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

Alex Schulte[†] Becky Staiger Amanda L. Brewster Lori Freedman
Hector P. Rodriguez

March 31, 2026

Abstract

As demand for postpartum contraception increases, a growing number of hospitals are adopting religious restrictions known as the Ethical and Religious Directives (ERDs) that explicitly prohibit contraception provision. Yet, despite ongoing state and federal policy debates, causal evidence on the longer term health consequences of institutional religious restrictions remains scarce. We use quasi-random variation in geographic proximity to hospitals with and without ERDs to evaluate the effect of giving birth at an ERD-adherent hospital on postpartum contraception and downstream reproductive health outcomes. Our instrumental variable approach uses data on over 9 million birth hospitalizations from 2010–2023 across eleven states. We find giving birth at an ERD-adherent hospital reduces permanent contraception by 3.82 percentage points (pp, 63% relative to the mean) and non-permanent contraception by 1.39 pp (36% relative decrease). Effects are substantially larger for rural patients, for whom ERD-adherent hospitalization reduces permanent and non-permanent contraception by 70% and 71%, respectively. Among rural patients, we subsequently document a 33% increase in short-interval pregnancies, which carry serious maternal and infant health risks. Urban patients show no significant effects on non-permanent contraception or short-interval pregnancy, consistent with greater access to non-hospital contraception options. These findings provide the first causal evidence linking hospital religious restrictions to adverse downstream health outcomes, informing policy debates on hospital transparency, merger oversight, and institutional conscience protections.

*This project was supported by funding from the Agency for Healthcare Research and Quality (AHRQ) under a Ruth L. Kirschstein National Research Service Award T32 (T32HS022241), the Eunice Kennedy Shriver National Institute for Child Health and Human Development (NICHD) under an Institutional Training Grant (5T32HD101364), and the Society of Family Planning (SFP) under an Emerging Scholars award. Its contents are solely the responsibility of the authors and do not necessarily represent the official views or opinions of AHRQ, NICHD, or SFP. None of the funders were involved in the study design; in the collection, analysis, and interpretation of data; in the writing of the articles; or in the decision to submit it for publication. This study was reviewed and considered exempt by the University of California, Berkeley institutional review board (#2024-08-17681).

[†]Corresponding author; University of California, Berkeley alex_schulte@berkeley.edu

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, one in seven U.S. hospitals adheres to the Ethical and Religious Directives for Catholic Health Care Services (ERDs), a set of 77 specific directives that prohibit provision of any type 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 during 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). 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?

Understanding obstetric service restrictions is crucial, as the birth hospitalization represents an important point of access for contraception. Immediate postpartum contraception, defined as contraception provided during a hospital stay for childbirth, is medically efficient, cost-effective, and eliminates the need for additional procedures or appointments. About half of all tubal ligation procedures, a common form of postpartum contraception, occur during the birth hospitalization (ACOG, 2021). For rural patients facing mounting barriers to reproductive healthcare, the birth hospitalization often represents the only feasible opportunity to receive contraception (Cooper et al., 2024; Clark and Levy, 2025; ACOG, 2021).

Whether (and how) hospital religious restrictions impact patient health outcomes remains an open and important empirical question—one that has taken on new urgency amid active policy debates. During the 2024 U.S. Vice Presidential debate, Senator J.D. Vance interrogated Governor Tim Walz

[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.

about his "freedom of conscience" views and whether he would force Catholic hospitals to perform services "against their will." Subsequently, President Trump signed an executive order strengthening institutional protections for religiously-affiliated hospitals (DHHS, 2024). Conversely, at the state level, policymakers have increasingly treated these religious service restrictions as market failures, introducing legislation to either: i) increase government oversight of healthcare mergers and acquisitions (M/A), or ii) mandate transparency about hospital services not provided. Appendix Table A1 describes nine state-level policies related to reproductive healthcare provided in Catholic hospitals.

Despite this policy activity, causal evidence on the consequences of hospital religious restrictions remains scarce. 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 lead to adverse health 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 contraception provision during the birth hospitalization and subsequent pregnancy spacing. 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,[2] 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. Our instrumental variable approach identifies effects for the "complier" population, patients whose hospital choice responds to geographic proximity. This population is precisely the one most harmed when market events (such as hospital acquisitions or closures) eliminate local non-religious options, making our estimates particularly relevant to ongoing policy debates.

We first find that admission to an ERD-adherent hospital causes substantial reductions in the likelihood

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

of receiving immediate postpartum contraception. Specifically, giving birth at an ERD-adherent hospital reduces the likelihood of receiving permanent contraception (e.g., sterilization) by 3.82 percentage points (pp), a 63% decrease relative to the mean, and non-permanent contraception by 1.39 pp (36%), 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 5.19 pp (70% decrease) and non-permanent contraception by 3.12 pp (71% decrease). We then document downstream reproductive health consequences: rural patients who deliver at ERD-adherent hospitals experience a statistically significant 4.36 pp increase in the likelihood of a subsequent pregnancy within 18 months, representing a 33% increase relative to the mean.

Our study makes three key contributions to the literature. First, we provide the first causal evidence linking hospital religious restrictions to downstream health outcomes. Most prior research on this topic has been non-causal or has used qualitative interview designs to generate insights for a relatively small sample (Thorne et al., 2019; Freedman, 2023). Previous associational studies have documented associations between proximity to a Catholic-affiliated hospital and reduced contraception provision (Meille and Monnet, 2024; Rodriguez et al., 2023) and increased risk of short-interval pregnancy (Liu et al., 2025; Caldwell et al., 2022), but these observational approaches are limited by the potential for patient selection and unobserved confounding. One quasi-experimental study by Hill et al. (2019) exploited hospital-level conversions in Catholic-affiliation and found a 31% reduction in the per-bed rate of tubal ligation, suggesting that more "women [may] face the risks of unintended pregnancies," but the authors "do not find substantial changes in welfare." We build upon Hill et al. (2019) in three key areas: i) we examine rural-urban heterogeneity and trace effects through to downstream reproductive health outcomes, demonstrating that hospital service restrictions increase the risk of short-interval pregnancy, particularly for rural patients; ii) we use an instrumental variable design to estimate effects for patients who choose hospitals based on geographic convenience ("compliers"), a particularly policy-relevant population given they would be most harmed if consolidation eliminated non-religious alternatives; and iii) we introduce a precise, time-varying measurement of ERD adherence (see Appendix A.2), distinguishing hospitals officially required to follow religious directives from those merely affiliated with Catholic systems.[3]

[3] About 10% of hospitals affiliated with Catholic systems are not required to follow the ERDs. Previous research on this topic, such as Hill et al. (2019), does not distinguish between "Catholic-affiliated" vs. "ERD-adherent" hospitals, despite the discrepancy.

Second, we 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; Bailey et al., 2025; 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.

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 were 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 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 hospitalization (ACOG, 2021). Overall, female sterilization procedures follow 7-10% of all hospital deliveries

[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.

(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-permanent contraception includes both LARC devices, 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. While conventional wisdom suggests that restricting access to contraception harms health, the causal evidence supporting this relationship is surprisingly limited. Quasi-experimental studies exploiting changes in family planning programs have established that expanded contraception access reduces birth rates, especially among teens and low-income women (Packham, 2017; Bailey, 2013; Lindo and Packham, 2017). More recently, experimental evidence demonstrates that reducing financial barriers at Title X clinics substantially increases use of long-acting reversible contraception (LARC) and decreases overall pregnancies (Bailey et al., 2025).

However, while these studies rigorously evaluate how financial barriers impact fertility outcomes, they do not follow women over time to study subsequent reproductive health consequences, such as pregnancy spacing. One observational study documented associations between unfulfilled postpartum sterilization

[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.

requests and shorter pregnancy intervals, but is limited by the potential for patient selection and unobserved confounding (Thurman and Janecek, 2010). This gap is critical as short-interval pregnancies—those occurring within 18 months of a previous birth—carry well-established health risks, including preterm birth, low birth weight, and maternal morbidities, with risks highest for pregnancies within 6 months (Conde-Agudelo et al., 2006, 2007; Ali et al., 2023; Wang et al., 2022; ACOG, 2019b). These health consequences have made reducing the occurrence of short-interval pregnancies a priority target of the Healthy People 2030 objectives (DHHS, 2025). Understanding and addressing unfulfilled contraceptive demand is therefore critical for assessing both reproductive autonomy and downstream health outcomes.

2.2 Hospitals with religious restrictions

One important, yet understudied, barrier to reproductive healthcare access is institutional 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 have expanded. Between 2000 and 2020, the number of Catholic-affiliated hospitals increased by 28%. Today, six of the ten largest U.S. health systems (by net patient revenue) are Catholic, and approximately one in seven U.S. hospitals is Catholic (Solomon et al., 2020; Schulte et al., 2025; Popowitz, 2025). In terms of geographic presence, the market share of Catholic-affiliated hospitals varies

[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, 89% of hospitals affiliated with a Catholic health system were ERD-adherent in 2023.

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 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.

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-

[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.

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).[10] We also exclude 1% 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[11] 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

[10] 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).

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

the ERDs. This approach extends prior research (Hill et al., 2019; Meille and Monnet, 2024; Rodriguez et al., 2023) that has relied on measures of Catholic affiliation that may not capture true adherence to the ERDs. For instance, a hospital may be acquired and affiliated with a Catholic system but have a specific agreement that exempts it from the ERDs, or there may be a delay between an acquisition and the implementation of the directives. Dignity Health, for example, is one of the largest U.S. Catholic health systems and owns several hospitals that are not expected to adhere to the ERDs.[12]

To construct precise, time-varying indicators of hospital ERD adherence, we implemented a rigorous multi-step validation process (detailed in Appendix A.2). To briefly summarize: we first cross-referenced AHA Catholic-affiliation variables against six secondary data sources.[13] 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.

Our secondary outcome of interest is short-interval pregnancy, defined as a pregnancy within 18 months

[12] 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."

[13] 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.

of a birth.[14] 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 Tables A4 and A5 provide 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 provider density (as a proxy for competition, as urban areas typically have a higher concentration of providers and therefore more competition).[15]

Patient clinical characteristics include type of admission (emergency/urgent vs. elective) and dichotomous indicators for several comorbidities known to be associated with reproductive health outcomes: obesity, diabetes, hypertension, hypothyroidism, chronic pulmonary disease, depression, and substance use (Aubry et al., 2019; Chuang et al., 2005; Hayes et al., 2020; Phillips-Bell et al., 2016; Hall et al.,

[14] 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.

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

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).[16]

We control for several relevant characteristics of the birth hospital including: ownership structure (non-profit, for-profit, or government), health system membership, total annual admissions, Medicaid patient share (proportion of total annual inpatient days covered by Medicaid), presence of an obstetric unit, and obstetric caseload (proportion of total annual admissions for obstetric care). We also control for level of competition in the hospital service area, given suggestive evidence that hospitals in more concentrated (i.e., rural) markets may more strictly adhere to the ERDs (Freedman, 2023; Kramer et al., 2021).[17] 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. Therefore, 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.

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

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-

[16]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.

[17]Evidence suggests that Catholic hospitals in more concentrated markets may be more likely to turn away patients for contraceptive or fertility care prohibited by the ERDs, potentially related to more conservative culture in more concentrated (i.e., rural) markets. 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.

productive health events, specifically short-interval pregnancies. The mechanism driving our results is straightforward: the Ethical and Religious Directives explicitly prohibit hospitals from providing services that "render procreation impossible," including sterilization and contraception.[18] In order to build intuition for why 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 Sections 4.2 and 4.3, we outline our 2SLS model that leverages differential distance between ERD-adherent and non-ERD-adherent hospitals, relative to where a patient lives, as an instrument that quasi-randomly assigns patients to hospitals. We discuss assumptions and validity of our identification strategy in Section 4.4 and potential limitations in 4.5.

4.1 Naive OLS estimation

If patients were randomly assigned to hospitals, we could estimate a causal relationship between treatment and outcomes using a standard ordinary least squares (OLS) regression. 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:

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

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 $\mathrm{Cov}(\mathrm{ERD}_{ihmt}, \varepsilon_{ihmt}) = 0$. However, this exogeneity assumption may be violated in our setting as patients may select hospitals based on unobservable factors—such as individ-

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

ual religiosity, preferences, or unmeasured health severity—that are also correlated with reproductive health outcomes. For example, Catholic patients may prefer to go to ERD-adherent hospitals and also be less likely to seek or accept contraceptive services due to religious beliefs. Alternatively, particularly vulnerable patients (e.g., uninsured, low-income, or medically complex patients) may be more likely to seek care at Catholic hospitals (given the mission-based identity) and also be at higher risk for adverse reproductive health outcomes. In either scenario, $\text{Cov}(\text{ERD}_{ihmt}, \varepsilon_{ihmt}) \neq 0$, violating the exogeneity assumption required for causal interpretation of β_1. Given these concerns, 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.[19] For example, if a patient lives 15 miles from the nearest ERD-adherent hospital and 5 miles from the nearest non-ERD-adherent hospital, DD = 10 miles, indicating the non-ERD-adherent hospital is relatively closer. This instrument captures quasi-random variation in the likelihood of admission to a type of hospital driven by geographic convenience, rather than unobserved patient preferences correlated with health outcomes.

