THE ROLE OF CRISIS PREGNANCY CENTERS IN FERTILITY DECISIONS*

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Abstract

This paper provides the first causal estimates of how Crisis Pregnancy Centers (CPCs) affect fertility. CPCs counsel pregnant women toward carrying to term and operate more than 2,500 locations nationwide, outnumbering abortion clinics roughly three to one. I develop a simulated instruments strategy that addresses endogenous CPC placement by constructing counterfactual expansion paths from a hazard model of organizational entry. 2SLS estimates show that CPCs reduce log abortion rates by 0.081 to 0.180 across the age distribution. Monte Carlo experiments with synthetic data and known treatment effects confirm that the simulated instrument recovers the true causal parameter under severe designed endogeneity.

Keywords: Fertility, abortion, birth, crisis pregnancy centers, simulated instruments

JEL Classification: J12, J13, C15, C36, and K23

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1 Introduction

Crisis pregnancy centers (CPCs) are the most widespread anti-abortion intervention in the United States. More than 2,500 centers operate nationwide, outnumbering abortion clinics roughly three to one (Jones & Jerman, 2017). CPCs counsel pregnant women toward carrying to term, offering pregnancy testing, material support, and community networks (Care Net, 2018). They receive substantial public funding, approximately $89 million in federal and state funds in fiscal year 2021–22 (Kruesi, 2022), and by their own account prevent tens of thousands of abortions each year.[1] Yet no causal evidence exists on whether they actually affect fertility outcomes. This paper provides such evidence for two southeastern states. Using a 30-year county panel from North and South Carolina and a simulated instruments strategy, I estimate that one additional CPC per 10,000 women reduces log abortion rates by 8–18 percent in 2SLS regressions. Translating log coefficients to level changes using age-specific baseline rates is consistent with abortion-to-birth substitution, with the strongest evidence for women aged 25–29 where the birth coefficient is statistically significant and robust. For younger women, the point estimates imply substitution rates near unity, but confidence intervals include both zero and full substitution. The birth effect concentrates in smaller counties where per-capita CPC intensity is highest, consistent with a dose-response for a treatment that scales inversely with county population. The IV estimates substantially exceed OLS in absolute magnitude, consistent with positive selection: CPCs locate where unobserved factors independently drive high abortion rates, attenuating OLS toward zero.

The central identification challenge is that CPCs choose locations strategically. Organizations may target counties based on unobserved local conditions that independently affect

[1] According to self-reported data from the umbrella organization Care Net, their 1,100 affiliated CPCs prevented 677,248 abortions between 2008 and 2017, or about 1 in 10 abortions (Care Net, 2022).

fertility outcomes, and CPC expansion responds to past abortion rates, creating feedback that standard panel controls cannot fully absorb. Unlike many policy interventions, CPC growth is gradual and not triggered by discrete policy shocks. I address this with a simulated instruments approach that isolates quasi-random variation in the *timing* of CPC openings. I estimate a hazard model of CPC entry based on predetermined county characteristics, then forward-simulate 1,000 counterfactual expansion paths from 1990 onward. The instrument is the average of these simulations: expected CPC presence conditional on observable county history. The identifying assumption is that conditional on rich historical observables and fixed effects, the exact year a CPC opens depends on logistical frictions unrelated to contemporaneous fertility shocks.[2] Monte Carlo validation confirms the estimator recovers the true causal parameter under severe designed endogeneity across a range of DGP specifications.

Understanding these effects has become more urgent since the Supreme Court's 2022 decision in *Dobbs v. Jackson Women's Health Organization* (Supreme Court of the United States, 2022), which removed federal abortion access protections. With many states having restricted or banned abortion, the role of organizations that shape demand-side fertility decisions warrants scrutiny. Whether CPCs meaningfully alter the composition of fertility outcomes, or simply counsel women who would have carried to term regardless, has direct implications for reproductive health policy in the post-*Roe* landscape. Existing research on fertility and abortion access has focused on supply-side restrictions: policies that close clinics, impose waiting periods, or require parental involvement. Clinic closures substantially reduce abortion rates, increase births, and shape women's educational and labor market outcomes (Fischer, Royer, & White, 2018; Lindo, Myers, Schlosser, & Cunningham, 2020; Miller,

[2]Appendix 7 documents the spatial distribution of CPCs and abortion providers over time.

Wherry, & Foster, 2020; C. K. Myers, 2017). Travel distance to providers is a key mechanism (Jones, Jerman, & Ingerick, 2021; C. Myers & Ladd, 2020). This paper asks whether demand-side interventions, including counseling, information provision, and material support, also affect fertility outcomes, a question the supply-side literature leaves unanswered. The strongest existing descriptive evidence comes from Cartwright, Tumlinson, and Upadhyay (2021), who find that women who visited a CPC were 21 percentage points less likely to have had an abortion, though this estimate does not account for the endogeneity of CPC location or visit decisions.

This paper makes two contributions. First, it provides the first causal estimates of CPC effects on fertility, using county-level data from North and South Carolina between 1990 and 2019. The abortion effects are 8–18 percent per unit of CPC intensity across all five age groups. Within this setting, these magnitudes are of similar order to the supply-side effects Lindo et al. (2020) estimate for clinic closures (approximately 20 percent) in their setting. Level-change accounting using age-specific baseline rates is consistent with averted abortions appearing as additional births, with the strongest evidence for women aged 25–29 where the birth effect is statistically significant. For younger women, the confidence intervals include both zero and full substitution. Whether these effects generalize beyond the specific demographic, religious, and regulatory environment of NC and SC is an open question, but the finding establishes that demand-side interventions can meaningfully reshape pregnancy outcomes in settings where abortion remains legally accessible. Second, the simulated instruments methodology extends the simulation-based approach of Currie and Gruber (1996) and Angeles, Guilkey, and Mroz (1998) to dynamic settings with path-dependent treatment assignment. The approach is applicable wherever treatment is strategically assigned through

a process that can be modeled but not experimentally manipulated, and no discrete policy shock is available for identification.

2 Background

2A. Crisis Pregnancy Centers

CPCs offer pregnancy testing, counseling from a pro-life perspective, and material aid such as baby clothes, cribs, diapers, and direct financial assistance (Care Net, 2018). When a pregnancy is confirmed, many CPCs provide limited ultrasounds intended to inform about gestational age and viability.[3] CPCs also offer adoption referrals and abstinence education classes aimed at teenagers, held either at the center or in schools. The vast majority are faith-based organizations affiliated with evangelical Christian or Catholic networks such as Care Net, Heartbeat International, or the National Institute of Family and Life Advocates (Swartzendruber, Mercaldo, Newton-Levinson, & Rathbun, 2018). Most are not licensed medical facilities, meaning medical ethics rules and patient privacy laws do not apply.[4]

Abstinence education is a significant component of CPC outreach to teenagers, historically funded through federal programs including the Community-Based Abstinence Education (CBAE) program (Fernandes-Alcantara, 2018; Sabia, 2006). Existing evidence largely suggests abstinence education is ineffective at preventing teen sexual activity or pregnancy (Carr & Packham, 2017; Kohler, Manhart, & Lafferty, 2008). The expected effect on fertility is therefore ambiguous: if effective, it would lower both abortion and birth rates; if

[3]The provision of ultrasound services does not necessarily imply that the CPC offers comprehensive medical examinations. A review of CPC websites indicates that only some CPCs have staff certified as registered diagnostic medical sonographers, and that a small share employ registered nurses or other medical professionals.

[4]The most consequential regulatory attempt, California's FACT Act (2015), was struck down by the Supreme Court in *National Institute of Family & Life Advocates v. Becerra* (2018) as an unconstitutional restriction on free speech.

ineffective, it may alter the composition of pregnancies without reducing their incidence.

CPCs are relatively small nonprofit organizations, primarily funded through private donations from affiliated religious networks. On average, organizations in the analysis sample had annual revenues of $230,275 in 2018. Public funding supplements this private base: in FY2021–22, $89 million in federal and state funds supported CPC operations across a dozen states (Kruesi, 2022). Historically, CPCs received public funding primarily as providers of abstinence education. Over the past decade they have received increasing amounts of Temporary Assistance for Needy Families (TANF) funding and, for a period, Title X funding for reproductive healthcare. The two states in my sample provide additional support: North Carolina directs state and federal grants through the umbrella organization Carolina Pregnancy Care Fellowship to over 70 affiliated CPCs,[5] while South Carolina allocates proceeds from "Choose Life" license plate sales to CPCs.

The CPC network has expanded steadily since abortion legalization in 1973. In my sample, the number of CPC locations in North and South Carolina grew from approximately 70 in 1990 to over 200 by 2019 (Table I, Figures IV–VII). Growth was gradual rather than concentrated around particular policy events, reflecting decentralized decisions by individual organizations responding to local conditions. By 2019, the typical county resident lived within 8 miles of a CPC, down from over 20 miles in 1990. CPCs and abortion providers overlap geographically: by 1990, all but one county with an abortion provider already had a nearby CPC, and subsequent CPC expansion targeted underserved areas farther from existing centers (§8A.). This expansion pattern—responsive to local demographics and existing service coverage—is the institutional basis for the hazard model of CPC entry developed in

[5]Federal funds were first used in FY2014 and have been supplemented by state funding since 2018 (see Table V). The North Carolina General Assembly began designating money from the Title V Maternal and Child Health Block Grant for the Carolina Pregnancy Care Fellowship in FY2013–14.

§4D..1.

There is limited research on who visits CPCs. Rice, Chakraborty, Keder, Turner, and Gallo (2021) find that 13.5 percent of surveyed women in Ohio reported having ever visited one, with attendance higher among Black women, lower-income women, and women without college degrees. Cartwright et al. (2021) find that 13.1 percent of women searching for abortion services online visited a CPC during their pregnancy, and that living closer to a CPC is associated with greater odds of visiting one. Women who visited a CPC were 21 percentage points less likely to have had an abortion, the strongest available descriptive evidence that CPC contact shapes pregnancy outcomes. Importantly, 58 percent of CPC clients in their sample were unaware of the center's pro-life mission or were actively seeking abortion services. Public health researchers have documented that CPC websites frequently contain misleading health information and often fail to disclose that they do not provide or refer for abortion services (Rosen, 2012; Swartzendruber, Newton-Levinson, et al., 2018).

3 Data

I focus on North and South Carolina because these states provide fertility counts by age, ethnicity, and county of residence beginning in 1990, allowing me to study CPC openings and fertility outcomes over a 30-year horizon (1990–2019). The analysis sample covers 146 counties observed annually, yielding 4,380 county-year observations. Summary statistics are reported in Table I.

3A. CPC and Abortion Provider Data

I construct a longitudinal registry of CPC and abortion provider locations in NC and SC, including precise addresses and dates of operation. The dataset contains 288 CPC locations associated with 138 CPC organizations and 43 abortion provider locations.[6] I validate the CPC data by cross-referencing four independent sources: the Birthright umbrella organization database, IRS tax filings, CPC websites, and Yellow Pages entries.[7] Abortion provider data combine Title X grant recipient records from the Department of Health and Human Services, state licensing records, and provider lists from the National Abortion Federation and Planned Parenthood, verified against Myers' Abortion Facility Database (C. Myers & Ladd, 2020). The resulting dataset tracks when and where each CPC and abortion provider opened and closed over the full sample period. Figures IV–VII show the spatial distribution of CPCs and providers over time, overlaid on county-level abortion rates.

The main treatment variable is the lagged number of CPCs per 10,000 women ages 10–44 in a county.[8] Over the sample period, mean CPC presence roughly doubled (from 1.1 to 2.8 per county) while mean distance to the nearest CPC fell from 20 miles to 8 miles (Table I).

[6]The number of locations exceeds the number of CPC organizations because some organizations operate multiple facilities and because address changes are tracked. A relocation is defined as the closure of a facility at one address and the opening of a facility in the same county within a year.

[7]Financial filings and news articles provide additional information on address changes and closures.

[8]A typical county in the sample has approximately 10,000 women of childbearing age. Adjusting by the population in the service area follows Lindo et al. (2020). The treatment is lagged one year to avoid simultaneity and to reflect the institutional reality that CPC effects operate through established community presence rather than contemporaneous opening.

3B. Fertility Outcomes

The primary outcomes are the log abortion rate and log birth rate per 1,000 women in a county and year.[9] Counts are obtained from administrative vital records provided by the NC and SC vital statistics offices, reported by county of residence, age group (10–19, 20–24, 25–29, 30–34, 35–44), and ethnicity (white/nonwhite).

Two features of the data merit note. NC resident abortion data include abortions performed in any state, while SC data are limited to abortions performed in SC, NC, or Georgia, attenuating estimated CPC effects in South Carolina.[10] County-level abortion rates declined from 16.8 per 1,000 women in 1990 to 8.6 in 2019; teen rates fell from 19.0 to 2.9.

The empirical specifications control for time-varying county characteristics that enter both the hazard model (§4D..1) and the second-stage equation, including distance to the nearest abortion provider, demographics, economic conditions, political ideology, and religious composition (Table I lists the full set). All controls are lagged one year. County and year fixed effects are included in all specifications.

[9]The total number of pregnancies is an undercount because miscarriages, which occur in approximately 13 percent of pregnancies (Andersen, Wohlfahrt, Christens, Olsen, & Melbye, 2000), are not fully reported.

[10]At the county-year level, 4 observations have zero abortion counts (isolated single-year zeroes in small rural NC counties) and are dropped from the log specification. All 146 counties remain in the panel. The Poisson control function (Appendix 14B.) retains these observations. At the county-year-age level, zero counts are addressed using $\log(\text{rate} + 0.1)$; results are robust to alternative transformations.

Table I: Summary Statistics

	1990–2019		1990		2019	
	Mean	SD	Mean	SD	Mean	SD
Abortion rate (per 1,000 women)						
Total	11.07	(4.90)	16.81	(5.95)	8.63	(3.14)
Age 10-19	7.86	(5.35)	19.02	(6.56)	2.91	(0.99)
Birth rate (per 1,000 women)						
Total	45.87	(15.03)	49.45	(13.39)	42.02	(13.44)
Age 10-19	22.06	(10.28)	34.09	(8.86)	9.73	(3.63)
Crisis Pregnancy Centers						
No. of CPCs	2.15	(2.46)	1.11	(1.40)	2.81	(2.80)
CPCs per 10,000 women	0.35	(0.39)	0.20	(0.30)	0.41	(0.42)
Distance to nearest CPC (miles)	11.65	(14.30)	20.44	(20.80)	7.68	(9.35)
Annual CPC revenue (thousands)	463.30	(1,013.04)	0.00	(0.00)	799.33	(1,312.19)
County Characteristics						
Female population age 10-44	67,405.81	(69,965.99)	48,246.06	(42,286.10)	88,599.06	(92,885.17)
Non-white share	0.32	(0.15)	0.29	(0.15)	0.34	(0.15)
Share age 10-19	0.27	(0.03)	0.26	(0.03)	0.28	(0.02)
Nearest abortion clinic (in miles)	27.90	(26.12)	32.66	(29.63)	29.32	(23.92)
Unemployment rate	5.95	(2.66)	4.36	(1.54)	3.56	(0.79)
U.S. House GOP vote share	0.53	(0.20)	0.45	(0.24)	0.52	(0.18)
Protestant share	0.54	(0.12)	0.60	(0.12)	0.51	(0.11)
Catholic share	0.04	(0.03)	0.02	(0.02)	0.04	(0.03)
No. of county-year obs.	4,380		146		146	

4 Empirical Framework

The research question is whether CPC presence causally affects fertility outcomes. I estimate the following structural equation separately for abortion rates and birth rates:

$$y_{c,t} = \theta_0 + \theta_1 \text{CPC}_{c,t} + \theta_2' X_{c,t} + \mu_c + \lambda_t + u_{c,t}, \tag{1}$$

where $y_{c,t}$ is the natural log of the outcome (abortion rate or birth rate) in county c and year t; $\text{CPC}_{c,t}$ measures CPC exposure; $X_{c,t}$ is a vector of time-varying county characteristics; and μ_c and λ_t denote county and year fixed effects, respectively. Estimating θ_1 by OLS requires strict exogeneity: $E[u_{c,t} \mid \{\text{CPC}_{c\tau}, X_{c\tau}\}_{\tau=1}^{T}] = 0$. This condition fails under strategic placement.

4A. The Endogeneity Problem

Strategic CPC placement creates two endogeneity problems. First, CPCs may target counties based on unobserved confounders (Z_c) correlated with fertility outcomes, so that OLS conflates the causal effect with selection bias. Second, treatment assignment is a dynamic, path-dependent process: the probability a county receives its kth CPC depends on the entire history of past placements, compounding over 30 years into a nonlinear function of past observables and organizational shocks.

No discrete policy shocks or regulatory discontinuities are available for identification. CPC growth was gradual, decentralized, and effectively absorbing (centers rarely close). The forward-simulation IV addresses these challenges by modeling the treatment assignment process directly and extracting the conditionally exogenous component of treatment timing. Event study evidence of flat pre-trends for abortion rates and Monte Carlo validation under designed misspecification support the identifying assumptions (§4F.).

4B. Instrumental Variables Strategy

CPCs choose locations based on observable county characteristics, including demographics, religiosity, past abortion rates, and existing service availability. These observables change over time and change differently across counties. The IV strategy exploits the fact that a dynamic nonlinear model of CPC entry translates these differential movements into cross-county variation in predicted CPC trajectories that cannot be replicated by the linear controls in the second stage.

I construct the instrument by forward-simulating counterfactual CPC expansion paths. Starting from observed 1990 conditions, I estimate a discrete-time hazard model of CPC

openings based on predetermined county characteristics (§4D..1), then simulate 1,000 counterfactual expansion histories. Each simulation draws independent shocks and uses predicted opening probabilities to determine CPC entry, updating county characteristics recursively each year. Averaging simulated CPC presence across draws yields the conditional expectation of CPC presence given predetermined characteristics:

$$\text{IV}_{c,t} = \frac{1}{S}\sum_{s=1}^{S} \text{CPC}_{c,t}^{(s)} \xrightarrow{p} \mathbb{E}[\text{CPC}_{c,t} \mid W_{c,t-1}] \tag{2}$$

where $W_{c,t-1}$ denotes the full set of lagged county characteristics entering the hazard model. Path dependence makes closed-form computation intractable: CPC presence at time t depends on the full history through $t-1$, and Monte Carlo integration via forward simulation provides a consistent approximation (McFadden, 1989).

The simulation is a computational device. The simulated shocks are computer-generated random numbers, independent of structural errors by construction. Averaging over 1,000 draws eliminates these shocks by the law of large numbers, leaving a smooth function of $W_{c,t-1}$. Regressing $\text{IV}_{c,t}$ on the full set of $W_{c,t-1}$ variables, county fixed effects, and year fixed effects yields $R^2 \approx 1$ (§4F.), confirming that the instrument is a deterministic function of predetermined observables up to simulation noise.[11]

The hazard model is what makes the instrument a strong predictor of CPC presence after partialling out linear controls. CPC placement responds to county characteristics that interact dynamically: whether a CPC opened in 2003 changes the probability of another opening in 2010, because it changes the local CPC stock, the distance to the nearest center,

[11]This R^2 is from a flexible regression of $\text{IV}_{c,t}$ on $W_{c,t-1}$, μ_c, and λ_t. The linear second-stage controls $X_{c,t} \subset W_{c,t-1}$ absorb most but not all of this variation when entered linearly. The residual nonlinear gap is what provides first-stage power.

and the remaining unserved population. A linear first stage treats each variable in each period independently. The hazard model lets earlier values compound through later ones. The logit link, the cumulative CPC stock entering through state dependence, the recursive distance updating, and the compounding of early opening probabilities across 30 years all reflect features of the actual organizational expansion process. A county with a 4% annual opening probability has a very different 30-year predicted CPC trajectory than a county with a 3% probability, because early openings compound through state dependence. These are not arbitrary polynomial transformations of covariates; they encode a structural model of how organizations expand over time.

The instrument approximates the conditional expectation $\mathbb{E}[\text{CPC}_{c,t} \mid W_{c,t-1}]$ via Monte Carlo integration, extracting maximal predictive content from predetermined variables ($F \approx 125$).[12]

4C. Identification

The instrument predicts CPC presence using predetermined county characteristics fed through the hazard model. 2SLS uses that prediction to estimate the causal effect. Predetermination means those characteristics do not forecast current fertility shocks, so the prediction is valid.

The core identifying assumption is:

Predetermination Assumption. The structural error $u_{c,t}$ has mean zero condi-

[12]This implements optimal instruments theory (Chamberlain, 1987; Newey, 1990), encoding how each lagged variable affects current CPC presence through all intervening paths (Liu, Mroz, & Van der Klaauw, 2010; Mroz & Surette, 1998). The strategy follows Angeles et al. (1998), who modeled endogenous placement of family planning facilities using a similar hazard-based approach. See Appendix 9B. and 9D. for details on optimal instruments and dynamic over-identification.

tional on lagged observables, county and year fixed effects:

$$\mathbb{E}[u_{c,t} \mid W_{c,t-1}, \mu_c, \lambda_t] = 0. \tag{3}$$

County fixed effects absorb time-invariant differences across counties. Year fixed effects absorb aggregate trends. Lagged abortion rates in $W_{c,t-1}$ absorb persistent demand dynamics. What remains in $u_{c,t}$ are period-specific, county-specific fertility shocks that prior observables cannot forecast.

