The Effects of Restricted Access to Postpartum Contraception on Reproductive Health: Evidence from Hospitals with Religious Directives*

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Abstract

As demand for postpartum contraception increases, more hospitals are adopting religious restrictions prohibiting these services. We use quasi-random variation in geographic proximity to hospitals adhering to Ethical and Religious Directives (ERDs) to evaluate access to contraception among over 9 million birth hospitalizations (2010-2023). Giving birth at an ERD-adherent hospital reduces postpartum contraception provision by 70%, with effects largest among rural patients. We subsequently document a 29% increase in short-interval pregnancies among rural patients. These findings suggest that institutional constraints lead to reduced access and adverse health effects, informing policy debates on hospital transparency and merger oversight.

JEL classification: I11, I18, J13

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1 Introduction

A growing, yet understudied, tension exists in the U.S. healthcare landscape: as demand for immediate postpartum contraception rises, especially post-*Dobbs*,[1] an increasing number of hospitals restrict these services due to religious directives. Today, nearly one-sixth of U.S. hospitals adhere to the Ethical and Religious Directives for Catholic Health Care Services (ERDs), a set of 77 specific directives that prohibit provision of medical care against religious doctrine, including contraception. The number of hospitals operating under the ERDs has grown by over 20% in the last two decades (Freedman, 2023; Solomon et al., 2020). At the same time, there is substantial unmet demand for contraception, especially in the immediate postpartum period. 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 at approximately 20% of birth hospitalizations (Potter et al., 2013, 2014; Zite et al., 2005; Seibel-Seamon et al., 2009; Wolfe et al., 2017; ACOG, 2021).[2] The gap between demand and access has widened in recent years, with multiple studies documenting increased requests for immediate postpartum contraception following the 2022 *Dobbs* Supreme Court decision (Strasser et al., 2025; Ellison et al., 2024; Wahlstedt et al., 2025; Schmall, 2024). This tension between rising demand and expanding service restrictions poses a fundamental question: what are the consequences when institutional religious policies constrain access to reproductive healthcare?

Contraception provided during a hospital stay for childbirth (i.e., immediate postpartum contraception) is medically efficient, cost-effective, and eliminates the need for additional procedures or appointments—advantages that are especially important for rural patients, who face mounting barriers to accessing reproductive healthcare (Cooper et al., 2024; Clark and Levy, 2025; ACOG, 2021). While conventional wisdom suggests that restricted access to contraception can lead to adverse reproductive health outcomes, the causal evidence supporting this relationship is surprisingly limited. In the U.S., quasi-experimental studies exploiting Medicaid policy changes, for example, have established credible causal estimates of how contraception access affects fertility outcomes, showing that expanded access reduces the birth rate, especially among teens and low-income women (Packham, 2017; Bailey, 2013). Yet these studies typically stop at birth rate and do not trace through to subsequent reproductive health outcomes, such as effects on pregnancy spacing. One observational study documented associations between unful-

[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.

[2] ACOG refers to the American College of Obstetricians and Gynecologists.

filled postpartum sterilization requests and shorter pregnancy intervals, but is limited by the potential for patient selection and unobserved confounding (Thurman and Janecek, 2010).

Understanding whether reduced contraception access leads to shorter interpregnancy intervals is critical because of the well-established health risks associated with closely spaced pregnancies. A substantial body of research has linked short-interval pregnancies—defined as those occurring within 18 months of a previous birth—to adverse maternal and infant health outcomes, including preterm birth, low birth weight, and maternal morbidities, with risks highest for pregnancies occurring within 6 months of a previous birth (Conde-Agudelo et al., 2006, 2007; Ali et al., 2023; Wang et al., 2022; ACOG, 2019b). These health consequences make pregnancy spacing a key clinical outcome and priority target of the Healthy People 2030 objectives (DHHS, 2025).

The relationship between contraception access and reproductive health outcomes is particularly relevant in the context of hospital-level service restrictions. While recent research has documented associations between proximity to a Catholic-affiliated hospital and reduced contraception provision (Meille and Monnet, 2024; Rodriguez et al., 2023), these approaches face identification challenges related to patient selection and secular trends. Moreover, no prior research has established whether institutional religious restrictions on contraception provision lead to adverse patient outcomes after the index birth—a critical link for understanding welfare implications and informing policy debates about hospital consolidation and transparency.

In this paper, we seek to fill this gap by analyzing the impact of hospital ERD adherence on both immediate postpartum contraception and subsequent short-interval pregnancies. Our primary empirical challenge is patient selection: individuals who prefer not to use contraception may be more likely to seek care at ERD-adherent hospitals, which would bias naive estimates. To circumvent this issue, we 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. We estimate the relationship between instrumented hospital admission and our outcomes of interest using two-stage least squares (2SLS) with a dataset of over 9 million birth hospitalizations from 2010-2023, constructed using 11 state inpatient databases from the Healthcare Cost and Utilization Project.

We first find that admission to an ERD-adherent hospital causes substantial reductions in the likelihood of receiving immediate postpartum contraception. Specifically, giving birth at an ERD-adherent hospital reduces the likelihood of receiving permanent contraception (e.g., sterilization) by 4.23 percentage points (pp), a 70% decrease relative to the mean, and non-permanent contraception by 1.61 pp, a 41% decrease relative to the mean, compared to giving birth at a non-ERD-adherent hospital. These effects are concentrated among rural patients, for whom ERD-adherent hospital admission reduces permanent contraception by 5.95 pp (80% decrease) and non-permanent contraception by 3.65 pp (83% decrease). We then document downstream reproductive health consequences: rural patients who deliver at ERD-adherent hospitals experience a statistically significant 3.81 pp increase in the likelihood of a subsequent pregnancy within 18 months, representing a 29% increase relative to the mean.

Our study makes four key contributions to the literature. First, we provide causal evidence of the relationship between hospital religious restrictions and access to reproductive healthcare. Much of the 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). For example, several studies document associations between proximity to a Catholic-affiliated hospital and reduced contraception provision (Meille and Monnet, 2024; Rodriguez et al., 2023) or increased short-interval pregnancy risk (Liu et al., 2025; Caldwell et al., 2022). However, these findings do not represent causal relationships given potential for unobserved confounding. The study most closely related to ours is Hill et al. (2019), which exploits hospital conversions to Catholic ownership using a difference-in-differences design and finds a 31% reduction in the per-bed rate of tubal ligation post-conversion. We build upon this research by employing an instrumental variable strategy that addresses the endogeneity of patient hospital choice at the individual level. We also introduce a more precise, time-varying treatment definition of "ERD-adherent" hospitals (see Appendix A.1) and extend the analysis to examine downstream reproductive health consequences.

Second, our findings 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 (Bailey, 2013; Packham, 2017; Myers et al., 2025; Laszlo et al., 2023), but the role of facility-level restrictions as an independent, binding constraint has remained largely unexplored. Our 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 heighten the need for recognition and transparency of institutional restrictions as a distinct determinant of patient care.

Third, our paper informs the literature on the behavior of not-for-profit and mission-driven organizations. A central question in this literature is whether not-for-profit firms act as social welfare maximizers or as "for-profits in disguise" (Sloan, 2000; Taylor et al., 2023; Schulte et al., 2025). We study a case where explicit adherence to the ERDs in Catholic hospitals (the vast majority of which are nonprofits) acts as a powerful constraint on service provision. Our results show this mission leads to outcomes that are detrimental to the health of key patient populations, providing a clear example of how organizational mission can conflict with patient welfare and generate significant negative externalities, such as an increased rate of short-interval pregnancies. This finding adds important nuance to the debate on the social benefit of not-for-profit status in healthcare.

Fourth, we contribute to the literature on healthcare market consolidation, which has largely focused on price and quality effects, while overlooking impacts to service availability and patient outcomes (Dafny et al., 2016; Gaynor, 2018; Cooper et al., 2019; Beaulieu et al., 2020; Eliason et al., 2020). This gap is critical given the expansion of Catholic health systems, which typically impose service restrictions by requiring newly acquired hospitals to adhere to the ERDs.[3] Our analysis provides evidence on this phenomena, demonstrating that the imposition of non-clinical religious restrictions substantially alters the availability of reproductive health services, with effects concentrated in rural markets where patients have limited alternatives. These findings suggest that traditional merger reviews, which historically focus on price and quality effects, may overlook significant anti-competitive effects and patient welfare losses due to institutional service restrictions.

The paper proceeds as follows. Section 2 provides a brief background on immediate postpartum contraception and hospital religious restrictions. In Section 3, we introduce the data, study sample, and variables, including our approach to precisely identifying which hospitals are ERD-adherent. We describe our empirical approach and identification strategy in Section 4. We present descriptive results in Section 5 and causal estimates in Section 6. In Section 7, we discuss the implications of our findings

[3] As of 2023, 89% of hospitals affiliated with a Catholic health system were also ERD-adherent.

and conclude.

2 Background

2.1 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, 2018; 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 is also typically medically optimal due to technical ease and efficiency (ACOG, 2021).[4] 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).[5]

Access to contraception during the birth hospitalization 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 hospital-

[4] 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).

[5] 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.

ization (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). Non-White populations and those with lower incomes or more children are more likely to both request and receive permanent contraception (Chan and Westhoff, 2010; Borrero et al., 2007; ACOG, 2021).

Non-permanent contraception includes both long-acting reversible contraception (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).[6] 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 desired contraception during the birth hospitalization, 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. Prior work has shown that patients with unfulfilled immediate postpartum sterilization requests have roughly twice the rate of pregnancy within one year compared to those not requesting sterilization, though this relationship is not necessarily causal (Thurman and Janecek, 2010). These unintended short-interval pregnancies represent not only a deviation from patients' stated reproductive intentions but also carry well-established health risks: short-interval pregnancies, especially those within 6 months of a previous birth, increase the risk of adverse maternal and infant outcomes (Conde-Agudelo et al., 2006, 2007; ACOG, 2019b). Understanding and addressing unfulfilled contraceptive demand is therefore critical for

[6] 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.

assessing both reproductive autonomy and downstream health outcomes.

2.2 Hospitals with religious restrictions

One important, yet understudied, barrier to contraception access is hospital religious restrictions. The vast majority of all religiously-affiliated hospitals are Catholic.[7] Most, but not all, of Catholic-affiliated hospitals are also ERD-adherent, meaning they are required to follow the Ethical and Religious Directives for Catholic Health Care Services (ERDs).[8] The ERDs are a set of 77 specific religious directives that govern hospital operations, provider decision-making, and clinical care. 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, such services are permitted as an *indirect* effect of another procedure meant to cure or alleviate a "serious pathological condition" (U.S. Conference of Catholic Bishops, 2018; Freedman, 2023).

The number of hospitals with religious service restrictions has grown substantially in recent decades as Catholic systems expand. Between 2001 and 2020, the number of Catholic-affiliated hospitals increased by 28%. Today, four of the ten largest U.S. health systems are Catholic and, overall, more than one-sixth of U.S. hospitals are Catholic-affiliated (Solomon et al., 2020; Schulte et al., 2025). In terms of geographic presence, the market share of Catholic-affiliated hospitals varies based on locality (Solomon et al., 2020; Drake et al., 2020) but, on average, Catholic-affiliated hospitals are equally likely to be located in rural vs. urban areas.[9]

Despite this expansion, 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

[7] 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).

[8] Based on the primary author's analysis, 80% of all ERD-adherent hospitals were owned by a Catholic system in 2023, with 14% owned by a non-Catholic system and 6% independent. Of the hospitals affiliated with a Catholic health system, 89% were also ERD-adherent.

[9] Based on primary author's analysis, 69% of U.S. Catholic-affiliated hospitals were in rural areas as of 2023, compared to 68% of non-Catholic hospitals.

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). This information asymmetry raises concerns about whether patients are able to make fully informed choices about where to receive care.

Policymakers have begun to respond to these concerns, evidenced by the numerous federal and state policies—both passed and proposed—related to the growth of Catholic healthcare. While some policies aim to protect healthcare institutions' ability to provide care as guided by their religious beliefs,[10] other policies seek to ensure patients' access to comprehensive reproductive healthcare services (or at least require hospital transparency about what services are not provided). Appendix Table A1 describes nine state-level policies related to reproductive healthcare provided in Catholic hospitals.

3 Data and variables

Our primary data sources are 2010-2023 Healthcare Cost and Utilization Project State Inpatient Databases (HCUP-SIDs) (AHRQ, 2025). HCUP-SIDs are state-specific all-payer datasets derived from hospital discharge records from all inpatient care hospitalizations in the U.S. Inpatient settings are particularly relevant to our 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.