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

[19] 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. 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).

a hospital for obstetric care were quality and location (76% and 72%, respectively).[20]

One potential concern with the DD instrument is that patients facing extreme travel trade-offs may not represent a population with meaningful choice, potentially violating the common support assumption required for instrument validity. To address this, we exclude observations where the absolute DD exceeds 100 miles (approximately 2.5 to 3 hours of driving time).[21] This excludes about 1% of the sample, focusing the analysis on patients for whom both types of hospitals are reasonably accessible. As shown in Figure 1, the closest non-ERD-adherent hospital is 14.6 miles closer to the patient zip code centroid, on average, than the closest ERD-adherent hospital. Appendix Figures A1 and A2 present the DD distribution for the rural and urban sub-samples, respectively. As expected, DD is more widely dispersed for rural patients given the lower density of hospitals and therefore greater variation in travel distance. The mean DD is 26.2 and 10.4 miles for rural and urban patients, respectively. In addition, Appendix Figure A3 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. 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.

Appendix Table A6 presents a few key descriptive statistics related to hospital distance and patient location. As shown in Panel A, the average distance from a patient's residence to the birth hospital was 9 miles (SD: 13). 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 corresponds to slightly over 14 minutes of drive time. The mean distance traveled was 13 and 8 miles for rural and urban patients, respectively, in our sample, which also aligns with estimates from previous research (Fontenot et al., 2024). On average, the closest hospital was ERD-adherent for 14% of hospitalizations in our sample (Panel B). If the closest hospital was ERD-adherent, the patient gave birth there 43% of the time. This proportion is slightly higher for rural patients compared to urban (50% vs. 39%), which aligns with our finding that the first stage F-statistic is higher for rural vs. urban patients (72.27 vs. 24.77, see Section 6.1) and demonstrates that distance is more predictive of hospital choice when there

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

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

are fewer alternatives.

4.3 2SLS-IV model

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

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

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

We present the first-stage relationship in Figure 2 to confirm the relevance assumption needed for a valid 2SLS-IV design. The binned scatterplot visually confirms the strong, negative first-stage relationship between our instrument and ERD-adherent hospitalization.[22] The coefficient of -0.00537 (SE: 0.0006, p-value: <0.001) indicates that each mile increase in differential distance—meaning the ERD-adherent hospital becomes relatively farther away—decreases the probability of ERD-adherent hospitalization by approximately 0.54 percentage points, or 5.4 percentage points for a 10-mile change in relative distance. In addition, the first-stage F-statistic, 95.14, is well above conventional thresholds for weak instrument concerns (e.g., $F > 10$), indicating that the instrument has substantial explanatory power with respect to the endogenous treatment variable.[23]

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

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

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

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

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 changes in differential distance. In this context, the estimate gives more weight to the treatment effects of individuals whose hospital choice is most sensitive to the instrument.[24] Therefore, γ_1 should be interpreted as the average causal effect of admission to an ERD-adherent hospital for compliers, the population of patients whose choice of hospital is determined by geographic convenience.

4.4 Instrument validity

In addition to the relevance and common support assumptions discussed above, the 2SLS-IV model requires instrument independence and support for the exclusion restriction. Instrument independence requires that the instrument is "as-good-as-randomly" assigned conditional on controls—that is, no unobserved patient characteristics are correlated with both differential distance and the outcomes. In 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-, hospital-, and market-level characteristics). The exclusion restriction requires that the differential distance instrument affects the outcomes only through the treatment, with no direct pathway 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

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

outcome (NCO) tests—supporting the plausibility of both assumptions.

Conceptual support. From a conceptual standpoint, several features of our empirical setting support both instrument 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 hospital selection support the independence assumption by suggesting that patients do not systematically sort into religiously-affiliated hospitals. In addition, national surveys show that Catholic women use contraception at the same rate as the U.S. population: 99.0% of Catholic women use any type of contraception (vs. 99.6% nationally) and 32% use permanent contraception (vs. 33% nationally) (Jones and Dreweke, 2011; Jones, 2020). Taken together, this evidence suggests that marginal differences in geographic proximity are unlikely to be systematically correlated with unobserved patient characteristics such as religiosity or preferences regarding contraception. Rather, the variation in 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 the identifying assumptions. Strategic hospital location by Catholic health systems might create systematic correlations between ERD-adherent hospitals and area religiosity or abortion attitudes. However, the instrument exploits variation in *relative* distances within geographic areas rather than absolute proximity to ERD hospitals. Conditional on the controls—which include county-level religiosity, abortion clinic availability, and OB/GYN density—the remaining variation in differential distance reflects which hospital type happens to be marginally closer to a given patient. In addition, the continuous nature of the instrument allows us to control for broader healthcare infrastructure (e.g., distance to nearest hospital of any type) while isolating the variation from relative proximity.

Balance tests. To 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 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.[25]

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

NCO 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 instrument independence (Shi et al., 2020). We estimate reduced-form regressions of six NCOs on differential distance, using

[25]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.

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.[26] 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).[27] Finally, NCOs of *parity* and *age at first observed birth* test whether patients living closer to ERD-adherent hospitals have different reproductive histories or fertility preferences.[28] Appendix Table A7 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.

4.5 Limitations

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). 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

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

[27]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 Health and Human Development, 2023).

[28]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.

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.

5 Descriptive results

5.1 Sample characteristics

Table 2 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).

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, 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 addition, patients discharged from ERD-adherent hospitals live in counties with slightly fewer OBGYNs per capita (12 vs. 13) and fewer abortion clinics (3 vs. 5) compared to patients who went to non-ERD-adherent hospitals.

Our sample includes 9,013,727 discharges in total, 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). The sample includes 137 unique ERD-adherent and 729 non-ERD-adherent hospitals.[29] Figure 4 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.

5.2 Outcome rates

In Table 3 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. 2.95%). 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 across hospital types.

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

5.3 Delivery type characteristics

Postpartum contraception is more frequently provided after delivery by Cesarean section (c-section) compared to vaginal births, given the reduced clinical, logistical (and in some cases, financial) costs to the patient (ACOG, 2021; Rodriguez et al., 2024). In Appendix Table A8, we present descriptive statistics on delivery type and postpartum contraception provision, which helps to confirm that the observed reductions in contraception are driven by religious restrictions rather than alternative clinical channels (e.g., underlying differences in c-section rates between hospital types). Within our study sample, 33.3% of all births were by c-section compared to vaginal delivery, a proportion that is consistent across hospital type (Panel A; 31.9% at ERD-adherent hospitals vs. 33.5% at non-ERD-adherent hospitals). As expected, we also find that both permanent and non-permanent contraception is more frequently provided after c-section births (Panels B and C, respectively). Given that postpartum contraception is more frequently performed alongside c-sections, the slightly lower c-section rates at ERD-adherent hospitals could explain reduced contraception rates independent of religious restrictions. However, we find this is not the case: we estimate an OLS model with the same controls described in Section 3 and find no significant relationship between ERD-adherent hospitalization and the probability of delivering by c-section (coef: 0.00491, SE: 0.00707, p-value: 0.487). In addition, to confirm the robustness of our main results, we re-estimate our main 2SLS models controlling for delivery type (see Section 6.3) and find results are consistent with our main analyses.

5.4 Complier characteristics

Our 2SLS-IV approach identifies treatment effects for compliers—patients whose hospital choice is determined by geographic proximity (as compared to those willing or able to travel for care that better meets their preferences). This complier population is most harmed when hospital M/A eliminates local non-religious alternatives, a key concern in current policy debates regarding Catholic healthcare expansion. In Appendix Table A9, we characterize the complier population compared to "always-takers" (patients who deliver at ERD-adherent hospitals regardless of proximity) and "never-takers" (patients who deliver at non-ERD-adherent hospitals regardless of proximity). To do so, we use 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).[30]

[30]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

Compliers are less likely to be white, and more likely to have Medicaid, live in an urban area, and be admitted on an urgent basis compared to both always-takers and never-takers. Compliers also have a slightly higher Elixhauser score, suggesting modestly worse health status (-0.01 vs. -0.20 and -0.25 for always-takers and never-takers, respectively). Similar c-section rates across all groups (32–34%) suggest that differential selection into planned surgical deliveries is unlikely to explain our results. Taken together, these results suggest that the complier population is composed of lower-SES, racially diverse, urban patients experiencing urgent obstetric situations who deliver at the nearest hospital due to time and resource constraints. Always- and never-takers, in contrast, generally have more resources and are able to make planned hospital choices independent of proximity constraints. These findings align with previous evidence showing that less advantaged families are more affected by distance constraints (Card et al., 2023; Fontenot et al., 2024), and underscore that compliers—the population for whom our LATE is identified—are precisely those most harmed when mergers or acquisitions eliminate local non-religious hospital alternatives.

6 Main Results

6.1 Immediate postpartum contraception

Table 4 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 3.92 percentage point (pp) and 2.57 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 65% and 66% decrease.

The LATE from our 2SLS-IV models is reported in Column (2). Overall, these estimates are 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

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.

an ERD-adherent hospital causes a 3.82 pp ($p < 0.01$) decrease in the probability of receiving the procedure—a substantial 63% reduction relative to the sample mean of 6.06%. The LATE is slightly smaller in magnitude than the OLS estimate of -3.92 pp, suggesting modest upward selection bias in the OLS specification: patients who prefer permanent contraception may be somewhat less likely to seek care at ERD-adherent hospitals, biasing OLS estimates away from zero. For non-permanent contraception (Panel B), we estimate a decrease of 1.39 pp ($p < 0.10$) in Column (2), corresponding to a 36% reduction relative to the mean of 3.88%. The non-permanent contraception LATE is notably smaller in magnitude than the OLS estimate of -2.57 pp, suggesting that unobserved selection patterns may differ by contraception type. Taken together, the OLS and 2SLS results both point to a strong, negative relationship between giving birth at an ERD-adherent hospital and receipt of postpartum contraception.

Appendix Table A10 presents the reduced-form estimates, capturing the intent-to-treat relationship between differential distance and postpartum contraception provision. For permanent contraception (Panel A), the instrument is positive and significant across all subgroups: a 10-mile increase in differential distance is associated with a 0.21 pp ($p < 0.01$) increase in the probability of sterilization in the full sample, with similar magnitudes for rural (0.23 pp, $p < 0.01$) and urban patients (0.25 pp, $p < 0.01$). For non-permanent contraception (Panel B), the pattern diverges by geography: the instrument is positive and significant for rural patients (0.14 pp for 10-mile change, $p < 0.01$), but near zero and insignificant for urban patients. As discussed in the next section, this pattern likely reflects that urban patients are less reliant on the birth hospitalization as a point of access for non-permanent contraception (consistent with their lower baseline rates) given the increased ease in accessing outpatient alternatives.

6.1.1 Heterogeneity by patient location

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. Our main model defines rural vs. urban using NCHS definitions, and we explore robustness with definitions from OMB and competition (i.e., concentration of healthcare providers).

The LATE for rural patients is reported in Column (1) of Table 5. Admission to an ERD-adherent hospital reduces sterilization by 5.19 pp ($p < 0.01$), an 70% decrease relative to the rural mean of 7.41%, and non-permanent contraception by 3.12 pp ($p < 0.01$), a 71% decrease relative to the mean of 4.41%. The first-stage F-statistic is 72.27. Column (2) presents results for urban patients.[31] ERD-adherent hospitalization reduces sterilization by 5.05 pp ($p < 0.05$), a 91% reduction relative to the urban mean of 5.58%. In contrast to rural populations, the effect on non-permanent contraception for urban patients is effectively zero and statistically insignificant. We offer two complementary explanations for this pattern. First, the lower mean rate of non-permanent contraception at delivery among urban patients (3.69% vs. 4.41% for rural patients) suggests that urban women may intend to obtain non-permanent contraception (e.g., IUDs and implants) at postpartum visits rather than at the birth hospitalization. In contrast, rural women face greater barriers to returning for separate visits, making the birth hospitalization a less replaceable point of access. Second, it is possible that ERD-adherent hospitals in urban—often more politically liberal—areas are subject to less stringent diocesan oversight, allowing for greater de facto provision of non-permanent contraception compared to their rural counterparts.

In Appendix Table A11, we present the LATE of ERD-adherent hospitalization on provision of postpartum contraception using two alternative rural-urban definitions. First, using the OMB definition, we find nearly identical patterns to our main results: large, significant reductions in both sterilization (6.09 pp, $p < 0.01$) and non-permanent contraception (3.23 pp, $p < 0.01$) for rural patients, while urban patients experience significant sterilization reductions (3.86 pp, $p < 0.01$) but insignificant effects on non-permanent contraception. Second, we split the sample by market competition using the Herfindahl-Hirschman Index (HHI)—areas with low competition have fewer providers, analogous to rural areas. We find consistent results when defining rurality by provider competition: patients in low-competition (rural) areas experience 5.58 pp and 2.85 pp reductions in permanent and non-permanent contraception, respectively (both $p < 0.01$). In high-competition (urban) areas, effects of ERD-adherent hospitalization are only significant for permanent contraception. Taken together, these findings underscore the disproportionate vulnerability of rural populations to ERD-adherent hospital service restrictions.

