Downstream Effects of Restricted Access to Contraception: Evidence from Hospitals with Religious Directives

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Abstract

A large literature shows that religion shapes economic and demographic outcomes, mostly by studying how changes in personal religious beliefs alter individual behavior. Less is known about organizational religious doctrine: what happens when a religious institution restricts service provision, often without the knowledge or consent of consumers? I address this question in the setting of U.S. Catholic hospitals, which comprise about 15% of the national market share and adhere to Ethical and Religious Directives (ERDs). The ERDs prohibit all medical care against religious doctrine, including contraception. Using quasi-random variation in geographic proximity to hospitals with and without ERDs, I evaluate the effect of giving birth at an ERD-adherent hospital on postpartum contraception and downstream reproductive health outcomes. This instrumental variable approach uses data on 11 million birth hospitalizations from 2010–2023 across ten states. I find giving birth at an ERD-adherent hospital reduces permanent contraception by 3.72 percentage points (pp), a 60% decrease relative to the mean, with insignificant effects on non-permanent contraception in the full sample. However, this pooled estimate masks important differences by patient location: rural patients who delivered at an ERD-adherent hospital experienced 64% and 63% relative decreases in permanent and non-permanent contraception, respectively. Rural patients were then 4.18 pp more likely to be pregnant within 18 months (30% relative increase), faced elevated risk of short-interval pregnancies complicated by severe maternal morbidity or preterm birth, and had 0.07 additional births over three years. I find no fertility effects for urban patients, who have greater access to outpatient contraception. These findings demonstrate that organizational religious beliefs can have significant impacts on patient health and fertility, even absent changes in personal belief.

Keywords: Economics of religion; Catholic hospitals; Postpartum contraception; Fertility; Rural healthcare

JEL classification: Z12, I11, I18, J13

1 Introduction

Religion remains one of the most powerful institutional forces shaping economic and demographic behavior across societies (Rubin, 2017; Becker et al., 2024, 2021). A foundational literature in the economics of religion has documented how religious doctrine shapes human capital accumulation, labor market outcomes, and demographic behavior, typically through individual behavior change (Bazzi et al., 2020; Nunn, 2010, 2014; Lehrer, 2004). While historical studies have extensively examined how religious authorities influence long-run development, less is known about how religious institutions act as binding supply-side constraints within modern economic markets.

U.S. Catholic hospitals offer a natural setting for studying such constraints. One in seven U.S. hospitals operates under the Ethical and Religious Directives for Catholic Health Care Services (ERDs), a set of 77 specific directives written and enforced by the U.S. Conference of Catholic Bishops. The ERDs prohibit the provision of any health care contrary to Catholic doctrine, including every form of contraception (U.S. Conference of Catholic Bishops, 2025; Freedman, 2023). Two key features distinguish this empirical setting: (i) ERD-adherent hospitals are officially recorded and tracked over time, and (ii) these religious restrictions apply to every patient admitted to a Catholic hospital, even though patients may not share the same Catholic beliefs and most patients are not aware that the ERDs restrict their healthcare options (Guiahi et al., 2014; Wascher et al., 2018; Boulware et al., 2024; Freedman, 2023). Religious doctrine here is an observable, enforceable constraint on a firm's production set, imposed on a largely unaware population.

The constraint on contraception is particularly consequential during birth hospitalizations. Immediate postpartum contraception, defined as contraception provided during a hospital stay for childbirth, is medically efficient, cost-effective, and eliminates the need for additional procedures or appointments. Demand for immediate postpartum contraception is substantial and rising, particularly post-*Dobbs*.[1] Estimates suggest about half of postpartum contraception requests are unfulfilled. Permanent contraception (i.e., sterilization), for example, is provided after 7–10% of deliveries but requested during approximately 20% of birth hospitalizations (Potter et al., 2013, 2014, 2021; Zite et al., 2005; Seibel-Seamon et al., 2009; Wolfe et al., 2017; ACOG, 2021). For rural patients facing mounting barriers to reproductive healthcare, the birth hospitalization often represents the only feasible opportunity to

[1] *Dobbs* refers to the 2022 U.S. Supreme Court decision in the case of *Dobbs v. Jackson Women's Health Organization*, which overturned the federal constitutional right to abortion.

receive contraception (Cooper et al., 2024; Clark and Levy, 2025; ACOG, 2021).

The number of hospitals operating under the ERDs has grown by over 20% in the last two decades (Schulte et al., 2025; Solomon et al., 2020), and their expansion has spurred state and federal policy debates. During the 2024 U.S. Vice Presidential debate for example, Senator J.D. Vance questioned Governor Tim Walz on whether he would compel Catholic hospitals to perform services "against their will," and President Trump subsequently signed an executive order strengthening institutional protections for religiously affiliated hospitals (DHHS, 2024). At the state level, legislatures have introduced bills to increase oversight of healthcare mergers and acquisitions, or to mandate disclosure of services a hospital does not provide (Appendix 1 Table A1 describes ten such state-level policies). However, causal evidence is largely absent from these debates. Existing research documents associations between proximity to a Catholic-affiliated hospital and reduced contraception provision (Meille and Monnet, 2024; Rodriguez et al., 2023), but these approaches face identification challenges related to patient selection and secular trends. Moreover, no prior work traces the effect of institutional religious restrictions through to downstream reproductive health outcomes.

In this paper, I seek to fill this gap by analyzing the impact of hospital ERD adherence on: (i) contraception provided during the index birth hospitalization, and (ii) downstream reproductive health outcomes, including short-interval pregnancy, severe maternal morbidity (SMM), preterm birth, and fertility. The primary empirical challenge is patient selection: individuals who prefer not to use contraception may also be more likely to seek care at ERD-adherent hospitals,[2] which would bias naive estimates. To circumvent this issue, I instrument for a patient's admission to an ERD-adherent hospital using the differential distance from a patient's residence to the nearest ERD-adherent vs. non-ERD-adherent hospital. This approach leverages quasi-random variation in geographic proximity to isolate the causal effect of being treated at a facility with religious service restrictions. I estimate the relationship between instrumented hospital admission and the outcomes of interest using two-stage least squares (2SLS) with a dataset of 11 million birth hospitalizations from 2010-2023, constructed using ten state inpatient databases from the Healthcare Cost and Utilization Project. This instrumental variable approach identifies effects for the "complier" population, patients whose hospital choice responds to geographic proximity.

I first find that giving birth at an ERD-adherent hospital decreases the likelihood of receiving permanent contraception (e.g., sterilization) by 3.72 pp, or 60% relative to the mean ($p < 0.01$). In the full sample,

[2]I use "ERD-adherent" rather than "Catholic" hospital for specificity, as not all hospitals owned by a Catholic health system are required to adhere to the ERDs. See Section 3.2 for details.

I see no significant effect of ERD-adherent hospitalization on provision of non-permanent contraception (e.g., intrauterine devices, IUDs, or the birth control pill). When analyzing rural-urban heterogeneity, however, I find large significant effects for rural patients: ERD-adherent hospitalization reduces provision of permanent contraception by 4.75 pp (64% relative decrease, $p < 0.01$) and non-permanent contraception by 2.20 pp (63% relative decrease, $p < 0.01$).

I then follow patients over time and document downstream reproductive health consequences, particularly for rural patients who often lack outpatient contraceptive options after leaving the birth hospital. Rural patients who delivered at an ERD-adherent hospital were 4.18 pp more likely to conceive within 18 months (30% relative, $p < 0.05$), 0.31 pp more likely to experience a short-interval pregnancy ending in an SMM-complicated birth ($p < 0.01$), and 0.33 pp more likely to experience one ending in preterm birth ($p < 0.10$). Over three years, they were 6.0 pp more likely to have any subsequent birth (31% relative) and have 0.07 additional births on average (both $p < 0.05$). I find no corresponding effects for urban patients.

Back-of-the-envelope estimates suggest that these coefficients translate into substantial impact. With about 15% of the roughly 3.6 million annual U.S. births occurring at ERD-adherent hospitals, the findings imply that approximately 20,000 permanent sterilizations are foregone across the country each year. This burden falls most heavily on rural communities (representing about 151,000 births annually), for whom estimates suggest roughly 10,500 fewer contraceptive procedures, 6,300 additional short-interval pregnancies, 470 short-interval births complicated by SMM, and about 10,000 additional births over a three-year window for each cohort of new mothers.[3]

This study makes four contributions. First, I provide causal evidence on how organizational religious doctrine impacts health and demographic outcomes in a modern setting. Most economic research on religious rules, such as historical interest prohibitions, studies long-run historical differences across regions (Rubin, 2017; Becker et al., 2024, 2021). Here, I study a contemporary setting where the rule is written, implementation is observable, and restricted services are recorded across eleven million hospitalizations. Therefore, I can separate hospitals that are Catholic "in name" from those actually required to follow the ERDs. About 11% of hospitals owned by Catholic systems are **not** required to follow the ERDs, and I build a novel dataset to distinguish them (see Appendix 2).

[3]These illustrative calculations scale the findings to 3.6 million annual U.S. births (Centers for Disease Control and Prevention, 2026) and represent upper bounds rather than exact national tallies, assuming that the shares of births in Catholic (15%) and rural (28%) in the sample approximate broader national values.

Second, I identify a supply-side institutional mechanism through which religion shapes fertility. Work on religion and demographic behavior has largely operated through belief and persuasion (Bassi and Rasul, 2017; McQuillan, 2004), while historical work on Christian missions traces long-run and sometimes opposing effects of religious organizations on health and demography (Nunn, 2010; Cagé and Rueda, 2020). In contrast, I document a channel requiring no change in personal belief: a religious facility withholds contraceptive care at the bedside, and I can observe changing fertility rates within years rather than generations.

Third, I provide the first causal evidence linking hospital religious restrictions to downstream health outcomes. Most prior research on this topic has been non-causal or has used qualitative interview designs to generate insights for a relatively small sample (Thorne et al., 2019; Freedman, 2023). Hill et al. (2019) exploited hospital-level conversions in Catholic-affiliation and found a 31% reduction in the per-bed rate of tubal ligation, suggesting that more "women [may] face the risks of unintended pregnancies," but the authors "do not find substantial changes in welfare." I build upon Hill et al. (2019) in three key areas: i) I examine rural-urban heterogeneity and trace effects through to downstream reproductive health outcomes, demonstrating that hospital service restrictions increase the risk of short-interval pregnancy and adverse birth outcomes, particularly for rural patients; ii) I use an instrumental variable design to estimate effects for patients who choose hospitals based on geographic convenience ("compliers"), a particularly policy-relevant population given they would be most affected if consolidation eliminated non-religious alternatives; and iii) I introduce a precise, time-varying measurement of ERD adherence (see Appendix 2), distinguishing hospitals officially required to follow religious directives from those merely affiliated with Catholic systems.

Fourth, I highlight institutional religious restrictions as an overlooked mechanism—distinct from policy, legal, or financial barriers—that impacts reproductive health access and outcomes. Prior research has established causal links between legal and financial barriers and reproductive healthcare, for example quasi-experimental evaluations of state initiatives that subsidized contraception (Bailey, 2013; Lindo and Packham, 2017; Kelly et al., 2020; Bailey et al., 2025; Kiser et al., 2026; Laszlo et al., 2023). That literature, however, evaluates policies that *expand* access through the outpatient sector; the role of facility-level restrictions as an independent, binding constraint on inpatient care has remained largely unexplored. This study provides causal evidence of this mechanism in the context of reproductive healthcare specifically, but the ERDs also restrict providers' ability to offer evidence-based care in other domains, such as end-of-life care. Understanding this hospital-level mechanism is especially urgent

amid broader shifts in the U.S. healthcare landscape: Catholic health systems are expanding; obstetric services are contracting, particularly in rural regions; options for abortion and contraception care have narrowed post-*Dobbs*; and impending Medicaid funding cuts threaten rural hospitals' financial stability. Together, these trends make the role of institutional restrictions increasingly consequential for patient care.

The paper proceeds as follows. Section 2 provides a brief background on hospital religious restrictions and immediate postpartum contraception. In Section 3, I introduce the data, study sample, and variables, including the approach to precisely identifying which hospitals were ERD-adherent. I also present descriptive statistics in Section 3. In Section 4, I describe the empirical approach and identification strategy. Main results are presented in Section 5. Last, I discuss the implications of the findings and conclude in Section 6.

2 Background

2.1 The Ethical and Religious Directives

In the U.S., the vast majority of all religiously-affiliated hospitals are Catholic.[4] Most, but not all, Catholic-affiliated hospitals are also ERD-adherent, meaning they are required to follow the Ethical and Religious Directives for Catholic Health Care Services (ERDs).[5] The ERDs are a set of 77 specific religious directives that govern hospital operations, provider decision-making, and clinical care. The directives are written and enforced by the U.S. Conference of Catholic Bishops. To be considered officially Catholic by the diocese, hospital leadership states their intention to fully abide by all aspects of the ERDs and the local bishop is typically then invited to bless the hospital infrastructure (e.g., building, land, etc. (Freedman, 2023)). The ERDs emphasize serving vulnerable populations and providing pronatalist obstetric care to support "the sanctity of human life from its very beginning," but also explicitly forbid hospitals from providing services that "render procreation impossible," such as contraception. In some cases, with approval of the local bishop and hospital ethics committee, contraception is permitted as an *indirect* effect of another procedure meant to cure or alleviate a "serious pathological condition" (U.S.

[4]Catholic-affiliated hospitals represent over 70% of all religious hospitals in the United States. The remaining proportion primarily have Jewish or Protestant affiliations, with less consistent and strictly enforced religious directives for medical care (Genazzani et al., 2025; Guiahi et al., 2019; Freedman et al., 2018; Freedman, 2023).

[5]Based on the primary author's analysis, 89% of hospitals affiliated with a Catholic health system were ERD-adherent in 2023.

Conference of Catholic Bishops, 2025).

The number of hospitals with religious service restrictions has grown substantially in recent decades as Catholic systems have expanded. Between 2000 and 2020, the number of Catholic-affiliated hospitals increased by 28%. Today, five of the ten largest U.S. health systems (by net patient revenue) are Catholic, and approximately one in seven U.S. hospitals is Catholic (Solomon et al., 2020; Schulte et al., 2025; Popowitz, 2025). Notably, this growth has occurred through acquisition rather than religious conversion: when a Catholic system acquires a hospital, the directives extend to that facility's patients regardless of their religious affiliation. Therefore, in a local market, the share of hospitals abiding by the ERDs is determined by mergers and acquisitions rather than by the religious composition of the local population. Catholic hospitals are about equally prevalent in rural and urban markets (14% of rural hospitals were ERD-adherent in 2023 vs. 15% of urban hospitals), but the marginal significance of a single facility differs sharply—in rural markets, one acquisition can eliminate the only local source of an entire category of care, such as inpatient contraception.

Despite the expansion of Catholic health systems, patient awareness of hospital religious restrictions remains low. While some hospitals have names that indicate their religious affiliation (e.g., St. Joseph's, St. Mary's), many facilities do not prominently advertise their religious affiliation or clearly communicate implications for patient care (Takahashi et al., 2019; Guiahi, 2020; Wascher et al., 2020). Multiple studies have found that patients are generally unaware of hospitals' Catholic affiliation and the associated restrictions on medical care, nor would they prioritize a hospital's religious affiliation when choosing where to receive care (Guiahi et al., 2014; Wascher et al., 2018; Stulberg et al., 2019; Hebert et al., 2020; Boulware et al., 2024). Only 3% of women report selecting a hospital for obstetric care because it was religiously affiliated (Hebert et al., 2020), and national surveys indicate that Catholic women in the United States use contraception at rates similar to the population overall (Jones and Dreweke, 2011; Jones, 2020). The population subject to ERDs is therefore composed largely of patients who neither share the underlying doctrine nor chose their hospital because of it, an information asymmetry that raises questions about whether patients are able to make fully informed choices about where to receive care.

2.2 Immediate postpartum contraception

The hospitalization where a birth occurs (i.e., the "birth hospitalization") is a critical and unique point of access for postpartum contraception. However, contraception provision, as well as contraceptive counseling (during the prenatal and postpartum periods), is prohibited at ERD-adherent hospitals (U.S. Conference of Catholic Bishops, 2025; Boulware et al., 2024). For many patients, the birth hospitalization provides the most feasible opportunity to receive desired contraception, as it doesn't require additional medical appointments, procedures, or recovery periods. Immediate postpartum contraception, defined as contraception provided during the birth hospitalization, is also medically optimal due to technical ease and efficiency (ACOG, 2021).[6] While advance planning for postpartum contraception provision is recommended during prenatal counseling visits, the only strict requirement is for Medicaid-covered sterilization, where federal regulations mandate a 30-day waiting period between signing a federally-required consent form and the procedure (Zapata et al., 2015; Borrero et al., 2014; ACOG, 2019a).[7]

Access to immediate postpartum contraception is especially important for rural populations, who face declining access to both inpatient obstetric care and outpatient reproductive health providers. With ongoing closures of rural hospital obstetric units, the birth hospitalization often represents the only feasible opportunity for many rural patients to receive desired contraception (Lee et al., 2020; Janis et al., 2021; Clark and Levy, 2025). When contraception is not provided during the birth hospitalization, postpartum visits often serve as a critical alternative access point. However, rural populations are less likely than urban populations to attend postpartum visits and receive contraceptive counseling—during either the prenatal and postpartum periods (Bozkurt et al., 2024; Thayagabalu et al., 2025).

Permanent contraception, also known as sterilization, is most commonly performed in the immediate postpartum period, with approximately half of all tubal ligations occurring during the birth hospitalization (ACOG, 2021). Overall, female sterilization procedures follow 7-10% of all hospital deliveries (Potter et al., 2013, 2014; ACOG, 2021). However, this rate represents only half of patients who request it, revealing substantial unmet demand (Zite et al., 2005; Seibel-Seamon et al., 2009; Wolfe et al., 2017; Potter et al., 2021). Non-permanent contraception includes both long-acting reversible contraception

[6] Tubal ligation, for example, is substantially easier to perform during the birth hospitalization—particularly following cesarean delivery, when the abdominal cavity is already open—than as a separate procedure requiring additional surgery and anesthesia. Similarly, intrauterine device (IUD) insertion is more straightforward immediately postpartum while the cervix remains dilated and can be integrated into delivery care without extending hospitalization length (ACOG, 2021).

[7] For private insurance patients seeking immediate postpartum sterilization, there are no federal requirements but individual hospitals may have their own consent policies. For non-permanent contraception, advance planning is recommended, but not required to receive the service during the birth hospitalization.

(LARC), such as intrauterine devices (IUDs) and hormonal implants, and short-acting reversible contraception (SARC), such as the birth control pill and shot. Historically, non-permanent contraception has been less common to receive during the birth hospitalization, but rates have more than doubled in the last decade, likely driven by expanded insurance coverage and rising provider awareness. The most recent data available suggests non-permanent contraception is provided in 1-3% of birth hospitalizations, a rate that's expected to increase with growing patient demand (Sharma et al., 2024; Sheyn and Arora, 2021; Bullard et al., 2024).[8] Economic evaluations consistently find that immediate postpartum LARC is a highly cost-effective intervention, largely due to its role in preventing the adverse outcomes associated with short-interval pregnancies (Bullard et al., 2024; Gariepy et al., 2015; Marcelino et al., 2025).

The consequences of unfulfilled contraceptive demand can be substantial, affecting both reproductive autonomy and health outcomes. When patients cannot access immediate postpartum contraception when desired, they face threats to their individual agency—unable to fully determine whether, when, and under what circumstances to have children. This denial imposes both immediate practical burdens (requiring patients to seek additional procedures with various logistical and clinical hurdles) and longer-term constraints on their ability to determine the spacing and timing of future pregnancies. While conventional wisdom suggests that restricting access to contraception may negatively impact health, the causal evidence supporting this relationship is surprisingly limited. Quasi-experimental studies exploiting changes in family planning programs have established that expanded contraception access reduces birth rates, especially among teens and low-income women (Packham, 2017; Bailey, 2013; Bailey et al., 2025).

However, while these studies rigorously evaluate how financial barriers impact fertility outcomes, they do not follow women over time to study subsequent reproductive health consequences, such as pregnancy spacing. One observational study documented associations between unfulfilled postpartum sterilization requests and shorter pregnancy intervals, but is limited by the potential for patient selection and unobserved confounding (Thurman and Janecek, 2010). This gap is critical as short-interval pregnancies—those occurring within 18 months of a previous birth—carry well-established health risks, including preterm birth, low birth weight, and maternal morbidities, with risks highest for pregnancies within 6

[8]Historically, rates of non-permanent postpartum contraception vary widely across states due to both provider practices and insurance coverage. For example, Sharma et al. (2024) found that immediate postpartum LARC rates ranged from 2.55 per 10,000 deliveries in Kentucky to 637.25 per 10,000 deliveries in Delaware.

months (Conde-Agudelo et al., 2006, 2007; Ali et al., 2023; Wang et al., 2022; ACOG, 2019b). These health consequences have made reducing the occurrence of short-interval pregnancies a priority target of the Healthy People 2030 objectives (DHHS, 2025). Understanding and addressing unfulfilled contraceptive demand is therefore critical for assessing both reproductive autonomy and downstream health outcomes.

3 Data and variables

The primary data sources are 2010-2023 Healthcare Cost and Utilization Project state inpatient databases (HCUP-SIDs), all-payer datasets derived from hospital discharge records (AHRQ, 2025). Inpatient settings are particularly relevant to the study given that ERD adherence is most consistently enforced within the hospital facility (Guiahi, 2018; Freedman, 2023). HCUP-SIDs contain hundreds of clinical and non-clinical variables for each hospital discharge, including patient demographics; admission type; patient, provider, and hospital identifiers; International Classification of Diseases (ICD) 9/10 diagnosis and procedure codes; Current Procedural Terminology (CPT) codes; and Diagnosis Related Groups (DRG) codes.