We 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 bench-

[10] "Safeguarding the Rights of Conscience as Protected by Federal Statutes" rule of January 2024 strengthens institutional conscious protections for individuals and religiously-affiliated institutions at the federal level (DHHS, 2024).

marking methods (AHA, 2025b; CDC, 2024).

3.1 Study sample

Our main analytic sample consists of all birth hospitalizations for females aged 12–49 at general medical and surgical hospitals across eleven geographically diverse states: Arkansas, Arizona, Colorado, Florida, Kentucky, Maryland, New Jersey, New York, Oregon, Washington, and Wisconsin (n=9,013,727, Table 1). Our sample includes years 2010-2023, but not all years were available for each state. State-years in our 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 our sample may contain multiple hospitalizations for the same patient.

As outlined in Appendix Table A2, we arrived at this sample by making several exclusions. First, from the sample of all births (n=10,925,859), we exclude records missing patient county or zip code (14.4%), then records missing hospital location (1.7%), as these variables are required for our empirical strategy. Second, we exclude 0.75% 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).[11] We also exclude 1.2% 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. We further discuss this analytical decision in Section 4.2.

For our secondary analyses of short-interval pregnancy, we create a sub-sample of five states[12] with unique patient identifiers that enables linkage of hospitalizations by the same patient over time (Appendix Table A3). In this sub-sample, we drop index birth hospitalizations in the last two years for each state to allow sufficient time to observe short-interval pregnancies.

3.2 Treatment: Identifying ERD-adherent hospitals

Our 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

[11] We do not exclude observations where patient and hospital states share a border (and both states are in our sample). For example, we 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).

[12] The five states in this sub-sample are Arkansas, Florida, Maryland, New York, and Washington.

the ERDs. This approach extends prior research that has relied on measures of Catholic affiliation that may not capture true adherence to the ERDs.[13] For instance, a hospital may be acquired by a Catholic system but have a specific affiliation 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.[14]

To construct precise, time-varying indicators of hospital ERD adherence, we implemented a rigorous multi-step validation process (detailed in Appendix A.1). To briefly summarize: we first cross-referenced AHA Catholic-affiliation variables against six secondary data sources.[15] In cases of conflicting information, we then conducted a targeted search of hospital websites and historical news reports. In 44 cases where adherence to the ERDs remained inconclusive, we 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), we verified the timing of any changes and impact on service restrictions.

3.3 Primary and secondary outcomes

Our primary outcome of interest is provision of postpartum contraception (permanent and non-permanent) during the birth hospitalization. We define permanent contraception, also known as sterilization, 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 the hormonal birth control shot or pill), and other unspecified contraceptive care/management that is delivered during the birth hospitalization.

[13] Appendix Table A4 presents the proportion of sample observations each year at ERD-adherent and Catholic-affiliated hospitals.

[14] 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-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."

[15] These are: P.J. Kenedy & Sons (2023) Official Catholic Directory, Catholic Health Association of the United States (2023) Directory, CMS (2024) Hospital Change of Ownership dataset, AHRQ (2024) Compendium of U.S. Health Systems, Levin Associates (2024) HC database, and Cooper et al. (2022) Health Care Pricing Project.

Our secondary outcome of interest is short-interval pregnancy, defined as a pregnancy within 18 months of a birth.[16] Short interpregnancy intervals are associated with elevated risks of adverse maternal and infant outcomes, and reducing the proportion of short-interval pregnancies is a key priority of the 2030 Healthy People Objectives (DHHS, 2025; ACOG, 2019b). Given that risks of severe adverse outcomes increase as the interpregnancy interval shortens, we report results for pregnancies occurring within 6, 12, and 18 months of the index birth (Ali et al., 2023; ACOG, 2019b).

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 A.5 provides the complete list of codes used to define our study sample and outcomes.

3.4 Covariates

We include a rich set of patient, hospital, and market-level covariates. Patient-level covariates include both sociodemographic and clinical traits. For sociodemographic characteristics, we 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 our main analyses, we use the National Center for Health Statistics (NCHS) urban-rural county classification. We assess robustness using two alternative definitions of urban: (i) metropolitan counties according to the Office of Management and Budget (OMB), and (ii) counties with above average obstetric provider concentration (given that urbanicity is often a proxy for provider density).[17]

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:

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

[17] Using the NCHS classification, urban counties are those part of large metropolitan areas of 1 million or more residents. Examples of large U.S. metropolitan areas include New York, NY (20 million), Atlanta, GA (6 million), San Jose, CA (2 million), Tulsa, OK (1 million), and Omaha, NE (1 million). OMB defines metropolitan counties as those with cities or urbanized areas of over 50,000 residents (NCHS, 2023; U.S. Census Bureau, 2025). Obstetric provider concentration was defined using the Herfindahl-Hirschman Index with annual birth volumes at hospitals located in patient county of residence.

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). We 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).[18]

In addition, we control for several relevant characteristics of the birth hospital including: ownership structure (nonprofit, 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). We also control for hospital competition, given suggestive evidence that hospitals in less concentrated markets may more strictly adhere to the ERDs (Freedman, 2023; Kramer et al., 2021).[19] It is important to control for these hospital-level characteristics as ERD-adherent and non-ERD-adherent hospitals may differ systematically on dimensions that independently affect contraception provision. Including these controls allows us to more precisely isolate the effect of ERD adherence, holding constant other hospital attributes that might confound the relationship between religious restrictions and contraception access.[20]

Our models also account for several market characteristics at the patient county level. As individual religiosity is unobserved, we control for the religious composition of a patient's county using measures from the U.S. Religious Congregations and Membership Study: Catholic adherents per 1,000 population and the number of Catholic congregations with (Grammich et al., 2020). We include several other market-level controls as a robustness check, as described in Section 6.3.

4 Empirical approach

Our 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 re-

[18]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.

[19]Evidence suggests that Catholic hospitals in less 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 less concentrated hospital markets. We 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.

[20]We recognize the technical concern that controlling for characteristics of the birth hospital could introduce post-treatment (collider) bias. However, omitting them risks violating the exclusion restriction, which we view as a more direct threat to identification. As a robustness check, we estimate our main specification without hospital-level controls. Results are consistent, though slightly less precise (see Appendix Table A12).

productive health events, specifically short-interval pregnancies. In order to build intuition for why our preferred empirical approach is necessary to estimate this relationship as causal, we 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 Section 4.2, we outline an alternative approach 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. We discuss assumptions and validity of our identification strategy in Section 4.3

4.1 Ordinary Least Squares (OLS) estimation

If patients were randomly assigned to hospitals, we could estimate our causal relationship of interest using standard ordinary least squares (OLS). In this approach, we 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:

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

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 our 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 our 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 its 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, we 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, we 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.[21] 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-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; Lee and Basu, 2025; Card et al., 2023; Cornell et al., 2019; McClellan et al., 1994). This 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).[22]

[21] To calculate our DD instrument, we 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. We then calculate the difference between these distances. A negative DD value indicates the hospital closest to the patient adheres to the ERDs. For patient residence, we 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).

[22] 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."

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, we exclude observations where the absolute DD exceeds 100 miles (approximately 2.5 to 3 hours of driving time).[23] This excludes 1.2% 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 14.6 miles closer to the patient zip code centroid, on average, than the closest ERD-adherent hospital.[24] As a robustness check, we re-estimate our primary outcomes using the untrimmed DD measure (see Section 6.3). Results are consistent, although with a lower first-stage F statistic, especially for the rural sub-population.

We use a two-stage least squares (2SLS) procedure to estimate the causal relationship between hospital type (ERD-adherent vs. not) and each outcome. The key insight of this method is to decompose the variation in ERD_{ihmt} into two components: (1) variation correlated with unobserved confounders (the "problematic" component, $\hat{\mu}_{ihmt}$), and (2) variation driven by the exogenous instrument (the "problem-free" component, $\widehat{\text{ERD}}_{ihmt}$). The IV strategy isolates and uses only the exogenous component for estimation. In the first stage, we estimate the relationship between the endogenous treatment variable and the plausibly exogenous instrument:

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

where DD_{ihmt} is the differential distance instrument; $\mathbf{X}_{ihmt}$ 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. This first stage decomposes the endogenous treatment variable into two components: an exogenous component $\widehat{\text{ERD}}_{ihmt} = \hat{\alpha}_0 + \hat{\alpha}_1 DD_{ihmt}$ driven by the instrument, and an endogenous component $\hat{\mu}_{ihmt}$ correlated with unobservables. By using only $\widehat{\text{ERD}}_{ihmt}$ in the second stage, we isolate

[23] 100 miles was selected as the cutoff as this isolates extreme outliers in approximately 1% of our sample, and given prior literature examining hospital choice for obstetric care (Fontenot et al., 2024; Hebert et al., 2020).

[24] Appendix Figure A2 presents the DD distribution for the full (non-trimmed) sample, which is highly right-skewed. The DD mean is 16.0, with -46.7 as the minimum and 206.8 as the maximum. Appendix Figures A3 and A4 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 26.2 and 10.4 miles for rural and urban patients, respectively.

the exogenous variation in hospital choice and discard the problematic endogenous component.

To demonstrate the feasibility of the 2SLS design, Table 2 presents the first-stage results, demonstrating that the DD instrument strongly predicts hospital choice and therefore meets the relevance assumption. The coefficient of -0.00507 implies that each mile increase in differential distance (i.e., the ERD-adherent hospital gets relatively farther away) decreases the probability of admission by about 0.507 percentage points, or by 5.07 percentage points for a 10 mile change. Relative to the average rate of 15.4% for ERD-adherent hospitalizations, this 10-mile change represents a substantial 33% reduction, demonstrating that the instrument has a powerful effect on hospital choice. The first stage F-statistic is 79.49, suggesting that the instrument has substantial explanatory power with respect to the endogenous variable.[25] Figure 2 illustrates this relationship in a binned scatterplot that visually confirms the strong, negative first-stage relationship between our instrument and the endogenous treatment variable.[26] The resulting downward slope provides clear visual evidence supporting the relevance of our instrument: as the relative distance to an ERD-adherent hospital increases, the likelihood of a patient going to one decreases.

In the second stage, we estimate the causal effect using only the exogenous variation:

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

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.

Our 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

[25] As our specification clusters standard errors by patient county, we 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).

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

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.[27] Therefore, γ_1 should be interpreted as the average causal effect of admission to an ERD-adherent hospital for the population of patients whose choice of hospital is determined by marginal changes in relative travel distance.

4.3 Instrument validity

In addition to the relevance and common support assumptions discussed above, instrumental variable validity requires outcome independence and support for the exclusion restriction. Outcome 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 outcome. In our context, this means that patients living relatively closer to ERD-adherent hospitals cannot otherwise differ systematically on unobserved characteristics that also affect contraception use (after controlling for patient, institutional, and geographic features of the empirical setting). The exclusion restriction requires that the differential distance instrument affects the outcomes only through the treatment, with no direct pathway 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 the type of birth hospitalization (ERD-adherent vs. not). While neither assumption can be tested directly, we provide both conceptual and empirical evidence—specifically, balance and negative control outcome (NCO) tests—supporting the plausibility of both assumptions.

4.3.1 Conceptual support

From a conceptual standpoint, several features of our empirical setting support both outcome independence and the exclusion restriction. 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

[27] 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. Our estimate therefore places greater weight on patients whose hospital choice is most sensitive to relative geographic proximity.

hospital selection support the outcome 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 our instrument captures quasi-random differences in which hospital type happens to be more geographically convenient.

Second, our use of continuous differential distance instrument (rather than a binary indicator for closest hospital type) provides additional support for outcome independence and the exclusion restriction. Strategic hospital location by Catholic systems might create systematic correlations between ERD-adherent hospitals and area religiosity. However, our instrument exploits variation in *relative* distances within geographic areas. Conditional on our controls (which include county-level measures of religiosity), the remaining variation in differential distance captures which hospital type happens to be marginally closer to a given patient—variation more likely to reflect geographic randomness rather than systematic patient sorting. This fine-grained, continuous IV is less susceptible to confounding from strategic hospital location compared to binary measures of proximity (Lee and Basu, 2025; McClellan et al., 1994).

4.3.2 Balance tests

To empirically provide support for our identification strategy, we 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 our 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 outcome independence assumption.

We implement these balance tests in two steps using linear regression models. First, we 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, we residualize both $\widehat{Y}_{ihmt}$ and the instrument DD_{ihmt} 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.[28]

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 outcome 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.