[31] The weaker first stage in urban areas (F-stat: 24.77) likely reflects that urban patients have access to a denser hospital network, and that great-circle distance is a noisier proxy for actual travel burden in areas with complex road networks and traffic congestion.

6.1.2 Heterogeneity by patient 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.[32] 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 2). 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.

Table 6 presents 2SLS estimates stratified by insurance type. For privately-insured patients, ERD-adherent hospitalization reduces permanent contraception by 3.77 pp (74% relative to mean, $p < 0.01$) and non-permanent contraception by 1.76 pp (60% relative to mean, $p < 0.01$). For Medicaid patients, permanent contraception decreases by 3.77 pp (53% relative to mean, $p < 0.05$), while the effect on non-permanent contraception is smaller and not statistically significant. The null result for non-permanent contraception likely reflects financial, logistical, and provider-level barriers for Medicaid patients, rather than religious restrictions alone. Historically, Medicaid reimbursed all obstetric care under a global fee, making separate reimbursement for LARC devices financially infeasible for hospitals (Rodriguez et al., 2024). Beginning in 2012, states began amending billing policy to allow separate reimbursement (Rodriguez et al., 2024). However, even after states unbundled Medicaid reimbursement, Medicaid LARC rates remained low and uptake was gradual, reflecting persistent implementation barriers, such as the need for provider awareness, staff training, and device stocking (Rodriguez et al., 2024). In contrast, permanent contraception—which was consistently covered by both Medicaid and private insurance—shows similar, significant reductions at ERD-adherent hospitals across insurance types.

6.2 Short-interval pregnancy

A critical downstream consequence of barriers to 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 7 presents the LATE for SIP occurring

[32] 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).

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).[33] In the full longitudinal sample (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 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 an ERD-adherent hospital. Specifically, rural patients are 1.07 pp ($p < 0.05$) more likely to be pregnant within 6 months, representing a 28% increase relative to the rural mean of 3.77%. Results are consistent across varied pregnancy intervals: rural patients experience a 2.53 pp increase ($p < 0.05$) in pregnancies within 12 months and a 4.36 pp increase ($p < 0.05$) in pregnancies within 18 months of the index birth. Relative to the rural baseline means, these estimates represent increased relative risks of 30% and 33%, respectively. In contrast, urban patients show no statistically significant effects, with small coefficients not meaningfully different from zero. This geographic heterogeneity aligns with 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.

Appendix Table A12 presents estimates of the effect of ERD-adherent hospitalization on SIP rates using two alternative rural-urban classifications, confirming the robustness of our main findings. For SIP defined within 6 months (Panel A), results are consistent, though not statistically significant. As shown in Panel B, rural patients have 2.94 pp increased risk of SIP within 12 months after delivering at an ERD-adherent hospital using the OMB definition ($p < 0.10$, 35% relative to the mean), and a 2.22 pp increased risk when rural was defined as low competition counties ($p < 0.05$, 27% relative to the mean). Results are similar and slightly larger for pregnancies within 18 months (Panel C): rural patients have an 4.71 pp increased risk using the OMB definition ($p < 0.10$, 36% relative to the mean) and a 4.05 pp increased risk using the competition-based measure ($p < 0.05$, 31% relative to the mean). Estimates

[33] Appendix Table A13 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).

for urban patients are small and statistically insignificant across all panels. The consistency of these findings across alternative definitions of rurality provides strong evidence that our main findings are robust.

6.3 Additional robustness checks

We present four additional robustness checks in Appendix Tables A14-A17. 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 (Hill et al., 2019; Solomon et al., 2020; Rodriguez et al., 2023; Menegay et al., 2022; Caldwell et al., 2022; Liu et al., 2025), but as discussed in Section 3.2, ERD-adherent hospitals and Catholic-affiliated hospitals are not perfectly analogous. Appendix Table A14 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 63% decrease in likelihood of permanent contraception provision, compared to 61% decrease for Catholic-affiliated hospitalization.

Second, we re-estimate our 2SLS specification using the untrimmed DD instrument. In our main analyses, we excluded 1% of observations where DD $> |100|$ miles (approximately 2.5 to 3 hours of driving time), which helps ensure the common support assumption is met. Appendix Table A15 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-statistics are lower compared to our main trimmed sample, especially for the rural sub-population (35.45 untrimmed DD vs. 72.27 trimmed), which is not surprising given that extreme differential distances are more common in rural areas and likely include patients whose hospital choices are less responsive to relative proximity.

Third, in Appendix Table A16, we re-estimate our 2SLS specification controlling for delivery type (vaginal birth vs. c-section). Results are very similar to our main specification. For example, patients who gave birth at an ERD-adherent hospital were -3.82 pp less likely to receive permanent contraception in our main specification, compared to -3.84 pp when controlling for delivery type (both $p < 0.01$). We observe similar patterns when stratifying by contraception type and patient location. This similarity confirms that observed reductions in contraception are driven by institutional religious restrictions rather than systematic differences in obstetric management or c-section delivery rates between hospital types.

Fourth, we present 2SLS estimates with covariates selected by the machine learning LASSO-orthogonalized algorithm (Chernozhukov et al., 2015). Rather than relying solely on theory to select control variables, this approach uses a Least Absolute Shrinkage and Selection Operator (LASSO) penalized regression to autonomously identify the most predictive covariates from a high-dimensional pool, removing potential researcher bias from the covariate selection process. This approach is further described in Appendix Section A.3. Results from this robustness check are shown in Appendix Table A17. The LASSO estimates are consistent with and slightly larger in magnitude than our main specification. For example, the estimated reduction in permanent contraception increases from 3.82 pp (a 63% relative decrease) in the main model to 5.21 pp (an 86% relative decrease) in the LASSO specification. Similarly, the reduction in non-permanent contraception more than doubles in magnitude, increasing from a 1.39 pp (36% relative) reduction to a 3.25 pp (84% relative) reduction. The larger magnitudes likely reflect the LASSO algorithm identifying and controlling for additional confounders that attenuate estimates in the main specification when omitted.

Last, it is worth nothing that we do not present a robustness check using the two-stage residual inclusion (2SRI) control function method. While 2SRI models are sometimes employed for binary outcomes (typically by incorporating residuals from a linear first stage into a nonlinear second stage), linear 2SLS models are generally preferred when outcomes are rare, as is the case in this study. Nonlinear specifications such as probit frequently encounter separation problems (perfect prediction) when applied to rare events. In addition, previous econometric research demonstrates that linear 2SLS estimates are qualitatively identical to probit 2SRI marginal effects with rare outcomes. Moreover, the predicted probabilities generated by 2SLS rarely exceed the logical $[0, 1]$ bound when baseline rates are low, mitigating the primary practical concern motivating nonlinear specifications (Angrist and Pischke, 2009; Chiburis et al., 2012; Terza et al., 2008).

7 Discussion

This study seeks to answer a fundamental question facing the U.S. healthcare system: what are the consequences of institutional religious policies that constrain access to reproductive healthcare? 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 Catholic health systems have grown by over 20% in the last two decades. In addition, 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 3.82 percentage points (63% relative to the mean) and non-permanent contraception by 1.39 percentage points (36% relative to the mean). These effects are concentrated among rural patients, for whom ERD-adherent hospitalization reduces permanent contraception by 5.19 pp (70%) and non-permanent contraception by 3.12 pp (71%). Critically, we also document downstream health consequences: rural patients who delivered at an ERD-adherent hospital were 4.36 pp more likely to be pregnant within 18 months of the index birth (33% relative increase), compared to delivering at a non-ERD-adherent hospital. Concerningly, rural patients also had 28% 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 contraception, have fewer alternative providers and are less likely to attend postpartum visits (Bozkurt et al., 2024; Thayagabalu et al., 2025). For rural patients, a denial of care at the birth hospitalization is more likely to result in an unfulfilled need. Urban patients, by contrast, can more readily access a denser network of outpatient providers, which likely mitigates the impact of hospital restrictions on non-permanent contraception and subsequent pregnancy rates.

To isolate these causal effects, 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 and racially diverse patients compared to the full sample, which is consistent with other evidence demonstrating less advantaged families are more affected by distance constraints (Card et al., 2023; Fontenot et al., 2024). While it is unclear whether our estimates generalize to planned deliveries or higher-SES patients, they suggest that Catholic hospital expansions may disproportionately impact vulnerable patients with fewer resources (e.g., time or money). In addition, our results are highly policy relevant, as compliers—patients with weaker preferences and/or fewer resources—are most affected when healthcare M/A eliminates local, non-religious alternatives.

We 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, rural patients have fewer alternative access points for postpartum contraception.

Our findings build upon and extend related work. While previous studies document modest declines in postpartum sterilization following Catholic hospital exposure (a 31% reduction by Hill et al. (2019) and a 14% reduction by Meille and Monnet (2024)), our instrumental variable approach yields a markedly larger causal estimates: a 63% overall decrease in permanent contraception, rising to 70% for rural patients. This difference in magnitude stems from our use of a precise measure of strict ERD adherence, which eliminates the attenuation bias of broader "Catholic-affiliated" measures, and an IV design that estimates the LATE for compliers. In addition, our results clarify the welfare reductions suggested by Hill et al. (2019): the "minimal overall welfare reductions" they report reflect average effects across heterogeneous geographies, masking serious harm concentrated among rural patients. By tracing effects through to downstream reproductive health outcomes, we show that ERD-adherent hospitalization increases the risk of short-interval pregnancy by 33% among rural patients who lack viable outpatient alternatives (Bozkurt et al., 2024; Thayagabalu et al., 2025).

7.1 Policy implications

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 design of hospital transparency requirements. First, when Catholic health systems acquire hospitals and require ERD adherence, federal antitrust authorities currently evaluate these mergers primarily through the lens of price effects and traditional quality metrics (Gaynor et al., 2015). 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 healthcare. The Federal Trade Commission recently increased scrutiny of mergers on grounds beyond price effects (FTC, 2024); our findings support extending this scrutiny to healthcare service availability.

In addition, our results highlight information asymmetries as a source of market failure that policy can address. Prior research documents that patients are largely unaware of hospital religious restrictions, limiting their ability to make informed choices (Guiahi et al., 2014; Wascher et al., 2018; Stulberg et al., 2019; Hebert et al., 2020; Boulware et al., 2024). Policy responses such as Washington's and Colorado's transparency requirements attempt to correct this market failure by requiring public information about service restrictions (see Appendix Table A1). By providing patients with accurate, accessible information, such policies can enhance patient autonomy and mitigate the welfare losses associated with unexpected denials of care. However, our finding that effects are concentrated among rural patients—whose hospital choices are most constrained by geography—suggests transparency alone may be insufficient. In markets with limited hospital competition, service availability requirements or stronger merger oversight may be necessary complements to transparency policies.

7.2 Conclusion

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, with effects concentrated among rural patients for whom the birth hospitalization represents a less replaceable point of contraceptive access. By addressing the endogeneity of patient choice, our findings circumvent methodological challenges to demonstrate the direct impact of religious restrictions. As Catholic health systems continue to expand and demand for postpartum contraception increases post-*Dobbs*, policymakers must balance institutional conscience protections with the broader goal of patient welfare.

References

ACOG (2019a). ACOG Committee Opinion No. 762: Prepregnancy Counseling. *Obstetrics & Gynecology 133*(1), e78–e89.

ACOG (2019b). Interpregnancy Care, Obstetric Care Consensus No. 8. *Obstetrics & Gynecology 133*(1), e51–e72.

ACOG (2021). Access to Postpartum Sterilization: ACOG Committee Opinion, Number 827. *Obstetrics & Gynecology 137*(6), e169–e176.

AHA (2025a). American hospital association annual survey database. Database. Accessed 2025-09-01.

AHA (2025b). Data Collection Methods. Accessed on June 11, 2025.

AHRQ (2024). Compendium of U.S. Health Systems. Accessed on September 1, 2025.

AHRQ (2025). Healthcare Cost and Utilization Project State Inpatient Databases (HCUP-SID). Accessed on September 1, 2025.

Ali, M. M. et al. (2023). The risk of perinatal mortality following short inter-pregnancy intervals—insights from 692,402 pregnancies in 113 Demographic and Health Surveys from 46 countries: a population-based analysis. *The Lancet Global Health 11*(10), e1544–e1552.

Andrews, I., J. H. Stock, and L. Sun (2019). Weak Instruments in Instrumental Variables Regression: Theory and Practice. *Annual Review of Economics 11*, 727–753.