I merge HCUP-SID hospital discharge records with hospital characteristics from the American Hospital Association (AHA) Annual Survey, linked using unique hospital identifiers that are consistent across both databases (AHA, 2025a). The AHA dataset provides comprehensive information on hospital location, system affiliation, organizational structure, ownership status, operational characteristics, and available service lines. The survey maintains rigorous data collection standards to ensure temporal consistency across years. Response rates average approximately 75% annually, with AHA employing systematic imputation procedures for non-respondents using historical data and peer hospital benchmarking methods (AHA, 2025b; CDC, 2024).

3.1 Study sample

The main analytic sample consists of all birth hospitalizations for females aged 12–49 at general medical and surgical hospitals across ten geographically diverse states: Arkansas, Arizona, Colorado, Florida, Kentucky, Maryland, New Jersey, New York, Oregon, and Washington (n=10,994,881, Table 1). The sample includes years 2010-2023, but not all years were available for each state. State-years in the sample were selected based on data availability and quality criteria, including presence of unique hospital

identifiers, availability of encrypted AHA linkage files, completeness of patient demographic variables, and sufficient variation in state-level ERD-adherent hospital market share. The unit of analysis is the hospitalization, and the sample may contain multiple hospitalizations for the same patient.

I arrived at this sample by making several exclusions. First, from the sample of all births (n=13,281,015), I exclude 1.6% of observations where the patient's state of residence differs from the hospital state, assuming these cases represent atypical care-seeking patterns (e.g., tourism or travel).[9] Second, I exclude records missing patient county or zip code (13.8%), then records missing hospital location (1.1%), as these variables are required for the empirical strategy. Third, I exclude 1.4% of observations where the differential distance exceeds 100 miles (2.5-3 hours of drive time), as patients facing extreme trade-offs may not represent a population with meaningful choice, potentially violating the common support assumption. I further discuss this analytical decision in Section 4.2.

For the secondary analyses of short-interval pregnancy, I create a sub-sample of five states (Arkansas, Florida, Maryland, New York, and Washington) with unique patient identifiers that enables linkage of hospitalizations by the same patient over time (n=6,842,695, Appendix 1 Table A2). When examining short-interval pregnancies and other downstream outcomes, I drop index birth hospitalizations in the last two years for each state to allow a sufficient follow up window.

3.2 Treatment: Identifying ERD-adherent hospitals

My empirical strategy requires accurately characterizing ERD-adherent hospitals, meaning those that are officially recognized by the local bishop as Catholic, and are therefore expected to fully comply with the ERDs. My approach focusing on hospital-level ERD-adherence extends prior research (Hill et al., 2019; Meille and Monnet, 2024; Rodriguez et al., 2023) that has relied on measures of Catholic affiliation that may not capture true adherence to the ERDs. For instance, a hospital may be acquired and affiliated with a Catholic system but have a specific agreement that exempts it from the ERDs, or there may be a delay between an acquisition and the implementation of the directives. Dignity Health, for example, is one of the largest U.S. Catholic health systems and owns several hospitals that are not expected to adhere to the ERDs.[10]

[9] I do not exclude observations where patient and hospital states share a border (and both states are in the sample). For example, I retain patients who live in Oregon and go to a Washington hospital, or live in New Jersey and go to a New York hospital (and vice versa).

[10] In 2012, Dignity Health restructured to "satisfy the requirements of the U.S. Conference of Catholic Bishops" regarding partnerships with non-Catholic hospitals while still allowing Dignity Health to maintain its ERD-adherent and non-ERD-

In the hospital-year panel sample, 89% of Catholic-affiliated hospitals were required to adhere to the ERDs, and 11% were not. Appendix 1 Table A3 reports characteristics of Catholic-affiliated hospitals by ERD-adherence status. Hospitals were generally similar across types, although Catholic-affiliated ERD-adherent hospitals were more likely to be for-profit (2.7% vs. 0.7%) and had slightly higher Medicaid payer mix (0.23 vs. 0.21) compared to Catholic-affiliated non-ERD-adherent hospitals. Catholic-affiliated ERD-adherent hospitals also were slightly more likely to have an obstetrics unit (80.2% vs. 79.3%) and had a higher obstetrics caseload (0.33 vs. 0.28).

To construct precise, time-varying indicators of hospital ERD adherence, I implemented a rigorous multi-step validation process (detailed in Appendix 2). To briefly summarize: I first cross-referenced AHA Catholic-affiliation variables against six secondary data sources.[11] In cases of conflicting information, I then conducted a targeted search of hospital websites and historical news reports. In 44 cases where adherence to the ERDs remained inconclusive, I conducted "secret shopper" calls to the hospitals to clarify the implementation of religious directives. During these secret shopper calls, research assistants identified themselves as prospective patients new to the area seeking information about perinatal hospital services. Callers then asked whether the hospital followed Catholic religious directives and, specifically, whether postpartum tubal ligation was available at the facility. For hospitals that had undergone ownership changes (e.g., acquisition by a Catholic system), I verified the timing of any changes and impact on service restrictions.

3.3 Outcomes

3.3.1 Index birth hospitalization

The initial set of outcomes is measured during the index delivery. The primary outcome is provision of immediate postpartum contraception (permanent and non-permanent), defined as contraception provided during the birth hospitalization. Permanent contraception (i.e., sterilization) is defined as fallopian tube ligation, occlusion, destruction, or excision; hysterectomy; and bilateral oophorectomy. Non-permanent contraception includes LARC (such as IUDs and hormonal implants), SARC (such as

adherent hospitals under a single organization (Vizient, Inc. and JD Healthcare, Inc., 2018). The hospitals that are not expected to fully comply with ERDs instead operate under a "Statement of Common Values."

[11] These six secondary data sources are: the Official Catholic Directory (proprietary, all study years), Catholic Health Care Directory (publicly available for the current year), CMS Hospital Change of Ownership dataset (public, data starts 2016), AHRQ Compendium of U.S. Health Systems (public, data starts 2016), LevinPro HC database (proprietary, all study years), and Cooper et al. Health Care Pricing Project (public, 2001-2014 data).

the hormonal birth control shot or pill), and other unspecified contraceptive care/management that is delivered during the birth hospitalization.

I also measure several adverse birth outcomes that may occur at the index delivery. First, I measure severe maternal morbidity (SMM), including and excluding blood transfusions, as defined by the CDC (Centers for Disease Control and Prevention, 2024). I also measure preterm birth (gestation <37 weeks) and complications that are likely to be impacted by the presence of ERDs (pelvic infection, obstetric sepsis, hemorrhage, embolism, and transfusion), following related research (Freedman et al., 2008; Freedman, 2023; Wall and Yemane, 2022).

3.3.2 Downstream reproductive health outcomes

I next measure reproductive health outcomes downstream of the index delivery. The primary downstream outcome is short-interval pregnancy (SIP), defined as a subsequent pregnancy within 18 months of the index birth.[12] Short interpregnancy intervals are associated with elevated risks of adverse maternal and infant outcomes, and reducing them is a priority of the Healthy People 2030 objectives (DHHS, 2025; ACOG, 2019b). Because these risks rise as the interval shortens, I report SIP occurring within 6, 12, and 18 months of the index birth (Ali et al., 2023; ACOG, 2019b).

I then examine adverse outcomes (SMM and preterm birth) occurring at a subsequent birth after a short-interval pregnancy. As an exploratory analysis, I also estimate SMM and preterm birth *conditional* on a short-interval pregnancy. (Because the conditioning event is itself affected by treatment, these estimates are susceptible to post-treatment selection bias and I interpret them cautiously.) I analyze two fertility measures: (i) any subsequent birth within three years of the index delivery (binary), and (ii) the number of subsequent births within three years (count). Finally, I estimate the probability of switching hospital type at the next delivery (ERD-adherent to non-ERD-adherent, or vice versa).

All outcomes were identified using ICD-9/10 diagnosis and procedure codes, CPT codes, and DRG codes based on established definitions from Centers for Disease Control and Prevention (2024), Centers for Medicare & Medicaid Services (2024), and prior literature (Stulberg et al., 2017; Hill et al., 2019; Caldwell et al., 2022; Dude et al., 2022; Fang and Westhoff, 2022; Meille and Monnet, 2024; Pollack et al., 2022; Steenland et al., 2021, 2022; Zhu et al., 2023). Appendix 1 Tables A4 and A5 provide the complete list of codes used to define the study sample and outcomes.

[12]Date of conception was calculated as 9 months before birth hospitalization, accounting for preterm birth. I excluded births within 6 months of a previous birth, under the presumption that these were likely to be erroneous.

3.4 Covariates

I include a rich set of patient, hospital, and market-level covariates. Patient-level covariates include both sociodemographic and clinical traits. For sociodemographic characteristics, I consider age (in bins that capture ages ranging from 12-17, 18-25, 26-30, 31-36, and 37-49), race/ethnicity (White, Black, Asian, Hispanic, other/missing), primary payer (private insurance, Medicaid, other/missing), marital status (binary), and patient residence (urban vs. rural). In the main analyses, urban is defined as living in a large metropolitan county using USDA Urban Influence Codes, UIC (and rural otherwise). I assess robustness using two alternative definitions of urban: (i) metropolitan county according to the Office of Management and Budget (OMB) Core Based Statistical Areas, and (ii) counties with below average provider concentration (as a proxy for urban, as urban areas typically have lower provider concentration and more competition).[13]

Patient clinical characteristics include type of admission (emergency/urgent vs. elective) and dichotomous indicators for several comorbidities known to be associated with reproductive health outcomes: obesity, diabetes, hypertension, hypothyroidism, chronic pulmonary disease, depression, and substance use (Aubry et al., 2019; Chuang et al., 2005; Hayes et al., 2020; Phillips-Bell et al., 2016; Hall et al., 2013). I also construct a summary measure of patient comorbidity burden using the van Walraven Elixhauser comorbidity index (van Walraven et al., 2009; Li et al., 2008; Quan et al., 2005).[14]

I control for several relevant characteristics of the birth hospital including: ownership structure (non-profit, for-profit, or government), health system membership, total annual admissions, Medicaid patient share (proportion of total annual inpatient days covered by Medicaid), presence of an obstetric unit, and obstetric caseload (proportion of total annual admissions for obstetric care). I also control for level of competition in the hospital service area, given suggestive evidence that hospitals in more concentrated (i.e., rural) markets may more strictly adhere to the ERDs (Freedman, 2023; Kramer et al., 2021).[15] It is important to control for these characteristics as ERD-adherent and non-ERD-adherent hospitals

[13]Provider concentration was defined using the Herfindahl-Hirschman Index with annual birth volumes at hospitals located in patient county of residence.

[14]The Elixhauser Comorbidity Index is a validated summary measure of overall patient health status and comorbidity burden that is meant to complement, but not replace, individual comorbidity indicators (Li et al., 2008). The index creates a single numeric score that captures the cumulative effect of multiple comorbidities on patient outcomes. A positive score indicates increased risk of mortality, whereas a negative score indicates healthier patients.

[15]Evidence suggests that Catholic hospitals in more concentrated markets may be more likely to turn away patients for contraceptive or fertility care prohibited by the ERDs, potentially related to more conservative culture in more concentrated (i.e., rural) markets. I measured hospital concentration using the Herfindahl-Hirschman Index (HHI) with annual birth volumes per hospital service area (HSA). HSAs are defined by the Dartmouth Atlas Project.

may differ systematically on dimensions that independently affect contraception provision. Therefore, including these controls allows me to more precisely isolate the effect of ERD adherence, holding constant other hospital attributes that might confound the relationship between religious restrictions and the study outcomes.

Last, the models control for several market characteristics at the patient county level. As individual religiosity is unobserved, I control for county religious composition (number of Catholic adherents per 1,000 population) using data from the 2020 U.S. Religious Congregations and Membership Study (Grammich et al., 2020). I also control for outpatient reproductive healthcare access using number of obstetrician-gynecologist physicians per 100,000 population per year (HRSA, 2024) and number of abortion clinics per year from Dr. Caitlin Myers' restricted-use Abortion Facility Database (Myers, 2025). Most abortion clinics provide comprehensive reproductive healthcare services, including contraception (Dreweke et al., 2025).

3.5 Sample description

Table 2 presents summary statistics of the analytic sample, stratified by hospital type (ERD-adherent vs. not). Panel A reports patient characteristics. In general, patients appear to be similar on observable demographic and clinical characteristics across hospital type. Mean age is consistent (29, SD: 6). ERD-adherent hospitals served a slightly higher proportion of White patients (56% vs. 48%) and those with private insurance (52% vs. 50%), but lower proportion of urban patients (67% vs. 72%) compared to non-ERD-adherent hospitals. Rates of comorbidities were generally low and consistent across hospital types. Patients at ERD-adherent hospitals were slightly healthier than those discharged from non-ERD-adherent hospitals (-0.27 Elixhauser score vs. -0.18).

Panel B reports hospital characteristics. A higher proportion of ERD-adherent discharges were from nonprofit hospitals (96% vs. 75%). More ERD-adherent hospitals were part of a health system relative to non-ERD-adherent hospitals (89% vs. 73%). ERD-adherent hospitals were slightly smaller in size (measured by total annual admissions) compared to non-ERD-adherent hospitals, but Medicaid patient share was similar across hospital types. Almost all hospitals in the dataset had an obstetrics unit and more than a third of total admissions were for obstetrics care—proportions that were similar for both types of hospitals. Concentration was similar across hospital types, indicating that ERD-adherent and non-ERD-adherent hospitals face comparable levels of market competition and helps ensure that

observed differences in service provision were not driven by competitive pressures.

Panel C reports market-level characteristics. Importantly, I find that patients discharged from ERD-adherent hospitals came from counties with slightly **lower** per capita Catholic adherents, on average, compared to patients discharged from non-ERD-adherent hospitals (20 vs. 22 per 1,000 population). While counterintuitive, this finding strengthens the identification strategy by suggesting that patient religiosity is unlikely to be an unobserved confounder affecting both hospital proximity and reproductive health outcomes. In addition, patients discharged from ERD-adherent hospitals live in counties with slightly fewer OBGYNs per capita (12 vs. 13) and fewer abortion clinics (3 vs. 5) compared to patients who went to non-ERD-adherent hospitals.

The sample includes 10,994,881 total birth hospitalizations, 15% at ERD-adherent hospitals (a proportion that aligns with previous research (Solomon et al., 2020; Meille and Monnet, 2024)). The sample includes 109 unique ERD-adherent and 666 non-ERD-adherent hospitals.[16] Figure 4 shows the geographic distribution of hospitals in the study sample by ERD-adherence. Visual inspection suggests that ERD-adherent hospitals are dispersed throughout the states in the sample rather than geographically clustered, supporting the plausibility of the identification strategy.

In Table 3, I describe differences in key study outcomes by hospital type (ERD-adherent vs. not). Unadjusted rates of permanent contraception were more than double at non-ERD-adherent compared to ERD-adherent hospitals (6.80% vs. 3.07%). Similarly, the rate of non-permanent contraception provision was much higher at non-ERD-adherent compared to ERD-adherent hospitals (3.68% vs. 1.37%). In contrast, unadjusted rates of short-interval pregnancy within 6, 12, and 18 months of an index delivery were comparable across hospital types.

Appendix 1 Table A6 describes postpartum contraception provision stratified by delivery mode and hospital type. Consistent with prior research, both permanent and non-permanent postpartum contraception were more frequently provided following cesarean section than vaginal delivery (ACOG, 2021; Rodriguez et al., 2024).[17] The proportion provided after a c-section was slightly higher at ERD-adherent hospitals, especially for non-permanent contraception (84.0% vs. 71.8% at non-ERD-adherent hospitals). I conduct two analyses to confirm that differences in delivery mode across hospital types are

[16] Given that ERD status can change over time, the bottom row of Table 2 was defined the first year each hospital appears in the dataset.

[17] Providing immediate postpartum contraception—particularly tubal ligation and IUD placement—after cesarean delivery minimizes additional clinical risk, logistical complexity, and patient cost (ACOG, 2021; Rodriguez et al., 2024).

not confounding the results. First, I regress delivery type on ERD-adherent status with the full set of patient-, hospital-, and market-level controls and find no significant relationship (coef: 0.00333, SE: 0.00669, p-value: 0.618). This helps to rule out differential c-section rates as an alternative explanation for differences in contraception provision across hospital types. Second, I re-estimate the main 2SLS models adding delivery type as a control (see Section 5.3) and find results are consistent with the main analyses.

4 Empirical approach

My empirical goal is to estimate the causal relationship between giving birth at an ERD-adherent hospital and i) the likelihood of receiving postpartum contraception; and ii) downstream adverse reproductive health events, specifically short-interval pregnancies and fertility. The mechanism driving the results is straightforward: the Ethical and Religious Directives explicitly prohibit hospitals from providing services that "render procreation impossible," including sterilization and contraception.[18] In order to build intuition for why the preferred empirical approach is necessary to estimate this relationship as causal, I first discuss a naive ordinary least squares (OLS) estimation strategy in Section 4.1 that does not account for endogeneity of patient hospital choice. In Sections 4.2 and 4.3, I outline the 2SLS model that leverages differential distance between ERD-adherent and non-ERD-adherent hospitals, relative to where a patient lives, as an instrument that quasi-randomly assigns patients to hospitals. I discuss assumptions and validity of the identification strategy in Section 4.5 and describe the "complier" population in Section 4.4. In Section 4.6, I discuss limitations of the empirical approach.

4.1 Naive OLS estimation

If patients were randomly assigned to hospitals, I could estimate a causal relationship between treatment and outcomes using a standard ordinary least squares (OLS) regression. In this approach, I model outcome Y of patient i in market m admitted to hospital h in year t as a function of hospital type (ERD-adherent vs. not) and patient-, hospital-, and market-level characteristics:

$$Y_{ihmt} = \beta_0 + \beta_1 \text{ERD}_{ihmt} + \mathbf{X}'_{ihmt}\boldsymbol{\beta}_2 + \delta_{st} + \varepsilon_{ihmt} \tag{1}$$

[18] Qualitative research with clinicians and hospital administrators further confirms that postpartum contraception is rarely provided at ERD-adherent hospitals (outside of exceptional circumstances requiring approval of the local bishop and hospital ethics board (Freedman, 2023)).

where ERD_{ihmt} is an indicator variable equal to 1 if the patient was admitted to an ERD-adherent hospital and 0 otherwise; $\mathbf{X}_{ihmt}$ is a vector of patient-, hospital-, and market-level controls described in Section 3; and ε_{ihmt} is the error term. δ_{st} are state-by-year fixed effects, which control for time-varying, state-specific factors (e.g., changes in data collection protocols, Medicaid policies, and market dynamics) and are particularly important given the unbalanced panel dataset. Standard errors are clustered at the patient's county of residence to account for intra-county correlation in unobserved factors specific to where a patient lives.

The coefficient of interest, β_1, represents the average treatment effect of ERD-adherent hospital admission on outcome Y only if $\text{Cov}(\text{ERD}_{ihmt}, \varepsilon_{ihmt}) = 0$. However, this exogeneity assumption may be violated in the setting as patients may select hospitals based on unobservable factors—such as individual religiosity, preferences, or unmeasured health severity—that are also correlated with reproductive health outcomes. For example, Catholic patients may prefer to go to ERD-adherent hospitals and also be less likely to seek or accept contraceptive services due to religious beliefs. Alternatively, particularly vulnerable patients (e.g., uninsured, low-income, or medically complex patients) may be more likely to seek care at Catholic hospitals (given the mission-based identity) and also be at higher risk for adverse reproductive health outcomes. In either scenario, $\text{Cov}(\text{ERD}_{ihmt}, \varepsilon_{ihmt}) \neq 0$, violating the exogeneity assumption required for causal interpretation of β_1. Given these concerns, I turn to a quasi-experimental setting to more plausibly estimate the causal relationship of interest.

4.2 Differential distance instrument

To account for the endogeneity of hospital choice, I instrument for the potentially endogenous type of hospital where a patient gives birth (ERD-adherent vs. not) using differential distance (DD), defined as the great-circle ("as the crow flies") distance from a patient's zip code centroid to the nearest ERD-adherent hospital minus the distance to the nearest non-ERD-adherent hospital, calculated annually to account for hospital openings and closures.[19] For example, if a patient lives 15 miles from the nearest ERD-adherent hospital and 5 miles from the nearest non-ERD-adherent hospital, DD = 10 miles, indicating the non-ERD-adherent hospital is relatively closer. This instrument captures quasi-

[19]To calculate the DD instrument, I first compute distances in miles from each patient's zip code centroid to the nearest ERD-adherent and non-ERD-adherent hospital using hospital latitude and longitude coordinates. I then calculate the difference between these distances. For patient residence, I use the population weighted zip code centroid, which is calculated using the center of mass of the population within each zip code and is shown to be a more accurate measure of where people live (Berke and Shi, 2009).

random variation in the likelihood of admission to a type of hospital driven by geographic convenience, rather than unobserved patient preferences correlated with health outcomes.

The DD instrument is commonly employed in studies examining operational characteristics of health organizations to address bias stemming from the endogeneity of hospital choice (Lee et al., 2025; Card et al., 2023; Templeton et al., 2023; Cornell et al., 2019; McClellan et al., 1994). The DD instrument leverages the well-known preference of healthcare consumers for nearby medical providers, including for obstetrics care (Ellis et al., 2020; Gowrisankaran et al., 2015; Fontenot et al., 2024; Hebert et al., 2020; Koller et al., 2024; Raval and Rosenbaum, 2021; McGuirk and Porell, 1984). For instance, Gowrisankaran et al. (2015) found that a five-minute increase in travel time reduces demand for general inpatient hospital care by 17-41%. In addition, Hebert et al. (2020) found that the top two reasons women gave for choosing a hospital for obstetric care were quality and location (76% and 72%, respectively).[20]

One potential concern with the DD instrument is that patients facing extreme travel trade-offs may not represent a population with meaningful choice, potentially violating the common support assumption required for instrument validity. To address this, I exclude observations where the absolute DD exceeds 100 miles (approximately 2.5 to 3 hours of driving time).[21] This excludes about 1% of the sample, focusing the analysis on patients for whom both types of hospitals are reasonably accessible. As shown in Figure 1, the closest non-ERD-adherent hospital is 16.4 miles closer to the patient zip code centroid, on average, than the closest ERD-adherent hospital. Appendix 1 Figures A1 and A2 present the DD distribution for the rural and urban sub-samples, respectively. As expected, DD is more widely dispersed for rural patients given the lower density of hospitals and therefore greater variation in travel distance. The mean DD is 28.4 and 11.6 miles for rural and urban patients, respectively. In addition, Appendix 1 Figure A3 presents the DD distribution for the full (non-trimmed) sample, which is highly right-skewed. The DD mean is 17.9, with -39.3 as the minimum and 206.8 as the maximum. As a robustness check, I re-estimate the primary outcomes using the untrimmed DD measure (see Section 5.3). Results are consistent, although with a lower first-stage F statistic, especially for the rural sub-population.

