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.

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

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1 Introduction

The federal legalization of induced abortion in 1973 fundamentally altered women's reproductive decision-making. In response, pro-life organizations began establishing crisis pregnancy centers (CPCs) with the goal of reducing abortion incidence (Care Net, 2022).[1] CPCs are motivated by religious belief, yet their emergence also reflects an economic response to shifting reproductive norms. Akerlof, Yellen, and Katz (1996) argue that abortion legalization and improved contraceptive technology reshaped the equilibrium of marriage markets by weakening men's incentives to commit to partners and disadvantaging women who did not adopt the new technologies. Viewed through this lens, CPCs promote 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] Existing research has focused 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 also profoundly shape women's educational and labor market outcomes.[3] Yet supply-side restrictions represent only one side of the coin. This paper addresses the other side 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

[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 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).

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

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. Following Angeles, Guilkey, and Mroz (1998), who developed this approach for strategic family planning program placement, 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 CPC presence in year t conditional on historical observables—that isolates systematic placement patterns by averaging out the idiosyncratic shocks in the simulation. This approach synthesizes optimal instruments theory (Amemiya, 1974; Chamberlain, 1987; Newey, 1990) with simulation-based estimation (McFadden, 1989). When treatment

[4]Section 7 documents the spatial distribution of CPCs and abortion providers over time.

assignment is path-dependent and closed-form expressions for the IV are intractable, Monte Carlo simulation provides consistent approximations. 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. The simulated instrument corrects this bias by exploiting variation from organizational timing shocks that are plausibly orthogonal to abortion demand conditional on rich historical observables. 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, information 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.

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

(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 which 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 abstinence education or "sexual integrity" classes. Abstinence education is aimed at teenagers and is held either at the CPC or in schools.

[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.

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

[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.

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 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

[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.

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. §§ 44-41-20). 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

[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.

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

[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 7. 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.

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 III to VI show CPCs and abortion providers over time, overlaid on a

[11]The 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.

map of county-level abortion rates in NC and SC. Table I provides descriptive statistics.[11]

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.[12]

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.[13] 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).

[12]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).

[13]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.

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.[14] 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.[15] Analyses are conducted at both at the county level and the age–group level.

[14]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).

[15]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

	1990-2019		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 Empirical Framework

The research question is whether CPC presence causally affects abortion rates. The structural equation of interest is:

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

where $\text{AbortionRate}_{c,t}$ is the natural log of the abortion 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.

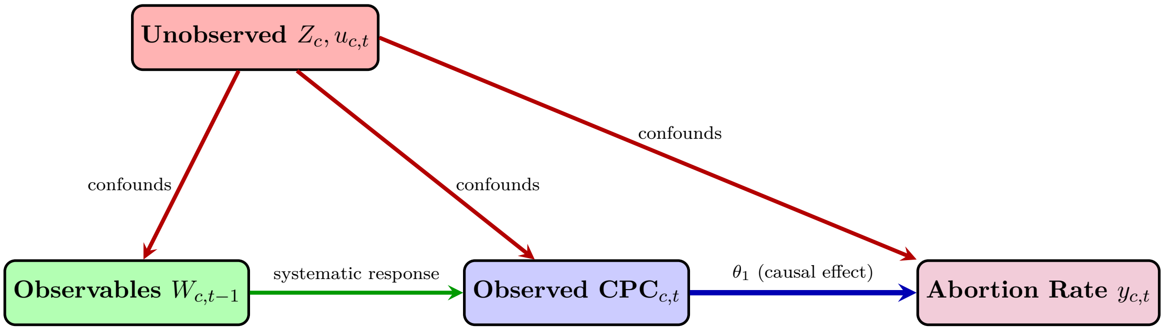
Estimating θ_1 correctly requires addressing endogenous CPC location choices. Obtaining a causal estimate via OLS requires strict exogeneity: $E[u_{c,t} \mid \{\text{CPC}_{c\tau}, X_{c\tau}\}_{\tau=1}^{T}] = 0$. This condition fails under strategic placement: organizations choosing locations in period t may respond to unobserved factors correlated with $u_{c,t}$. Figure I illustrates these confounding pathways. The instrumental variables approach developed below requires weaker conditions than strict exogeneity.

4A. The Endogeneity Problem

Strategic CPC placement creates two endogeneity problems. First, CPCs may target counties based on unobserved confounders (Z_c) correlated with abortion demand—community pro-life sentiment, informal support networks, or persistent cultural attitudes. If these unobservables both attract CPCs and independently affect abortion demand, OLS conflates the causal effect with selection bias.

Second, treatment assignment is a dynamic, path-dependent process where current CPC presence depends on the entire history of past placements. The hazard model conditions

Figure I: Directed Acyclic Graph: The Endogeneity Problem



The Challenge: Unobserved factors Z_c (community pro-life sentiment, informal support networks, cultural attitudes) and contemporaneous shocks $u_{c,t}$ create confounding pathways. These unobservables affect both which counties receive CPCs and their abortion rates, violating the exclusion restriction required for causal identification. The systematic component of CPC placement responds to observable county characteristics $W_{c,t-1}$, but these observables are themselves correlated with unobserved demand factors, creating endogeneity. Standard OLS estimation conflates the causal effect θ_1 with selection bias from these confounding pathways.

on lagged CPC counts, creating state dependence: the probability a county receives its second CPC depends on whether it already has one. This path dependence compounds over 30 years, making treatment status at time t a nonlinear function of the complete sequence of past observables and organizational shocks.

Lagged abortion rates capture both organizational responses to observable demand patterns and persistent unobserved factors Z_c. Simply controlling for past abortion rates in the structural equation does not solve the endogeneity problem because those lagged rates may proxy for the same confounders affecting current outcomes. Moreover, standard sequential exogeneity conditions fail when placement decisions respond to the accumulated history of observables through a state-dependent process.

4B. Instrumental Variables Strategy

I develop an instrumental variables (IV) strategy that exploits quasi-random variation in the timing of CPC openings. CPC openings reflect both systematic responses to observable county characteristics and idiosyncratic timing factors. While CPCs systematically target counties based on demographics, religiosity, past abortion rates, and existing service availability, the exact year a center opens depends on logistical frictions—when funding clears, when a lease becomes available, or when volunteers are recruited. Conditional on rich observables, these organizational logistics plausibly operate independently of contemporaneous abortion demand shocks, providing quasi-experimental variation.

CPC openings can be formally decomposed as

$$\mathrm{CPC}_{c,t}^{\mathrm{DGP}} = \underbrace{\mathbb{E}[\mathrm{CPC}_{c,t} \mid W_{c,t-1}]}_{\text{Systematic component}} + \underbrace{\varepsilon_{c,t}^{\mathrm{DGP}}}_{\text{Organizational timing shock}} \tag{2}$$

where the systematic component captures predictable expansion patterns based on predetermined county characteristics $W_{c,t-1}$, such as demographics, religious composition, existing CPC availability, and past abortion rates. This component may correlate with persistent unobservables Z_c affecting both CPC placement and abortion outcomes, and is thus potentially endogenous: $\mathrm{Cov}(\mathbb{E}[\mathrm{CPC}_{c,t} \mid W_{c,t-1}], u_{c,t}) \neq 0$. While $\mathrm{IV}_{c,t}$ inherits correlation with unobserved confounders Z_c through its dependence on $W_{c,t-1}$, the second-stage specification absorbs this "bad" variation through county fixed effects μ_c, year fixed effects λ_t, and time-varying controls $X_{c,t}$.Controlling for observables in the second stage purges the endogenous component of the instrument, leaving only quasi-random timing variation for identification. The timing shock $\varepsilon_{c,t}^{\mathrm{DGP}}$ captures the unpredictable organizational factors that determine when centers actually open—logistical frictions, timing of funding, and volunteer recruitment. Conditional on $W_{c,t-1}$, these shocks are plausibly orthogonal to contemporaneous abortion demand.

The simulated instrument is defined as

$$\mathrm{IV}_{c,t} = \mathbb{E}[\mathrm{CPC}_{c,t} \mid W_{c,t-1}],$$

and isolates the systematic component of CPC openings. In the two-stage least squares (2SLS) framework, second-stage controls ($X_{c,t}$, μ_c, λ_t) absorb any direct effects of predetermined observables on abortion outcomes. Identification relies on the first-stage residual variation—the timing shocks $\varepsilon^{\mathrm{DGP}}_{c,t}$—which provide quasi-random variation under the assumption

$$\mathbb{E}[\varepsilon^{\mathrm{DGP}}_{c,t} \cdot u_{c,t} \mid W_{c,t-1}] = 0.$$

I construct this instrument using forward simulation of counterfactual CPC expansion paths. First, I estimate a discrete-time hazard model of CPC openings based on predetermined county characteristics. Second, I simulate thousands of counterfactual expansion histories. Each simulation draws independent organizational shocks and uses predicted opening probabilities to determine where CPCs locate, then updates county characteristics and projects forward through time. This randomization mimics the idiosyncratic factors driving actual CPC expansion. Third, I average simulated CPC presence across all draws to construct the instrument, producing the expected probability that a county hosts a CPC in year t given predetermined characteristics.

This approach draws on optimal instruments theory (Chamberlain, 1987; Newey, 1990) and simulation-based estimation (McFadden, 1989). When treatment assignment is path-dependent—CPC presence at time t depends on the full history through $t-1$—closed-form expressions for conditional expectations are intractable. Monte Carlo simulation provides consistent approximations as the number of observations grows, even with a fixed number of simulation draws per county. The design aligns with recent work emphasizing identification from the structure of treatment assignment rather than functional form assumptions (Borusyak et al., 2022, 2025).

