THE ROLE OF CRISIS PREGNANCY CENTERS IN FERTILITY DECISIONS*

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Abstract

Crisis pregnancy centers (CPCs) seek to reduce abortion incidence by providing counseling to pregnant women. Despite the presence of more than 2,500 CPCs across the United States, there is no causal evidence of their impact on abortion outcomes. This paper develops an instrumental variables strategy that constructs a plausibly exogenous measure of CPC presence by forward-simulating their expansion. Using a 30-year county-level panel from North and South Carolina, I estimate that CPC presence reduces abortion rates by 18 percent, with the largest effects among teenagers and young women. The results provide new evidence on how demand-side interventions shape reproductive decisions.

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

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1 Introduction

The federal legalization of induced abortion in the United States in 1973 fundamentally altered women's ability to make reproductive choices. In response, the pro-life movement established the first crisis pregnancy centers (CPCs) with the stated goal of reducing abortion incidence (Care Net, 2022).[1] CPCs' objective of reducing abortion is rooted in the religious belief that abortion is morally impermissible. From an economic perspective, the emergence of CPCs also represents a strategic response to changes in reproductive norms. Akerlof, Yellen, and Katz (1996) show that abortion legalization and improved contraceptive technology shifted the equilibrium of relationship and marriage markets. As unintended pregnancies no longer had to result in childbirth, men faced weaker incentives to commit to partners, thereby disadvantaging women who did not adopt the new reproductive technologies. This suggests that CPCs also aim to restore the pre-legalization marriage market equilibrium by promoting traditional norms around sexual behavior, contraception, and family formation.

Despite their prevalence and public funding, there is no causal evidence that CPCs succeed in reducing abortion incidence.[2] The literature has focussed on policies that led to abortion clinic closures, which substantially reduce abortion rates (Fischer, Royer, & White, 2018; Lindo, Myers, Schlosser, & Cunningham, 2020). These supply-side abortion restrictions

[1] Although abortion rates have broadly declined since the early 1990s, unintended pregnancies remain widespread: 45 percent of pregnancies are reported as mistimed or unwanted, and 42 percent of these result in abortion (Finer & Zolna, 2016).

[2] Between 2,500 and 4,500 centers operate across all U.S. states, and more than half of reproductive-age women live closer to a CPC than to an abortion provider (R. K. Jones & Jerman, 2017; McVeigh, Crubaugh, & Estep, 2017; Swartzendruber & Lambert, 2020; Thomsen, Baker, & Levitt, 2022). In the fiscal year 2021–22, approximately $89 million in federal and state funds supported CPC operations across a dozen states (Kruesi, 2022). According to a survey by the umbrella organization Care Net, their 1100 affiliated CPCs prevented 677,248 abortions between 2008 and 2017, or about 1 in 10 abortions (Care Net, 2022).

also profoundly shape women's educational and labor market outcomes.[3] Yet supply-side limits represent only one side of the coin. This paper addresses this gap by asking whether CPCs affect abortion rates through demand-side interventions. I study this question using a 30-year county-level panel from North and South Carolina and an instrumental-variables strategy that exploits quasi-random variation in CPC presence.[4]

The central identification challenge arises from potentially endogenous CPC location choices. Organizations strategically locate in response to unobserved local conditions that independently affect fertility outcomes, violating the exclusion restriction required for causal inference. If CPCs open where they expect to be most effective, simple outcome comparisons either overstate or understate their true effects. CPC expansion also responds to past outcomes, creating feedback that standard controls cannot address. And because CPC growth is gradual and not triggered by clear policy shocks, researchers cannot rely on natural experiments or difference-in-differences designs. As a result, prior work can describe CPC activities but cannot credibly identify their causal impact (Cartwright, Tumlinson, & Upadhyay, 2021; Rosen, 2012).

To address these challenges, I construct instruments by forward-simulating CPC expansion paths. I first estimate a hazard model of CPC openings using predetermined county characteristics; then I simulate thousands of counterfactual expansion histories in which independent organizational shocks and predicted opening probabilities determine where CPCs locate, updating county characteristics and projecting forward each year. Averaging simulated CPC presence across draws yields an instrument—the expected probability that a county hosts a CPC in year t—that isolates systematic placement patterns and removes the

[3] See Goldin and Katz (2002); Miller, Wherry, and Foster (2020); C. K. Myers (2017).

[4] Section 8A. documents the spatial distribution of CPCs and abortion providers over time.

idiosyncratic shocks driving actual expansion. This approach synthesizes optimal instruments theory (Amemiya, 1974; Chamberlain, 1987; Newey, 1990) with simulation-based estimation (McFadden, 1989). When treatment assignment is path-dependent and closed-form expressions for the IV are intractable, Monte Carlo simulation provides consistent approximations even with fixed draws per observation. Conceptually, the design aligns with recent identification strategies emphasizing that causal inference can arise from the structure of treatment assignment itself rather than from functional-form assumptions (Borusyak, Hull, & Jaravel, 2022, 2025).

Instrumental-variables estimates show that CPC presence reduces county abortion rates by 18 percent. These effects are concentrated among teenagers and young women. The IV estimate substantially exceeds the corresponding ordinary least squares (OLS) estimate (10 percent) in absolute magnitude, consistent with positive selection: CPCs tend to locate in counties with persistently high abortion demand (unobserved to the econometrician), causing OLS to understate true effects in absolute value. By isolating the systematic component of placement, the simulated instrument corrects this bias. Validation using synthetic data with known treatment effects demonstrates that the IV strategy successfully recovers true causal parameters while OLS remains substantially biased.

This paper makes three contributions. First, it provides the first causal evidence on the effectiveness of CPCs in reducing abortion incidence. As a result of the 2022 *Dobbs v. Jackson Women's Health Organization*[5] decision, which removed abortion access protections in many U.S. states, it is ever more important to understand the role of CPCs in shaping demand for abortions. Second, it shows that demand-side interventions—counseling, infor-

[5] Supreme Court of the United States (2022)

mation provision, and material support—can meaningfully shape fertility decisions. The 18 percent reduction in abortion rates is comparable to supply-side restrictions: (Lindo et al., 2020) find that clinic closures reduced abortion by approximately 20 percent. This demonstrates that policy debates should attend to both supply and demand dimensions of abortion access, expanding a literature focused primarily on provider restrictions. Third, it introduces a generalizable simulation-based IV method for settings with endogenous treatment and path dependence. When treatment assignment is strategic and sequential, standard design-based inference methods struggle to recover the causal estimand. Forward simulation provides a systematic way to construct instruments from the process by with treatment is assigned. By generating quasi-experimental variation, and without requiring natural experiments, the IV recovers the causal effect.

2 Background

2A. Crisis Pregnancy Centers

CPCs offer services relating to sexual behavior, pregnancy and relationships. Pregnancy counseling from a pro-life perspective by volunteers or staff is a core service. Typically, counseling is offered in conjunction with additional services, such as free over-the-counter pregnancy tests (Swartzendruber et al., 2018). When a pregnancy is confirmed, many CPCs offer limited ultrasounds intended to inform about the gestational age, heartbeat, and viability of the pregnancy.[6] CPCs also offer material aid, such as baby clothes, cribs, diapers, and direct financial assistance. Some CPCs refer to an adoption agency. Others provide ab-

[6]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 who are certified as registered diagnostic medical sonographers and that a small share of CPCs employ registered nurses, obstetrician-gynecologists and other medical professionals. Further, some CPCs have partnerships with offsite physicians.

stinence education or “sexual integrity” classes. Abstinence education is aimed at teenagers and is held either at the CPC or in schools.

CPCs are relatively small nonprofit organizations. On average, the organizations in the analysis sample had annual revenues of $230,275 in 2018. Some of this funding comes from the federal and state governments. In FY2021–22, $89 million in federal and state funding was allocated to CPCs across a dozen states (Kruesi, 2022). Historically, CPCs primarily received public funding in their role as providers of abstinence education programs. Over the past decade, CPCs have received increasing amounts of Temporary Assistance for Needy Families (TANF) funding and, for a few years, Title X funding for the provision of reproductive healthcare. North and South Carolina, the two states in my sample, provide some state funding in addition to distributing federal funds. In North Carolina, state and federal grants have been provided to the umbrella organization Carolina Pregnancy Care Fellowship, which directs funds to over 70 affiliated CPCs.[7] South Carolina's Department of Motor Vehicles, similarly to most state motor vehicle departments, sells “Choose Life” license plates, with the proceeds allocated to CPCs.

There is some 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 a CPC. CPC attendance was higher among Black women, women with lower incomes 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. This study also finds that living closer to a CPC is associated with greater odds of visiting a CPC and that

[7]Federal funds were first used in FY2014 and have been supplemented by state funding since 2018 (see Table VI). The North Carolina General Assembly began to designate money from the Title V Maternal and Child Health Block Grant (MCHBG) for the Carolina Pregnancy Care Fellowship in FY2013–14. The goal of MCHBG grants is to support the health and well-being of mothers, children, and families.

women who report having visited a CPC are 21 percentage points less likely to have had an abortion.

Systematic evidence on the information content of CPC counseling is lacking, but public health researchers have analyzed the information that CPCs provide online, which is indicative of their counseling content. Swartzendruber et al. (2018) find that CPC websites contain false and misleading health information, that the advertised services do not align with prevailing medical guidelines, and that 58 percent of CPC websites fail to disclose that they do not provide abortion services or refer clients to an abortion provider. Some of these findings have been corroborated by Rosen (2012), who identify that CPC websites commonly provide inaccurate information on the medical risks of abortion. It is thus unsurprising that Cartwright et al. (2021) report that 58% of CPC clients are unaware of CPCs' pro-life mission and or are even seeking abortion services. Similarly, (Swartzendruber, Solsman, & Lambert, 2021) finds that many CPC clients hold misconceptions about CPC policies and practices.