Appendix 1 Table A7 presents a few key descriptive statistics related to hospital distance and patient

[20] This rationale is also a common theme qualitative research. For example, Boulware et al. (2024) found that location and convenience were given as common reasons for choosing a delivery hospital. For example, women stated: "It was convenient to my home ... and, with the insurance plan that I had, that's where I was allowed to go," or "It was the one closest to my house, and I had heard really good things about it and the doctors who I worked with."

[21] 100 miles was selected as the cutoff as this isolates extreme outliers in approximately 1% of the sample and corresponds to previous literature examining hospital choice for obstetric care (Fontenot et al., 2024; Hebert et al., 2020).

location. As shown in Panel A, the average distance from a patient's residence to the birth hospital was 10 miles in the full sample (12-20 minutes estimated drive time), and 14 and 8 miles for rural and urban populations, respectively. These numbers align with estimates from previous research (Fontenot et al., 2024). On average, the closest hospital was ERD-adherent for 13% of hospitalizations in the sample (Panel B). If the closest hospital was ERD-adherent, the patient gave birth there 36% of the time. This proportion is slightly higher for rural patients compared to urban (43% vs. 32%), which aligns with the finding that the first stage F-statistic is higher for rural vs. urban patients (73.49 vs. 29.63, see Table 5), and demonstrates that distance is more predictive of hospital choice in rural areas where there are fewer alternatives.

4.3 2SLS-IV model

I use a two-stage least squares (2SLS) model to estimate the causal relationship between hospital type (ERD-adherent vs. not) and the outcomes of interest. In the first stage, I estimate the relationship between the endogenous treatment variable and the plausibly exogenous instrument:

$$\text{ERD}_{ihmt} = \alpha_0 + \alpha_1 DD_{izt} + \mathbf{X}'_{ihmt}\boldsymbol{\alpha}_2 + \delta_{st} + \mu_{ihmt} \tag{2}$$

where DD_{izt} is the differential distance for patient i in zipcode z in year t; $\mathbf{X}_{ihmt}$ denotes the full set of patient, hospital, and market-level controls; and δ_{st} represents state-by-year fixed effects. Standard errors are clustered at the patient county level. The coefficient α_1 captures how differential distance—relative proximity to an ERD-adherent vs. non-ERD-adherent hospital—influences the likelihood of admission to an ERD-adherent hospital.

I present the first-stage relationship in Figure 2 to confirm the relevance assumption needed for a valid 2SLS-IV design. The binned scatterplot visually confirms the strong, negative first-stage relationship between the instrument and ERD-adherent hospitalization.[22] The coefficient of -0.00517 (SE: 0.0005, p-value: <0.001) indicates that each mile increase in differential distance—meaning the ERD-adherent hospital becomes relatively farther away—decreases the probability of ERD-adherent hospitalization by

[22]The plot is constructed by first residualizing both the treatment (admission to an ERD-adherent hospital) and the differential distance instrument on the full set of exogenous controls and fixed effects. Residualizing both variables removes variation explained by observable covariates, isolating the core relationship between the instrument and treatment that drives identification. The figure plots the average of the residualized treatment variable for each vigintile (i.e., 20 equal-sized bins) of the residualized instrument.

approximately 0.52 percentage points, or 5.2 percentage points for a 10-mile change in relative distance. In addition, the first-stage F-statistic, 97.19, is well above conventional thresholds for weak instrument concerns (e.g., F > 10), indicating that the instrument has substantial explanatory power with respect to the endogenous treatment variable.[23]

In the second stage, I estimate the causal effect using only the exogenous variation from the first stage:

$$Y_{ihmt} = \gamma_0 + \gamma_1 \widehat{\text{ERD}}_{ihmt} + \mathbf{X}'_{ihmt}\boldsymbol{\gamma}_2 + \delta_{st} + \nu_{ihmt} \tag{3}$$

The key explanatory variable, $\widehat{\text{ERD}}_{ihmt}$, is the predicted probability of ERD-adherent hospital admission from the first stage. Because $\widehat{\text{ERD}}_{ihmt}$ is constructed from the exogenous instrument, it is uncorrelated with unobserved confounders: $\text{Cov}(\widehat{\text{ERD}}_{ihmt}, \nu_{ihmt}) = 0$. Consequently, γ_1 provides a consistent estimate of the causal effect of ERD-adherent hospital admission on outcome Y.

The differential distance instrument is continuous, so γ_1 represents a weighted average of local average treatment effects (LATE) across different values of the differential distance instrument. Following Imbens and Angrist (1994), the weights correspond to the density of "compliers" at each value of the instrument—that is, the proportion of patients whose hospital choice changes in response to marginal changes in differential distance. In this context, the estimate gives more weight to the treatment effects of individuals whose hospital choice is most sensitive to the instrument.[24] Therefore, γ_1 should be interpreted as the average causal effect of admission to an ERD-adherent hospital for compliers, the population of patients whose choice of hospital is determined by geographic convenience.

4.4 Complier characteristics

The 2SLS-IV approach identifies treatment effects for compliers—patients whose hospital choice is determined by geographic proximity (as compared to those willing or able to travel for care that better meets their preferences). This complier population is most affected when hospital mergers or acquisitions eliminate local non-religious alternatives, a key concern in current policy debates regarding Catholic

[23] As the specification clusters standard errors by patient county, I report the robust Kleibergen-Paap Wald rk F statistic, which is appropriate when the assumption of independent and identically distributed (i.i.d.) errors is violated (Andrews et al., 2019).

[24] Following Imbens and Angrist (1994), the LATE weights observations by both the density of patients at each differential distance value and how responsive hospital choice is to distance at that value. The estimate therefore places greater weight on patients whose hospital choice is most sensitive to relative geographic proximity.

healthcare expansion. In Appendix 1 Table A8, I characterize the complier population compared to "always-takers" (patients who deliver at ERD-adherent hospitals regardless of proximity) and "never-takers" (patients who deliver at non-ERD-adherent hospitals regardless of proximity). To do so, I use a binary version of the instrument with the method developed by Marbach and Hangartner (2020), which builds on the principal stratification framework of Angrist et al. (1996).[25]

Compliers are less likely to be White and more likely to have Medicaid and be admitted on an urgent basis compared to both always-takers and never-takers. Compliers also have a higher Elixhauser score, indicating worse health status (-0.07 vs. -0.24 and -0.26 for always-takers and never-takers, respectively). These results suggest that the complier population is composed of lower-SES, racially diverse patients experiencing urgent obstetric situations who deliver at the nearest hospital due to time and resource constraints. Always- and never-takers, in contrast, generally have more resources and are able to make planned hospital choices independent of proximity. These findings align with previous evidence showing that less advantaged families are more affected by distance constraints (Card et al., 2023; Fontenot et al., 2024), and underscore that compliers—the population for whom the LATE is identified—are precisely those most affected when mergers or acquisitions eliminate local non-religious hospital alternatives.

4.5 Instrument validity

In addition to the relevance and common support assumptions discussed above, the 2SLS-IV model requires instrument independence and support for the exclusion restriction. Instrument independence requires that the instrument is "as-good-as-randomly" assigned conditional on controls—that is, no unobserved patient characteristics are correlated with both differential distance and the outcomes. In the context, this means that patients living relatively closer to ERD-adherent hospitals cannot otherwise differ systematically on unobserved characteristics that also affect study outcomes (after controlling for patient-, hospital-, and market-level characteristics). The exclusion restriction requires that the differential distance instrument affects the outcomes only through the treatment, with no direct pathway

[25]Following Marbach and Hangartner (2020), under standard instrumental variable assumptions (monotonicity, exclusion restriction, random assignment of the instrument, and stable unit treatment value assumption), I can identify the mean of any pretreatment covariate for each compliance type. This method requires a binary instrument; I therefore create an indicator for whether a patient resides closer to an ERD-adherent vs. non-ERD-adherent hospital from the continuous differential distance measure. While the main analysis exploits continuous variation in differential distance, the binary instrument identifies all patients whose hospital choice is distance-sensitive. The marginal compliers in the continuous specification—those whose choices respond to incremental differential distance changes—are likely a subset of the binary compliers I characterize here, concentrated among patients facing the strongest distance constraints. The patterns I document therefore likely represent lower bounds on the characteristics of marginal compliers in the main estimates.

between the two. This would be violated, for instance, if differential distance acted as a proxy for rurality or general healthcare access, and thus had a direct causal effect on contraception access that was separate from hospital type (ERD-adherent vs. not). While neither assumption can be tested directly, I provide both conceptual and empirical evidence (specifically, balance and negative control outcome, NCO, tests) supporting the plausibility of both assumptions.

Conceptual support. From a conceptual standpoint, several features of the setting support these assumptions. First, variation in differential distance is plausibly uncorrelated with unobserved patient preferences regarding contraception. Evidence suggests that patients are often unaware of a hospital's Catholic affiliation or the associated care restrictions (Guiahi et al., 2014; Wascher et al., 2018; Stulberg et al., 2019; Hebert et al., 2020), and Catholic hospitals are often not transparent about their religious restrictions (Takahashi et al., 2019; Guiahi, 2020; Wascher et al., 2020). For instance, a national study found that only 3% of women selected their hospital for obstetric care because it was religiously-affiliated (Hebert et al., 2020). This lack of awareness and low rates of religious-based hospital selection support the independence assumption by suggesting that patients do not systematically sort into religiously-affiliated hospitals. In addition, national surveys show that Catholic women use contraception at the same rate as the U.S. population: 99.0% of Catholic women use any type of contraception (vs. 99.6% nationally) and 32% use permanent contraception (vs. 33% nationally) (Jones and Dreweke, 2011; Jones, 2020). Taken together, this evidence suggests that marginal differences in geographic proximity are unlikely to be systematically correlated with unobserved patient characteristics such as religiosity or preferences regarding contraception. Rather, the variation in the instrument captures quasi-random differences in which hospital type happens to be more geographically convenient.

Second, the use of continuous differential distance instrument (rather than a binary indicator for closest hospital type) provides additional support for the identifying assumptions. Strategic hospital location by Catholic health systems might create systematic correlations between ERD-adherent hospitals and area religiosity or abortion attitudes. However, the instrument exploits variation in *relative* distances within geographic areas rather than absolute proximity to ERD hospitals. Conditional on the controls—which include county-level religiosity, abortion clinic availability, and OB/GYN density—the remaining variation in differential distance reflects which hospital type happens to be marginally closer to a given patient. In addition, the continuous nature of the instrument allows me to control for broader healthcare infrastructure (e.g., distance to nearest hospital of any type) while isolating the variation from relative

proximity.

Balance tests. To provide empirical support for my identification strategy, I conduct balance tests that examine whether the instrument predicts patient characteristics that might be correlated with the outcomes. The logic of these tests is analogous to randomization checks in experimental settings: if my instrument captures quasi-random variation in geographic access to hospitals, it should not systematically predict patient characteristics associated with contraception use. Finding that the instrument is unrelated to *observed* patient characteristics provides evidence that it is also unlikely to be correlated with *unobserved* characteristics that could threaten the independence assumption. I implement these balance tests in two steps using linear regression models. First, I construct predicted outcomes for each patient by regressing the outcome Y_{ihmt} on patient-level demographic and clinical covariates (described in Section 3) and obtaining the fitted value $\widehat{Y}_{ihmt}$. This predicted outcome, or "risk score," captures the component of the outcome explained by observable patient characteristics—specifically, the likelihood that a patient would seek or use contraception based on their observed traits. By aggregating information across all patient covariates into a single summary measure, this approach provides a more powerful test of balance than examining each covariate separately. Second, I residualize both $\widehat{Y}_{ihmt}$ and the instrument DD_{izt} with respect to hospital and market-level covariates, and state-by-year fixed effects. This residualization removes variation attributable to observable contextual factors, isolating the relationship between the instrument and patient-level characteristics net of geographic and temporal variation.[26]

If residualized differential distance is associated with residualized predicted outcomes, this would indicate that, even after accounting for contextual factors, the instrument predicts the types of patients who seek (or avoid) contraception, violating independence. Figure 3 presents binned scatterplots of the residualized instrument against residualized predicted outcomes for permanent contraception (Panel A) and non-permanent contraception (Panel B). The plots show a flat relationship between the residualized predicted outcomes and DD instrument, with estimated coefficients not significant and close to zero.

Negative control outcome (NCO) tests. To further assess the validity of the identification strategy, I conduct negative control outcome (NCO) tests following the framework established by Danieli

[26]Since the predicted outcome is constructed as a linear combination of patient-level covariates, testing whether the instrument predicts this single risk score is equivalent to a joint test of whether the instrument is uncorrelated with all patient-level covariates. This approach is more powerful than testing each covariate individually and avoids multiple testing concerns.

et al. (2026), which adapts established epidemiological methods to instrumental variable designs to evaluate instrument independence (Shi et al., 2020). I regress differential distance on each negative control together with the full vector of patient-, hospital-, and market-level controls and state-by-year fixed effects (six separate models). NCOs were selected to test for specific sources of potential selection bias. First, for unobserved religiosity and contraceptive preferences, I use marital status and Catholic adherents per capita.[27] Second, for unobserved health status and obstetric complexity, I use asthma and prolonged pregnancy,[28] both tied to maternal health but neither plausibly impacted by the ERDs. Third, for unobserved fertility preferences, I use parity and age at first birth,[29] which summarize reproductive history determined prior to the index delivery. Appendix 1 Table A9 shows no statistically significant associations between differential distance and each of these six NCOs. These null results increase confidence that the effects observed for the main study outcomes reflect the causal impact of hospital ERD adherence rather than patient selection or other unobserved confounding.

4.6 Limitations

Although the instrumental variable design addresses patient selection through quasi-randomization, a key study limitation is that patient-level instruments cannot fully eliminate confounding from hospital-level organizational factors (Konetzka and Werner, 2025). This concern takes two forms. First, the instrument itself could induce variation in hospital attributes beyond ERD adherence: if differential distance shifted patients not only between ERD-adherent and non-adherent hospitals but also between hospitals of different size or ownership, the estimates would blend the effect of religious restrictions with the effects of these attributes. Appendix 1 Table A10 addresses this concern by regressing six hospital organizational characteristics on differential distance (conditional on ERD adherence, patient and market characteristics, and state-by-year fixed effects). While three of the six associations are statistically significant, all are economically negligible: a DD unit change corresponds to a -0.2% change in for-profit ownership, a 0.2% change in admissions, and a 0.1% change in the presence of an obstetric unit (relative to the full sample means, Column 1).

[27] On average, Catholic women are more likely to be married and less likely to get divorced (Lehrer, 2004; Call and Heaton, 1997).

[28] The specific reasons for prolonged pregnancy, defined as delivery after 42 weeks gestation, are unknown. A common cause is inaccurate dating. Common risk factors for "true" prolonged pregnancy include primiparity, previous postterm pregnancy, male fetus, and obesity (Galal et al., 2012).

[29] Age at first birth and parity analyses use the longitudinal subsample with unique patient identifiers (see Appendix 1 Table A2). These variables capture births observed in the 2010–2023 data; for women whose first birth preceded 2010, I observe age at first *observed* birth, introducing potential left-censoring. This measurement error would only threaten the conclusions if correlated with differential distance, which is unlikely given the instrument's quasi-random variation.

Second, ERD-adherent hospitals may differ from non-ERD-adherent hospitals on unobserved dimensions (e.g., institutional culture or staffing models) that are correlated with both religious restrictions and patient outcomes. While I control for a wide range of observable hospital characteristics to mitigate this concern, no test can fully address this possibility as such differences would persist even under random assignment of patients across hospitals. Therefore, I cannot rule out the possibility that the estimates capture both the direct effect of ERD adherence and correlated organizational attributes. This limitation has important implications for interpretation and policy application. From a patient perspective, these results accurately characterize the consequences of giving birth at an ERD-adherent hospital as they currently exist: institutions that, as a package, include both religious service restrictions and the organizational characteristics that accompany them. As long as these organizational attributes continue to coincide together in practice, the estimates reflect the treatment effect of giving birth at an ERD-adherent hospital. However, the results may not predict the effects of external policy changes that alter ERD status independently, such as state mandates requiring contraception provision. Such interventions would change religious restrictions without necessarily changing underlying organizational characteristics, and thus could have different effects than I estimate. This caveat does not diminish the relevance of the findings for understanding current patient experiences, but suggests caution in extrapolating to counterfactual policy scenarios.

5 Main results

5.1 Index birth

5.1.1 Immediate postpartum contraception

Table 4 presents the effect of ERD-adherent hospitalization on provision of immediate postpartum contraception. OLS estimates in Column (1) show a strong negative association for both types: admission to an ERD-adherent hospital leads to a 3.81 percentage point (pp) and 2.24 pp reduction in the likelihood of receiving permanent and non-permanent contraception, respectively (61% and 67% decrease relative to the sample means). Columns (2)–(5) report the LATE from the 2SLS-IV models under progressively richer sets of covariates: Column (2) includes only patient-level covariates, Column (3) includes patient- and hospital-level covariates, Column (4) includes hospital- and market-level covariates, and the pre-

ferred specification, Column (5), includes all three (patient-, hospital-, and market-level covariates).[30] The instrument progressively strengthens, with first-stage F-statistics starting from 48.69 in the sparsest specification (Column 2) to 97.19 in the fullest (Column 5).

For permanent contraception (Panel A), the effect is negative and significant ($p < 0.01$) in every specification, ranging from -4.68 pp with patient-level covariates alone to -3.72 pp in the preferred full specification, which corresponds to a 60% reduction relative to the mean. Observed similarity between the preferred LATE estimate in Column (5) and OLS estimate in Column (1) suggests limited patient selection once observables are accounted for. For non-permanent contraception (Panel B), evidence on the effect of ERD-adherent hospitalization is weaker. The estimate is negative throughout, but only statistically significant in the OLS model (Column 1, -2.24 pp, $p < 0.01$) and 2SLS estimates with patient-level controls (Column 2, -1.19 pp, $p < 0.10$). The 2SLS estimate attenuates and loses statistical significance as hospital- and market-level controls are introduced in Columns (3)-(5). The divergence between the two types of contraception likely reflects where each method is typically obtained. Sterilization is more commonly performed during the delivery hospitalization, whereas non-permanent can be accessed more easily post-discharge at an outside provider.

Appendix 1 Table A11 presents the reduced-form estimates, capturing the intent-to-treat relationship between differential distance and postpartum contraception provision. For permanent contraception (Panel A), the instrument is positive and significant across all subgroups: a 10-mile increase in differential distance is associated with a 0.19 pp ($p < 0.01$) increase in the probability of sterilization in the full sample, with similar magnitudes for rural (0.21 pp, $p < 0.01$) and urban patients (0.23 pp, $p < 0.01$). For non-permanent contraception (Panel B), the pattern diverges by geography: the instrument is positive and significant for rural patients (0.10 pp for 10-mile change, $p < 0.01$), but near zero and insignificant for urban patients. As discussed in the next section, this pattern likely reflects that urban patients are less reliant on the birth hospitalization as a point of access for non-permanent contraception (as evidenced by their lower baseline rates) given the increased ease in accessing outpatient alternatives.

Heterogeneity by patient location. The density and competitiveness of hospital markets differs substantially between rural and urban areas. Rural patients typically have fewer hospital choices, making their selection more sensitive to geographic distance and potentially amplifying the impact of facility-level service restrictions. In addition, the birth hospitalization is a particularly important point of access

[30] All specifications include state-by-year fixed effects and cluster standard errors by patient county.

for rural populations given the lower density of healthcare providers. Therefore, I explore heterogeneity in the effects of ERD-adherent hospitalization on contraception provision by patient location, estimating separate models for rural and urban populations. The main model defines urban as a large metropolitan county using USDA Urban Influence Codes (UIC). I explore robustness using two alternative definitions: (i) metropolitan areas from OMB Core-Based Statistical Areas (CBSAs), and (ii) counties with below average provider concentration.[31] In all models, rural is defined as non-urban.

The LATE for rural patients is reported in Column (1) of Table 5. Admission to an ERD-adherent hospital reduces sterilization by 4.75 pp ($p < 0.01$), a 64% decrease relative to the rural mean of 7.48%, and non-permanent contraception by 2.20 pp ($p < 0.01$), a 63% decrease relative to the mean of 3.50%. The first-stage F-statistic is 73.49. Column (2) presents results for urban patients, which has a weaker first stage (F-stat: 29.63).[32] ERD-adherent hospitalization reduces sterilization by 4.84 pp ($p < 0.05$) for urban patients, an 84% relative reduction. In contrast to rural populations, the effect on non-permanent contraception for urban patients is close to zero and statistically insignificant. I offer two complementary explanations for this pattern. First, the lower mean rate of non-permanent contraception at delivery among urban patients (3.27% vs. 3.50% for rural patients) suggests that urban women may intend to obtain non-permanent contraception (e.g., IUDs and implants) at outpatient postpartum visits rather than at the birth hospitalization. In contrast, rural women face greater barriers to returning for separate visits, making the birth hospitalization a less replaceable point of access. Second, it is possible that ERD-adherent hospitals in urban (often more politically liberal) areas are subject to less stringent diocesan oversight, allowing for greater de facto provision of non-permanent contraception compared to their rural counterparts.

