375^{***}	-1.775^{***}	-1.892^{***}	-1.776^{***}	-2.047^{***}	-1.709^{**}
	(0.263)	(0.282)	(0.316)	(0.339)	(0.505)	(0.620)
3 Years After	-1.157^{**}	-1.756^{***}	-1.959^{***}	-1.880^{***}	-2.178^{**}	-1.468
	(0.448)	(0.457)	(0.510)	(0.526)	(0.774)	(0.921)
4 Years After	-0.844^{**}	-1.637^{***}	-1.925^{***}	-1.885^{***}	-1.751^{**}	-1.914^{*}
	(0.280)	(0.264)	(0.371)	(0.393)	(0.640)	(0.752)
Avg effect years 2-4	-1.125	-1.723	-1.925	-1.847	-1.992	-1.697
p-value (avg effect = 0)	.0001	.0000	.0000	.0000	.0006	.0141
Ratio of pre-post effect	4.65	11.41	6.15	4.55	28.46	4.09
County and year FE's	Y	Y	Y	Y	Y	Y
County linear trends	N	Y	Y	Y	Y	Y
Main controls	N	N	Y	Y	Y	Y
Policy controls	N	N	N	Y	Y	Y
Only Colorado	N	N	N	N	Y	N
Only Iowa/St. Louis	N	N	N	N	N	Y
Observations	15510	15510	15510	15510	12267	12348

Note: Standard errors in parentheses, clustered at the county level. This table displays estimates of the effect of LARC interventions on the rate of extremely preterm births per 1,000 live births. Column one estimates the standard two-way fixed effects (TWFE) specification. Column two adds county-specific linear trends. Column three add demographic and economic controls. Column 4 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. Columns five and six address concerns about staggered treatment timing by estimating the model separately based on when the intervention took place. Column five includes only Colorado as treated, while column six includes only St. Louis and Iowa. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

comparisons are between treated units and never-treated controls. The Goodman-Bacon (2021) decomposition of the TWFE specification is presented in Appendix Table 1, with the accompanying plot as Appendix Figure 1. 99.3% of the weight in the specification is from comparisons of treated units versus never-treated units, while only 0.4% of the weight is from the problematic later treated vs. earlier treated comparison group. Even though this comparison does bias the difference-in-difference estimate towards zero, it bears so little weight in the regression that it effectively makes no impact.

Figure 3 — Event-Study - Extremely Preterm Births



Note: This figure uses natality data from the National Vital Statistics System to plot coefficients from the event-study specification comparing preterm birth rates in the three treated areas compared with other counties with Title X clinics which are in states that expanded medicaid and do not border treated counties.

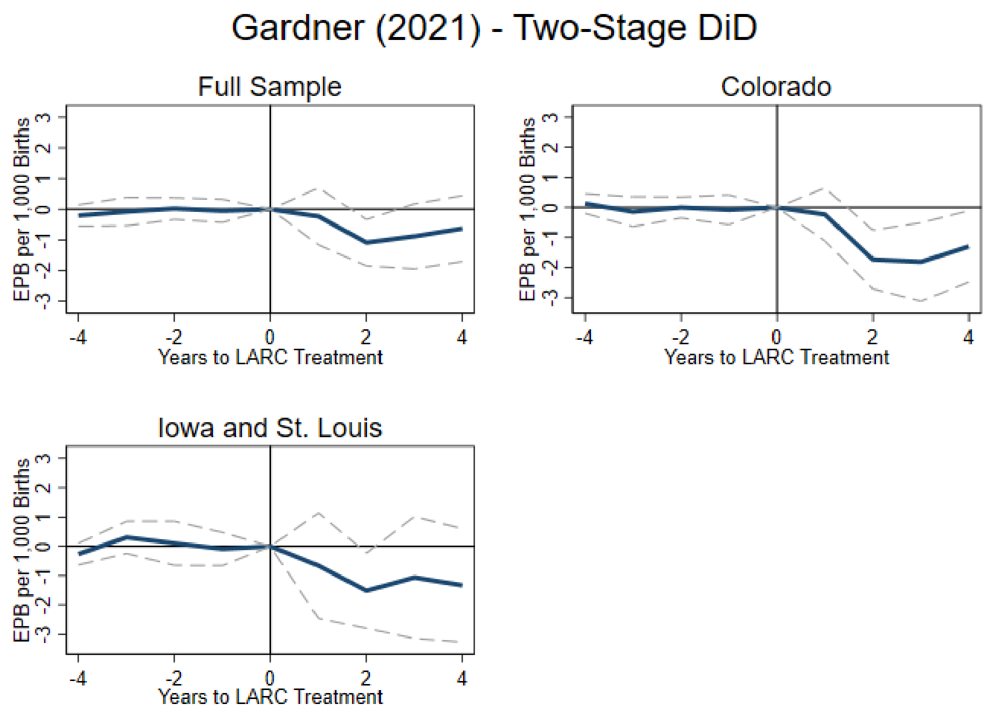
Columns five and six further address these concerns by removing the staggered component of the treatment and estimating the effects separately based on when the initiatives began. Column five estimates equation (1) on Colorado alone, and there is no concern over pretreatment trends in this specification. The leads are all small in magnitude and insignificant, and they bounce back and forth around zero. The lag on the first year after treatment is again negative but insignificant, while the lags on years two through four are all larger than in the previous columns and individually significant at 1%. In column six it becomes clear that the potentially concerning pretrends occurred in the St. Louis and Iowa sample, which was treated in 2007. Pretreatment leads decline from .65 to .41 and then .01 before remaining steady in 2007 and then declining much further. Still, the difference in the posttreated years is much larger than the changes taking place beforehand.

What is clear from Table 1 is that EPBs dropped substantially in the second through fourth year after treatment across all three interventions. To illustrate this, Figure 3 displays the coefficient estimates for columns 4-6 of Table 1 with 95% confidence intervals. In both the full sample and Iowa and St. Louis graphs, there is a slight pretrend leading up to treatment, but the much larger decline that occurs after the interventions still suggests that the expanded LARC access played a role in reducing EPBs. The Colorado graph, on the other hand, can stand alone. The pretreated outcomes track very closely with the control group before the CFPI and then a large reduction occurs in the second year after treatment which stays around 2 EPBs per 1,000 live births for each of years two, three and four.

Figure 4 displays estimates from columns 4-6 once again, only reestimated using Gardner (2021)'s two-stage difference-in-difference estimator, which is robust to heterogeneous treatment effects with staggered timing. Here, because year and county fixed effects could be contaminated by the treatment effect, these effects are all estimated in a first stage using untreated observations to get year and county fixed effects[10]. The first stage is then residualized and regressed on the leads and lags, resulting in fixed effects that are uncontaminated and parameter estimates which are robust to heterogeneous treatment timing. In Figure 4, the parallel trends assumption looks even more plausible in both the full sample and Iowa and St. Louis graphs, suggesting that perhaps the fixed effects had been contaminated by the treatment effect in these groups. In each version there is little to no movement in the pretreatment period, followed by a small decline in the first year after treatment and then a larger, statistically significant decline in the second

[10]In other words, year fixed effects are estimated from the full group of never treated observations, while county fixed effects for treated observations are estimated using only the pretreated observations from these groups.

Figure 4 — Event-Study - Two Stage DiD (Gardner (2021))



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 preterm birth rates in the three treated areas compared with other counties with Title X clinics which are in states that expanded medicaid and do not border treated counties.

period after treatment.