Angrist, J. D., G. W. Imbens, and D. B. Rubin (1996). Identification of Causal Effects Using Instrumental Variables. *Journal of the American Statistical Association 91*(434), 444–455.

Angrist, J. D. and J.-S. Pischke (2009). *Mostly Harmless Econometrics.* Princeton, NJ: Princeton University Press.

Aubry, E. M., S. Oelhafen, N. Fankhauser, L. Raio, and E. L. Cignacco (2019). Adverse perinatal outcomes for obese women are influenced by the presence of comorbid diabetes and hypertensive disorders. *Scientific Reports 9*(1), 9793.

Bailey, M. J. (2013). Fifty Years of Family Planning: New Evidence on the Long-Run Effects of Increasing Access to Contraception. *Brookings Papers on Economic Activity 2013*(1), 341–409.

Bailey, M. J., E. Brito Rebolledo, D. Gorgulu, K. Figone, V. W. Lang, A. Prettyman, and V. Dalton (2025, oct). Does Increasing Financial Access to Contraception in the U.S. Reduce Undesired Pregnancies? Evidence from the M-CARES Randomized Control Trial at Two Years. NBER Working Paper 34400, National Bureau of Economic Research. Revised February 2026.

Berke, E. M. and X. Shi (2009). Computing travel time when the exact address is unknown: a comparison of point and polygon ZIP code approximation methods. *International Journal of Health Geographics 8*, 23.

Borrero, S., N. Zite, J. E. Potter, and J. Trussell (2014). Medicaid policy on sterilization–anachronistic or still relevant? *New England Journal of Medicine 370*(2), 102–104.

Boulware, A., J. Wascher, Z. Wong, L. Hasselbacher, J. Chen, L. Freedman, and D. Stulberg (2024). Missed opportunities in postpartum contraception: Bridging the gaps for patients who deliver at Catholic hospitals in Illinois. *Reproductive, Female and Child Health 3*(1), e75.

Bozkurt, B., A. M. Planey, M. Aijaz, J. M. Weinstein, D. Cilenti, C. M. Shea, and S. Khairat (2024). Disparities in Maternal Health Visits Between Rural and Urban Communities in the United States, 2016-2018. *The Permanente Journal 28*(2), 36–46.

Bullard, K. A., S. Ramanadhan, A. B. Caughey, and M. I. Rodriguez (2024). Immediate Postpartum Long-Acting Reversible Contraception for Preventing Severe Maternal Morbidity: A Cost-Effectiveness Analysis. *Obstetrics & Gynecology 144*(3), 294–303.

Caldwell, A., P. Schumm, M. Murugesan, and D. Stulberg (2022). Short-interval pregnancy in the Illinois Medicaid population following delivery in Catholic vs non-Catholic hospitals. *Contraception 112*, 105–110.

Call, V. R. A. and T. B. Heaton (1997). Religious Influence on Marital Stability. *Journal for the Scientific Study of Religion 36*(3), 382–392.

Card, D., A. Fenizia, and D. Silver (2023). The Health Impacts of Hospital Delivery Practices. *American Economic Journal: Economic Policy 15*(2), 42–81.

Catholic Health Association of the United States (2023). Catholic Health Care Directory. Accessed on April 13, 2024.

CDC (2024, jul). American Hospital Association (AHA) Annual Survey of Hospitals. Accessed on June 11, 2025.

Centers for Disease Control and Prevention (2024, may). Identifying Severe Maternal Morbidity (SMM). Accessed on September 10, 2025.

Centers for Medicare & Medicaid Services (2024, oct). ICD Code Lists. Accessed on September 24, 2025.

Chernozhukov, V., C. Hansen, and M. Spindler (2015). Post-selection and post-regularization inference in linear models with many controls and instruments. *American Economic Review: Papers & Proceedings 105*(5), 486–490.

Chiburis, R. C., J. Das, and M. Lokshin (2012). A Practical Comparison of the Bivariate Probit and Linear IV Estimators. *Economics Letters 117*(3), 762–766.

Chuang, C. H., G. A. Chase, D. M. Bensyl, and C. S. Weisman (2005). Contraceptive use by diabetic and obese women. *Women's Health Issues 15*(4), 167–173.

Clark, S. and Z. Levy (2025). Sterilization, Infecundity, and Reproductive Autonomy in Rural, Suburban, and Urban America: Results From a National Survey. *Perspectives on Sexual and Reproductive Health 57*, 72–84.

CMS (2024, mar). Hospital Change of Ownership. CMS Data. Accessed on September 1, 2025.

Conde-Agudelo, A., A. Rosas-Bermúdez, and A. C. Kafury-Goeta (2006). Birth spacing and risk of adverse perinatal outcomes: a meta-analysis. *JAMA 295*(15), 1809–1823.

Conde-Agudelo, A., A. Rosas-Bermúdez, and A. C. Kafury-Goeta (2007). Effects of birth spacing on maternal health: a systematic review. *American Journal of Obstetrics and Gynecology 196*(4), 297–308.

Cooper, M., K. Black, and S. Cameron (2024). Expanding access to postpartum contraception. *Current Opinion in Obstetrics and Gynecology 36*(5), 331–337.

Cooper, Z., S. Craig, M. Gaynor, and J. Van Reenen (2022). Merger Data. Health Care Pricing Project. Accessed on November 12, 2024.

Cornell, P. Y., D. C. Grabowski, E. C. Norton, and M. Rahman (2019). Do report cards predict future quality? The case of skilled nursing facilities. *Journal of Health Economics 66*, 208–221.

Danieli, O., D. Nevo, I. Walk, B. Weinstein, and D. Zeltzer (2023). Negative Control Falsification Tests for Instrumental Variable Designs. arXiv preprint 2312.15624, arXiv. v3, revised 27 Apr 2025.

DHHS (2024, jan). Safeguarding the Rights of Conscience as Protected by Federal Statutes. *Federal Register 89*(8), 2078–2109. Final Rule; 45 CFR Part 88.

DHHS (2025). Reduce the proportion of pregnancies conceived within 18 months of a previous birth — FP-02. Healthy People 2030. Accessed on November 5, 2025.

Drake, C., M. Jarlenski, Y. Zhang, and D. Polsky (2020). Market Share of US Catholic Hospitals and Associated Geographic Network Access to Reproductive Health Services. *JAMA Network Open 3*(1), e1920053.

Dreweke, J., J. Frost, M. Cherins, and A. VandeVusse (2025, nov). Publicly Supported Family Planning Services in the United States. Fact sheet, Guttmacher Institute.

Dude, A. M., K. Schueler, L. P. Schumm, M. Murugesan, and D. B. Stulberg (2022). Preconception care and severe maternal morbidity in the United States. *American Journal of Obstetrics and Gynecology MFM 4*(2), 100549.

Ellis, R. J., T. K. Yuce, D. B. Hewitt, R. P. Merkow, C. V. Kinnier, J. K. Johnson, and K. Y. Bilimoria (2020). National evaluation of patient preferences in selecting hospitals and health care providers. *Medical Care 58*(10), 894–901.

Ellison, J. E., B. L. Brown-Podgorski, and J. R. Morgan (2024). Changes in Permanent Contraception Procedures Among Young Adults Following the Dobbs Decision. *JAMA Health Forum 5*(4), e240424. An erratum was published for this article.

Eunice Kennedy Shriver National Institute of Child Health and Human Development (2023, aug). What are possible causes of stillbirth? Accessed on November 20, 2025.

Fang, N. Z. and C. L. Westhoff (2022). Update on incidence of inpatient tubal ligation and long-acting reversible contraception in the United States. *American Journal of Obstetrics and Gynecology 227*(3), 477.e1–477.e7.

Fontenot, J., C. Brigance, R. Lucas, and A. Stoneburner (2024). Navigating geographical disparities: access to obstetric hospitals in maternity care deserts and across the United States. *BMC Pregnancy and Childbirth 24*(1), 350.

Freedman, L. (2023). *Bishops and Bodies: Reproductive Care in American Catholic Hospitals.* New Brunswick, NJ: Rutgers University Press.

Freedman, L. R., L. E. Hebert, M. F. Battistelli, and D. B. Stulberg (2018). Religious hospital policies on reproductive care: what do patients want to know? *American Journal of Obstetrics and Gynecology 218*(2), 251.e1–251.e9.

FTC (2024, oct). FTC Finalizes Changes to Premerger Notification Form. Press Release. Accessed on January 6, 2026.

Galal, M., I. Symonds, H. Murray, F. Petraglia, and R. Smith (2012). Postterm pregnancy. *Facts, Views & Vision in ObGyn 4*(3), 175–187.

Gariepy, A. M., J. Y. Duffy, and X. Xu (2015). Cost-Effectiveness of Immediate Compared With Delayed Postpartum Etonogestrel Implant Insertion. *Obstetrics & Gynecology 126*(1), 47–55.

Gaynor, M., K. Ho, and R. J. Town (2015). The Industrial Organization of Health-Care Markets. *Journal of Economic Literature 53*(2), 235–284.

Genazzani, A. R., T. Fidecicchi, D. Arduini, G. Benagiano, M. Birkhaeuser, J. Schenker, J. F. Strauss, D. Vasquez-Awad, J. F. Arnal, L. Bahamondes, J. Bitzer, T. Buchholz, M. Brincat, C. Celis-González, G. Creatsas, N. R. De Melo, J. Douxfils, K. Gemzell-Danielsson, A. D. Genazzani, A. Gompel, G. Grandi, O. Heikinheimo, L. Hernández-Guzmán, S. Kumari, M. M. Lintner, S. Luisi, L. Mettler, R. E. Nappi, S. Palacios, F. Parazzini, F. Petraglia, G. Pfleiderer, G. Rosano, G. I. Serour, T. Simoncini, R. Sitruk-Ware, R. Sohail, R. Urrutia, V. Versace, and A. B. Kihara (2025). Contraception today and family planning: a comprehensive review and position statement on the ethical, medical, and social dimensions of modern contraception. *Gynecological Endocrinology 41*(1), 2543423.

Gowrisankaran, G., A. Nevo, and R. Town (2015). Mergers when prices are negotiated: evidence from the hospital industry. *American Economic Review 105*(1), 172–203.

Grammich, C. A., E. Dollhopf, M. Gautier, R. Houseal, and D. E. Jones (2020). U.S. Religion Census - Religious Congregations and Membership Study (County File).

Guiahi, M. (2018). Catholic Health Care and Women's Health. *Obstetrics & Gynecology 131*(3), 534–537.

Guiahi, M. (2020). Readability of Catholic institutional health care directives on reproductive care. *American Journal of Obstetrics and Gynecology 222*(4), 382.

Guiahi, M., P. E. Helbin, S. B. Teal, D. Stulberg, and J. Sheeder (2019). Patient Views on Religious Institutional Health Care. *JAMA Network Open 2*(12), e1917008.

Guiahi, M., J. Sheeder, and S. Teal (2014). Are women aware of religious restrictions on reproductive health at Catholic hospitals? A survey of women's expectations and preferences for family planning care. *Contraception 90*(4), 429–434.

Hall, K. S., C. Moreau, and J. Trussell (2013). The link between substance use and reproductive health service utilization among young U.S. women. *Substance Abuse 34*(3), 283–291.

Hayes, D. K., C. L. Robbins, and J. Y. Ko (2020). Trends in Selected Chronic Conditions and Related Risk Factors Among Women of Reproductive Age: Behavioral Risk Factor Surveillance System, 2011–2017. *Journal of Women's Health 29*(12), 1576–1585.

Hebert, L. E., L. Freedman, and D. B. Stulberg (2020). Choosing a hospital for obstetric, gynecologic, or reproductive healthcare: what matters most to patients? *American Journal of Obstetrics and Gynecology MFM 2*(1), 100067.

Hill, E. L., D. J. G. Slusky, and D. K. Ginther (2019). Reproductive health care in Catholic-owned hospitals. *Journal of Health Economics 65*, 48–62.

HRSA (2024, nov). Area Health Resources Files, County Level Data. Dataset. Accessed on November 23, 2025.

Imbens, G. W. and J. D. Angrist (1994). Identification and Estimation of Local Average Treatment Effects. *Econometrica 62*(2), 467–475.

Janis, J. A., K. A. Ahrens, K. B. Kozhimannil, and E. C. Ziller (2021). Contraceptive Method Use by Rural–Urban Residence among Women and Men in the United States, 2006 to 2017. *Women's Health Issues 31*(3), 277–285.

Jones, R. K. (2020, oct). People of All Religions Use Birth Control and Have Abortions. Guttmacher Institute. Accessed on November 8, 2025.

Jones, R. K. and J. Dreweke (2011, apr). Countering Conventional Wisdom: New Evidence on Religion and Contraceptive Use. Technical report, Guttmacher Institute.

Koller, D., W. Maier, N. Lack, E. Grill, and R. Strobl (2024). Choosing a maternity hospital: a matter of travel distance or quality of care? *Research in Health Services & Regions 3*, 7.

Konetzka, R. T. and R. M. Werner (2025). Correct Usage and Limitations of Differential Distance as an Instrumental Variable. *JAMA Network Open 8*(8), e2525343.