In Appendix 1 Table A12, I present the LATE of ERD-adherent hospitalization on immediate postpartum contraception using two alternative rural-urban definitions. Findings are consistent across both alternative definitions. First, using the OMB definition, I find ERD-adherent hospitalization leads to 62% and 54% relative decreases in permanent and non-permanent contraception, respectively, for rural populations (both $p < 0.01$). Urban populations experience a 62% relative decrease in permanent contraception at ERD-adherent hospitals ($p < 0.01$), but no significant change in non-permanent contraception. Second, I also see consistent results, though slightly higher in magnitude, when rural vs.

[31] Provider concentration was defined using the Herfindahl-Hirschman Index with annual birth volumes at hospitals located in patient county of residence.

[32] The weaker first stage in urban areas (F-stat: 29.63) likely reflects that urban patients have access to a denser hospital network, and that great-circle distance is a noisier proxy for actual travel burden in urban areas.

urban is defined as a measure of provider concentration. For rural populations, ERD-adherent hospitalization leads to 75% and 63% decrease in permanent and non-permanent contraception, respectively (both $p < 0.01$). Urban populations experience an -6.44 pp change ($p < 0.01$) in permanent contraception provision at ERD-adherent hospitals, but no significant change in non-permanent contraception. Taken together, these findings underscore the disproportionate vulnerability of rural populations to ERD-adherent hospital service restrictions.

Heterogeneity by patient insurance. Insurance coverage policies may mediate the relationship between hospital religious restrictions and contraception access. Throughout the study period, Medicaid coverage of postpartum contraception evolved, especially coverage of immediate postpartum LARC.[33] While the state-by-year fixed effects absorb most variation from these policy changes, differential exposure could remain if ERD-adherent hospitals serve different patient populations. Descriptively, ERD-adherent hospitals do serve a slightly lower share of Medicaid patients (42% vs. 44%, Table 2). I therefore estimate separate models for privately-insured and Medicaid patients to assess whether insurance coverage policies interact with religious restrictions in ways that the main specification does not fully capture.

Table 6 presents 2SLS estimates stratified by insurance type. For privately-insured patients, ERD-adherent hospitalization reduces permanent contraception by 3.77 pp (72% relative to mean, $p < 0.01$) and non-permanent contraception by 1.03 pp (42% relative to mean, $p < 0.05$). For Medicaid patients, permanent contraception decreases by 3.66 pp (50% relative to mean, $p < 0.05$), while the effect on non-permanent contraception is smaller and not statistically significant. The null result for non-permanent contraception likely reflects financial, logistical, and provider-level barriers for Medicaid patients, rather than religious restrictions alone. Historically, Medicaid reimbursed all obstetric care under a global fee, making separate reimbursement for LARC devices financially infeasible for hospitals (Rodriguez et al., 2024). Beginning in 2012, states began amending billing policy to allow separate reimbursement (Rodriguez et al., 2024). However, even after states unbundled Medicaid reimbursement, Medicaid LARC rates remained low and uptake was gradual, reflecting persistent implementation barriers, such as the need for provider awareness, staff training, and device stocking (Rodriguez et al., 2024). In contrast, permanent contraception—which was consistently covered by both Medicaid and private insurance—

[33]Throughout the study period, states gradually shifted from bundled childbirth payments, which created financial barriers to immediate postpartum LARC provision for Medicaid patients, to unbundled reimbursement systems that separately compensated postpartum contraception (Rodriguez et al., 2024).

shows similar, significant reductions at ERD-adherent hospitals across insurance types.

Dobbs' impact and temporal consistency. In addition to heterogeneity by patient location and insurance, I also examined whether the Supreme Court's decision in *Dobbs v. Jackson Women's Health Organization* impacted the effect of Catholic hospital religious directives on postpartum contraception (see Appendix 1 Table A13). Appendix 3.1 provides additional details on the methodological approach here. In the split-sample analyses (Columns 1-2), I find the effect of ERD-adherent hospitalization on permanent contraception (Panel A) was slightly lower in the post-*Dobbs* period (48% relative decrease compared to 55% pre-*Dobbs*, both $p < 0.01$). Consistent with the main model, I find no effect on non-permanent contraception (Panel B).

Interaction analyses are presented in Columns 3-5, split by states protective of abortion rights post-*Dobbs* in the sample (MD, NJ, NY, OR, and WA) and those hostile (AZ, FL, and KY). For permanent contraception (Panel A), I find the effect of ERD-adherent hospitalization did not significantly change post-*Dobbs*, indicated by the non-significant interaction term in Columns (3)-(5). For non-permanent contraception (Panel B), two interesting findings emerge. First, in states hostile to abortion rights post-*Dobbs* in the study sample (Column 5), the effect of ERD-adherent hospitalization was significant in the pre-*Dobbs* period (-2.59 pp, 78% decrease relative to the mean), suggesting the ERDs may be more binding in states generally hostile to reproductive rights (also typically politically conservative). Second, the interaction term is positive and significant in Columns (3)-(5) in both protective and hostile states, meaning the relative negative effect of delivering at an ERD-adherent facility on non-permanent contraception provision was attenuated (or increased positively) in the post-*Dobbs* period compared to the pre-*Dobbs* baseline. This likely reflects a combination of factors: reduced administrative enforcement of the ERDs on non-permanent contraception, increased provider-level workarounds, and increased patient demand.[34] However, the first stage F-statistics are relatively low for these interaction analyses (ranging from 22.12 to 44.87), so I urge caution when interpreting results.

I also estimated year-by-year treatment effects and plotted the annual coefficients in Appendix 1 Figure A4. While the effect of ERD-adherent hospitalization on permanent contraception is stable (Panel

[34]For example, following Dobbs, heightened legal and clinical stakes may have prompted Catholic health systems or hospital ethics committees to expand access to, or soften institutional restrictions on, non-permanent contraceptive methods. In addition, clinicians at ERD-adherent hospitals where permanent sterilization remained strictly prohibited may have increasingly substituted toward non-permanent methods. Last, post-*Dobbs*, patients may have shown higher baseline demand for non-permanent postpartum contraception, leading to more active patient requests and subsequent provision prior to discharge.

A), estimates for non-permanent contraception (Panel B) attenuate over time and become statistically indistinguishable from zero in later years. I believe this pattern reflects a combination of several factors: (i) rates of non-permanent contraception at non-ERD-adherent hospitals may be suppressed in later years given changes in ICD-10 reimbursement procedures,[35] and (ii) rates of non-permanent contraception have likely been increasing at ERD-adherent hospitals over time (for reasons discussed above, such as increased administrative tolerance, higher patient demand, and Medicaid unbundling of LARC).

5.1.2 Index birth outcomes

In Appendix 1 Table A14 I examine the effect of ERD-adherent hospitalization on adverse outcomes potentially experienced during the index birth, including SMM, preterm birth, and complications.[36] In the full sample (Column 1), coefficients for SMM, preterm birth, and complications are positive, although not statistically significant. I find that urban patients, however, are 33% and 31% more likely to have a preterm birth and experience complications, respectively, if they gave birth at an ERD-adherent hospital vs. not (Column 3).

5.2 Downstream reproductive health outcomes

5.2.1 Short-interval pregnancy

A critical downstream consequence of barriers to contraception is an increased risk of short-interval pregnancy (SIP). I investigate this relationship by examining the effect of delivering at an ERD-adherent hospital vs. non-ERD-adherent on the risk of SIP. Table 7 presents the LATE for SIP occurring within 6 months (Panel A), 12 months (Panel B), and 18 months (Panel C) of the index birth. For these analyses, I use a subsample of five states with unique identifiers to follow patients over time (AR, FL, MD, NY, WA, n=6,842,695). In the full longitudinal sample (Column 1), I find small and statistically insignificant effects of delivering at an ERD-adherent hospital on the likelihood of a subsequent SIP. The point estimates are positive, suggesting a potential increase, but not precisely estimated. However, when stratifying by geographic location, a clear pattern emerges that aligns with the previous postpartum

[35] Under ICD-10 guidelines, codes for routine non-permanent contraceptive management do not increase a patient's severity-of-illness and therefore do not increase facility reimbursement. Consequently, non-ERD-adherent hospitals, which had higher rates of using these codes in the ICD-9 era, were likely to stop including these codes on patient records post-2015.

[36] Complications include pelvic infection, obstetric sepsis, hemorrhage, embolism, and transfusion, which previous research has found likely to be impacted by the presence of Catholic ERDs (Freedman et al., 2008; Freedman, 2023; Wall and Yemane, 2022).

contraception findings. Rural patients show statistically significant increases in SIP rates after giving birth at an ERD-adherent hospital. Specifically, rural patients are 0.95 pp ($p < 0.05$) more likely to be pregnant within 6 months, representing a 25% increase relative to the rural mean of 3.87%. Results are consistent across varied pregnancy intervals: rural patients experience a 2.42 pp increase ($p < 0.05$) in pregnancies within 12 months and a 4.18 pp increase ($p < 0.05$) in pregnancies within 18 months of the index birth. Relative to the rural baseline means, these estimates represent increased relative risks of 28% and 30%, respectively. In contrast, urban patients show no statistically significant effects. This geographic heterogeneity aligns with the contraception findings, where rural patients experienced the largest reductions in postpartum contraceptive provision at ERD-adherent hospitals. These results provide compelling evidence of a causal chain: hospital religious restrictions reduce contraception provision, which subsequently increases the likelihood of short-interval pregnancies among rural patients who have limited alternative sources of reproductive healthcare.

Appendix 1 Table A15 presents estimates of the effect of ERD-adherent hospitalization on SIP rates using the two alternative rural-urban definitions described previously (see Section 3.3), confirming the robustness of the main findings. For SIP within 6 months (Panel A), results are consistent, though not statistically significant. As shown in Panel B, rural patients have 2.62 pp increased risk of SIP within 12 months after delivering at an ERD-adherent hospital using the OMB definition ($p < 0.10$, 30% relative to the mean), and a 2.37 pp increased risk using the concentration definition ($p < 0.01$, 28% relative to the mean). Results are similar and slightly larger for pregnancies within 18 months (Panel C): rural patients have a 4.43 pp increased risk using the OMB definition ($p < 0.10$, 33% relative to the mean) and a 4.24 pp increased risk using the concentration-based measure ($p < 0.01$, 31% relative to the mean). Estimates for urban patients are small and statistically insignificant across all panels. The consistency of these findings across alternative definitions of rurality provides strong evidence that the main findings are robust.

5.2.2 Short-interval pregnancy with SMM or preterm Birth

Having shown that ERD-adherent delivery increases short-interval pregnancies among rural patients, I next ask whether these more closely spaced pregnancies translate into adverse outcomes (specifically SMM and preterm birth) at the subsequent delivery. I find that giving birth at an ERD-adherent hospital increases the risk of a subsequent short-interval pregnancy complicated by SMM (Table 8). In the full sample, previous ERD-adherent delivery raises the probability of a subsequent pregnancy

within 6, 12, and 18 months ending in an SMM-complicated birth by 87%, 82%, and 78%, respectively (relative to sample means). The effect is stronger among rural patients. For example, ERD-adherent delivery increases the probability of a subsequent pregnancy within 18 months and SMM-complicated birth by 0.31 pp ($p < 0.01$, 130% relative to the mean).[37] There is no statistically significant effect for urban patients. These patterns are consistent across shorter interpregnancy intervals ($\leq$6 and $\leq$12 months) and remain robust when excluding blood transfusions from the SMM definition (Appendix 1 Table A16). In exploratory analyses, I also examine whether ERD-adherent hospitalization elevates subsequent SMM risk *conditional* on having a short-interval pregnancy (Appendix 1 Table A17). The pattern is consistent, though smaller in relative terms. For example, among patients pregnant within 18 months of the index delivery, a previous ERD-adherent delivery raises the probability of subsequent SMM-complicated birth by 1.1 pp ($p < 0.1$, 57% increase relative to the mean). For rural patients, the risk increases by 1.6 pp ($p < 0.01$, 88% relative increase). I interpret these conditional estimates cautiously as the conditioning event (SIP) is itself affected by treatment.

Effects on subsequent SIP and preterm birth are weaker and concentrated among rural patients (Table 9). In the full sample, ERD-adherent delivery has no statistically significant effect on the probability of a short-interval pregnancy with preterm birth (Column 1). However, for rural patients, ERD-adherent delivery raises the risk of a short-interval pregnancy with preterm birth by 0.16–0.33 pp across the 6-, 12-, and 18-month windows (roughly 30–41% relative to the sample means). These effects are not robust to conditioning on having a short-interval pregnancy (Appendix 1 Table A18), suggesting that the rural increase in short-interval preterm births operates largely through the higher incidence of short-interval pregnancies among rural patients, rather than through elevated preterm risk at those births.

5.2.3 Fertility and hospital switching

Next I examine the effect of ERD-adherent hospitalization on fertility outcomes and hospital switching (Table 10). Consistent with reduced contraceptive access, rural patients who delivered at an ERD-adherent hospital show elevated fertility over a three-year window: they are 6.0 pp more likely to have any subsequent birth (Panel A, $p < 0.05$, 31% relative to the mean) and have 0.07 more births on average (Panel B, $p < 0.05$, 34% relative). I find no effect for urban patients, likely reflecting greater access to outpatient contraceptive options. After an ERD-adherent birth, patients are also more likely to switch hospital types to a non-ERD-adherent hospital for their next delivery (Panel C, 37.9 pp, $p < 0.01$).

[37] Relative percentage changes can exceed 100% given the low baseline incidence of SIP and SMM in the rural sub-sample.

However, this switching behavior should be interpreted with caution, as non-ERD-adherent hospitals are broadly more common (85% of all deliveries in the sample), creating a higher baseline probability of switching away from ERD-adherent facilities mechanically.

5.3 Robustness checks

I present five additional robustness checks in Appendix 1 Tables A19-A24. First, I re-estimate the postpartum contraception results on the sub-sample of five states with unique patient identifiers (Arkansas, Florida, Maryland, New York, and Washington). Table A19 presents OLS regressions and 2SLS models with various combinations of controls, and Table A20 examines rural vs. urban heterogeneity. Overall, results are consistent, though slightly larger in magnitude when using the five state sub-sample. For example, ERD-adherent hospitalization leads to a 3.92 pp decrease in permanent contraception (vs. -3.72 pp in the full sample).

Second, I re-estimate postpartum contraception 2SLS models using the untrimmed DD instrument. In the main analyses, I excluded 1% of observations where DD $> |100|$ miles (approximately 2.5 to 3 hours of driving time), which helps ensure the common support assumption is met. Appendix 1 Table A21 reports estimated effects using the untrimmed sample that includes DD up to a maximum of 206.8 miles. Results are qualitatively similar, indicating that the findings are robust to DD trimming decisions. For example, ERD-adherent hospitalization leads to a 3.40 pp decrease in permanent contraception (vs. -3.72 pp in the full sample, both $p < 0.01$). Notably, the first stage F-statistics are lower compared to the main trimmed sample, especially for the rural sub-population (35.23 untrimmed DD vs. 73.49 trimmed), which is not surprising given that extreme differential distances are more common in rural areas and likely include patients whose hospital choices are less responsive to relative proximity.

Third, in Appendix 1 Table A22, I re-estimate the 2SLS specification controlling for delivery type (vaginal birth vs. c-section). Results are very similar to the main specification. For example, when controlling for delivery type, patients who gave birth at an ERD-adherent hospital were 3.64 pp less likely to receive permanent contraception, compared to -3.72 pp in the main specification (both $p < 0.01$). I observe similar consistency when stratifying by contraception type and patient location. This similarity confirms that observed reductions in contraception are driven by institutional religious restrictions rather than systematic differences in obstetric management or c-section delivery rates between hospital types.

Fourth, I present 2SLS estimates with covariates selected by a machine learning Least Absolute Shrinkage

and Selection Operator (LASSO) penalized regression algorithm. This approach, further described in Appendix 3.3, identifies the most predictive covariates from a high-dimensional pool, removing researcher discretion from the covariate selection process (Belloni et al., 2014). Results from this robustness check are shown in Appendix 1 Table A23. Estimates for permanent contraception are consistent with the main specification (-3.53 pp vs. -3.72 pp). For non-permanent contraception, the LASSO estimate is somewhat larger in magnitude (-1.91 pp vs. -0.58 pp) and is statistically significant at the 5% level. This largely reflects the LASSO algorithm selecting more predictive covariates. However, I interpret this estimate cautiously, given the sensitivity of the non-permanent contraception measure as described above.

Fifth, I define treatment as giving birth at a hospital that adheres to the ERDs *or* is affiliated with a Catholic health system. Related literature uses this definition for "Catholic" hospitals (Hill et al., 2019; Solomon et al., 2020; Rodriguez et al., 2023; Menegay et al., 2022; Caldwell et al., 2022; Liu et al., 2025), but as discussed in Section 3.2, ERD-adherent hospitals and Catholic-affiliated hospitals are not perfectly analogous. Appendix 1 Table A24 presents results of Catholic-affiliation for the full sample (Column 1) and the sub-set of hospitals part of a Catholic system, but not required to adhere to the ERDs (Column 2). In the full sample, results are consistent, but slightly lower in magnitude, which is expected given the less strict treatment definition. I find no effect in the sub-sample of Catholic-affiliated, non-ERD-adherent hospitals, as expected.[38]

Last, it is worth noting that I do not present a robustness check using the two-stage residual inclusion (2SRI) control function method. While 2SRI models are sometimes employed for binary outcomes (typically by incorporating residuals from a linear first stage into a nonlinear second stage), linear 2SLS models are generally preferred when outcomes are rare, as is the case in this study. Nonlinear specifications such as probit frequently encounter separation problems (perfect prediction) when applied to rare events. In addition, previous econometric research demonstrates that linear 2SLS estimates are qualitatively identical to probit 2SRI marginal effects with rare outcomes. Moreover, the predicted probabilities generated by 2SLS rarely exceed the logical $[0, 1]$ bound when baseline rates are low, mitigating the primary practical concern motivating nonlinear specifications (Angrist and Pischke, 2009; Chiburis et al., 2012; Terza et al., 2008).

[38]Causal identification from results should be interpreted cautiously given the low first-stage F-statistic in the non-ERD-adherent sub-sample (4.51).

6 Discussion

A central question in the economics of religion literature is how religious institutions and doctrine shapes economic behavior, market outcomes, and demographic trajectories (Becker et al., 2021; Rubin, 2017; Nunn, 2010, 2014). Much of what we know comes from studying personal beliefs: religious teachings change what people want, and their choices often follow (Bassi and Rasul, 2017; McQuillan, 2004; Lehrer, 2004). Missing from this literature is the impact when religious institutions impose supply-side restrictions directly on consumers, often without their knowledge or choice. U.S. Catholic hospitals offer a natural setting to study this question, as they comprise about 15% of the national market share and follow religious directives that forbid medical care conflicting with Church teaching (e.g., contraception) (Freedman, 2023; Solomon et al., 2020; Popowitz, 2025).

In this study, I focus on the birth hospitalization, a critical point of contraception access. For many vulnerable and underserved patients, particularly those in rural areas with limited healthcare access, the birth hospitalization is often the most feasible—and sometimes the only—opportunity to receive desired contraception. I provide causal evidence that hospital adherence to the Ethical and Religious Directives creates substantial barriers to contraception with measurable downstream health consequences. In the full sample, delivering at an ERD-adherent hospital reduces the likelihood of receiving permanent contraception by 3.72 pp, a 60% decline relative to the mean. The estimated effect on non-permanent contraception is small and statistically insignificant. This pooled estimate, however, masks substantial differences based on patient location. Rural patients who delivered at an ERD-adherent hospital experienced large significant effects for both method types: 64% and 63% decrease in permanent and non-permanent contraception provision, respectively, relative to sample means. Following patients over time, rural patients who delivered at an ERD-adherent hospital were 4.18 pp more likely to be pregnant again within 18 months (a 30% increase) and 25% more likely within six months, the window with the highest risk of harm. Rural patients who delivered at an ERD-adherent hospital were also 0.31 pp more likely to experience a short-interval pregnancy ending in an SMM-complicated birth ($p < 0.01$), 0.33 pp more likely to experience one ending in preterm birth ($p < 0.10$), and had 0.07 additional births over three years on average ($p < 0.05$).

The pronounced differences between rural and urban patients underscore how market context shapes the impact of these religious restrictions. Rural patients, who demonstrate a greater baseline reliance on

hospital-based contraception, have fewer alternative providers and are less likely to attend postpartum visits (Bozkurt et al., 2024; Thayagabalu et al., 2025). For rural patients, a denial of care at the birth hospitalization is more likely to result in an unfulfilled need. Urban patients, by contrast, can more readily access a denser network of outpatient providers, which likely mitigates the impact of hospital restrictions on non-permanent contraception and subsequent pregnancy rates.

To isolate these causal effects, the identification strategy exploits quasi-random variation in travel distance, estimating a Local Average Treatment Effect for "compliers"—patients whose hospital choice is sensitive to geographic proximity. As discussed in Section 4.4, the complier population is composed of more low-income and racially diverse patients compared to the full sample, which is consistent with other evidence demonstrating less advantaged families are more affected by distance constraints (Card et al., 2023; Fontenot et al., 2024). While it is unclear whether the estimates generalize to planned deliveries or higher-SES patients, they suggest that Catholic hospital expansions may disproportionately impact vulnerable patients with fewer resources (e.g., time or money).

These findings build upon and extend related work. While previous studies document more modest declines in postpartum sterilization following Catholic hospital exposure (e.g., a 31% reduction in per-bed tubal ligation rates by Hill et al. (2019)), the instrumental variable approach yields markedly larger causal estimates: a 60% overall decrease in permanent contraception, rising to 64% for rural patients. Three differences explain the gap. First, I measure strict ERD adherence rather than Catholic affiliation, which reduces the attenuation induced by included non-ERD-adherent Catholic-affiliated hospitals (and, in a conversion design, newly acquired hospitals that have not yet adopted the directives) as treated. Second, I define the outcome more expansively, capturing all permanent contraception from both procedure and diagnosis codes at the individual birth hospitalization rather than tubal ligation procedure codes at the hospital-bed level. Finally, the IV design estimates a LATE for distance-sensitive compliers in a steady state, whereas a within-hospital conversion design recovers a more short-run, within-hospital effect.