4.2. *Infant Mortality*

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 three years before the interventions, and then there are large declines concentrated in years two through four following treatment. In the baseline two-way fixed effects model, IMR actually appears to be increasing in the treated areas relative to the control counties in the years before treatment, with that trend reversing after the LARC interventions began. Including county linear trends and demographic, economic and policy controls

Table 2 — IMR Event-Study - LARC Treated vs. Control

	(1)	(2)	(3)	(4)	(5)	(6)
	IMR	IMR	IMR	IMR	IMR	IMR
3 Years Before	-0.982*	-0.323	-0.119	-0.0171	-0.984	0.0161
	(0.416)	(0.451)	(0.448)	(0.452)	(0.631)	(0.645)
2 Years Before	-0.587*	-0.147	0.0177	0.0944	-0.526	0.0529
	(0.284)	(0.316)	(0.319)	(0.321)	(0.413)	(0.568)
1 Year Before	-0.504	-0.284	-0.224	-0.203	-0.423	-0.400
	(0.355)	(0.381)	(0.379)	(0.380)	(0.546)	(0.531)
Avg pretreated effect	-.691	-.251	-.108	-.042	-.644	.110
p-value (avg effect = 0)	.0179	.4362	.7349	.8966	.1342	.8259
1 Year After	-0.547	-0.766	-0.846*	-0.870*	-0.606	-0.786
	(0.415)	(0.417)	(0.427)	(0.426)	(0.509)	(0.826)
2 Years After	-0.970**	-1.410***	-1.548***	-1.568***	-1.136	-1.566**
	(0.353)	(0.370)	(0.385)	(0.390)	(0.664)	(0.517)
3 Years After	-1.438***	-2.096***	-2.324***	-2.370***	-1.592**	-2.429***
	(0.397)	(0.360)	(0.395)	(0.405)	(0.584)	(0.699)
4 Years After	-1.079**	-1.956***	-2.244***	-2.324***	-1.433*	-2.182***
	(0.370)	(0.353)	(0.403)	(0.409)	(0.691)	(0.635)
Avg effect years 2-4	-1.162	-1.821	-2.039	-2.087	-1.387	-2.059
p-value (avg effect = 0)	.0003	.0000	.0000	.0000	.0131	.0004
Ratio of pre-post effect	1.68	7.25	18.88	49.69	2.15	18.72
County and year FE's	Y	Y	Y	Y	Y	Y
County linear trends	N	Y	Y	Y	Y	Y
Main controls	N	N	Y	Y	Y	Y
Policy controls	N	N	N	Y	Y	Y
Only Colorado	N	N	N	N	Y	N
Only Iowa/St. Louis	N	N	N	N	N	Y
Observations	15554	15554	15544	15544	12293	12374

Note: Standard errors in parentheses, clustered at the county level. This table displays estimates of the effect of LARC interventions 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 county-specific linear trends. Column three add demographic and economic controls. Column 4 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. Columns five and six address concerns about staggered treatment timing by estimating the model separately based on when the intervention took place. Column five includes only Colorado as treated, while column six includes only St. Louis and Iowa. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

both reduce the pre-treatment differences and increases the post-treatment effect. In Colorado, the pretrends are somewhat concerning, though as in the basic TWFE model they appear to actually be increasing prior to treatment, and there is still a large decrease of 1.6 infant deaths per 1,000 live births by year three. The results are even larger when looking at only St. Louis and Iowa, with almost no movement prior to treatment and a large decline of between 1.5 and 2.5 infant deaths per 1,000 live births in year two through four.

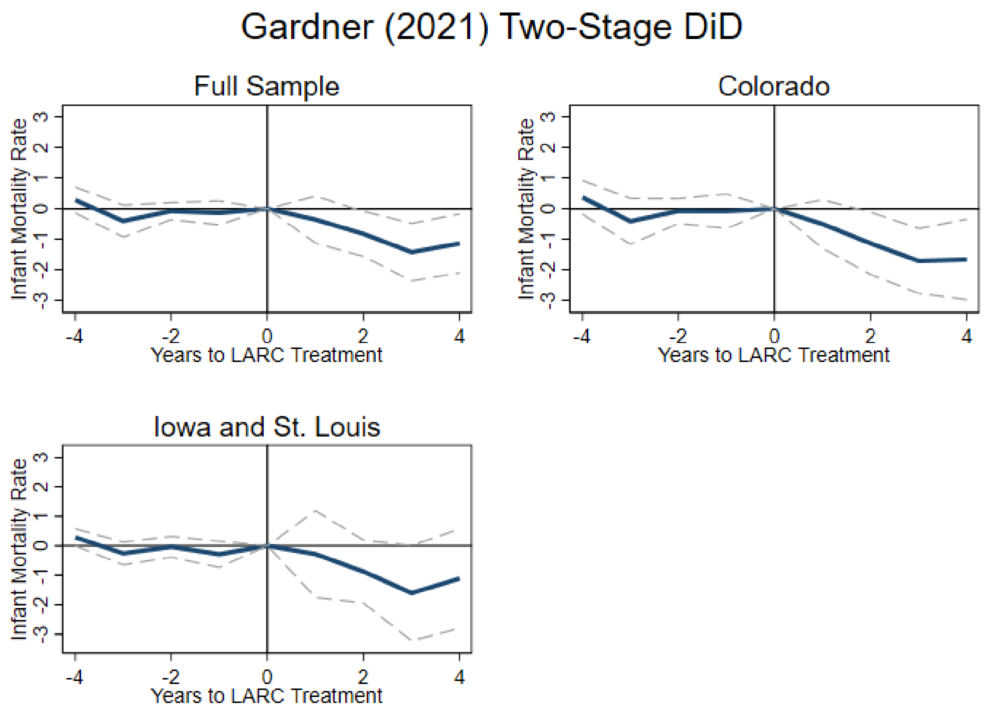
Figure 5 display estimates of the same model, only this time using Gardner (2021)'s two-stage difference-in-difference estimator. As before, the overall effect sizes are now somewhat smaller, but the pre-treatment trends look considerably more stable. For the full sample, there is an average treatment effect of 1.1 infant death per 1,000 live births for years two through four after treatment, with similar effects showing up when the samples are run separately to avoid concerns over the staggered treatment timing.

4.3. Infant Mortality by Cause of Death

Figure 6 displays coefficient estimates from the full-specified TWFE model separately for each of the six most commonly listed causes of death in the NVSS data. Large decreases appear for deaths due to birth defects, SIDS, maternal pregnancy complications and homicide. Deaths from prematurity and low birth weight were actually declining throughout the period, which may seem odd because the event studies looking at EPBs showed no imbalance prior to treatment. This discrepancy is due to two reasons. First, the vast majority of EPB cases measured in the previous section survived and therefore are not included in this figure. Second, almost half of deaths from births prior to 28 weeks are actually caused by birth defects or maternal pregnancy complications and not because of prematurity.

Deaths from injuries do not respond to LARC treatment, which is comforting as it seems unlikely that they would be impacted. Low socioeconomic status is associated with higher rates of SIDS (Athanasakis E (2011)), birth defects (Yang et al. (2007)) and maternal pregnancy complications (Kim et al. (2018)), so it makes sense that expanding LARC access to low-income women might improve these outcomes. Additionally, unwanted births have been shown to increase the risk of violence to the mother (Roberts et al. (2014)) and prolong the mothers contact with the father (Mauldon et al. (2015)), so it also makes sense that there would