Predetermination delivers the exclusion restriction. Since the instrument converges to $\mathbb{E}[\text{CPC}_{c,t} \mid W_{c,t-1}]$ and is therefore measurable with respect to $W_{c,t-1}$, the law of iterated expectations implies:

$$\mathbb{E}[u_{c,t} \cdot \text{IV}_{c,t} \mid \mu_c, \lambda_t] = \mathbb{E}\Big[\text{IV}_{c,t} \cdot \underbrace{\mathbb{E}[u_{c,t} \mid W_{c,t-1}, \mu_c, \lambda_t]}_{=0}\Big] = 0. \tag{4}$$

If past observables are uninformative about current fertility shocks, then any function of those observables—including a nonlinear, path-dependent function—is also uninformative. The exclusion restriction requires no separate argument about the exogeneity of shocks embedded in the instrument, because there are no shocks in the instrument.

2SLS decomposes actual CPC presence into a predicted component (the instrument) and a timing residual $\varepsilon_{c,t}^{\text{DGP}}$, discarding the residual:

$$\text{CPC}_{c,t}^{\text{DGP}} = \underbrace{\mathbb{E}[\text{CPC}_{c,t} \mid W_{c,t-1}]}_{\text{Predicted by instrument}} + \underbrace{\varepsilon_{c,t}^{\text{DGP}}}_{\text{Timing shock}} \tag{5}$$

Predetermination ensures the predicted component is orthogonal to $u_{c,t}$; the formal relation-

ship between predetermination and timing-shock orthogonality is developed in Appendix 9.[13]

The hazard model's role is to determine instrument strength and the implicit weighting of treatment effect heterogeneity across counties, not to deliver the exclusion restriction. Misspecifying the hazard model weakens the first stage and changes which counties contribute most to the IV estimate, but does not introduce bias as long as predetermination holds.

4C..1 Threats to Identification

Four specific concerns could violate predetermination or otherwise compromise identification.

Anticipatory organizational responses. If CPC organizations respond to anticipated future changes in fertility outcomes, then organizational timing would carry information about $u_{c,t}$, violating predetermination. I test for anticipatory effects by controlling for state-level policy changes (parental notification laws, mandatory waiting periods, clinic regulations) and showing results are stable when excluding years surrounding major policy shifts. The event study in Appendix 13 finds no evidence of differential pre-trends in abortion rates before CPC entry.

Correlated changes in abortion provider access. If abortion clinics close in the same counties where CPCs open, and clinic closures independently affect fertility outcomes, the instrument could capture provider access changes rather than CPC effects. The second-stage specification controls for distance to the nearest abortion provider and provider counts, absorbing the direct effect of provider access on abortion and birth rates. Results are stable when restricting to the subsample of counties with unchanged provider access during the

[13]The condition is analogous to sequential ignorability (Robins, Mark, & Newey, 1992).

sample period, and the first stage remains strong ($F > 100$) in this restricted sample.

Persistent unobservables operating through lagged abortion rates. The hazard model conditions on lagged abortion rates $\text{AR}_{c,t-1}$, which could proxy for persistent unobserved factors (community attitudes, social networks) affecting both CPC placement and current fertility outcomes. County fixed effects absorb the time-invariant component of such factors. If lagged rates nonetheless carried residual information about $u_{c,t}$ beyond what μ_c captures, the instrument would predict differential pre-treatment trends. Placebo tests (Appendix 13) find no evidence of such pre-trends. As a direct test, the treatment variable is lagged one year ($\text{CPC}_{c,t-1}$), so twice-lagged abortion rates ($\text{AR}_{c,t-2}$) are predetermined relative to both treatment and outcome. Including them as a second-stage control attenuates the IV coefficient from -0.145 to -0.099 (Appendix Table XXVII), a reduction consistent with some demand persistence but still economically meaningful and statistically significant.

Functional form as the sole source of identifying variation. First-stage power derives from the nonlinear gap between the logit-based forward simulation and the linear second-stage controls. Under predetermination, any function of $W_{c,t-1}$ satisfies the exclusion restriction, so this concern reduces to the prior three threats. The IV coefficient is insensitive to polynomial degree in the hazard model (-0.139 to -0.146; Table XXIX), and Monte Carlo validation confirms parameter recovery under deliberate misspecification (§4F.).

4D. Implementation

4D..1 Hazard Model of CPC Entry

I model CPC opening probabilities using a random utility framework where organizations compare opening a new CPC in county c at time t to maintaining the status quo:

$$p_{c,t} = P(\text{NewCPC}_{c,t} = 1 \mid W_{c,t-1}) = \frac{\exp(\beta' W_{c,t-1})}{1 + \exp(\beta' W_{c,t-1})} \tag{6}$$

where the latent utility shock $\eta_{c,t} \sim$ Type I Extreme Value generates the logistic form.[14] Counties can receive multiple CPCs; the model allows repeated events by conditioning on the accumulated stock $\text{CPC}_{c,t-1}$.[15]

The empirical specification is:

$$\text{logit}(p_{c,t}) = \beta_1 \text{CPC}_{c,t-1} + \beta_2 \text{Dist}_{c,t-1} + \beta_3 \text{AR}_{c,t-1} + \beta_4' X_{c,t-1} + \mu_c + g(t) \tag{7}$$

where $\text{CPC}_{c,t-1}$ and $\text{Dist}_{c,t-1}$ capture existing service accessibility, $\text{AR}_{c,t-1}$ captures organizational responses to local abortion demand, and $X_{c,t-1}$ includes abortion provider presence, demographics (log population, age shares for 10–19 and 20–29, nonwhite share), religious composition (Protestant share and other-religions share), political ideology (GOP vote share), and unemployment. County fixed effects μ_c absorb time-invariant placement factors. The function $g(t)$ is a sixth-degree polynomial in calendar time, used in place of year fixed effects because the forward simulation extends beyond the estimation sample and

[14] The latent utility shock $\eta_{c,t}$ in the random utility model is distinct from the observed-outcome timing shock $\varepsilon_{c,t}^{\text{DGP}}$ in the decomposition (Equation 5). The logit structure maps $\eta_{c,t}$ into CPC opening probabilities; $\varepsilon_{c,t}^{\text{DGP}}$ is the resulting deviation of realized openings from those probabilities.

[15] This extends the single-spell hazard to allow multiple openings following Mroz (2012), with the likelihood taking the standard pooled logit form. Details are in Appendix 8B..

year dummies cannot be projected into out-of-sample periods. The second stage uses year fixed effects; this difference is a mechanical constraint of the simulation procedure rather than a modeling choice.

Functional form misspecification of included variables—using logit when the true link is probit, for instance—affects the precision of the instrument approximation and therefore efficiency, but does not introduce inconsistency. Omitting variables that jointly determine CPC placement and abortion outcomes is more serious: excluded placement determinants correlated with $u_{c,t}$ would violate the orthogonality condition. I address this by including a rich set of predetermined county characteristics and demonstrating robustness across alternative hazard specifications (Appendix 15).

4D..2 Forward Simulation Procedure

For each county c and target year t, I generate $S = 1{,}000$ simulated CPC expansion paths:

1. **Initialize:** Set $\widehat{\text{CPC}}_{c,1990}^{(s)} = \text{CPC}_{c,1990}$ (observed initial state) for all s.

2. **Forward simulate:** For each draw $s = 1, \ldots, S$ and year $\tau = 1991, \ldots, t$:

   (a) Compute predicted opening probability $\widehat{p}_{c\tau}^{(s)}$ from Equation 7 using simulated lagged CPC counts, distances, and time trends, with all other covariates (including lagged abortion rates) held at their observed values;[16]

   (b) Draw $U_{c\tau}^{(s)} \sim \text{Uniform}(0, 1)$;

   (c) Open a CPC if $U_{c\tau}^{(s)} \leq \widehat{p}_{c\tau}^{(s)}$;

[16]Lagged abortion rates are frozen at 1990 observed values throughout the simulation; see the discussion below.

(d) Update cumulative count, distance to nearest CPC, and polynomial time trend.[17]

The uniform draws generate realizations from the logit error distribution, enabling Monte Carlo integration. The synthetic shocks are computational devices; by averaging over them, the instrument depends only on predetermined observables $W_{c,t-1}$. The recursive structure is essential: current opening probabilities depend on lagged CPC counts and distances, which themselves depend on all previous openings, making the forward simulation necessary rather than a convenience. A worked example appears in Appendix 8C..

Variable updating protocol. Table VIII (Appendix 8) documents which variables are updated within the forward simulation and which are held fixed. CPC count, distance to nearest CPC, and the polynomial time trend are updated recursively within each simulation draw. Demographics, religiosity, and economic conditions enter at their observed panel values. Lagged abortion rates are frozen at 1990 values to avoid embedding assumed treatment effects.

Including lagged abortion rates. The conditioning set $W_{c,t-1}$ includes lagged abortion rates in the hazard model, reflecting the institutional argument that CPCs target high-abortion areas (Cartwright et al., 2021). During the forward simulation, lagged abortion rates are frozen at 1990 observed values to prevent the instrument from embedding assumed treatment effects. County fixed effects absorb the frozen cross-sectional level, and the first stage remains strong with and without lagged rates.

[17]Distance is updated using the cross-county median driving distance to the nearest simulated CPC within draw s, avoiding mechanical feedback from a county's own simulated stock into its opening probability.

4E. Estimation

First Stage:

$$\text{CPC}_{c,t} = \pi_0 + \pi_1 \text{IV}_{c,t} + \pi_2' X_{c,t} + \mu_c + \lambda_t + v_{c,t} \tag{8}$$

Second Stage:

$$y_{c,t} = \alpha + \gamma \widehat{\text{CPC}}_{c,t} + \delta' X_{c,t} + \mu_c + \lambda_t + u_{c,t} \tag{9}$$

where $y_{c,t}$ denotes the log fertility outcome (abortion rate or birth rate).

Standard errors are clustered at the county level. Because the instrument depends on estimated hazard parameters, I report bootstrapped standard errors (resampling counties, re-estimating the hazard model and re-simulating within each draw) as the primary inference method. In practice, bootstrap and conventional standard errors are similar, consistent with simulation noise vanishing as $S \to \infty$ (Murphy & Topel, 1985).[18]

The second-stage controls match the hazard model covariates (all lagged one year), with two differences: lagged abortion rates enter the hazard model but not the second stage (county fixed effects absorb the cross-sectional variation), and the hazard model uses polynomial time trends while the second stage uses year fixed effects (§4D..1). Observations are weighted by the square root of female population for variance stabilization.[19] County

[18] With a continuous instrument and continuous endogenous variable, 2SLS estimates a weighted average of marginal causal responses, where weights come from the covariance between the instrument and the endogenous variable across observations. The instrument assigns the most weight to mid-sized counties (10,000–30,000 women) in the 1990s and early 2000s with evangelical institutional presence; metro counties with 90,000+ women contribute little because per-capita CPC intensity is low, and the smallest rural counties contribute little because the hazard model predicts slow expansion. The estimate is thus most informative for counties that experienced the policy-relevant variation in CPC intensity during this period.

[19] Results are qualitatively similar under population-level weighting and unweighted estimation.

and year fixed effects are included in all specifications.

4F. Validation

Monte Carlo experiments validate the estimator under severe designed endogeneity using a 3×3 factorial design (three true treatment effects $\times$ three DGP specifications including nonlinear confounding and heterogeneous effects). OLS is severely biased to the wrong sign (mean $\approx +1.5$); the IV recovers the true β with bias below 0.07 in all nine cells, despite deliberate hazard model misspecification (Table IX, Figure IX). Full details appear in Appendix 10. In the real data, first-stage F-statistics are stable across specifications, event study pre-trends are flat for abortion rates (Appendix 13),[20] and a permutation test shuffling the instrument across counties yields a first-stage F of 0.65 (median) versus 169 in the actual data ($p = 0.000$; Appendix Table XI).

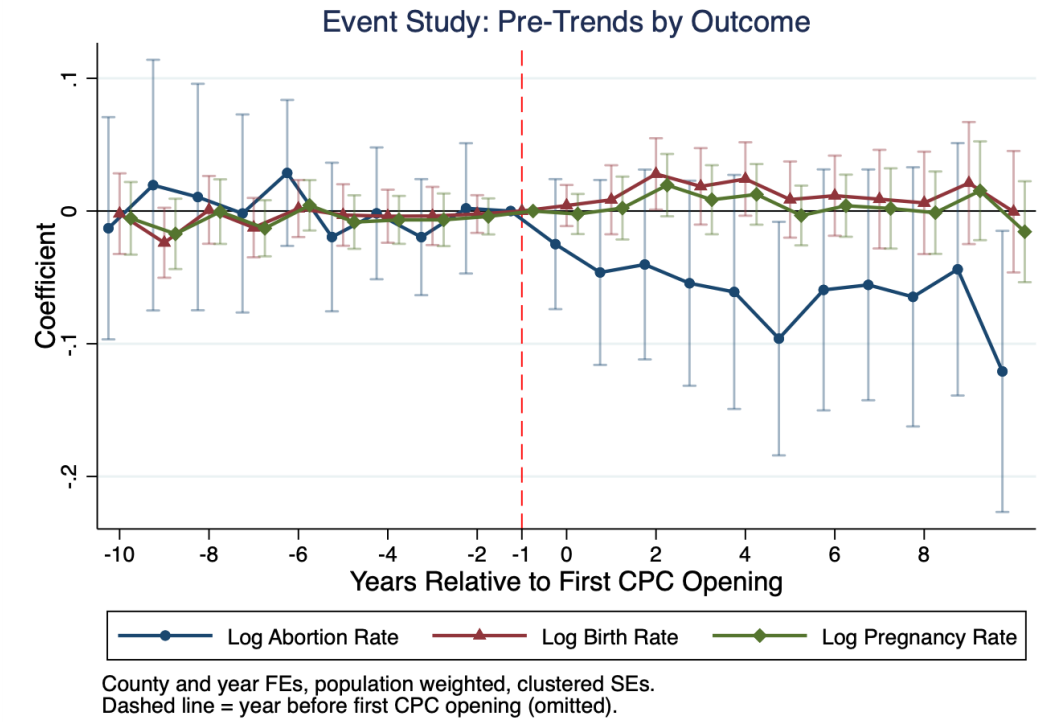
5 Results

5A. Descriptive Evidence

Figures IV–VII document the geographic expansion of CPCs from 1990 to 2019; Figure III (Appendix 7) shows the staggered adoption pattern. Treated counties have higher baseline abortion rates (Appendix Figure VIII), confirming positive selection into CPC placement and motivating the IV approach. Figure I presents event study estimates around first CPC opening: abortion rate pre-trends are flat, supporting the identifying assumption, and post-treatment effects are consistent with CPC-induced fertility substitution.

[20] The event study uses binary treatment (first CPC entry) with within-county timing variation only, identifying the extensive margin effect. The IV uses continuous treatment and both within- and cross-county variation, identifying the marginal effect of CPC intensity. A full comparison appears in Appendix 9C..

Figure I: Event Study: CPC Effects on Abortion and Birth Rates



Notes: Event study coefficients for log abortion rate and log birth rate relative to first CPC opening (event time = −1 is the omitted period). All specifications include county and year fixed effects, time-varying controls, and population weights. 95% confidence intervals based on standard errors clustered at the county level.

5B. Main Results

Table II presents the main fertility estimates. The OLS estimate (Column 1) indicates that one additional CPC per 10,000 women reduces the abortion rate by 7.7%. The 2SLS estimate (Column 2) is roughly double at −14.5% (first-stage $F = 169$), consistent with positive selection bias: CPCs locate where abortion demand is high for unobserved reasons, attenuating OLS toward zero. Measurement error in CPC counts would produce similar attenuation.[21] Across all specifications, the IV estimate exceeds OLS in absolute value, confirming that positive selection attenuates naive estimates. The range of IV estimates (−0.099 to −0.145, from the lagged-outcome-controlled and baseline specifications respectively, with −0.131 when absorbing trending confounders via $X_c \times$ year FE) bounds the CPC effect from multiple directions, with OLS below this range.

The aggregate birth coefficient is positive but imprecisely estimated (0.019, s.e. 0.012). The age-specific decomposition (§5C.) partially resolves this: for women aged 25–29, the birth effect is statistically significant.

[21] Heterogeneous treatment effects across counties with staggered adoption (Callaway & Sant'Anna, 2021; Sun & Abraham, 2021) could also contribute. The IV and OLS estimators weight observations differently, so they may identify effects for different subpopulations. A specification jointly instrumenting CPC count and distance to nearest CPC (both treated as endogenous) yields a larger abortion effect but with a first-stage F of 2.4; see Appendix Table XXXVIII.

Table II: Effect of Crisis Pregnancy Centers on Fertility Outcomes

	(1) OLS	(2) 2SLS	(3) Poisson-CF
First Stage: CPCs per 10,000 on Simulated Instrument			
Simulated CPCs per 10,000		0.836***	
		(0.064)	
Panel A: Log Abortion Rate			
CPCs per 10,000	-0.077***	-0.145***	-0.074*
	(0.029)	(0.040)	(0.041)
CF residual (endog. test)			0.099**
			(0.048)
N	4,376	4,376	4,380
First-stage F		169.1	
Panel B: Log Birth Rate			
CPCs per 10,000	0.010	0.019	0.020
	(0.008)	(0.012)	(0.017)
CF residual (endog. test)			-0.017
			(0.019)
N	4,380	4,380	4,380
First-stage F		169.2	
Panel C: Log Pregnancy Rate			
CPCs per 10,000	0.009	0.014	0.025
	(0.008)	(0.012)	(0.018)
CF residual (endog. test)			-0.017
			(0.020)
N	4,380	4,380	4,380
First-stage F		169.2	
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
$\sqrt{\text{pop}}$ weights	Yes	Yes	—

Notes: Standard errors clustered at the county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1): OLS with county and year fixed effects. Column (2): 2SLS using simulated CPC instrument. Column (3): Poisson regression with control function (Wooldridge, 2014); exposure = county female population; analytic SEs. A specification jointly instrumenting CPC count and distance to nearest CPC (both endogenous regressors instrumented) yields a larger abortion coefficient (-0.265) but with a first-stage F of 2.4; see Appendix Table XXXVIII. All specifications include county demographic controls.

5C. Age-Specific Results and Magnitudes

Table III presents separate county-year 2SLS estimates for each age group under $\sqrt{\text{population}}$ weighting.[22] The AYFE specification (Table XIV, Column 4) pools all age groups with age × year fixed effects and age-group-specific treatment interactions, absorbing national age-specific trends that are confounded with CPC effects in the separate regressions.

Abortion coefficients are negative and significant for all age groups under both specifications, ranging from −0.081 to −0.180 in separate regressions. The AYFE specification absorbs age-specific national trends that steepen the age gradient in separate regressions; the flattened AYFE gradient (−0.155 to −0.207) suggests the separate-regression gradient partly reflects confounding.

For women aged 25–29, both the log abortion and log birth coefficients are statistically significant. The age 25–29 birth result (+0.096, $p < 0.01$) is robust to all alternative controls and weighting schemes; I treat it as the primary evidence of substitution. For younger women, point estimates imply substitution rates near unity, but the log birth coefficients are statistically insignificant because the absolute change is small relative to high baseline birth rates. In level specifications (Appendix Table XIX), birth effects are statistically significant for all five age groups.

[22] Treatment is measured as lagged CPCs per 10,000 women using the county-level total female population as the denominator across all age groups, ensuring a common treatment measure since CPC services are not age-restricted. Using age-group-specific population for weighting provides correct variance stabilization for log-transformed rates, since the variance of a rate is proportional to the inverse of the group-specific count (Wooldridge, 2010).