4B..1 Instrument Construction via Forward Simulation

Forward simulation constructs the instrument by averaging over many counterfactual CPC expansion paths, following the procedure developed by Angeles et al. (1998) for endogenous family planning program placement. I estimate a hazard model of CPC openings using predetermined county characteristics, then simulate 1,000 expansion paths from 1990 through 2019. Each path combines systematic predictions from observables with independently drawn timing shocks.

CPC expansion is path-dependent: whether a CPC opens in one year changes future observables (lagged CPC counts, distances) and thus affects all subsequent opening probabilities. An early timing shock that triggers a CPC opening alters the entire future trajectory. Each simulated path represents one plausible history of how the expansion could have unfolded given the structural model and random timing variation.

Averaging across simulations yields the instrument:

$$\mathrm{IV}_{c,t} = \frac{1}{S}\sum_{s=1}^{S}\mathrm{CPC}_{c,t}^{(s)} \xrightarrow{p} \mathbb{E}[\mathrm{CPC}_{c,t}|W_{c,t-1}] \tag{3}$$

The law of large numbers eliminates the idiosyncratic timing shocks, leaving only the systematic component. The simulations use both systematic responses and timing shocks to generate realistic trajectories—the timing shocks shape the distribution of paths—but averaging removes their specific realizations from the instrument. This isolates systematic variation (which second-stage controls absorb), leaving quasi-random timing variation in the first-stage residuals for identification. Technical details on the error structure are provided in Appendix 10.

4B..2 Structural Model and Exclusion

The structural model formalizes how timing shocks enter the data generating process. CPC location decisions follow a random utility framework where organizations compare opening a new center to maintaining the status quo:

$$\text{CPC Decision:} \quad \text{NewCPC}_{c,t} = \mathbb{1}[\beta' W_{c,t-1} + \varepsilon_{c,t} > 0], \tag{4}$$

$$\text{Outcome Equation:} \quad \text{AbortionRate}_{c,t} = \theta_1 \text{CPC}_{c,t} + \theta_2' X_{c,t} + \mu_c + \lambda_t + u_{c,t}, \tag{5}$$

where $\mathbb{1}[\cdot]$ denotes the indicator function. The timing shock $\varepsilon_{c,t}$ enters only the CPC decision equation, not the outcome equation, ensuring timing shocks affect abortion rates solely through their effect on whether a CPC opens.

The exclusion restriction follows: conditional on predetermined observables and controls, timing shocks influence abortion rates only through observed CPC presence. Organizational logistics—fundraising cycles spanning months, real estate negotiations facing idiosyncratic delays, volunteer recruitment depending on local networks—operate on different timescales and through different mechanisms than the contemporaneous shocks $u_{c,t}$ that drive annual abortion rate fluctuations. This structure parallels standard timing-shock IV arguments in labor and public economics where the timing of program rollout conditional on eligibility is treated as quasi-random.

4C. Identifying Assumptions

Valid instrumental variables estimation requires two core assumptions beyond the standard first-stage relevance condition of 2SLS.

Assumption 1 (Conditional Orthogonality): Conditional on observable history, actual organizational timing shocks are mean-independent of unobserved abortion demand shocks.

After controlling for past county characteristics—lagged CPC presence, distances to existing services, demographics, religious composition, political ideology, and past abortion rates—the remaining variation in *when* CPCs open must be uncorrelated with contemporaneous unobserved shocks to abortion demand.

Formally, let $\mathcal{F}_{c,t-1} = \{W_{c,\tau}\}_{\tau=1}^{t-1}$ denote the historical information set, where $W_{c,\tau}$ contains the observable county characteristics that enter the hazard model. The assumption requires:

$$\varepsilon_{c,t}^{\mathrm{DGP}} \perp u_{c,t} \mid \mathcal{F}_{c,t-1}, \tag{6}$$

or equivalently,

$$\mathbb{E}[\varepsilon_{c,t}^{\mathrm{DGP}} \mid u_{c,t}, \mathcal{F}_{c,t-1}] = \mathbb{E}[\varepsilon_{c,t}^{\mathrm{DGP}} \mid \mathcal{F}_{c,t-1}]. \tag{7}$$

This is a standard sequential ignorability condition (Robins, Mark, & Newey, 1992). The assumption does not require unconditional exogeneity of timing shocks. Instead, it requires that conditional on a rich set of lagged observables, the remaining variation in organizational timing is uncorrelated with contemporaneous unobservables not captured by

[16] Identification requires that *DGP* timing shocks $\varepsilon_{c,t}^{\mathrm{DGP}}$ satisfy this condition. The simulation draws $\varepsilon_{c,t}^{(s)}$ used to construct the instrument satisfy mean independence mechanically by design, but this is not the source of identification. The simulation provides a computational method to approximate $\mathbb{E}[\mathrm{CPC}_{c,t} \mid W_{c,t-1}]$, while identification derives from the economic argument that actual organizational timing is mean-independent of unobserved abortion demand conditional on $\mathcal{F}_{c,t-1}$.

county fixed effects, year fixed effects, or observed time-varying characteristics.[16]

Assumption 2 (Predetermination): Current-period abortion shocks are assumed to be unpredictable from the historical information set. Formally,

$$\mathbb{E}[u_{c,t} \mid \mathcal{F}_{c,t-1}] = 0, \tag{8}$$

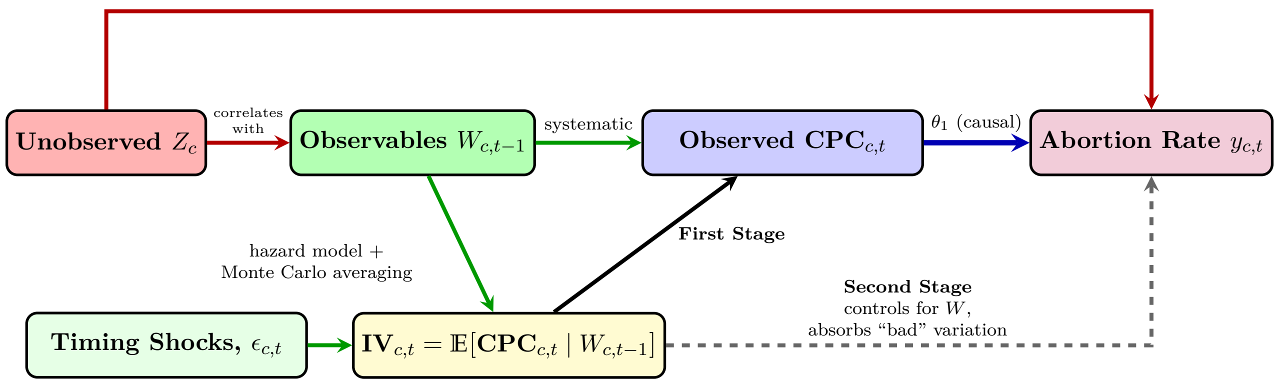
where $\mathcal{F}_{c,t-1} = \{W_{c,\tau}\}_{\tau=1}^{t-1}$ denotes the history of observable county characteristics.

This assumption requires the structural error $u_{c,t}$ to have zero conditional mean given the *entire* observed history. It is therefore *stronger* than assuming $\mathbb{E}[u_{c,t} \mid W_{c,\tau}] = 0$ for each $\tau < t$: conditioning on the full history rules out any linear or nonlinear combination of past observables being able to forecast the shock.

After conditioning on county fixed effects μ_c, year fixed effects λ_t, and the history in $\mathcal{F}_{c,t-1}$, the remaining period-t shocks must be orthogonal to all past observables. County fixed effects absorb time-invariant heterogeneity, and lagged observables absorb persistent trends operating through observable channels. The assumption then requires that any remaining variation in abortion shocks is genuinely unanticipated relative to past information.

Importantly, this condition is not a completeness assumption: it does not require $\mathcal{F}_{c,t-1}$ to include all determinants of abortion rates. It only requires that any omitted determinants enter through an error term whose *unexplained component has zero conditional mean* with respect to the observed history.

Figure II: Directed Acyclic Graph: Simulated Instrument Identification Strategy



The Solution: The instrument is the simulated conditional mean $\text{IV}_{c,t} = \mathbb{E}[\text{CPC}_{c,t} \mid W_{c,t-1}]$, constructed by forward-simulating the structural model. Because each simulation averages over 1,000 draws of the idiosyncratic timing shock, the instrument contains *no specific realization* of $\varepsilon_{c,t}$. The instrument $\text{IV}_{c,t}$ is *conditionally* endogenous before controlling for $W_{c,t-1}$, but becomes exogenous conditional on second-stage controls. Identification comes from the remaining source of variation: the gap between observed CPC presence and the simulated mean. These gaps are driven entirely by the realized organizational timing shocks $\varepsilon_{c,t}^{\text{DGP}}$. The first stage regresses observed CPC on the instrument, exploiting variation from realized timing shocks to predict observed CPC openings. The second stage then uses only this "good" shock-driven variation to estimate θ_1. The maintained identifying assumption is that CPC opening shocks are orthogonal to abortion demand shocks conditional on $W_{c,t-1}$: $\mathbb{E}[\varepsilon_{c,t}^{\text{DGP}} \cdot u_{c,t} \mid W_{c,t-1}] = 0$.