Figure 5 — Event-Study - Infant Mortality Rate



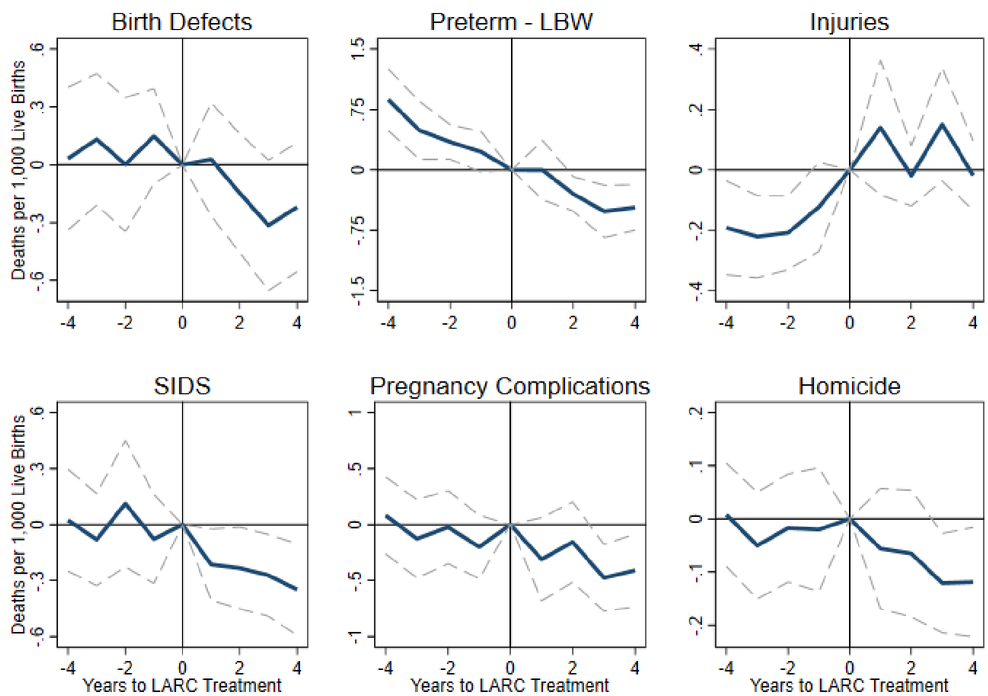
Note: This figure uses natality data from the National Vital Statistics System to plot coefficients from the event-study specification comparing infant mortality rates in the three treated areas compared with other counties with Title X clinics which are in states that expanded medicaid and do not border treated counties using Gardner (2021)'s two-stage difference-in-difference estimator

be a reduction in the number of homicides in response to the LARC interventions assessed in this paper.

4.3.1 Where are These Improvements Happening?

So far, it has been established that large, statistically significant declines in EPBs and infant mortality occurred in the regions treated with a LARC intervention. 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 programs were implemented. In this section, I compare counts of EPBs and infant mortalities in treated versus untreated counties in Colorado and Iowa, in order to rule out any statewide policies which could have impacted

Figure 6 — Event-Study - Causes of Infant Mortality

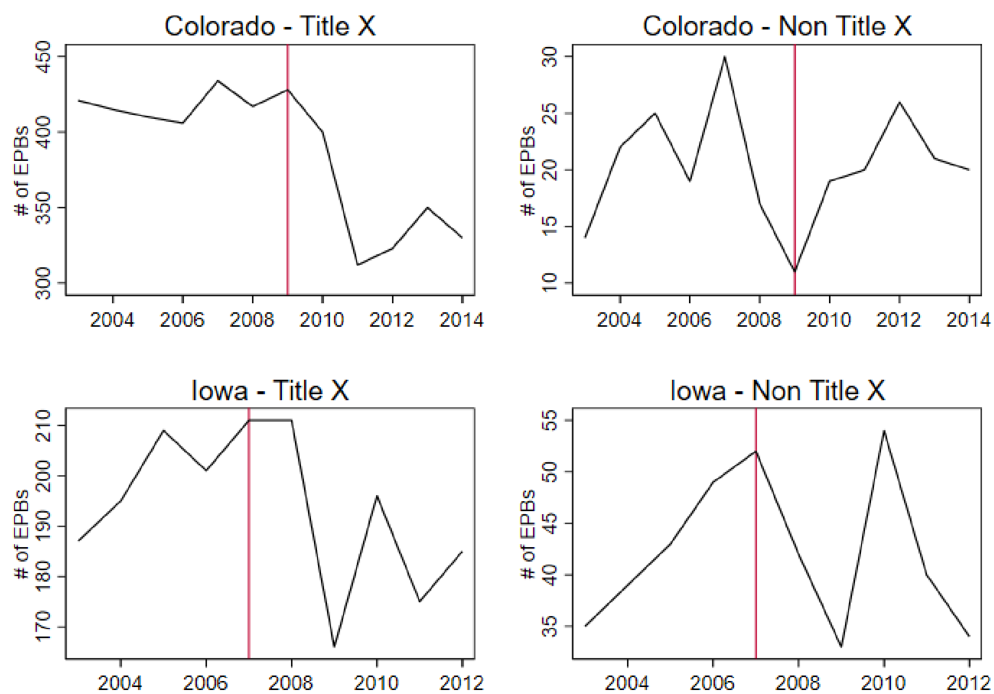


Note: This figure uses natality data from the National Vital Statistics System to plot coefficients from event-study specification for each of the six most common causes of infant death

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 7 displays the raw number of EPB cases for both Colorado and Iowa, broken out by whether or not they were born to a resident of a county with a Title X clinic. The top right graph displays the EPB count over time for counties with a Title X clinic in Colorado. As over 90% of births occur to such women, it is perhaps not surprising that the overall shape of the graph in the top right panel looks similar to the trends for Colorado overall. From 2003 to 2009, the count hovers between 400 and 450. The rate drops slightly in 2010 before declining in 2011 and then remaining between 300 and 350 for the remainder of the sample. The count in counties without a Title X clinic tell a very different story, fluctuating

Figure 7 — Preterm Birth Outcomes in Colorado and Iowa by Title X Status

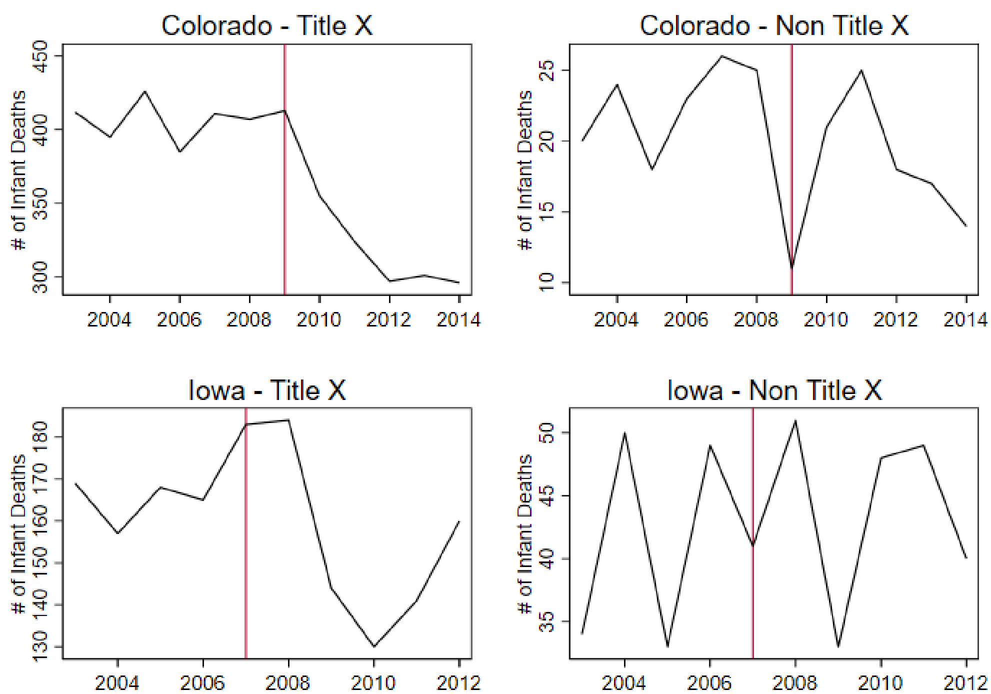


Note: This figure displays the raw number of extremely preterm birth cases in Colorado and Iowa counties with and without a Title X clinic. Graphs on the left display the outcome for Title X counties, while graphs on the right display the non-Title X counties. The top row displays outcomes for Colorado,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

apparently at random throughout much of the sample period and actually rising from 2009-2012 when EPBs were falling throughout the treated counties.

The story is similar for Iowa, with rising EPB counts from 2003 through 2008, before a dramatic decline in which the count dropped from 210 to just under 170 before rebounding slightly. Non Title X counties also show a decline around this time, but this looks similar to the noise which occurred throughout the sample and does not necessarily look like a treatment effect. The rebound in non Title X counties is also much larger, and brings the total in 2010 to a point even higher than it was before the IIRUP. Additionally, it is worth pointing out that counties in Iowa are much smaller on average than counties in Colorado, so a resident of an untreated county in Iowa would not have to travel nearly as far to get to a treated county as a resident of an untreated county in Colorado might. This could explain why the contrast between treated and untreated counties is not as clear in Iowa as it is in Colorado. Still, whatever caused the dramatic decline in EPBs in Colorado and Iowa was mainly happening in the counties which had Title X clinics, which is consistent with the hypothesis that expanded LARC access was the driving force behind the reduction.