Table III: CPC Effects by Age Group: Coefficients and Implied Magnitudes

	(1) 10–19	(2) 20–24	(3) 25–29	(4) 30–34	(5) 35–44
Panel A: Log Abortion Rate					
$\hat{\beta}$ (2SLS)	-0.112**	-0.180***	-0.171***	-0.124***	-0.081**
	(0.044)	(0.042)	(0.046)	(0.041)	(0.032)
Δ abort rate (per 1,000)	-0.863	-4.725	-3.293	-1.411	-0.313
Baseline rate	7.7	26.2	19.3	11.4	3.9
Panel B: Log Birth Rate					
$\hat{\beta}$ (2SLS)	0.043	0.036	0.096***	0.099***	0.102***
	(0.038)	(0.024)	(0.021)	(0.025)	(0.024)
Δ birth rate (per 1,000)	1.169	4.332	9.397	6.186	1.599
Baseline rate	27.0	120.3	97.9	62.8	15.7
Panel C: Implied Substitution					
$\lvert\Delta\text{birth}\rvert/\lvert\Delta\text{abort}\rvert$	1.35	0.92	2.85	4.38	5.11
N	8,759	8,750	8,750	8,760	8,760
First-stage F	131.3	138.5	137.0	139.2	129.2
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

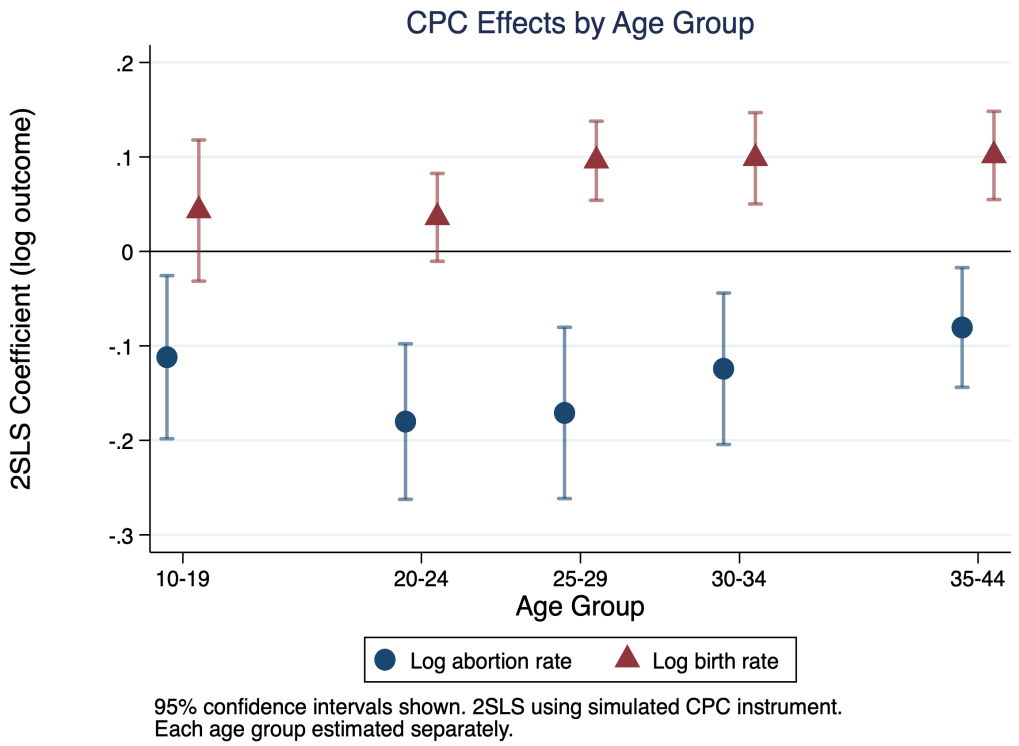
Notes: Each column estimates the 2SLS specification from Table II, Column (2) separately for each age group. $\hat{\beta}$ is the coefficient on CPCs per 10,000 women (log outcome). Δ rate converts the semi-elasticity to a level change using the age-group baseline rate: $\Delta r = \bar{r} \times \hat{\beta}$. Implied substitution is the ratio $|\Delta\text{birth rate}|/|\Delta\text{abort rate}|$ in level terms; values below 1 indicate that some prevented abortions do not result in additional births (prevention rather than substitution). Standard errors clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Marital status decomposition. Appendix Table XXXVI decomposes the age 25–29 results by marital status. Effects are present for both married and unmarried women, with larger effects among unmarried women (-0.147 vs. -0.082 for abortions; $+0.208$ vs. $+0.146$ for births).

Panel C of Table III translates log-point coefficients into implied substitution ratios using age-specific baseline rates. For women aged 25–29, the birth coefficient is statistically significant, and the level-change arithmetic implies a substitution ratio of 2.85, establishing substitution rather than prevention as the dominant mechanism. For younger women, point estimates imply substitution rates near unity, but the birth coefficients are not statistically significant and confidence intervals include zero.

Figure II summarizes the age-specific coefficients for abortion and birth effects.

Figure II: CPC Effects by Age Group: Abortion and Birth Rates (2SLS)



Notes: Point estimates and 95% confidence intervals from the age-specific 2SLS specifications in Table III. Abortion coefficients are negative across all age groups. Birth coefficients are positive. For ages 25–29, the birth effect is statistically significant. For younger women, level changes (Table III) are consistent with substitution, but confidence intervals include both zero and full substitution.

At the aggregate county level, the 2SLS coefficient implies that one CPC per 10,000 women reduces abortion rates by 14.5% and increases birth rates by 1.9%. The policy-relevant magnitude depends on county size: in a median county (11,632 women), one CPC equals 0.86 units of intensity, implying a 12.5% abortion reduction; in a metro county with 90,000 women, the implied reduction is 1.6%. Over the sample period, CPC expansion accounts for approximately 6% of the aggregate decline in county abortion rates. For calibration, Lindo et al. (2020) estimate approximately 20% abortion reductions from clinic closures in their setting. Reconciliation of the IV and synthetic control estimates appears in Appendix Table XXXVII (Appendix 17).

5C..1 Poisson Control Function

The Poisson control function (Wooldridge, 2014) offers a natural robustness check because it models $\log \mathrm{E}[Y \mid X]$ directly, accommodating zero counts without ad hoc transformations and requiring only correct specification of the conditional mean. Table XVII (Appendix 14B.) reports the results. At the county level, the Poisson abortion coefficient is -0.074 ($p < 0.10$); the age-specific Poisson with age $\times$ year fixed effects implies a 13.9 percent aggregate abortion reduction ($p = 0.001$).

The Poisson estimates are smaller than the log-linear 2SLS estimates for two reasons, one statistical and one substantive. First, the Poisson imposes a multiplicative conditional mean and equidispersion, while log-linear 2SLS is agnostic about the functional form of the conditional expectation beyond linearity in the log outcome. Second, the Poisson implicitly weights observations by expected counts, giving more influence to large counties. Because CPC treatment intensity is highest in smaller counties, this weighting attenuates the esti-

mated effect toward zero.[23]

I prefer the log-linear 2SLS estimates as the primary specification. Identification comes from within-county variation in CPC exposure over time, and the policy-relevant quantity is the proportional effect on rates rather than a population-weighted average effect on counts. The Poisson results nonetheless confirm that the qualitative finding survives both an alternative functional form and a shift in implicit weighting.

5D. Robustness

The abortion result is stable across identification tests, alternative estimators, and potential confounders. Table **??** summarizes; full results appear in the referenced appendix tables.

Identification threats. Adding state × post-enactment indicators for TRAP regulations and parental involvement laws leaves the CPC coefficient unchanged (Table XXIII). Restricting to counties always more than 30 miles from the nearest abortion provider yields -0.153 ($F = 148$; Table XXIV). Differential distance tests following Card, Fenizia, and Silver (2023) find no evidence that spatial competition with abortion providers drives the results (Appendix 17). OLS estimates by distance band show a decay pattern consistent with a real spatial mechanism (Table XXXI).

Instrument diagnostics. Adding $X_c \times$ year FE interactions for the key hazard-model predictors (Protestant share, Republican vote share, unemployment) attenuates the IV coeffi-

[23]The log specification drops 4 county-year observations where the abortion count is zero (Clay, Graham, Perquimans, and Yadkin counties in North Carolina, each in a single year). These are isolated single-year zeroes in small counties, not persistently zero counties. The instrument significantly predicts having a zero abortion rate for 4 of 5 age groups (Appendix Table XVIII), implying sample selection correlated with the instrument in the log specification. IHS and $\log(\text{rate} + 0.1)$ transformations that retain zeros produce generally larger coefficients.

cient from −0.145 to −0.131, a 10% reduction, with first-stage F unchanged at 162 (Table XX). A Monte Carlo exercise confirms IV bias remains under 13% even under strong trending confounders when these interactions are included (Table X). A leave-two-county-out jackknife shows no pair of counties shifts the estimate by more than 20% (Figure XXII), and a permutation test confirms instrument strength is not an artifact of overfitting (Table XI). Event study pre-trends are flat for both abortion and birth rates (Appendix 13).

Alternative estimators. Oster (2019) bounds imply unobservables would need to be roughly 15 times as important as all included controls to explain the OLS coefficient ($\delta^* = 14.9$; Appendix 15E.). Augmented synthetic control and SCPI estimators using binary treatment produce negative post-treatment ATTs; multiplying the IV coefficient by mean treatment intensity among treated counties reproduces the SCPI point estimate within sampling error (Appendix 14A.). The log abortion coefficient attenuates when low-baseline-rate counties are dropped, reflecting a mechanical property of the log transformation rather than fragility (Appendix 15B.).

6 Conclusions

This paper provides the first causal estimates of CPC effects on fertility. Using county-level data from North and South Carolina between 1990 and 2019, population-weighted IV estimates imply that one additional CPC per 10,000 women reduces abortion rates by 10 to 15 percent, depending on specification. Age-specific estimates range from 8 to 18 percent across all five age groups in 2SLS regressions with comprehensive controls, with strong first-stage F-statistics. The conclusion that CPCs reduce local abortion rates is

robust to weighting scheme, control set, functional form, and alternative estimators including augmented synthetic control, SCPI, and Poisson control function.

The birth result is more nuanced than the log-point coefficients suggest. For women aged 25–29, the birth coefficient is statistically significant and robust across specifications, providing direct evidence of abortion-to-birth substitution. For younger women, the point estimates imply substitution rates near unity, but confidence intervals include both zero and full substitution. The log birth coefficient is insignificant for these ages because the absolute change is small relative to the high baseline birth rate, not necessarily because the effect is absent. Pregnancy rate coefficients are near zero for younger women, consistent with substitution but also consistent with no effect on either margin. The birth effect concentrates in smaller counties where per-capita CPC intensity is highest, consistent with a dose-response relationship for a treatment that scales inversely with county population.

These estimates are identified from two southeastern states with specific evangelical Protestant demographics, a particular regulatory environment, and a CPC network that expanded during a specific historical window. The IV is most informative for mid-sized counties (10,000–30,000 women) that experienced meaningful CPC expansion during between 1990 and the 2010s. Generalizability to other regions or time periods is unknown, and the post-*Dobbs* environment presents a qualitatively different setting: in ban states, CPCs are no longer competing with accessible abortion services. The estimates are most easily translated to states where abortion remains legal and CPCs operate as a demand-side influence.

Several avenues for future research follow naturally. First, what are the long-run consequences of CPC-influenced fertility decisions for maternal and child outcomes? Linking CPC exposure to longitudinal data on maternal employment, earnings, and children's educa-

tional and health outcomes would provide critical evidence (cf. Miller et al., 2020). Second, understanding how CPCs interact with broader social policies, such as Medicaid expansions, paid family leave, and childcare subsidies, could clarify whether they complement or substitute for public programs. Third, pinning down the specific behavioral pathways requires individual-level data on CPC service utilization and client outcomes.

Within NC and SC during 1990–2019, the CPC network produced measurable effects on abortion rates, with magnitudes of similar order to the supply-side effects of clinic closures estimated by Lindo et al. (2020) in their setting. For women aged 25–29, the birth effect is statistically significant, providing direct evidence of substitution. For younger women, level-change accounting is consistent with substitution, but the confidence intervals include both zero and full substitution. Whether these demand-side effects extend to other states, time periods, or the post-*Dobbs* policy environment remains to be established.

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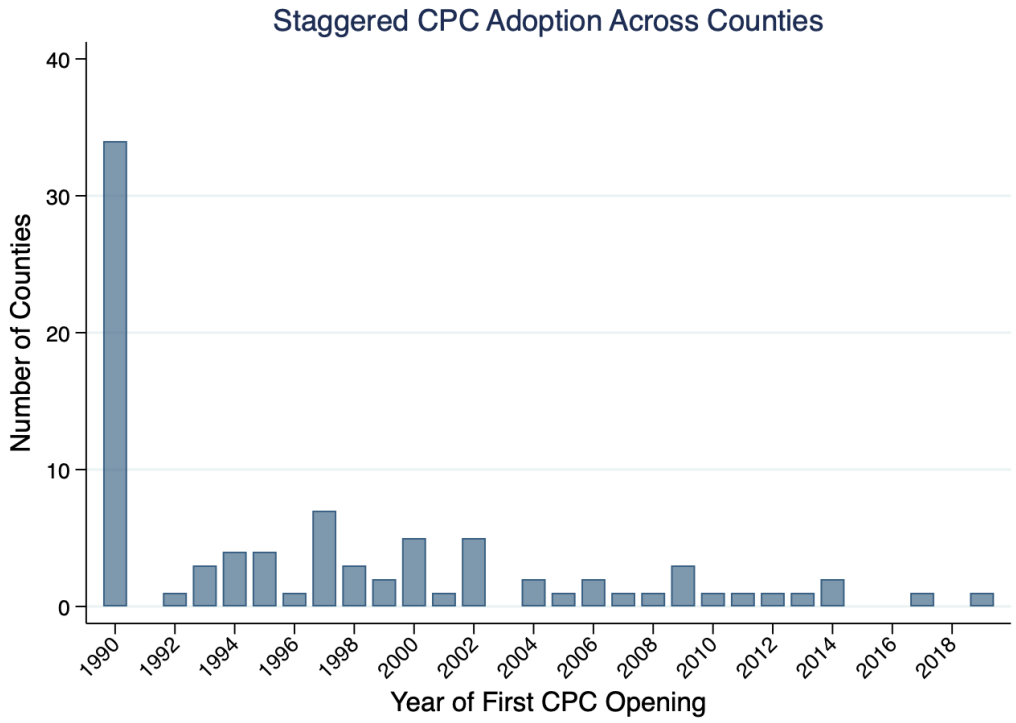
Appendix

7 Appendix A. Institutional Background

Figure III: CPC Expansion: Staggered Adoption Across Counties



Notes:

Distribution of first CPC opening year across NC and SC counties. The top panel shows the number of counties adopting their first CPC in each year. The bottom panel shows the cumulative share of counties with at least one CPC.

Figure IV: CPCs, Abortion Providers and Abortion Rate (NC & SC): 1990

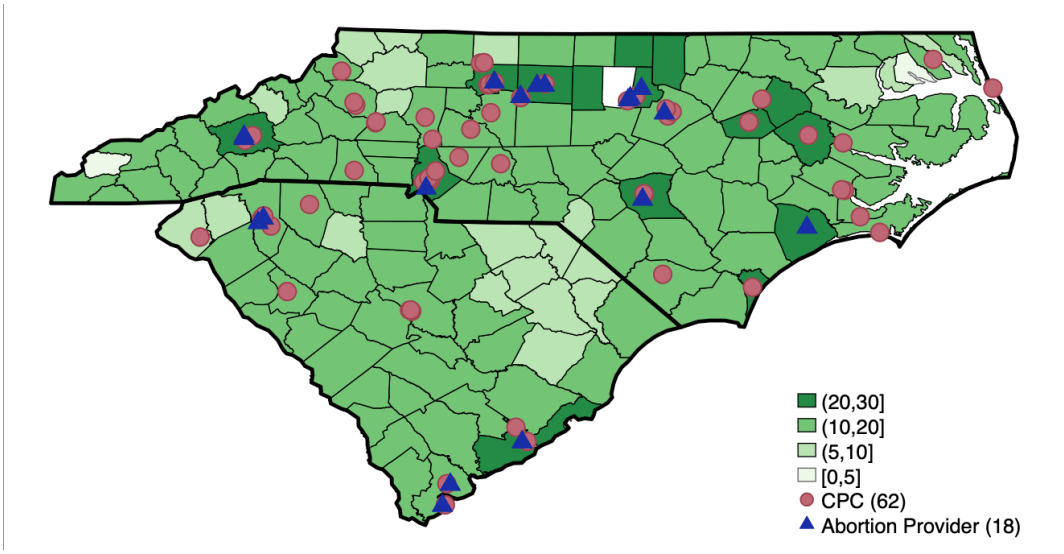


Figure V: CPCs, Abortion Providers and Abortion Rate (NC & SC): 2000

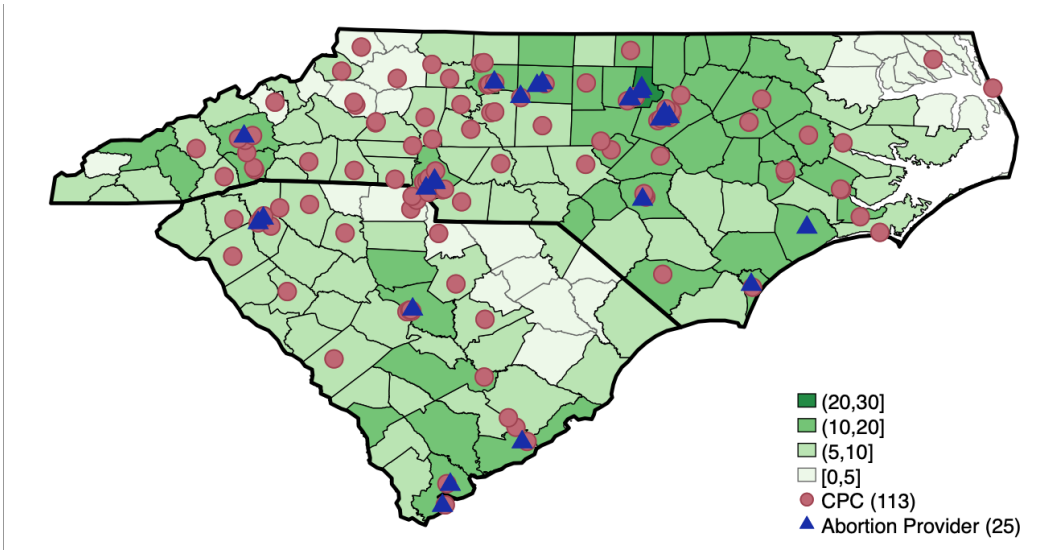


Figure VI: CPCs, Abortion Providers and Abortion Rate (NC & SC): 2010

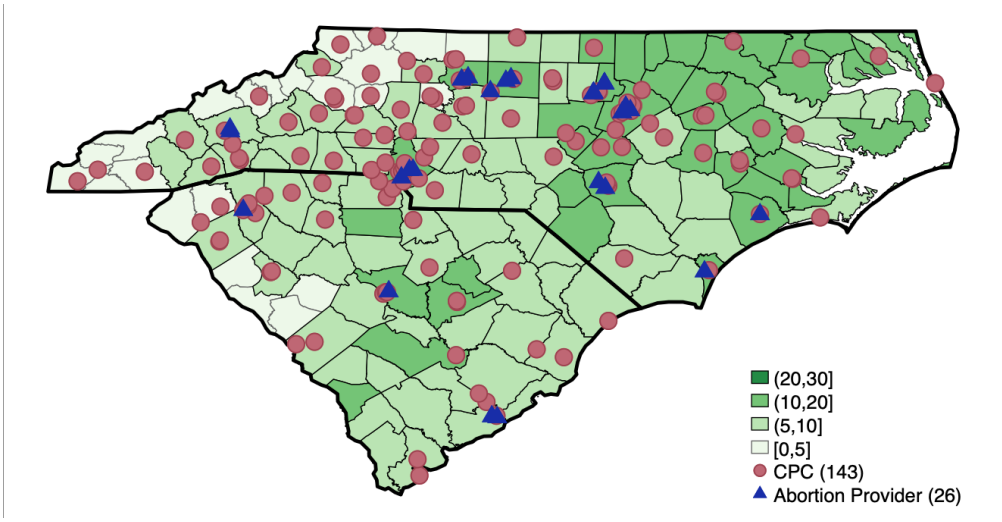


Figure VII: CPCs, Abortion Providers and Abortion Rate (NC & SC): 2019

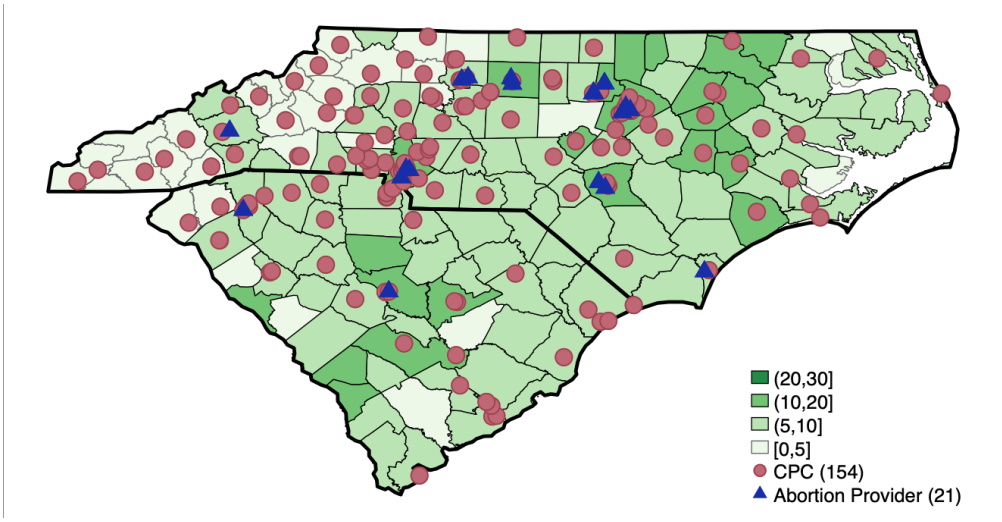
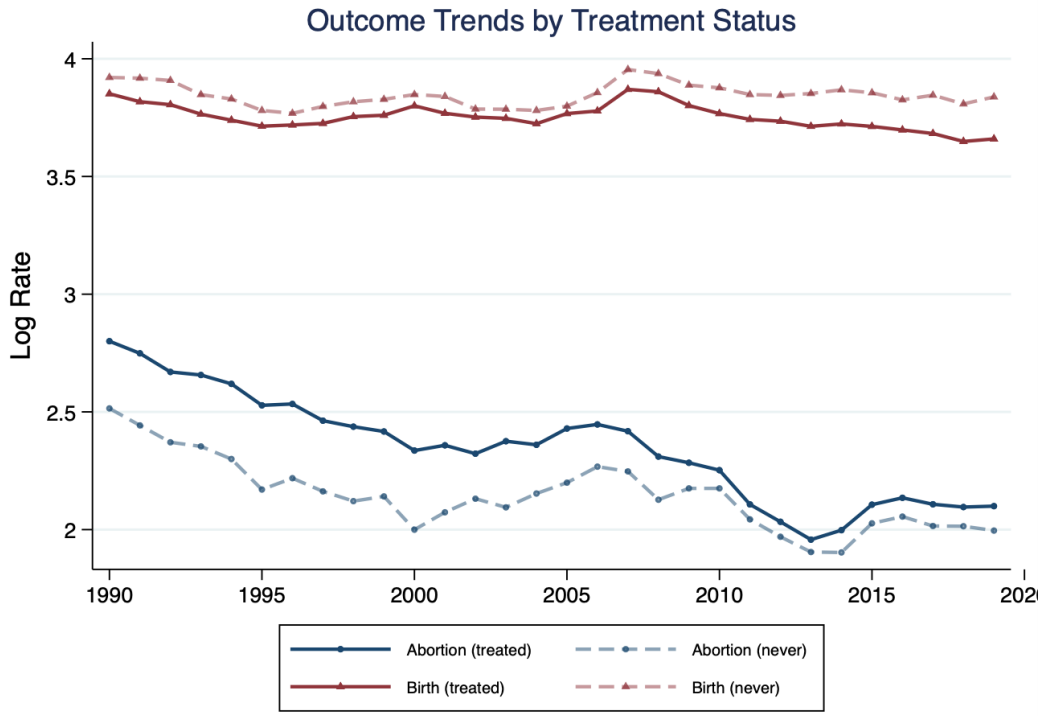


Figure VIII: Outcome Trends: Treated vs. Never-Treated Counties



Notes: Mean log abortion rate (left) and log birth rate (right) for counties that ever received a CPC (treated) versus counties that never received a CPC (never-treated) over the sample period 1990–2019. Trends are unconditional on covariates.