4.3.3 Negative Control Outcome tests

To further assess the validity of our identification strategy, we conduct Negative Control Outcome (NCO) tests following the framework established by Danieli et al. (2023), which adapts established epidemiological methods to instrumental variable designs to evaluate outcome independence (Shi et al., 2020). We estimate reduced-form regressions of six NCOs on differential distance, using the same controls as in our main specification as described in Section 3. NCOs were selected to test for specific sources of potential selection bias. We first test *marital status*, which serves as a proxy for individual religiosity.[29] Second, we examine three clinical diagnoses that may be related to underlying health status or obstetric complexity, but should not be causally affected by ERD restrictions: *asthma*, *stillbirth*, and *prolonged pregnancy* (defined as delivery after 42 weeks gestation).[30] Finally, NCOs of *parity* and *age at first*

[28] 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.

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

[30] The causes of stillbirth and prolonged pregnancy are unknown. The most common cause of prolonged pregnancies is inaccurate dating. Common risk factors for "true" prolonged pregnancy include primiparity, previous postterm pregnancy, male fetus, and obesity (Galal et al., 2012). Possible causes of (or contributors to) stillbirth include pregnancy with twins or triplets, placental abruption, fetal genetic problems, and infection (Eunice Kennedy Shriver National Institute of Child

observed birth test whether patients living closer to ERD-adherent hospitals have different reproductive histories or fertility preferences.[31] Appendix Table A5 shows no statistically significant associations between differential distance and each of these six NCOs, with uniformly small and insignificant coefficients. The absence of significant associations across these diverse NCO tests provides evidence that our instrument is not capturing unobserved patient characteristics that would threaten the validity of our design. Overall, these null results strengthen confidence that the large, significant effects we observe for our main study outcomes reflect the causal impact of hospital ERD adherence rather than patient selection or other confounding factors.

5 Descriptive results

5.1 Sample characteristics

Table 3 presents summary statistics of our 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), and the majority (approximately half) of patients in both hospital types are White, with a slightly higher percentage of patients in ERD-adherent hospitals being White (55%) relative to non-ERD-adherent hospitals (48%). Approximately half of patients in both types of hospitals are privately insured, with Medicaid enrollment slightly lower in ERD-adherent hospitals (42% vs. 45%). Approximately 20% of patients in both hospital types are married. Over two-thirds of total discharges are from patients living in urban counties, with a slightly lower share in ERD-adherent hospitals (67% vs. 74%). Patients are also similar on observable clinical characteristics. Patients discharged from ERD-adherent hospitals were slightly healthier than those discharged from non-ERD-adherent hospitals (-0.24 Elixhauser score vs. -0.16).[32] The average distance from the patient's residence to birth hospital was consistently 9 miles (SD: 12-13) across hospital types. This aligns with previous research by Fontenot et al. (2024), which found that the average distance from census tract centroid to obstetric hospitals was 8.3 miles, which

Health and Human Development, 2023).

[31] Age at first birth and parity analyses use our longitudinal subsample with unique patient identifiers (see Appendix Table A3). These variables capture births observed in our 2010–2023 data; for women whose first birth preceded 2010, we observe age at first *observed* birth, introducing potential left-censoring. This measurement error would only threaten our conclusions if correlated with differential distance, which is unlikely given our instrument's quasi-random variation.

[32] Negative Elixhauser scores indicate reduced chance of mortality (van Walraven et al., 2009; Li et al., 2008).

corresponds to slightly over 14 minutes of drive time.[33]

Panel B reports hospital characteristics. A higher proportion of ERD-adherent discharges were from nonprofit hospitals (97% vs. 75%). More ERD-adherent hospitals were part of a health system relative to non-ERD-adherent hospitals (87% vs. 71%). 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 our 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. Hospital concentration is similar across hospital types, indicating that ERD-adherent and non-ERD-adherent hospitals face comparable levels of market competition, which helps ensure that differences in service provision are not driven by competitive pressures.

Panel C reports market-level characteristics. Importantly, we find that patients discharged from ERD-adherent hospitals came from counties with slightly lower per capita Catholic adherents and fewer Catholic congregations, on average, compared to patients discharged from non-ERD-adherent hospitals. While counterintuitive, this finding strengthens our identification strategy by suggesting that patient religiosity is unlikely to be an unobserved confounder affecting both hospital proximity and reproductive health outcomes. In total, the sample includes 9,013,727 discharges, with non-ERD-adherent hospitals accounting for 84.6% (n=7,627,452) of discharges and ERD-adherent hospitals representing 15.4% (n=1,386,275)—proportions that align with previous research (Solomon et al., 2020; Meille and Monnet, 2024). Finally, our sample includes 137 unique ERD-adherent and 729 non-ERD-adherent hospitals.[34] Appendix Figure A1 shows the geographic distribution of hospitals in our study sample by ERD-adherence. Visual inspection suggests that ERD-adherent hospitals are dispersed throughout the states in our sample rather than geographically clustered, supporting the plausibility of our identification strategy.

In Table 4 we explore differences in the occurrence of our outcomes between hospital types (ERD-adherent vs. not). The mean rate of permanent contraception is more than double in non-ERD-adherent compared to ERD-adherent hospitals (6.63% vs. 2.96%). Similarly, the rate of non-permanent contraception provision is much higher in non-ERD-adherent compared to ERD-adherent hospitals (4.29% vs. 1.61%). Rates of short-interval pregnancies, within 6, 12, and 18 months of an index birth, are similar

[33] The mean distance traveled was 13 and 8 miles for rural and urban patients, respectively, in our sample (SD: 18, 10, respectively). This also aligns with estimates from previous research (Fontenot et al., 2024).

[34] ERD status defined using the first year each hospital appears in the dataset.

across hospital types.

5.2 Complier characteristics

To further explore the effects of differential distance and assess the external validity of our instrumental variable analysis, we profile the characteristics of compliers—the subpopulation whose hospital choice is influenced by differential distance and for whom the LATE is identified. We contrast compliers with "always-takers," patients who deliver at ERD-adherent hospitals regardless of proximity, indicating strong preferences that override distance considerations. We characterize these subpopulations using a binary version of our instrument with the method developed by Marbach and Hangartner (2020), which builds on the principal stratification framework of Angrist et al. (1996).[35]

Appendix Table A6 reports the results. A comparison of the demographic characteristics of the full sample (column 1) and the compliers (column 2) shows that the compliers are less likely to be white, more likely to have Medicaid, and more likely to live in urban areas. Compliers were slightly less healthy than the full sample, evidenced by the higher Elixhauser score. In addition, more compliers had an emergency or urgent (vs. elective) admission, indicating that time-sensitive obstetric situations drive proximity-based hospital choices. In contrast, compared to the full sample, always-takers were more likely to be white, less likely to be Medicaid, less likely to live in an urban area, and less likely to be admitted on an emergency/urgent basis.

Taken together, these results suggest that the complier population is composed of lower-SES, racially diverse, urban patients experiencing urgent obstetric situations who are more likely to deliver at the nearest hospital due to time and resource constraints. Always-takers, in contrast, generally have more resources and are able to make planned hospital choices independent of proximity constraints. These findings are consistent with other evidence suggesting less advantaged families are more more affected by distance constraints (Card et al., 2023; Fontenot et al., 2024).

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

6 Main Results

6.1 Immediate postpartum contraception

Table 5 presents the effect of ERD-adherent hospitalization on provision of postpartum contraception. OLS estimates in Column (1) show a strong negative association between ERD-adherent hospitalization and the provision of both types of contraception: admission to an ERD-adherent hospital is associated with a 3.83 percentage point (pp) and 2.51 pp reduction in the likelihood of receiving permanent and non-permanent contraception, respectively ($p < 0.01$). Relative to their respective sample means, these associations correspond to a 63% and 65% decrease.

Column (2) reports the reduced form estimates, which show the direct relationship between the instrument and the outcomes. The instrument is positively and significantly associated with sterilization: after rescaling from 1-mile increments, a 10-mile increase in the differential distance to the nearest ERD-adherent hospital is associated with a 0.22 pp ($p < 0.01$) increase in the probability of receiving postpartum sterilization. For non-permanent contraception, the coefficient is also positive and statistically significant: a 10-mile increase in differential distance is associated with a 0.08 pp ($p < 0.1$) increase in the probability of receiving non-permanent contraception. Overall, these results confirm that the instrument is correlated with the outcomes in the expected direction, with a stronger relationship for sterilization.

The 2SLS results are reported in Column (3). Overall, the 2SLS estimates are notably close in magnitude and significance to the OLS associations in Column (1), strengthening our confidence that the relationship is robust. For permanent contraception (Panel A), we find that admission to an ERD-adherent hospital causes a 4.23 pp ($p < 0.01$) decrease in the probability of receiving the procedure—a substantial 70% reduction relative to the sample mean of 6.06%.[36] The 2SLS estimate is slightly larger in magnitude than the OLS estimate of -3.83 pp, suggesting modest attenuation bias in the OLS specification: patients who prefer not to use permanent contraception may be somewhat more likely to seek care at ERD-adherent hospitals, biasing OLS estimates toward zero. For non-permanent contraception (Panel B), we estimate a decrease of 1.61 pp ($p < 0.05$) in Column (3), corresponding to a 41% reduction relative to the mean of 3.88%. The non-permanent contraception estimate is slightly smaller

[36] Importantly, we find no statistically significant relationship between admission to an ERD-adherent hospital and the probability of Cesarean section (coefficient = 0.0075 pp; SE = 0.03, p = 0.80), ruling out differential mode of delivery as an explanation for lower contraception rates at ERD-adherent hospitals.

in magnitude than the OLS estimate of -2.51 pp in Column (1), suggesting that unobserved selection patterns may differ by contraception type. Taken together, the OLS and 2SLS results both point to a strong, negative causal relationship between giving birth at an ERD-adherent hospital and receipt of postpartum contraception.

6.1.1 Effect heterogeneity: Rural vs. urban patients

The density and competitiveness of hospital markets differ 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 for rural populations given the lower density of healthcare providers. Therefore, we explore heterogeneity in the effects of ERD-adherent hospital admission on contraception provision by patient location, estimating separate models for rural and urban sub-groups.

The 2SLS estimates for the rural patient sub-group are reported in Column (1) of Table 6. We find that admission to an ERD-adherent hospital reduces the likelihood of receiving sterilization by 5.95 pp (Panel A). This estimate is highly significant ($p < 0.01$) and represents a substantial 80% decrease relative to the rural sample mean of 7.41%. For non-permanent contraception (Panel B), we find a similarly large and significant effect for rural populations: a 3.65 pp reduction ($p < 0.01$), which corresponds to a 83% decrease relative to the mean of 4.41%. The instrument is strong in the rural sub-group, with a first-stage F-statistic of 68.38. These effect magnitudes are considerably larger than those for the full sample,[37] highlighting that rural patients are disproportionately affected by religious service restrictions.

Column (2) of Table 6 presents the results for the urban patient sub-sample. We find that admission to an ERD-adherent hospital reduces the likelihood of receiving sterilization by 5.06 pp ($p < 0.05$). Relative to the urban sample mean of 5.58%, this represents a 91% reduction for the urban complier population. The first-stage F-statistic for the urban population is 20.90, suggesting that the predictive power of differential distance on hospital choice is weaker in urban populations relative to rural (though still above conventional levels required for a first stage relationship).[38] In contrast to rural populations, the effect

[37] The LATE of ERD-adherent hospitalization on permanent contraception provision in the full sample is -4.23 pp (70% relative decrease to the mean), compared to -5.95 pp for the rural sub-sample (80% relative decrease). For non-permanent contraception, the LATE is -1.61 pp (41% relative decrease), compared to -3.65 pp for the rural sub-sample (83% relative decrease).

[38] The weaker first stage in urban areas likely reflects several factors. First, urban patients have access to a denser hospital network, meaning small differences in distance may be less determinative of hospital choice. Second, we measure

on non-permanent contraception for urban patients is effectively zero and statistically insignificant. This suggests that while access to sterilization is significantly curtailed for compliers in both rural and urban areas, urban patients may have more alternative pathways or opportunities to receive non-permanent methods, therefore mitigating the impact of ERD-adherent hospitalization on this specific outcome. Overall, the heterogeneity in results by rural vs. urban patient county of residence underscores the unique vulnerability of rural populations, for whom the birth hospitalization represents a more critical and less replaceable point of care for all forms of contraception.

In Appendix Table A7, we present estimates of the effect of ERD-adherent hospitalization on provision of postpartum contraception using two alternative rural-urban classifications. Results are very similar. When we use the OMB definition of urban, compared to the NCHS definition in our main results, we find nearly identical patterns: large, significant reductions in both sterilization (6.39 pp) and non-permanent contraception (3.20 pp) for rural patients, while urban patients experience significant sterilization reductions (4.20 pp) but minimal effects on non-permanent contraception. Using obstetric provider concentration to define rural-urban status yields consistent results, with rural patients experiencing 5.44 pp reductions in sterilization and 2.86 pp reductions in non-permanent contraception, while urban effects on non-permanent contraception remain small and insignificant. These robustness checks confirm that our core finding—rural patients are disproportionately affected by ERD-adherent hospital service restrictions—is not sensitive to the specific definition of rurality.