4C..1 How Assumptions 1 and 2 Ensure Valid Identification

These two assumptions work together to ensure valid IV identification:

Under Assumption 2, the instrument satisfies the exclusion restriction. Since the instrument converges to $\text{IV}_{c,t} \xrightarrow{p} \mathbb{E}[\text{CPC}_{c,t}|W_{c,t-1}]$ and is measurable with respect to $\mathcal{F}_{c,t-1}$, predetermination directly implies:

$$\mathbb{E}[u_{c,t} \cdot \text{IV}_{c,t}] = \mathbb{E}[\mathbb{E}[u_{c,t} \cdot \text{IV}_{c,t}|\mathcal{F}_{c,t-1}]] \tag{9}$$

$$= \mathbb{E}[\text{IV}_{c,t} \cdot \mathbb{E}[u_{c,t}|\mathcal{F}_{c,t-1}]] \tag{10}$$

$$= 0 \tag{11}$$

This shows the instrument is exogenous to the structural error, meaning it affects outcomes only through treatment—the exclusion restriction.

Assumption 1 ensures the first-stage residuals—the actual timing shocks $\varepsilon_{c,t}^{\text{DGP}}$ that drive deviations between predicted and realized CPC presence—satisfy $\mathbb{E}[\varepsilon_{c,t}^{\text{DGP}} \cdot u_{c,t}|\mathcal{F}_{c,t-1}] = 0$. This is essential because the first stage uses these deviations as its source of variation. If these deviations were correlated with outcome errors, the 2SLS estimator would be inconsistent even though the instrument itself is exogenous.

Together, the assumptions ensure: (1) the instrument affects outcomes only through treatment (exclusion from Assumption 2), and (2) the variation that identifies the first stage is orthogonal to outcome errors (valid identification from Assumption 1).

4D. Implementation

4D..1 Hazard Model of CPC Entry

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:

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

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

$$p_{c,t} = P(\text{NewCPC}_{c,t} = 1 \mid W_{c,t-1}) = \frac{\exp(\beta' W_{c,t-1})}{1 + \exp(\beta' W_{c,t-1})} \tag{12}$$

The empirical specification is:

$$\text{logit}(p_{c,t}) = \beta_1 \text{CPC}_{c,t-1} + \beta_2 \text{Dist}_{c,t-1} + \beta_3 \text{AR}_{c,t-1} + \beta_4' X_{c,t-1} + \mu_c + \lambda_t, \tag{13}$$

where $\text{AR}_{c,t-1}$ captures organizational responses to local abortion demand, $\text{CPC}_{c,t-1}$ and $\text{Dist}_{c,t-1}$ measure existing service accessibility, and $X_{c,t-1}$ includes abortion provider presence, demographics (population, age structure, religious composition), and economic controls (unemployment, nonprofit wages). County fixed effects μ_c absorb time-invariant differences; year fixed effects λ_t capture temporal shocks. I estimate via maximum likelihood with standard errors clustered at the county level.

4D..2 Forward Simulation Procedure

The forward simulation algorithm computes $\mathbb{E}[\text{CPC}_{c,t}|W_{c,t-1}]$ through Monte Carlo averaging. For each county c and target year t, I generate S = 1,000 simulated CPC expansion paths:

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

   $$\widehat{p}^{(s)}_{c\tau} = \Lambda\left(\widehat{\beta}_1 \widehat{\text{CPC}}^{(s)}_{c,\tau-1} + \widehat{\beta}_2 \widehat{\text{Dist}}^{(s)}_{c,\tau-1} + \widehat{\beta}_3 \text{AR}_{c,\tau-1} + \widehat{\beta}'_4 X_{c,\tau-1} + \widehat{\mu}_c + \widehat{\lambda}_\tau\right) \quad (14)$$

   where $\Lambda(\cdot) = \exp(\cdot)/(1+\exp(\cdot))$ is the logistic CDF;

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

   (c) Determine opening: $\widehat{\text{NewCPC}}^{(s)}_{c\tau} = \mathbb{1}[U^{(s)}_{c\tau} \leq \widehat{p}^{(s)}_{c\tau}]$;

   (d) Update cumulative count: $\widehat{\text{CPC}}^{(s)}_{c\tau} = \widehat{\text{CPC}}^{(s)}_{c,\tau-1} + \widehat{\text{NewCPC}}^{(s)}_{c\tau}$;

   (e) Update distance: $\widehat{\text{Dist}}^{(s)}_{c\tau}$ using median distance to nearest CPC across counties in simulation s.

3. **Construct instrument:** Average across independent draws:

$$\text{IV}_{c,t} = \frac{1}{S}\sum_{s=1}^{S} \widehat{\text{CPC}}^{(s)}_{c,t} \xrightarrow{p} \mathbb{E}[\text{CPC}_{c,t} \mid W_{c,t-1}] \quad (15)$$

The uniform draws $U_{c\tau}^{(s)}$ generate realizations from the logit error distribution, enabling Monte Carlo integration. As S increases, the Law of Large Numbers ensures the instrument converges to the conditional expectation. The synthetic shocks $\varepsilon_{c,t}^{(s)}$ (implicitly defined through the uniform draws) are computational devices, not the actual shocks determining real CPC placement. By averaging over these synthetic shocks, the instrument depends only on predetermined observables $W_{c,t-1}$, not on any particular timing realization.

Role of Path Dependence. The recursive structure is essential because current opening probabilities depend on lagged CPC counts and distances, which themselves depend on all previous openings. This path dependence makes analytical computation of $\mathbb{E}[\text{CPC}_{c,t}|W_{c,t-1}]$ intractable. Forward simulation handles this naturally by generating complete expansion paths that preserve the state-dependent dynamics. Additional implementation details appear in Appendix 11..3.

Including Lagged Abortion Rates. The conditioning set $W_{c,t-1}$ includes lagged abortion rates alongside exogenous variables (demographics, geography, religious composition). Including lagged outcomes is crucial: CPCs systematically respond to past abortion demand when choosing locations. Excluding these variables would leave predictable organizational responses in the residual, violating orthogonality. Including them ensures all systematic behavior is captured in $\mathbb{E}[\text{CPC}_{c,t}|W_{c,t-1}]$, making the residual truly idiosyncratic. This does not violate exclusion because: (1) lagged rates are predetermined—realized before current shocks, and (2) their direct effects on current rates operate through observable channels absorbed by second-stage controls ($X_{c,t}$, μ_c, λ_t).

4E. Estimation

The 2SLS estimation proceeds in two stages:

First Stage:

$$\mathrm{CPC}_{c,t} = \pi_0 + \pi_1 \mathrm{IV}_{c,t} + \pi_2' X_{c,t} + \mu_c + \lambda_t + v_{c,t} \tag{16}$$

Second Stage:

$$\mathrm{AbortionRate}_{c,t} = \alpha + \gamma \widehat{\mathrm{CPC}}_{c,t} + \delta' X_{c,t} + \mu_c + \lambda_t + u_{c,t} \tag{17}$$

Standard errors are clustered at the county level to account for arbitrary within-county correlation.

The first-stage regression decomposes observed CPC presence into predicted values $\widehat{\mathrm{CPC}}_{c,t}$ (variation explained by the instrument and controls) and residuals $v_{c,t}$ (variation unexplained by the instrument). Second-stage controls ($X_{c,t}$, μ_c, λ_t) absorb any direct effects of current observables on abortion rates. What remains in $\widehat{\mathrm{CPC}}_{c,t}$—the variation orthogonal to these controls—reflects only the systematic component of historical observables net of direct effects.

Under the identifying assumptions, first-stage residuals capture organizational timing shocks: $v_{c,t} = \varepsilon_{c,t}^{\mathrm{actual}}$. These residuals are orthogonal to the instrument by construction and orthogonal to structural errors $u_{c,t}$ by assumption (conditional orthogonality).[17] Therefore, the fitted values $\widehat{\mathrm{CPC}}_{c,t}$ provide valid identification of the causal effect γ.

[17]More precisely, 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. Misspecification affects the *approximation* of $\mathbb{E}[\mathrm{CPC}_{c,t} \mid W_{c,t-1}]$ but not the *orthogonality* of the remaining variation, provided the maintained assumptions hold in the DGP.

The hazard model for the first stage includes lagged CPC counts, distance to nearest CPC and abortion provider, lagged abortion rates, demographic composition (age structure, racial composition, population), religious adherence (Catholic and evangelical shares), political ideology (Republican vote share), and economic conditions (unemployment, median income). All predictors are lagged one year to ensure predetermination. I model CPC counts as a renewal process, allowing multiple openings over time while maintaining the logit structure for each marginal opening decision. Full specification details are provided in Section ??.

4E..1 What Identification Requires

Valid identification requires that the actual organizational timing shocks satisfy the orthogonality and predetermination conditions stated in Assumptions 1 and 2. The simulation draws $\varepsilon_{c,t}^{(s)}$ satisfy orthogonality mechanically by construction, but this is not the source of identification. Rather, the simulation provides a computational method to approximate $\mathbb{E}[\mathrm{CPC}_{c,t}|W_{c,t-1}]$. Identification derives from the economic assumption that actual organizational timing—conditional on rich predetermined observables—is mean-independent of contemporaneous abortion demand shocks.

Hazard model misspecification affects efficiency but not consistency, provided the orthogonality and predetermination assumptions hold. Monte Carlo validation (§4H.) demonstrates that the approach successfully recovers causal parameters even with misspecified hazard models, confirming that robustness stems from isolating quasi-random timing variation rather than perfectly modeling all placement determinants.

4F. Methodological Foundations

The three methodological traditions underlying the approach provide complementary tools for addressing endogenous organizational placement.

4F..1 Why This Approach Works

Chamberlain (1987) and Newey (1990) establish that instruments equal to $\mathbb{E}[\text{CPC}_{c,t} \mid W_{c,t-1}]$ achieve asymptotic efficiency while requiring only that $W_{c,t-1}$ be predetermined. The conditioning set can include variables correlated with unobserved confounders, provided their direct effects on outcomes are controlled in the second stage.