Figure 8 — Infant Mortality in Colorado and Iowa by Title X Status

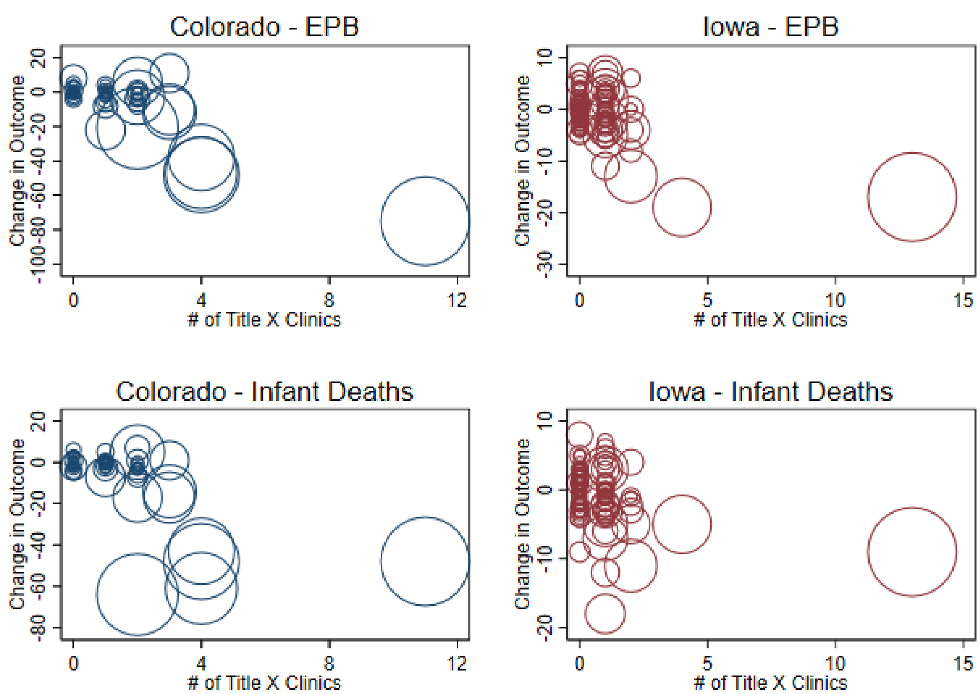


Note: This figure displays the raw number of infant mortality cases in Colorado and Iowa counties with and without a Title X clinic. Graphs on the left display the outcome for Title X counties, while graphs on the right display the non-Title X counties. The top row displays outcomes for Colorado, while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

Figure 8 repeats this process for infant mortality counts, and the results tell roughly the same story. Counts for Colorado Title X counties hover around 400 from 2003 to 2009, before declining each subsequent until 2012, where the count settles around 300 per year. In non Title X counties in Colorado, counts actually reach a minimum in 2009, before rebounding back up to pre-CFPI levels. There appears to be a clear treatment effect in Colorado Title X counties, but not in non Title X counties.

For Iowa, infant mortalities also rise from 2004 to 2008 before declining from around 180 in 2008 to 130 in 2010. Again there is a slight rebound, but infant mortality cases are still far less common in Title X counties in the years following the IIRUP than in the years preceding it. For non Title X counties, there is again no clear treatment effect. Counts fluctuate apparently at random from 2003 to 2010 and do not appear to be meaningfully effect by the IIRUP.

Figure 9 — Change in Outcomes by of TitleX Clinics - Iowa and Colorado



Note: This figure displays the change in the average number of EPB's and infant deaths from the four years leading up to a LARC intervention to the four years after the intervention for counties in Colorado and Iowa compared with the number of Title X clinics in that county. The circles for each county are weighted by the average number of births in the county across all sample years. Blue circles represent Colorado counties, while red circles represent Iowa counties.

Finally, Figure 9 displays the change in average number of EPBs and infant deahts per year from the four years before to the four years after a LARC intervention for Colorado and Iowa counties compared with the number of Title X clinics in that county. Each observation is weighted by the average number of births per year. As both the change in the outcomes and the number of clinics are highly correlated with population, it is not surprising that the counties with the most clinics saw the largest declines, but it is comforting that there appears to be a dose response, where more clinics typically translates to a larger decline, even among relatively similarly sized circles. The negative relationship is particularly clear for EPB's, where large declines occur in the population centers of Denver and Des Moines. Cedar Rapids, with four Title X clinics, has an even larger decline than Des Moines. For infant deaths, the relationship is less obvious, but still shows that more populated areas with multiple clinics had the largest improvements. Overall, it appears that the declining rates of both EPB and infant mortality occurred mainly in areas which had the most access to LARCs through the CFPI and IIRUP.

5. Conclusion

This paper uses the implementation of three separate 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 a preterm birth or an infant death, improving their ability to avoid unwanted pregnancies has the potential to improve the overall health of the cohorts that are born to women with this expanded access. By looking at the Colorado Family Planning Initiative, the St. Louis Contraceptive CHOICE project, and the Iowa Initiative to Reduce Unintended Pregnancies and using both event-study and synthetic control research designs, I demonstrate that expanded LARC access in these regions led to reductions in both the rates of 'extremely preterm' births and overall infant mortality.

The largest results appeared in Colorado, which also had the largest LARC initiative. Using the event-study estimates for Colorado, the CFPI appears to have reduced infant mortality by 1.4 infant deaths per 1,000 live births for 2011-2013. With a back-of-the envelope calculation, this translates to 61 avoided infant deaths in 2011, 86 in 2012, and 103 in 2013, for a total of 250 avoided infant deaths in the first four years after the CFPI was implemented. If avoiding infant deaths were the entire goal of the CFPI, it would have cost approximately $108,000 per infant death avoided. The CFPI has been shown to have numerous other benefits, however, as prior research has shown that it reduced teen births and increased female high school graduation rates. This study builds on this literature by demonstrating that LARC interventions have the ability to reduce adverse infant health outcomes as well. States looking to reduce unintended pregnancies and improve overall infant health should consider adopting similar policies to the three assessed in this paper.

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A. Appendix Figures

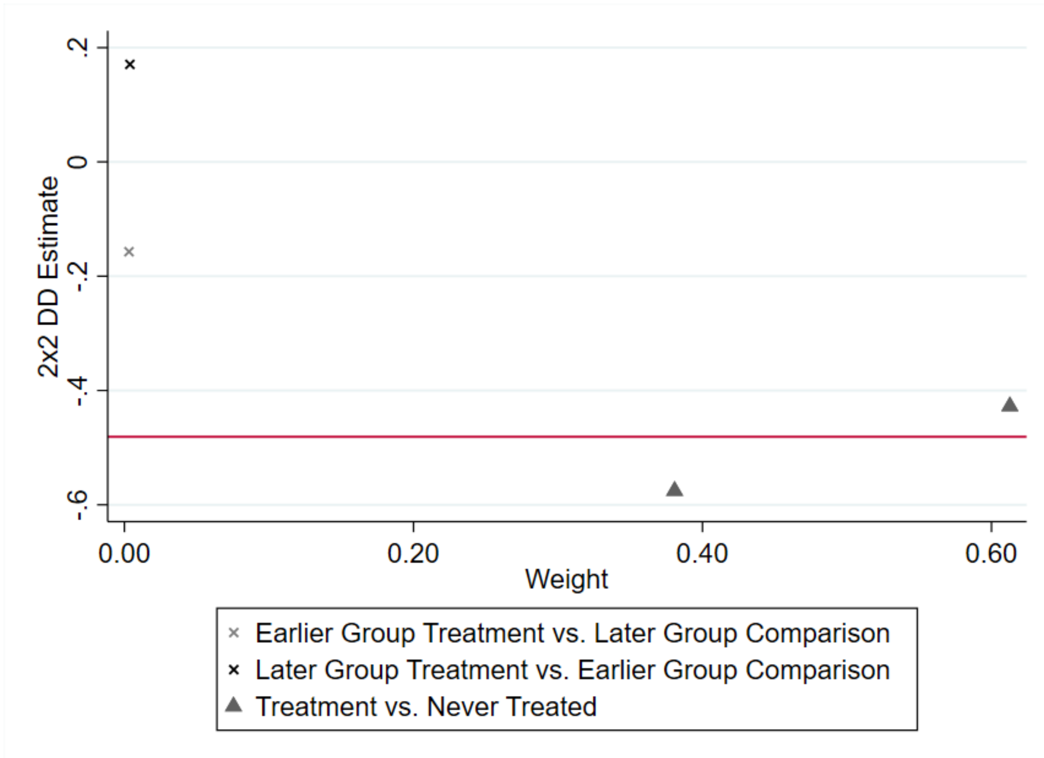
Table 1 — Goodman-Bacon Decomposition Table - TWFE Model