This study provides causal evidence that giving birth at an ERD-adherent hospital reduces access to postpartum contraception and increases fertility, with effects concentrated among rural patients for whom the birth hospitalization represents a less replaceable point of access. By addressing the endogeneity of patient choice, the findings circumvent methodological challenges to demonstrate the direct impact of religious restrictions. As Catholic health systems continue to expand and demand for postpartum contraception rises post-*Dobbs*, more U.S. births will likely take place under these restrictions.

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7 Tables

Table 1: Study sample by state and year (n=10,994,881)

State	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022	2023
AR	15,178	17,336	16,960	16,665	–	33,975	32,218	–	–	–	–	–	–	–
AZ	74,868	73,731	74,852	74,200	75,361	74,453	73,473	67,792	70,114	69,337	67,101	68,374	66,581	68,897
CO	60,869	60,022	60,004	59,472	60,769	61,317	61,446	58,979	–	–	–	–	–	–
FL	196,838	199,478	199,543	201,291	206,555	209,552	209,939	208,145	206,572	204,611	195,855	197,132	203,612	206,040
KY	–	–	–	–	–	–	–	48,868	48,024	47,163	45,494	45,529	45,819	45,903
MD	–	–	–	–	–	–	–	–	62,913	59,538	60,727	59,929	60,854	57,879
NJ	100,139	98,951	98,184	96,624	97,530	97,031	96,550	93,874	93,812	92,390	89,951	93,504	94,730	92,853
NY	203,720	205,192	212,121	207,973	210,209	216,839	218,448	217,331	213,439	206,708	195,226	192,216	190,484	187,135
OR	42,905	42,133	40,758	40,651	41,875	42,167	42,158	40,908	39,764	39,311	37,113	38,126	36,778	35,694
WA	77,552	77,514	76,138	75,897	78,581	79,654	81,065	79,169	76,669	75,886	74,590	75,324	74,633	70,557
Total	772,069	774,357	778,560	772,773	770,880	814,988	815,297	815,066	811,307	794,944	766,057	770,134	773,491	764,958

Table 2: Sample Summary Statistics

	Hospital Type		
	Non-ERD-adherent	ERD-adherent	All Hospitals
Panel A. Patient characteristics			
Mean age (SD)	29 (6)	29 (6)	29 (6)
Race/ethnicity (%)			
White	48	56	49
Black	14	12	14
Hispanic	23	18	22
Asian/PI	6	6	6
Other/missing	9	8	9
Insurance type (%)			
Private	50	52	50
Medicaid	44	42	44
Self pay/other	6	6	6
Married (%)	21	18	21
Urban (%)	72	67	71
Clinical characteristics			
Alcohol/drug abuse (%)	2	3	2
Chronic pulmonary disease (%)	5	5	5
Depression (%)	3	4	3
Diabetes (%)	0.85	0.79	0.84
Hypertension (%)	0.32	0.24	0.30
Hypothyroidism (%)	4	4	4
Obesity (%)	7	8	7
Mean Elixhauser index (SD)	-0.18 (2.04)	-0.27 (2.11)	-0.20 (2.05)
Hospitalization is urgent/emergent (%)	51	41	49
Panel B. Hospital characteristics			
Ownership type (%)			
Government	14	0.21	12
Non-profit	75	96	78
For-profit	12	4	10
System member (%)	73	89	75
Presence of obstetrics unit (%)	89	90	89
Mean total annual admissions (SD)	22,387 (11,740)	19,272 (9,313)	21,917 (11,461)
Mean Medicaid patient share (SD)	0.24 (0.11)	0.23 (0.08)	0.24 (0.11)
Mean obstetric case load (SD)	0.36 (0.11)	0.38 (0.11)	0.36 (0.11)
Mean hospital concentration (SD)	0.61 (0.35)	0.59 (0.32)	0.61 (0.34)
Panel C. Market characteristics (patient county)			
Mean Catholic adherents (SD)	22 (11)	20 (14)	22 (11)
Mean OBGYN physicians (SD)	13 (8)	12 (6)	13 (8)
Mean abortion clinics (SD)	5 (5)	3 (3)	4 (4)
Total discharges	9,336,954 (84.9%)	1,657,927 (15.1%)	10,994,881 (100.0%)
Total hospitals	666 (85.9%)	109 (14.1%)	775 (100.0%)

Notes: Urban defined as large metropolitan counties using USDA Urban Influence Codes (UIC). Patient comorbidity burden measured using the van Walraven Elixhauser comorbidity index (van Walraven et al., 2009; Li et al., 2008; Quan et al., 2005). Medicaid patient share is the proportion of total annual inpatient days covered by Medicaid. Obstetric case load is the proportion of total annual admissions for obstetric care. Hospital concentration measured using the Herfindahl-Hirschman Index (HHI) with annual birth volumes per hospital service area (HSA).

Table 3: Outcome Summary Statistics

	Hospital Type		
	Non-ERD-adherent	ERD-adherent	All Hospitals
Panel A. Contraception			
Permanent contraception	6.80%	3.07%	6.24%
Non-permanent contraception	3.68%	1.37%	3.33%
Panel B. Short-interval pregnancy			
Within 6 months	3.25%	3.26%	3.25%
Within 12 months	7.98%	7.93%	7.97%
Within 18 months	12.93%	12.93%	12.93%

Notes: Panel A sample comprised of all birth-related hospitalizations (n=10,994,881). Panel B sample comprised of a sub-set of birth-related hospitalizations with unique patient identifiers to link hospitalizations by patient (n=6,842,695).

Table 4: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision

	(1)	(2)	(3)	(4)	(5)
	OLS	2SLS			
Panel A. Permanent contraception					
ERD-adherent	-0.0381***	-0.0468***	-0.0464***	-0.0387***	-0.0372***
	(0.00378)	(0.0130)	(0.0128)	(0.00941)	(0.0116)
Mean dep. var.	0.0624	0.0624	0.0624	0.0624	0.0624
Panel B. Non-permanent contraception					
ERD-adherent	-0.0224***	-0.0119*	-0.0120	-0.00482	-0.00579
	(0.00218)	(0.00663)	(0.00764)	(0.00969)	(0.00826)
Mean dep. var.	0.0333	0.0333	0.0333	0.0333	0.0333
Observations	10,994,881	10,994,881	10,994,881	10,994,881	10,994,881
First stage F-stat		48.69	72.92	84.98	97.19

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. Column (1) reports OLS estimates. Columns (2)–(5) report 2SLS estimates with varying sets of covariates: column (2) includes patient-level covariates only; column (3) includes patient- and hospital-level covariates; column (4) includes hospital- and market-level covariates; and column (5) includes patient-, hospital-, and market-level covariates.

Table 5: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision, Rural-Urban Heterogeneity, 2SLS Estimates

	(1) Rural Patients	(2) Urban Patients
Panel A. Permanent contraception		
ERD-adherent	-0.0475***	-0.0484**
	(0.0111)	(0.0196)
Mean dep. var.	0.0748	0.0574
Panel B. Non-permanent contraception		
ERD-adherent	-0.0220***	0.00682
	(0.0063)	(0.0175)
Mean dep. var.	0.0350	0.0327
Observations	3,145,629	7,849,252
First stage F-stat	73.49	29.63

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. Urban defined as large metropolitan counties using USDA Urban Influence Codes (UIC). Rural defined as non-urban.

Table 6: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision, Private-Medicaid Heterogeneity, 2SLS Estimates

	(1) Private insurance	(2) Medicaid
Panel A. Permanent contraception		
ERD-adherent	-0.0377***	-0.0366**
	(0.0096)	(0.0141)
Mean dep. var.	0.0525	0.0738
Panel B. Non-permanent contraception		
ERD-adherent	-0.0103**	-0.00341
	(0.0047)	(0.0121)
Mean dep. var.	0.0246	0.0435
Observations	5,536,587	4,838,089
First stage F-stat	85.19	88.83

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives.

Table 7: The Effect of ERD-adherent Hospitalization on Subsequent Short-Interval Pregnancy, 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. <6 months short-interval pregnancy			
ERD-adherent	0.00136	0.00954**	-0.00479
	(0.00422)	(0.00469)	(0.00754)
Mean dep. var.	0.0325	0.0387	0.0300
Panel B. <12 months short-interval pregnancy			
ERD-adherent	0.00904	0.0242**	0.00516
	(0.00828)	(0.0102)	(0.0152)
Mean dep. var.	0.0797	0.0878	0.0765
Panel C. <18 months short-interval pregnancy			
ERD-adherent	0.0163	0.0418**	0.0139
	(0.0128)	(0.0162)	(0.0238)
Mean dep. var.	0.1293	0.1375	0.1259
Observations	5,897,341	1,696,910	4,200,431
First stage F-stat	68.50	54.11	24.34

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by patient county, are in parentheses. ERD, Ethical and Religious Directives. These analyses use a sub-sample of five states (AR, FL, MD, NY, WA) with unique patient identifiers to allow for longitudinal linkage of patients from the index delivery to subsequent pregnancies. Index births in the last two years of the sample (2022-2023) were dropped to allow sufficient time for follow up.

Table 8: The Effect of ERD-adherent Hospitalization on Subsequent Short-Interval Pregnancy with SMM (incl. transfusion), 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. <6 months short-interval pregnancy with SMM			
ERD-adherent	0.000694**	0.00126***	0.000381
	(0.000282)	(0.000343)	(0.000444)
Mean dep. var.	0.0008	0.0008	0.0007
Panel B. <12 months short-interval pregnancy with SMM			
ERD-adherent	0.00131***	0.00241***	0.000928
	(0.000496)	(0.000638)	(0.000784)
Mean dep. var.	0.0016	0.0016	0.0016
Panel C. <18 months short-interval pregnancy with SMM			
ERD-adherent	0.00194***	0.00313***	0.00178
	(0.000713)	(0.000826)	(0.00122)
Mean dep. var.	0.0025	0.0024	0.0026
Observations	5,753,529	1,650,615	4,102,914
First stage F-stat	68.39	53.58	24.49

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by patient county, are in parentheses. ERD, Ethical and Religious Directives. SMM, severe maternal morbidity, defined according to the CDC definition (including blood transfusions). These analyses use a sub-sample of five states (AR, FL, MD, NY, WA) with unique patient identifiers to allow for longitudinal linkage of patients from the index delivery to subsequent pregnancies. Index births in the last two years of the sample (2022–2023) were dropped to allow sufficient time for follow up.

Table 9: The Effect of ERD-adherent Hospitalization on Subsequent Short-Interval Pregnancy with Preterm Birth, 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. <6 months short-interval pregnancy with preterm birth			
ERD-adherent	0.000180	0.00162*	-0.000909
	(0.000782)	(0.000893)	(0.00143)
Mean dep. var.	0.0036	0.0040	0.0035
Panel B. <12 months short-interval pregnancy with preterm birth			
ERD-adherent	0.000420	0.00280**	-0.000948
	(0.00122)	(0.00139)	(0.00227)
Mean dep. var.	0.0073	0.0076	0.0071
Panel C. <18 months short-interval pregnancy with preterm birth			
ERD-adherent	0.000102	0.00334*	-0.00155
	(0.00159)	(0.00202)	(0.00286)
Mean dep. var.	0.0111	0.0112	0.0110
Observations	5,753,525	1,650,610	4,102,915
First stage F-stat	68.39	53.58	24.49

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by patient county, are in parentheses. ERD, Ethical and Religious Directives. Preterm birth defined as gestation <37 weeks. These analyses use a sub-sample of five states (AR, FL, MD, NY, WA) with unique patient identifiers to allow for longitudinal linkage of patients from the index delivery to subsequent pregnancies. Index births in the last two years of the sample (2022–2023) were dropped to allow sufficient time for follow up.

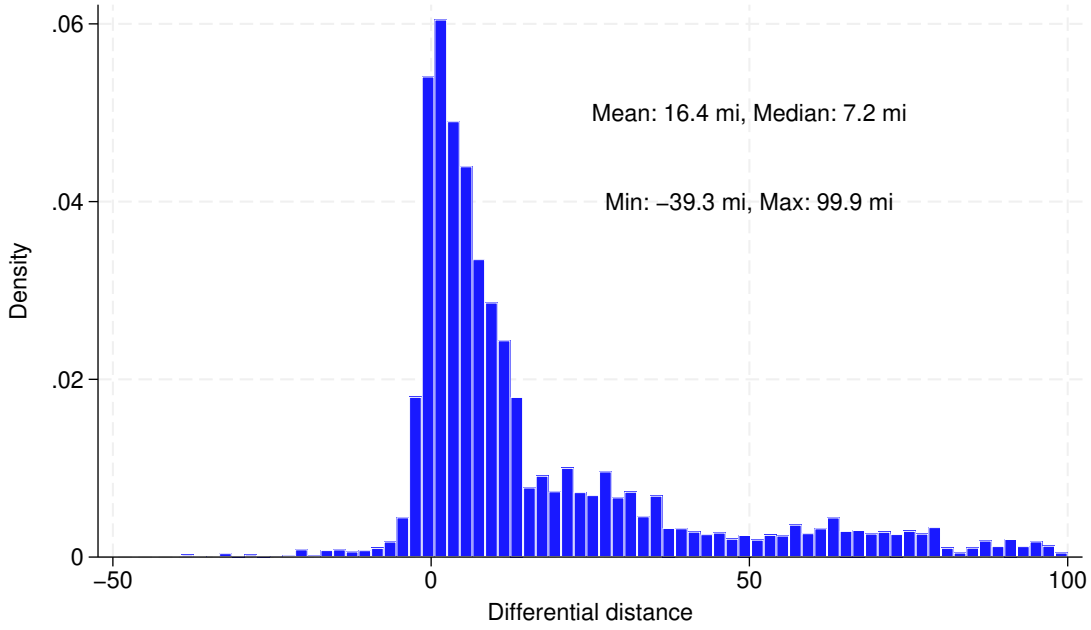
Table 10: The Effect of ERD-adherent Hospitalization on Subsequent Fertility and Switching Behavior, 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. Any subsequent birth (binary)			
ERD-adherent	0.0200	0.0597**	0.00937
	(0.0196)	(0.0248)	(0.0364)
Mean dep. var.	0.1849	0.1898	0.1830
Observations	5,354,486	1,511,548	3,842,938
First stage F-stat	66.00	51.29	24.39
Panel B. Number of subsequent births (count)			
ERD-adherent	0.0227	0.0669**	0.0112
	(0.0218)	(0.0273)	(0.0405)
Mean dep. var.	0.1935	0.1995	0.1911
Observations	5,351,999	1,509,074	3,842,925
First stage F-stat	65.90	51.17	24.39
Panel C. Switch hospital type			
ERD-adherent	0.379***	0.294***	0.441***
	(0.0297)	(0.0258)	(0.0375)
Mean dep. var.	0.0657	0.0672	0.0650
Observations	1,683,694	492,713	1,190,981
First stage F-stat	54.48	43.93	23.44

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by patient county, are in parentheses. ERD, Ethical and Religious Directives. These analyses use a sub-sample of five states (AR, FL, MD, NY, WA) with unique patient identifiers to allow for longitudinal linkage of patients from the index delivery to subsequent deliveries. Fertility outcomes use a 3 years follow up window (observed births are retained in Panel A, even if lacking the full 3 year window). Panel C is limited to patients with a subsequent birth and at least two years of follow-up so hospital type can be appropriately compared across deliveries. Hospital types are defined as ERD-adherent vs. not.

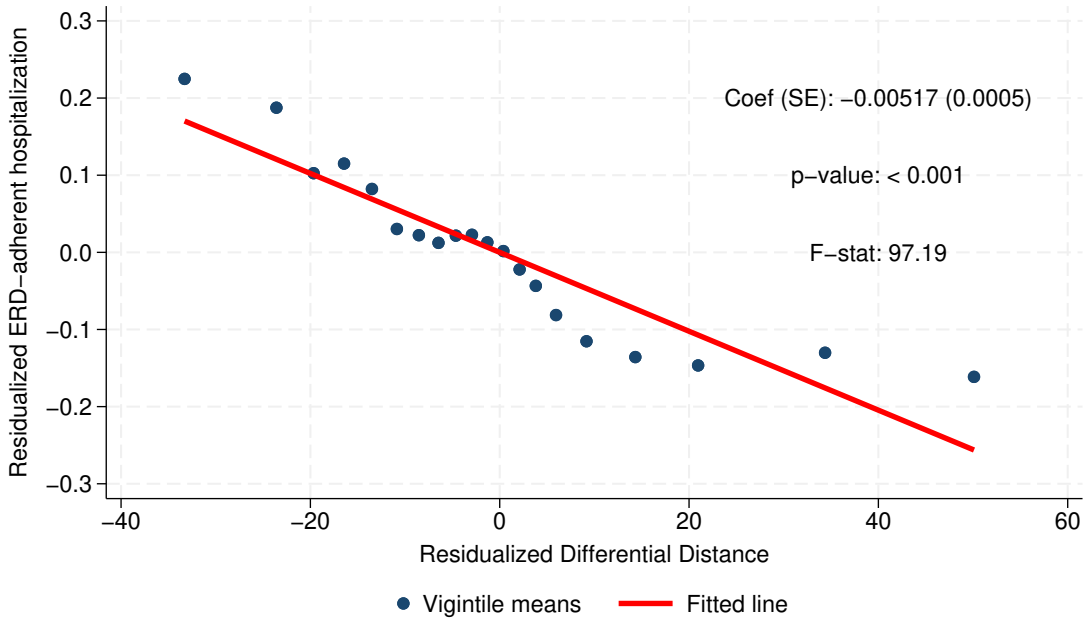
8 Figures

Figure 1: DD instrument distribution, trimmed $|\text{DD}| < 100$



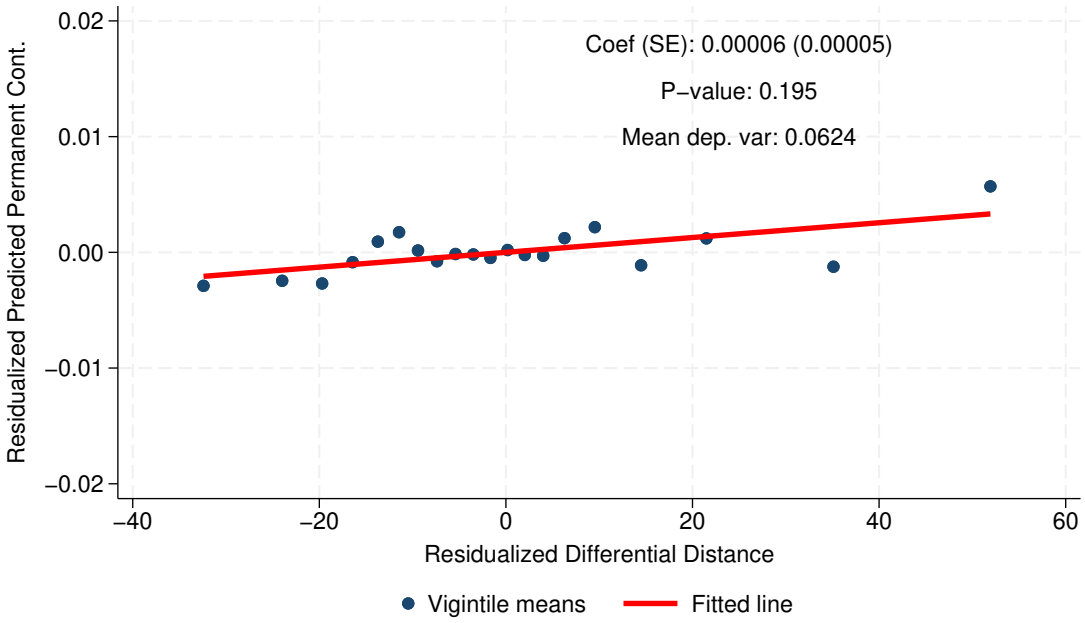
Notes: This figure shows the distribution of the differential distance (DD) instrument for the main analytic sample, which is restricted to observations where the absolute DD is less than 100 miles to ensure common support. The instrument is calculated as the distance from a patient's ZIP code centroid to the nearest ERD-adherent hospital minus the distance to the nearest non-ERD-adherent hospital. Positive values indicate the nearest hospital is non-ERD-adherent.

Figure 2: First Stage Relationship: Differential Distance and ERD-adherent Hospitalization

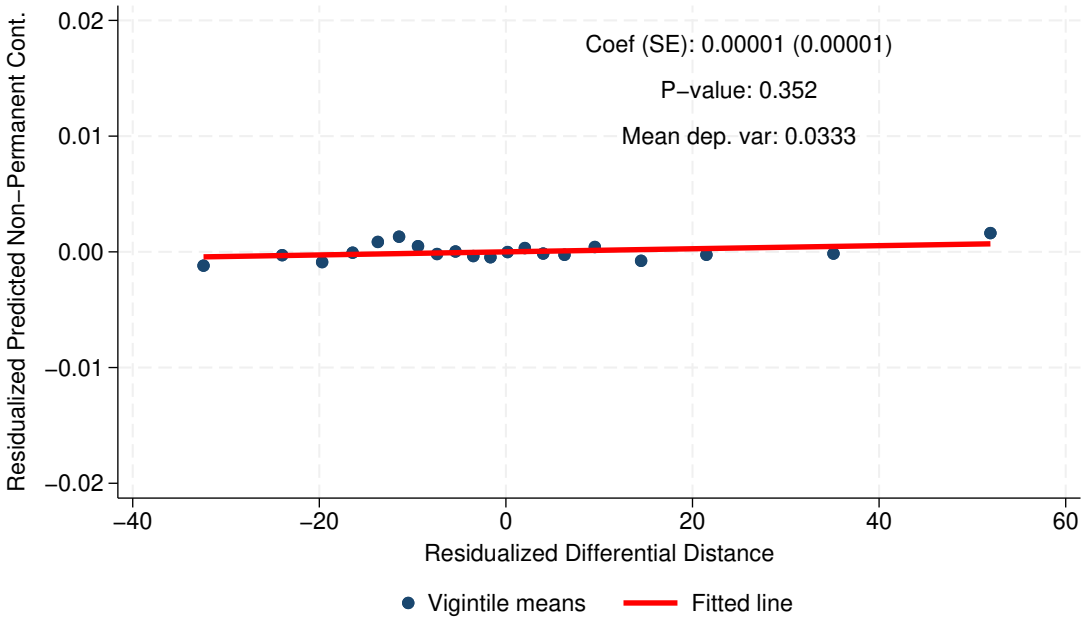


Notes: This figure presents a binned scatterplot illustrating the first-stage relationship between the differential distance (DD) instrument and admission to an ERD-adherent hospital. Both the treatment variable (admission to an ERD-adherent hospital) and the differential distance instrument are residualized with respect to the full set of exogenous controls. Each point represents the mean of the residualized treatment variable for each vigintile (20 equal-sized bins) of the residualized instrument (n=10,994,881).