CPCs are almost universally unregulated. Most CPCs are not licensed medical facilities, meaning that medical ethics rules and patient privacy laws are not applicable. In instances when women have felt misled by CPCs, state attorneys have mostly declined to open investigations because CPCs do not charge fees for their services (Office of the Attorney General, New York, 2002). The most consequential attempt at regulating CPCs is the California Reproductive Freedom, Accountability, Comprehensive Care, and Transparency Act (FACT Act; CA AB 775), passed in 2015. This legislation intended to limit CPC practices deemed deceptive, particularly regarding anti-abortion counseling.[8] In the 2018

[8]Under the law, unlicensed CPCs would have had to disclose to their clients in writing, or post on a sign, that the center is not a licensed medical facility and has no medical staff to provide services. The disclosure requirement extended to advertising. However, some CPCs in California are licensed medical providers. The FACT Act required licensed CPCs not providing a full range of reproductive care to post a sign informing clients that the state provides free or low-cost access to reproductive care, including abortions.

decision *National Institute of Family & Life Advocates v. Becerra*, the Supreme Court of the United States deemed the FACT Act unconstitutional on the grounds that it violates the first amendment, which protects free speech.

2B. Abortion Access

In North and South Carolina, abortion was legal during the entire period under consideration in this paper. Over time, state laws placed restrictions on abortion providers, required waiting periods, and prohibited health insurance reimbursement for abortion services.[9]

In North Carolina, a 1973 law legalized abortion up to 20 weeks of gestation (NC G.S. 14-45.1). In 1995, the state enacted a parental consent law requiring minors to obtain parental consent or judicial bypass before obtaining an abortion (NC G.S. 90-21.6 through 90-21.10). A 1994 TRAP (Targeted Regulation of Abortion Providers) law imposed building and facility requirements on abortion clinics (K. M. Jones & Pineda-Torres, 2024). Since 2011, abortion providers in North Carolina have been required to consult with a patient at least 72 hours before an abortion procedure. The state also requires the physical presence of the physician for both surgical and medical abortions (NC G.S. Ch. 90, Art. 1I.). In 2023, outside the study period, legal abortion was limited to 12 weeks' gestation, except under specified circumstances (NC G.S. 90-21.81B).

In South Carolina, a 1974 law broadly legalizes abortion up to 24 weeks of gestation and under specified circumstances beyond 24 weeks of gestation (SC Code Ann. §§ 44-41-

[9] As a result of the 1977 Hyde amendment, certain federal funds, including Medicaid funding, cannot be used to pay for pregnancy termination except in cases of rape, incest, and health conditions threatening the life of the pregnant woman. Both North and South Carolina implemented these restrictions in state law. Federal Title X funds also cannot be used to pay for abortion services.

20). In 1990, the state enacted a parental consent law for minors (C. Myers & Ladd, 2020). In 1995, state law was revised such that any health care provider that performs at least five abortions a month must be licensed as an abortion clinic, subject to new regulations and inspection at any time (SC Code Ann. §§ 44-41-75). A 1996 TRAP law required abortion facilities to maintain hospital admitting privileges and transfer agreements (K. M. Jones & Pineda-Torres, 2024). In 2010, Sec. 44-41-330 was amended to include a mandatory 24-hour waiting period following an abortion consultation.

These policy changes had heterogeneous effects on abortion access across counties and demographic groups. Parental involvement laws particularly affected teenagers, who may have traveled to bordering states without such requirements (C. Myers & Ladd, 2020). TRAP laws led to some facility closures, as clinics unable to comply with building codes or admitting privileges requirements ceased operations. North Carolina experienced approximately five facility closures, representing a reduction of nearly 20 percent in abortion service providers during this period. South Carolina maintained only one facility providing abortion services (located in Greenville) throughout this period, though additional family planning clinics provided abortion referrals.[10]

Gestational limits for abortion remained unchanged between 1990 and 2019, but the cumulative effect of TRAP laws, parental involvement requirements, waiting periods, and facility closures increased barriers to abortion access over time. Several recent studies have shown that increased travel distances to abortion clinics because of closures reduce the abortion rate (Fischer et al., 2018; Lindo et al., 2020; Quast, Gonzalez, & Ziemba, 2017),

[10] My analysis includes both facilities providing abortion services directly and those providing referrals to other facilities. The distinction is noted in maps in Section 8A.. While referrals differ from direct service provision in terms of access barriers, both types of facilities represent information channels through which women learn about abortion options.

and increase birth rates (Lu & Slusky, 2019).

3 Data

I study the location choice of CPCs and the effect of CPCs on the abortion rate. These analyses require detailed longitudinal data on CPCs, specifically the geographic location and timing of opening. Second, outcome measures of abortions and births are needed. These fertility rates are constructed from vital statistics and Census data. Third, data on county characteristics (including unemployment, election vote shares, religiosity) are required. I focus on North and South Carolina for several reasons. These states, unlike many others, provide fertility counts by age, ethnicity and county of residence starting in the year 1990, allowing me to study CPC openings and the fertility outcomes of women by age group over a long time horizon. Summary statistics of the variables used in the analysis for the time period 1990 to 2019, and the first and last year of the sample are reported in Table **??**.

3A. CPC and Clinic Data

Obtaining a longitudinal registry of CPCs is an important contribution of this paper. I construct a dataset of CPC and abortion clinic addresses, as well as information on the dates of their operation. The dataset contains 288 CPC locations (addresses) associated with 138 CPC organizations and 43 abortion provider locations (and referrers). These data are the basis for the study of how CPCs affect local abortions. I thus observe the opening of CPCs in North and South Carolina between 1990 and 2019, the period for which fertility data are available. Figures I to IV show CPCs and abortion providers over time, overlaid on a map of county-level abortion rates in NC and SC. Table I provides descriptive statistics.

footnoteThe number of locations exceeds the number of CPC organizations because some organizations operate multiple facilities and because address changes are tracked. For a given CPC organization, a relocation (“move”) is defined as the closure of a facility at one address and the opening of a facility at a location in the same county within a year. I validate the CPC data by comparing four sources: (1) A database maintained by the umbrella organization Birthright that contains the majority of CPCs, both its affiliates and independent CPCs; (2) tax filings to the Internal Revenue Service; (3) CPC websites; and (4) Yellow Pages entries. In the case of CPCs, I can also observe some address changes and closures in financial fillings from news articles.

Data on abortion clinics are sourced from records of Title X grant recipients, which include many abortion providers, which are provided by the United States Department of Health and Human Services (HHS) for the years 2013 to present. I categorize some clinics as “referrers” if they do not provide abortion services but have provided them at some point in time or are part of a network, such as Planned Parenthood, of abortion providers. This data is verified using Myer’s Abortion Facility Database (C. Myers & Ladd, 2020).

Furthermore, I obtain state licensing information on abortion clinics in NC and SC. I cross-check this information with the provider lists of the National Abortion Federation and Planned Parenthood and through a generic online search of newspaper reports. The resulting dataset provides the precise geographic location of each facility and allows me to track at what point in time facilities open and close.

I use proxies of women’s exposure to CPCs in the analysis. The main treatment variables is defined as the number of CPCs per 10,000 women of age 10-44 in a county.[11]

[11] A typical county in the sample has approximately 10,000 women of childbearing age. Adjusting the treatment variable by the population in the service area has precedent in the abortion access literature (Lindo et al., 2020).

3B. Fertility Rates

The primary outcome of interest is the (log) abortion rate in a county, which is constructed using the abortion count per 1,000 women, for the period from 1990 to 2019.[12] Abortion and birth counts were obtained from administrative and vital records provided by vital statistics offices in North and South Carolina. Pregnancy outcome data were provided by a woman's county of residence, aggregated by age group and ethnicity (white/nonwhite). In most states, including North and South Carolina, abortion providers are required to submit regular and confidential reports on the number of abortions performed to the state. The analysis sample is restricted to the following age-groups: 10-19, 20-24, 25-29, 30-34, and 35-44. All males are excluded from the analysis.[13] Rates are constructed by combining fertility rates with Census data on demographic information on age group, ethnicity, and county of residence. The result is a repeated cross-section of fertility rates by age–ethnicity group by county of residence. This implies that at the county level, the data have a balanced panel structure.[14] Analyses are conducted at both at the county level and the age–group level.

[12]Birth rates, used in additional analyses, are constructed in the same way, and birth count data are sourced from birth certificates. The number of pregnancies is defined as the sum of births, abortions and fetal deaths.

[13]The total number of pregnancies is an undercount because a significant share of pregnancies go unreported, for example due to miscarriage, which occurs in approximately 13 percent of all pregnancies (Andersen, Wohlfahrt, Christens, Olsen, & Melbye, 2000).

[14]In the case of North Carolina, resident abortion data include abortions that occur in any state. In the case of South Carolina, resident abortion data are limited to abortions by South Carolina residents that occur in South Carolina, North Carolina or Georgia. The analysis in this study only uses fertility outcomes of women that reside in North and South Carolina. The analysis is conducted at the level of the county of residence of the pregnant woman, which means that the fertility of out-of-state women is not represented in the analysis data—a boon to this analysis. Residents of of North or South Carolina who sought abortion services in another state are also not included in these data, a limitation of this analysis.

Table I: County Characteristics: NC & SC 1990

	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)
No. of CPCs (per 1,000)	0.35	(0.39)	0.20	(0.30)	0.41	(0.42)
CPC Distance (in miles)	11.65	(14.30)	20.44	(20.80)	7.68	(9.35)
Annual CPC revenue (in $1,000s)	463.30	(1,013.04)	0.00	(0.00)	799.33	(1,312.19)
County Characteristics						
Female Population Age 10-44	67,406	(69,966)	48,246	(42,286)	88,599	(92,885)
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	1	(0)	0	(0)	1	(0)
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-level obs.	4,380		146		146	

4 Impact of CPCs on the Abortion Rate

CPCs seek to lower the abortion incidence by providing pregnancy counseling. A CPC visit may thus dissuade a woman from seeking abortion services or cause a delay sufficient to prevent her receiving abortion care within the gestational time limit set by law. It is plausible that women who are certain that they want to carry to term are neither the target demographic of CPCs nor going to alter their decision as a result of a CPC visit. Changes in local abortion rates are then driven by two groups of women. First, CPCs are plausibly affecting the decision of a woman who is on the margin, that is, uncertain whether to have a child or terminate the pregnancy. This is a potentially large share of women, as 45 percent of all pregnancies are reportedly mistimed or unwanted (Finer & Zolna, 2016). The second group of women who are expected to be more likely to visit a CPC are those facing substantial barriers, for example a great travel distance, to access pregnancy counseling and abortion services from a medical provider. They may be seeking any available support or mistake a CPC for an organization that provides abortion services.