DD Comparison	Weight	Avg DD Est
Earlier T vs. Later C	.003	-.157
Later T vs. Earlier C	.004	.171
T vs. Never-treated	.993	-.484

T = Treatment; C = Comparison

Notes: This table was created using Goldring (2019)'s ddtiming package in Stata.

Figure 1 — Goodman-Bacon Decomposition - TWFE Specification



Note: This figure was created using Goldring (2019)'s ddtiming package in Stata.

B. Synthetic Controls

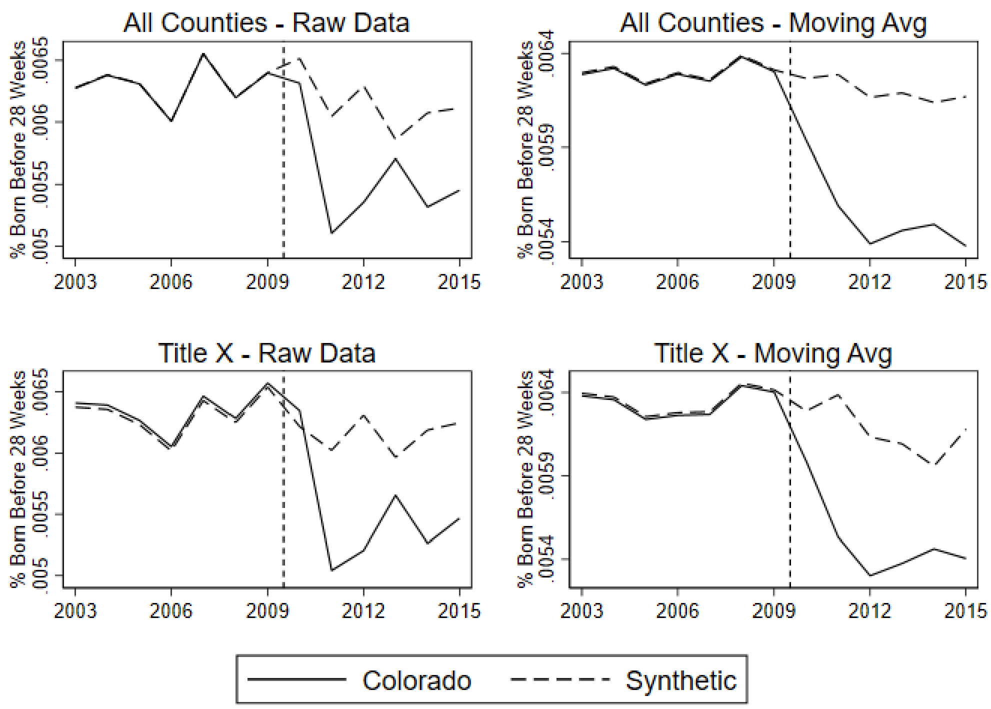
B.1. Extremely Preterm Births

In order to get a better understanding of the separate effect of each intervention, and to more adequately address the concerning pretrends which appeared in the Iowa and St. Louis specification, 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). For both Colorado and Iowa, where some counties were treated and some were not, 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. If the LARC interventions were the reason behind the decline we saw in Table 1, then we should see larger impacts when the specifications only include Title X counties. 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 than on the latent variables which are causing differences in trends.

Appendix Figure 2 displays all four specifications for Colorado, while Appendix Figures 3 and 4 display the standard graphs for inference with synthetic control specifications. 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 Figure 2 reestimates the same specification on a three-year moving average of the rate of 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

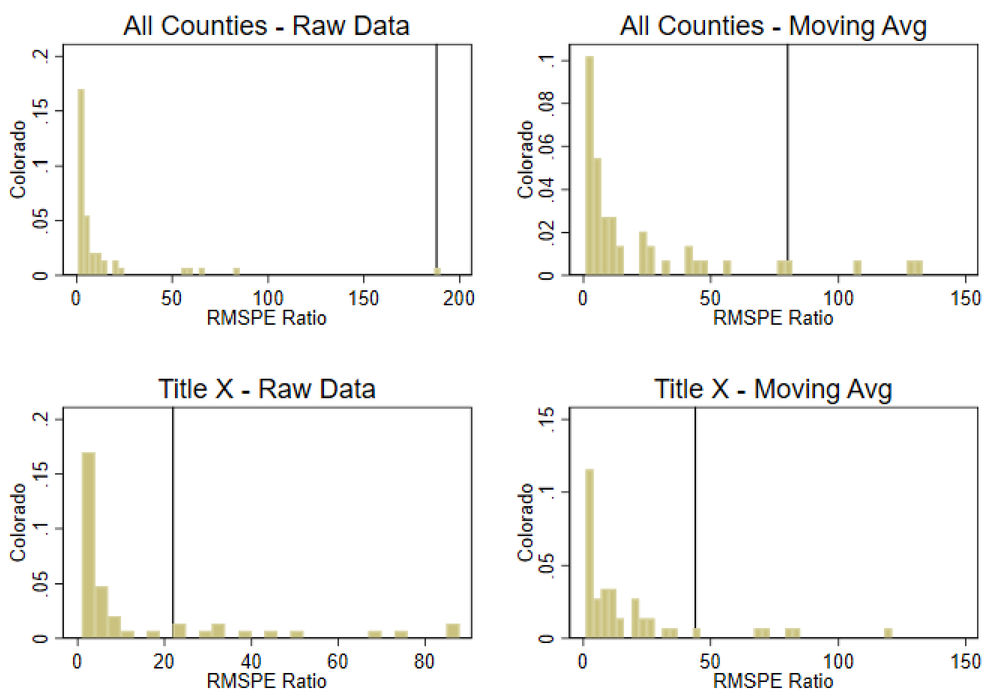
Figure 2 — Synthetic Control - Colorado



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) = .00, .06, .22, .1

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 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