Kramer, R. D., J. A. Higgins, M. E. Burns, D. B. Stulberg, and L. R. Freedman (2021). Expectations about availability of contraception and abortion at a hypothetical Catholic hospital: Rural-urban disparities among Wisconsin women. *Contraception 104*(5), 506–511.

Laszlo, S., M. F. Majid, and L. Renée (2023). Conditional cash transfers and women's reproductive choices. *Health Economics*, 1–25.

Lee, D., A. Basu, J. A. Dugan, and P. Karaca-Mandic (2025). Do for-profit hospitals cream-skim patients? Evidence from inpatient psychiatric care in California. *Journal of Health Economics 102*, 103027.

Lee, H., C. C. C. Lin, and J. E. Snyder (2020). Rural–Urban Differences in Health Care Access Among Women of Reproductive Age: A 10-Year Pooled Analysis. *Annals of Internal Medicine 173*, S55–S58.

Lehrer, E. L. (2004). Religion as a Determinant of Economic and Demographic Behavior in the United States. *Population and Development Review 30*, 707–726.

Levin Associates (2024). LevinPro HC.

Li, B., D. Evans, P. Faris, S. Dean, and H. Quan (2008). Risk adjustment performance of Charlson and Elixhauser comorbidities in ICD-9 and ICD-10 administrative databases. *BMC Health Services Research 8*, 12.

Lindo, J. M. and A. Packham (2017). How Much Can Expanding Access to Long-Acting Reversible Contraceptives Reduce Teen Birth Rates? *American Economic Journal: Economic Policy 9*(3), 348–376.

Liu, A., V. Hernandez, D. Stulberg, P. Schumm, M. Murugesan, A. McHugh, and A. Dude (2025). Short-Interval Pregnancy Following Delivery in Catholic-Affiliated Versus Non-Catholic-Affiliated Hospitals Among Patients Insured Through the Medicaid Program. *Perspectives on Sexual and Reproductive Health*.

Marbach, M. and D. Hangartner (2020). Profiling Compliers and Noncompliers for Instrumental-Variable Analysis. *Political Analysis 28*(3), 435–444.

Marcelino, A. C., P. d. C. Pereira, and L. Bahamondes (2025). The economic impact of Long-Acting Contraceptives (LARCs) on public health. *Clinics 80*, 100598.

McClellan, M., B. J. McNeil, and J. P. Newhouse (1994). Does more intensive treatment of acute myocardial infarction in the elderly reduce mortality?: analysis using instrumental variables. *JAMA 272*(11), 859–866.

McGuirk, M. A. and F. W. Porell (1984). Spatial patterns of hospital utilization: the impact of distance and time. *Inquiry : a journal of medical care organization, provision and financing 21*(1), 84–94.

Meille, G. and J. N. Monnet (2024). Catholic Hospital Affiliation and Postpartum Contraceptive Care and Subsequent Deliveries. *JAMA Internal Medicine*.

Menegay, M. C., R. Andridge, K. Rivlin, and M. F. Gallo (2022). Delivery at Catholic hospitals and postpartum contraception use, five US states, 2015–2018. *Perspectives on Sexual and Reproductive Health 54*(1), 5–11.

Myers, C. (2025). Myers Abortion Facility Database. Open Science Framework. Published online May 9, 2025.

Myers, C. K., D. L. Dench, and M. Pineda-Torres (2025, mar). The Road Not Taken: How Driving Distance and Appointment Availability Shape the Effects of Abortion Bans. NBER Working Paper 33548, National Bureau of Economic Research.

NCHS (2023, sep). NCHS Urban-Rural Classification Scheme for Counties. Accessed on September 12, 2025.

Packham, A. (2017). Family planning funding cuts and teen childbearing. *Journal of Health Economics 55*, 168–185.

Phillips-Bell, G. S., W. Sappenfield, C. L. Robbins, and L. Hernandez (2016). Chronic Diseases and Use of Contraception Among Women at Risk of Unintended Pregnancy. *Journal of Women's Health 25*(12), 1262–1269.

P.J. Kenedy & Sons (2023). *Official Catholic Directory*. P.J. Kenedy & Sons. Accessed 2024-04-13.

Pollack, L. M., J. Chen, S. Cox, et al. (2022). Healthcare Utilization and Costs Associated With Perinatal Depression Among Medicaid Enrollees. *American Journal of Preventive Medicine 62*(6), e333–e341.

Popowitz, E. (2025, apr). Top 10 Largest Health Systems in the U.S. Definitive Healthcare. Accessed on February 10, 2026.

Potter, J. E., K. Hopkins, A. R. A. Aiken, and C. Hubert (2014). Unmet demand for highly effective postpartum contraception in Texas. *Perspectives on Sexual and Reproductive Health 46*(3), 129–135.

Potter, J. E., A. J. Stevenson, K. White, K. Hopkins, and D. Grossman (2013). Hospital variation in postpartum tubal sterilization rates in California and Texas. *Obstetrics & Gynecology 121*(1), 152–158.

Quan, H., V. Sundararajan, P. Halfon, A. Fong, B. Burnand, J.-C. Luthi, L. D. Saunders, C. A. Beck, T. E. Feasby, and W. A. Ghali (2005). Coding Algorithms for Defining Comorbidities in ICD-9-CM and ICD-10 Administrative Data. *Medical Care 43*(11), 1130–1139.

Raval, D. and T. Rosenbaum (2021). Why is Distance Important for Hospital Choice? Separating Home Bias From Transport Costs. *The Journal of Industrial Economics 69*(2), 338–368.

Rodriguez, M. I., A. Daly, T. Meath, K. Watson, and K. J. McConnell (2023). Catholic sole community hospitals are associated with decreased receipt of postpartum permanent contraception among Medicaid recipients. *Contraception 122*, 109959.

Rodriguez, M. I., T. H. A. Meath, K. Watson, A. Daly, K. J. McConnell, and H. Kim (2024). Medicaid Policy Change and Immediate Postpartum Long-Acting Reversible Contraception. *JAMA Health Forum 5*(6), e241359.

Schmall, E. (2024, sep). When States Banned Abortion, Sterilizations Rose Among Women. The New York Times. Accessed on November 4, 2025.

Schulte, A., B. Staiger, H. P. Rodriguez, and A. L. Brewster (2025). Mission vs. Margin: The Effects of Catholic Health System Ownership on Hospital Operations. *Medical Care Research and Review*, 10775587251355541.

Seibel-Seamon, J., J. F. Visintine, B. E. Leiby, and L. Weinstein (2009, mar). Factors predictive for failure to perform postpartum tubal ligations following vaginal delivery. *The Journal of Reproductive Medicine 54*(3), 160–164.

Sharma, K., S. Cox, L. Romero, D. Ekwueme, M. Whiteman, C. Kroelinger, and L. Ouyang (2024). State variations in insertion of long-acting reversible contraception during delivery hospitalization. *Contraception 138*, 110509.

Sheyn, D. and K. S. Arora (2021). Changes in rates of inpatient postpartum long-acting reversible contraception and sterilization in the USA, 2012–2016. *Maternal and Child Health Journal 25*(10), 1562–1573.

Shi, X., W. Miao, and E. J. Tchetgen Tchetgen (2020). A Selective Review of Negative Control Methods in Epidemiology. *Current Epidemiology Reports 7*(4), 190–202.

Sloan, F. (2000). Not-for-profit ownership and hospital behavior. In A. J. Culyer and J. P. Newhouse (Eds.), *Handbook of Health Economics*, Volume 1, pp. 1141–1174. Elsevier.

Solomon, T., L. Uttley, P. HasBrouck, and Y. Jung (2020). Bigger and Bigger: The Growth of Catholic Health Systems. Technical report, Community Catalyst, Boston, MA. Accessed on 2023-09-01.

Steenland, M. W., C. K. Geiger, L. Chen, et al. (2021). Declines in contraceptive visits in the United States during the COVID-19 pandemic. *Contraception 104*(6), 593–599.

Steenland, M. W., R. Vatsa, L. E. Pace, and J. L. Cohen (2022). Immediate Postpartum Long-Acting Reversible Contraceptive Use Following State-Specific Changes in Hospital Medicaid Reimbursement. *JAMA Network Open 5*(10), e2237918.

Strasser, J., E. Schenk, S. Luckenbill, D. Tsevat, L. King, Q. Luo, and J. Maslowsky (2025, jan). Tubal Sterilization And Vasectomy Increased Among US Young Adults After The Dobbs Supreme Court Decision In 2022. *Health Affairs 44*(1).

Stulberg, D. B., L. Cain, I. H. Dahlquist, and D. S. Lauderdale (2017). Pre-pregnancy and Early Prenatal Care are Associated with Lower Risk of Ectopic Pregnancy Complications in the Medicaid Population: 2004–08. *Paediatric and Perinatal Epidemiology 31*(1), 4–10.

Stulberg, D. B., M. Guiahi, L. E. Hebert, and L. R. Freedman (2019). Women's Expectation of Receiving Reproductive Health Care at Catholic and Non-Catholic Hospitals. *Perspectives on Sexual and Reproductive Health 51*(3), 135–142.

Takahashi, J., A. Cher, J. Sheeder, S. Teal, and M. Guiahi (2019). Disclosure of Religious Identity and Health Care Practices on Catholic Hospital Websites. *JAMA 321*(11), 1103.

Taylor, L. A., B. Franz, A. Zink, A. Fair, and C. E. Cronin (2023, oct). Public perceptions of US for-profit, nonprofit, and public hospitals. *Health Affairs Scholar 1*(4), qxad046.

Templeton, Z. S., N. C. Apathy, R. T. Konetzka, M. M. Skira, and R. M. Werner (2023). The health effects of nursing home specialization in post-acute care. *Journal of Health Economics 92*, 102823.

Terza, J. V., A. Basu, and P. J. Rathouz (2008). Two-Stage Residual Inclusion Estimation. *Journal of Health Economics 27*(3), 531–543.

Thayagabalu, S., D. J. Lemas, S. Wright, B. L. Gillum, and J. S. Bahorski (2025). Cross-Sectional Study of Mothers' Perception of Postpartum Visit Benefit by Rural Status. *Journal of Prenatal and Perinatal Psychology and Health 39*(1), 5–21.

Thorne, N. B., T. K. Soderborg, J. J. Glover, L. Hoffecker, and M. Guiahi (2019). Reproductive Health Care in Catholic Facilities: A Scoping Review. *Obstetrics & Gynecology 133*(1), 105.

Thurman, A. R. and T. Janecek (2010). One-year follow-up of women with unfulfilled postpartum sterilization requests. *Obstetrics & Gynecology 116*(5), 1071–1077.

U.S. Census Bureau (2025, mar). Growth in Metro Areas Outpaced Nation. Accessed on September 12, 2025.

U.S. Conference of Catholic Bishops (2018, June). Ethical and religious directives for Catholic health care services. Technical Report 6th ed., U.S. Conference of Catholic Bishops, Washington, D.C. Accessed 2023-10-08.

van Walraven, C., P. C. Austin, A. Jennings, H. Quan, and A. J. Forster (2009). A modification of the Elixhauser comorbidity measures into a point system for hospital death using administrative data. *Medical Care 47*(6), 626–633.

Vizient, Inc. and JD Healthcare, Inc. (2018, sep). Effect of the Ministry Alignment Agreement between Dignity Health and Catholic Health Initiatives on the Availability and Accessibility of Healthcare Services to the Communities Served by Dignity Health's Hospitals Located in Nevada County. Health impact report, Office of the California Attorney General. Accessed on September 12, 2025.

Wahlstedt, J., E. Wahlstedt, J. Ramsay, A. Woodcock, J. Sanders, M. Pangasa, D. Turok, K. Gross, J. Hotaling, and J. Schardein (2025). A nationwide analysis of the trends in permanent contraception utilization before and after the Dobbs ruling. *Fertility and Sterility 124*(2), 372–374.

Wang, Y., C. Zeng, Y. Chen, L. Yang, D. Tian, X. Liu, and Y. Lin (2022). Short interpregnancy interval can lead to adverse pregnancy outcomes: A meta-analysis. *Frontiers in Medicine 9*, 922053.

Wascher, J. M., L. E. Hebert, L. R. Freedman, and D. B. Stulberg (2018). Do women know whether their hospital is Catholic? Results from a national survey. *Contraception 98*(6), 498–503.

Wascher, J. M., D. B. Stulberg, and L. R. Freedman (2020). Restrictions on Reproductive Care at Catholic Hospitals: A Qualitative Study of Patient Experiences and Perspectives. *AJOB Empirical Bioethics 11*(4), 257–267.

Wolfe, K. K., M. D. Wilson, M. Y. Hou, and M. D. Creinin (2017). An updated assessment of postpartum sterilization fulfillment after vaginal delivery. *Contraception 96*(1), 41–46.