6.1.2 Effect heterogeneity: Private vs. Medicaid insurance

Insurance coverage policies may mediate the relationship between hospital religious restrictions and contraception access. Throughout our study period, Medicaid coverage of postpartum contraception evolved, especially coverage of immediate postpartum LARC.[39] While our 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. 45%; Table 3). We therefore estimate separate models for privately-insured and Medicaid patients to assess whether insurance coverage policies interact with religious restrictions in ways that our main specification does not fully capture.

differential distance using great-circle distance rather than drive time. In urban areas with complex road networks and traffic congestion, great-circle distance may be a noisier proxy for actual travel burden compared to rural areas.

[39]Throughout our 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).

Table 7 presents 2SLS estimates stratified by insurance type. For privately-insured patients, ERD-adherent hospitalization reduces permanent contraception by 4.16 pp (82% relative to mean, $p < 0.001$) and non-permanent contraception by 1.96 pp (67% relative to mean, $p < 0.001$). For Medicaid patients, permanent contraception also decreases by 4.16 pp (58% relative to mean, $p < 0.001$) when giving birth at an ERD-adherent hospital vs. not. The effect on non-permanent contraception is smaller and not statistically significant for Medicaid patients.

The null result for Medicaid non-permanent contraception (which includes LARC) likely reflects that reimbursement barriers, rather than religious restrictions, represented a binding constraint for much of our study period. All hospital types, with or without religious restrictions, faced financial disincentives to provide immediate postpartum LARC to Medicaid patients under bundled payment systems. In contrast, permanent contraception—which was consistently covered by both Medicaid and private insurance—shows comparable, significant effects across insurance types.

6.2 Short-interval pregnancy

A critical downstream consequence of unfulfilled demand for contraception is an increased risk of short-interval pregnancy (SIP). We investigate this relationship by examining the effect of delivering at an ERD-adherent hospital vs. non-ERD-adherent on the risk of SIP. Table 8 presents the 2SLS estimates for SIP occurring within 6 months (Panel A), 12 months (Panel B), and 18 months (Panel C) of the index birth. For these analyses, we use a subsample of patients with unique identifiers that we can follow across hospitals over time (n=5,217,931).[40]

In the full longitudinal sample (Table 8, Column 1), we 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 they are not precisely estimated. However, when stratifying by geographic location, a clear pattern emerges that aligns with our previous postpartum contraception findings. Rural patients show statistically significant increases in SIP rates after giving birth at a hospital that adheres to the ERDs (vs. not). Specifically, rural patients are 0.96 pp ($p < 0.05$) more likely to be pregnant within 6 months after giving birth at a hospital that adheres to the ERDs compared to one that does not. Relative to the mean of 3.77%, this represents a 25% increase. Results

[40] Appendix Table A9 reports effects from estimating our main immediate postpartum contraception results using this smaller subsample. Results are generally consistent, though the smaller sample size renders the effect on non-permanent contraception non-significant (though the magnitude of the estimated effect is similar to the effect size in our main specification).

are consistent across varied lengths of short-interval pregnancies. Rural patients experience a relative 2.18 pp increase ($p < 0.05$) in pregnancies within 12 months of the index birth, and a relative 3.81 pp increase ($p < 0.05$) in pregnancies within 18 months of the index birth after delivering at an ERD-adherent vs. non-ERD-adherent hospital. Compared to the baseline means for rural populations, these changes represent an increased relative risk of 26% and 29% for pregnancies within 12 and 18 months, respectively.

In contrast, urban patients show no statistically significant effects, with small positive coefficients that are not meaningfully different from zero. This geographic heterogeneity directly mirrors our 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.

In Appendix Table A8, we present estimates of the effect of ERD-adherent hospitalization on short-interval pregnancy using two alternative rural-urban classifications. Results confirm the robustness of our main findings. When we use the OMB definition of urban, compared to the NCHS definition in our main results, we see a similar pattern: rural patients experience significant increases in the likelihood of short-interval pregnancies at 6, 12, and 18 months after the index birth. Specifically, rural patients are 1.14 pp ($p < 0.10$, 30% relative to the mean), 3.24 pp ($p < 0.05$, 38% relative to the mean), and 5.19 pp ($p < 0.10$, 40% relative to the mean) more likely to be pregnant at 6, 12, and 18 months, respectively, after the index birth. Urban patients show no significant effects. Similarly, when defining rurality by obstetric provider concentration, we continue to find significant increases in the likelihood of SIP for rural patients (2.04 pp, 25% relative increase for <12 months; 3.93 pp, 30% relative increase for <18 months). These robustness checks provide strong evidence that our main finding—rural patients experience measurable downstream reproductive health consequences from ERD service restrictions—is robust.

6.3 Additional robustness checks

We present four additional robustness checks in Appendix Tables A10-A14. First, we 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 (Solomon et al., 2020; Hill et al., 2019; 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 Table A10 shows that results are consistent, but slightly lower in magnitude, which is expected given the less strict treatment definition. For example, in our main analyses, ERD-adherent hospitalization caused a 70% decrease in likelihood of permanent contraception provision, compared to 67% decrease for Catholic-affiliated hospitalization.

Second, we re-estimate our 2SLS specification using the untrimmed DD instrument. In our main analyses, we excluded 1.2% 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 Table A11 reports estimated effects using the untrimmed sample that includes DD up to a maximum of 206.8 miles. Results are qualitatively similar, indicating that our findings are robust to DD trimming decisions. Notably, the first stage F-statistic for the rural population is much lower compared to our main trimmed sample (37.48 vs. 68.38), 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, we re-estimate our 2SLS specification excluding hospital-level covariates to address concerns that controlling for these characteristics could introduce post-treatment (collider) bias. Appendix Table A12 presents these results. The estimated effects remain substantively similar to our main specification: ERD-adherent hospitalization reduces permanent contraception by 4.08 pp (67% relative to the mean) and non-permanent contraception by 1.85 pp (48% relative to the mean), compared to 4.23 and 1.61 pp in our main specification that includes hospital controls. Estimates are also similar for rural and urban sub-groups. The first-stage F-statistic is somewhat lower without hospital controls (62.09 vs. 79.49), reflecting the expected loss in precision from excluding hospital-level covariates.

Fourth, we re-estimated our postpartum contraception and short-interval pregnancy results (Appendix Tables A13 and A14, respectively) with additional covariates: number of OB/GYNs per 100,000 population and number of abortion clinics, both measured per patient county per year.[41] Controlling for OB/GYN presence and abortion clinics (most of which also offer other reproductive healthcare, such as contraception) is meant to strengthen the interpretation of our findings as reflecting the specific impact

[41]We calculated number of obstetrician-gynecologist physicians per 100,000 population per year using county-level data from HRSA (2024). For abortion clinic information, we requested restricted-use data from Dr. Caitlin Myers' Abortion Facility Database (Myers, 2025). The average number of OB/GYNs per 100,000 was 13.22 (SD: 8.16), and the average number of abortion clinics was 4.40 (SD: 4.46).

of hospital-level religious restrictions rather than broader geographic access to reproductive healthcare providers. We do not include these measures in our main specifications to avoid potential endogeneity (as provider location decisions may themselves be influenced by the presence of ERD-adherent hospitals) and collinearity with patient urbanicity measures (as provider and clinic density are correlated with urbanicity). However, including them as a robustness check tests whether our effects are driven by provider-level barriers to reproductive healthcare.

Appendix Tables A13 and A14 show that results remain robust across both contraception and pregnancy outcomes. For postpartum contraception, ERD-adherent hospitalization reduces permanent contraception by 3.90 pp overall (vs. 4.23 pp in the main specification) and 5.29 pp for rural patients (vs. 5.95 pp), with similar patterns for non-permanent contraception. The slight attenuation is expected when absorbing additional variation through provider supply controls. For short-interval pregnancies, the statistically significant rural estimates are stable and actually slightly larger: 0.99 pp for <6 months (vs. 0.96 pp in the main specification), 2.35 pp for <12 months (vs. 2.18 pp), and 4.16 pp for <18 months (vs. 3.81 pp). The pattern of results—modest attenuation for contraception outcomes coupled with maintained or slightly larger effects for downstream pregnancy outcomes—is reassuring, confirming that our results are driven by hospital-level restrictions rather than a lack of alternative providers in the surrounding area.

7 Discussion

This study seeks to answer a fundamental question facing the U.S. healthcare system: what are the consequences when access to reproductive healthcare is constrained? As demand for immediate postpartum contraception increases (especially post-*Dobbs*) and access to such services declines, understanding the consequences are crucial for both patient welfare and policy design. Hospital-level service restrictions (imposed via religious directives) in particular are important to examine, as receiving contraception during the birth hospitalization represents a critical point of access: it is medically efficient, cost-effective, and eliminates the need for subsequent appointments or recovery periods. 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.

We provide causal evidence that hospital adherence to the Ethical and Religious Directives creates substantial barriers to contraception access with measurable downstream health consequences. Delivery at an ERD-adherent hospital reduces the likelihood of receiving permanent contraception by 4.23 percentage points (70% relative to the mean) and non-permanent contraception by 1.61 percentage points (41%). These effects are concentrated among rural patients, for whom ERD-adherent hospitalization reduces permanent contraception by 5.95 pp (80%) and non-permanent contraception by 3.65 pp (83%). Critically, we also document downstream health consequences: rural patients who delivered at an ERD-adherent hospital were 3.81 pp more likely to be pregnant within 18 months of the index birth (29% relative increase), compared to delivering at a non-ERD-adherent hospital. Concerningly, rural patients also had 25% increased odds of very short-interval pregnancies (<6 months), which carry the highest risks for severe adverse maternal and infant health outcomes.

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 reproductive services, 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, our 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 5, the complier population is composed of more low-income, racially diverse, and urban 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). Our estimates may not generalize to planned deliveries or higher-SES patients, but suggest that Catholic hospital expansions may disproportionately impact vulnerable patients with fewer resources (e.g., time or money).

We also note that a slightly larger proportion of compliers live in urban areas compared to the full sample (77.6% vs. 73.3%), but treatment effects are substantially larger for rural compliers. This pattern reflects a critical distinction between who responds to the instrument (complier composition) and the magnitude of harm experienced when treated (treatment effect size). On average, urban patients' hospital choices are slightly more responsive to marginal differential distance (making them more likely to be compliers), but rural compliers experience larger treatment effects—when denied contraception at the birth hospitalization, they have fewer alternative access points for postpartum contraception.

Our findings extend prior research on this topic. Hill et al. (2019) found a 31% decline in tubal ligation rates following hospital conversion to Catholic ownership, while Meille and Monnet (2024) found that living in a zip code where the closest hospital was Catholic-affiliated reduced the probability of postpartum sterilization by 0.95 pp (14% relative decrease). Our estimates are considerably larger—a 70% overall decrease in permanent contraception and an 80% decrease for rural patients. These differences likely stem from several factors. First, both prior studies define treatment more broadly (Catholic affiliation rather than strict ERD adherence), potentially diluting effects. As discussed in Section 2 and Appendix A.1, not all hospitals affiliated with a Catholic health systems adhere to the ERDs. Second, Hill et al. (2019) estimate per-bed averages using a difference-in-differences approach, while Meille and Monnet (2024) employ an intent-to-treat design (e.g., only 21 pp more of the "exposed" group actually delivered at a Catholic hospital, likely attenuating estimates through non-compliance). Our instrumental variables approach, by contrast, identifies the causal effect of actual ERD-adherent hospitalization. The rural-urban heterogeneity we document represents a novel contribution, as previous studies have not examined how effects vary by geographic context.

Similarly, our causal estimates of adverse downstream outcomes stemming from religious restrictions represent a new contribution to the literature. Using Medicaid claims data, Liu et al. (2025) and Caldwell et al. (2022) found associations between delivering at Catholic-affiliated hospitals and a 5-12% increased risk of short-interval pregnancy. Our estimated 29% increase for rural patients is more than twice these magnitudes, consistent with the presence of selection bias in prior observational studies. Our instrumental variable approach corrects for this endogeneity, isolating a larger causal effect for the complier population.