Figure 3 — Synthetic Controls - RMSPE Distributions - Colorado

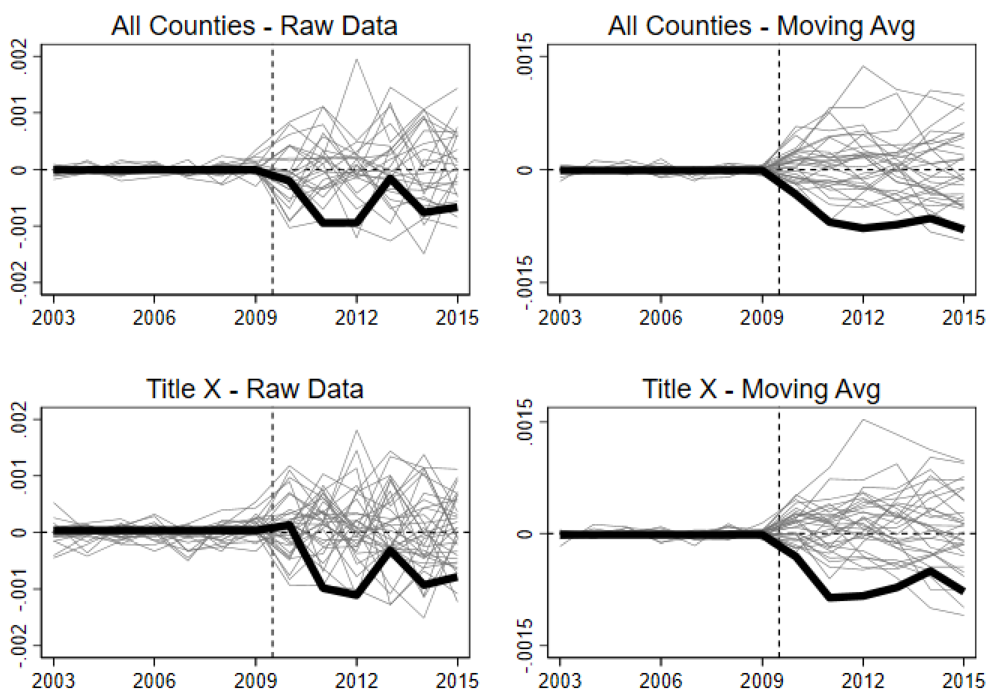


Note: This figure displays the distribution of root mean squared predicted error (RMSPE) ratios for Colorado compared with placebo ratios for each of the other 49 states. The top row estimates the SCM on all counties, while the bottom row only includes counties with a Title X clinic.

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 5 repeats this exercise for Iowa, and the results are similar though not quite as compelling. In all four graphs, there is a close pretreatment match between treated counties and their synthetic control. In 2009, in the raw data, there is a large decline of about 1.2 EPBs per 1,000 live births, followed by a rebound in 2010. This mirrors the experience of Colorado, where there was a large decline in EPB in 2011, two years after the CFPI went into effect, followed by a smaller rebound. While the initial decline appears equally large for both the Title X and non Title X counties, the rebound is much larger in the top left, suggesting that this rebound effect was stronger in the untreated Iowa counties. Because of

Figure 4 — Synthetic Controls - Placebo Treatment Effects - Colorado



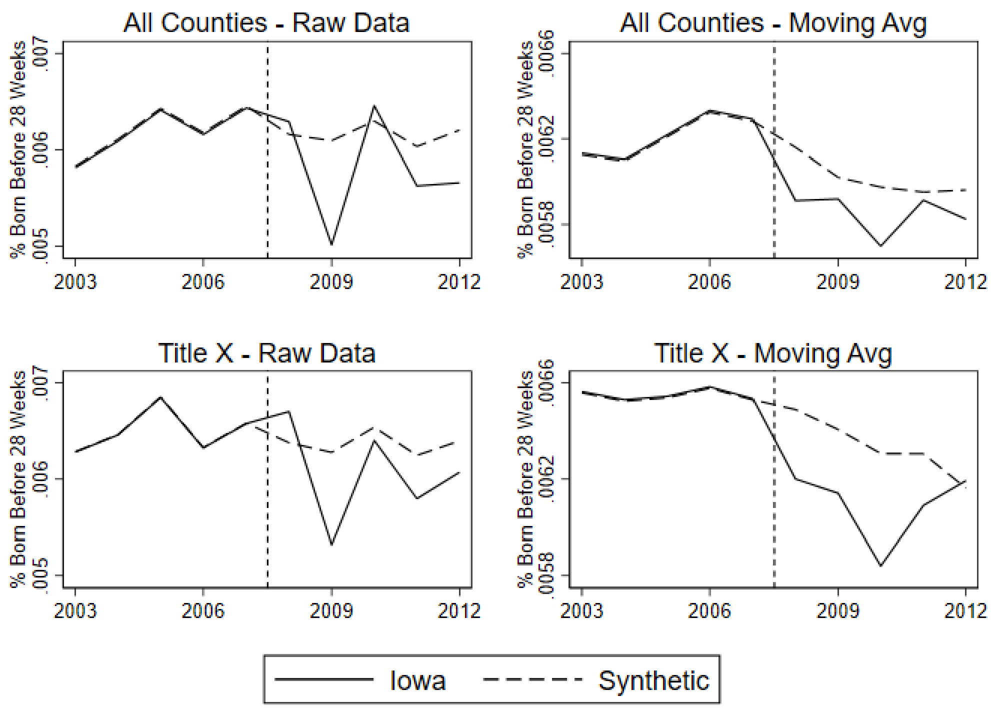
Note: This figure displays the difference between each state and its synthetic control for each period from 2003-2015. The bold line represents Colorado, while each of the other lines represents one of the 49 placebo states. The top row estimates the SCM on all counties, while the bottom row only includes counties with a Title X clinic.

the smaller rebound in Title X counties, the moving average effect is much larger in the Title X counties than in the sample overall, which is consistent with a causal impact of the LARC intervention. Appendix Figures 6 and 7 display the respective graphs of the distribution of RMSPE ratios and placebo treatment comparisons. When conducting inference, none of the Iowa specifications is individually significant[11], though they all have p-values of .30 or smaller.

Finally, Figure 8 displays synthetic control estimate for St. Louis. Since this is estimated at a local level, I compare St. Louis to other counties with at least 3,000 births per year, of which there are 242. Since there are no ‘untreated’ units within

[11]The p-value on all counties estimated with raw data is .30, for the all county moving average the p-value is also .3, for the Title X raw data it is .16 and for the Title X moving average it is .18

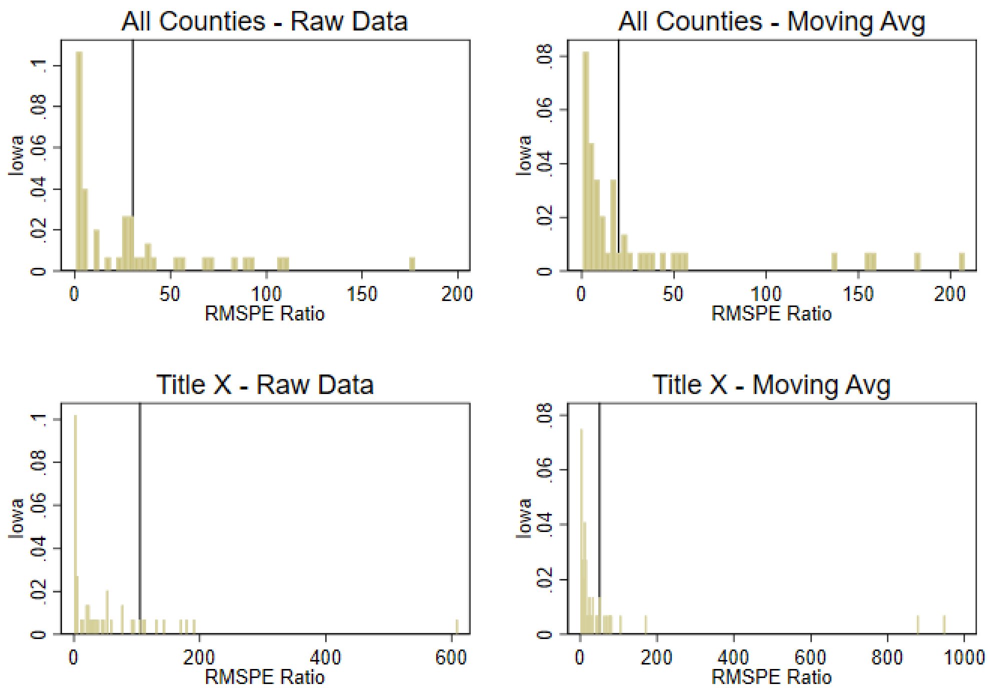
Figure 5 — Synthetic Control - Iowa



*Note:*This figure displays outcomes from applying the SCM to Iowa, estimating the effect of the IIRUP 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) = .30, .30, .16, .18