Robins et al. (1992) shows that modeling the exposure mechanism can avoid specifying complex outcome pathways. This applies naturally here: CPC location decisions follow observable patterns through the hazard model, while the channels through which unobserved community characteristics affect fertility outcomes may be far more complex. Identification requires that the hazard model captures systematic organizational responses and that idiosyncratic placement factors are orthogonal to unobserved abortion demand. The logit specification improves efficiency when correct, but consistency depends only on the orthogonality assumption.

McFadden (1989) provides computational tools for settings where analytical solutions are infeasible. In this dynamic panel setting with path-dependent treatment, current treatment probabilities depend on the entire history of past realizations. Forward simulation recursively computes $\mathbb{E}[\text{CPC}_{c,t} \mid W_{c,t-1}]$ by generating complete expansion paths, naturally accommodating the nonlinear interactions between lagged CPC counts, distances, abortion rates, and other covariates.

4F..2 Connection to Formula Instruments

The simulated IV approach parallels the "formula instruments" framework of Borusyak et al. (2025). Both address the problem that random shocks alone do not guarantee valid instruments when units have differential exposure through observable characteristics. Even with i.i.d. simulation draws, the instrument $\mathrm{IV}_{c,t}$ varies systematically with $W_{c,t-1}$. Borusyak et al. (2025) show that identification requires adjusting for the expected instrument $\mu_{c,t} = \mathbb{E}[\mathrm{IV}_{c,t}|\mathcal{F}_{c,t-1}]$ through either recentering ($\widetilde{\mathrm{IV}}_{c,t} = \mathrm{IV}_{c,t} - \mu_{c,t}$) or controlling for functions of $W_{c,t-1}$ in the second stage.

The baseline specification includes county fixed effects, year fixed effects, and time-varying controls. County fixed effects absorb the time-invariant component of the expected instrument, while year effects capture common trends. This approach bears resemblance to the Borusyak et al. (2025) controlling framework, though the specification includes some contemporaneous controls $X_{c,t}$ beyond the predetermined information set.

4G. 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 responds to the predictable component of organizational expansion captured in $\mathrm{IV}_{c,t}$.

Compliers are policy-relevant for two reasons. First, they represent marginal locations where predictable factors determine CPC entry—counties on the boundary between treatment and control. These are 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 local conditions. Compliers thus correspond to counties where 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, 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.

4H. Validation and Limitations

Valid identification requires that Assumptions 1 and 2 hold in the actual data generating process. I provide four forms of evidence supporting the plausibility of the identification strategy, while acknowledging what cannot be empirically verified.

4H..1 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. I construct latent confounders Z_c that affect both CPC placement and abortion rates, creating the type of endogeneity the IV strategy is designed to address.

Across 1,000 replications with true causal effect $\theta_1 = -0.30$, OLS yields severely biased estimates of the wrong sign (mean $\approx +1.5$), 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 quasi-random timing variation rather than perfectly modeling all placement determinants. Full simulation details and results appear in Appendix 12

4H..2 Empirical Validation

Specification Robustness. Results are stable across alternative hazard model specifications, alternative instrument constructions, and alternative sets of second-stage controls. This stability suggests findings reflect genuine causal relationships rather than artifacts of particular modeling assumptions.

Placebo Tests. The instruments do not predict pre-1990 abortion rates (before any CPC openings in the sample), confirming temporal precedence and suggesting the instruments are not simply proxying for persistent county characteristics that independently determine abortion trends.

First-Stage Strength. Strong first-stage F-statistics (≈ 120) indicate high instrument relevance, reducing finite-sample bias concerns. Instrument strength is stable across alternative specifications, suggesting the hazard model captures systematic patterns in CPC expansion rather than fitting idiosyncratic noise.

4H..3 Specific Limitations

What Identification Requires. Valid identification requires that actual organizational timing shocks satisfy the orthogonality and predetermination conditions (Assumptions

1-2). These are maintained assumptions that cannot be directly tested. The exclusion restriction rests on the economic argument that organizational timing factors—donor fundraising cycles, real estate availability, volunteer recruitment—operate on different timescales and through different mechanisms than contemporaneous abortion demand shocks. While this assumption is supported by institutional context, it remains an identifying assumption rather than an empirically verifiable fact.

Hazard Model Specification. The instrument $\widehat{\mathrm{IV}}_{c,t} = \mathbb{E}[\mathrm{CPC}_{c,t}|W_{c,t-1}, \hat{\beta}]$ depends on estimated parameters $\hat{\beta}$. Misspecification affects efficiency but not consistency, provided Assumptions 1-2 hold. However, severe misspecification could leave systematic patterns in residuals, potentially violating orthogonality. Strong and stable first-stage F-statistics (≈ 120) across alternative specifications suggest these concerns are modest empirically. Conventional 2SLS standard errors do not account for first-stage estimation uncertainty, though bootstrapped standard errors provide a robustness check.

Lagged Outcomes as Controls. The hazard model includes $\mathrm{AbortionRate}_{c,t-1}$ in $W_{c,t-1}$. If persistent unobservables Z_c affect abortion rates across periods, lagged outcomes may capture this persistence, potentially reintroducing correlation with current-period errors. The one-year lag provides protection against mechanical simultaneity. More importantly, if lagged abortion rates proxied for omitted persistent confounders, the instrument would systematically predict differential pre-treatment trends. Placebo tests (§**??**) find no evidence of pre-trends, supporting the interpretation that lagged rates capture predictable demand dynamics rather than persistent unobserved confounding.

Additional Robustness. The panel structure provides additional robustness through dynamic over-identification (Bhargava, 1991; Liu, Mroz, & Van der Klaauw, 2010). With 10 time-varying covariates over 30 years, approximately 290 moment conditions hold simultaneously under the predetermination condition. The forward simulation implicitly aggregates information from these moment conditions through the structural hazard model, providing robustness to specification errors. Details appear in Appendix 11..2.

5 Results

5A. Two-Stage Least Squares Results

Table II presents 2SLS estimation results using the forward-simulated instruments described in Section 10D. The specifications progressively introduce distance measures to test whether geographic access independently affects abortion rates beyond the discrete presence of CPCs.

5A..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 observed 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 col-

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 10D.. 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.

umn (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 10D.. 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.

5A..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 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

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.

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.

5B. Policy Considerations

5B..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.[18] During most of the study period, all neighboring states—Tennessee, Georgia, and Virginia—also enforced comparable parental involvement laws.[19] 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.

6 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

[18] 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).

[19] 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); **?**.

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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Appendix