The mechanism driving these effects is straightforward: the Ethical and Religious Directives explicitly prohibit hospitals from providing services that "render procreation impossible," including sterilization and contraception. Conversations with providers at ERD-adherent hospitals confirm that postpartum sterilization is rarely performed outside of exceptional circumstances requiring approval of the local bishop and hospital ethics board (Freedman, 2023). For example, one obstetrician mentioned that when patients have a family history of ovarian cancer, postpartum sterilization may be permitted, but only with substantial advance planning and institutional approval.[42]

[42]This insight stems from 22 key informant interviews the primary author conducted with clinicians and administrators working at ERD-adherent hospitals. The strict prohibition on sterilization, except in rare circumstances related to a

This strict adherence to religious doctrine, even when it conflicts with patient preferences, provide empirical evidence regarding theories of nonprofit hospital behavior. While economic theory often models nonprofits as solving market failures by prioritizing quality or access over profit (Sloan, 2000; Newhouse, 1970), we document a case where mission adherence actively generates negative externalities. By restricting services to align with religious directives, ERD-adherent hospitals impose significant costs on patients—specifically, unfulfilled demand for contraception and increased health risks from short-interval pregnancies. Unlike prior studies that find little difference between nonprofit and for-profit behavior in hospital charity care or pricing (Duggan, 2002; Bruch and Bellamy, 2021), our analysis isolates a dimension where mission-driven constraints lower patient welfare.

Although our instrumental variable design addresses patient-level selection, a key study limitation is that patient-level instruments cannot fully eliminate confounding from hospital-level organizational factors (Konetzka and Werner, 2025). While our balance tests demonstrate that differential distance does not predict patient characteristics (Section 4.3), the instrument does not randomize hospitals themselves. Therefore, it is possible that ERD-adherent hospitals differ from non-ERD-adherent hospitals on unobserved dimensions—such as institutional culture or staffing models—that are correlated with both hospital religious restrictions and patient outcomes. While we control for a wide range of observable hospital characteristics to mitigate this concern, we cannot rule out the possibility that our estimates capture both the direct effect of ERD adherence and these 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, our estimates reflect the treatment effect of giving birth at an ERD-adherent hospital. However, our results may not predict the effects of external policy changes that alter ERD status independently, such as state mandates requiring service provision. Such interventions would change religious restrictions without necessarily changing underlying organizational characteristics, and thus could have different effects than we estimate. This caveat does not diminish the relevance of our findings for understanding current patient experiences, but suggests caution in extrapolating to counterfactual policy scenarios.

woman's family history or clinical diagnosis, was a common theme throughout the qualitative interviews.

Our findings on the effect of hospital-level service restrictions have heightened urgency amid broader shifts in the U.S. healthcare landscape. Catholic health systems continue to expand—growing by over 20% in the last two decades—while obstetric services contract, particularly in rural regions experiencing widespread hospital and maternity unit closures. Simultaneously, post-*Dobbs* restrictions have narrowed outpatient abortion and contraception access in many states, and impending Medicaid funding cuts threaten rural hospitals' financial stability. Together, these trends are compressing the already-limited opportunities for reproductive healthcare access, making the birth hospitalization an increasingly critical point of care. In this context, hospital religious restrictions operate not in isolation but as one barrier among many. Our results underscore the need for greater recognition and transparency regarding how religious directives shape patient care, particularly as these restrictions affect a growing share of U.S. births at a moment when alternative access points are disappearing.

More specifically, our findings have direct implications for ongoing policy debates in two key areas: the regulation of healthcare consolidation and the design of hospital transparency requirements (see Appendix Table A1). First, when Catholic-acquired hospitals are required to adhere to the ERDs, the resulting changes in service provision represent an important dimension of consolidation that has received limited attention in traditional merger analysis. Our evidence suggests that regulators should incorporate assessments of how consolidation will affect the availability of essential services, especially in rural markets where a single hospital merger or acquisition can effectively eliminate local access to certain forms of reproductive healthcare. Second, our results highlight information asymmetries as a source of market failure. Prior research documents that patients are largely unaware of hospital religious restrictions, limiting their ability to make informed choices. Policy responses such as Washington's and Colorado's transparency requirements attempt to correct this market failure by providing accessible information about service restrictions. By providing patients with accurate, accessible information, such policies can enhance patient autonomy and mitigate the welfare losses associated with unexpected denials of care.

In summary, this study provides causal evidence that giving birth at an ERD-adherent hospital significantly reduces access to postpartum contraception and increases the risk of short-interval pregnancies, especially for rural populations. By addressing the endogeneity of patient choice, our findings circumvent methodological challenges to demonstrate the direct impact of religious restrictions on care. As Catholic health systems continue to expand, policymakers must balance institutional conscience protections with the broader goal of patient welfare.

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8 Tables

Table 1: Study sample by state and year (n=9,013,727)

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	74,856	73,724	74,838	74,188	75,344	74,438	73,461	67,785	70,104	–	–	–	–	–
CO	60,871	60,018	59,997	59,472	60,770	61,371	61,465	58,969	–	–	–	–	–	–
FL	196,838	199,478	199,543	201,291	206,555	209,552	209,939	208,145	206,572	–	–	–	–	–
KY	–	–	–	–	–	–	–	48,868	–	–	–	–	–	–
MD	–	–	–	–	–	–	–	–	62,919	59,264	60,567	56,727	60,854	57,879
NJ	100,139	98,951	98,184	96,625	97,530	97,031	96,554	93,880	93,812	90,996	88,491	91,682	92,846	90,990
NY	224,136	223,846	229,451	224,020	226,441	223,819	218,428	217,298	213,413	203,396	192,030	188,667	186,828	–
OR	42,905	42,133	40,758	40,651	41,875	42,167	42,158	40,908	–	–	–	–	–	–
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	–
WI	–	–	–	–	–	–	–	59,022	–	–	–	–	–	–
Total	792,475	793,000	795,869	788,809	787,096	822,007	815,288	874,044	723,489	429,542	415,678	412,400	415,161	148,869

Table 2: First Stage: Effect of Differential Distance on ERD-Adherent Hospital Admission

	ERD-adherent hospital
Differential distance	-0.00507***
	(0.000568)
Observations	9,013,727
F	79.49

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 presents first-stage regression results from the 2SLS-IV model. The dependent variable is an indicator equal to 1 if the patient was admitted to an ERD-adherent hospital, and 0 otherwise. Differential distance is measured in miles as the distance from the patient's zip code centroid to the nearest ERD-adherent hospital minus the distance to the nearest non-ERD-adherent hospital. The specification includes patient-, hospital-, market-level controls, and state-by-year fixed effects.

Table 3: 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	55	49
Black	14	12	14
Hispanic	22	19	21
Asian/PI	7	6	7
Other/missing	10	9	10
Insurance type (%)			
Private	50	52	50
Medicaid	45	42	44
Self pay/other	6	6	6
Married (%)	22	19	22
Living in urban area (%)	74	67	73
Clinical characteristics			
Alcohol/drug abuse (%)	2	3	2
Chronic pulmonary disease (%)	5	5	5
Depression (%)	3	3	3
Diabetes (%)	0.87	0.82	0.86
Hypertension (%)	0.37	0.26	0.35
Hypothyroidism (%)	4	4	4
Obesity (%)	7	7	7
Mean Elixhauser index (SD)	-0.16 (1.98)	-0.24 (2.03)	-0.17 (1.99)
Hospitalization is urgent/emergent (%)	50	41	49
Mean distance traveled to hospital (SD)	9 (13)	9 (12)	9 (13)
Panel B. Hospital characteristics			
Ownership type (%)			
Government	14	0.25	12
Non-profit	75	97	79
For-profit	11	3	9
System member (%)	71	87	73
Presence of obstetrics unit (%)	89	90	89
Mean total annual admissions (SD)	22,134 (11,643)	19,146 (9,055)	21,674 (11,335)
Mean Medicaid patient share (SD)	0.25 (0.12)	0.24 (0.09)	0.25 (0.12)
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)	0.61 (0.32)	0.61 (0.35)
Panel C. Market characteristics			
Mean Catholic adherents (SD)	23 (11)	21 (14)	23 (11)
Mean Catholic congregations (SD)	48 (35)	34 (28)	46 (35)
Total discharges	7,627,452 (84.6%)	1,386,275 (15.4%)	9,013,727 (100.0%)
Total hospitals	729 (84.2%)	137 (15.8%)	866 (100.0%)

Notes: Urban counties are those part of large metropolitan areas of 1 million or more residents using the National Center for Health Statistics classification. Mean distance to hospital was calculated in miles from the patient zip code centroid to the site of hospitalization (using hospital latitude and longitude). 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. Catholic adherents per 1,000 population and number of Catholic congregations are defined by patient county. Hospital concentration measured using the Herfindahl-Hirschman Index (HHI) with annual birth volumes per hospital service area (HSA).

Table 4: Outcome Summary Statistics

	Hospital Type		
	Non-ERD-adherent	ERD-adherent	All Hospitals
Panel A. Contraception			
Permanent contraception	6.63%	2.95%	6.06%
Non-permanent contraception	4.29%	1.61%	3.88%
Panel B. Short-interval pregnancy			
Within 6 months	3.18%	3.16%	3.18%
Within 12 months	7.79%	7.62%	7.76%
Within 18 months	12.64%	12.45%	12.62%

Notes: Panel A sample comprised of all birth-related hospitalizations (n=9,013,727). Panel B sample comprised of a sub-set of birth-related hospitalizations with unique patient identifiers to link hospitalizations by patient (n=5,217,931).

Table 5: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision

	(1) OLS	(2) Reduced Form	(3) 2SLS-IV
Panel A. Permanent contraception			
ERD-adherent	-0.0383***		-0.0423***
	(0.0038)		(0.0121)
Differential distance		0.000215***	
		(0.0000571)	
Mean dep. var.	0.0606	0.0606	0.0606
Panel B. Non-permanent contraception			
ERD-adherent	-0.0251***		-0.0161**
	(0.0024)		(0.00817)
Differential distance		0.0000817*	
		(0.0000429)	
Mean dep. var.	0.0388	0.0388	0.0388
Observations	9,013,727	9,013,727	9,013,727
First stage F-stat			79.49

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

Table 6: 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	-0.0595***	-0.0506**
	(0.0137)	(0.0232)
Mean dep. var.	0.0741	0.0558
Panel B. Non-permanent contraception		
ERD-adherent	-0.0365***	-0.0000758
	(0.0090)	(0.0179)
Mean dep. var.	0.0441	0.0369
Observations	2,405,223	6,608,504
First stage F-stat	68.38	20.90

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. Urban counties, defined using patient county of residence and the National Center for Health Statistics (NCHS) urban-rural classification, are those part of large metropolitan areas of 1 million or more residents. Rural is defined as non-urban counties.

Table 7: 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	-0.0416***	-0.0416***
	(0.0104)	(0.0154)
Mean dep. var.	0.0509	0.0716
Panel B. Non-permanent contraception		
ERD-adherent	-0.0196***	-0.0147
	(0.00603)	(0.0107)
Mean dep. var.	0.0291	0.0499
Observations	4,528,677	3,978,114
First stage F-stat	72.06	72.40

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

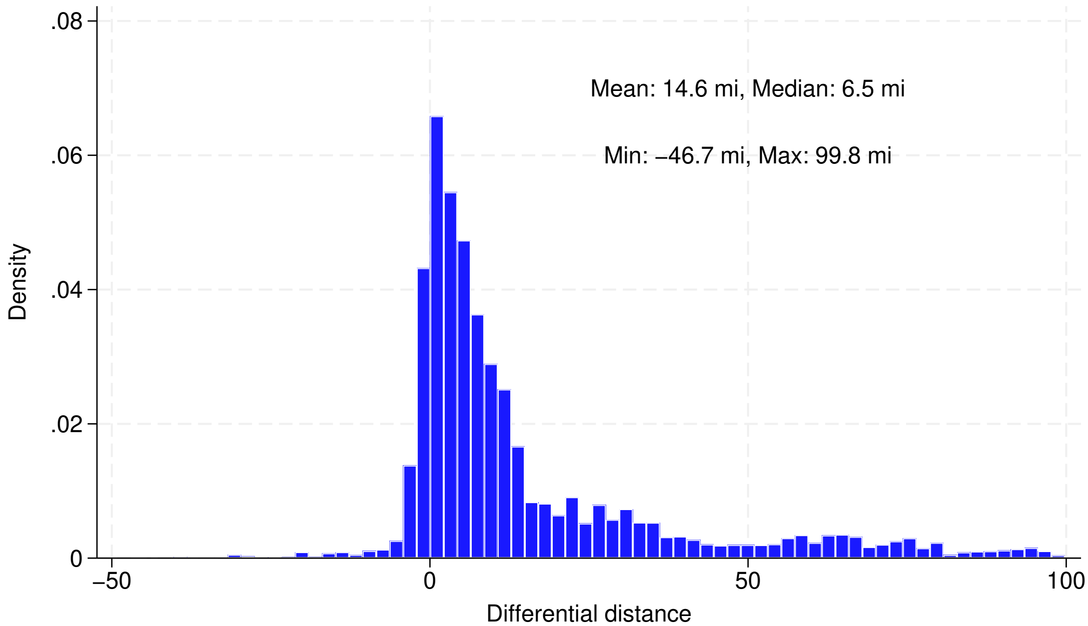
Table 8: The Effect of ERD-adherent Hospitalization on Short-Interval Pregnancy, 2SLS Estimates

	(1) Overall	(2) Rural Patients	(3) Urban Patients
Panel A. <6 months short-interval pregnancy			
ERD-adherent	0.000986	0.00959**	-0.00744
	(0.00437)	(0.00446)	(0.00835)
Mean dep. var.	0.0318	0.0377	0.0295
Panel B. <12 months short-interval pregnancy			
ERD-adherent	0.00747	0.0218**	0.00167
	(0.00902)	(0.00967)	(0.0185)
Mean dep. var.	0.0776	0.0846	0.0749
Panel C. <18 months short-interval pregnancy			
ERD-adherent	0.0129	0.0381**	0.00838
	(0.0143)	(0.0157)	(0.0308)
Mean dep. var.	0.1262	0.1329	0.1235
Observations	5,217,931	1,460,139	3,757,792
First stage F-stat	61.59	50.10	19.73

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 states with unique patient identifiers to allow for longitudinal linkage of patients from the index birth hospitalization to subsequent pregnancies.