Tables IV and V document the scale of public funding for CPCs. Table VI summarizes the services offered by CPCs in the sample, based on author review of CPC websites matched to the Birthright database.

Table IV: CPC Funding (2010-2021)

Texas	$204,076,058
Pennsylvania	$86,989,000
Missouri	$44,930,673
Florida	$43,000,000
Minnesota	$37,641,000
Indiana	$18,250,000
Louisiana	$15,968,738
Ohio	$13,000,000
North Carolina	$10,303,437
Georgia	$9,000,000
Oklahoma	$5,000,000
North Dakota	$3,500,000
Michigan	$3,300,000

Source: State budgets and health departments via Associated Press report, "Millions in tax dollars flow to anti-abortion centers in US." Kimberlee Kruesi. 02/05/2022.

Table V: North Carolina Funding: Carolina Pregnancy Care Fellowship

FY14	$250,000 federal funding, no state funding
FY15	$300,000 federal funding, no state funding
FY16	$300,000 federal funding, no state funding
FY17	$300,000 federal funding, no state funding
FY18	$400,000 federal funding, $1,300,000 state non-recurring funding
FY19	$400,000 federal funding, $1,000,000 state non-recurring funding
FY20	$400,000 federal funding, $400,000 state non-recurring (carry forward)
FY21	$400,000 federal funding, no state funding

Source: North Carolina Department of Health and Human Services.

Table VI: CPC Services

	No. of CPCs	% Share
Over-the-Counter Pregnancy tests	93	0.80
After abortion support	76	0.66
Ultrasound services	65	0.56
Adoption agency or adoption support	52	0.45
Abstinence education in schools	42	0.36
Abortion reversal pill consult/provison.	29	0.25
Off-site partnership with physician	25	0.22
STI testing	20	0.17
N	116	

Source: Birthright database (2019). Author review of CPC websites.

8 Appendix B. Instrument Construction

8A. Results from the CPC Logit Model

Table VII presents estimates from the hazard model. Column (1) provides the baseline specification used to construct instruments. An existing CPC reduces the probability of additional openings in the same county, indicating market saturation or coordination to avoid duplication. Greater driving distance to the nearest CPC increases the probability of a new opening, confirming CPCs target underserved areas. The hazard model controls are harmonized with the second-stage specification: log population, age shares (10–19, 20–29), nonwhite share, unemployment, GOP vote share, Protestant share, and other-religions share. Lagged abortion rates enter the hazard model to capture CPC targeting of high-abortion areas but are frozen at 1990 values during the forward simulation (§4D..2).

The coefficient on distance to the nearest abortion provider is close to zero, but by 1990 all but one county with an abortion provider already had a nearby CPC, suggesting proximity to providers was prioritized in earlier expansion. The negative coefficient on GOP vote share is surprising; one possibility is that high Republican vote share proxies for areas

already saturated with informal pro-life support networks, reducing the marginal value of a formal CPC.

Table VII: Predicting the Opening of Crisis Pregnancy Centers

	(1)	(2)
No. of CPCs (lagged)	-0.050	-0.057
	(0.017)	(0.016)
Nearest CPC (lagged)	0.001	0.001
	(0.001)	(0.001)
Nearest Clinic (lagged)	-0.000	-0.000
	(0.000)	(0.000)
Abortion rate per 1000 women 10–44 (lagged)	0.001	-0.001
	(0.003)	(0.004)
Log population (lagged)	0.000	0.000
	(0.000)	(0.000)
Pop. share age 10–19 (lagged)	0.072	0.018
	(0.553)	(0.603)
Pop. share age 20–29 (lagged)		
Non-white share (lagged)	0.186	0.386
	(0.554)	(0.605)
Unemployment rate (lagged)	0.001	0.004
	(0.003)	(0.006)
U.S. GOP vote share (lagged)	-0.068	-0.076
	(0.037)	(0.038)
Protestant share (lagged)	0.113	0.174
	(0.224)	(0.187)
Other religions share (lagged)		
N	2,129	1,987
State FE	Yes	Yes
County FE	Yes	Yes
Time Trend (6th degree)	Yes	No
Year FE	No	Yes

Notes: Estimates from the renewal model of CPC location choice. All independent variables lagged one period. Average marginal effects via delta method; each coefficient is a percentage-point change in the probability of a new CPC opening. Standard errors in parentheses. Protestant share combines mainline and evangelical denominations; other religions share combines Catholic, Black Protestant, and other denominations. Pop. share age 20–29 and other religions share are omitted from display because their coefficients are near zero and statistically insignificant across both specifications. Population enters in logs. Lagged abortion rates enter the estimation but are frozen at 1990 values during the forward simulation (§4D..2).

Table VIII: Variable Treatment in the Forward Simulation

Category	Variables	Rationale
Updated within simulation	CPC count, distance to nearest CPC, polynomial time trend	Endogenous to simulated expansion process
Held at observed values	Demographics, religious composition, political ideology, unemployment, distance to nearest abortion provider	Evolve in data but treated as exogenous to CPC openings within each draw
Frozen at 1990	Lagged abortion rates	Avoids embedding assumed treatment effects

Notes: "Updated" variables change recursively within each of the $S = 1{,}000$ simulation draws as new CPCs open. "Observed" variables enter at their actual panel values in each county-year. "Frozen" variables are fixed at 1990 cross-sectional values throughout the simulation. County fixed effects in the second stage absorb the frozen cross-sectional level.

8B. Renewal Framework for Multiple Openings

Counties can receive multiple CPCs over the sample period. I extend the single-spell hazard to a renewal framework following Mroz (2012). The renewal decomposition expresses the count distribution as a sequence of independent binary events, each estimable by logit regardless of the true count-generating process. State dependence enters through $\text{CPC}_{c,t-1}$, so the hazard at each step conditions on accumulated stock. The outcome $\text{NewCPC}_{c,t}$ resets to zero each year and can equal one multiple times for the same county across t, with the likelihood taking the standard pooled logit form:

$$\mathcal{L} = \prod_{c} \prod_{t} p_{c,t}^{\text{NewCPC}_{c,t}} (1 - p_{c,t})^{1-\text{NewCPC}_{c,t}} \tag{10}$$

where $p_{c,t}$ is defined in Equation 6. The random utility derivation proceeds as follows. Organizations compare opening a new CPC in county c at time t to maintaining the status

quo:

$$U_{c,t}(d) = \begin{cases} \beta' W_{c,t-1} + \eta_{c,t} & \text{if } d = 1 \\ 0 & \text{if } d = 0 \end{cases}$$

where $\eta_{c,t} \sim$ Type I Extreme Value is the latent utility shock. An organization opens a CPC when $U_{c,t}(1) > U_{c,t}(0)$, yielding the logistic probability in Equation 6. The latent utility shock $\eta_{c,t}$ is distinct from the observed-outcome timing shock $\varepsilon_{c,t}^{\text{DGP}}$ in the decomposition (Equation 5): $\eta_{c,t}$ is the unobserved component of organizational utility, while $\varepsilon_{c,t}^{\text{DGP}}$ is the residual gap between actual CPC presence and its conditional expectation given observables.

8C. Monte Carlo Integration

The instrument $\text{IV}_{c,t} = \mathbb{E}[\text{CPC}_{c,t} \mid W_{c,t-1}]$ requires computing a conditional expectation that is analytically intractable due to path dependence. CPC presence at time t depends recursively on all prior shocks through the cumulative stock, requiring a $(t - 1990)$-dimensional integral. Monte Carlo integration resolves this: each of $S = 1{,}000$ draws generates a complete shock sequence, forward-simulates the expansion, and computes $\text{CPC}_{c,t}^{(s)}$. By the law of large numbers, $\text{IV}_{c,t} = S^{-1} \sum_s \text{CPC}_{c,t}^{(s)} \xrightarrow{\text{a.s.}} \mathbb{E}[\text{CPC}_{c,t} \mid W_{c,t-1}]$, with approximation error $O_p(S^{-1/2})$ independent of dimensionality (McFadden, 1989).

9 Appendix C. Formal Identification

The main text presents identification through a single predetermination assumption (§4C.). This appendix develops the formal two-assumption structure, clarifies the error term hierarchy, and establishes the conditions under which one-period predetermination is

equivalent to conditioning on the full observable history.

9A. Two-Assumption Decomposition

The main text's predetermination assumption can be decomposed into two conditions that clarify the distinct roles of the exclusion restriction and first-stage validity.

Assumption 1 (Conditional Orthogonality): Actual organizational timing shocks are mean-independent of unobserved fertility shocks, conditional on observable history and fixed effects:

$$\varepsilon_{c,t}^{\text{DGP}} \perp u_{c,t} \mid \mathcal{F}_{c,t-1}, \mu_c, \lambda_t \tag{11}$$

where $\mathcal{F}_{c,t-1} = \{W_{c,\tau}\}_{\tau=1}^{t-1}$ denotes the historical information set. This is a sequential ignorability condition (Robins et al., 1992): after conditioning on county fixed effects, year fixed effects, and past county characteristics, the remaining variation in *when* CPCs open must be uncorrelated with contemporaneous unobserved shocks to fertility outcomes.

The 2SLS estimator exploits deviations between actual and predicted CPC presence: $v_{c,t} = \text{CPC}_{c,t}^{\text{actual}} - \mathbb{E}[\text{CPC}_{c,t} \mid W_{c,t-1}]$. Under the structural model, $v_{c,t} \approx \varepsilon_{c,t}^{\text{DGP}}$, making Assumption 1 the key condition ensuring first-stage validity. Identification requires that *actual DGP* timing shocks satisfy this condition. The simulation draws $\varepsilon_{c,t}^{(s)}$ satisfy mean independence mechanically by design, but this is not the source of identification. The simulation provides a computational method to approximate $\mathbb{E}[\text{CPC}_{c,t} \mid W_{c,t-1}]$; identification derives from the economic argument that actual organizational timing is mean-independent of unobserved fertility shocks conditional on $\mathcal{F}_{c,t-1}$, μ_c, and λ_t.

Assumption 2 (Predetermination): Observable county characteristics are prede-

termined with respect to current-period fertility shocks:

$$\mathbb{E}[u_{c,t} \mid W_{c,t-1}, \mu_c, \lambda_t] = 0. \tag{12}$$

Since the instrument converges to $\mathbb{E}[\text{CPC}_{c,t} \mid W_{c,t-1}]$ and is measurable with respect to $W_{c,t-1}$, predetermination directly establishes the exclusion restriction:

$$\begin{aligned}
\mathbb{E}[u_{c,t} \cdot \text{IV}_{c,t} \mid \mu_c, \lambda_t] &= \mathbb{E}[\mathbb{E}[u_{c,t} \cdot \text{IV}_{c,t} \mid W_{c,t-1}, \mu_c, \lambda_t]] && (13)\\
&= \mathbb{E}[\text{IV}_{c,t} \cdot \mathbb{E}[u_{c,t} \mid W_{c,t-1}, \mu_c, \lambda_t]] && (14)\\
&= 0 && (15)
\end{aligned}$$

Division of labor. Assumption 2 delivers the exclusion restriction for the instrument via the law of iterated expectations. Assumption 1 ensures the first-stage residual variation—driven by actual timing shocks—is valid for identification. Under Assumptions 1–2, the first-stage residuals satisfy $\mathbb{E}[v_{c,t} \cdot u_{c,t} \mid X_{c,t}, \mu_c, \lambda_t] = 0$ even if the hazard model is misspecified, provided the approximation error—being a function of $W_{c,t-1}$—is spanned by the second-stage controls and fixed effects.

One-period predetermination is equivalent to full-history conditioning under two maintained conditions: (i) lagged CPC counts and distances are cumulative state variables encoding the full placement history by construction ($\text{CPC}_{c,t-1} = \sum_{\tau \leq t-1} \text{NewCPC}_{c,\tau}$), and (ii) the fertility outcome follows an approximately first-order Markov process conditional on μ_c and $X_{c,t-1}$. If abortion rates exhibit higher-order serial dependence not captured by a single lag, additional lags would be needed.

9B. Optimal Instruments

Chamberlain (1987) established that $\text{IV}^*_{c,t} = \mathbb{E}[\text{CPC}_{c,t} \mid W_{c,t-1}]$ attains the semiparametric efficiency bound in i.i.d. settings; Newey (1990) showed estimated optimal instruments preserve asymptotic efficiency. The panel setting differs, but the core intuition carries over: the conditional expectation extracts maximal predictive content from $W_{c,t-1}$. Consistency does not depend on this choice; approximating the conditional expectation improves efficiency but is not required for identification.

9C. Comparison to Difference-in-Differences

DID requires parallel counterfactual trends; IV requires only that timing shocks be orthogonal to contemporaneous fertility shocks conditional on observable history. Under heterogeneous effects, the IV identifies a LATE (Imbens & Angrist, 1994), while DID identifies an ATT.

9D. Dynamic Over-Identification

Under predetermination, every lag of each predetermined variable satisfies $\mathbb{E}[W_{c,t-s} \cdot u_{c,t}] = 0$ for all $s \geq 1$. With $K = 10$ covariates and $T = 30$ years, approximately 290 moment conditions hold simultaneously. Rather than constructing 290 separate instruments, the forward simulation aggregates this information into a single strong instrument ($F \approx 125$) through the structural hazard model. The F-statistic is stable across alternative hazard specifications, Hansen J-statistics from alternative constructions do not reject valid overidentification, and the second-stage estimate is stable across specifications. Dynamic overidentification provides robustness but does not weaken the core assumptions; passing J-tests

provides testable implications consistent with predetermination, not proof that it holds.

10 Appendix D. Monte Carlo Validation

This appendix details the Monte Carlo validation summarized in §4F..

10A. Design

The 3×3 factorial design crosses three true treatment effects ($\beta \in \{-0.10, -0.20, -0.30\}$) with three DGP specifications (correct specification with additive confounder Z_c; nonlinear confounding via Z_c^2; heterogeneous effects by county age composition), each repeated for 100 replications. Synthetic outcomes are generated on the actual panel structure (146 counties, 1990–2019), with residuals bootstrapped from an initial OLS regression. The hazard model is deliberately misspecified: estimated from 1990 baseline data, omitting Z_c. Three estimators are compared: naive OLS, oracle OLS controlling for Z_c (infeasible), and forward-simulation IV.

10B. Results

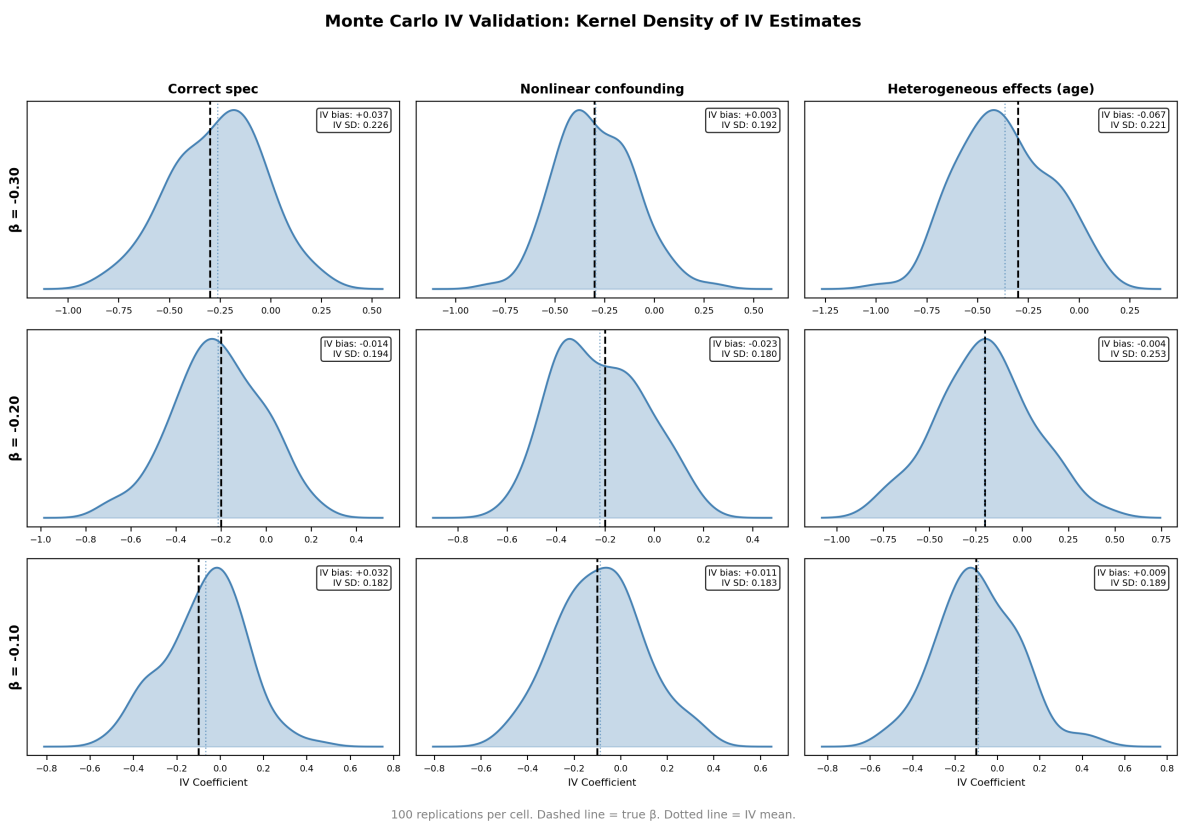
Table IX reports summary statistics. Naive OLS is severely biased (mean $\approx +1.5$, wrong sign). The IV recovers the true parameter across all nine cells, with bias below 0.07 in absolute value and first-stage $F \approx 78$ throughout. Figure IX presents the kernel densities, centered on the true β across all panels.

Table IX: Monte Carlo Validation: Summary Statistics Across DGP Specifications

True β	DGP	Mean OLS	Mean IV	Bias OLS	Bias IV	SD(IV)	Mean F	Reps
−0.10	Correct spec	+1.620	-0.068	+1.720	+0.032	0.183	78	100
	Nonlinear confounding	-0.086	-0.089	+0.014	+0.011	0.184	78	100
	Heterogeneous effects (age)	+1.629	-0.091	+1.729	+0.009	0.190	78	100
−0.20	Correct spec	+1.526	-0.214	+1.726	-0.014	0.195	78	100
	Nonlinear confounding	-0.167	-0.223	+0.033	-0.023	0.181	78	100
	Heterogeneous effects (age)	+1.535	-0.204	+1.735	-0.004	0.255	78	100
−0.30	Correct spec	+1.453	-0.263	+1.753	+0.037	0.227	78	100
	Nonlinear confounding	-0.230	-0.297	+0.070	+0.003	0.193	79	100
	Heterogeneous effects (age)	+1.443	-0.367	+1.743	-0.067	0.222	78	100

Notes: Each cell reports summary statistics from Monte Carlo replications. True β is the known treatment effect in the synthetic DGP. "Correct spec" uses linear, additive confounding. "Nonlinear confounding" replaces Z with Z^2. "Heterogeneous effects (age)" introduces treatment effect heterogeneity scaled by county age composition (share aged 20–29). Mean OLS uses the confounded treatment variable; Mean IV uses the forward-simulation instrument. Bias = Mean estimate − True β. F is the first-stage Kleibergen-Paap F-statistic.

Figure IX: Monte Carlo IV Validation: Kernel Density of IV Estimates



Notes: Kernel density of IV coefficient estimates across the 3×3 Monte Carlo design (100 replications per cell).