Zapata, L. B., S. Murtaza, M. K. Whiteman, D. J. Jamieson, C. L. Robbins, P. A. Marchbanks, D. V. D'Angelo, and K. M. Curtis (2015). Contraceptive counseling and postpartum contraceptive use. *American Journal of Obstetrics and Gynecology 212*(2), 171.e1–171.e8.

Zhu, Y., B. T. Bateman, S. Hernandez-Diaz, et al. (2023). Validation of claims-based algorithms to identify non-live birth outcomes. *Pharmacoepidemiology and Drug Safety 32*(4), 468–474.

Zite, N., S. Wuellner, and M. Gilliam (2005). Failure to obtain desired postpartum sterilization: risk and predictors. *Obstetrics & Gynecology 105*(4), 794–799.

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: 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
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 (patient county)			
Mean Catholic adherents (SD)	23 (11)	21 (14)	23 (11)
Mean OBGYN physicians (SD)	13 (8)	12 (6)	13 (8)
Mean abortion clinics (SD)	5 (5)	3 (3)	4 (5)
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. Hospital concentration measured using the Herfindahl-Hirschman Index (HHI) with annual birth volumes per hospital service area (HSA).

Table 3: Outcome Summary Statistics

	Hospital Type		
	Non-ERD-adherent	ERD-adherent	All Hospitals
Panel A. Contraception			
Permanent contraception	6.63%	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 4: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision

	(1) OLS	(2) 2SLS
Panel A. Permanent contraception		
ERD-adherent	-0.0392***	-0.0382***
	(0.00390)	(0.0123)
Mean dep. var.	0.0606	0.0606
Panel B. Non-permanent contraception		
ERD-adherent	-0.0257***	-0.0139*
	(0.00246)	(0.00822)
Mean dep. var.	0.0388	0.0388
Observations	9,013,727	9,013,727
First stage F-stat		95.14

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

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

	(1) Rural Patients	(2) Urban Patients
Panel A. Permanent contraception		
ERD-adherent	-0.0519***	-0.0505**
	(0.0130)	(0.0221)
Mean dep. var.	0.0741	0.0558
Panel B. Non-permanent contraception		
ERD-adherent	-0.0312***	-0.00304
	(0.0087)	(0.0173)
Mean dep. var.	0.0441	0.0369
Observations	2,405,223	6,608,504
First stage F-stat	72.27	24.77

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

	(1) Private insurance	(2) Medicaid
Panel A. Permanent contraception		
ERD-adherent	-0.0377***	-0.0377**
	(0.0097)	(0.0155)
Mean dep. var.	0.0509	0.0716
Panel B. Non-permanent contraception		
ERD-adherent	-0.0176***	-0.0120
	(0.0055)	(0.0114)
Mean dep. var.	0.0291	0.0499
Observations	4,528,677	3,978,114
First stage F-stat	87.73	85.39

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

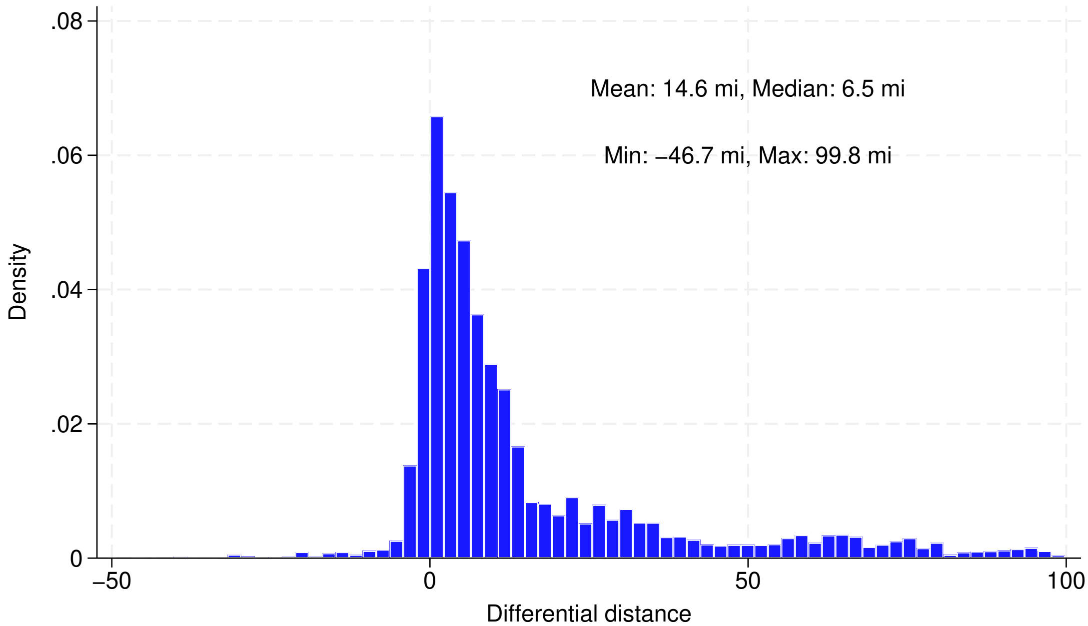
Table 7: The Effect of ERD-adherent Hospitalization on Short-Interval Pregnancy, 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. <6 months short-interval pregnancy			
ERD-adherent	0.00112	0.0107**	-0.00641
	(0.00437)	(0.00495)	(0.00788)
Mean dep. var.	0.0318	0.0377	0.0295
Panel B. <12 months short-interval pregnancy			
ERD-adherent	0.00826	0.0253**	0.00260
	(0.00898)	(0.0112)	(0.0167)
Mean dep. var.	0.0776	0.0846	0.0749
Panel C. <18 months short-interval pregnancy			
ERD-adherent	0.0146	0.0436**	0.00924
	(0.0142)	(0.0182)	(0.0269)
Mean dep. var.	0.1262	0.1329	0.1235
Observations	5,217,931	1,460,139	3,757,792
First stage F-stat	69.60	52.26	23.35

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.

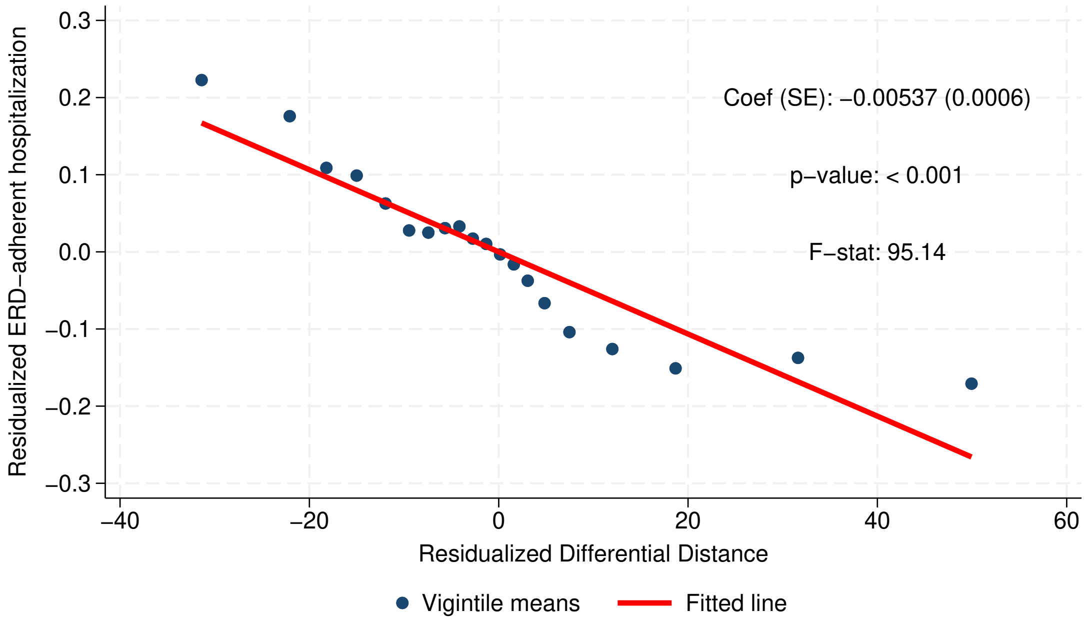
9 Figures

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



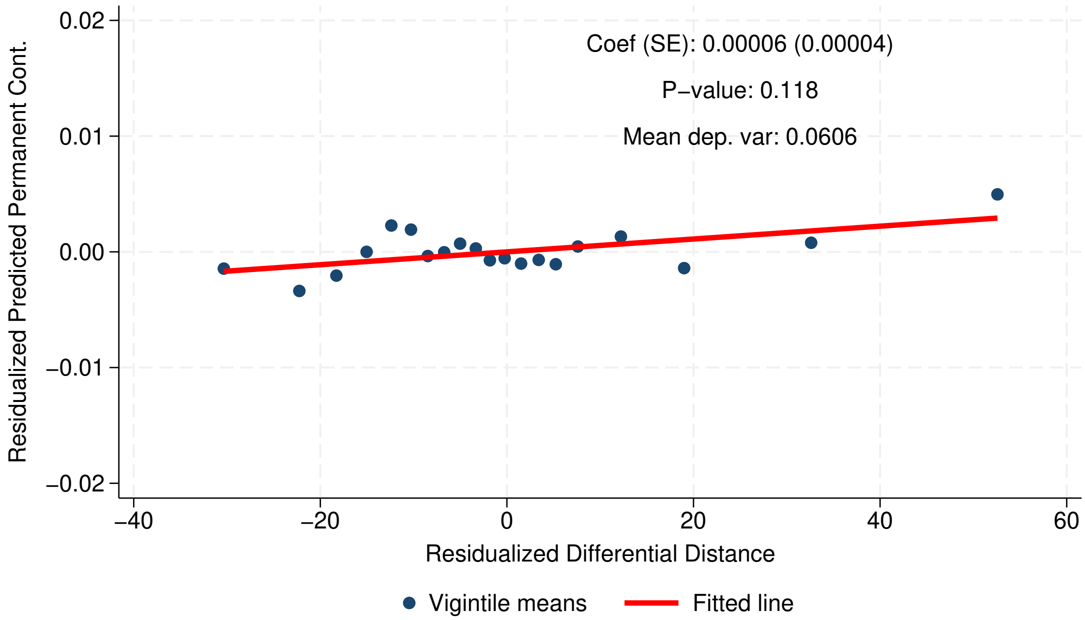
Notes: This figure shows the distribution of the differential distance (DD) instrument for the main analytic sample, which is restricted to observations where the absolute DD is less than 100 miles to ensure common support. The instrument is calculated as the distance from a patient's ZIP code centroid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

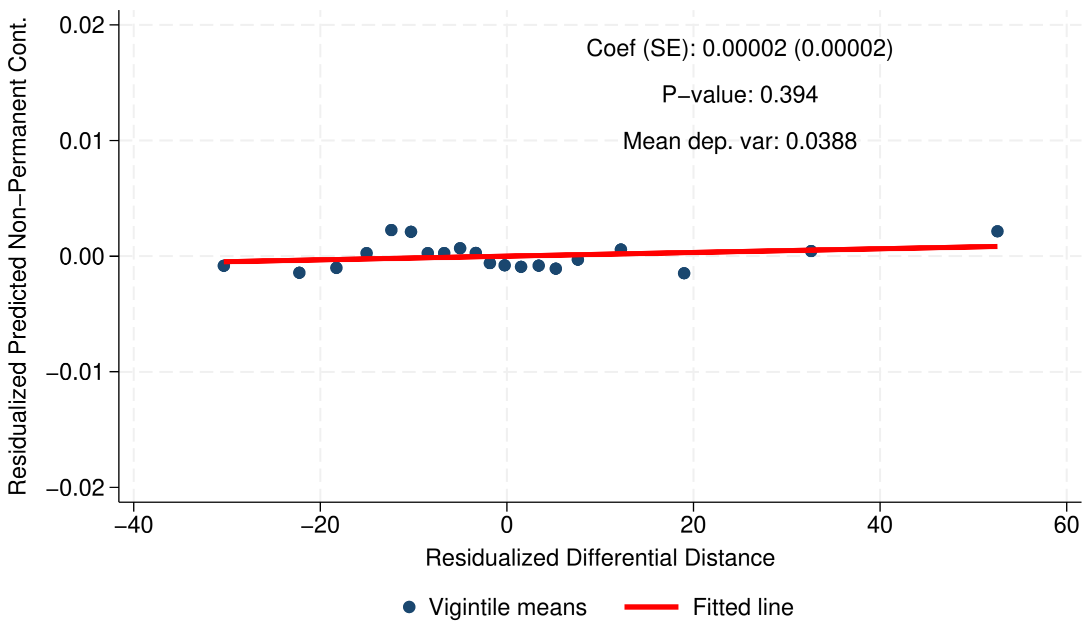


Notes: This figure presents a binned scatterplot illustrating the first-stage relationship between the differential distance (DD) instrument and admission to an ERD-adherent hospital. Both the treatment variable (admission to an ERD-adherent hospital) and the differential distance instrument are residualized 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