St. Louis, I only estimate the model on the raw data and the moving average, so the bottom half of Appendix Figure 8 also includes the placebo treatment plot. As in Colorado and Iowa, St. Louis appears to be relatively steady in the few years leading up to treatment, before declining rapidly 1-2 years after the LARC intervention takes place. Similar to Colorado and Iowa, there is a slight rebound after the drop, but overall there appears to be a substantial change in levels, from about 11 EPBs per 1,000 live births to somewhere between 8 and 9. Comparing the RMSPE ratio of St. Louis with the placebos returns a p-value of .02 on the raw data and .14 on the three year moving average. Taken together, however, the results across the three separate interventions provide evidence for a causal impact

Figure 6 — Synthetic Controls - RMSPE Distributions - Iowa



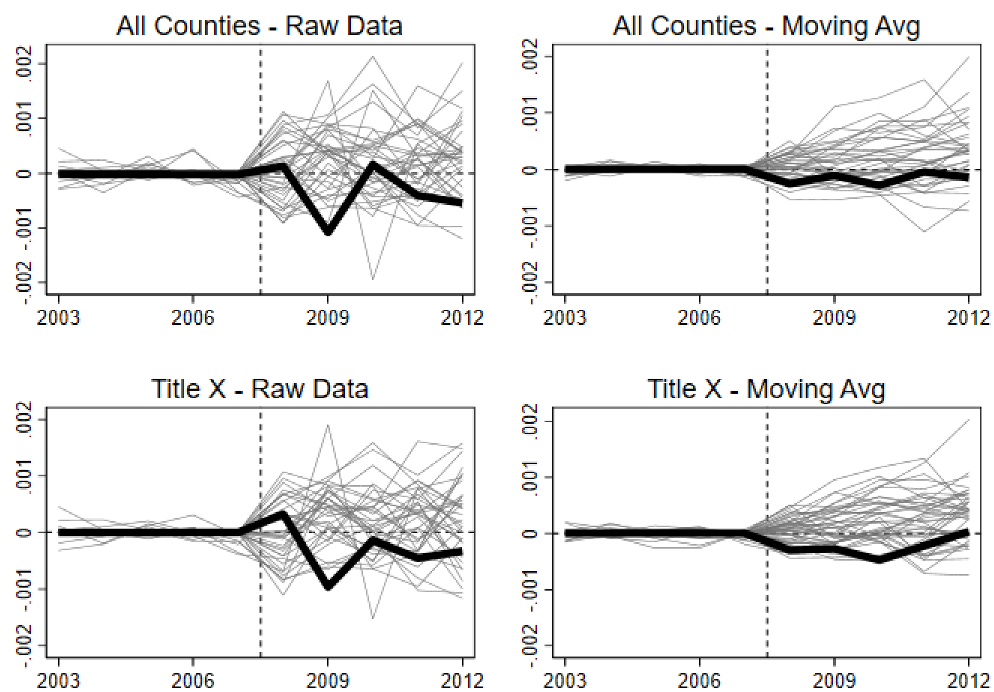
Note: This figure displays the distribution of root mean squared predicted error (RMSPE) ratios for Iowa compared with placebo ratios for each of the other 49 states. The top row estimates the SCM on all counties, while the bottom row only includes counties with a Title X clinic.

of LARC access on the rate of EPB's, with the largest impact occurring 1-2 years after treatment. In order for that not to be the case, some other factor would have had to cause large declines in EPBs in all three treated areas within two years of the LARC interventions, which seems unlikely.

B.2. Infant Mortality

In order to compare changes in infant mortality which are showing in Colorado and Iowa with other states who were evolving similarly in the pretreatment years, Appendix Figures 9 and 10 plot three-year moving averages in infant mortality in Colorado and Iowa against their synthetic controls and compares these treatment effects with placebo estimates for the other 48 states and Washington D.C. Beginning with Colorado and the top left graph in Figure 9, the synthetic control

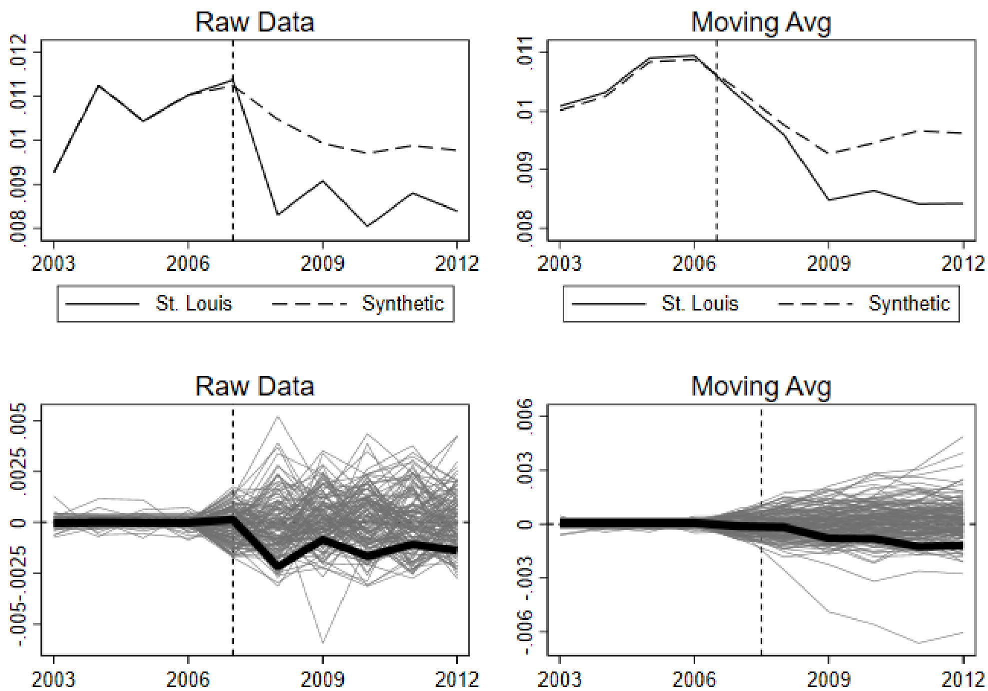
Figure 7 — Synthetic Controls - Placebo Treatment Effects - Iowa



Note: This figure displays the difference between each state and its synthetic control for each period from 2003-2015. The bold line represents Iowa, while each of the other lines represents one of the 49 placebo states. The top row estimates the SCM on all counties, while the bottom row only includes counties with a Title X clinic.

matches closely in the pretreatment period, hovering between 6.0 and 6.3 without any large changes. Both groups begin to decline in 2009, though the drop in Colorado is substantially more pronounced than its control. The divergence does not occur right away, which makes sense because of the staggered treatment of the CFPI as well as the lag between receiving a LARC and the avoidance of an unintended pregnancy. In 2010 there is only a difference of 0.1 infant deaths per 1,000 births, but this grows to 0.41 in 2011 and 0.60 in 2012, before remaining between 0.62-0.73 in 2013 and 2014. While infant mortality was declining after the CFPI, the decline in Colorado after its implementation was substantially larger than in the states that most closely matched Colorado's trends in the pretreatment period. The top-right graph in Figure 9 plots the distribution of RMSPE ratios of Colorado