Rows correspond to true effects ($\beta = -0.30, -0.20, -0.10$); columns correspond to DGP specifications (correct, nonlinear confounding, heterogeneous effects by age). Dashed black line marks the true β; dotted blue line marks the IV mean. Each replication estimates a misspecified logit from 1990 baseline data (omitting Z_c), forward-simulates through 2019, and implements 2SLS.

10C. Time-Varying and Interaction Confounders

The time-varying confounder results (Table XXI) are presented in §5D.. Table X extends the confounder design to an *interaction* that operates through the product of two observables: $Z_{c,t} = Z_c + \delta \cdot t \cdot X_{1c} \cdot X_{2c}$, where X_{1c} is standardized Republican vote share and X_{2c} is standardized Protestant share. Unlike the single-variable case, controlling for each predictor's year interaction separately ($X_{1c} \times$ year and $X_{2c} \times$ year) does not absorb the product trend $X_{1c} \cdot X_{2c} \cdot t$. The middle column confirms this: bias at $\delta = 0.15$ remains -31% even with separate $X_{kc} \times$ year FE. The right column adds the product interaction $X_{1c} \cdot X_{2c} \times$ year FE as the absorber and reduces bias to under 4% with nominal coverage throughout. The bias direction here is negative (overstating the effect in absolute value) rather than attenuating toward zero, because the product $X_{1c} \cdot X_{2c}$ correlates with the instrument differently than either X_{1c} or X_{2c} alone.

Table X: IV Performance Under Interaction Confounder

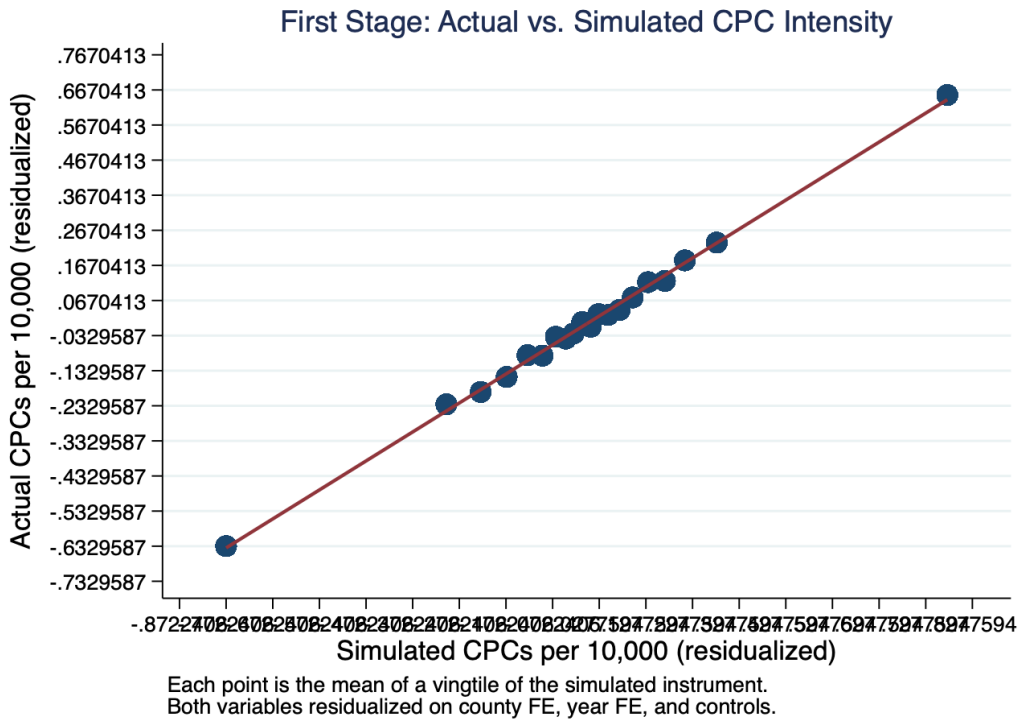
	Omitted			$X_{1c}, X_{2c} \times$ Year			$X_{1c} \cdot X_{2c} \times$ Year		
δ	$\hat{\beta}$	Bias (%)	Cov.	$\hat{\beta}$	Bias (%)	Cov.	$\hat{\beta}$	Bias (%)	Cov.
0.03	-0.199	0.7	0.99	-0.210	-4.8	0.96	-0.198	0.8	0.96
0.06	-0.215	-7.3	0.97	-0.240	-19.8	0.96	-0.233	-16.4	0.97
0.10	-0.269	-34.7	0.94	-0.256	-27.9	0.92	-0.220	-10.2	0.94
0.15	-0.263	-31.3	0.98	-0.262	-30.9	0.93	-0.208	-3.8	0.95
True β					-0.200				
Replications					100				

Notes: Each cell reports IV results from 100 Monte Carlo replications. The time-varying confounder is $Z_{c,t} = Z_c + \delta \cdot t \cdot X_{1c} \cdot X_{2c}$ where X_{1c} is standardized Republican vote share and X_{2c} is standardized Protestant share. Unlike the single-variable case, the interaction $X_{1c} \cdot X_{2c}$ is not absorbed by individual $X_{kc} \times$ Year FE. "Omitted" includes no trending controls. "$X_{1c}, X_{2c} \times$ Year" adds each variable's year interactions separately (insufficient for the interaction confounder). "$X_{1c} \cdot X_{2c} \times$ Year" adds the product interaction with year dummies (correct absorber).

11 Appendix E. First Stage Diagnostics

Figure X shows the binscatter relationship between actual and simulated CPC intensity, confirming strong first-stage relevance. Figure XI presents rolling-window first-stage coefficients across the sample period.

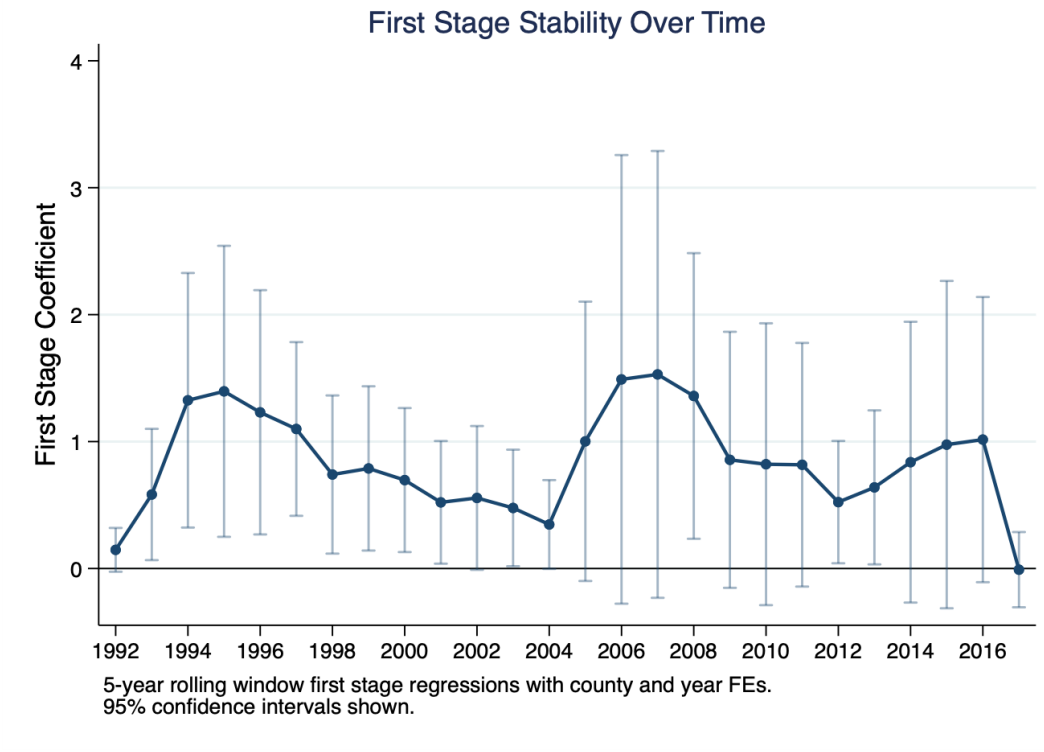
Figure X: First Stage: Actual vs. Simulated CPC Intensity



Notes:

Binscatter of actual CPC intensity (CPCs per 10,000 women) against simulated CPC intensity. Both variables residualized on county and year fixed effects and controls.

Figure XI: Rolling-Window First-Stage Coefficients



Notes:

First-stage coefficients from rolling 5-year windows across the sample period, with 95% confidence intervals.

Table XI reports results from a permutation test that randomly shuffles the instrument across counties 1,000 times. The actual first-stage F-statistic of 169 far exceeds the shuffled distribution (median 0.65, max 7.2), yielding $p = 0.000$ and ruling out overfitting as the source of instrument strength.

Table XI: Shuffled Instrument Permutation Test

	First-stage F	Reduced-form $\hat{\pi}$
Actual instrument	169.1	-0.1153
		(0.0345)
Shuffled median	0.652	-0.0033
Shuffled mean	1.611	-0.0027
Shuffled SD		0.0272
Shuffled 95th pctile	6.758	
Permutation p-value	0.0000	0.0000

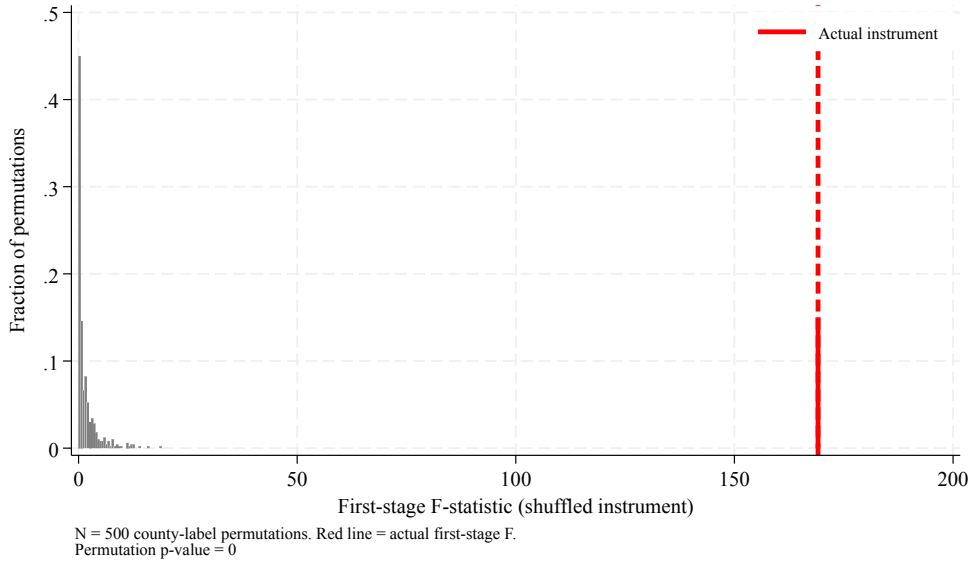


Figure XII: Permutation Distribution of First-Stage F-Statistic

Notes: Histogram of first-stage F-statistics from 500 permutations that randomly reassign county labels to instrument paths. The dashed red line marks the actual first-stage $F = 169$. The permutation p-value is 0.000.

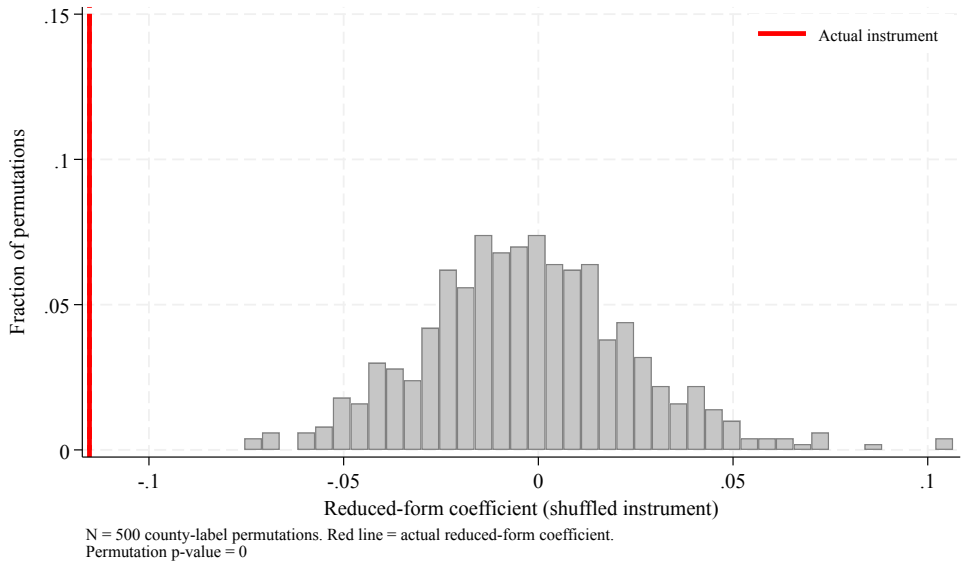


Figure XIII: Permutation Distribution of Reduced-Form Coefficient

Notes: Histogram of reduced-form coefficients from 500 permutations that randomly reassign county labels to instrument paths. The dashed red line marks the actual reduced-form estimate. The permutation p-value is 0.000.

Table XII re-estimates the IV using an alternative hazard specification that replaces the polynomial time trend with year fixed effects and drops lagged abortion rates from the predictor set. The abortion coefficient is virtually unchanged (-0.142 vs. baseline -0.145), confirming that the results do not depend on the particular hazard model specification.

Table XII: Sensitivity to Alternative Hazard Model Specifications

	County Aggregate				Age 25-29 Separate			
	(1) Baseline	(2) No abort	(3) No religion	(4) Cubic	(5) Baseline	(6) No abort	(7) No religion	(8) Cubic
CPCs per 10,000	-0.137***	-0.134***	-0.140***	-0.137***	-0.269***	-0.257***	-0.276***	-0.271***
	(0.039)	(0.039)	(0.040)	(0.039)	(0.080)	(0.081)	(0.082)	(0.079)
N	4,380	4,380	4,380	4,380	4,380	4,380	4,380	4,380
First-stage F	180.2	146.8	163.8	179.2	141.3	115.9	130.8	145.3

Notes: Each column re-estimates the full pipeline (hazard model, forward simulation, 2SLS) under an alternative hazard specification. County and year FE, controls, $\sqrt{\text{pop}}$ weights. Standard errors clustered at county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table XIII characterizes the complier population by interacting baseline county characteristics with the instrument. Complier counties have higher Protestant shares, more

Republican vote shares, and intermediate population sizes, consistent with the institutional narrative of CPC expansion through evangelical networks in mid-sized communities.

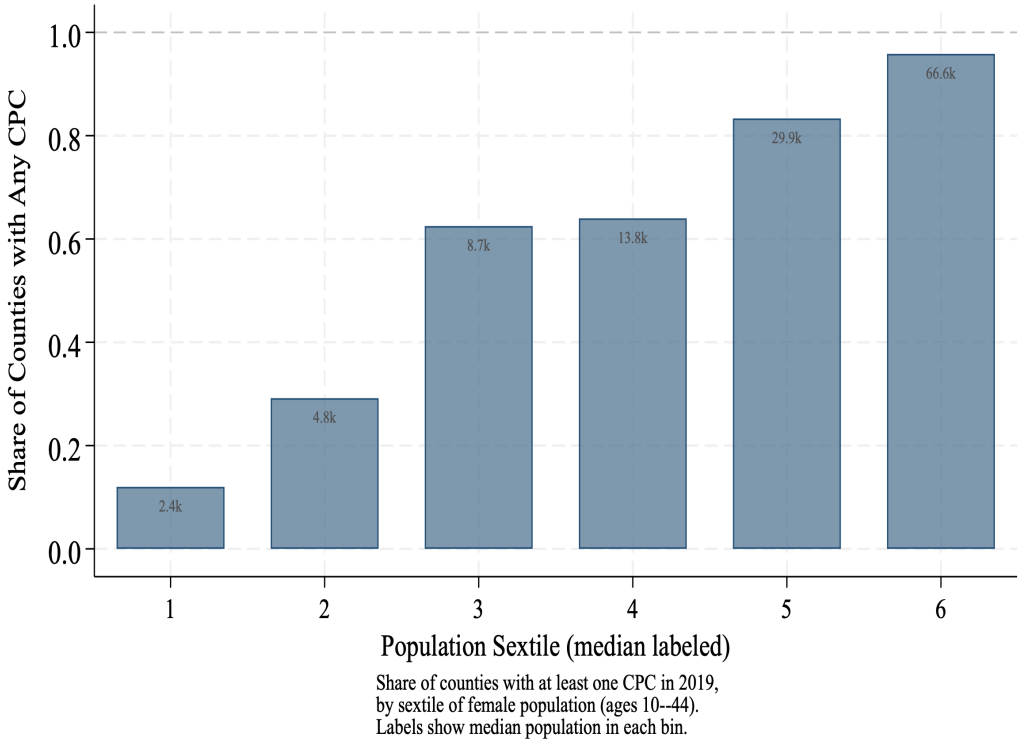
Table XIII: Complier County Baseline Characteristics (1990)

	COMP			
	Full Sample	Top Leverage Q	Bottom 3 Q	p-value
Aggregate Abort Rate	13.81	12.25	14.32	0.005
Aggregate Birth Rate	50.46	49.52	50.77	0.553
Aggregate Birth/Abort	4.10	4.25	4.04	0.493
Age 25-29 Abort Rate	15.82	13.70	16.52	0.028
Age 25-29 Birth Rate	83.31	87.93	81.80	0.395
Age 25-29 Birth/Abort	6.04	7.12	5.68	0.079
Age 20-24 Abort Rate	29.19	26.22	30.17	0.108
Age 20-24 Birth Rate	112.74	120.41	110.23	0.177
Age 20-24 Birth/Abort	4.94	5.42	4.79	0.414

Leverage quartiles based on SD of simulated CPC intensity across years.
Top quartile = counties most affected by the instrument.
Birth/Abort ratio = baseline birth rate / baseline abortion rate.
p-values from two-sample t-test with unequal variances.

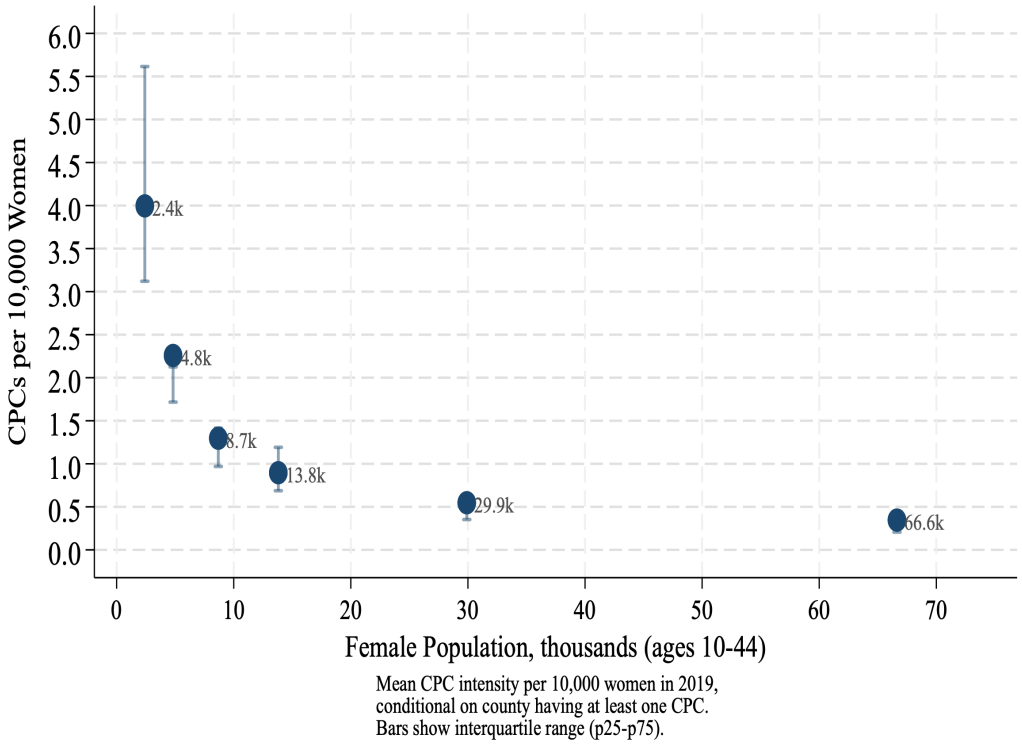
Figure XIV decomposes the cross-sectional relationship between county size and CPC presence. Nearly all large counties have at least one CPC by 2019, while only about 12% of the smallest sextile does. Figure XV conditions on counties with at least one CPC and plots mean per-capita intensity by population bin. The gradient is steep: the smallest counties average roughly 4 CPCs per 10,000 women, compared with 0.3 in the largest, an order-of-magnitude difference driven by the mechanical scaling of a discrete facility count against a small denominator. The interquartile range narrows with population, consistent with a law-of-large-numbers compression. Together the two panels show that small counties contribute both the extensive-margin variation (most lack any CPC) and the intensive-margin variation (those that do have one receive a far larger per-capita dose).

Figure XIV: Extensive Margin: Share of Counties with Any CPC by Population



Notes: Share of counties with at least one CPC in 2019, by sextile of female population ages 10–44. Labels show median population in each bin.