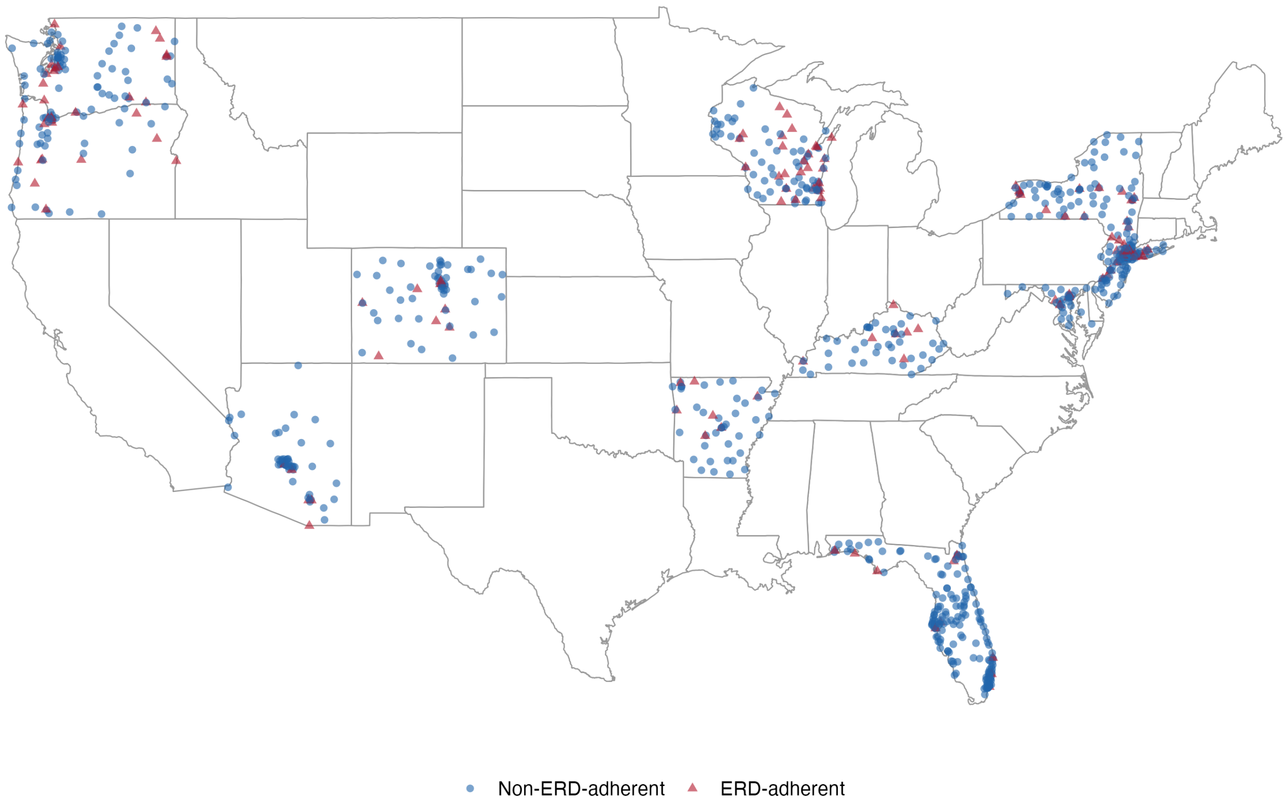
(a) Permanent Contraception



(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.

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



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

A Appendix

A.1 Additional Tables and Figures

Additional 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
$\|DD\| > 100$	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: 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 A5: 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.

Appendix Table A6: Descriptive Statistics: Closest hospital and patient location

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. Distance to birth hospital			
Mean distance, miles (SD)	9.1 (13.2)	13.1 (18.4)	7.7 (10.4)
Panel B. Type of closest hospital			
ERD-adherent closest to patient (%)	13.6	16.8	12.4
ERD-adherent closest & patient went there (%)	42.5	50.2	38.7
Observations	9,013,727	2,405,223	6,608,504

Notes: 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.

Appendix Table A7: Negative control outcome (NCO) tests

	Coefficient
Panel A. Main dataset	
Marital status	-0.000117
	(0.000159)
Asthma	-0.000000266
	(0.0000000898)
Stillborn	0.0000000915
	(0.00000412)
Prolonged pregnancy	-0.00000747
	(0.000151)
Panel B. Dataset with unique patient identifiers	
Parity	-0.000422
	(0.000485)
Age at first birth	-0.00139
	(0.00389)

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by patient county, are in parentheses. 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 A8: Rates of Contraception Provision by Delivery Method

	(1) Vaginal birth	(2) C-section	(3) Full sample
Panel A. Delivery type			
All hospitalizations (%)	66.8	33.3	100
ERD-adherent hospitalizations (%)	68.1	31.9	100
Non-ERD-adherent hospitalizations (%)	66.5	33.5	100
Panel B. Permanent contraception provided			
All hospitalizations (%)	17.1	82.9	100
ERD-adherent hospitalizations (%)	13.4	86.6	100
Non-ERD-adherent hospitalizations (%)	17.4	82.6	100
Panel C. Non-permanent contraception provided			
All hospitalizations (%)	25.1	74.9	100
ERD-adherent hospitalizations (%)	14.9	85.1	100
Non-ERD-adherent hospitalizations (%)	25.8	74.2	100

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

Appendix Table A9: Characteristics of Compliers, Always-Takers, and Never-Takers

	(1) Compliers	(2) Always-Takers	(3) Never-Takers
Age (mean)	29	29	29
White (%)	46	53	49
Black (%)	11	13	16
Hispanic (%)	24	19	21
Private insurance (%)	47	55	51
Medicaid (%)	48	39	43
Urban (%)	78	71	71
Married (%)	21	21	22
Urgent visit (%)	67	40	41
C-section (%)	32	33	34
Elixhauser score (mean)	-0.01	-0.20	-0.25

Note: This table reports average characteristics for compliers, always-takers, and never-takers. Compliers are patients whose hospital choice responds to differential distance. "Always-takers" deliver at ERD-adherent hospitals regardless of proximity. "Never-takers" deliver at non-ERD-adherent hospitals regardless of proximity. Binary variables reported as percentages. n = 9,013,727.

Appendix Table A10: Reduced-Form Effect of Instrument on Postpartum Contraception Provision

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. Permanent contraception			
Differential distance	0.000205***	0.000230***	0.000250***
	(0.0000624)	(0.0000585)	(0.0000798)
Y-mean	0.0606	0.0741	0.0558
Panel B. Non-permanent contraception			
Differential distance	0.0000744	0.000138***	0.0000150
	(0.0000457)	(0.0000395)	(0.0000863)
Y-mean	0.0388	0.0441	0.0369
Observations	9,013,727	2,405,223	6,608,504

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

Appendix Table A11: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision (using alternative rural-urban definitions), 2SLS Estimates

	OMB definition		Competition (HHI)	
	Rural (1)	Urban (2)	Low, rural (3)	High, urban (4)
Panel A. Permanent contraception				
ERD-adherent	-0.0609***	-0.0386***	-0.0558***	-0.0618**
	(0.0142)	(0.0134)	(0.0099)	(0.0280)
Y-mean	0.0814	0.0593	0.0669	0.0568
Panel B. Non-permanent contraception				
ERD-adherent	-0.0323***	-0.0112	-0.0285***	-0.0069
	(0.0091)	(0.0102)	(0.0056)	(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	82.67	65.98	88.40	27.72

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 above average levels of competition, defined using the Herfindahl-Hirschman Index (HHI) with annual birth volumes at hospitals located in patient county of residence. Rural is defined as non-urban.

Appendix Table A12: The Effect of ERD-adherent Hospitalization on Short-Interval Pregnancy (using alternative rural-urban definitions), 2SLS Estimates

	OMB definition		Competition (HHI)	
	Rural (1)	Urban (2)	Low, rural (3)	High, urban (4)
Panel A. <6 months short-interval pregnancy				
ERD-adherent	0.0100	-0.000671	0.00544	-0.0129
	(0.00698)	(0.00480)	(0.00427)	(0.00889)
Y-mean	0.0385	0.0314	0.0351	0.0299
Panel B. <12 months short-interval pregnancy				
ERD-adherent	0.0294*	0.00698	0.0222**	-0.0159
	(0.0169)	(0.0101)	(0.0100)	(0.0205)
Y-mean	0.0848	0.0772	0.0825	0.0749
Panel C. <18 months short-interval pregnancy				
ERD-adherent	0.0471*	0.0140	0.0405**	-0.0140
	(0.0280)	(0.0163)	(0.0179)	(0.0340)
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	25.51	47.47	63.52	22.86

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 above average levels of competition, defined using the Herfindahl-Hirschman Index (HHI) with annual birth volumes at hospitals located in patient county of residence. Rural is defined as non-urban.

Appendix Table A13: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision (using sub-sample with unique patient identifiers), 2SLS Estimates

	2SLS
Panel A. Permanent contraception	
ERD-adherent	-0.0392**
	(0.0159)
Y-mean	0.0617
Panel B. Non-permanent contraception	
ERD-adherent	-0.0126
	(0.0103)
Y-mean	0.0415
Observations	5,217,931
First stage F-stat	69.60

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 necessary for the short-interval pregnancy analyses.

Robustness Checks

Appendix Table A14: 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			
Catholic-affiliated	-0.0369***	-0.0488***	-0.0479**
	(0.0118)	(0.0113)	(0.0207)
Y-mean	0.0606	0.0741	0.0558
Panel B. Non-permanent contraception			
Catholic-affiliated	-0.0134*	-0.0293***	-0.00289
	(0.00797)	(0.00765)	(0.0164)
Y-mean	0.0388	0.0441	0.0369
Observations	9,013,727	2,405,223	6,608,504
First stage F-stat	106.09	96.20	27.48

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. In this robustness check, we define treatment as going to a hospital that adheres to the ERDs or is affiliated with a Catholic system (but may not be required to adhere to the ERDs).

Appendix Table A15: 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	-0.0360***	-0.0544***	-0.0505**
	(0.0129)	(0.0148)	(0.0221)
Y-mean	0.0608	0.0741	0.0558
Panel B. Non-permanent contraception			
ERD-adherent	-0.0151*	-0.0308***	-0.00304
	(0.00785)	(0.00947)	(0.0173)
Y-mean	0.0389	0.0443	0.0369
Observations	9,125,018	2,516,510	6,608,508
First stage F-stat	77.63	35.45	24.78

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 A16: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision (controlling for delivery type), 2SLS Estimates

	(1) Full Sample	(2) Rural Patients	(3) Urban Patients
Panel A. Permanent contraception			
ERD-adherent	-0.0384***	-0.0452***	-0.0488**
	(0.0121)	(0.0132)	(0.0227)
Y-mean	0.0606	0.0741	0.0558
Panel B. Non-permanent contraception			
ERD-adherent	-0.0140*	-0.0275***	-0.00215
	(0.00829)	(0.00868)	(0.0175)
Y-mean	0.0388	0.0441	0.0369
Observations	9,013,727	2,405,223	6,608,504
First stage F-stat	95.15	72.30	24.77

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

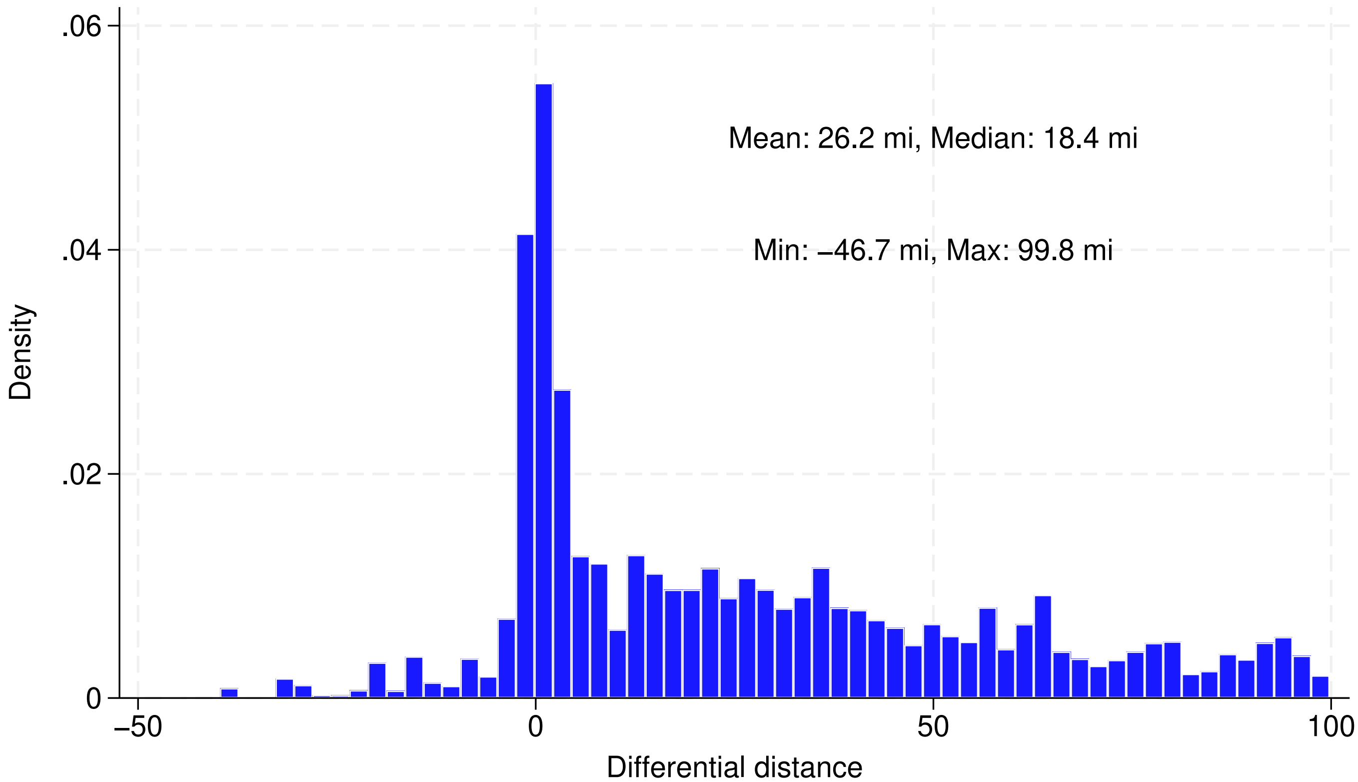
Appendix Table A17: The Effect of ERD-adherent Hospitalization on Postpartum Contraception (using machine learning LASSO algorithm), 2SLS estimates