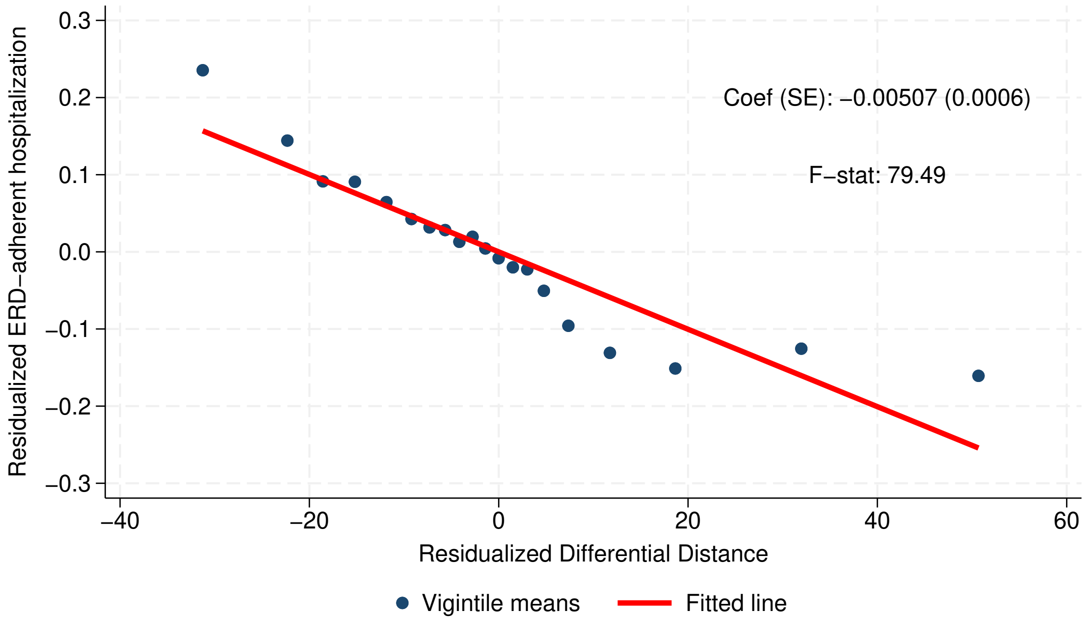
9 Figures

Figure 1: DD instrument distribution, trimmed |DD| < 100



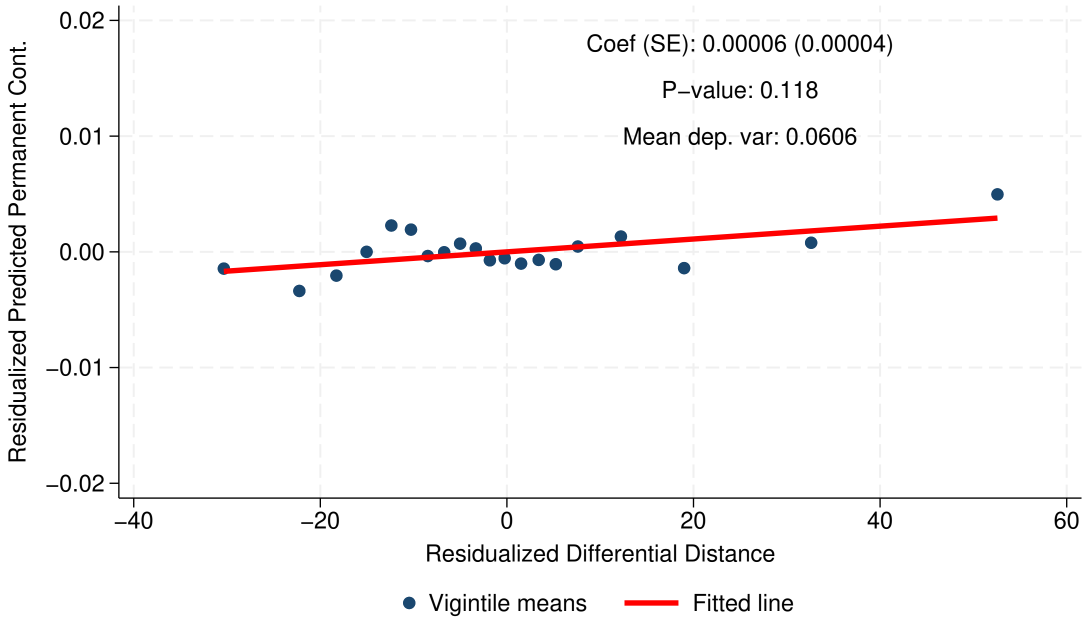
Notes: This figure shows the distribution of the differential distance (DD) instrument for the main analytic sample, which is restricted to observations where the absolute DD is less than 100 miles to ensure common support. The instrument is calculated as the distance from a patient's ZIP code centroid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

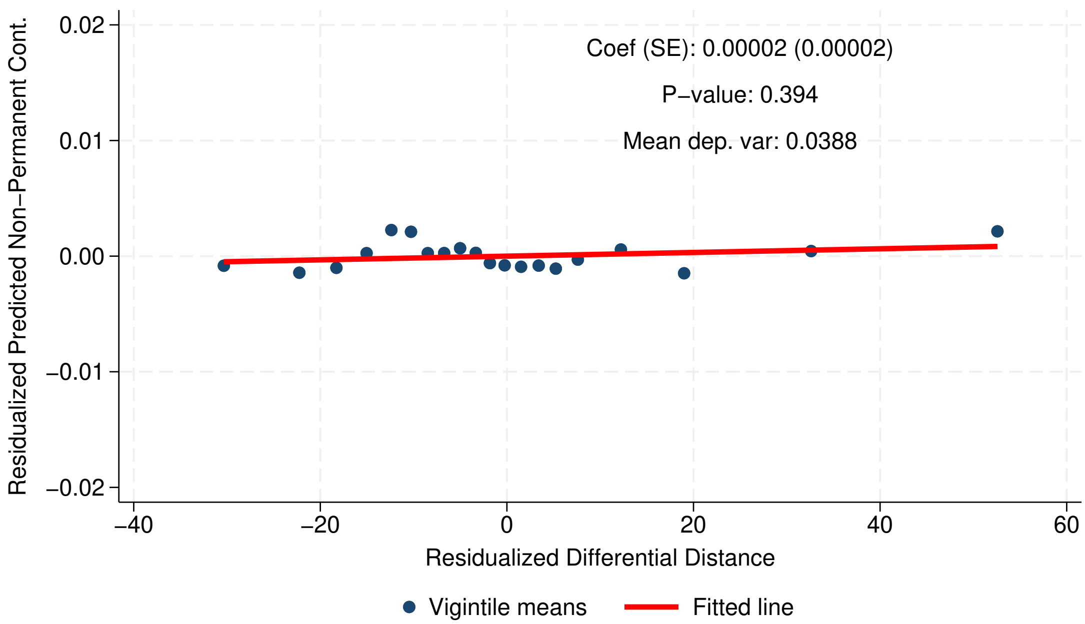


Notes: This figure presents a binned scatterplot illustrating the first-stage relationship between the differential distance (DD) instrument and admission to an ERD-adherent hospital. Both the treatment variable (admission to an ERD-adherent hospital) and the differential distance instrument are residualized 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. $N = 9{,}013{,}727$.

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 outcome independence assumption. The y-axis plots a residualized "risk score," which is the predicted outcome based only on observable patient characteristics. The x-axis plots the residualized differential distance (DD) instrument. Both variables have been residualized on the full set of hospital and market controls, and state-by-year fixed effects.

A Appendix

A.1 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 our 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 approximately 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. Our 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 our 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 our analysis. Our 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, our 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 our measurement approach using similar sources and validation procedures.

A.2 Descriptive Tables

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

State	**Name**	**Description**	**Status**
Connecticut	House Bill 5424	Protects clinicians who work in Catholic institutions and provide referrals or counseling on reproductive healthcare	Proposed in 2024
Washington	Keep Our Care Act, Senate Bill 5241 / House Bill 1263	Increases government oversight of healthcare mergers/acquisitions to ensure access to comprehensive services, including reproductive healthcare	Passed the House but not the Senate in 2023
New York	Hospital Transparency Act, Senate Bill S1003A / Assembly Bill A733A	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 Senate in 2024, vetoed, reintroduced
New Mexico	Hospital Mergers Oversight, House Bill 5	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
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	Requires Catholic hospitals to allow providers to perform abortions in life-threatening situations	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

Appendix Table A2: Sample restrictions

	% Dropped	Remaining N
All births in sample state-years		10,925,859
No patient county or zip code	14.4%	9,352,535
No hospital location	1.7%	9,193,542
Patient state of residence does not match hospital state	0.75%	9,124,591
$\lvert DD \rvert > 100$	1.2%	9,013,727
Total for primary outcomes (postpartum contraception)		**9,013,727**
HCUP-SIDs missing unique patient identifiers	34.1%	5,942,974
Births with insufficient follow-up period	12.2%	5,217,931
Total for for secondary outcomes (short-interval pregnancy)		**5,217,931**

Note: Sample state-years include births from females aged 12-49 in 11 states between 2010-2023 (Arkansas, Arizona, Colorado, Florida, Kentucky, Maryland, New Jersey, New York, Oregon, Washington, and Wisconsin). Not all years were available for each state. State-years in our 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. DD, differential distance, of $>$ 100 corresponds to 2.5-3 hours of drive time. SIDs from five states (Arkansas, Florida, Maryland, New York, Washington) include unique patient identifiers allowing researchers to track patients across hospitals over time. We dropped births in the last 2 years to allow sufficient follow up time to track short-interval pregnancies, defined as a pregnancy within 18 months of a previous birth.

Appendix Table A3: Sample of Birth Hospitalizations with Unique Patient Identifiers (n=5,217,931)

State	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021
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	–	–	–
MD	–	–	–	–	–	–	–	–	62,911	59,263	60,562	56,722
NY	222,103	221,877	227,516	222,386	224,596	221,911	216,566	214,311	204,906	194,563	185,060	–
WA	77,552	77,514	76,138	75,897	78,581	79,654	81,065	79,169	76,669	75,886	74,590	–
Total	493,277	498,522	502,206	497,075	489,736	519,580	512,352	475,282	523,255	329,712	320,212	56,722

Appendix Table A4: Sample Observations in ERD-adherent and Catholic-affiliated Hospitals, 2010-2023

Year	**ERD-adherent hospitals**	**Catholic-affiliated hospitals**
2010	121,485 (15.3%)	129,051 (16.3%)
2011	115,924 (14.6%)	124,963 (15.8%)
2012	120,779 (15.2%)	129,706 (16.3%)
2013	117,764 (14.9%)	126,373 (16.0%)
2014	114,283 (14.5%)	138,887 (17.7%)
2015	122,612 (14.9%)	141,992 (17.3%)
2016	122,745 (15.1%)	143,977 (17.7%)
2017	141,224 (16.2%)	172,203 (19.7%)
2018	100,786 (13.9%)	119,449 (16.5%)
2019	72,496 (16.9%)	77,033 (17.9%)
2020	72,810 (17.5%)	75,581 (18.2%)
2021	67,806 (16.4%)	70,739 (17.2%)
2022	68,381 (16.5%)	71,339 (17.2%)
2023	27,180 (18.3%)	27,180 (18.3%)
Total	1,386,275 (15.4%)	1,548,473 (17.2%)

Notes: This table presents the proportion of birth hospitalizations in our sample that occurred at hospitals required to adhere to the Ethical and Religious Directives for Catholic Health Care Services (ERDs) and those that occurred at Catholic-affiliated hospitals that may or may not be required to adhere to the ERDs.