CPCs may also affect the local abortion rate through services for teenagers and young women who are not pregnant. Abstinence education, for instance, teaches teenagers that abstaining from sexual activity is beneficial and sexual abstinence outside marriage is the norm. Preventing abortion is not an explicit goal of sexual abstinence education, but some abstinence education providers, for instance CPCs, are anti-abortion.[15] If CPCs providing abstinence education reduce the sexual activity of teenagers, this would mechanically lower both abortion and birth rates. However, existing evidence, with some exceptions, suggests that abstinence education is ineffective at preventing teenage sexual intercourse and preg-

[15]See Section 510 (b) of Title V of the Social Security Act, P.L. 104-193 for the federal statutory definition of abstinence education that applies to Title V programs.

nancy.[16] In sum, the expected effect of CPC-provided abstinence education on pregnancies, and indirectly abortions, is ambiguous. By providing abstinence education, CPCs may lower or, inadvertently, increase the chance of pregnancy among teenagers and young adults.

5 Empirical Framework

The empirical strategy exploits variation in CPC access across counties and over time to identify the causal effect of CPC exposure on abortion rates. The structural equation of interest is:

$$\text{AbortionRate}_{ct} = \theta_0 + \theta_1 \text{CPC}_{ct} + \theta_2' X_{ct} + \mu_c + \lambda_t + u_{ct}, \tag{1}$$

where AbortionRate_{ct} is the natural log of the abortion rate in county c and year t; CPC_{ct} measures CPC exposure; X_{ct} is a vector of time-varying county characteristics; and μ_c and λ_t denote county and year fixed effects, respectively.

Correct estimation of θ_1 requires solving a complex endogeneity problem. Formally, obtaining a causal estimate of θ_1 using OLS requires strict exogeneity: $E[u_{ct} \mid \text{CPC}_{c1}, \ldots, \text{CPC}_{cT}, X_{c1}, \ldots, X_{cT}] = 0$, which fails under endogenous selection. Organizations may strategically locate CPCs in response to unobserved local conditions—community attitudes toward abortion or latent pregnancy rates—that independently affect fertility outcomes, violating the exclusion restriction required for causal inference.

First, if CPCs tend to open in places where abortion demand is already high, simple

[16] For example, Kohler, Manhart, and Lafferty (2008) found that abstinence-only education did not reduce the likelihood of engaging in intercourse. Similarly, Trenholm et al. (2008) reported that abstinence education caused no difference in teen sexual activity and no differences in rates of unprotected sex, and Carr and Packham (2017) noted that state-level education mandates have no effect on teen birth or abortion rates. In contrast, Cannonier (2012) showed that Title V abstinence-based funding only significantly decreases birth rates for white 15-17-year olds but not other groups. Abortion is also a rarely studied outcome in the abstinence education literature (with the exception of Carr and Packham (2017) and citations therein).

OLS estimation of equation 1 will make CPCs look less effective than they actually are. If they instead avoid counties where abortion rates are already falling for unrelated reasons, OLS will make them look more effective than they are. Either way, if CPCs choose locations strategically, OLS fails to recover causal effects.

Second, CPC expansion is shaped by its own past effects. When a CPC opens, it may reduce abortions the following year. But those lower abortion numbers then influence where future CPCs choose to open. This creates a feedback loop: counties with CPCs in place for many years look different precisely because they have been treated for a long time. Trying to "control" for past abortion rates does not solve the problem, because those past rates were already influenced by CPC presence.

Third, CPC growth is not driven by clear policy shifts or sudden events that researchers can use as natural experiments. Unlike abortion clinic closures—often triggered by new regulations—CPC expansion happens gradually and reflects internal organizational choices. Without sharp timing or policy-based variation, standard difference-in-differences approaches do not apply.

5A. Instrumental Variables Strategy

I develop an instrumental variable strategy that synthesizes three methodological traditions. First, optimal instruments theory (Amemiya, 1974; Chamberlain, 1987; Newey, 1990) establishes that asymptotic efficiency is achieved when instruments equal the conditional expectation of the endogenous variable given exogenous information, providing the theoretical foundation for IV construction.[17] Second, Robins, Mark, and Newey (1992)'s

[17] Amemiya (1974, 1977) first characterized the form of optimal instruments, Chamberlain (1987) demonstrated that these achieve the semiparametric efficiency bound, and Newey (1990) showed that efficiency is preserved when optimal instruments are estimated nonparametrically.

E-estimator framework—which models the exposure mechanism rather than the outcome relationship—provides the practical motivation why this approach is feasible: organizational placement decisions may be more predictable than the complex pathways through which confounders affect fertility outcomes. Third, McFadden (1989)'s simulation-based estimation methods provide the computational tools to construct these instruments when conditional expectations are analytically intractable.[18]

CPC placement is conceived of as having two components: a systematic part based on observable county attractiveness (demographics, existing services, and lagged abortion rates), and an idiosyncratic part based on random organizational factors (e.g. whether a donor shows up, whether a suitable building becomes available). The decomposition of actual CPC presence clarifies this distinction:

$$\text{CPC}_{ct} = \underbrace{\mathbb{E}[\text{CPC}_{ct} \mid \mathcal{F}_{c,t-1}]}_{\text{Systematic component (instrument)}} + \underbrace{(\text{CPC}_{ct} - \mathbb{E}[\text{CPC}_{ct} \mid \mathcal{F}_{c,t-1}])}_{\text{Idiosyncratic component (first-stage residual)}} \tag{2}$$

The identifying instrument then becomes:

$$\text{IV}_{ct} = \mathbb{E}[\text{CPC}_{ct} \mid \mathcal{F}_{c,t-1}] \tag{3}$$

where $\mathcal{F}_{c,t-1}$ denotes the complete observable history through period $t-1$. Since this conditional expectation involves integration over all possible CPC expansion paths and is analytically intractable, I construct it through Monte Carlo simulation. Specifically, I estimate a discrete-time hazard model for CPC openings and simulate 1,000 counterfactual expan-

[18] McFadden (1989) established that replacing intractable moments with simulation-based approximations yields consistent and asymptotically normal estimators, with simulation error controlled by the law of large numbers operating across observations rather than requiring precise estimation for each unit. See also Pakes and Pollard (1989) and **?** for related developments in simulation-based inference.

sion paths for each county. Following McFadden (1989), I set the random seed once and maintain fixed draws across all parameter evaluations to ensure the objective function is deterministic while preserving independence of simulation errors across counties. This forward simulation preserves the dynamic structure of organizational decision-making while averaging out idiosyncratic placement shocks across the simulated paths. The distance from a county population centroid to the nearest CPC mechanically changes when a CPC opens. This distance is also informative for the CPC opening decision, which is why it is used to predict the opening of a CPC and in the construction of the instrument for the CPC count. Moreover, a distance instrument is constructed in the same fashion and used in the 2SLS estimation.

Identification requires that after controlling for observable county trajectories and fixed attributes, variation in CPC placement is orthogonal to contemporaneous abortion demand shocks. More formally, unobserved abortion shocks are mean-independent of the full history of predetermined county characteristics conditional on county fixed effects. Temporal separation in the model (lagged predictors) ensures that CPC organizations are not responding to current abortion shocks when making location decisions. For the exclusion restriction to fail, unobserved shocks would need to simultaneously: (a) affect abortion across multiple periods, (b) be predictable from lagged abortion but not from the extensive set of predetermined covariates, and (c) correlate with the precise timing and sequencing of CPC expansion conditional on all observed county trajectories. The stringency of this joint requirement provides robustness. The approach exploits dynamic over-identification from the panel structure (Bhargava, 1991; Liu, Mroz, & Van der Klaauw, 2010; Mroz & Surette, 1998). Structural parameter stability across 30 years means hundreds of moment conditions are implicitly aggregated through the single instrument, providing substantial over-identification relative to the single endogenous variable. I use this simulated instrument in 2SLS estimation to identify the causal effect of CPC presence on abortion rates.

5A..1 Overview of IV Construction

Instrument construction proceeds in three steps: (1) Estimate a discrete-time hazard model of CPC placement decisions, (2) forward-simulate 1,000 counterfactual expansion paths using estimated probabilities and random organizational shocks, and (3) average across simulations to compute $\mathbb{E}[\text{CPC}_{ct}|\mathcal{F}_{c,t-1}]$. A more detailed description is provided in Appendix **??**.

1. Estimate CPC Location Choice

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

$$U_{ct}(d) = \begin{cases} \beta' X_{c,t-1} + \gamma \cdot \text{CPC}_{c,t-1} + \varepsilon_{ct} & \text{if } d = 1 \\ 0 & \text{if } d = 0 \end{cases}$$

   where $d \in \{0, 1\}$ indicates the opening decision, the utility of the status quo is normalized to zero, and ε_{ct} follows a logit error distribution (Type I Extreme Value). An organization opens a CPC when $U_{ct}(1) > U_{ct}(0)$, which occurs with probability:

$$p_{ct} = P(\text{NewCPC}_{ct} = 1 \mid \mathcal{F}_{c,t-1}) = \frac{\exp(\beta' X_{c,t-1} + \gamma \cdot \text{CPC}_{c,t-1})}{1 + \exp(\beta' X_{c,t-1} + \gamma \cdot \text{CPC}_{c,t-1})} \tag{4}$$

The empirical specification is:

$$\text{logit}(p_{ct}) = \beta_1 \text{CPC}_{c,t-1} + \beta_2 \text{Dist}_{c,t-1} + \beta_3 \text{AR}_{c,t-1} + \beta_4' X_{c,t-1} + \mu_c + \lambda_t, \qquad (5)$$

where $\text{AR}_{c,t-1}$ captures organizational responses to local abortion demand, $\text{CPC}_{c,t-1}$ and $\text{Dist}_{c,t-1}$ measures existing CPC service accessibility, and $X_{c,t-1}$ includes abortion provider presence, demographic (population, age structure, religious composition) and economic controls (unemployment, nonprofit wages). County fixed effects μ_c absorb time-invariant differences in community or policy environment, while year fixed effects λ_t capture national or regional temporal shocks. I estimate the model via maximum likelihood with standard errors clustered at the county level to account for within-county correlation in opening decisions over time.