	2SLS
Panel A. Permanent contraception	
ERD-adherent	-0.0521***
	(0.01246)
Y-mean	0.0606
Panel B. Non-permanent contraception	
ERD-adherent	-0.0325***
	(0.0078)
Y-mean	0.0388
Observations	9,013,727

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. This table reports results using covariates selected with a machine learning LASSO algorithm (Appendix Section A.3 provides details about this model).

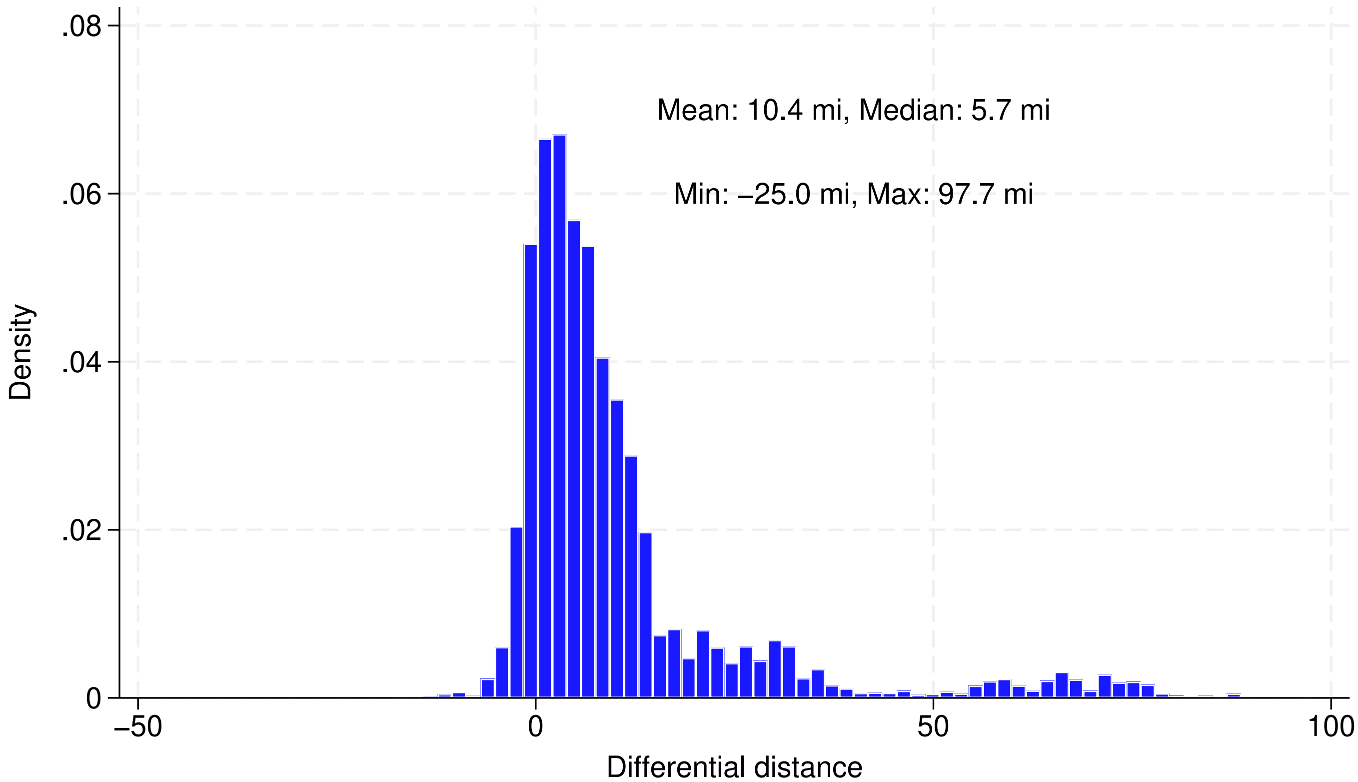
Additional Figures

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



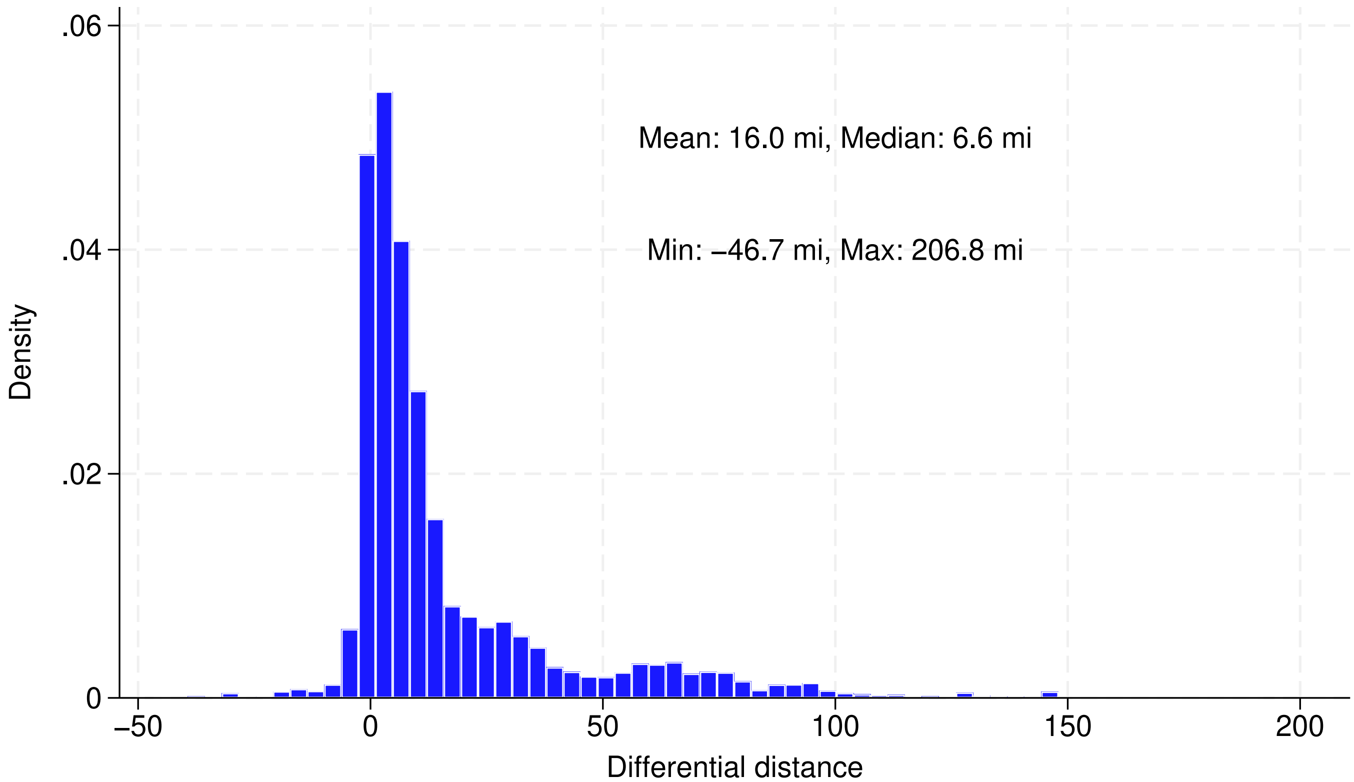
Notes: This figure shows the distribution of the differential distance (DD) instrument for the rural patient sub-sample ($|DD| < 100$ miles). The differential distance instrument is calculated as the distance from a patient's zip code centroid to the nearest ERD-adherent hospital minus the distance to the nearest non-ERD-adherent hospital. Rural classification follows the National Center for Health Statistics urban-rural classification scheme, with rural defined as counties outside large metropolitan areas of 1 million or more residents. Consistent with lower hospital density in rural areas, the distribution is more widely dispersed than for the urban sub-sample, with a larger mean of 26.2 miles. $N = 2{,}405{,}223$.

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



Notes: This figure shows the distribution of the differential distance (DD) instrument for the urban patient sub-sample (|DD| < 100 miles). The differential distance instrument is calculated as the distance from a patient's zip code centroid to the nearest ERD-adherent hospital minus the distance to the nearest non-ERD-adherent hospital. Urban classification follows the National Center for Health Statistics urban-rural classification scheme, with urban defined as counties in large metropolitan areas of 1 million or more residents. Compared to the rural sample, the distribution for urban patients is more tightly centered around zero (mean = 10.4 miles), reflecting the higher density of hospitals and smaller differences in travel distances in urban areas. $N = 6{,}608{,}504$.

Appendix Figure A3: DD instrument distribution, non-trimmed



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

A.2 Validation Process for ERD-Adherent Hospital Status

Accurately identifying which hospitals adhere to the Ethical and Religious Directives for Catholic Health Care Services (ERDs) each year is critical for 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.3 Machine learning covariate selection: LASSO-orthogonalized algorithm

To verify that our core findings are not artifacts of researcher bias in selecting covariates, we estimate our main outcomes using the LASSO-orthogonalized machine learning algorithm developed by Chernozhukov et al. (2015) as a robustness check. This approach addresses a fundamental challenge in empirical research: the "right" set of control variables is rarely known with certainty. Including too few controls risks omitted variable bias; including too many risks overfitting. The Chernozhukov et al. (2015) LASSO methodology resolves this tension using a data-driven penalized regression procedure that automatically selects the most predictive covariates from a high-dimensional candidate pool.

This method relies on the Frisch-Waugh-Lovell theorem to enact a "double-orthogonalization" process. Specifically, the algorithm uses LASSO-estimated coefficients to generate predicted values for the outcome, the endogenous treatment variable, and the instrument with respect to the high-dimensional controls. These predictions are subtracted from the actual data to produce orthogonalized residuals—variables from which the influence of the high-dimensional controls has been mathematically removed. The final IV regression is then estimated using only these residualized variables. Because the orthogonalization step uses raw (i.e., penalized) LASSO coefficients rather than post-selection OLS estimates, this approach is robust to imperfect variable selection and does not require the sparsity assumptions that motivate other LASSO approaches (Chernozhukov et al., 2015).

High-dimensional candidate pool

Our main specification includes 24 covariates along with state-year fixed effects (see Section 3). For the LASSO robustness check, we expanded the candidate pool by adding 45 additional variables across three domains:

- **Patient-level:** Indicators for 31 comorbidities: AIDS/HIV, alcohol abuse, blood loss anemia, cardiac arrhythmias, chronic pulmonary disease, coagulopathy, congestive heart failure, deficiency anemia, depression, diabetes (complicated), diabetes (uncomplicated), drug abuse, fluid and electrolyte disorders, hypertension (complicated), hypertension (uncomplicated), hypothyroidism, liver disease, lymphoma, metastatic cancer, obesity, other neurological disorders, paralysis, peptic ulcer disease (excluding bleeding), peripheral vascular disorders, psychoses, pulmonary circulation disorders, renal failure, rheumatoid arthritis/collagen vascular, solid tumor without metastasis, valvular disease, and weight loss.

- **Hospital-level:** Average daily census, community hospital designation, Medicaid expansion status, Medicaid payer mix, metropolitan location, presence of an emergency department, rural referral center status, sole community provider status, system member, total full-time equivalent employees, and total operating expenses.
- **Market-level:** Total religious adherents, total religious congregations (patient county), and median household income state quartile (patient zip code).
- Note: Patient age, race/ethnicity, and payer, as well as state-year fixed effects, were designated as "unpenalized," which forced the algorithm to retain these key variables in the model.