Appendix Table A5: Negative control outcome (NCO) tests

	DD Coefficient
Panel A. Main dataset	
Marital status	-0.0000855
	(0.000158)
Asthma	-0.000000207
	(0.0000000908)
Stillborn	0.0000000885
	(0.00000448)
Prolonged pregnancy	-0.0000116
	(0.000150)
Panel B. Dataset with unique patient identifiers	
Parity	-0.000339
	(0.000450)
Age at first birth	-0.00295
	(0.00362)

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by patient county, are in parentheses. DD, differential distance. This table reports coefficients from reduced-form OLS regressions of the differential distance instrument on six negative control outcomes (NCOs). All models include state-by-year fixed effects and the full vector of patient, hospital, and market-level controls used in the main 2SLS specification. 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 = 9,013,727. Panel B, n = 5,217,712.

Appendix Table A6: Characteristics of Compliers and Always-Takers

	(1) Full Sample	(2) Compliers	(3) Always-Takers
Age (mean)	29.2 (0.0)	29.3 (0.0)	29.2 (0.0)
White (%)	48.7 (0.2)	46.4 (1.1)	53.4 (0.5)
Black (%)	13.9 (0.1)	10.5 (0.8)	13.4 (0.4)
Hispanic (%)	21.3 (0.1)	24.1 (1.1)	19.3 (0.4)
Asian (%)	6.5 (0.1)	9.0 (0.5)	6.1 (0.3)
Private insurance (%)	50.2 (0.2)	46.8 (1.1)	55.3 (0.5)
Medicaid (%)	44.1 (0.2)	47.9 (1.1)	38.8 (0.5)
Urban (%)	73.3 (0.1)	77.6 (1.0)	70.9 (0.5)
Married (%)	21.9 (0.1)	21.2 (0.9)	21.3 (0.4)
Elixhauser score (mean)	-0.2 (0.0)	-0.0 (0.0)	-0.2 (0.0)
Emergency/urgent (%)	49.0 (0.2)	66.8 (1.1)	40.5 (0.5)

Note: This table reports average characteristics for the overall sample, compliers, and always-takers. Compliers are patients whose hospital choice responds to differential distance. "Always-takers" deliver at ERD-adherent hospitals regardless of proximity. Standard errors, calculated using the delta method, reported in parentheses. Binary variables reported as percentages. n = 9,013,727.

A.3 Robustness Checks

Appendix Table A7: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision (using alternative rural-urban definitions), 2SLS Estimates

	OMB definition		Obstetric provider concentration	
	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)
Panel A. Permanent contraception				
ERD-adherent	-0.0639***	-0.0420***	-0.0544***	-0.0720**
	(0.0148)	(0.0136)	(0.0104)	(0.0294)
Y-mean	0.0814	0.0593	0.0669	0.0568
Panel B. Non-permanent contraception				
ERD-adherent	-0.0320***	-0.0120	-0.0286***	-0.0075
	(0.0093)	(0.0105)	(0.0059)	(0.0236)
Y-mean	0.0468	0.0383	0.0367	0.0401
Observations	557,342	8,456,385	3,455,396	5,558,331
First stage F-stat	80.71	53.78	77.29	23.58

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. In our main analyses, we we use the National Center for Health Statistics (NCHS) urban-rural classification and define urban as counties part of large metropolitan areas of 1 million or more residents. Here we present results from two other definitions of urban: (i) metropolitan counties according to the Office of Management and Budget (OMB), and (ii) counties with obstetric provider concentration above our sample mean, concentration defined using the Herfindahl-Hirschman Index with annual birth volumes at hospitals located in patient county of residence. Rural is defined as non-urban.

Appendix Table A8: The Effect of ERD-adherent Hospitalization on Short-Interval Pregnancy (using alternative rural-urban definitions), 2SLS Estimates

	OMB definition		Obstetric provider concentration	
	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)
Panel A. <6 months short-interval pregnancy				
ERD-adherent	0.0114*	-0.00105	0.00413	-0.0169*
	(0.00669)	(0.00509)	(0.00398)	(0.00979)
Y-mean	0.0385	0.0314	0.0351	0.0299
Panel B. <12 months short-interval pregnancy				
ERD-adherent	0.0324**	0.00581	0.0204**	-0.0258
	(0.0165)	(0.0108)	(0.00959)	(0.0219)
Y-mean	0.0848	0.0772	0.0825	0.0749
Panel C. <18 months short-interval pregnancy				
ERD-adherent	0.0519*	0.0117	0.0393**	-0.0284
	(0.0275)	(0.0177)	(0.0180)	(0.0361)
Y-mean	0.1307	0.1259	0.1327	0.1225
Observations	307,836	4,910,095	1,875,428	3,342,503
First stage F-stat	25.53	38.95	46.26	16.45

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. In our main analyses, we we use the National Center for Health Statistics (NCHS) urban-rural classification and define urban as counties part of large metropolitan areas of 1 million or more residents. Here we present results from two other definitions of urban: (i) metropolitan counties according to the Office of Management and Budget (OMB), and (ii) counties with obstetric provider concentration above our sample mean, concentration defined using the Herfindahl-Hirschman Index with annual birth volumes at hospitals located in patient county of residence. Rural is defined as non-urban.

Appendix Table A9: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision (using sub-sample with unique patient identifiers), 2SLS Estimates

	LATE
Panel A. Permanent contraception	
ERD-adherent	-0.0442***
	(0.0151)
Y-mean	0.0617
Panel B. Non-permanent contraception	
ERD-adherent	-0.0150
	(0.0099)
Y-mean	0.0415
Observations	5,217,931
First stage F-stat	61.59

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 results use the sub-sample of states with unique patient identifiers, which is required for the short-interval pregnancy analyses.

Appendix Table A10: The Effect of Catholic-affiliated Hospitalization on Postpartum Contraception Provision, 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. Permanent contraception (sterilization)			
Catholic-affiliated	-0.0409***	-0.0553***	-0.0479*
	(0.0117)	(0.0115)	(0.0217)
Y-mean	0.0606	0.0741	0.0558
Panel B. Non-permanent contraception			
Catholic-affiliated	-0.0156*	-0.0330***	-0.0000719
	(0.00798)	(0.00768)	(0.0170)
Y-mean	0.0388	0.0441	0.0369
Observations	9,013,727	2,405,223	6,608,504
First stage F-stat	86.20	101.62	23.53

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses.

Appendix Table A11: 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	-0.0363***	-0.0601***	-0.0506**
	(0.0132)	(0.0173)	(0.0232)
Y-mean	0.0608	0.0741	0.0558
Panel B. Non-permanent contraception			
ERD-adherent	-0.0151*	-0.0359***	-0.0000741
	(0.00824)	(0.00987)	(0.0179)
Y-mean	0.0389	0.0442	0.0369
Observations	9,125,018	2,516,510	6,608,508
First stage F-stat	72.18	37.48	20.90

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.

Appendix Table A12: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision (without hospital-level controls), 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. Permanent contraception			
ERD-adherent	-0.0408***	-0.0588***	-0.0439*
	(0.0121)	(0.0122)	(0.0231)
Y-mean	0.0606	0.0741	0.0558
Panel B. Non-permanent contraception			
ERD-adherent	-0.0185**	-0.0383***	0.00419
	(0.00766)	(0.00813)	(0.0204)
Y-mean	0.0388	0.0441	0.0369
Observations	9,013,727	2,405,223	6,608,504
First stage F-stat	62.09	48.93	15.89

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. This specification excludes hospital-level covariates to address potential concerns about post-treatment bias from conditioning on characteristics of the birth hospital. Results are substantively consistent with our main specifications, though first-stage F-statistics are lower without hospital controls.

Appendix Table A13: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision (with additional market-level controls), 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. Permanent contraception			
ERD-adherent	-0.0390***	-0.0529***	-0.0480**
	(0.0120)	(0.0134)	(0.0221)
Y-mean	0.0606	0.0741	0.0558
Panel B. Non-permanent contraception			
ERD-adherent	-0.0145*	-0.0330***	0.000546
	(0.00785)	(0.00880)	(0.0172)
Y-mean	0.0388	0.0441	0.0369
Observations	9,013,727	2,405,223	6,608,504
First stage F-stat	94.72	67.92	23.64

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. All specifications include the same patient, hospital, and market-level controls as our main analysis, plus additional controls for reproductive healthcare access: OB/GYN physicians per 100,000 population and number of abortion clinics per patient county-year. These additional controls ensure our estimates reflect hospital ERD restrictions rather than provider-level barriers to reproductive healthcare that might be related to postpartum contraception access or rates of short-interval pregnancies.

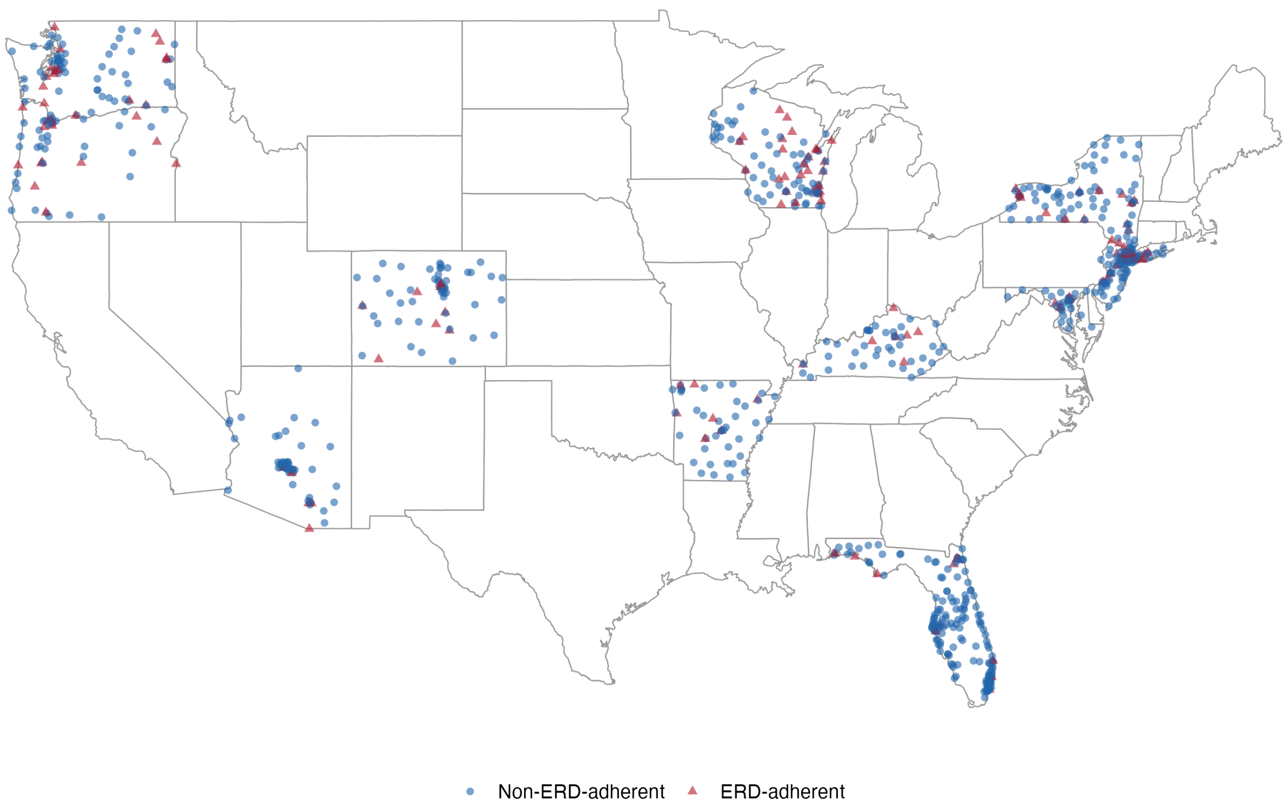
Appendix Table A14: The Effect of ERD-adherent Hospitalization on Short-Interval Pregnancy (with additional market-level controls), 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. <6 months short-interval pregnancy			
ERD-adherent	0.00102	0.00990**	-0.00685
	(0.00432)	(0.00482)	(0.00811)
Mean dep. var.	0.0318	0.0377	0.0295
Panel B. <12 months short-interval pregnancy			
ERD-adherent	0.00819	0.0235**	0.00230
	(0.00882)	(0.0109)	(0.0180)
Mean dep. var.	0.0776	0.0846	0.0749
Panel C. <18 months short-interval pregnancy			
ERD-adherent	0.0147	0.0416**	0.00906
	(0.0138)	(0.0182)	(0.0295)
Mean dep. var.	0.1262	0.1329	0.1235
Observations	5,217,931	1,460,139	3,757,792
First stage F-stat	69.31	45.87	21.61

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 states with unique patient identifiers to allow for longitudinal linkage of patients from the index birth hospitalization to subsequent pregnancies. All specifications include the same patient, hospital, and market-level controls as our main analysis, plus additional controls for reproductive healthcare access: OB/GYN physicians per 100,000 population and number of abortion clinics per patient county-year. These additional controls ensure our estimates reflect hospital ERD restrictions rather than provider-level barriers to reproductive healthcare that might be related to postpartum contraception access or rates of short-interval pregnancies.