7 Maps

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

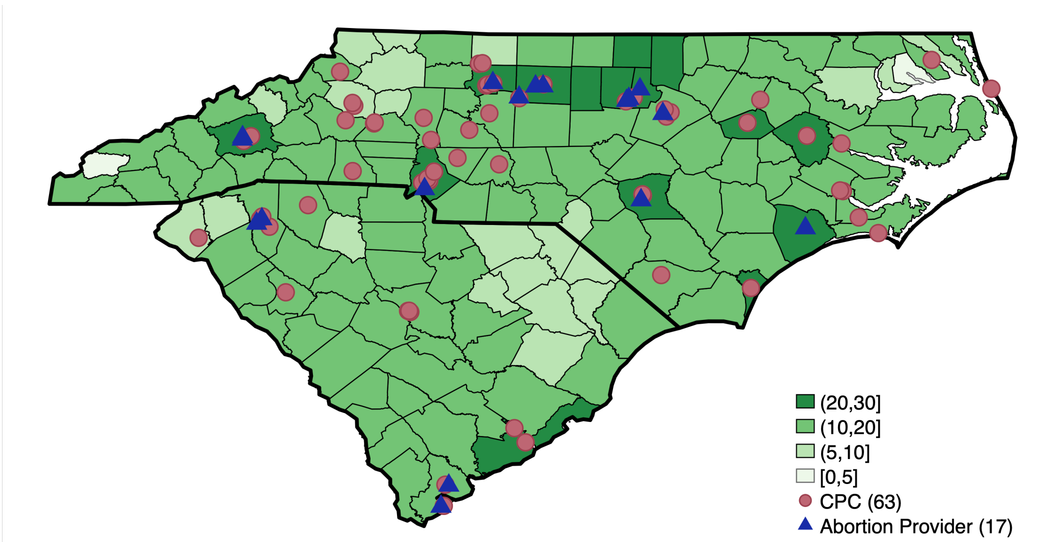


Figure IV: CPCs, Abortion Providers and Abortion Rate (NC & SC): 2000

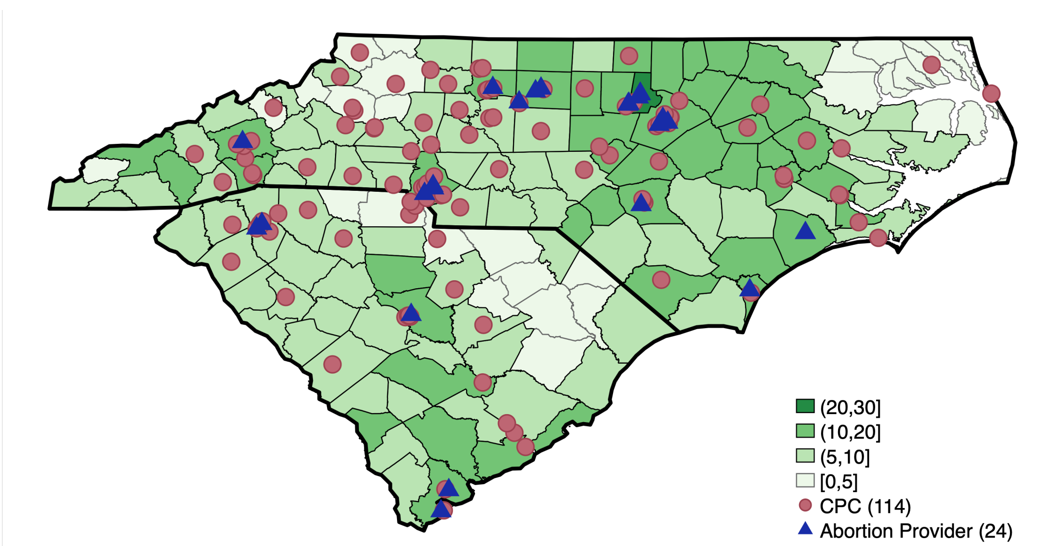


Figure V: CPCs, Abortion Providers and Abortion Rate (NC & SC): 2010

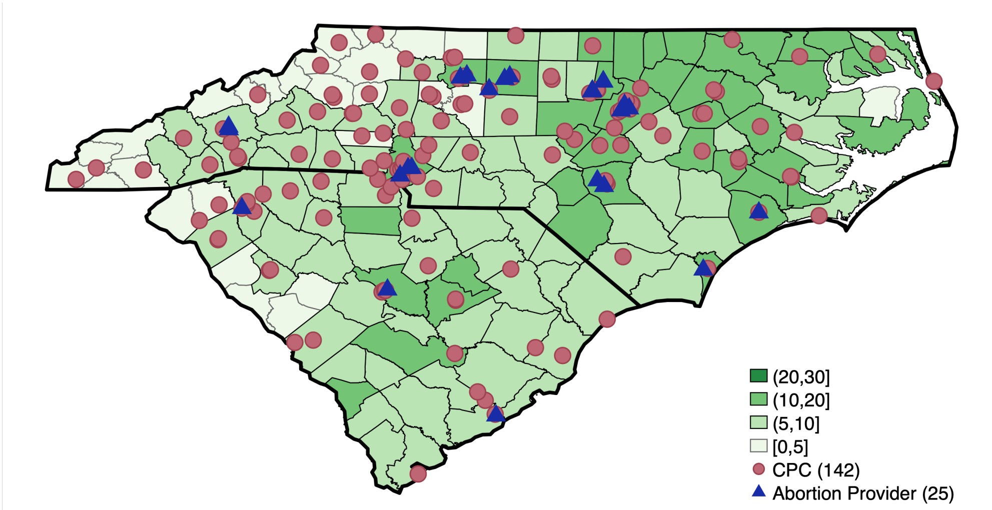
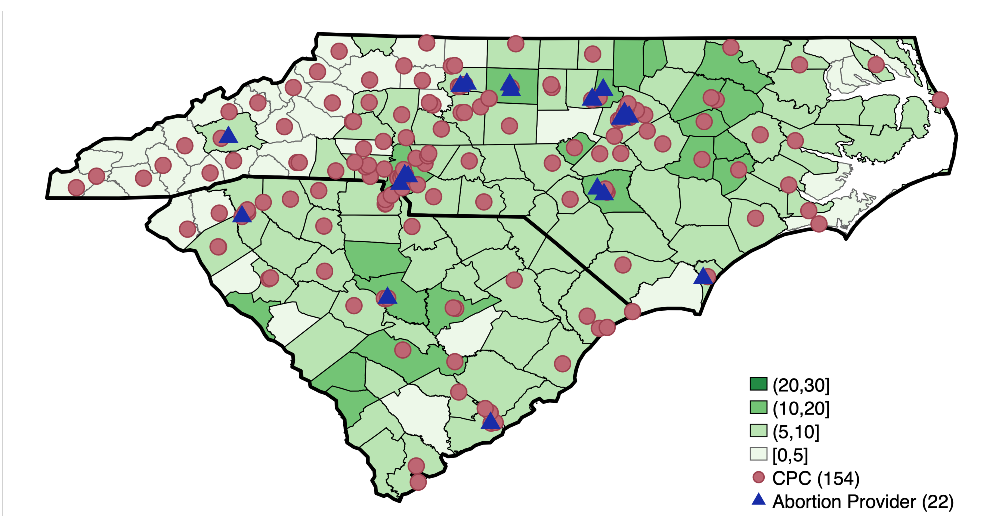


Figure VI: CPCs, Abortion Providers and Abortion Rate (NC & SC): 2019



8 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

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 $\mathrm{IV}_{c,t} = \mathbb{E}[\mathrm{CPC}_{c,t} \mid \mathcal{F}_{c,t-1}]$ 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:

$$\mathbb{E}[\mathrm{CPC}_{c,t} \mid \mathcal{F}_{c,t-1}] = \int \mathrm{CPC}_{c,t}(\boldsymbol{\varepsilon}, \mathcal{F}_{c,t-1})\, f(\boldsymbol{\varepsilon} \mid \mathcal{F}_{c,t-1})\, d\boldsymbol{\varepsilon} \tag{18}$$

where $\mathrm{CPC}_{c,t}(\boldsymbol{\varepsilon}, \mathcal{F}_{c,t-1})$ is CPC presence as a function of the shock sequence $\boldsymbol{\varepsilon} = \{\varepsilon_{c,1991}, \ldots, \varepsilon_{c,t}\}$ 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:

$$\mathrm{CPC}_{c,t} = \sum_{\tau=1991}^{t} \mathrm{NewCPC}_{c\tau}$$

$$\mathrm{NewCPC}_{c\tau} = \mathbb{1}\left\{\varepsilon_{c\tau} \leq \boldsymbol{\Phi}^{-1}\left(\Lambda(\beta'\mathbf{X}_{c,\tau-1} + \gamma \cdot \mathrm{CPC}_{c,\tau-1})\right)\right\}$$

where $\Lambda(\cdot)$ is the logistic CDF, $\boldsymbol{\Phi}^{-1}(\cdot)$ is the inverse Type I Extreme Value CDF, and $\mathrm{CPC}_{c,\tau-1}$ 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 $S = 1,000$ 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, $\widehat{\mathrm{IV}}_{c,t} \xrightarrow{\text{a.s.}} \mathbb{E}[\mathrm{CPC}_{c,t} \mid \mathcal{F}_{c,t-1}]$ 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,

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

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 $\widehat{\mathrm{CPC}}_{c,t}^{(s)}$ 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:

$$\text{Var}\left[\widehat{\text{IV}}_{c,t}\right] = \frac{1}{S}\text{Var}\left[\widehat{\text{CPC}}^{(s)}_{c,t}\right]$$

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

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

10 Error Structure and Identification

This appendix provides a detailed exposition of the error structure underlying the instrumental variables strategy, clarifying the relationships between theoretical distributions, population data generating processes, observed realizations, and simulated quantities. Understanding these distinctions is essential for evaluating the identifying assumptions.

10A. Hierarchy of Error Terms

The empirical strategy involves four conceptually distinct types of variation, each playing a specific role in identification:

10A..1 Structural Errors ($u_{c,t}$)

The structural error $u_{c,t}$ in the abortion rate equation represents unobserved shocks to abortion demand that are orthogonal to observables and fixed effects:

$$\text{AbortionRate}_{c,t} = \theta_1 \text{CPC}_{c,t} + \theta_2' X_{c,t} + \mu_c + \lambda_t + u_{c,t} \tag{19}$$

These errors capture contemporaneous variation in abortion rates not explained by CPC presence, time-varying observables, county fixed effects, or year fixed effects. Examples include unexpected economic shocks, changes in local social norms, or transitory shifts in pregnancy rates.

The fundamental endogeneity problem arises if $\mathbb{E}[\text{CPC}_{c,t} \cdot u_{c,t}] \neq 0$. This correlation could emerge through two channels: (1) CPCs respond to persistent unobserved factors Z_c (community pro-life sentiment, informal support networks) that also affect abortion rates, or (2) CPCs respond to transitory demand shocks that persist long enough to influence placement decisions.

10A..2 Organizational Timing Shocks ($\varepsilon_{c,t}$)

Theoretical Distribution. The organizational timing shock $\varepsilon_{c,t}$ is a random variable distributed Type I extreme value (Gumbel) with location 0 and scale 1:

$$\varepsilon_{c,t} \sim \text{Gumbel}(0,1) \quad \text{with CDF } F(\varepsilon) = \exp(-\exp(-\varepsilon)) \tag{20}$$

This distributional assumption generates the logit model for CPC opening probabilities through the random utility framework.

Population Data Generating Process ($\varepsilon_{c,t}^{\mathbf{DGP}}$). In the population DGP, each county-year has a realized organizational timing shock $\varepsilon_{c,t}^{\text{DGP}}$ drawn from this distribution. These population-level shocks determine the systematic process by which CPCs actually choose locations:

$$\text{CPC}_{c,t}^{\text{DGP}} = f(W_{c,t-1}, \varepsilon_{c,t}^{\text{DGP}}) \quad \text{where } \varepsilon_{c,t}^{\text{DGP}} \sim \text{Gumbel}(0,1) \tag{21}$$

The function $f(\cdot)$ represents the recursive forward simulation: starting from initial conditions, each period's CPC presence depends on lagged observables and that period's organizational shock, with current presence affecting future observables.

Observed Realization ($\tilde{\varepsilon}_{c,t}$). In the observed data, we see one particular realization $\tilde{\varepsilon}_{c,t}$ of the population shock for each county-year. This realized shock, combined with observed history $W_{c,t-1}$, generated the observed CPC count $\widetilde{\text{CPC}}_{c,t}$ in the data. We never observe $\tilde{\varepsilon}_{c,t}$ directly—we only observe its effect through $\widetilde{\text{CPC}}_{c,t}$.