Figure XV: Intensive Margin: CPC Intensity Conditional on Any CPC



Notes: Mean CPC intensity per 10,000 women in 2019, conditional on the county having at least one CPC. Dots are bin means; bars show the interquartile range (p25–p75). Population bins use the same sextile cutpoints as Figure XIV.

12 Appendix F. AYFE Specification Analysis

This appendix presents the 2×2 diagnostic crossing estimation framework (separate regressions vs. AYFE) with weighting scheme (unweighted vs. $\sqrt{\text{pop}}$). Table XIV reports the full results. The AYFE specification (Columns 3–4) absorbs age $\times$ year fixed effects and allows age-group-specific treatment coefficients, removing the confounding from differential secular trends that steepens the abortion age gradient in the separate regressions. The abortion coefficients are roughly homogeneous under AYFE (-0.155 to -0.207, Column 4) compared with the wider range in separate regressions (-0.081 to -0.180, Table III). The diagnostic confirms that population weighting, not model structure, is the source of specification dependence in the birth result: moving from unweighted to $\sqrt{\text{pop}}$ attenuates birth coefficients in both frameworks, while moving from separate to AYFE does not systematically change them.

Table XIV: 2 × 2 Diagnostic: Estimation Framework × Weighting Scheme

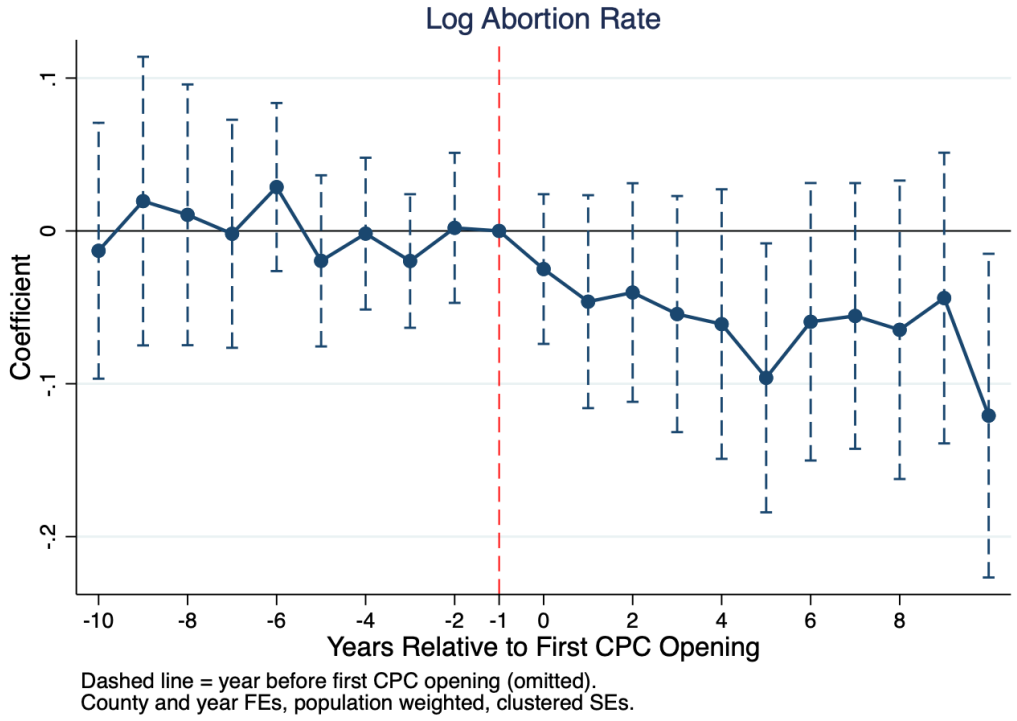
	(1) Separate Unweighted	(2) Separate $\sqrt{\text{pop}}$	(3) AYFE Unweighted	(4) AYFE $\sqrt{\text{pop}}$
Panel A: Log Abortion Rate				
Ages 10-19	-0.192**	-0.227***	-0.171**	-0.155***
	(0.084)	(0.069)	(0.067)	(0.060)
Ages 20-24	-0.226***	-0.225***	-0.201***	-0.207***
	(0.046)	(0.058)	(0.047)	(0.047)
Ages 25-29	-0.206***	-0.183***	-0.200***	-0.206***
	(0.060)	(0.056)	(0.051)	(0.047)
Ages 30-34	-0.088	-0.121**	-0.163*	-0.201***
	(0.086)	(0.061)	(0.085)	(0.058)
Ages 35-44	-0.197***	-0.161***	-0.178***	-0.178***
	(0.050)	(0.046)	(0.056)	(0.056)
First-stage F	133.4	132.6	28.3	27.1
N	6,152	6,146	30,131	30,122
RMSE	0.5284	0.4526	0.5511	0.4847
Panel B: Log Birth Rate				
Ages 10-19	0.028	-0.028	0.028	-0.002
	(0.036)	(0.036)	(0.041)	(0.052)
Ages 20-24	0.047*	-0.023	0.024	-0.006
	(0.025)	(0.022)	(0.035)	(0.037)
Ages 25-29	0.069***	0.043***	0.107***	0.058***
	(0.022)	(0.015)	(0.024)	(0.022)
Ages 30-34	0.129***	0.056***	0.131***	0.050*
	(0.027)	(0.019)	(0.029)	(0.029)
Ages 35-44	0.158***	0.068**	0.137***	0.019
	(0.035)	(0.028)	(0.039)	(0.035)
First-stage F	87.5	109.4	17.6	21.8
N	6,744	6,744	33,720	33,720
RMSE	0.3819	0.3076	0.4378	0.3958

Notes: Each column reports 2SLS coefficients on CPCs per 10,000 women from a different specification. Columns (1)–(2) use separate county-year regressions by age group; Columns (3)–(4) pool all age groups with age × year fixed effects (AYFE) and age-group-specific treatment interactions. Columns (1) and (3) are unweighted; Columns (2) and (4) use $\sqrt{\text{population}}$ weights with age-group-specific female population. RMSE is the root mean squared error of the second stage. For separate regressions, N and RMSE are per age group (ages 25–29 shown); for AYFE, N is pooled across all age groups. Standard errors clustered at county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

13 Appendix G. Event Study Details

Figures XVI and XVII present event study estimates for abortion and birth rates around first CPC opening. The key finding is flat pre-trends for both outcomes, supporting the identifying assumption. Post-treatment, abortion rates decline significantly while birth rates increase, consistent with the CPC-induced fertility substitution documented in the main results.

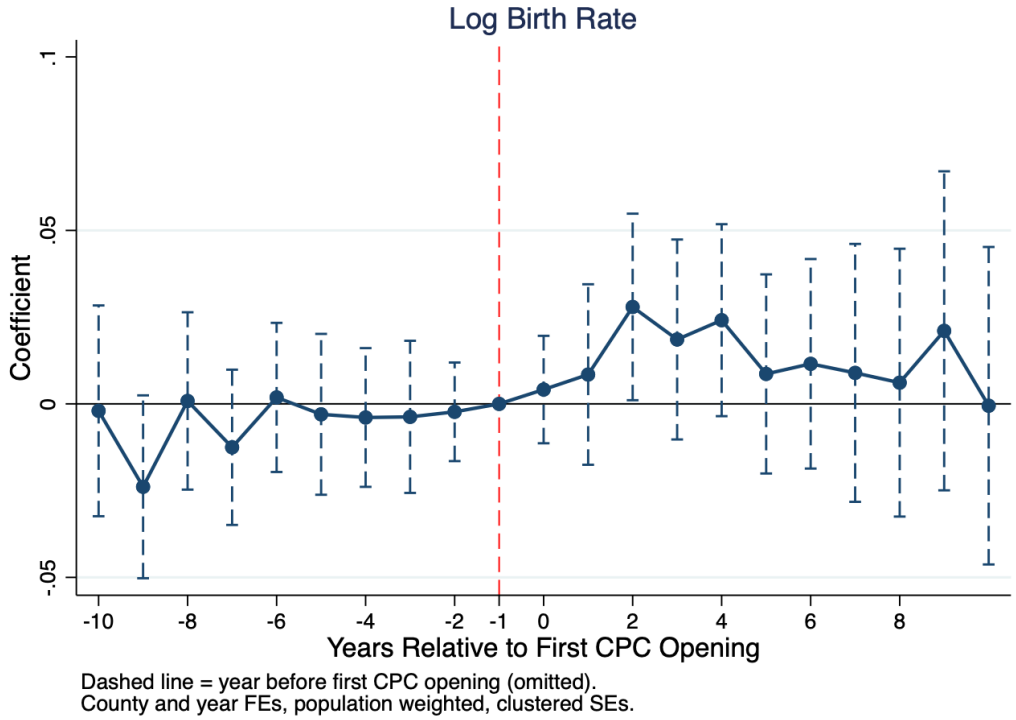
Figure XVI: Event Study: Log Abortion Rate



Notes: Event study coefficients for log abortion rate relative to first CPC opening. Period $t = -1$ is the omitted category. County and year FEs, controls, and population weights included. 95% CIs from SEs clustered at county level.

Table XV reports the full set of event study coefficients for both outcomes.

Figure XVII: Event Study: Log Birth Rate



Notes: Event study coefficients for log birth rate relative to first CPC opening. Period $t = -1$ is the omitted category. County and year FEs, controls, and population weights included. 95% CIs from SEs clustered at county level.

Table XV: Event Study Coefficients: Pre-Trend Comparison

	(1) Log Abort	(2) Log Birth	(3) Log Preg
$t = -10$	-0.0130	-0.0020	-0.0055
	(0.0422)	(0.0153)	(0.0138)
$t = -9$	0.0195	-0.0239*	-0.0172
	(0.0477)	(0.0133)	(0.0134)
$t = -8$	0.0105	0.0008	-0.0005
	(0.0430)	(0.0129)	(0.0123)
$t = -7$	-0.0019	-0.0125	-0.0130
	(0.0377)	(0.0113)	(0.0107)
$t = -6$	0.0287	0.0019	0.0043
	(0.0277)	(0.0108)	(0.0096)
$t = -5$	-0.0196	-0.0030	-0.0083
	(0.0283)	(0.0117)	(0.0102)
$t = -4$	-0.0018	-0.0039	-0.0067
	(0.0251)	(0.0101)	(0.0091)
$t = -3$	-0.0197	-0.0037	-0.0066
	(0.0221)	(0.0111)	(0.0100)
$t = -2$	0.0020	-0.0023	-0.0041
	(0.0248)	(0.0072)	(0.0069)
$t = -1$	0.0000	0.0000	0.0000
	(.)	(.)	(.)
$t = 0$	-0.0250	0.0041	-0.0023
	(0.0247)	(0.0078)	(0.0076)
$t = 1$	-0.0463	0.0085	0.0022
	(0.0352)	(0.0131)	(0.0120)
$t = 2$	-0.0403	0.0279**	0.0195
	(0.0361)	(0.0136)	(0.0119)
$t = 3$	-0.0544	0.0186	0.0085
	(0.0390)	(0.0145)	(0.0131)
$t = 4$	-0.0610	0.0241*	0.0125
	(0.0445)	(0.0140)	(0.0115)
$t = 5$	-0.0961**	0.0086	-0.0034
	(0.0444)	(0.0145)	(0.0114)
$t = 6$	-0.0594	0.0116	0.0040
	(0.0458)	(0.0152)	(0.0118)
$t = 7$	-0.0556	0.0089	0.0019
	(0.0439)	(0.0188)	(0.0153)
$t = 8$	-0.0647	0.0061	-0.0013
	(0.0493)	(0.0195)	(0.0157)
$t = 9$	-0.0439	0.0211	0.0152
	(0.0480)	(0.0232)	(0.0188)
$t = 10$	-0.1209**	-0.0005	-0.0156
	(0.0534)	(0.0231)	(0.0192)
N	3,356	3,360	3,360
Adj. R^2	0.718	0.935	0.933

Notes: Event time relative to first CPC opening in the county. Period $t = -1$ is the omitted category. All specifications include county and year FEs, exogenous controls, and population weights. Standard errors clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

14 Appendix H. Alternative Estimators and Functional Forms

14A. Synthetic Control Estimates

Both estimators below use binary treatment (first CPC entry) and identify the ATT for the extensive margin, a different estimand from the continuous IV's marginal effect of CPC intensity. Agreement is therefore a calibration check rather than a replication.

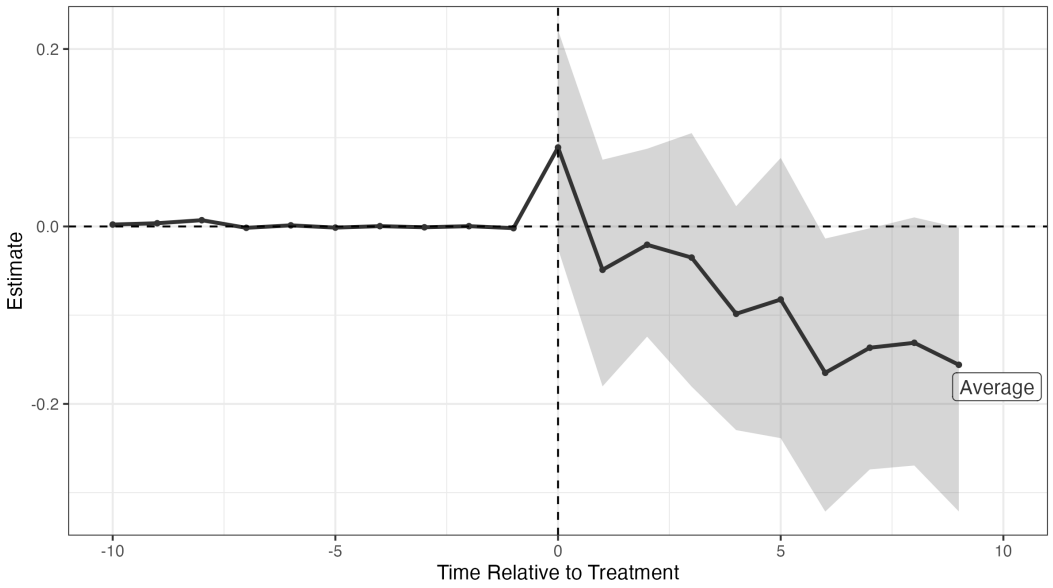
The augmented synthetic control estimator (Ben-Michael, Feller, & Rothstein, 2021) produces negative post-treatment ATTs for abortion rates (Figure XVIII) and positive ATTs for births (Figure XIX), with effects building gradually over 5–10 years. The SCPI estimator (Cattaneo, Feng, & Titiunik, 2021) provides finite-sample valid inference (Table XVI, Figures XX–XXI). The county-level log abortion ATT is -0.069, which reconciles with the IV at the correct dose: $-0.145 \times 0.5 \approx -0.075$, within sampling error. The near-null aggregate birth ATT (0.005) is consistent with birth effects concentrating at high per-capita doses that the binary treatment definition dilutes.

Table XVI: Synthetic Control with Prediction Intervals (Cattaneo, Feng, and Titiunik 2021)

	(1) Abort Rate	(2) Log Abort	(3) Birth Rate	(4) Log Birth
CPC (SC)	-0.6673	-0.0692	0.5045	0.0048
	[0.5119]	[0.0492]	[1.9619]	[0.0401]
Method	SC	SC	SC	SC
Inference	Pred. Int.	Pred. Int.	Pred. Int.	Pred. Int.

Notes: Synthetic control estimates with prediction intervals following Cattaneo, Feng, and Titiunik (2021). Treatment is the presence of at least one CPC in the county (staggered adoption, absorbing). For each treatment cohort (counties first treated in the same year), treated counties are averaged into a single series and a synthetic control is constructed from the never-treated donor pool. Reported ATTs are cohort-size-weighted averages across treatment cohorts. Values in brackets are pseudo-standard errors derived from the width of the 95% prediction intervals (half-width / 1.96). SC weights are constrained to the simplex (non-negative, sum to one). Prediction intervals account for both in-sample uncertainty (pre-treatment fit) and out-of-sample uncertainty (extrapolation).

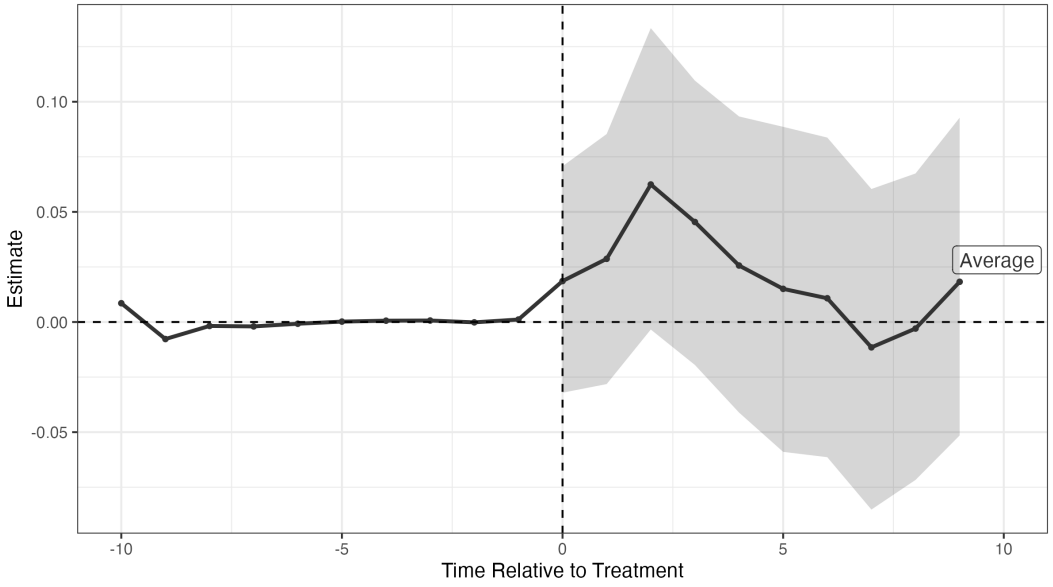
Figure XVIII: Multisynth: Log Abortion Rate



Notes:

Augmented synthetic control estimates (Ben-Michael et al., 2021) for log abortion rate. Staggered adoption design with CPC opening as binary treatment. Average treatment effect on the treated across cohorts.

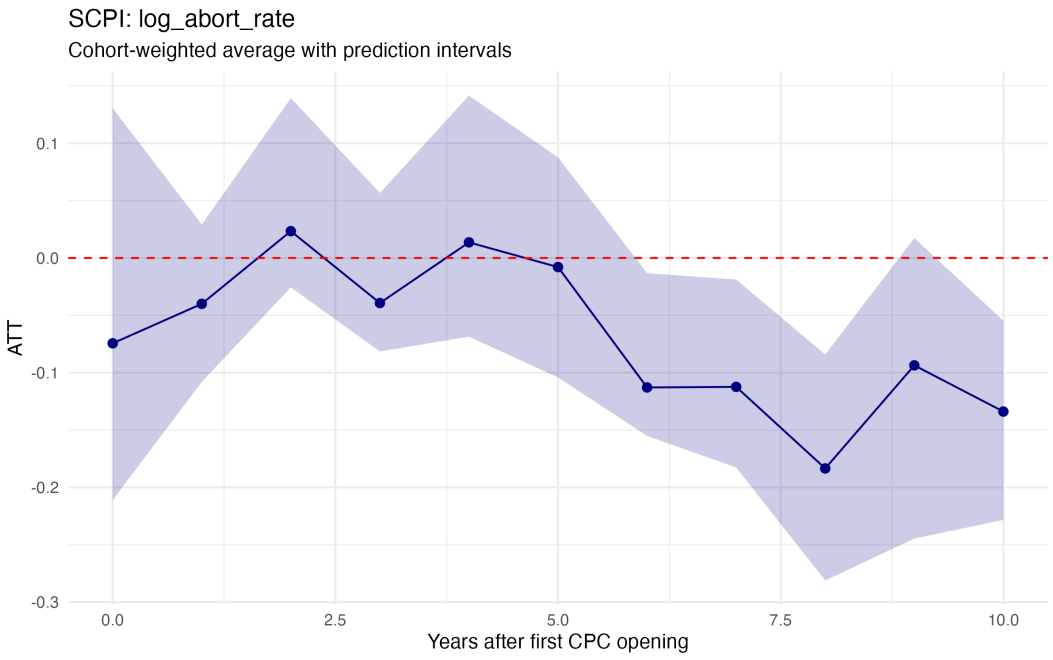
Figure XIX: Multisynth: Log Birth Rate



Notes:

Augmented synthetic control estimates (Ben-Michael et al., 2021) for log birth rate. Staggered adoption design with CPC opening as binary treatment. Average treatment effect on the treated across cohorts.