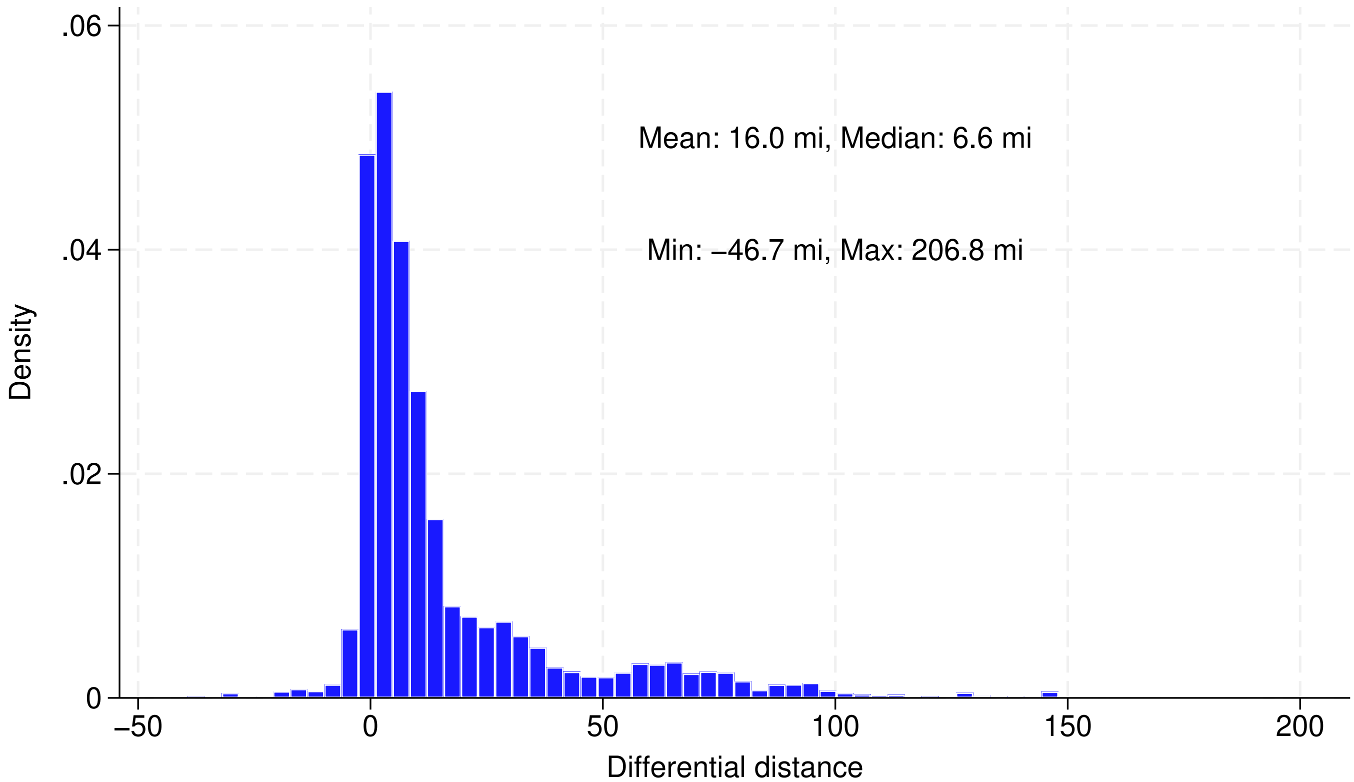
A.4 Descriptive Figures

Appendix Figure A1: Geographic Distribution of ERD-adherent Hospitals in Study Sample



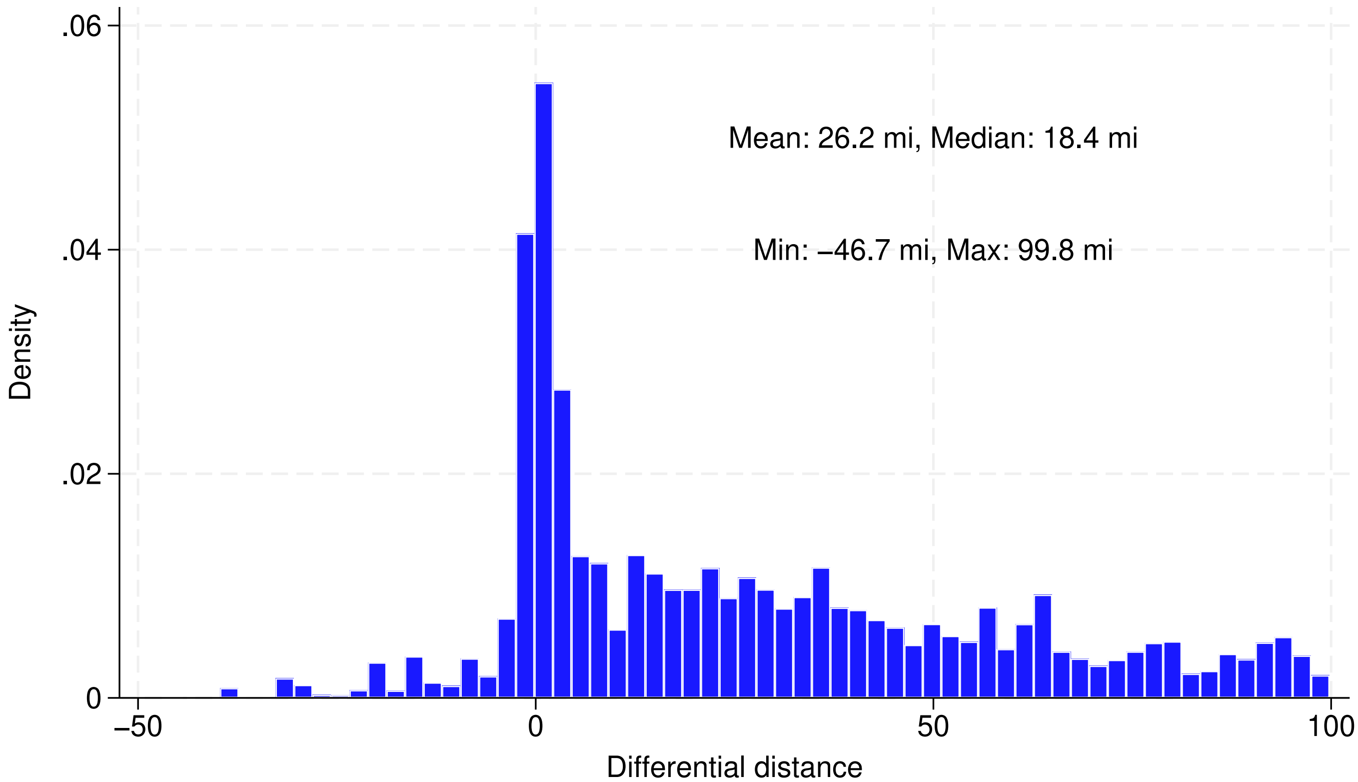
Notes: This figure shows the geographic distribution of hospitals in our study sample by ERD status, determined by the first year each hospital appears in our study sample. ERD-adherent hospitals (n = 137, red triangles) are those that adhere to the Ethical and Religious Directives for Catholic Health Care Services, which prohibit the provision of contraception and sterilization services. Non-ERD-adherent hospitals (n = 729) are shown in blue circles.

Appendix Figure A2: DD instrument distribution, non-trimmed



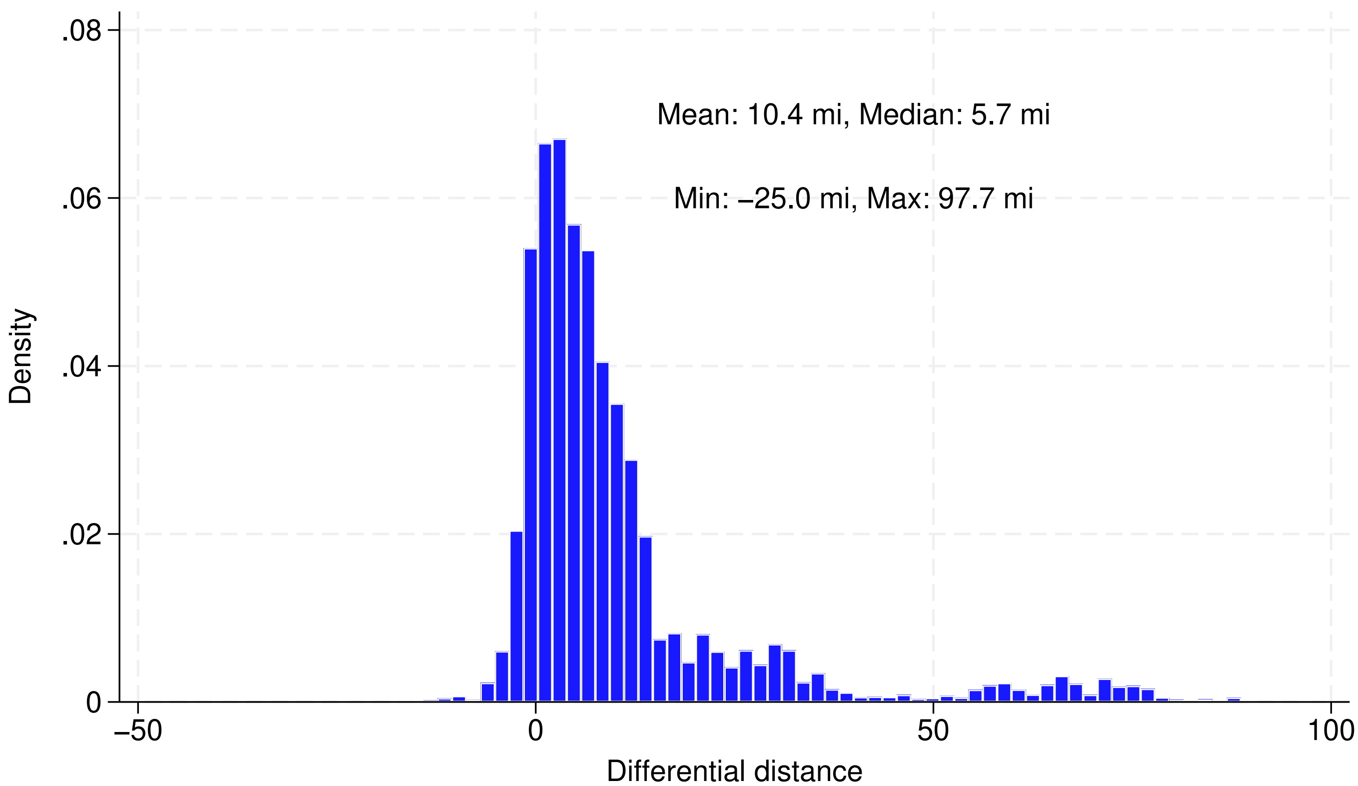
Notes: This figure shows the distribution of the differential distance (DD) instrument for the full, untrimmed sample. The differential distance instrument is calculated as the distance from a patient's zip code centroid to the nearest ERD-adherent hospital minus the distance to the nearest non-ERD-adherent hospital. The distribution exhibits substantial right skew, with some patients facing differential distances exceeding 200 miles. $N = 9{,}125{,}018$.

Appendix Figure A3: 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 26.2 miles. $N = 2{,}405{,}223$.

Appendix Figure A4: 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 = 10.4 miles), reflecting the higher density of hospitals and smaller differences in travel distances in urban areas. $N = 6{,}608{,}504$.

A.5 Outcome Coding

Appendix Table A15: 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.

Appendix Table A16: ICD, CPT, and DRG codes 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.