2. Forward Simulation

Monte Carlo integration mitigates the curse of dimensionality through random sampling. The approach exploits the Law of Large Numbers: for independent draws $\boldsymbol{\varepsilon}^{(s)} \sim f(\boldsymbol{\varepsilon} \mid \mathcal{F}_{c,t-1})$, the sample average converges to the population expectation.

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

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

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

i. Compute predicted opening probability:

$$\widehat{p}^{(s)}_{c,t} = \Lambda(\widehat{\beta}_1 \text{CPC}^{(s)}_{c,t-1} + \widehat{\beta}_2 \text{Dist}^{(s)}_{c,t-1} + \widehat{\beta}_3 \text{AR}^{(s)}_{c,t-1} + \widehat{\beta}_4' X_{c,t-1} + \widehat{\mu}_c + \widehat{\lambda}_t);$$

ii. Draw $U_{c,t}^{(s)} \sim \text{Uniform}(0,1)$;

iii. Determine opening: $\widehat{\text{NewCPC}}_{c,t}^{(s)} = \mathbb{1}[U_{ct}^{(s)} \leq \widehat{p}_{c,t}^{(s)}]$;

iv. Update cumulative count: $\widehat{\text{CPC}}_{c,t}^{(s)} = \widehat{\text{CPC}}_{c,t-1}^{(s)} + \widehat{\text{NewCPC}}_{ct}^{(s)}$.

v. Update $\widehat{\text{Dist}}_{c,t}{}^{(s)}$ using median CPC distance across all counties

The uniform draws $U_{ct}^{(s)} \sim \text{Uniform}(0,1)$ enable Monte Carlo integration.

(c) Instrument Construction

Averaging across $S = 1{,}000$ independent draws yields:

$$\text{IV}_{ct} = \frac{1}{S}\sum_{s=1}^{S} \widehat{\text{CPC}}_{ct}^{(s)} \xrightarrow{S\to\infty} \mathbb{E}[\text{CPC}_{ct} \mid \mathcal{F}_{c,t-1}]$$

As S increases, idiosyncratic shocks average to zero, leaving only the systematic component determined by observables.

5A..2 Identification

The approach decomposes CPC placement into systematic and idiosyncratic components. Each simulation draw s represents one possible realization of the organizational decision-making process from 1991 through year t, where decisions follow the hazard model and incorporate random shocks representing idiosyncratic factors—donor availability, real estate opportunities, local volunteer initiatives—orthogonal to contemporaneous abortion demand by assumption. Averaging across $S = 1{,}000$ such realizations computes the expected CPC presence arising from the systematic, predictable component of organizational expansion. The Monte Carlo integration provides a computational device for constructing this conditional expectation.

The identification strategy builds on Robins et al. (1992)'s insight that causal identification in observational studies requires conditioning on sufficient covariates to render treatment assignment independent of potential outcomes. In this setting, the conditional exchangeability assumption becomes:

$$\mathbb{E}[\mathrm{AR}_{c,t}^{\mathrm{CPC}=k} \mid \mathrm{CPC}_{c,t}, \mathcal{F}_{c,t-1}, \mu_c] = \mathbb{E}[\mathrm{AR}_{c,t}^{\mathrm{CPC}=k} \mid \mathcal{F}_{c,t-1}, \mu_c] \tag{6}$$

where $\mathrm{AR}_{c,t}^{\mathrm{CPC}=k}$ denotes the potential abortion rate under k CPCs, $\mathcal{F}_{c,t-1} = \{X_{c,1}, \ldots, X_{c,t-1}, \mathrm{CPC}_{c,1}, \ldots, \mathrm{CPC}_{c,t-1}\}$ represents the complete observable history through period $t-1$, and μ_c is the county fixed effect. Equivalently, for the structural error term:

$$\mathbb{E}[u_{ct} \mid \mathcal{F}_{c,t-1}, \boldsymbol{\theta}, \mu_c] = 0 \tag{7}$$

where $\boldsymbol{\theta}$ denotes the hazard model parameters. This states that unobserved contemporaneous shocks to abortion demand are mean-independent of the full history of predetermined covariates, their nonlinear interactions through the dynamic CPC placement process, and time-invariant county characteristics.[19]

In Robins et al. (1992)'s framework, the instrument $IV_{ct} = \mathbb{E}[\mathrm{CPC}_{ct} | \mathcal{F}_{c,t-1}]$ captures expected exposure, and 2SLS exploits the quasi-random deviation $(\mathrm{CPC}_{ct} - IV_{ct})$ for causal inference. The systematic component serves as the instrument, exploiting variation driven solely by predetermined observables, while the first-stage residual captures quasi-random variation in organizational decisions orthogonal to both $\mathcal{F}_{c,t-1}$ and to

[19]This is why the method is robust to hazard model misspecification: exclusion depends on the economic environment (what determines CPC placement versus abortion), not on perfect functional form. This parallels the distinction in Newey (1990) between parametric and nonparametric estimation of optimal instruments: the logit specification provides efficiency gains when correctly specified, but consistency of the IV estimator depends only on the validity of the exclusion restriction, not on the functional form of the first stage.

u_{ct}.

Critically, lagged abortion rates are permitted to influence CPC openings—organizations may strategically target high-abortion areas—but the instrument captures only this predictable response to observable history. The exclusion restriction holds if contemporaneous abortion demand shocks are orthogonal to unobserved factors that influence CPC location choice.

Exclusion and Threats to Validity. The exclusion restriction (7) could be violated if time-varying unobserved shocks simultaneously: (a) affect abortion rates across multiple periods, (b) are predictable from lagged abortion rates but not from the rich set of observables in $\mathcal{F}_{c,t-1}$, (c) persist in patterns not captured by county fixed effects or the nonlinear dynamic structure, and (d) correlate with the specific timing and sequencing of CPC expansion decisions conditional on all lagged observables and their interactions. To address potential direct effects of $\mathcal{F}_{c,t-1}$ on current abortion rates, the second-stage regression explicitly controls for: current time-varying observables X_{ct}, county fixed effects μ_c absorbing time-invariant factors, and year fixed effects λ_t capturing common shocks. This ensures the instrument's predictive power operates only through organizational responses to predetermined conditions rather than through serial correlation in outcomes.

The instrument construction ensures both relevance and monotonicity hold by design. *Relevance* requires the instrument to predict actual CPC presence, which holds because $IV_{ct} = \mathbb{E}[\text{CPC}_{ct}|\mathcal{F}_{c,t-1}]$ conditions on the same observables driving organizational location decisions. The first-stage F-statistic exceeds 120, confirming strong predictive power. *Monotonicity* requires treatment to respond weakly positively to the instrument—no "defiers" whose CPC presence decreases when IV_{ct} increases. This holds because organizations act on observable county attractiveness: if $IV_{ct,A} > IV_{ct,B}$, county A is more attractive for placement than county B, making it weakly more likely to receive a CPC.[20]

Robustness through Over-identification. The exclusion restriction is substantially weaker than simple sequential exogeneity because it exploits two distinct sources of over-identification in dynamic panel models.[21] First, following Bhargava (1991), structural stability of the hazard model parameters means each lag of each predetermined variable provides an independent moment condition: $\mathbb{E}[X_{c,t-s} \cdot u_{ct}] = 0$ for all $s \geq 1$. With 30 years and 10 time-varying covariates, approximately $K \times (T-1) \approx 290$ moment conditions are implicitly aggregated through the single instrument $IV_{ct} = \mathbb{E}[\text{CPC}_{ct} \mid \mathcal{F}_{c,t-1}]$. Second, following Liu et al. (2010), nonlinear path dependence means the effect of any lagged variable depends on the *entire intervening sequence* of observables: different timing and ordering of shocks through the state-dependent hazard model provide exponentially many additional identification conditions. The forward simulation explicitly captures these dynamic interactions by recursively computing CPC presence through all 30 years of the panel. This high-dimensional over-identification provides robustness to individual specification errors: the hazard model need not be perfectly specified for valid inference, as Monte Carlo validation confirms (see §5A..4).

[20] Defiers would require organizations systematically locating opposite to predicted patterns, producing a negative first-stage relationship. The strong positive first stage confirms both relevance and monotonicity empirically.

[21] Crucially, the IV requires exclusion restriction in equation 7, which is weaker than sequential exogeneity and weaker still than strict exogeneity as required by OLS.

5A..3 Estimation

The 2SLS estimation to obtain the causal effect of CPC exposure on the abortion rate proceeds as follows:

First Stage:

$$\text{CPC}_{ct} = \pi_0 + \pi_1 IV_{ct} + \pi_2' X_{ct} + \mu_c + \lambda_t + v_{ct}. \tag{8}$$

Second Stage:

$$\text{AbortionRate}_{ct} = \alpha + \gamma \widehat{\text{CPC}}_{ct} + \delta' X_{ct} + \mu_c + \lambda_t + u_{ct}. \tag{9}$$

Standard errors are clustered at the county level to allow for arbitrary within-county correlation over time.

5A..4 Monte Carlo Validation

Monte Carlo experiments validate the approach under severe endogeneity. Following Gilleskie and Mroz (2004), I use residual resampling to construct synthetic datasets preserving the observed panel structure while introducing known confounding. Across 1,000 replications, OLS yields severely biased estimates of the wrong sign (mean $\approx +1.5$ vs. true -0.30), while the simulated IV successfully recovers the true parameter (mean -0.32, SD 0.08).

Crucially, the true effect is recovered despite hazard model misspecification: using a functional form and omitting variables that differ from the true data-generating process. This demonstrates that robustness stems from isolating the random component of organizational decisions rather than perfectly modeling all placement determinants. Full simulation details and results appear in Appendix 9E..

5A..5 Interpretation

The estimated coefficient γ has a local average treatment effect (LATE) interpretation (Angrist & Imbens, 1995), capturing the causal effect for *compliers*—counties whose CPC presence is determined by the predictable component of organizational expansion captured in IV_{ct}.

Compliers are policy-relevant for two reasons. First, they represent marginal locations where the instrument "tips" organizational decisions—counties on the boundary between treatment and control where predictable factors determine CPC entry. These are the places where policy interventions influencing CPC expansion (e.g., funding or regulation) would change treatment status. Second, the hazard model reveals systematic expansion patterns based on observable community characteristics—existing service gaps, demographic demand, and related local conditions. Compliers thus correspond to counties where these observable factors predict CPC entry, making them identifiable and relevant for policy design.