Figure 3: Balance Tests



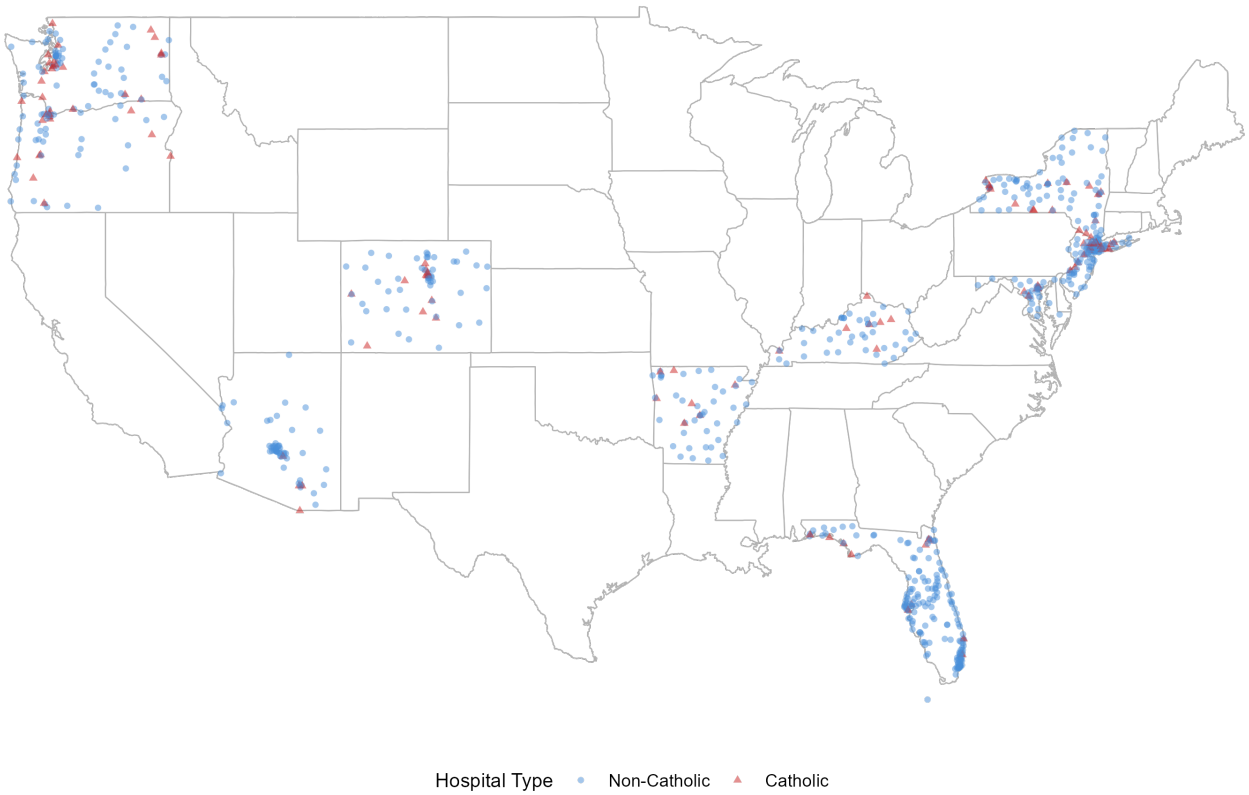
(a) Permanent Contraception



(b) Non-Permanent Contraception

Notes: This figure presents binned scatterplots for our balance tests, which provide empirical support for the instrument independence assumption. The y-axis plots a residualized "risk score," the outcome predicted from patient-level covariates only, weighting each characteristic by its association with contraception use. The x-axis plots the residualized differential distance (DD) instrument. Both variables are residualized on the hospital and market controls and state-by-year fixed effects from our main specification; the patient covariates enter only the risk score and are never included as controls in the balance regression. Points are vigintile means; the fitted line is the individual-level regression, with standard errors clustered at patient county of residence. See Appendix 3 for additional methodological details on the risk score.

Figure 4: Geographic Distribution of ERD-adherent Hospitals in Study Sample



Notes: This figure shows the geographic distribution of hospitals in our study sample by ERD status (i.e., Catholic vs. not), determined by the first year each hospital appears in our study sample. Catholic, ERD-adherent hospitals (red triangles) are those that adhere to the Ethical and Religious Directives for Catholic Health Care Services, which prohibit the provision of contraception and sterilization services. Non-ERD-adherent hospitals are shown in blue circles.

Appendix

1 Additional Tables and Figures

Table A1: State Legislation Related to Reproductive Healthcare in Catholic Hospitals

State	Name	Description	Status
New York	Hospital Transparency Act, Senate Bill S1003A / Assembly Bill A733 (S3486/A3862)	Requires hospitals to fill out a form regarding which reproductive health services they provide and post on both their website and the Department of Health webpage	Passed both chambers twice; vetoed by Gov. Hochul in 2024 (S1003A/A733) and 2025 (S3486/A3862)
Washington	Keep Our Care Act, Senate Bill 5704 / House Bill 1881	Increases government oversight of healthcare mergers/acquisitions to ensure access to comprehensive services, including reproductive healthcare	Passed the Senate but not the House in 2024, re-introduced in 2026 and awaiting action
Connecticut	House Bill 5516	Protects clinicians who work in Catholic institutions and provide referrals or counseling on reproductive healthcare	Proposed in 2026
New Mexico	Health Care Consolidation Oversight Act, House Bill 586	Increased government oversight of healthcare mergers/acquisitions to ensure access to comprehensive services, including reproductive healthcare	Made law in 2025
New Jersey	Senate Bill 3275 / Assembly 4829	Launched state information portal with increased transparency on patient rights and reproductive healthcare options	Made law in 2023
Colorado	Patients' Right to Know Act, House Bill 23-1218	Requires every hospital/health facility to publicly disclose services (including reproductive health care) they do not provide for non-medical reasons, such as religious directives	Made law in 2023
California	Health Care Quality and Affordability Act, Senate Bill 184	Established the Office of Health Care Affordability (OHCA) with greater oversight to evaluate M/A transactions on competition, cost, and access	Made law in 2022
Oregon	House Bill 2362	Increased government oversight of healthcare mergers/acquisitions to ensure access to comprehensive services, including reproductive healthcare	Made law in 2021
Washington	Protecting Pregnancy and Miscarriage-Related Care, Senate Bill 5140	Prohibits hospitals from interfering with providers' ability to manage pregnancy complications where the patient's health or life is at risk	Made law in 2021
Washington	Senate Bill 5602	Requires hospitals to fill out a form regarding which reproductive health services they provide and post on both their website and the Department of Health webpage	Made law in 2019

Table A2: Sample of Birth Hospitalizations with Unique Patient Identifiers (n=6,842,695)

State	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022	2023
AR	14,367	16,218	15,860	15,662	–	31,881	30,050	–	–	–	–	–	–	–
FL	179,255	182,913	182,692	183,130	186,559	186,134	184,671	181,802	178,769	174,230	164,826	162,317	159,570	158,577
MD	–	–	–	–	–	–	–	–	62,905	59,536	60,722	59,924	60,854	57,879
NY	202,285	203,753	210,649	206,766	208,770	215,086	216,587	214,343	204,929	197,727	188,139	185,891	183,788	179,496
WA	77,552	77,514	76,138	75,897	78,581	79,654	81,065	79,169	76,669	75,886	74,590	75,279	74,633	70,557
Total	473,459	480,398	485,339	481,455	473,910	512,755	512,373	475,314	523,272	507,379	488,277	483,411	478,845	466,509

Table A3: Characteristics of Catholic-affiliated hospitals by ERD status

	Non-ERD-adherent	**ERD-adherent**
% of hospitals	11.0	89.0
For-profit ownership (%)	0.7	2.7
Admissions	13,443	12,123
Medicaid payer mix	0.21	0.23
OB caseload	0.28	0.33
OB unit (%)	79.3	80.2
HHI concentration	0.70	0.69

Notes: ERD, Ethical and Religious Directives. Reported values are sub-sample means (except for OB unit, %) from Catholic-affiliated hospitals in our study sample. Data collapsed to the hospital-year level.

Table A4: ICD, CPT, and DRG codes for identifying birth hospitalizations

ICD-9 DX	650, 65100, 65101, 65110, 65111, 65120, 65121, 65170, 65171, 65173, 65180, 65181, 65190, 65191, V240, V241, V242, V270, V271, V272, V724, V7242, V9100, V9101, V9102, V9103, V9109, V9110, V9111, V9112, V9119, V9120, V9121, V9122, V9129, V9190, V9191, V9192, V9199, 64000, 64001, 64080, 64081, 64100, 64101, 64110, 64111, 64130, 64131, 64200, 64201, 64300, 64301, 64420, 64421, 64440, 64441, 64511, 65421, 66970, 66971, 65640, 65641, 65643, V273, V274, V276, V277, 64410, 64411
ICD-9 PR	740, 741, 742, 744, 7499
ICD-9 CCS	DX single: 196; PR single: 134; PR multiple: 13.02
ICD-10 DX	O80, O81, O82, O83, O84, O6020X0, O6020X1, O6020X2, O6020X3, O6020X4, O6020X5, O6020X9, O6022X0, O6022X1, O6022X2, O6022X3, O6022X4, O6022X5, O6022X9, O6023X0, O6023X1, O6023X2, O6023X3, O6023X4, O6023X5, O6023X9, Z370, Z371, Z372, Z373, Z374, Z377, Z3750, Z3751, Z3752, Z3753, Z3754, Z3759, Z3760, Z3761, Z3762, Z3763, Z3764, Z3769, Z3800, Z3801, Z381, Z382, Z3830, Z3831, Z384, Z385, Z3861, Z3862, Z3863, Z3864, Z3865, Z3866, Z3868, Z3869, Z387, Z388, Z390, O4202, O4212, O4292, O6010X0, O6010X1, O6010X2, O6010X3, O6010X4, O6010X5, O6010X9, O6012X0, O6012X1, O6012X2, O6012X3, O6012X4, O6012X5, O6012X9, O6013X0, O6013X1, O6013X2, O6013X3, O6013X4, O6013X5, O6013X9, O6014X0, O6014X1, O6014X2, O6014X3, O6014X4, O6014X5, O6014X9, O601, O6010, O6012, O6013, O6014, O364, O364XX0, O364XX1, O364XX2, O364XX3, O364XX4, O364XX5, O364XX9, P95
ICD-10 PR	10D07Z3, 10D07Z4, 10D07Z5, 10D07Z6, 10D07Z7, 10D07Z8, 10E0XZZ, 10D00Z0, 10D00Z1, 10D00Z2
CPT	59409, 59410, 59612, 59614, 59514, 59515, 59620, 59622
DRG	798, 797, 796, 807, 806, 805, 775, 774, 768, 767, 788, 787, 786, 785, 784, 783, 766, 765

Notes: This table presents the diagnosis (DX) and procedure (PR) codes used to identify birth hospitalizations (including normal delivery, preterm birth, cesarean section, and stillbirth). International Classification of Diseases, Ninth Revision (ICD-9) codes apply to admissions before Q4 2015, while ICD-10 codes apply to admissions from Q4 2015 onward. ICD Clinical Classifications Software (CCS) codes include both single-level and multi-level classifications. Current Procedural Terminology (CPT) and Diagnosis-Related Group (DRG) codes have no time restriction.

Table A5: ICD, CPT, and DRG codes to identify permanent and non-permanent contraception

Panel A. Permanent contraception (sterilization)	
ICD-9 DX	V252, V2651, V2652
ICD-9 PR	6621, 6622, 6629, 6631, 6632, 6639, 683, 6831, 6839, 684, 6841, 6849, 685, 6851, 6859, 686, 6861, 6869, 687, 6871, 6879, 689
ICD-9 CCS	DX multiple: 11.01.01; PR single: 121, 124; PR multiple: 12.03, 12.05
ICD-10 DX	Z302
ICD-10 PR	0U570ZZ, 0U573ZZ, 0U574ZZ, 0U577ZZ, 0U578ZZ, 0UB70ZZ, 0UB73ZZ, 0UB74ZZ, 0UB77ZZ, 0UB78ZZ, 0UL70CZ, 0UL70DZ, 0UL70ZZ, 0UL73CZ, 0UL73DZ, 0UL73ZZ, 0UL74CZ, 0UL74DZ, 0UL74ZZ, 0UL77DZ, 0UL77ZZ, 0UL78DZ, 0UL78ZZ, 0UT70ZZ, 0UT74ZZ, 0UT77ZZ, 0UT78ZZ, 0UT7FZZ, 0UT90ZL, 0UT90ZZ, 0UT94ZL, 0UT94ZZ, 0UT97ZL, 0UT97ZZ, 0UT98ZL, 0UT98ZZ, 0UT9FZL, 0UT9FZZ, 0UT20ZZ, 0UT24ZZ, 0UT27ZZ, 0UT28ZZ, 0UT2FZZ
CPT	0567T, 58600, 58605, 58611, 58615, 58670, 58671, 51925, 58150, 58152, 58180, 58200, 58210, 58240, 58260, 58262, 58263, 58267, 58270, 58275, 58280, 58285, 58290, 58291, 58292, 58294, 58541, 58542, 58543, 58544, 58548, 58550, 58552, 58553, 58554, 58570, 58571, 58572, 58573, 58575, 58951, 58953, 58954, 58956, 59525
DRG	783, 784, 785, 796, 797, 798, 734, 735
Panel B. Non-permanent contraception	
ICD-9 DX	V250, V2501, V2502, V2503, V2504, V2509, V2511, V2513, V251, V2540, V2541, V2542, V2543, V2549, V255, V258, V259
ICD-9 PR	697
ICD-9 CCS	DX single: 176; DX multiple: 11.01
ICD-10 DX	Z30014, Z30430, Z30431, Z30433, Z30017, Z3046, Z30011, Z30019, Z30013, Z30018, Z30016, Z30015, Z30012, Z308, Z3009
ICD-10 PR	0UH90HZ, 0UH97HZ, 0UH98HZ, 0UHC7HZ, 0UHC8HZ, 0JH60HZ, 0JH63HZ, 0JH80HZ, 0JH83HZ, 0JHD0HZ, 0JHD3HZ, 0JHF0HZ, 0JHF3HZ, 0JHG0HZ, 0JHG3HZ, 0JHH0HZ, 0JHH3HZ, 0JHL0HZ, 0JHL3HZ, 0JHM0HZ, 0JHM3HZ, 0JHN0HZ, 0JHN3HZ, 0JHP0HZ, 0JHP3HZ, 0U2DXHZ
CPT	J7296, J7297, J7298, J7299, J7300, J7301, J7302, J7303, J7304, J7305, J7306, J7307, S4981, S4982, S4983, S4984, S4985, S4986, S4987, S4988, S4989, 11981, 11983, 58300

Notes: This table presents the diagnosis (DX) and procedure (PR) codes used to identify contraception provided during hospital admissions. Panel A includes codes for permanent contraception (i.e., sterilization), including tubal ligation, hysterectomy, and bilateral oophorectomy. Panel B includes codes for non-permanent contraception, including intrauterine devices (IUDs), hormonal implants, shots, prescriptions, and nonspecified contraceptive care. International Classification of Diseases, Ninth Revision (ICD-9) codes apply to admissions before Q4 2015, while ICD-10 codes apply to admissions from Q4 2015 onward. ICD Clinical Classifications Software (CCS) codes include both single-level and multi-level classifications. Current Procedural Terminology (CPT) and Diagnosis-Related Group (DRG) codes have no time restriction.

Table A6: Rates of Contraception Provision by Delivery Method

	(1) Non-ERD-adherent	(2) ERD-adherent	(3) Full sample
Panel A. Permanent contraception			
Vaginal birth (%)	16.7	14.9	16.5
C-section (%)	83.3	85.1	83.5
Panel B. Non-permanent contraception			
Vaginal birth (%)	28.2	16.0	27.4
C-section (%)	71.8	84.0	72.6

Notes: ERD, Ethical and Religious Directives. C-section refers to a birth by cesarean section.

Table A7: Descriptive Statistics: Distance and closest hospital by patient location

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. Distance to birth hospital			
Mean distance, miles (SD)	9.9 (14.4)	14.3 (20.2)	8.1 (10.9)
Panel B. Type of closest hospital			
ERD-adherent closest to patient (%)	12.9	15.2	12.0
ERD-adherent closest & patient went there (%)	35.9	43.2	32.2
Observations	10,994,881	3,145,629	7,849,252

Notes: Urban defined as large metropolitan counties using USDA Urban Influence Codes (UIC). Rural defined as non-urban.

Table A8: Characteristics of Compliers, Always-Takers, and Never-Takers

	(1) Compliers	(2) Always-Takers	(3) Never-Takers
Age (mean)	29	29	29
White (%)	47	54	50
Black (%)	9	13	16
Hispanic (%)	27	18	20
Private insurance (%)	48	55	51
Medicaid (%)	47	39	43
Urban (%)	72	70	71
Married (%)	20	20	21
Urgent visit (%)	66	41	42
Elixhauser score (mean)	-0.07	-0.24	-0.26

Note: This table reports average characteristics for compliers, always-takers, and never-takers. Compliers are patients whose hospital choice responds to differential distance. "Always-takers" deliver at ERD-adherent hospitals regardless of proximity. "Never-takers" deliver at non-ERD-adherent hospitals regardless of proximity. Binary variables reported as percentages. n = 10,994,881.

Table A9: Negative control outcome (NCO) tests

	Coefficient
Panel A. Main dataset	
Marital status	-0.50
	(0.66)
Catholic adherents per capita	-0.23
	(0.15)
Asthma	-0.59
	(0.43)
Prolonged pregnancy	0.06
	(0.38)
Panel B. Dataset with unique patient identifiers	
Parity	-0.23
	(0.26)
Age at first birth	-0.0012
	(0.06)

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by patient county, are in parentheses. This table reports coefficients from OLS regressions of differential distance on six negative control outcomes, NCOs (six separate models). All regressions include state-by-year fixed effects and the full vector of patient, hospital, and market-level controls (excluding the NCO per each model). Catholic adherents measured per 1,000 population per patient county. Prolonged pregnancy is defined as delivery after 42 weeks. Parity and Age at first birth are calculated based on the patient's history observed within the study window (2010–2023). Panel A, n = 10,994,881. Panel B, n = 6,842,695.

Table A11: Reduced-Form Effect of Instrument on Postpartum Contraception Provision

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. Permanent contraception			
Differential distance	0.000192***	0.000213***	0.000233***
	(0.0000569)	(0.0000518)	(0.0000737)
Y-mean	0.0624	0.0748	0.0574
Panel B. Non-permanent contraception			
Differential distance	0.0000300	0.0000988***	-0.0000328
	(0.0000434)	(0.0000298)	(0.0000838)
Y-mean	0.0333	0.0350	0.0327
Observations	10,994,881	3,145,629	7,849,252

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. Urban defined as large metropolitan counties using USDA Urban Influence Codes (UIC). Rural defined as non-urban.

Table A10: Balance Tests of Hospital Organizational Characteristics

	(1) Full Sample	(2) Non-ERD-adherent	(3) ERD-adherent
For-profit ownership			
Differential distance	-0.002***	-0.003***	0.0002
	(0.001)	(0.001)	(0.0006)
ERD-adherent hospital	-0.105***		
	(0.023)		
Y-Mean	0.104	0.116	0.035
System member			
Differential distance	0.001	0.001	0.0005
	(0.001)	(0.001)	(0.0004)
ERD-adherent hospital	0.180***		
	(0.055)		
Y-Mean	0.752	0.728	0.887
Admissions			
Differential distance	51.9*	56.2*	132.8***
	(29.2)	(30.9)	(39.6)
ERD-adherent hospital	-595.8		
	(1324.5)		
Y-Mean	21,917	22,387	19,272
Medicaid payer mix			
Differential distance	-0.0001	-0.0002	0.001
	(0.0002)	(0.0002)	(0.003)
ERD-adherent hospital	-0.003		
	(0.01)		
Y-Mean	0.241	0.243	0.234
OB caseload			
Differential distance	-0.0004	-0.00005	-0.003
	(0.0002)	(-0.0002)	(0.005)
ERD-adherent hospital	0.018		
	(0.014)		
Y-Mean	0.360	0.356	0.383
OB unit			
Differential distance	0.001***	0.001***	0.001
	(0.0004)	(0.0004)	(0.001)
ERD-adherent hospital	0.004		
	(0.026)		
Y-Mean	0.890	0.889	0.899
Sample observations	10,994,881	9,336,954	1,657,927

Notes: Each column reports a separate OLS regression of a hospital organizational characteristic on differential distance. Column 1 reports estimates across the full sample controlling for hospital ERD status. Columns 2 and 3 are sub-groups of non-ERD-adherent and ERD-adherent hospitalizations, respectively. Models include patient and market-level controls, state-by-year fixed effects, cluster standard errors by patient county. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision (using alternative rural-urban definitions), 2SLS Estimates

	OMB definition		Concentration (HHI)	
	Rural	Urban	High, rural	Low, urban
	(1)	(2)	(3)	(4)
Panel A. Permanent contraception				
ERD-adherent	-0.0524***	-0.0377***	-0.0515***	-0.0644***
	(0.0118)	(0.0125)	(0.00913)	(0.0250)
Y-mean	0.0846	0.0609	0.0685	0.0585
Panel B. Non-permanent contraception				
ERD-adherent	-0.0210***	-0.00215	-0.0194***	0.00207
	(0.00664)	(0.0105)	(0.00459)	(0.0231)
Y-mean	0.0386	0.0330	0.0306	0.0350
Observations	694,740	10,300,141	4,281,805	6,713,076
First stage F-stat	90.70	66.53	82.66	35.85

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. Our main model defines urban as a large metropolitan county using USDA Urban Influence Codes (UIC). Here we present results from two other definitions of urban: (i) metropolitan areas from the Office and Management and Budget (OMB) Core-Based Statistical Areas (CBSAs), and (ii) counties with below average provider concentration. Provider concentration was defined using the Herfindahl-Hirschman Index with annual birth volumes at hospitals located in patient county of residence. In all models, rural is defined as non-urban.