Figure 8 — Synthetic Control - St. Louis

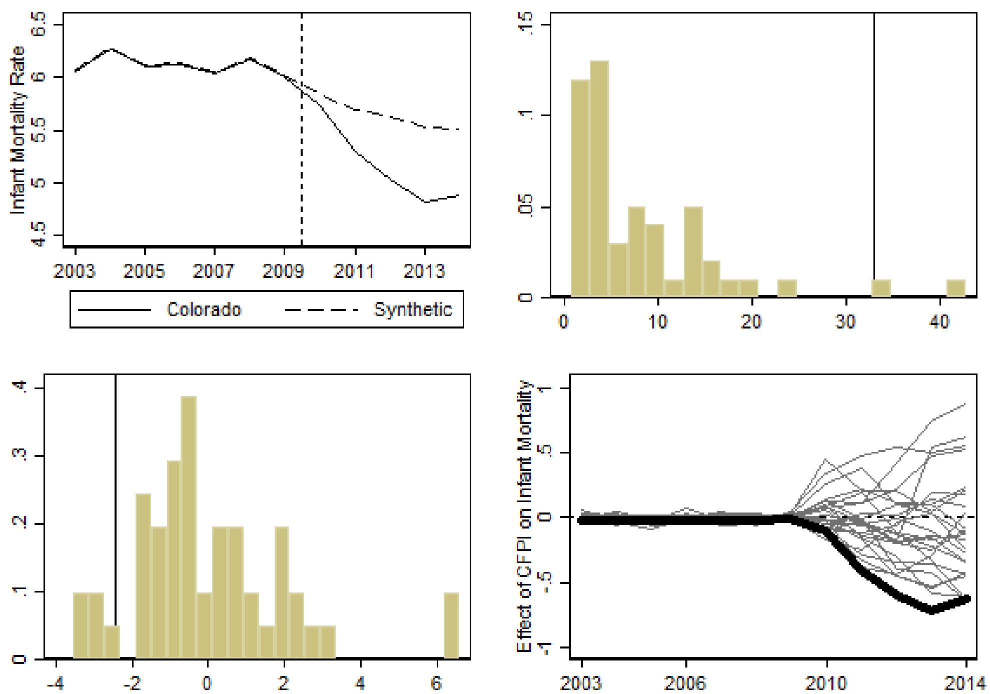


Note: This figure displays outcomes from applying the SCM to St. Louis as well as other counties with at least 3,000 births per year. The top left graph displays the synthetic control estimated on raw data, while the top right graph displays the same estimate on a three-year moving average. The bottom row displays the treated estimate compared with all of the placebo estimates which fit reasonably well in the pretreated period. The p-value for the raw data is .02, while on the weighted average it is .14

compared with the 50 placebo estimates. Colorado is the third largest ratio, corresponding with a p-value of .04, and is clearly out in the right tail of the distribution. The two placebos with larger ratios than Colorado are Kentucky and Oklahoma. In both cases, the ratio is larger than Colorado's because the states match in the pre-treatment period substantially more closely than Colorado, even though they show less variation in the posttreatment period. This is further illustrated by the bottom-left graph in Figure 9, which plots the distribution of total treatment effects, or the sum of the difference between the treated group and it's synthetic control for the years 2010-2014. Here, Colorado has the 5th largest negative treatment effect at -2.5, with both Kentucky and Oklahoma sitting in the middle of the distribution at 1.5 and 0.8, respectively. The four states with more negative sums (Wyoming, Vermont, Virginia and Massachusetts) all match poorly in the pretreatment period and have much smaller RMPSE ratios than Colorado. Finally, the graph on the bottom-right of Figure 9 plots the treatment effects of Colorado against the placebo estimates. In line with Abadie, Diamond, et al. (2010), I iteratively drop placebo estimates which match poorly in the pretreatment period. The graph in the figure display all the placebos with a root mean squared error in the pretreatment period no less than four times as large as Colorado's. This leaves 25 placebo estimates, of which Colorado is clearly the most negative. Overall, the decline in infant mortality in Colorado after the CFPI appears to be much larger than what could have been expected to happen by chance, and the staggered timing of the drop fits closely with what would be expected if it were caused directly by the CFPI.

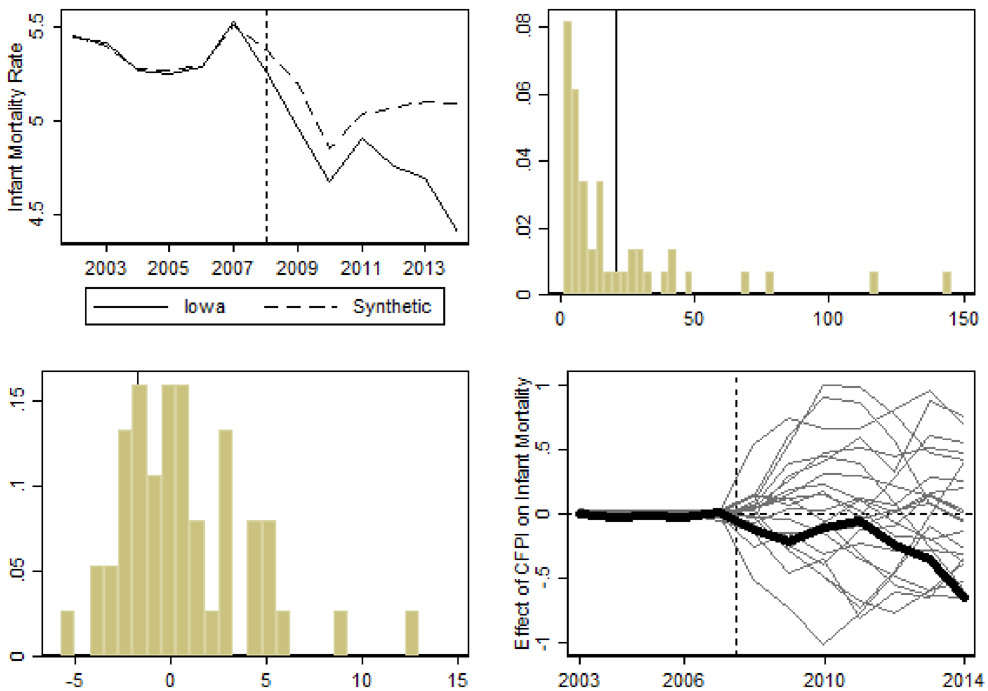
Looking at Iowa in Figure 10, there is also a clear decline in infant mortality around the time of the IIRUP, but it is much more closely matched by its synthetic control than Colorado. Both Iowa and its synthetic control remain between 5.2 and 5.6 from 2002-2007 dropping rapidly for two years and then rebounding slightly. Overall, Iowa still declines by more than its control by between 0.1-0.2 infant deaths per 1,000 births for 2008-2010 before this rebound. After this, Iowa continues to decline while its synthetic control remains steady at around 5. Since Title X clinics in Iowa were still giving out free LARCs in 2012, the continued drop could still be attributed to the IIRUP even after the synthetic control rebounded and then remained steady. Iowa displays the 35th largest RMSPE ratio out of 50 for a p-value of .28, while its total treatment effect of -1.73 is the 13th most negative. Finally, comparing trends in Iowa against placebos which fit well prior to 2008 shows a modest and insignificant decline. While the results for Iowa are not nearly as compelling as those for Colorado, the fact that both states show improvements in infant mortality shortly after increasing LARC access to low-income women suggests that there is a causal relationship. This interpretation is supported by the fact that Colorado saw a larger decline as the intervention in Colorado occurred on a larger scale than the one in Iowa.

Figure 9 — Synthetic Control - Colorado



Note: This figure compares synthetic control outcomes for Colorado against placebo specifications when measuring the impact of the CFPI on infant mortality. The top-left graph plots the evolution of infant mortality in Colorado versus its synthetic control. The top-right graph plost the distribution of RMSPE ratios with a vertical line where the true treatment effect lies, with one extreme outlier dropped. The bottom-left graph displays the distribution of total treatment effects, which are the sum of the difference between the designated treatment group and its synthetic control for the posttreatment period. Finally, the bottom-right graph displays the difference between evolution of the actual treated group versus its synthetic control in black against all placebos which match closely in the pretreatment period in grey.

Figure 10 — Synthetic Control - Iowa



Note: This figure compares synthetic control outcomes for Iowa against placebo specifications when measuring the impact of the IIRUP on infant mortality. The top-left graph plots the evolution of infant mortality in Iowa versus its synthetic control. The top-right graph plots the distribution of RMSPE ratios with a vertical line where the true treatment effect lies, with one extreme outlier dropped. The bottom-left graph displays the distribution of total treatment effects, which are the sum of the difference between the designated treatment group and its synthetic control for the posttreatment period. Finally, the bottom-right graph displays the difference between evolution of the actual treated group versus its synthetic control in black against all placebos which match closely in the pretreatment period in grey.