Compliers may differ from the full population in ways affecting treatment magnitude. They plausibly include areas where a new CPC fills a service gap and has large effects, or where CPCs can meaningfully shift social norms. This contrasts with always-takers in strongly pro-life areas, where norms already discourage abortion, potentially limiting marginal impacts. Never-takers—counties where observables predict zero CPC probability—are excluded from the LATE by construction. The estimated effect therefore pertains to counties on the margin of CPC entry, not to contexts where CPCs would never locate.

6 Results

6A. Two-Stage Least Squares Results

Table II presents 2SLS estimation results using the forward-simulated instruments described in Section 5A..3. The specifications progressively introduce distance measures to test whether geographic access independently affects abortion rates beyond the discrete presence of CPCs.

Table II: 2SLS Estimation Results

(Log) Abortion Rate	(1)	(2)	(3)
No. of CPCs (per 1,000)	-0.182	-0.186	-0.085
	(0.040)	(0.076)	(0.078)
CPC Distance (in miles)		0.006	0.055
		(0.011)	(0.023)
Distance Squared		0.006	-0.002
		(0.011)	(0.001)
Distance Cubed			0.000
			(0.000)
N	4,376	4,376	4,376
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Kleibergen-Paap rk LM statistic	7.0	17.2	9.9
Kleibergen-Paap rk Wald F statistic	120.0	15.1	3.2

Notes: Standard errors, clustered at the state–county level, are reported in parentheses. The dependent variable is the natural logarithm of the abortion rate per 1,000 women ages 15–44. All specifications include state–county and year fixed effects. The endogenous regressors—CPC presence per 1,000 women and CPC distance measures—are instrumented using forward-simulated expected CPC presence and distance, as described in Section 5A..3. Distance terms (linear, squared, and cubic) capture nonlinear effects of geographic access to the nearest CPC. The Kleibergen–Paap LM statistic tests for underidentification, and the Kleibergen–Paap Wald F statistic reports heteroskedasticity-robust first-stage instrument strength. Sample size is the number of county–year observations.

6A..1 Main Findings

Baseline Specification (Column 1). The parsimonious specification including only CPC count yields a coefficient of -0.182 (SE = 0.040), indicating that an additional CPC per 1,000 residents reduces the abortion rate by approximately 18.2 percent. This estimate is substantially larger in magnitude than the corresponding OLS estimate reported in the main text, consistent with endogenous selection bias attenuating OLS estimates toward zero. The Kleibergen-Paap Wald F-statistic of 120.0 far exceeds conventional thresholds for instrument strength, confirming that the simulated instrument strongly predicts actual CPC presence in the first stage.

Adding Distance Controls (Column 2). Introducing distance to the nearest CPC and its square yields a CPC count coefficient of -0.186 (SE = 0.076), nearly identical to column (1). The distance terms are statistically insignificant and economically small, suggesting that the discrete presence of a CPC matters more than continuous geographic proximity. The first-stage remains adequately strong with an F-statistic of 15.1, though instrument strength declines when instrumenting multiple endogenous variables simultaneously.

Polynomial Distance Specification (Column 3). Including cubic distance terms substantially weakens the first stage, with F-statistics falling to 3.2, well below the threshold of 10 conventionally used to diagnose weak instruments. The CPC count coefficients decline to -0.085 and are imprecisely estimated. These specifications likely suffer from weak identification, as the simulated instruments struggle to predict both CPC presence and multiple distance measures simultaneously. The Kleibergen-Paap LM statistics remain above conventional critical values, indicating that the instruments are not completely uninformative, but the weak first-stage F-statistics suggest substantial finite-sample bias may be present.

The preferred specification in column (1) indicates economically meaningful effects of CPC exposure. A one-standard-deviation increase in CPC presence (approximately 0.39 CPCs per 1,000 residents) reduces the abortion rate by roughly 7 percent. Given a mean abortion rate of 11.1 per 1,000 women aged 15–44 during the sample period, this corresponds to a reduction of approximately 0.8 abortions per 1,000 women, or roughly 79 fewer abortions in a county with 100,000 women of reproductive age. These results highlight that even modest increases in local CPC presence can have substantial marginal effects on abortion rates, particularly in counties on the margin of CPC entry.

Table III: 2SLS Estimation Results by Age

	(Log) Abortion Rate
	(1)
No. of CPCs per capita X Age 10–19	-0.251
	(0.0511)
No. of CPCs per capita X Age 20–24	-0.147
	(0.0365)
No. of CPCs per capita X Age 25–29	-0.0555
	(0.0452)
No. of CPCs per capita X Age 30–34	-0.0460
	(0.0407)
No. of CPCs per capita X Age 35–44	-0.0453
	(0.0409)
N	43,718
County FE	Yes
Year FE	Yes
Kleibergen–Paap rk LM statistic	12.85
Kleibergen–Paap rk Wald F statistic	18.15

Notes: Standard errors, clustered at the county level, are reported in parentheses. The dependent variable is the natural logarithm of the abortion rate for each age group. All specifications include county and year fixed effects. The endogenous regressors—CPC presence per capita—are instrumented using forward-simulated expected CPC presence and interacted with age-group indicators, as described in Section 5A..3. The Kleibergen–Paap LM statistic tests for underidentification, and the Kleibergen–Paap Wald F statistic reports heteroskedasticity-robust first-stage instrument strength. Sample size is the number of county–year–age group observations.

Results by Age. Table III presents 2SLS estimates of the causal effect of CPC exposure on abortion rates by age group. The coefficients are largest in magnitude for the youngest women (ages 10–19), with an estimated effect of -0.251 (SE = 0.051), indicating that an additional CPC per capita reduces the abortion rate in this group by roughly 25 percent. The effect diminishes for older age groups: ages 20–24 exhibit a coefficient of -0.147 (SE = 0.037), while ages 25–29, 30–34, and 35–44 show smaller and statistically less precise effects ranging from -0.055 to -0.045. These results suggest that CPC presence has the strongest marginal impact on abortion rates among younger women, consistent with the notion that these age groups are more sensitive to local service availability. All specifications include county and year fixed effects, and the Kleibergen–Paap statistics indicate that the instruments are adequately informative for these single-endogenous-variable regressions. The pattern of declining effects with age highlights the heterogeneity of CPC influence across demographic groups, with policy-relevant implications given that teenagers and college-aged women are most responsive to local CPC presence.

6A..2 Comparison to OLS

The IV estimate in Table II column (1) is nearly twice the magnitude of the corresponding OLS estimate in Table IV column (2) (-0.182 vs. -0.100), which can be explained as follows. First, endogenous selection attenuates OLS toward zero: if organizations strategically locate CPCs in areas with persistently high abortion demand (unobserved to the econometrician), this creates positive correlation between CPC presence and unobserved determinants of abortion rates, biasing OLS estimates downward in absolute

Table IV: Effect of CPCs on the Abortion Rate

	TWFE			DID_{l}
	(1)	(2)	(3)	(4)
No. of CPCs (per 10,000)	-0.113	-0.100	-0.0860	-0.095
	(0.0272)	(0.0301)	(0.0315)	(0.0367)
N	4376	4376	4376	2734
Control Vars.	No	Yes	Yes	Yes
Pop. weighted	No	No	Yes	No
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes

Notes: This table shows the percent change in the abortion rate as a result of an additional CPC per 10,000 women age 10-44. Column (1)-(3) shows two-way fixed effect estimates. Column (4) shows estimates from the heterogeneity-robust DID_{l} estimator of De Chaisemartin and d'Haultfoeuille (2020), which remains unbiased under heterogeneous treatment effects by comparing treated units only to not-yet-treated controls. Robust standard errors are in parentheses.

value. Second, the IV estimate captures a LATE for complier counties—those whose CPC presence is determined by the predictable component of organizational expansion patterns—which may differ from the average treatment effect estimated by OLS. Compliers represent marginal locations where the instrument "tips" organizational decisions. These may be underserved areas where a new CPC has particularly large effects, or counties with moderate ideological composition where CPCs can meaningfully shift social norms, in contrast to always-takers that receive CPCs due to exceptional organizational capacity or existing strong demand. The validation exercise confirms that endogenous selection can severely attenuate OLS estimates, supporting the interpretation that the larger IV estimate reflects both bias correction and measurement of effects for a complier subpopulation where impacts are substantial.

6B. Policy Considerations

6B..1 Parental Involvement Laws

Parental involvement (PI) laws require minors to obtain parental consent or provide parental notification before an abortion. These laws can influence teen abortion rates through two main channels: by directly deterring abortions and by increasing travel distances to providers in states without such requirements (C. Myers & Ladd, 2020). Nationally, C. Myers and Ladd (2020) find that the average distance minors must travel to obtain a confidential abortion rose from 58 miles in 1992 to 454 miles in 2016, and that PI laws increased teen births by about 3 percent.

In the context of North and South Carolina, however, cross-border avoidance of PI laws is unlikely to confound the estimated effects of CPC openings. South Carolina enacted a parental consent law in 1990, and North Carolina implemented a similar requirement in mid-1995.[22] During most of the study period, all neighboring states—Tennessee, Georgia, and Virginia—also enforced comparable parental involvement laws.[23] Between 1991 and 1996, two of the three border states had enforceable PI laws, and from 1997 onward, all three maintained them. This regional uniformity greatly reduced the feasibility of interstate travel to circumvent PI laws, limiting potential bias from policy heterogeneity in the identification of CPC effects.

[22] South Carolina: *S.C. Code Ann. § 44-41-31. Abortion upon minors; consent requirements; support obligations of parent or legal guardian who refuses to give consent for minor's abortion; penalty for false representation* (1990). North Carolina: *N.C. Gen. Stat. § 90-21.6. Abortion – definitions (part of parental or judicial consent provisions)* (1995); *N.C. Gen. Stat. § 90-21.7. Parental consent required (for abortion upon unemancipated minor)* (1998).

[23] Tennessee: *Tenn. Code Ann. § 37-10-303. Written consent of a parent or legal guardian required for an abortion on an unemancipated minor* (1988); **?**; Georgia: *Planned Parenthood Ass'n of Atlanta Area v. Miller* (1991); **?**; Virginia: *Va. Code Ann. § 16.1-241. Jurisdiction; consent for abortion* (1997); **?**.