Table A13: The Effect of ERD-adherent Hospitalization on Postpartum Contraception (examining *Dobbs'* impact), 2SLS IV estimates

	(1) Pre-*Dobbs*	(2) Post-*Dobbs*	(3) Interaction	(4) Interaction (protective states)	(5) Interaction (hostile states)
Panel A. Permanent contraception					
ERD-adherent	-0.0349***	-0.0279***			
	(0.0127)	(0.0103)			
ERD-adherent (pre-*Dobbs*)			-0.0354***	-0.0336***	-0.0547***
			(0.0130)	(0.0119)	(0.0211)
ERD-adherent × post-*Dobbs* interaction			0.0165	0.0169*	0.0333
			(0.0122)	(0.00889)	(0.0240)
Y-mean	0.0630	0.0579	0.0624	0.0534	0.0758
Panel B. Non-permanent contraception					
ERD-adherent	-0.00537	0.0108			
	(0.00844)	(0.0131)			
ERD-adherent (pre-*Dobbs*)			-0.00796	-0.0120	-0.0259**
			(0.00870)	(0.00754)	(0.0104)
ERD-adherent × post-*Dobbs* interaction			0.0496***	0.0555**	0.0530***
			(0.0141)	(0.0217)	(0.0196)
Y-mean	0.0346	0.0156	0.0324	0.0318	0.0334
Observations	9,026,279	1,166,515	10,192,794	6,095,496	4,097,298
First stage F-stat	84.52	72.66	44.87	44.60	22.12

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. To evaluate whether the causal impact of ERD adherence evolved following the June 2022 *Dobbs* decision, we estimated two complementary temporal specifications. First, we conducted split-sample instrumental variable regressions, stratifying the sample into pre-*Dobbs* (2010 to Q1 2022, Column 1) and post-*Dobbs* (Q3 2022 to 2023, Column 2). We also estimated pooled interaction models in Columns (3)-(5). ERD adherence and its interaction with the post-*Dobbs* indicator are both endogenous; they are instrumented by differential distance and its interaction with the post-*Dobbs* indicator (DD × post-*Dobbs*). Because the interaction models contain two endogenous regressors, we report the Sanderson–Windmeijer first-stage F. States in our sample protective of abortion rights in the immediate post-*Dobbs* period (2022–2023) include MD, NJ, NY, OR, and WA; hostile states include AZ, FL, and KY. All models include the full set of patient-, hospital-, and market-level controls and state-by-year fixed effects. Q2 2022 was dropped for these analyses. We also dropped two states missing 2023 data to ensure consistency in sample composition (AR and CO).

Table A14: The Effect of ERD-adherent Hospitalization on Index Birth Outcomes, 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. SMM (incl. transfusion)			
ERD-adherent	0.00548	-0.000762	0.0144
	(0.00459)	(0.00353)	(0.00955)
Y-mean	0.0199	0.0171	0.0210
Panel B. SMM (excl. transfusion)			
ERD-adherent	0.00327	-0.000461	0.00946
	(0.00380)	(0.00257)	(0.00843)
Y-mean	0.0150	0.0126	0.0160
Panel C. Preterm birth			
ERD-adherent	0.00603	-0.0146	0.0314**
	(0.00951)	(0.0123)	(0.0145)
Y-mean	0.0926	0.0848	0.0957
Panel D. Complications			
ERD-adherent	0.00799	-0.00307	0.0176*
	(0.00626)	(0.00657)	(0.00932)
Y-mean	0.0550	0.0510	0.0566
Observations	10,994,881	3,145,629	7,849,252
First stage F-stat	97.19	73.49	29.63

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. SMM, severe maternal morbidity. Complications include pelvic infection, obstetric sepsis, hemorrhage, embolism, and transfusion, which previous research has found likely to be impacted by the presence of Catholic ERDs (Freedman, 2023).

Table A15: The Effect of ERD-adherent Hospitalization on Short-Interval Pregnancy (using alternative rural-urban definitions), 2SLS Estimates

	OMB definition		Concentration (HHI)	
	Rural	Urban	High, rural	Low, urban
	(1)	(2)	(3)	(4)
Panel A. <6 months short-interval pregnancy				
ERD-adherent	0.00818	-0.000458	0.00594	-0.0133
	(0.00684)	(0.00472)	(0.00415)	(0.00858)
Y-mean	0.0393	0.0321	0.0362	0.0302
Panel B. <12 months short-interval pregnancy				
ERD-adherent	0.0262*	0.00766	0.0237***	-0.0198
	(0.0157)	(0.00944)	(0.00878)	(0.0185)
Y-mean	0.0871	0.0793	0.0859	0.0759
Panel C. <18 months short-interval pregnancy				
ERD-adherent	0.0443*	0.0158	0.0424***	-0.0181
	(0.0264)	(0.0150)	(0.0156)	(0.0296)
Y-mean	0.1339	0.1290	0.1376	0.1240
Observations	338,820	5,558,521	2,270,344	3,626,997
First stage F-stat	26.35	45.43	61.70	27.15

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. Our main model defines urban as a large metropolitan county using USDA Urban Influence Codes (UIC). Here we present results from two other definitions of urban: (i) metropolitan areas from the Office and Management and Budget (OMB) Core-Based Statistical Areas (CBSAs), and (ii) counties with below average provider concentration. Provider concentration was defined using the Herfindahl-Hirschman Index with annual birth volumes at hospitals located in patient county of residence. In all models, rural is defined as non-urban.

Table A16: The Effect of ERD-adherent Hospitalization on Subsequent Short-Interval Pregnancy with SMM (excl. transfusion), 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. <6 months short-interval pregnancy with SMM			
ERD-adherent	0.000351	0.000852***	0.0000949
	(0.000225)	(0.000232)	(0.000386)
Mean dep. var.	0.0005	0.0006	0.0005
Panel B. <12 months short-interval pregnancy with SMM			
ERD-adherent	0.000714*	0.00176***	0.000353
	(0.000411)	(0.000447)	(0.000731)
Mean dep. var.	0.0012	0.0012	0.0012
Panel C. <18 months short-interval pregnancy with SMM			
ERD-adherent	0.00109*	0.00221***	0.000902
	(0.000610)	(0.000576)	(0.00115)
Mean dep. var.	0.0019	0.0018	0.0019
Observations	5,753,529	1,650,615	4,102,914
First stage F-stat	68.39	53.58	24.49

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by patient county, are in parentheses. ERD, Ethical and Religious Directives. SMM, severe maternal morbidity, defined according to the CDC definition (excluding blood transfusions). These analyses use a sub-sample of five states (AR, FL, MD, NY, WA) with unique patient identifiers to allow for longitudinal linkage of patients from the index delivery to subsequent pregnancies. Index births in the last two years of the sample (2022–2023) were dropped to allow sufficient time for follow up.

Table A17: The Effect of ERD-adherent Hospitalization on Subsequent SMM (incl. transfusion), Conditional on Short-Interval Pregnancy, 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. SMM conditional on <6 months short-interval pregnancy			
ERD-adherent	0.0165**	0.0254***	0.0113
	(0.00739)	(0.00592)	(0.0128)
Mean dep. var.	0.0230	0.0215	0.0237
Panel B. SMM conditional on <12 months short-interval pregnancy			
ERD-adherent	0.0122*	0.0207***	0.00931
	(0.00642)	(0.00554)	(0.0109)
Mean dep. var.	0.0203	0.0190	0.0208
Panel C. SMM conditional on <18 months short-interval pregnancy			
ERD-adherent	0.0109*	0.0156***	0.0106
	(0.00599)	(0.00453)	(0.0105)
Mean dep. var.	0.0192	0.0177	0.0198
Observations	757,002	222,859	534,143
First stage F-stat	57.16	47.28	24.13

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by patient county, are in parentheses. ERD, Ethical and Religious Directives. SMM, severe maternal morbidity, defined according to the CDC definition (including blood transfusions). These analyses use a sub-sample of five states (AR, FL, MD, NY, WA) with unique patient identifiers to allow for longitudinal linkage of patients from the index delivery to subsequent pregnancies. Index births in the last two years of the sample (2022–2023) were dropped to allow sufficient time for follow up. Models analyze likelihood of SMM at subsequent delivery, conditional on short-interval pregnancy; analyses are exploratory as risk of SIP may be treatment-affected.

Table A18: The Effect of ERD-adherent Hospitalization on Subsequent Preterm Birth, Conditional on Short-Interval Pregnancy, 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. Preterm birth, conditional on <6 months short-interval pregnancy			
ERD-adherent	-0.000683	0.0125	-0.00562
	(0.00896)	(0.0157)	(0.0153)
Mean dep. var.	0.1082	0.1043	0.1102
Panel B. Preterm birth, conditional on <12 months short-interval pregnancy			
ERD-adherent	-0.00389	0.00453	-0.00872
	(0.00767)	(0.0105)	(0.0143)
Mean dep. var.	0.0901	0.0887	0.0907
Panel C. Preterm birth, conditional on <18 months short-interval pregnancy			
ERD-adherent	-0.00811	-0.00303	-0.0114
	(0.00714)	(0.0102)	(0.0130)
Mean dep. var.	0.0842	0.0826	0.0848
Observations	756,998	222,854	534,144
First stage F-stat	57.15	47.29	24.13

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by patient county, are in parentheses. ERD, Ethical and Religious Directives. Preterm birth defined as gestation <37 weeks. These analyses use a sub-sample of five states (AR, FL, MD, NY, WA) with unique patient identifiers to allow for longitudinal linkage of patients from the index delivery to subsequent pregnancies. Index births in the last two years of the sample (2022–2023) were dropped to allow sufficient time for follow up. Models analyze likelihood of preterm birth at subsequent delivery, conditional on short-interval pregnancy; analyses are exploratory as risk of SIP may be treatment-affected.

Table A19: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision (using sub-sample with unique patient identifiers), OLS and 2SLS estimates

	(1)	(2)	(3)	(4)	(5)
	OLS	2SLS			
Panel A. Permanent contraception					
ERD-adherent	-0.0348***	-0.0474***	-0.0490***	-0.0430***	-0.0392***
	(0.00565)	(0.0172)	(0.0163)	(0.0127)	(0.0148)
Mean dep. var.	0.0620	0.0620	0.0620	0.0620	0.0620
Panel B. Non-permanent contraception					
ERD-adherent	-0.0215***	-0.00959	-0.00807	-0.00202	-0.00103
	(0.00304)	(0.00917)	(0.00922)	(0.0133)	(0.0102)
Mean dep. var.	0.0345	0.0345	0.0345	0.0345	0.0345
Observations	6,842,695	6,842,695	6,842,695	6,842,695	6,842,695
First stage F-stat		31.74	55.43	52.75	67.70

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. Column (1) reports OLS estimates. Columns (2)–(5) report 2SLS estimates with varying sets of covariates: column (2) includes patient-level covariates only; column (3) includes patient- and hospital-level covariates; column (4) includes hospital- and market-level covariates; and column (5) includes patient-, hospital-, and market-level covariates.

Table A20: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision (using sub-sample with unique patient identifiers), by patient location

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. Permanent contraception			
ERD-adherent	-0.0392***	-0.0670***	-0.0559**
	(0.0148)	(0.0142)	(0.0266)
Y-mean	0.0620	0.0750	0.0568
Panel B. Non-permanent contraception			
ERD-adherent	-0.00103	-0.0310***	0.0201
	(0.0102)	(0.00833)	(0.0220)
Y-mean	0.0345	0.0368	0.0336
Observations	6,842,695	1,945,297	4,897,398
First stage F-stat	67.70	53.41	24.08

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. DD, differential distance. Models include the full set of patient-, hospital-, and market-level controls and state-by-year fixed effects.

Table A21: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision (using sample with untrimmed DD), 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. Permanent contraception			
ERD-adherent	-0.0340***	-0.0468***	-0.0484**
	(0.0121)	(0.0124)	(0.0196)
Y-mean	0.0625	0.0747	0.0574
Panel B. Non-permanent contraception			
ERD-adherent	-0.00698	-0.0201***	0.00682
	(0.00765)	(0.00704)	(0.0175)
Y-mean	0.0334	0.0350	0.0327
Observations	11,150,279	3,301,023	7,849,256
First stage F-stat	76.99	35.23	29.63

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. DD, differential distance.

Table A22: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision (controlling for delivery type), 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. Permanent contraception			
ERD-adherent	-0.0364***	-0.0404***	-0.0453**
	(0.0113)	(0.0111)	(0.0199)
Y-mean	0.0624	0.0748	0.0574
Panel B. Non-permanent contraception			
ERD-adherent	-0.00547	-0.0191***	0.00813
	(0.00837)	(0.00638)	(0.0176)
Y-mean	0.0333	0.0350	0.0327
Observations	10,994,881	3,145,629	7,849,252
First stage F-stat	97.20	73.52	29.62

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. In this robustness check, we add a control for delivery type (vaginal vs. c-section).

Table A23: The Effect of ERD-adherent Hospitalization on Postpartum Contraception (covariates selected by machine learning LASSO algorithm), 2SLS estimates

	2SLS
Panel A. Permanent contraception	
ERD-adherent	-0.0353***
	(0.0103)
Y-mean	0.0624
Panel B. Non-permanent contraception	
ERD-adherent	-0.0191**
	(0.0076)
Y-mean	0.0333
Panel C. Model specification	
Observations	10,994,881
Clusters (patient county)	566
Total controls considered	183
Forced controls	11
State × year fixed effects	111
LASSO candidate pool, penalized	61
Selected, permanent contraception	32
Selected, non-permanent contraception	27

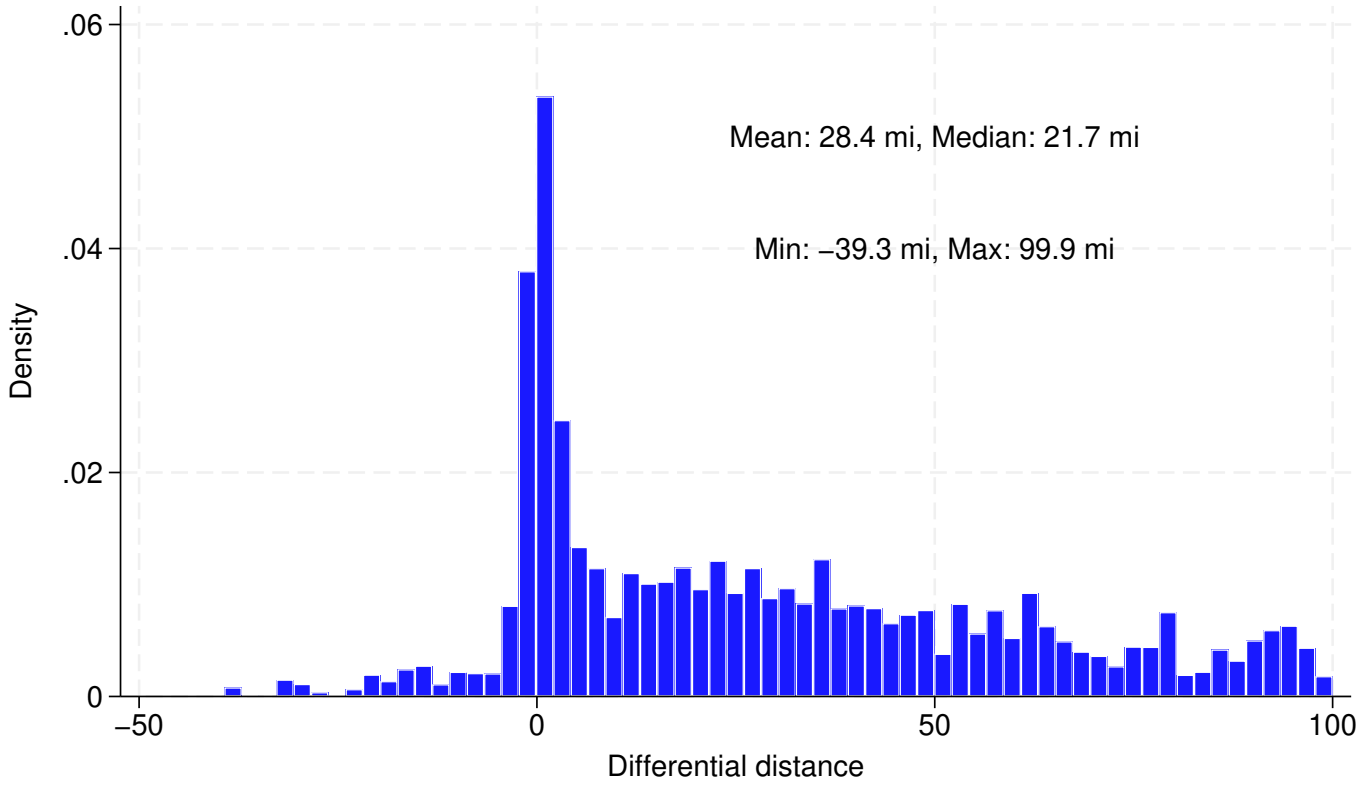
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. This table reports post-double-selection (PDS) instrumental variables estimates where covariates are chosen by a machine learning LASSO algorithm. Appendix 3 provides further methodological detail, including variables in the candidate pool and those selected.

Table A24: The Effect of Catholic Affiliation on Postpartum Contraception Provision

	(1) Full Sample	(2) Non-ERD-adherent
Panel A. Permanent contraception		
Catholic-affiliated	-0.0359***	-0.200
	(0.0112)	(0.119)
Mean dep. var.	0.0624	0.0680
Panel B. Non-permanent contraception		
Catholic-affiliated	-0.00559	0.159
	(0.00800)	(0.117)
Mean dep. var.	0.0333	0.0368
Observations	10,994,881	9,336,954
First stage F-stat	103.33	4.51

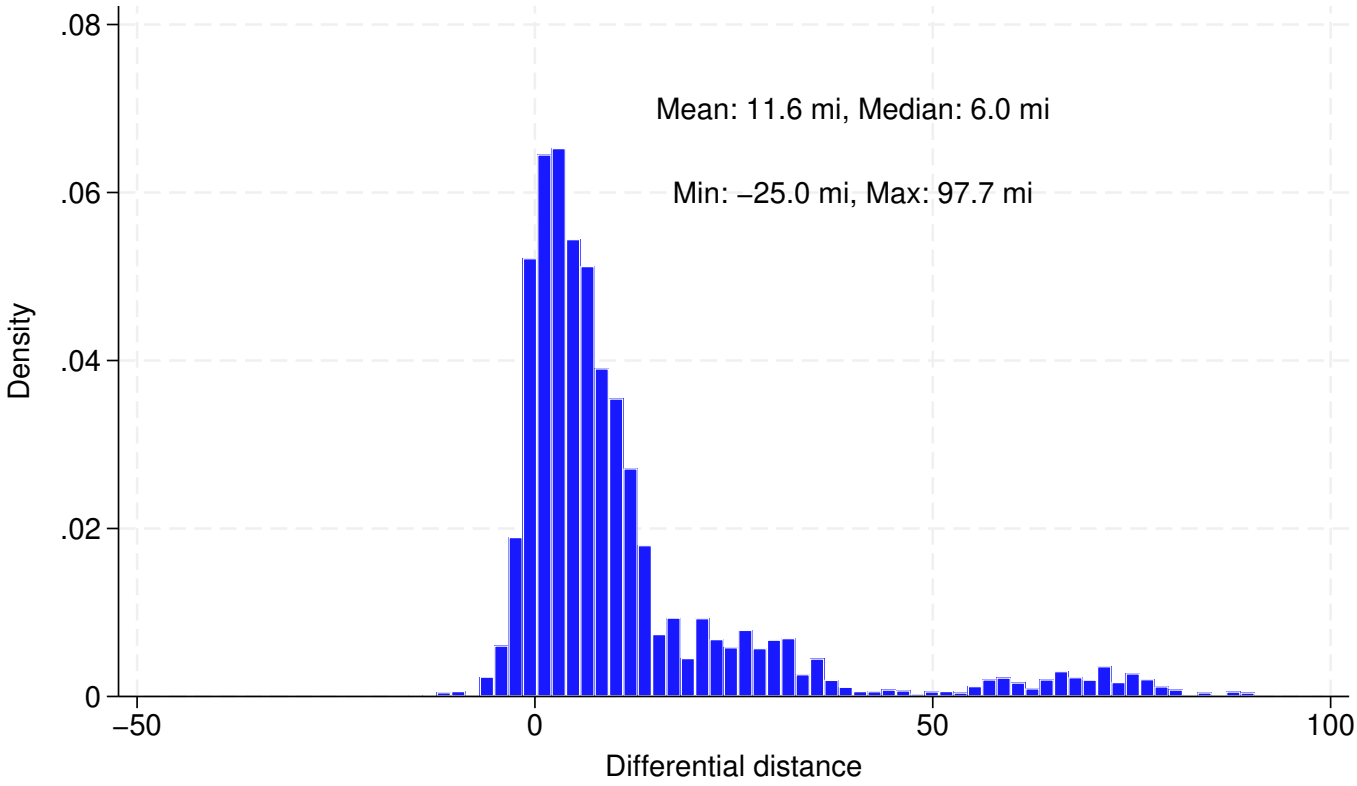
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives.