Simulated Shocks ($\varepsilon_{c,t}^{(s)}$). For each simulation $s = 1, \ldots, S$, we draw independent organizational shocks $\varepsilon_{c,t}^{(s)} \sim \text{Gumbel}(0,1)$ to generate counterfactual CPC expansion paths:

$$\text{CPC}_{c,t}^{(s)} = f(W_{c,t-1}, \varepsilon_{c,t}^{(s)}) \quad \text{where } \varepsilon_{c,t}^{(s)} \overset{\text{iid}}{\sim} \text{Gumbel}(0,1) \tag{22}$$

These simulated shocks are independent across simulations, across counties, and across time periods. They are also independent of structural errors $u_{c,t}$ by construction—this is not an assumption but a mechanical property of the simulation algorithm.

10A..3 First-Stage Residuals ($v_{c,t}$)

The first-stage regression yields residuals:

$$v_{c,t} = \text{CPC}_{c,t} - \mathbb{E}[\text{CPC}_{c,t} | \text{IV}_{c,t}, X_{c,t}, \mu_c, \lambda_t] \tag{23}$$

Under the identifying assumptions (detailed below), these residuals capture the realized organizational timing shock:

$$v_{c,t} = \varepsilon_{c,t}^{\text{DGP}} \tag{24}$$

This equality holds because: (1) the instrument $\text{IV}_{c,t} \approx \mathbb{E}[\text{CPC}_{c,t} | W_{c,t-1}]$ captures all systematic variation, and (2) controls $X_{c,t}, \mu_c, \lambda_t$ absorb direct effects of observables. What remains in $v_{c,t}$ is the pure timing shock component.

10B. Structural Model

The structural model specifies how timing shocks enter the data generating process. CPC location decisions follow a random utility framework where organizations compare opening a new center to maintaining the status quo:

$$\text{CPC Decision:} \quad \text{NewCPC}_{c,t} = \mathbb{1}[\beta' W_{c,t-1} + \varepsilon_{c,t} > 0], \tag{25}$$

$$\text{Outcome Equation:} \quad \text{AbortionRate}_{c,t} = \theta_1 \text{CPC}_{c,t} + \theta_2' X_{c,t} + \mu_c + \lambda_t + u_{c,t}, \tag{26}$$

where $\mathbb{1}[\cdot]$ denotes the indicator function. The timing shock $\varepsilon_{c,t}$ enters only the CPC decision equation, not the outcome equation. This specification ensures timing shocks affect abortion rates solely through their effect on whether a CPC opens—the exclusion restriction required for identification.

Intuitively, organizational logistics determining when centers open operate on different timescales and through different mechanisms than contemporaneous shocks $u_{c,t}$ that drive annual abortion rate fluctuations. Funding campaigns span months or years; real estate negotiations face idiosyncratic delays; volunteer recruitment depends on local social networks. These organizational factors plausibly vary independently of quarterly or annual shifts in abortion demand that generate $u_{c,t}$.

10C. The Role of Simulation in Identification

The simulation procedure serves two distinct purposes:

10C..1 Computational Purpose: Monte Carlo Integration

Simulation provides a computational method to approximate the conditional expectation $\mathbb{E}[\text{CPC}_{c,t}|W_{c,t-1}]$ through Monte Carlo integration. The path-dependent structure of

CPC expansion makes this expectation analytically intractable:

$$\mathbb{E}[\mathrm{CPC}_{c,t}|W_{c,t-1}] = \int \mathrm{CPC}_{c,t}(\varepsilon) \cdot f(\varepsilon)\, d\varepsilon \tag{27}$$

where $\mathrm{CPC}_{c,t}(\varepsilon)$ is the CPC count resulting from shock sequence $\varepsilon = \{\varepsilon_{c,\tau}\}_{\tau=1990}^{t}$ and $f(\varepsilon)$ is the joint density of these shocks.

Each simulation draw samples one complete shock sequence $\varepsilon^{(s)} = \{\varepsilon_{c,\tau}^{(s)}\}_{\tau=1990}^{t}$ from $f(\varepsilon)$, forward-simulates the entire expansion process from 1990 through year t, and computes the resulting $\mathrm{CPC}_{c,t}^{(s)}$. By the law of large numbers:

$$\mathrm{IV}_{c,t} = \frac{1}{S}\sum_{s=1}^{S} \mathrm{CPC}_{c,t}^{(s)} \xrightarrow{p} \mathbb{E}[\mathrm{CPC}_{c,t}|W_{c,t-1}] \quad \text{as } S \to \infty \tag{28}$$

This is standard Monte Carlo integration as described by McFadden (1989): when an expectation has no closed form, approximate it by averaging over many random draws from the distribution. The simulation accomplishes nothing more than computing this expectation numerically.

10C..2 Conceptual Purpose: Isolating Quasi-Random Variation

More importantly, the simulation implements the identifying assumption by isolating the systematic component of CPC placement. The decomposition

$$\mathrm{CPC}_{c,t}^{\mathrm{DGP}} = \underbrace{\mathbb{E}[\mathrm{CPC}_{c,t}|W_{c,t-1}]}_{\text{systematic}} + \underbrace{\varepsilon_{c,t}^{\mathrm{DGP}}}_{\text{idiosyncratic}} \tag{29}$$

separates observed CPC presence into two components:

- **Systematic component** $\mathbb{E}[\text{CPC}_{c,t}|W_{c,t-1}]$: Captures predictable organizational responses to observable county characteristics—demographics, religious composition, existing service availability, and lagged abortion rates. This component is potentially endogenous because it may reflect strategic responses to persistent unobservables Z_c.

- **Idiosyncratic component** $\varepsilon_{c,t}^{\text{DGP}}$: Captures unpredictable timing variation—when funding arrives, when real estate becomes available, when volunteers are recruited. These factors determine whether and when a CPC opens conditional on systematic factors.

The instrument $\text{IV}_{c,t} \approx \mathbb{E}[\text{CPC}_{c,t}|W_{c,t-1}]$ contains only the systematic component. Averaging over many simulation draws eliminates idiosyncratic shocks by the law of large numbers. This mechanical purging of timing shocks does not create identification—it merely operationalizes the assumption that actual organizational timing shocks are orthogonal to structural errors.

10D. Two-Stage Least Squares Estimation

The 2SLS estimation proceeds in two stages:

First Stage:

$$\text{CPC}_{c,t} = \pi_0 + \pi_1 \text{IV}_{c,t} + \pi_2' X_{c,t} + \mu_c + \lambda_t + v_{c,t} \tag{30}$$

Second Stage:

$$\text{AbortionRate}_{c,t} = \alpha + \gamma \widehat{\text{CPC}}_{c,t} + \delta' X_{c,t} + \mu_c + \lambda_t + u_{c,t} \tag{31}$$

Standard errors are clustered at the county level to account for arbitrary within-county correlation.

10D..1 What the First Stage Accomplishes

The first-stage regression decomposes observed CPC presence into two orthogonal components:

1. **Predicted values** $\widehat{\text{CPC}}_{c,t}$: Variation in CPC presence explained by the instrument and controls. This captures systematic organizational responses to predetermined observables $W_{c,t-1}$ plus any direct effects of current observables $X_{c,t}$ absorbed by controls.

2. **Residuals** $v_{c,t}$: Variation in CPC presence unexplained by the instrument and controls. Under the identifying assumptions, this residual equals the realized organizational timing shock: $v_{c,t} = \varepsilon_{c,t}^{\text{DGP}}$.

The key insight is that second-stage controls ($X_{c,t}$, μ_c, λ_t) absorb the endogenous component of the instrument that operates through observable channels. What remains—the variation in $\widehat{\text{CPC}}_{c,t}$ orthogonal to controls—reflects only the systematic component of historical observables $\mathbb{E}[\text{CPC}_{c,t}|W_{c,t-1}]$ net of direct effects.

The first-stage residuals $v_{c,t}$ isolate quasi-random organizational timing variation. By construction, $v_{c,t}$ is orthogonal to the instrument and controls. By assumption (conditional orthogonality), $v_{c,t} = \varepsilon_{c,t}^{\text{DGP}}$ is orthogonal to structural errors $u_{c,t}$. Therefore, the fitted values $\widehat{\text{CPC}}_{c,t}$ provide valid identification of the causal effect γ.

10E. Relating Simulated and Actual Shocks

A natural question arises: if identification depends on properties of actual shocks $\varepsilon_{c,t}^{\text{DGP}}$, what role do simulated shocks $\varepsilon_{c,t}^{(s)}$ play?

The simulated shocks are a computational device for approximating the conditional expectation. They are conceptually and mechanically different from actual shocks, but they "get at the same concept" in the following precise sense:

- **Same distribution**: Both $\varepsilon_{c,t}^{\text{DGP}}$ and $\varepsilon_{c,t}^{(s)}$ are independent draws from Gumbel(0,1)
- **Different realizations**: The actual shock is the specific value that occurred in reality (unobserved). Each simulated shock is a different hypothetical value used for integration.
- **Different purpose**: The actual shock determines what we observe in the data. The simulated shocks allow us to compute the expectation that isolates the systematic component.
- **Same identifying variation**: By isolating $\mathbb{E}[\text{CPC}_{c,t}|W_{c,t-1}]$, the instrument isolates the part of actual variation that comes from observables, implicitly using the actual timing shock $\varepsilon_{c,t}^{\text{DGP}} = \text{CPC}_{c,t}^{\text{DGP}} - \mathbb{E}[\text{CPC}_{c,t}|W_{c,t-1}]$ for identification.

The IV strategy succeeds if and only if actual organizational timing shocks are conditionally orthogonal to structural errors. The simulation procedure operationalizes this by computing the systematic component, but the fundamental identifying assumption concerns the actual data generating process, not the simulation algorithm.