7 Conclusions

This paper provides the first causal evidence that CPCs reduce abortion incidence. Using county-level data from North and South Carolina between 1990 and 2019, I estimate that CPC presence lowers local abortion rates by roughly 18 percent, with effects concentrated among teenagers and women under age 25. These results are directly relevant to current policy: CPCs receive public funding in many states, yet until now there has been little systematic evidence of their effectiveness.

The estimated impact substantially exceeds OLS results, which likely understate true effects because CPCs tend to locate in communities with stronger pro-life sentiment—areas where abortion rates would be lower even absent CPC activity. Correcting for this endogenous location choice through forward simulation of CPC expansion decisions yields consistent causal estimates. The 18 percent effect implies that CPC network growth contributed meaningfully to declining abortion rates over the study period, operating as a demand-side intervention distinct from regulatory restrictions such as parental notification laws, waiting periods, or Medicaid funding limits.

Beyond establishing program effectiveness, the findings speak to broader debates about reproductive health policy in the post-Roe landscape. During the study period, abortion remained constitutionally protected and relatively accessible. Since the Supreme Court's reversal of Roe v. Wade in 2022, CPCs now operate in a profoundly different institutional setting. In restrictive states, their role may have shifted from persuading women to forgo abortions to supporting those who face legal or financial barriers to obtaining one. Whether the mechanisms identified here—informational counseling and material support—continue to shape fertility decisions under these new constraints is an open question.

Several avenues for future research follow naturally. First, what are the long-run consequences of CPC-induced births for maternal and child outcomes? If CPCs help women continue wanted pregnancies, welfare effects may be positive. But if they persuade women to continue pregnancies they are unprepared for, the long-term costs to maternal mental health, child development, and family stability could be substantial. Linking CPC exposure to longitudinal data on maternal employment, earnings, and children's educational and health outcomes would provide critical evidence on these questions (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. In communities with generous safety nets, CPCs may mainly provide informational or emotional support, while in resource-poor areas they may deliver tangible assistance such as supplies or housing. Mapping these complementarities is essential for designing efficient support systems for pregnant women.

Third, the observed heterogeneity by age and region raises questions about mechanisms. Younger women may respond more strongly because they face greater uncertainty, stigma, or financial constraint, or because CPCs target them more intensively. Understanding these mechanisms would help tailor policy interventions toward the populations most likely to benefit.

Finally, evaluating the welfare implications of CPC-induced fertility increases requires moving beyond revealed preference frameworks. If CPCs change choices by withholding information or exerting social pressure, the resulting behavioral responses may not signal improved welfare. Developing credible welfare measures that account for persuasion, manipulation, and information asymmetries remains a key methodological challenge.

The results in this paper demonstrate that the pro-life movement's creation of a nationwide CPC network has measurable demographic effects. CPCs have become a significant institution in shaping fertility outcomes, operating at a scale comparable to formal family planning programs. As abortion access becomes increasingly uneven across states, understanding the causal mechanisms, welfare consequences, and policy interactions of these organizations will be essential for informed debate about reproductive health and family policy in the post-Roe era.

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8 Appendix

8A. Maps

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

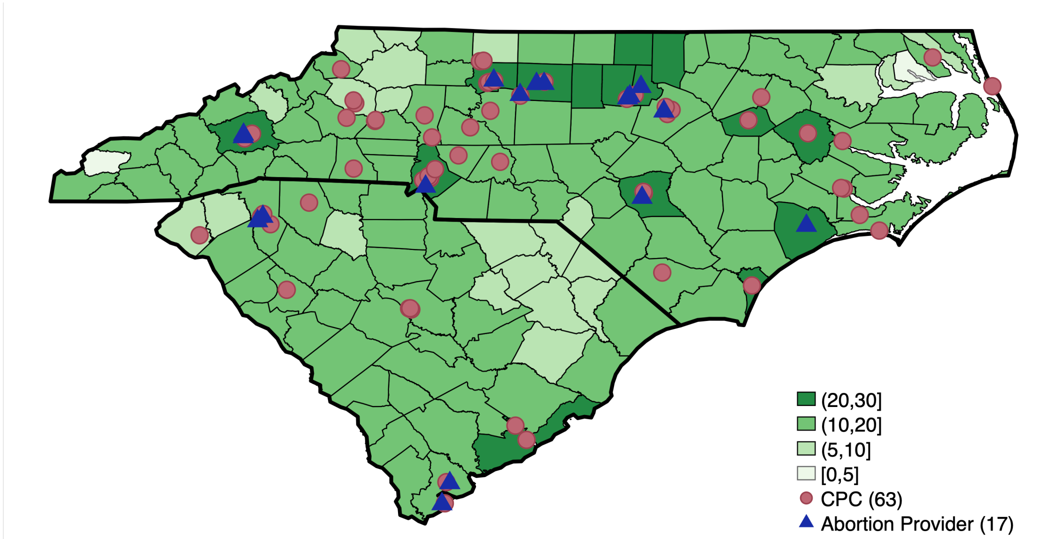


Figure II: CPCs, Abortion Providers and Abortion Rate (NC & SC): 2000

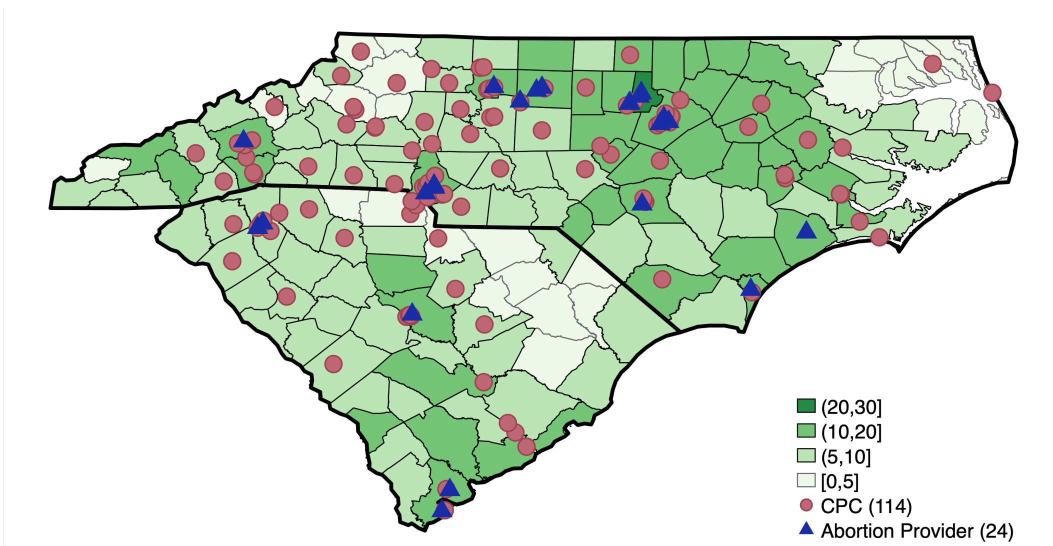


Figure III: CPCs, Abortion Providers and Abortion Rate (NC & SC): 2010

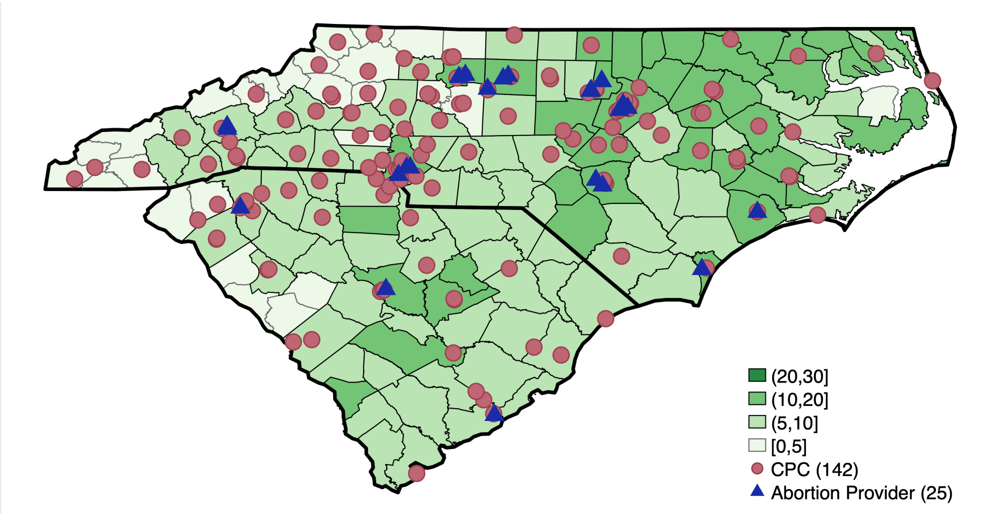
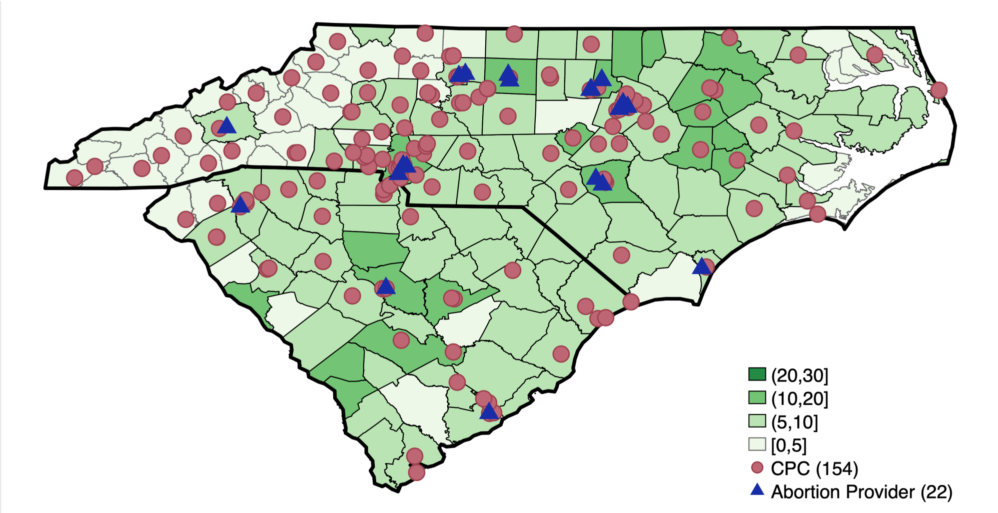


Figure IV: CPCs, Abortion Providers and Abortion Rate (NC & SC): 2019



8B. Background Tables

Table V: 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 VI: 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 VII: 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.