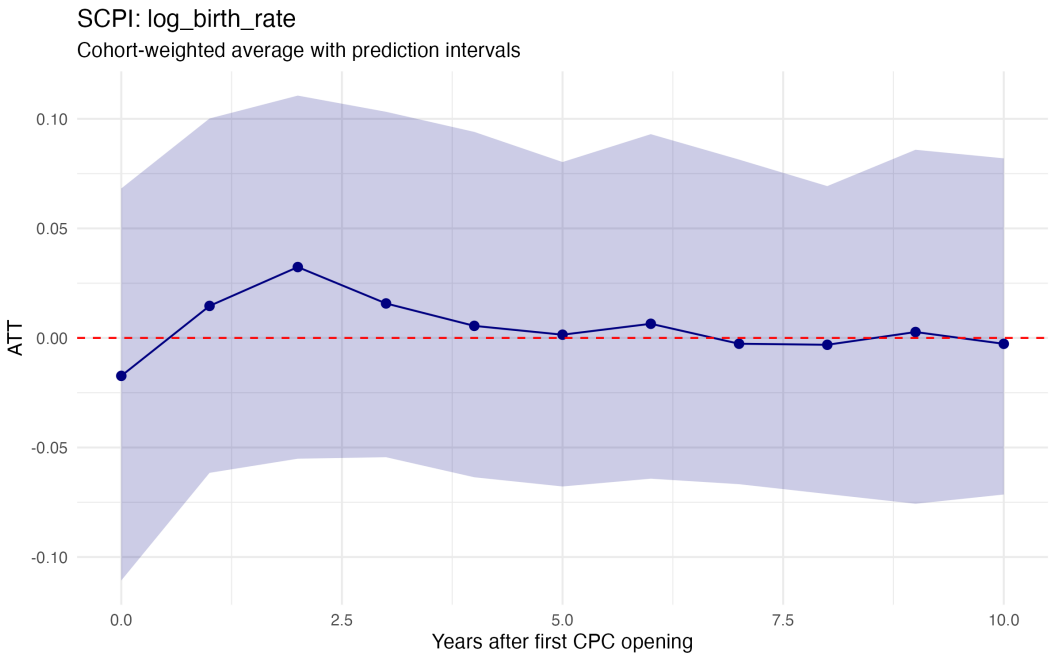
Figure XX: SCPI Estimates: Log Abortion Rate



Notes:

Synthetic control with prediction intervals (Cattaneo et al., 2021). Solid line shows the gap between treated and synthetic control units for log abortion rate. Shaded region shows 95% prediction intervals.

Figure XXI: SCPI Estimates: Log Birth Rate



Notes:

Synthetic control with prediction intervals (Cattaneo et al., 2021). Solid line shows the gap between treated and synthetic control units for log birth rate. Shaded region shows 95% prediction intervals.

14B. Poisson Control Function Estimates

Table XVII presents the Poisson control function results for abortion and birth rates at the county level, with both analytic and bootstrapped standard errors. The control function approach (Wooldridge, 2014) accounts for endogeneity in the count model setting by including first-stage residuals as additional regressors.

Table XVII: Poisson Control Function: County-Level Results

	Abortion		Birth	
	Analytic	Bootstrap	Analytic	Bootstrap
	Abortion		Birth	
CPCs per 10,000	-0.078*	-0.078*	0.022	0.022
	(0.044)	(0.044)	(0.018)	(0.019)
CF residual (endog. test)	0.094*	0.094*	-0.020	-0.020
	(0.051)	(0.049)	(0.019)	(0.021)
N	4,380	4,380	4,380	4,380

Notes: Poisson regression with control function (Wooldridge 2014). Exposure variable is county female population. Analytic SEs from delta method; bootstrap SEs from 200 replications resampling counties.

Table XVIII documents that the instrument significantly predicts having a zero abortion rate for 4 of 5 age groups, implying sample selection correlated with the instrument in the log specification. IHS and $\log(\text{rate} + 0.1)$ transformations that retain zero observations produce generally larger coefficients.

Table XVIII: Zero Abortion Rates by Age Group and Decade

	Age Group				
	10–19	20–24	25–29	30–34	35–44
Panel A: Zero abortion rates by decade					
1990-1999	9 (0.6%)	3 (0.2%)	17 (1.2%)	33 (2.3%)	42 (2.9%)
2000-2009	29 (2.0%)	14 (1.0%)	22 (1.5%)	40 (2.7%)	53 (3.6%)
2010-2019	66 (4.5%)	24 (1.6%)	39 (2.7%)	52 (3.6%)	80 (5.5%)
Total	104 (2.4%)	41 (0.9%)	78 (1.8%)	125 (2.9%)	175 (4.0%)
Panel B: Sample sizes by transformation					
log(rate)	4,276	4,339	4,302	4,255	4,205
$\log(\text{rate} + 0.1)$	4,380	4,380	4,380	4,380	4,380
$\sinh^{-1}(\text{rate})$	4,380	4,380	4,380	4,380	4,380
Panel C: Reduced form on zero indicator					
Coefficient	0.0264	0.0141	0.0204	0.0242	0.0322
	(0.0090)	(0.0085)	(0.0087)	(0.0067)	(0.0167)
p-value	[0.004]	[0.099]	[0.021]	[0.000]	[0.056]

Notes: Panel A reports the number of county-year-age observations with zero abortion rates, by age group and decade (percentage of age-group cell total in parentheses). Panel B shows the resulting sample sizes under each transformation. Panel C reports the reduced-form coefficient from regressing an indicator for zero abortion rate on the simulated CPC instrument with county and year fixed effects, county controls, and analytic weights. A significant coefficient indicates non-random sample selection when zeros are dropped.

Table XIX presents level-specification birth rate results by age group. Birth effects are statistically significant for all five age groups in both separate regressions and AYFE, consistent with the substitution arithmetic derived from the log specifications.

Table XIX: Separate Regressions by Age: Birth Rate (Level)

	(1)	(2)	(3)	(4)	(5)
	10-19	20-24	25-29	30-34	35-44
CPCs per 10,000	5.101***	21.695***	25.252***	26.305***	8.775***
	(1.828)	(7.047)	(5.319)	(5.611)	(1.833)
N	4,380	4,380	4,380	4,380	4,380
First-stage F	129.4	131.3	134.2	139.9	123.4

Standard errors in parentheses

Each column is a separate IV regression for one age group.

County and year FEs, controls, population weights. SEs clustered by county.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

15 Appendix I. Robustness: Specification and Sample Sensitivity

15A. Trending Confounder Controls

Table XX: Robustness to County Characteristic × Year FE Interactions

heightCPCs per 10,000 women	-0.145***	-0.137***	-0.139***	-0.138***	-0.131***	-0.134***
	(0.040)	(0.038)	(0.039)	(0.038)	(0.036)	(0.036)
X_c× Year FE	None	Protestant × Year	Republican × Year	Unemp. × Year	All three × Year	All + P×R×Yr
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	4,376	4,376	4,376	4,376	4,376	4,376
R-squared	0.113	0.103	0.096	0.069	0.055	0.055
First-stage F-stat	169.059	158.898	168.130	170.421	161.600	159.858

Notes: Dependent variable is log abortion rate. All columns are IV using the simulated instrument. X_c× Year FE flexibly absorb county-specific trends along observable dimensions correlated with CPC expansion. Protestant share combines mainline and evangelical denominations. Column 6 adds the product of Protestant share and Republican vote share interacted with year dummies, directly absorbing trends operating through the interaction of the two primary CPC-placement predictors. The baseline specification (Column 1) excludes X_c× Year FE because these interactions absorb not only confounding trends but also treatment effect heterogeneity along X_c; the attenuated estimates in Columns 2–6 are therefore a lower bound. County and year fixed effects, population weights, and the baseline control set included in all columns. Standard errors clustered at county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

15B. Functional Form: Log vs. Level Specifications

Table XXII combines the progressive restriction and quartile split analyses. The log coefficient attenuates from −0.145 (full sample) to near zero when restricting to above-median counties, reflecting a mechanical property of the log transformation: low-rate counties

Table XXI: IV Performance Under Increasing Trending Confounder Intensity

	$X_c \times$ Year FE Controlled			$X_c \times$ Year FE Omitted		
δ	Mean $\hat{\beta}$	Bias (%)	Coverage	Mean $\hat{\beta}$	Bias (%)	Coverage
0.03	-0.199	0.6	0.95	-0.167	16.6	0.95
0.06	-0.225	-12.4	0.92	-0.120	39.8	0.94
0.10	-0.206	-3.2	0.94	-0.040	80.0	0.85
0.15	-0.179	10.3	0.93	0.050	125.0	0.66
True β			-0.200			
First-stage F ($\delta = 0.03$)	66.8			80.3		
First-stage F ($\delta = 0.06$)	66.8			81.0		
First-stage F ($\delta = 0.10$)	66.8			82.0		
First-stage F ($\delta = 0.15$)	66.8			83.1		
Replications			100			

Notes: Each row reports IV results from 100 Monte Carlo replications at a given trending confounder intensity δ. The true treatment effect is $\beta = -0.20$ throughout. The time-varying confounder is $Z_{c,t} = Z_c + \delta \cdot t \cdot X_c$ where $Z_c \sim N(0,1)$ is a county-level draw (absorbed by county FE), X_c is standardized baseline Republican House vote share, and t is centered calendar time. The trending component $\delta \cdot t \cdot X_c$ creates county-specific confounding trends correlated with CPC expansion that county and year fixed effects cannot absorb. "$X_c \times$ Year FE Controlled" includes interactions between X_c and year dummies in the second-stage regression, which flexibly absorbs the trending confounder $\delta \cdot t \cdot X_c$. Controlling for X_c alone is redundant with county FE and does not absorb the trending component. "$X_c \times$ Year FE Omitted" excludes these interactions. Bias (%) is $(E[\hat{\beta}] - \beta)/|\beta| \times 100$. Coverage is the fraction of replications where the 95% confidence interval contains the true β. At the calibrated intensity ($\delta = 0.03$), trending confounding produces moderate IV attenuation when $X_c \times$ Year FE are omitted; including them restores performance across all intensities.

contribute disproportionately to proportional effects. Quartile-split estimates (Columns 1–4) show effects concentrated in the lowest quartile (-0.187, $p < 0.05$). The age heterogeneity results (Table III) are the more informative decomposition: abortion reductions among teenagers and women 20–24 are robust across specifications and do not depend on sample composition.

Table XXII: Sensitivity to Low Baseline Abortion Rates

	By Baseline Quartile					Progressive Restriction				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Q1	Q2	Q3	Q4	Full	<p10	<p25	<1/1k	<2/1k	<med
CPCs per 10k	-0.187**	-0.195	-0.126*	0.013	-0.145***	-0.114**	-0.071	-0.145***	-0.145***	-0.022
	(0.079)	(0.128)	(0.074)	(0.046)	(0.040)	(0.045)	(0.052)	(0.040)	(0.040)	(0.048)
N	1,076	1,110	1,110	1,080	4,376	3,958	3,300	4,376	4,376	2,190
Counties	36	37	37	36	146	132	110	146	146	73

Notes: Outcome is log abortion rate. Cols 1–4 split by within-county mean abortion rate quartile. Cols 5–10 progressively drop counties below each threshold. All specifications include county and year FEs, exogenous controls, and population weights. Standard errors clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

15C. Abortion Policy Landscape

Table XXIII adds state-level TRAP and parental involvement (PI) law indicators to the main IV specification for both abortion and birth outcomes. The CPC coefficient is virtually unchanged (-0.138 to -0.139 vs. baseline -0.145), confirming that state abortion restrictions do not confound the estimated CPC effect. Birth rate results are similarly robust.

15D. Clinic Access Stability

Table XXIV splits the sample by stability of abortion provider access for both outcomes. Restricting to counties always more than 30 miles from the nearest provider ($N = 101$, where clinic closures are irrelevant) yields a CPC coefficient of -0.153 ($F = 148$), nearly identical to the full-sample estimate. Stable-distance and changing-distance subsamples pro-

Table XXIII: Robustness to State Abortion Policy Controls

	Log Abortion Rate					Log Birth Rate		
CPCs per 10,000 women	-0.145***	-0.138***	-0.139***	-0.138***	-0.072**	0.019	0.010	0.010
	(0.040)	(0.038)	(0.039)	(0.038)	(0.028)	(0.012)	(0.013)	(0.013)
Policy controls	No	TRAP	PI	All	All	No	TRAP	All
N	4,376	4,376	4,376	4,376	4,376	4,380	4,380	4,380
R-squared	0.113	0.124	0.121	0.124	0.796	0.066	0.157	0.158
First-stage F-stat	169.059	167.692	167.764	167.598		169.160	167.794	167.700

Notes: Columns 1–4 report log abortion rate; Columns 6–8 report log birth rate. All IV specifications use the simulated instrument, with county and year fixed effects, population weights, and the baseline control set. Column 5 is OLS (TWFE). TRAP indicators are state × post-enactment dummies for NC building regulations (1994) and SC admitting privileges (1996). PI indicators are state × post-enactment dummies for parental consent laws (SC 1990, NC 1995). Standard errors clustered at county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

duce similar results. Birth rate results are similarly robust.

Table XXIV: CPC Effects by Stability of Abortion Provider Access

	Full	Stable dist.	Changing dist.	Always far (>30mi)
	Panel A: Log Abortion Rate			
CPCs per 10,000 women	-0.145***	-0.099**	-0.131**	-0.153***
	(0.040)	(0.042)	(0.064)	(0.044)
N	4,376	2,187	2,189	3,027
R-squared	0.113	0.142	0.103	0.108
First-stage F-stat	169.059	110.902	258.573	148.095
	Panel B: Log Birth Rate			
CPCs per 10,000 women	0.019	0.010	0.004	0.015
	(0.012)	(0.012)	(0.026)	(0.012)
N	4,380	2,190	2,190	3,030
R-squared	0.066	0.056	0.092	0.042
First-stage F-stat	169.160	110.924	258.601	148.100

Notes: Panel A reports log abortion rate; Panel B reports log birth rate. All columns are IV using the simulated instrument, with county and year fixed effects, $\sqrt{\text{pop}}$ weights, and baseline controls. Stable/Changing split at within-county median range of distance to nearest abortion provider. "Always far" restricts to counties where the nearest provider was always more than 30 miles away. Standard errors clustered at county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

15E. Oster Bounds on Selection

Following Oster (2019), Table XXV reports sensitivity bounds. Panel A applies the bounds to the hazard model's linear probability approximation; key predictors pass the $\delta > 1$

threshold despite low R^2 (~ 0.02, expected for a rare binary event). Panel B reports bounds for the OLS fertility estimates. For the log abortion rate, $\delta^* = 14.9$ at $R_{\max} = 1.3\tilde{R}^2$: unobservables would need to be roughly 15 times as important as all included controls to explain away the result.

Table XXV: Oster (2019) Bounds on Selection on Unobservables

Panel A: CPC Entry Hazard Model (Linear Probability Model)						
Variable	$\hat{\beta}_{short}$	$\hat{\beta}_{long}$	R^2_{short}	R^2_{long}	$\delta^*(R_{max}=1)$	$\delta^*(1.3\tilde{R}^2)$
Protestant share	0.031	0.010	0.007	0.022	0.01	0.77
Republican share	0.006	-0.015	0.007	0.022	-0.01	-1.49
Log population	0.017	0.019	0.015	0.022	0.02	1.57
Dist. to clinic	-0.000	0.000	0.009	0.022	-0.00	-0.01
Minority share	-0.049	-0.050	0.010	0.022	0.01	1.11
Abortion rate	0.001	0.000	0.007	0.022	0.00	0.11
Panel B: OLS Structural Equation (Fertility Outcomes)						
Outcome	$\hat{\beta}_{short}$	$\hat{\beta}_{long}$		$\tilde{R}^2$	$\delta^*(R_{max}=1)$	$\delta^*(1.3\tilde{R}^2)$
Log abortion rate	-0.086	-0.077		0.121	0.79	14.89
Log birth rate	0.011	0.010		0.067	0.72	30.96
Log pregnancy rate	0.011	0.009		0.116	0.63	15.34

Notes: $\delta^* > 1$ means unobservables would need to exceed observables to nullify the coefficient. Panel A uses a linear probability model without county FEs (Oster framework). Panel B residualizes county and year FEs before applying `psacalc`. Population-weighted.

15F. CPC Organizational Funding

Table XXVI tests whether CPC organizational capacity, proxied by IRS Form 990 total revenue, drives the abortion effect beyond CPC presence alone. The CPC $\times$ funding interaction is small and insignificant (Column 1), and per-capita funding has no independent predictive power (Column 2). When funding enters as an exogenous control in the IV specification (Columns 3–4), the CPC coefficient strengthens slightly, suggesting that unobserved heterogeneity in CPC resources does not confound the baseline estimate.

Table XXVI: CPC Effects with Organizational Funding Controls

	(1) Log Abort	(2) Log Abort	(3) Log Abort	(4) IHS Abort
CPCs per 10,000 women	-0.112*		-0.241**	-0.233**
	(0.059)		(0.100)	(0.101)
CPC × Funds ($100k$)	-0.009			
	(0.014)			
CPC funds ($100k$)	0.007		0.004	0.003
	(0.006)		(0.003)	(0.003)
CPC funds per 10k women		-0.000		
		(0.000)		
Estimator	OLS	OLS	IV	IV (IHS)
N	3,354	3,354	3,354	3,358
Adj. R-sq	0.5930	0.5888	0.0412	0.0383
First-stage F			28.6	28.6

Standard errors in parentheses

County-year, 1996–2018 (IRS filing years).

CPC funds from IRS Form 990 (total revenue, CPI-adjusted).

Col 1: OLS with CPC×*interaction*.

Col 2: OLS with per-capita CPC funding as treatment.

Cols 3–4: IV with funding as exogenous control.

County and year FE, controls, $\sqrt{\text{population}}$ weights.

Standard errors clustered by county.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

15G. Lagged Abortion Rate Control

Table XXVII: Robustness to Twice-Lagged Abortion Rate Control

	(1) Baseline	(2) With $AR_{c,t-2}$
heightCPCs per 10,000 women	-0.145***	-0.099***
	(0.040)	(0.034)
Twice-lagged log abortion rate		0.333***
		(0.041)
Twice-lagged abort. rate	No	Yes
County FE	Yes	Yes
Year FE	Yes	Yes
N	4,376	4,080
R-squared	0.113	0.214
First-stage F-stat	169.059	114.877

Notes: Dependent variable is log abortion rate. Both columns are IV using the simulated instrument, with county and year fixed effects, population weights, and the baseline control set. Treatment is $CPC_{c,t-1}$; the twice-lagged abortion rate $AR_{c,t-2}$ is predetermined relative to both treatment and outcome. The once-lagged rate $AR_{c,t-1}$ is contemporaneous with the treatment measure and mechanically absorbs the prior-year treatment effect; it is not reported. Standard errors clustered at county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

15H. Instrument Leverage and Influence Diagnostics

Table XXVIII: IV Estimates Excluding Predicted CPC Intensity Quartiles

Sample	$\hat{\beta}_{\text{IV}}$	SE	First-stage F	N	Counties
Full sample	-0.145	(0.040)	169.0	4,376	146
Drop Q1 (lowest)	-0.143	(0.044)	162.2	3,269	109
Drop Q4 (highest)	-0.126	(0.106)	71.6	3,297	110
Drop Q1 & Q4	-0.138	(0.121)	66.1	2,190	73

Notes: Counties are ranked by mean predicted CPC intensity (the county-level average of the simulated instrument) and assigned to quartiles. Each row re-estimates the baseline IV specification after excluding the indicated quartile(s). Q1 contains counties with the lowest predicted intensity; Q4 contains the highest. County and year fixed effects, baseline controls, and population weights included. Standard errors clustered at county level.

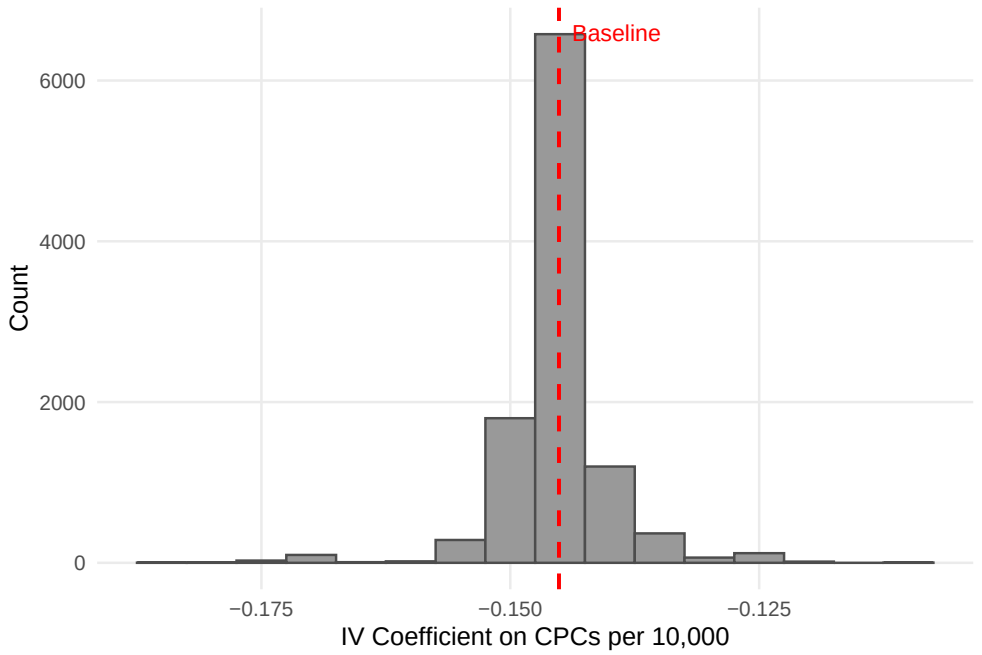


Figure XXII: Leave-Two-County-Out IV Coefficient Distribution

Notes: Leave-two-county-out jackknife. Each bar represents an IV coefficient estimate obtained by dropping one of the $\binom{N}{2}$ possible county pairs and re-estimating the baseline specification. The dashed red line marks the full-sample estimate. No county pair shifts the coefficient by more than 20% from the baseline.