10F. Summary: Complete Error Structure

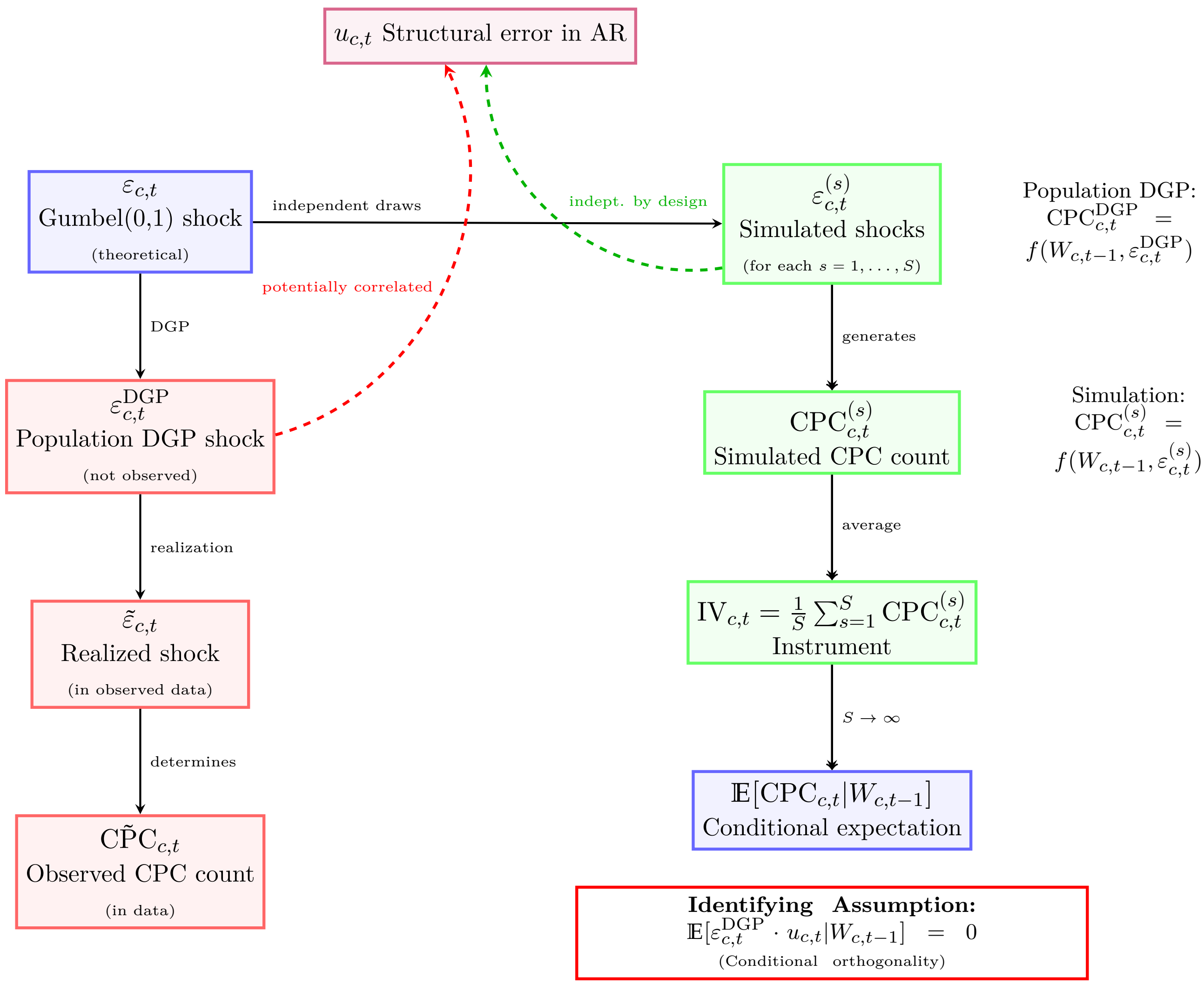
Table IX summarizes all error terms, their roles, and their relationships:

Table IX: Summary of Error Terms and Shocks

Term	Definition	Role
$u_{c,t}$	Structural error in abortion rate equation	Unobserved shocks to abortion demand
$\varepsilon_{c,t}$	Random variable, Gumbel(0,1)	Mechanical tool to construct $\mathbb{E}[\text{CPC}_{c,t}$, represents organizational timing shock
$\varepsilon_{c,t}^{\text{DGP}}$	Population DGP organizational shock	Determines $\text{CPC}_{c,t}^{\text{DGP}}$ in DGP
$\tilde{\varepsilon}_{c,t}$	Realized organizational shock in data	Specific value that occurred (unobserved)
$\varepsilon_{c,t}^{(s)}$	Simulated organizational shock (draw s)	Used to compute $\mathbb{E}[\text{CPC}_{c,t}\|W_{c,t-1}]$
$v_{c,t}$	First-stage residual	Equals $\varepsilon_{c,t}^{\text{DGP}}$ under assumptions
Key Relationships:		
Identifying assumption: $\mathbb{E}[\varepsilon_{c,t}^{\text{DGP}} \cdot u_{c,t}\|W_{c,t-1}] = 0$		
By construction: $\mathbb{E}[\varepsilon_{c,t}^{(s)} \cdot u_{c,t}] = 0$ (but does not create identification)		
Decomposition: $\text{CPC}_{c,t}^{\text{DGP}} = \mathbb{E}[\text{CPC}_{c,t}\|W_{c,t-1}] + \varepsilon_{c,t}^{\text{DGP}}$		

Figure VII provides a visual representation of these relationships, showing how theoretical distributions, population DGP, observed realizations, and simulated quantities relate to each other and to the structural error.

Figure VII: Error Structure and IV Construction



11 Theoretical Foundations of the IV

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

11..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: $IV_{c,t}^* = E[\text{CPC}_{c,t}|\mathcal{F}_{c,t-1}]$.

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 $E[\epsilon_{c,t}|\mathcal{F}_{c,t-1}] = 0$. Chamberlain showed that when instruments equal $E[\text{CPC}_{c,t}|\mathcal{F}_{c,t-1}]$, 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.

11..2 Dynamic Over-Identification

The panel structure provides additional robustness through dynamic over-identification. This appendix provides technical details on how the forward simulation approach implicitly leverages exponentially many moment conditions arising from path-dependent treatment assignment.

Under structural stability of hazard model parameters over 30 years, the predetermination condition $\mathbb{E}[u_{c,t}|\mathcal{F}_{c,t-1}] = 0$ implies that every lag of each predetermined variable satisfies the orthogonality condition:

$$\mathbb{E}[W_{c,t-s} \cdot u_{c,t}] = 0 \quad \text{for all } s \geq 1 \tag{32}$$

This generates a large set of moment conditions. With $K = 10$ time-varying covariates (lagged CPC count, distance to nearest CPC, abortion rate, abortion provider presence, population, age structure, religious composition, unemployment, nonprofit wages, plus interactions) and $T = 30$ years, approximately $K \times (T - 1) \approx 290$ moment conditions hold simultaneously under the maintained assumptions. These provide overidentifying restrictions that can be tested via Hansen J-statistics and offer robustness to first-stage specification errors.

11..3 Path-Dependent Moment Multiplication

The moment conditions multiply further due to path dependence in the hazard model. As demonstrated by Angeles et al. (1998) in the context of strategic family planning program placement, and formalized by Bhargava (1991) and Liu et al. (2010) in dynamic nonlinear models, the effect of any lagged variable $W_{c,t-s}$ on current CPC presence depends on the entire intervening sequence of observables through the recursive structure.

Consider a simple example with two periods and one time-varying covariate X. The effect of X_{t-2} on CPC_t operates through:

$$X_{t-2} \rightarrow \text{CPC}_{t-1} \rightarrow \text{CPC}_t \quad \text{(direct path)} \tag{33}$$

$$X_{t-2} \rightarrow \text{CPC}_{t-1} \rightarrow \text{Dist}_{t-1} \rightarrow \text{CPC}_t \quad \text{(indirect through distance)} \tag{34}$$

$$X_{t-2}, X_{t-1} \rightarrow \text{CPC}_t \quad \text{(joint effect)} \tag{35}$$

Each distinct temporal ordering and combination of observables generates a separate identifying moment condition. With $T = 30$ periods and state-dependent dynamics (where current opening probabilities depend on all past CPC presence and distances), the number of distinct moment conditions grows exponentially in T.

Formally, let $\mathcal{P}_t$ denote the set of all possible paths through the state space from period 1 to period t. Each path $p \in \mathcal{P}_t$ corresponds to a distinct sequence of past CPC openings and associated distance measures. The predetermination condition implies:

$$\mathbb{E}[u_{c,t} \mid \text{path } p] = 0 \quad \text{for all } p \in \mathcal{P}_t \tag{36}$$

With binary opening decisions in each period, $|\mathcal{P}_t| = 2^{t-1}$ grows exponentially. While many paths have zero probability (e.g., more CPCs opening than counties), the practical number of relevant paths with positive probability is still very large.

The forward simulation procedure exploits this high-dimensional over-identification through the estimated hazard model. Rather than constructing $K \times (T-1)$ separate instruments (one for each lagged variable-period pair), the approach generates a single instrument $\text{IV}_{c,t} = \mathbb{E}[\text{CPC}_{c,t}|W_{c,t-1}]$ that implicitly aggregates information from all moment conditions.

The recursive simulation structure:

$$\widehat{\text{CPC}}^{(s)}_{c,\tau} = \widehat{\text{CPC}}^{(s)}_{c,\tau-1} + \mathbb{1}\left[U^{(s)}_{c\tau} \leq \Lambda\left(\hat{\beta}' W^{(s)}_{c,\tau-1}\right)\right] \tag{37}$$

automatically captures how each lagged variable affects current CPC presence through all possible intervening paths. The hazard model coefficients $\hat{\beta}$ encode the systematic relationships between observables and CPC openings; the recursive forward projection traces these relationships through time.