9 Instrumental Variable Construction and Validation

9A. Results from the CPC Logit Model

Table VIII presents estimates from the hazard model. Column (1) provides the baseline specification used to construct instruments. The main result is that an existing CPC reduces the probability of additional CPC openings in the same county by 5.0 percent, indicating market saturation or central planning to avoid duplication. The distance coefficient implies that a 10-mile increase in driving distance to the nearest CPC increases the probability of a new opening by 1 percent, confirming CPCs target underserved areas.

The coefficient on distance to the nearest abortion provider is close to zero. However, this is potentially misleading: 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 positive coefficient on teenage population share (though imprecise) suggests CPCs target their core demographic. Surprisingly, higher GOP vote share predicts lower CPC opening probability, and religious composition shows no clear pattern.

These patterns suggest CPCs do not compete in a Hotelling-style framework but follow central planning to maximize coverage and client access. This organizational behavior—strategic avoidance of existing CPCs combined with systematic targeting of underserved areas—provides the predictable component that instruments leverage for identification.

Table VIII: 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 1000 women 10-44 (lagged)	0.001	-0.001
	(0.003)	(0.004)
Population (lagged)	0.000	0.000
	(0.000)	(0.000)
Pop. share age 10-19 (lagged)	0.072	0.018
	(0.553)	(0.603)
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)
Catholic share (lagged)	-0.197	0.717
	(0.816)	(1.025)
No religion share (lagged)	0.134	0.236
	(0.150)	(0.169)
N	2,129	1,987
State FE	Yes	Yes
County FE	Yes	Yes
Time Trend	Yes	No
Year FE	No	Yes

Notes: This table shows estimates from the renewal model of CPC location choice. All independent variables are lagged by one time period. Each county characteristic makes the opening of a new CPC more or less likely. Average marginal effects were derived using the delta method and each coefficient can be interpreted as a percentage. Standard errors are in parentheses.

9B. Monte Carlo Integration for Instrument Construction

The instrument $\text{IV}_{ct} = \mathbb{E}[\text{CPC}_{ct} \mid \mathcal{F}_{c,t-1}]$ requires computing a conditional expectation that is analytically intractable due to path dependence in the dynamic CPC expansion process. This appendix explains how Monte Carlo integration provides a computationally feasible solution.

9B..1 The Computational Challenge

The conditional expectation is formally an integral over the distribution of organizational decision shocks:

$$\mathbb{E}[\text{CPC}_{ct} \mid \mathcal{F}_{c,t-1}] = \int \text{CPC}_{ct}(\boldsymbol{\varepsilon}, \mathcal{F}_{c,t-1})\, f(\boldsymbol{\varepsilon} \mid \mathcal{F}_{c,t-1})\, d\boldsymbol{\varepsilon} \tag{10}$$

where $\text{CPC}_{ct}(\boldsymbol{\varepsilon}, \mathcal{F}_{c,t-1})$ is CPC presence as a function of the shock sequence $\boldsymbol{\varepsilon} = \{\varepsilon_{c,1991}, \ldots, \varepsilon_{ct}\}$ and observable history $\mathcal{F}_{c,t-1}$, and $f(\boldsymbol{\varepsilon} \mid \mathcal{F}_{c,t-1})$ is the conditional distribution of shocks.

Path dependence makes analytical integration infeasible. Current CPC presence depends on the entire history of organizational decisions:

$$\text{CPC}_{ct} = \sum_{\tau=1991}^{t} \text{NewCPC}_{c\tau}$$
$$\text{NewCPC}_{c\tau} = \mathbb{1}\left\{\varepsilon_{c\tau} \leq \boldsymbol{\Phi}^{-1}\left(\Lambda(\beta'\mathbf{X}_{c,\tau-1} + \gamma \cdot \text{CPC}_{c,\tau-1})\right)\right\}$$

where $\Lambda(\cdot)$ is the logistic CDF, $\boldsymbol{\Phi}^{-1}(\cdot)$ is the inverse Type I Extreme Value CDF, and $\text{CPC}_{c,\tau-1}$ itself depends recursively on all prior shocks $\{\varepsilon_{c,1991}, \ldots, \varepsilon_{c,\tau-1}\}$. For a county observed through 2019, this requires a 29-dimensional integral with complex nonlinear interactions through the cumulative CPC stock term. Standard numerical integration methods (e.g., Gaussian quadrature) would require M^{29} grid points—computationally infeasible even for modest grid sizes M.

9B..2 Computational Properties

Convergence rate. The Monte Carlo standard error is $O(S^{-1/2})$, making simulation error negligible relative to sampling uncertainty with $S = 1,000S = 1{,}000S = 1,000$draws. Following McFadden (1989), this error vanishes asymptotically as sample size increases, making the simulated instrument asymptotically equivalent to the true conditional expectation for inference purposes.

Theoretical Properties. The Monte Carlo approximation satisfies several key properties:

Consistency: By the Strong Law of Large Numbers, $\widehat{\text{IV}}_{ct} \xrightarrow{\text{a.s.}} \mathbb{E}[\text{CPC}_{ct} \mid \mathcal{F}_{c,t-1}]$ as $S \to \infty$, provided the shocks are drawn independently from the correct distribution.

Convergence Rate: The approximation error decreases at rate $O_p(S^{-1/2})$. By the Central Limit Theorem,

$$\sqrt{S}\left(\widehat{\text{IV}}_{ct} - \mathbb{E}[\text{CPC}_{ct} \mid \mathcal{F}_{c,t-1}]\right) \xrightarrow{d} N\left(0, \text{Var}[\text{CPC}_{ct} \mid \mathcal{F}_{c,t-1}]\right)$$

where the conditional variance can be estimated from the simulation draws. With $S = 1{,}000$, the Monte Carlo standard error is $\sigma/sqrt_{1000} \approx \sigma/31.62$ where σ is the standard deviation of $\widehat{\text{CPC}}_{ct}^{(s)}$ across simulations.

Dimension Independence: Unlike deterministic numerical integration methods whose accuracy deteriorates exponentially with dimension, Monte Carlo convergence depends only on the number of draws S, not on the dimensionality of the integral. This property makes

Monte Carlo integration particularly suitable for computing conditional expectations in high-dimensional dynamic models.

Variance Reduction: The averaging operation reduces the variance of the instrument relative to any single simulated path:

$$\mathrm{Var}\left[\widehat{\mathrm{IV}}_{ct}\right] = \frac{1}{S}\mathrm{Var}\left[\widehat{\mathrm{CPC}}_{ct}^{(s)}\right]$$

As S increases, idiosyncratic variation from individual shock realizations averages to zero, isolating the systematic component of CPC expansion determined by observable county characteristics.

9C. Example: Alamance County, North Carolina

Consider Alamance County, NC, which had no CPCs as of 1990. The simulation proceeds:

- **Year 1991**: Using 1990 conditions, the logit model yields $\Pr(\mathrm{CPC}_{1991} = 1 \mid W_{1990})$. A random draw $u_{1991}^{(s)} \sim \mathrm{Uniform}[0,1]$ determines whether a CPC opens in simulation s. If $u_{1991}^{(s)} < \Pr(\mathrm{CPC}_{1991} = 1 \mid W_{1990})$, a CPC opens and variables are updated.
- **Year 1992**: Using updated 1991 variables, compute $\Pr(\mathrm{CPC}_{1992} = 1 \mid W_{1990}, W_{1991})$. Another draw $u_{1992}^{(s)}$ determines the 1992 opening.
- This continues through 2019, generating trajectory $\{\mathrm{CPC}_{ct}^{(s)}\}_{t=1990}^{2019}$ for simulation s.

Averaging across $S = 1,000$ simulations produces IV_{ct} for Alamance County. Counties with different observables receive different simulated trajectories and instruments, capturing heterogeneous exposure to organizational expansion patterns.

9D. Detailed Theoretical Foundations

This section provides detailed theoretical justification for the forward-simulation IV approach, extending the overview presented in the main text.

9D..1 Optimal Instruments Theory

Following Chamberlain (1987) and Newey (1990), asymptotic efficiency is achieved when instruments equal the conditional expectation of the endogenous variable given exogenous information: $IV^*_{ct} = E[\text{CPC}_{ct}|\mathcal{F}_{c,t-1}]$.

Chamberlain (1987) established that optimal IV estimators attain the semiparametric efficiency bound for conditional moment restriction models. The semiparametric efficiency bound represents the lowest achievable asymptotic variance among all estimators using only the conditional moment restriction $E[\epsilon_{ct}|\mathcal{F}_{c,t-1}] = 0$. Chamberlain showed that when instruments equal $E[\text{CPC}_{ct}|\mathcal{F}_{c,t-1}]$, the resulting IV estimator achieves this bound—no estimator can do better without imposing additional assumptions about the error distribution or functional forms.

Newey (1990) addressed the practical challenge that optimal instruments involve conditional expectations of endogenous variables, which often require specifying the conditional distribution and performing integration. Newey demonstrated that these optimal instruments can be estimated nonparametrically—via series approximation (using polynomial or spline bases) or nearest neighbor regression—without sacrificing asymptotic efficiency. The key insight: estimation error in the instruments vanishes asymptotically at a rate that does not affect the limiting distribution of the parameter estimates, provided the nonparametric estimator converges fast enough. This circumvents the need to specify functional forms or conditional distributions while maintaining efficiency.

The forward simulation implements this insight through a different computational approach. Monte Carlo integration over organizational decision shocks provides a computationally feasible alternative to fully nonparametric methods. Rather than using series approximation or nearest neighbors, the simulation directly constructs $E[\text{CPC}|\mathcal{F}]$ by averaging over random draws from the structural model. With $S = 1,000$ draws, Monte Carlo error becomes negligible, and the approach inherits the efficiency properties Newey established for estimated optimal instruments.