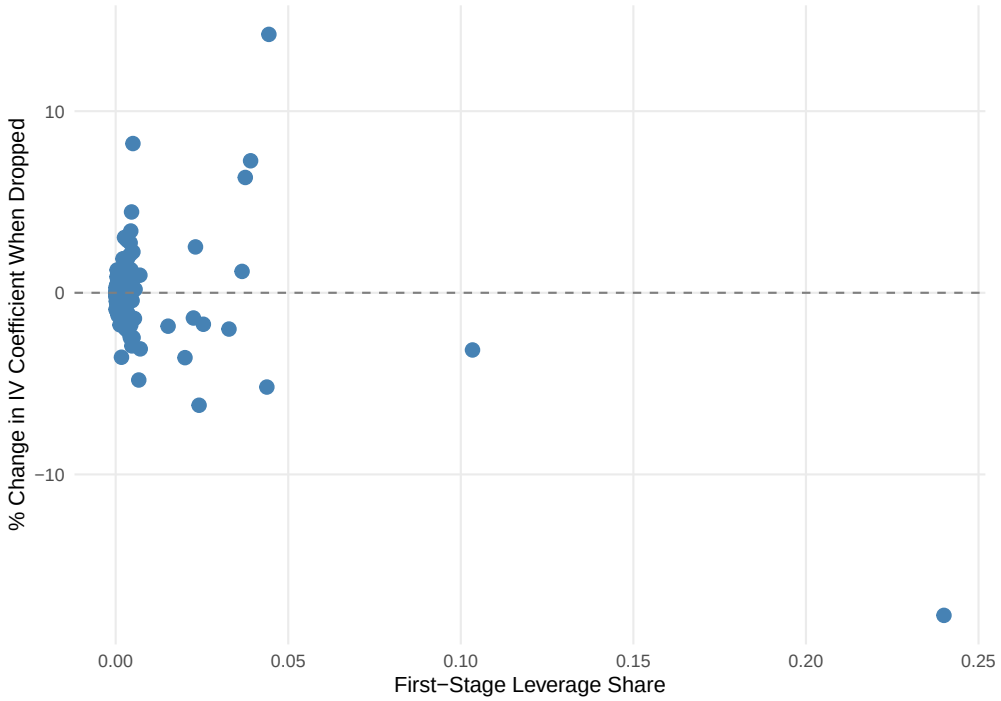


Figure XXIII: First-Stage Leverage and Coefficient Influence by County

Notes: Leave-one-county-out influence plot. Each point represents one county. The horizontal axis measures the county's share of total first-stage identifying variation (sum of squared partialled-out instrument values, weighted). The vertical axis measures the percentage change in the IV coefficient when that single county is dropped. High-leverage counties do not systematically push the coefficient in one direction.

15I. Polynomial Degree Sensitivity

Table XXIX: Sensitivity to Polynomial Degree in Hazard Model Time Trend

	(1) Linear	(2) Cubic	(3) Quartic	(4) Sextic (baseline)
CPCs per 10,000	-0.139***	-0.145***	-0.143***	-0.146***
	(0.041)	(0.040)	(0.040)	(0.040)
N	4,376	4,376	4,376	4,376
First-stage F	177.0	190.4	158.7	186.2

Notes: Each column re-estimates the full pipeline (hazard model, forward simulation with 1,000 draws, 2SLS) after replacing the baseline sixth-degree polynomial time trend in the hazard model with the indicated degree. The sextic column reproduces the baseline. Dependent variable is log abortion rate. County and year fixed effects, population weights, and the baseline control set included. Standard errors clustered at county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

15J. Border Analysis

Table XXX restricts the sample to counties within 50 miles of the NC–SC border, exploiting within-border-region variation in CPC timing. The abortion coefficient (-0.138, $p < 0.01$) is virtually identical to the full-sample estimate, and the birth coefficient is positive, confirming that the results are not driven by broad state-level trends.

Table XXX: Border County Analysis

	Log Abortion Rate				Log Birth Rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CPCs per 10,000	-0.145***	-0.118**	-0.113***	-0.145**	0.019	0.019	0.028**	0.019
	(0.040)	(0.056)	(0.036)	(0.057)	(0.012)	(0.021)	(0.013)	(0.014)
Sample	Full	Interior	Drop NC-SC border	Border only	Full	Interior	Drop NC-SC border	Border only
N	4,376	2,790	3,567	1,586	4,380	2,790	3,570	1,590
First-stage F	169.1	117.2	200.6	66.8	169.2	117.2	200.7	66.9

Standard errors in parentheses
County and year FEs, controls, population weights. SEs clustered by county.
Interior = counties not bordering any other state.
Drop NC-SC border = drop counties on the NC-SC boundary only.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

15K. Distance Gradient

Table XXXI reports the OLS CPC effect separately for abortion and birth rates. Figures XXIV and XXV decompose these effects by distance ring, showing a monotone decay that is near zero beyond 30 miles. Table XXXII confirms that the IV estimate is robust to controlling for distance to the nearest simulated CPC location.

Table XXXI: CPC Distance Gradient: OLS

	(1) Log Abort Rate	(2) Log Birth Rate
CPCs within 10mi (per 10k)	-0.119***	0.074***
	(0.040)	(0.017)
CPCs 10-20mi (per 10k)	-0.088**	0.047**
	(0.043)	(0.021)
CPCs 20-30mi (per 10k)	-0.023	0.022**
	(0.017)	(0.009)
CPCs 30+mi (per 10k)	-0.015	0.007
	(0.011)	(0.005)
N	4,376	4,380
Adjusted R-sq	0.6125	0.8403

Standard errors in parentheses
County-year aggregate (summed across age groups).
CPC counts within each distance ring from county centroid.
County and year FE, controls, $\sqrt{\text{population}}$ weights.
Standard errors clustered by county.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

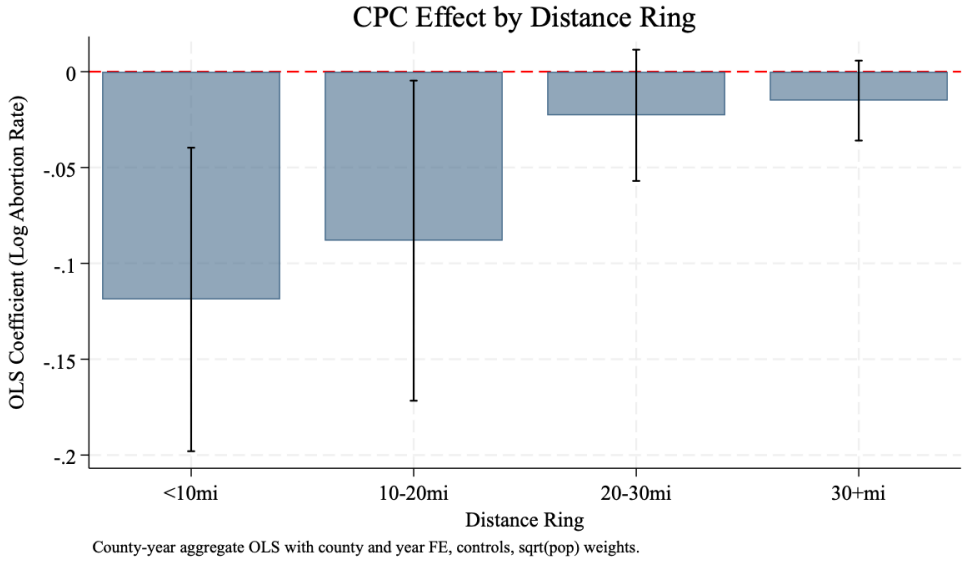


Figure XXIV: CPC Effect on Abortion Rate by Distance Ring (OLS)

Notes: OLS coefficients from a regression of log abortion rate on CPC counts (per 10,000 women) within each distance ring, with county and year fixed effects, controls, and $\sqrt{\text{pop}}$ weights. Bars show point estimates; whiskers show 95% confidence intervals. Standard errors clustered by county.

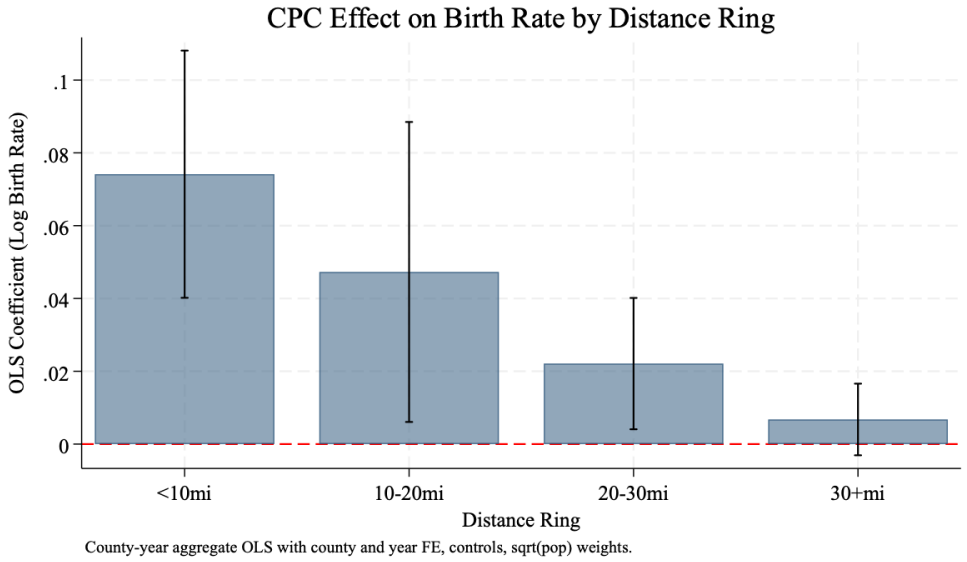


Figure XXV: CPC Effect on Birth Rate by Distance Ring (OLS)

Notes: OLS coefficients from a regression of log birth rate on CPC counts (per 10,000 women) within each distance ring, with county and year fixed effects, controls, and $\sqrt{\text{pop}}$ weights. Bars show point estimates; whiskers show 95% confidence intervals. Standard errors clustered by county.

Table XXXII: CPC Distance Gradient: IV Specifications

	(1) Baseline	(2) Near/Far	(3) Dist Control	(4) Dist Quadratic
CPCs per 10,000	-0.235***		-0.360***	-0.353***
	(0.077)		(0.111)	(0.108)
CPCs per 10k × near (<10mi)		-0.335**		
		(0.129)		
CPCs per 10k × far (≥10mi)		-0.229***		
		(0.080)		
Distance to sim CPC (mi)			-0.015***	-0.009
			(0.005)	(0.009)
Distance squared				-0.000
				(0.000)
Distance spec	None	Sim CPC <10mi	Dist control	Dist + Dist sq
N	4,376	4,376	4,376	4,376
First-stage F	129.8	48.8	137.7	135.9

Standard errors in parentheses

County-year aggregate, log abortion rate.

Col 2 splits treatment by whether nearest simulated CPC is within 10mi.

Distance interactions use simulated (predetermined) CPC locations.

County and year FE, controls, $\sqrt{\text{population}}$ weights.

Standard errors clustered by county.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

15L. Plausibly Exogenous Bounds

Table XXXIII implements the **?** procedure, allowing for direct effects of the instrument on fertility outcomes up to a specified threshold γ. The abortion coefficient remains negative and significant for plausible values of γ, indicating the result is robust to moderate violations of the exclusion restriction.

Table XXXIII: Plausibly Exogenous Bounds

	UCI Confidence Set for β (CPC $\rightarrow$ Outcome)		
Direct effect support $[\underline{\delta}, \bar{\delta}]$	Lower	Upper	Excludes zero?
Panel A: IHS Abortion Rate (Age 25–29)			
Baseline IV (no direct effect)	-0.2695	(0.0800)	Yes
$\delta \in [-0.02, 0.02]$	-0.4479	-0.0887	Yes
$\delta \in [-0.05, 0.05]$	-0.4887	-0.0507	Yes
$\delta \in [-0.10, 0.10]$	-0.5530	0.0133	No
Panel B: IHS Birth Rate (Age 25–29)			
Baseline IV (no direct effect)	0.1839		Yes (positive)
$\delta \in [-0.05, 0.05]$	0.0539	0.3234	Yes

15M. Post-2008 Falsification

Table XXXIV tests whether post-2008 ACA and policy changes confound the results by splitting the sample at 2008 and testing for structural breaks. The CPC coefficient is stable across sub-periods, and the interaction with the post-2008 indicator is not significant, ruling out post-ACA policy changes as a confound.

Table XXXIV: Post-2008 Falsification Test

	First Stage (DV: CPC density)			Reduced Form (DV: IHS abort rate)		
	(1)	(2)	(3)	(4)	(5)	(6)
Simulated CPC density	0.836***	0.841***	0.667**	-0.115***	-0.143***	0.070
	(0.064)	(0.125)	(0.290)	(0.034)	(0.036)	(0.075)
Period	1990-2019	1990-2008	2009-2019	1990-2019	1990-2008	2009-2019
N	4,380	2,774	1,606	4,380	2,774	1,606
R-squared	0.830	0.834	0.925	0.797	0.810	0.848
F-stat (first stage)						

Notes: Tests for structural breaks around 2008. Columns 1–3 are first-stage regressions of CPC density on the instrument; Columns 4–6 are reduced-form regressions of IHS abortion rate on the instrument. Standard errors clustered at county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

16 Appendix J. Heterogeneity

This appendix presents heterogeneity analyses by metro/nonmetro status (USDA Rural-Urban Continuum Codes 0–3 vs. 4–9) and marital status.

16A. Metro vs. Non-Metro

Table XXXV reports separate 2SLS estimates for metro and non-metro counties. The metro/non-metro classification follows the USDA Rural-Urban Continuum Codes (0–3 = metro, 4–9 = non-metro). Abortion coefficients are negative in both subsamples across all age groups, though precision varies with sample size. Birth coefficients in non-metro counties are positive for ages 25 and older, consistent with the dose interpretation: non-metro counties have higher per-capita CPC intensity, producing larger treatment effects per county.

16B. Marital Status

Table XXXVI tests whether CPCs disproportionately affect unmarried women, their stated target population. Columns 1–2 examine whether the unmarried share of abortions

Table XXXV: CPC Effects by Metro/Non-Metro Status (2SLS)

	(1) Metro (RUCC 0–3)	(2) Non-Metro (RUCC 4–9)
Panel A: Log Abortion Rate		
Ages 10–19	-0.180*	-0.221***
	(0.106)	(0.077)
Ages 20–24	-0.301***	-0.202***
	(0.105)	(0.062)
Ages 25–29	-0.224***	-0.176***
	(0.083)	(0.057)
Ages 30–34	-0.129	-0.117*
	(0.094)	(0.067)
Ages 35–44	-0.265***	-0.137***
	(0.095)	(0.047)
First-stage F	61.5	123.9
N (per age group)	1,897	4,249
Panel B: Log Birth Rate		
Ages 10–19	-0.022	-0.026
	(0.082)	(0.040)
Ages 20–24	-0.060	-0.017
	(0.065)	(0.023)
Ages 25–29	0.082	0.024*
	(0.062)	(0.014)
Ages 30–34	0.119*	0.035*
	(0.065)	(0.019)
Ages 35–44	0.134**	0.048*
	(0.067)	(0.028)
First-stage F	59.9	97.3
N (per age group)	1,943	4,801

Notes: Each column reports separate county-year 2SLS estimates on the metro or non-metro subsample. Metro defined as USDA Rural-Urban Continuum Codes 0–3; non-metro as 4–9. Treatment is CPCs per 10,000 women, instrumented with the forward simulation instrument. All specifications include county and year fixed effects, $\sqrt{\text{population}}$ weights, and the baseline control set. Standard errors clustered at county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

or births shifts with CPC exposure; Columns 3–4 decompose the abortion reduction by marital status using IHS-transformed marital-status-specific rates; Columns 5–6 report the corresponding birth rate decomposition.

Table XXXVI: Marital Status Decomposition (Age 25–29)

	Composition		IHS Abort Rate		IHS Birth Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
	Unmarr Share	Unmarr Share	Unmarried	Married	Unmarried	Married
CPCs per 10,000 women	0.0122	0.0259**	-0.1474***	-0.0821**	0.2080***	0.1457**
	(0.0134)	(0.0111)	(0.0402)	(0.0401)	(0.0525)	(0.0585)
N	4,094	4,374	4,380	4,380	4,380	4,380
First-stage F	141.8	134.2	134.2	134.2	134.2	134.2

Notes: County-year IV for women aged 25–29. Cols 1–2: Unmarried share of abortions/births (composition test). Cols 3–4: IHS unmarried/married abortion rate per total female pop. Cols 5–6: IHS unmarried/married birth rate per total female pop. County and year FE, controls, $\sqrt{\text{pop}}$ weights. Standard errors clustered by county. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

17 Appendix K. Dose-Response

Table XXXVIII reports the specification that jointly instruments CPC count and distance to nearest CPC using the simulated instrument and its implied distance counterpart. Both CPC count and distance are treated as endogenous. The first-stage F-statistic is 2.4, well below conventional thresholds, so the resulting coefficients are unreliable. The abortion coefficient (-0.265, s.e. 0.108) is larger than the baseline 2SLS but imprecise; the birth and pregnancy coefficients are similarly noisy. The weak identification reflects the high collinearity between CPC count and CPC distance after conditioning on county fixed effects. We report this specification for completeness but do not rely on it.

Following Card et al. (2023), Table XXXIX tests whether relative proximity to CPCs versus abortion clinics predicts fertility outcomes for both abortion and birth rates. Across all specifications, no differential distance measure achieves statistical or economic significance

Table XXXVII: Dose-Response Reconciliation: IV vs. Synthetic Control

	Log Abortion	Log Birth
Panel A: Method Comparison		
IV coefficient (per CPC/10k)	-0.145	0.019
	(0.040)	(0.012)
Mean treatment intensity (treated cty-yrs)	0.467	
IV-implied binary ATT	-0.068	0.009
SCPI binary ATT	-0.069	0.005
	[0.049]	[0.040]
Panel B: Level Decomposition (per CPC/10k)		
Baseline rate (per 1,000 women)	11.07	45.87
Implied change (per 1,000 women)	-1.61	0.86
Share of prevented abortions		53.6%

Notes: Panel A reconciles the IV per-unit dose-response estimates (Table II, Column 2) with the binary-treatment SCPI ATT. The IV estimates a marginal effect per CPC per 10,000 women. The SCPI compares counties with any CPC to never-treated counties, averaging over heterogeneous treatment intensities. The IV-implied binary ATT equals the IV coefficient multiplied by the mean treatment intensity among treated county-years (0.467 CPCs per 10,000, population-weighted). Panel B converts semi-elasticities to level changes using population-weighted baseline rates. The substitution share is the ratio of additional births to prevented abortions in level terms. Standard errors in parentheses (IV, clustered by county); pseudo-standard errors from prediction interval half-widths in brackets (SCPI).

Table XXXVIII: Joint CPC Count and Distance IV

	(1)	(2)	(3)
	Log Abort	Log Birth	Log Pregnancy
CPCs per 10,000	−0.265**	0.043	0.027
	(0.108)	(0.034)	(0.032)
Distance to nearest CPC	−0.012	0.003	0.003
	(0.016)	(0.006)	(0.005)
Distance squared	0.0000	−0.0000	−0.0000
	(0.0002)	(0.0001)	(0.0001)
N	4,376	4,380	4,380
First-stage F	2.4	2.4	2.4
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
$\sqrt{\text{pop}}$ weights	Yes	Yes	Yes

Notes: 2SLS jointly instrumenting CPC count and distance to nearest CPC using the simulated CPC instrument and its implied distance counterpart. Both endogenous regressors are instrumented. The first-stage F-statistic of 2.4 indicates weak identification; coefficients should be interpreted with caution. Standard errors clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

for either outcome. The null results are consistent with CPCs operating through community presence rather than spatial competition with abortion providers.

Table XXXIX: Differential Distance: Abortion and Birth Rates

	Log Abortion Rate			Log Birth Rate		
CPCs per 10,000 women	-0.145***		-0.145*	0.019		0.092***
	(0.040)		(0.075)	(0.012)		(0.031)
Dist. to clinic - Dist. to CPC (miles)		0.000			-0.000	
		(0.004)			(0.001)	
Log(clinic dist. / CPC dist.)						
CPCs per 10k x Clinic distance			0.000			-0.001***
			(0.001)			(0.000)
Nearest Clinic	-0.000	-0.001	-0.000	0.001	0.001	0.001***
	(0.001)	(0.004)	(0.001)	(0.001)	(0.001)	(0.000)
Specification	Baseline	Diff. dist. IV	CPC x Clinic dist.			
N	4,376	4,376	4,376	4,380	4,380	4,380
R-squared	0.113	0.111	0.113	0.066	0.065	0.070
First-stage F-stat	169.059	46.008	66.108	169.160	45.815	66.260

Notes: Columns 1–3 report log abortion rate; Columns 4–6 report log birth rate. Differential distance is distance to nearest abortion clinic minus distance to nearest CPC (miles). All columns use IV with the forward simulation instrument. County and year fixed effects, controls, and population weights included. Standard errors clustered at county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.