11A. Two Forms of Robustness

This dynamic over-identification provides robustness in two distinct ways:

11A..1 Specification Robustness

The hazard model can be misspecified in its functional form while maintaining consistent IV estimation, provided the predetermination condition holds. Overidentification tests can detect systematic specification failures.

To see this, suppose the true hazard model is:

$$p_{c,t}^{\text{true}} = h(W_{c,t-1}, \beta^{\text{true}}) \tag{38}$$

but we estimate the misspecified model:

$$\hat{p}_{c,t} = \Lambda(\hat{\beta}' W_{c,t-1}) \tag{39}$$

The instrument $\text{IV}_{c,t}$ constructed from $\hat{p}_{c,t}$ will differ from the true optimal instrument. However, as long as:

1. The predetermination condition $\mathbb{E}[u_{c,t}|W_{c,t-1}] = 0$ holds (Assumption 2)
2. The misspecified instrument is still correlated with observed CPC presence (relevance)
3. The misspecified instrument does not introduce spurious correlation with $u_{c,t}$ beyond what is controlled by second-stage covariates

the IV estimator remains consistent, though potentially less efficient than with the correctly specified model.

The Monte Carlo validation in Appendix 12 explicitly tests this by using hazard models that differ from the true DGP. Results show successful parameter recovery despite misspecification.

Moreover, with 290 moment conditions, overidentification tests (Hansen J-statistics) have substantial power to detect specification failures. If the hazard model systematically misses important determinants of CPC placement that correlate with abortion demand, the overidentification restrictions will reject. The empirical finding that overidentification tests do not reject across multiple specifications provides evidence that the hazard model captures the systematic component of placement adequately.

11A..2 Efficiency Gains

The rich dynamic structure allows more precise estimation of the systematic component $\mathbb{E}[\text{CPC}_{c,t}|W_{c,t-1}]$, strengthening the first stage. This differs from simply having multiple separate instruments.

Consider an alternative approach using $K \times (T-1)$ separate instruments—one for each lagged variable-period pair:

$$\text{IV}_{c,t}^{\text{separate}} = \{W_{c,1}, W_{c,2}, \ldots, W_{c,t-1}\} \tag{40}$$

This faces two problems:

1. **Curse of dimensionality**: With 290 instruments, the first stage becomes high-dimensional and potentially weak in finite samples.

2. **Ignores path dependence**: Separate instruments treat each lagged variable independently, missing the nonlinear interactions through recursive dynamics.

The forward simulation approach addresses both issues by:

1. **Dimension reduction**: Aggregating 290 moment conditions into a single strong in-

strument through the structural hazard model.

2. **Capturing dynamics**: The recursive structure naturally incorporates how lagged variables interact through state-dependent paths.

This explains why the first-stage F-statistic is consistently strong (≈ 120) despite the high-dimensional history $W_{c,t-1}$. The hazard model efficiently extracts the predictive content from past observables while avoiding the weak instrument problems that plague high-dimensional IV specifications.

11B. Empirical Evidence

Three empirical patterns support the dynamic over-identification interpretation:

(1) Stable F-statistics across specifications. The first-stage F-statistic remains around 120 across alternative hazard model specifications (logit vs. probit, different functional forms, alternative sets of controls). This stability suggests the instrument captures robust systematic patterns rather than fitting idiosyncratic noise. If the hazard model were severely misspecified, we would expect the F-statistic to vary substantially or weaken with different specifications.

(2) Overidentification tests do not reject. Hansen J-statistics from alternative instrument specifications (e.g., using different subsets of $W_{c,t-1}$ to construct instruments) do not reject the null hypothesis of valid overidentification. This provides evidence that the moment conditions hold in the data.

(3) Coefficient stability. The second-stage coefficient estimate $\hat{\gamma} \approx -0.18$ is stable across different hazard specifications and different instrument constructions. If misspecification were severe, we would expect substantial variation in $\hat{\gamma}$ as the instrument varies. The stability suggests the approach successfully isolates the causal effect despite functional form uncertainty.

11C. Important Caveats

Dynamic over-identification provides additional robustness but does not weaken the core identifying assumptions:

(1) Still requires Assumptions 1-2. The predetermination condition $\mathbb{E}[u_{c,t}|\mathcal{F}_{c,t-1}] = 0$ (Assumption 2) must hold for all 290 moment conditions. Over-identification provides *testable implications* of this assumption (via J-tests) but cannot validate it directly. If predetermination fails for some subset of observables, overidentification tests will reject—which is useful—but passing J-tests does not prove predetermination holds.

(2) Assumes structural stability. The dynamic over-identification argument assumes hazard model parameters are stable over 30 years. If the determinants of CPC placement changed fundamentally (e.g., different organizational strategies in 1990s vs. 2010s), the moment conditions from early periods may not apply to later periods. The empirical finding that results are stable across different sub-periods provides some evidence for structural stability.

(3) Not the primary source of identification. The core identification comes from Assumptions 1 and 2—conditional orthogonality and predetermination. Dynamic over-identification provides additional robustness and efficiency but does not fundamentally change what the approach identifies. The LATE interpretation and complier population depend on the structural assumptions, not on the number of moment conditions.

11D. Summary

The forward simulation approach implicitly exploits approximately 290 moment conditions arising from the predetermination condition applied to 10 time-varying covariates over 30 years. Path dependence in the hazard model multiplies these further, as different temporal orderings generate distinct restrictions. The single instrument $\mathrm{IV}_{c,t} = \mathbb{E}[\mathrm{CPC}_{c,t}|W_{c,t-1}]$ aggregates information from all moment conditions through the structural hazard model, providing both specification robustness and efficiency gains. Empirical patterns—stable F-statistics, passing overidentification tests, coefficient stability—support the interpretation that the approach successfully leverages the dynamic panel structure to strengthen identification.

12 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.

12..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:

$$CPC_{c,t}^{observed} = CPC_{c,t}^{true} + 0.2 \cdot Z_c \tag{41}$$

   where $CPC_{c,t}^{true}$ 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 ($\beta_{CPC} = -0.30$):

$$AbortionRate_{c,t}^{synthetic} = -0.30 \cdot CPC_{c,t}^{true} + 2.0 \cdot Z_c + 0.5 \cdot Unemp_{c,t} + \tilde{\epsilon}_{c,t} \tag{42}$$

   where $\tilde{\epsilon}_{c,t}$ 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 $CPC_{c,t}^{observed}$ 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 4D..2.

Step 4: Compare Estimators

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

1. **Naive OLS**: Regress $AbortionRate_{c,t}^{synthetic}$ on $CPC_{c,t}^{observed}$, 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 $CPC_{c,t}^{observed}$ using $IV_{c,t}$, the simulated expected CPC count. First stage:

$$CPC_{c,t}^{observed} = \pi_0 + \pi_1 IV_{c,t} + \pi_2' X_{c,t} + \mu_c + \lambda_t + v_{c,t} \tag{43}$$

Second stage:

$$AbortionRate_{c,t}^{synthetic} = \alpha + \gamma \widehat{CPC}_{c,t} + \delta' X_{c,t} + \mu_c + \lambda_t + u_{c,t} \tag{44}$$

12..2 Results

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

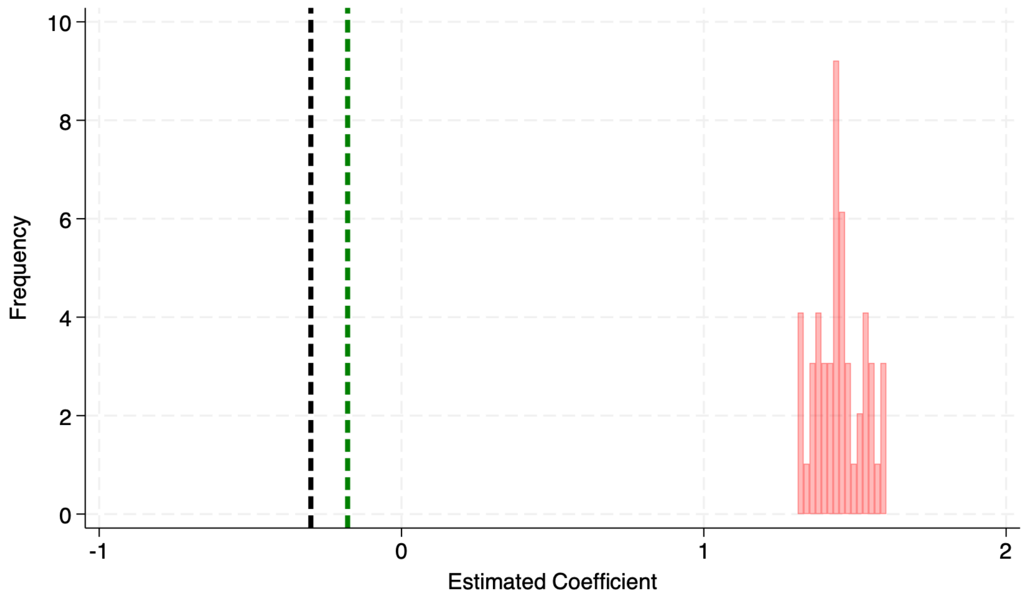
First, naive OLS fails catastrophically under endogenous selection. The mean estimate is approximately +1.5—the wrong sign and five times the true magnitude. The positive selection bias from Z_c completely dominates the negative causal effect. Across replications, not a single naive OLS estimate is negative, 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 VIII: 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.