Figure A1: DD instrument distribution for rural patients, trimmed |DD| < 100



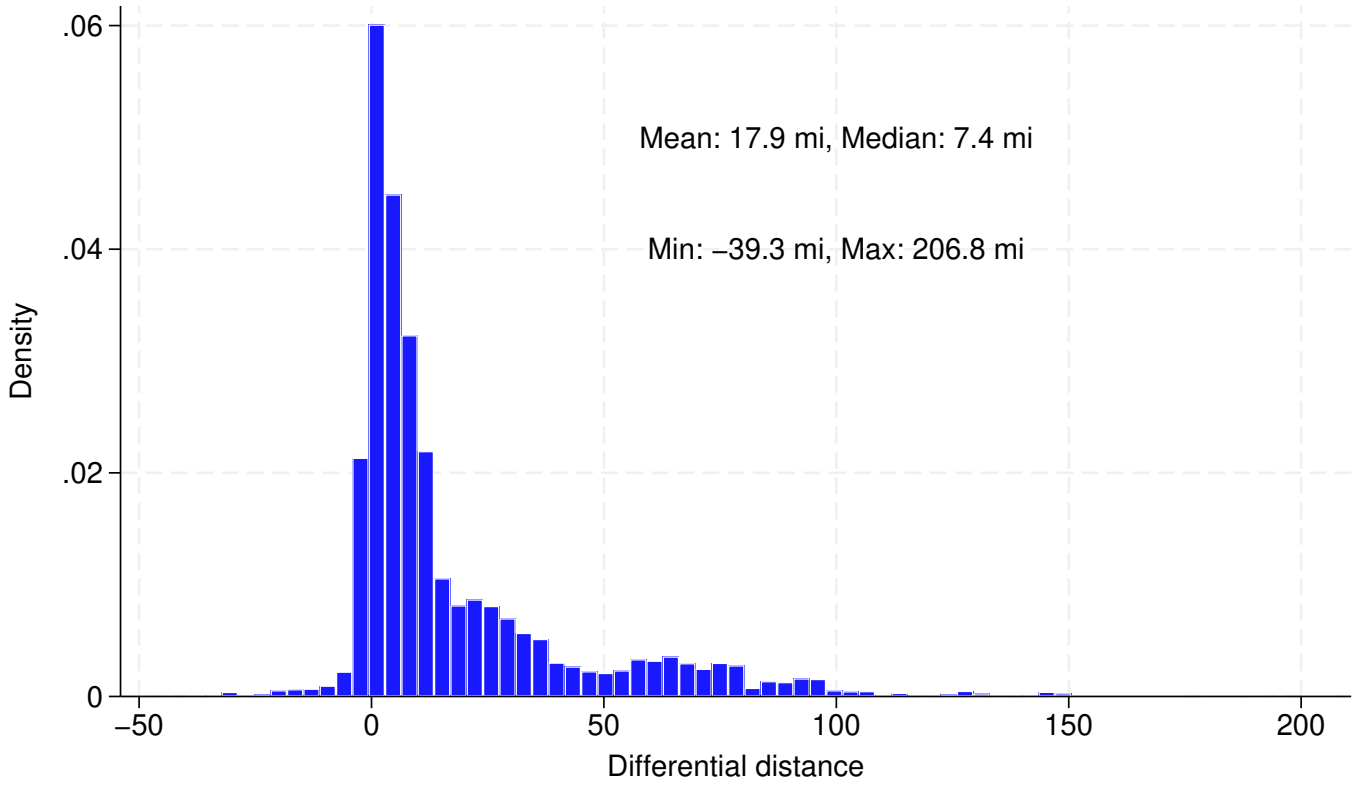
Notes: This figure shows the distribution of the differential distance (DD) instrument for the rural patient sub-sample (|DD| < 100 miles). The differential distance instrument is calculated as the distance from a patient's zip code centroid to the nearest ERD-adherent hospital minus the distance to the nearest non-ERD-adherent hospital. Rural classification follows the National Center for Health Statistics urban-rural classification scheme, with rural defined as counties outside large metropolitan areas of 1 million or more residents. Consistent with lower hospital density in rural areas, the distribution is more widely dispersed than for the urban sub-sample, with a larger mean of 28.4 miles (n=3,145,629).

Figure A2: DD instrument distribution for urban patients, trimmed $|DD| < 100$



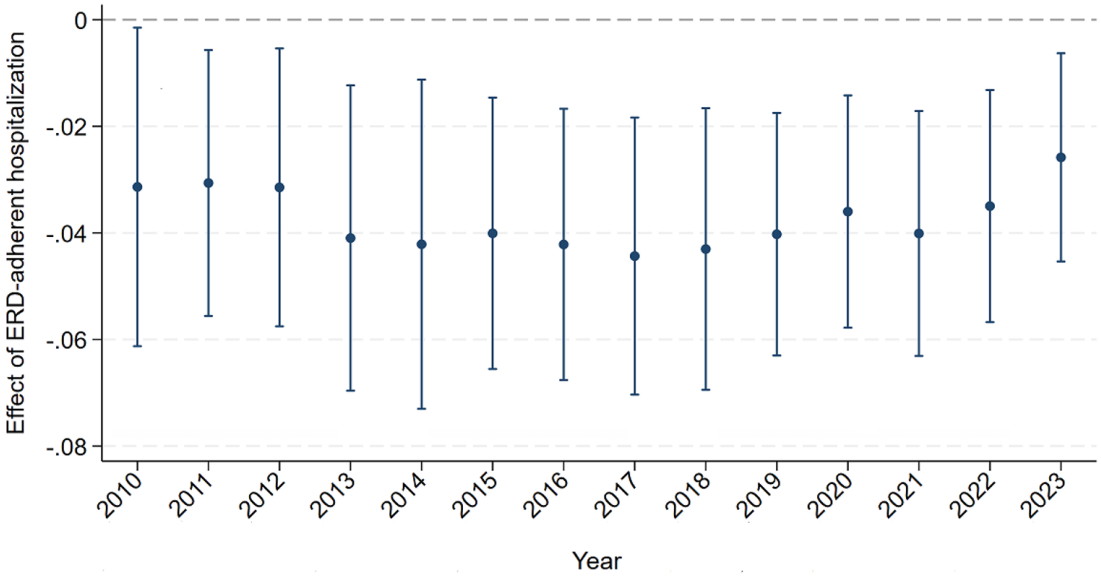
Notes: This figure shows the distribution of the differential distance (DD) instrument for the urban patient sub-sample ($|DD| < 100$ miles). The differential distance instrument is calculated as the distance from a patient's zip code centroid to the nearest ERD-adherent hospital minus the distance to the nearest non-ERD-adherent hospital. Urban classification follows the National Center for Health Statistics urban-rural classification scheme, with urban defined as counties in large metropolitan areas of 1 million or more residents. Compared to the rural sample, the distribution for urban patients is more tightly centered around zero (mean = 11.6 miles), reflecting the higher density of hospitals and smaller differences in travel distances in urban areas (n=7,849,252).

Figure A3: DD instrument distribution, non-trimmed

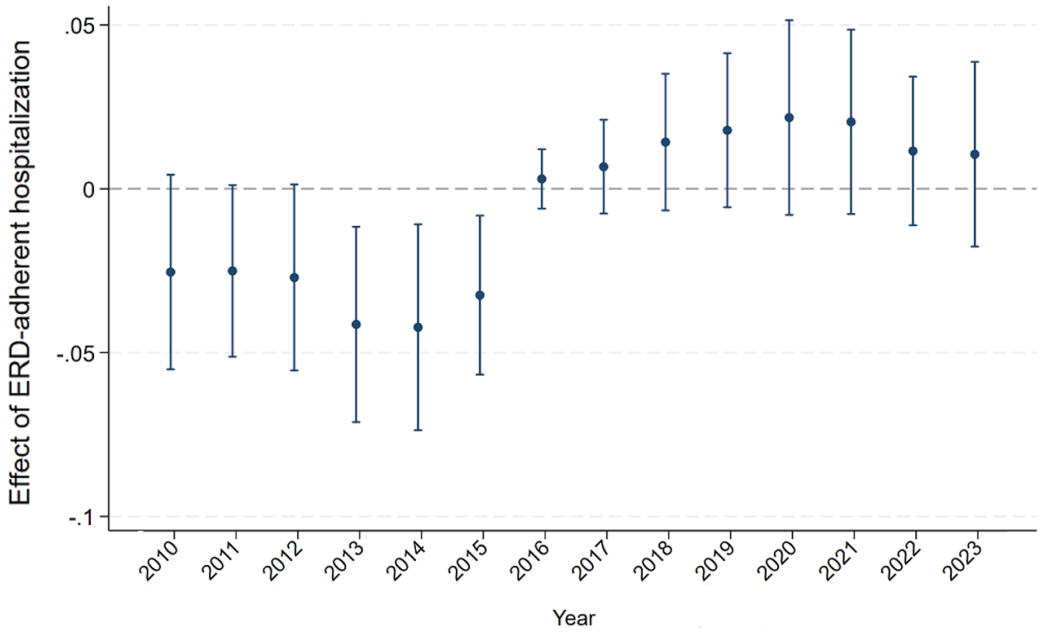


Notes: This figure shows the distribution of the differential distance (DD) instrument for the full, untrimmed sample. The differential distance instrument is calculated as the distance from a patient's zip code centroid to the nearest ERD-adherent hospital minus the distance to the nearest non-ERD-adherent hospital. The distribution exhibits substantial right skew, with some patients facing differential distances exceeding 200 miles (n=11,150,279).

Figure A4: 2SLS IV Estimates by Year



(a) Permanent Contraception



(b) Non-Permanent Contraception

Notes: Each marker plots the coefficient on ERD-adherent hospitalization from a separate 2SLS regression estimated on a single year of data, 2010–2023. All specifications include the full set of controls from the main analysis and state fixed effects. Vertical bars denote 95% confidence intervals from standard errors clustered at the patient county level.

2 Validation Process for ERD-Adherent Hospital Status

Accurately identifying which hospitals adhere to the Ethical and Religious Directives for Catholic Health Care Services (ERDs) each year is critical for the empirical strategy. This appendix details the multi-step validation process we implemented to construct precise, time-varying indicators of hospital ERD adherence. This data construction represents a substantial methodological contribution, as prior research has typically relied on simpler measures of Catholic affiliation that may not accurately capture strict ERD adherence (Freedman, 2023; Solomon et al., 2020; Hill et al., 2019).

Step 1: ERD-Adherence and System Affiliation

We began by cross-referencing the AHA "Catholic-operated" variable against multiple, more specialized data sources on Catholic healthcare. This process involved two parallel streams:

- **ERD-Adherence:** We checked the AHA variable against the Catholic Health Association of the United States (2023) directory, a publicly available source indicating ERD adherence, and the Official Catholic Directory (OCD) from P.J. Kenedy & Sons (2023), a proprietary data source that provides longitudinal data on ERD-adherent hospitals.
- **System-Level Affiliation:** Because ERD-adherence is often—but not always—tied to affiliation with a Catholic health system, we independently verified hospital ownership and system membership. We used the CMS (2024) Hospital Change of Ownership dataset, Levin Associates (2024) acquisition database, and Health Care Pricing Project dataset (Cooper et al., 2022) to validate AHA ownership and acquisition variables. We then used the Compendium of U.S. Health Systems from AHRQ (2024) to identify religiously-affiliated systems and confirmed their Catholic status through targeted searches of health system websites, mission statements, and news sources.

Step 2: Conflict Resolution Hierarchy

We developed a systematic algorithm to reconcile discrepancies across data sources:

- **CHA/OCD Priority:** If the AHA "Catholic-operated" variable conflicted with both the CHA Directory and the Official Catholic Directory, we assumed CHA/OCD was accurate, given their direct connection to the Catholic healthcare community.
- **Resolving CHA-OCD Conflicts:** When CHA and OCD conflicted (which occurred in approx-

imately 10% of cases), we conducted targeted manual verification. This allowed us to determine if and when a hospital adopted the ERDs, especially in cases of mergers or acquisitions. This process involved searching hospital websites for mission statements, religious affiliation disclosures, and service restriction policies, as well as reviewing historical news coverage of hospital acquisitions or conversions. We also cross-referenced multiple years of data to identify consistent patterns.

Step 3: Manual Verification ("Secret Shopper" Calls)

After completing the steps above, 44 hospitals remained inconclusive. These were typically cases with conflicting information, recent ownership changes, or complex affiliation agreements (e.g., non-Catholic hospitals acquired by a Catholic system that may or may not have adopted the ERDs). For these 44 cases, we conducted "secret shopper" calls to the hospitals to clarify a hospitals' religious directives.

- **Protocol:** Callers identified themselves as prospective patients new to the area seeking information about perinatal hospital services. Callers asked whether a hospital was Catholic, and specifically, whether postpartum tubal ligation was available at the facility. For hospitals that had undergone recent ownership changes, we also verified the timing of any changes and their impact on service restrictions.
- **Implementation:** Calls were conducted between March-May 2024 via main hospital operator lines during business hours. When transferred to obstetrics or patient services departments, we repeated the questions. If answers were ambiguous or contradictory, we conducted follow-up calls.

Summary & Implications

This comprehensive, multi-stage process yielded the final, time-varying indicator of ERD adherence used for the analysis. The precise measurement of ERD adherence has important implications for research on hospital religious restrictions. First, using ERD adherence rather than Catholic affiliation as the treatment variable reduces misclassification bias, as we correctly exclude Catholic-affiliated hospitals that do not impose service restrictions. Second, the year-specific indicators allow us to capture the exact timing of hospital conversions to ERD adherence, critical to reduce bias from pre-/post-conversion observations. Last, while proprietary data restrictions and data use agreements preclude public data sharing, this detailed documentation increases transparency and will enable other researchers to replicate the measurement approach using similar sources and validation procedures.

3 Methodological details

3.1 *Dobbs'* impact and temporal consistency

To evaluate whether the causal impact of ERD adherence evolved following the June 2022 Dobbs decision, I estimated two complementary temporal specifications. First, I conducted split-sample instrumental variable regressions, stratifying the sample into pre-Dobbs (2010 to Q1 2022) and post-Dobbs (Q3 2022 to 2023) periods. Q2 2022 was dropped for these analyses. Second, I estimated pooled interaction models that include an endogenous interaction term between ERD adherence and a post-Dobbs indicator (ERD-adherent X post-Dobbs), instrumented simultaneously by differential distance and its time-interaction (DD X post-Dobbs). To explore whether institutional restrictions interact with state-level reproductive healthcare access, I further stratified these interaction models across states in the sample that were protective of abortion rights in the immediate post-Dobbs period (Maryland, New Jersey, New York, Washington, and Oregon) and those that were hostile (Arizona, Florida, and Kentucky). I dropped two states missing 2023 data to ensure consistency in sample composition (AR and CO). All models include the full set of patient-, hospital-, and market-level controls and state-by-year fixed effects. Following standard econometric practice, first-stage strength is evaluated using multivariate Sanderson-Windmeijer (SW) F-statistics in the interaction models, which test individual endogenous regressor identification after partialling out linear projections of remaining endogenous variables (Sanderson and Windmeijer, 2016).

3.2 Downstream reproductive health outcomes

Sample construction & timing. These analyses use a subsample of five states (Arkansas, Florida, Maryland, New York, and Washington) with unique patient identifiers that allow linking patient's hospitalizations over time within that state. I drop index deliveries in the final two years of the study period (2022–2023) for sufficient follow up period. All downstream outcomes are based on the next delivery after the index birth and conception is dated as nine months (three quarters) before the subsequent birth. A short-interval pregnancy (SIP) within 6, 12, or 18 months therefore corresponds to a subsequent birth within roughly 15, 21, or 27 months of the index delivery; I exclude subsequent births within six months of the index birth as likely erroneous records. All models use the same patient-, hospital-, and market-level controls (measured at the index delivery) as the main specification with state-by-year fixed effects and standard errors clustered by patient county.

SIP & adverse birth outcomes. For severe maternal morbidity (SMM) and preterm birth, I estimate two related outcomes. The first specification indicates if the patient has a subsequent short-interval pregnancy *and* the birth is SMM-complicated or preterm (i.e., does ERD-adherent delivery raise the probability of experiencing an adverse short-interval birth?) The exploratory specification conditions on having a subsequent short-interval pregnancy (i.e., among those who had a SIP, does ERD-adherent delivery raise the probability of experiencing SMM or preterm birth at the subsequent birth?) However, because the conditioning event (SIP) is itself affected by treatment, this specification is susceptible to post-treatment selection bias and I report it only as suggestive. Here I use state and year fixed effects (rather than state-by-year fixed effects) given small sample size.

Fertility. I measure fertility two ways over a three-year window after the index delivery: a binary indicator for any subsequent birth, and a count of subsequent births.

Hospital switching. I measure hospital switching as an indicator equal to one if the patient's immediate next delivery occurs at a hospital of a different ERD type than the index delivery (ERD-adherent to non-ERD-adherent, or vice versa).

Limitations. These measures capture only births occurring at hospitals in the same state during the study period (2010–2023). I therefore do not observe subsequent births in other states, births at the same hospitals outside this window, or pregnancies that do not result in an inpatient delivery. These features generally bias us toward understating subsequent events.

3.3 Machine learning covariate selection: Post-Double-Selection Least Absolute Shrinkage and Selection (PDS-LASSO)

To verify that the core findings are not artifacts of researcher discretion in selecting covariates, I re-estimate the main outcomes using a machine learning covariate selection procedure as a robustness check. This approach addresses a fundamental challenge in empirical research: the "right" set of control variables is rarely known with certainty. Including too few controls risks omitted variable bias; including too many risks overfitting. Penalized regression resolves this tension by using a data-driven procedure, rather than the researcher's judgment, to choose which covariates from a high-dimensional candidate pool enter the model.

The preferred implementation is the instrumental variables post-double-selection (PDS) estimator (Bel-

loni et al., 2014, 2012). The procedure applies the Least Absolute Shrinkage and Selection Operator (LASSO) separately to each equation of the model. In the first step, the outcome is regressed on the full candidate pool with a LASSO penalty, which shrinks the coefficients on weakly predictive covariates exactly to zero and returns a sparse set of covariates that predict the outcome. In the second step, the same penalized regression is applied with ERD-adherent hospitalization as the dependent variable, returning the covariates that predict treatment. A third step applies the LASSO to the instrument equation. The union of the covariates selected across these steps is then entered as controls in an otherwise standard 2SLS regression. Selecting on both the outcome and the treatment equations—rather than on the outcome alone—is what gives the procedure its name and its central property: a covariate is retained if it predicts *either* the outcome or treatment assignment, so a potential confounder is dropped only when it is weakly related to both. This guards against the omitted variable bias that single-equation selection can introduce (Belloni et al., 2014). Estimation is carried out with the Stata `ivlasso` command (Ahrens et al., 2020).

High-dimensional candidate pool

The main specification includes 24 covariates along with year-by-state fixed effects (see Section 3). For the LASSO robustness check, the algorithm chooses among a penalized candidate pool of 61 covariates spanning three domains:

- **Patient-level (35):** Indicators for 31 comorbidities—AIDS/HIV, alcohol abuse, blood loss anemia, cardiac arrhythmias, chronic pulmonary disease, coagulopathy, congestive heart failure, deficiency anemia, depression, diabetes (complicated), diabetes (uncomplicated), drug abuse, fluid and electrolyte disorders, hypertension (complicated), hypertension (uncomplicated), hypothyroidism, liver disease, lymphoma, metastatic cancer, obesity, other neurological disorders, paralysis, peptic ulcer disease (excluding bleeding), peripheral vascular disorders, psychoses, pulmonary circulation disorders, renal failure, rheumatoid arthritis/collagen vascular, solid tumor without metastasis, valvular disease, and weight loss—together with the Elixhauser comorbidity index, emergency or urgent admission, marital status, and urban residence.
- **Hospital-level (19):** Ownership type (government, non-profit, for-profit), teaching status, total admissions, obstetric unit, obstetric caseload, Medicaid payer mix, average daily census, total operating expenses, total full-time equivalent employees, system membership, community hospital designation, critical access hospital status, rural referral center status, sole community provider

status, metropolitan location, emergency department, and state Medicaid expansion status.

- **Market-level (7):** Obstetric market concentration (HHI using births within hospital health service area), OB-GYNs per capita, number of abortion clinics, and total religious and Catholic adherence rates and congregation counts (all per patient county).
- **Forced variables:** Patient age, race/ethnicity, and expected payer (11 estimated columns after collinear base categories are dropped), together with 111 year-by-state fixed effects, are forced into every specification (partialled out before penalization, so the algorithm cannot discard them).

Selected covariates

- For **permanent contraception**, the algorithm retained 32 of the 61 candidates: metropolitan residence, congestive heart failure, cardiac arrhythmias, valvular disease, pulmonary circulation disorders, peripheral vascular disorders, uncomplicated hypertension, complicated hypertension, other neurological disorders, chronic pulmonary disease, uncomplicated diabetes, complicated diabetes, hypothyroidism, renal failure, liver disease, AIDS/HIV, lymphoma, solid tumor without metastasis, rheumatoid arthritis, coagulopathy, obesity, fluid and electrolyte disorders, blood loss anemia, deficiency anemia, depression, emergency or urgent admission, non-profit ownership, teaching status, Medicaid payer mix, hospital metropolitan location, sole community provider status, and Catholic congregations in the patient's county. Of these, 30 were selected in the outcome equation and 2 (non-profit ownership and teaching status) in the treatment equation.
- For **non-permanent contraception**, the algorithm retained 27 candidates: congestive heart failure, cardiac arrhythmias, valvular disease, pulmonary circulation disorders, peripheral vascular disorders, uncomplicated hypertension, complicated hypertension, other neurological disorders, chronic pulmonary disease, uncomplicated diabetes, complicated diabetes, hypothyroidism, renal failure, liver disease, AIDS/HIV, lymphoma, rheumatoid arthritis, obesity, blood loss anemia, deficiency anemia, drug abuse, psychoses, depression, non-profit ownership, teaching status, Medicaid payer mix, and Medicaid payer mix. Again, non-profit ownership and teaching status were the covariates selected in the treatment equation.