9D..2 Design-Based Formula Instruments

The relationship to Borusyak et al. (2025)'s formula instruments framework merits discussion. Their general formula $z_i = f_i(s, g)$ encompasses settings where instruments combine predetermined exposure shares s with exogenous shocks g.

borusyak2025design's central contribution is showing that shock exogeneity alone is insufficient. Even with truly random shocks g, nonrandom exposure through shares s can create omitted variable bias. Consider a shift-share instrument where $z_{it} = \sum_k s_{ik} g_{kt}$: industry-level shocks g_{kt} may be exogenous, but if exposure shares s_{ik} correlate with unobserved outcome determinants, the instrument is invalid. They propose two solutions:

1. Recentering: $\tilde{z}_{it} = z_{it} - E[z_{it}|X_{it}]$
2. Controlling: Include $E[z_{it}|X_{it}]$ as a control variable

This approach achieves recentering automatically. Rather than starting with raw CPC counts and adjusting them, I construct $IV_{ct} = E[\text{CPC}_{ct}|\mathcal{F}_{c,t-1}]$ as the instrument from the outset. This unifies the Newey (1990) optimal instruments perspective with the

Borusyak et al. (2025) design-based framework. Both require isolating variation from shocks conditional on the assignment mechanism.

To see the equivalence, note that borusyak2025design's recentering formula can be written:

$$\tilde{z}_{ct}^{BHJ} = \text{CPC}_{ct} - E[\text{CPC}_{ct}|\mathcal{F}_{c,t-1}] = \text{CPC}_{ct} - IV_{ct}$$

This is the deviation of actual from expected CPC presence—exactly the random component isolated by the simulation. Using this as an instrument (in borusyak2025design's framework) is equivalent to using IV_{ct} as an instrument (in the optimal instruments framework) because the first stage is identical:

$$\text{CPC}_{ct} = \pi_0 + \pi_1(\text{CPC}_{ct} - IV_{ct}) + \text{controls}$$

implies $\pi_1 = 1$ and the fitted values are exactly IV_{ct}.

9E. Monte Carlo Validation of Instrumental Variables Strategy

To verify the forward-simulation IV approach recovers causal effects under endogenous selection, I conduct Monte Carlo validation using synthetic data with known data-generating process. The validation design follows Gilleskie and Mroz (2004)'s methodology of calibrating synthetic data using real data structures.

9E..1 Validation Design

The procedure consists of four steps, repeated across 500 Monte Carlo replications:

Step 1: Construct Synthetic Dataset

Using the actual county-level panel structure (1990–2019, all counties in North and South Carolina), I:

1. Draw latent confounder $Z_c \sim N(0, 1)$ for each county. This represents unobserved factors (e.g., community attitudes, volunteer availability, latent pregnancy rates) that affect both CPC location decisions and abortion rates.

2. Generate endogenous CPC presence:

$$CPC_{ct}^{observed} = CPC_{ct}^{true} + 0.2 \cdot Z_c \tag{11}$$

   where CPC_{ct}^{true} is the actual observed CPC count in the real data. The confounder Z_c creates positive selection: counties with higher Z_c are more likely to attract CPCs.

3. Fit an initial OLS regression of actual abortion rates on actual CPC counts, controls, and fixed effects to obtain predicted values and residuals. Bootstrap residuals with replacement to maintain realistic error patterns including serial correlation, heteroskedasticity, and within-county clustering.

4. Generate synthetic outcomes with known causal effect ($\beta_{CPC} = -0.30$):

$$AbortionRate_{ct}^{synthetic} = -0.30 \cdot CPC_{ct}^{true} + 2.0 \cdot Z_c + 0.5 \cdot Unemp_{ct} + \tilde{\epsilon}_{ct} \tag{12}$$

   where $\tilde{\epsilon}_{ct}$ are the bootstrapped residuals. The parameters are chosen so that: (a) the true causal effect is negative, (b) the confounder creates substantial positive selection bias, and (c) the bias dominates the true effect in naive OLS.

This construction creates correlation between $CPC_{ct}^{observed}$ and structural errors through the confounder Z_c, violating strict exogeneity. The confounder is observed by the researcher but deliberately excluded from naive OLS to simulate the real-world problem where relevant confounders are unobserved.

Step 2: Estimate Location Choice Model

For each Monte Carlo replication, estimate the logit hazard model using only 1990 baseline data (before the synthetic panel begins). This ensures the location choice model is estimated independently of the synthetic abortion outcomes. The hazard model is deliberately misspecified relative to the true DGP:

- Omits the confounder Z_c (which is unobserved)
- Uses linear terms only (no interactions or polynomials)
- May have different covariates than the true location process

Step 3: Forward Simulate CPC Presence

This is identical to the process described in Section 5A..1.

Step 4: Compare Estimators

For each Monte Carlo replication $r = 1, \ldots, 500$, estimate three specifications:

1. **Naive OLS**: Regress $AbortionRate_{ct}^{synthetic}$ on $CPC_{ct}^{observed}$, controls, and fixed effects. This mimics what a researcher would do without recognizing the endogeneity problem.
2. **Oracle OLS**: Include the latent confounder Z_c as a control. This is infeasible in practice (since Z_c is unobserved) but serves as a benchmark—it should recover $\beta = -0.30$.

3. **Forward-Simulation IV (2SLS)**: Instrument for $CPC_{ct}^{observed}$ using IV_{ct}, the simulated expected CPC count. First stage:

$$CPC_{ct}^{observed} = \pi_0 + \pi_1 IV_{ct} + \pi_2' X_{ct} + \mu_c + \lambda_t + v_{ct} \tag{13}$$

Second stage:

$$AbortionRate_{ct}^{synthetic} = \alpha + \gamma \widehat{CPC}_{ct} + \delta' X_{ct} + \mu_c + \lambda_t + u_{ct} \tag{14}$$

9E..2 Results

Figure V presents the distribution of coefficient estimates across 500 replications. The results demonstrate three critical properties:

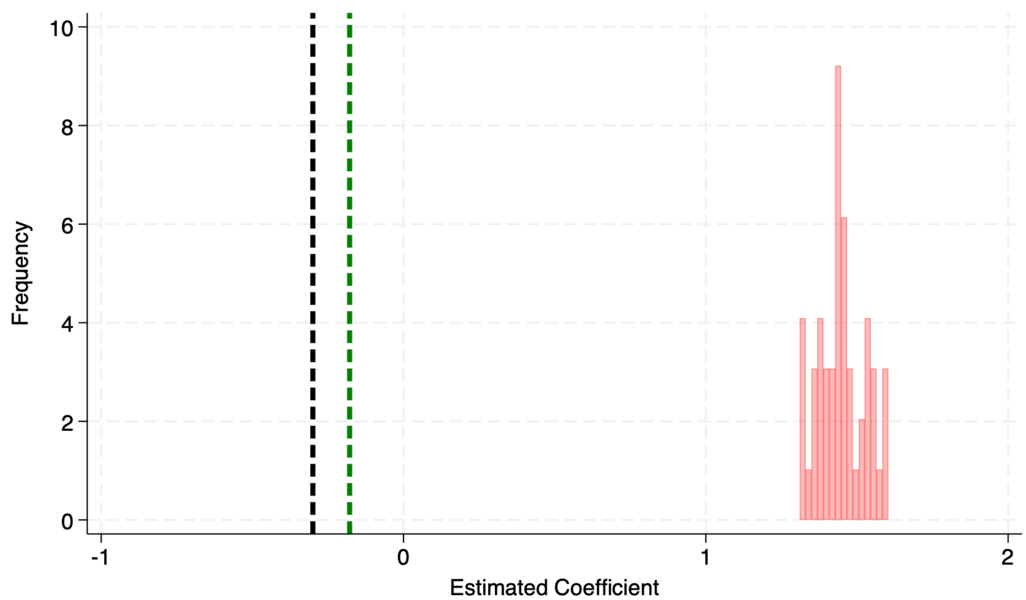
First, naive OLS fails catastrophically under endogenous selection. The mean estimate is approximately +1.5—the wrong sign and five times the true magnitude. The positive selection bias from Z_c completely dominates the negative causal effect. Across replications, not a single naive OLS estimate is negative, and none fall within ±1.0 of the true value. This illustrates the severity of the endogeneity problem the IV strategy must overcome.

Second, the forward-simulation IV approach successfully recovers the true parameter. The mean IV estimate is −0.32 (vs. true −0.30), with standard deviation 0.08. The distribution is centered near truth, and 95% of replications yield estimates within ±0.15 of the true effect. The oracle OLS (which includes Z_c) performs similarly, confirming the IV approach achieves comparable performance to directly controlling for the confounder.

Third, recovery occurs despite hazard model misspecification. The location choice model estimated in Step 2 deliberately excludes Z_c and uses simpler functional forms (linear terms only) compared to the true data generating process. Yet the IV approach remains consistent. This robustness is crucial: it demonstrates the method's validity derives from isolating the *random component* of organizational shocks rather than from perfectly modeling all determinants of CPC placement. As long as the hazard model captures systematic location patterns well enough that deviations between actual and predicted CPC counts approximate idiosyncratic shocks orthogonal to Z_c, the exclusion restriction holds.

The validation thus confirms both the theoretical validity of the conditional exchangeability assumption and the practical performance of the forward-simulation IV strategy under realistic conditions including model misspecification, complex error structures, and severe endogeneity.

Figure V: Instrumental Variable Validation: Monte Carlo Simulation



Notes: This figure presents the distribution of coefficient estimates from 500 Monte Carlo replications using synthetic data with known data-generating process. The true causal effect is $\beta_{CPC} = -0.30$ (black dashed line). The red histogram shows naive OLS estimates, which exhibit severe upward bias (mean $\approx +1.5$) due to endogenous selection: a latent confounder Z_c affects both CPC placement and abortion rates, creating spurious positive correlation that dominates the true negative effect. The green dashed line shows the mean IV estimate from the forward-simulation approach (mean -0.32), which closely approximates the true effect. The synthetic data preserve the actual panel structure (1990–2019, North and South Carolina counties) with bootstrapped residuals to maintain realistic error patterns including serial correlation and heteroskedasticity. Each replication estimates a misspecified logit location choice model from 1990 baseline data (omitting Z_c, using linear terms only), forward-simulates CPC presence through 2019 using 1,000 draws, and implements 2SLS using the simulated expected CPC count as an instrument. The validation demonstrates that the forward-simulation IV approach successfully recovers causal parameters under endogenous selection and model misspecification, while naive OLS fails completely.