Title
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ISBN
9798189270383

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Publication Date
2026-05-01

Peer reviewed|Thesis/dissertation

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By

Alex Schulte

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Health Policy

in the

Graduate Division

of the

University of California, Berkeley

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Spring 2026

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Abstract

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Catholic hospitals represent a large and growing segment of the U.S. health care system. The number of Catholic hospitals has grown by over 20% in the last two decades and now approximately one in seven U.S. hospitals is Catholic. These hospitals operate under the Ethical and Religious Directives for Catholic Health Care Services (ERDs), a set of 77 directives that emphasize the Church's role in caring for poor and vulnerable populations, while also prohibiting certain services such as contraception, sterilization, and abortion. As Catholic health systems continue to expand, a fundamental question emerges: what are the consequences for hospital operations, access to care, and patient outcomes?

This dissertation addresses that question across three empirical studies, each using rigorous quasi-experimental methods to provide causal evidence on the consequences of Catholic identity in U.S. health care. Together, the three chapters examine how religious directives shape organizational behavior—from broad hospital operations to specific reproductive health services—and who is ultimately harmed when religious and medical logics conflict.

Chapter 1 examines how hospital operations change after acquisition by Catholic versus non-Catholic health systems, using a 14-year national panel and a staggered difference-in-differences design. Catholic acquisitions increase provision of some mission-oriented services (e.g., chaplaincy and charity care) and preserve obstetric care. However, cost-cutting behavior (e.g., reductions in employment and operating expenses) are similar across Catholic and non-Catholic acquisitions, suggesting financial pressures shape post-acquisition behavior regardless of religious ownership.

Chapter 2 turns to reproductive healthcare, providing the first causal evidence that hospital adherence to the ERDs reduces access to postpartum contraception and increases the risk of short-interval pregnancy. Using over nine million birth hospitalizations and an instrumental

variable model, I find that giving birth at an ERD-adherent hospital reduces permanent contraception by 63% and non-permanent contraception by 36% relative to non-ERD-adherent hospitals. Effects are concentrated among rural patients, for whom ERD-adherent hospitalization also increases the likelihood of a short-interval pregnancy (within 18 months) by 33%.

Chapter 3 examines how Catholic hospitals manage the tension between conflicting religious and medical logics. Using a quasi-experimental analysis of over one million hospitalizations and 27 qualitative interviews, I find that hospitals rely on delayed intervention and patient transfers to manage the conflict—strategies that preserve institutional identity but result in worse patient outcomes. When patients have an urgent, pregnancy-related admission at an ERD-adherent hospital, they are 0.93 percentage points less likely to be induced for an abortion (179% relative to the mean). In addition, vulnerable populations, specifically Hispanic patients and those with complex pregnancies, are more likely to experience abortion-related complications at ERD-adherent hospitals.

Taken together, this dissertation provides the first causal evidence on how religious restrictions in U.S. Catholic hospitals impact operations, access, and outcomes. The findings inform active policy debates regarding institutional conscience protections, antitrust oversight, and hospital transparency—providing evidence that religious institutional identity has significant, and unevenly distributed, consequences for patient access and health.

Acknowledgments

I thank my advisor Amanda Brewster and dissertation committee (Becky Staiger, Lori Freedman, Hector Rodriguez) for guidance and support. I appreciate the useful feedback from Mathijs De Vaan, Will Dow, Elaine Hill, Michael French, Masoud Kamalahmadi, Tamara Konetzka, Ambar La Forgia, Jong Myeong Lim, Caitlin Myers, George Pink, Sameer Srivastava, Debra Stulberg, Dan Zeltzer and numerous conference and seminar participants. I also thank Linden Lovett and Bianca Chiu for undergraduate research assistance. Last, I am grateful to Jason Christopher, Joleen Locunas, Noah Baker, and Ronald Sprouse for research IT support and consulting.

This study was reviewed and considered exempt by the University of California, Berkeley institutional review board (#2024-08-17681). 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 author 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.

Contents

1 Chapter 1

Hospital Operations: Differences after Catholic and non-Catholic System Acquisitions[1]

1.1 Introduction

Perhaps best summarized by the phrase “no margin, no mission,” Catholic health systems face conflicting pressures to maintain their mission-oriented identity while staying financially viable in the competitive, profit-driven American health care landscape. Trends in consolidation and acquisition have accelerated in recent decades as health systems—both Catholic and non-Catholic—seek to expand their market power and financial strength (Andreyeva et al., 2024; Burns et al., 2015; Homan and White, 2021; Lee and Alexander, 1999). The number of Catholic-owned or affiliated hospitals grew by 28% between 2001 and 2020, and today about one in seven U.S. hospitals is Catholic (Schulte et al., 2025; Solomon et al., 2020). Catholic hospitals are also larger, with more beds on average, than their non-Catholic counterparts (Cartwright et al., 2023; Schulte et al., 2025). Especially in rural areas, patients may have to travel long distances, if that is feasible, to access non-Catholic hospitals (Kramer et al., 2021; Solomon et al., 2020).

Catholic hospitals adhere to the Ethical and Religious Directives (ERDs), a list of 77 specific directives that govern hospital operations and clinical care (U.S. Conference of Catholic Bishops, 2018). The ERDs emphasize Catholic health care’s commitment to serving poor and vulnerable populations, including the uninsured, racial/ethnic minorities, immigrants, Medicaid beneficiaries, and rural populations (Doderer, 2011; Haddad, 2023; Hochman, 2017). In addition, Catholic hospitals are expected to provide pro-natalist obstetrics care to support “the sanctity of human life from its very beginning.” However, the ERDs also explicitly forbid Catholic hospitals to provide services such as abortion, contraception, and sterilization that “render procreation impossible” (U.S. Conference of Catholic Bishops, 2018).

Previous research has found Catholic hospitals were more likely to provide charity care and community benefit services compared to non-religious nonprofit hospitals (Ferdinand et al., 2014; Zare and Gabow, 2023), and more likely to offer stigmatized services (e.g., HIV/AIDS, psychiatric care) compared to public and for-profit hospitals (White et al., 2006). Some studies also suggest that Catholic hospitals may be more likely to provide obstetrics care and operate a neonatal intensive care unit (Garrido et al., 2012; Weisman et al., 1999). Given the increasing trend of obstetric unit closures and consolidation, especially in rural areas, this suggestive evidence warrants further investigation (Fischer et al., 2024; Henke et al., 2021; Kozhimannil et al., 2020).

However, research has also found that organizational structure, management, and patient experience within modern Catholic hospitals have become increasingly similar to their non-Catholic counterparts (Homan and White, 2021; Kutney-Lee et al., 2014; O’Loughlin, 2024). This convergence aligns with broader trends in the health care industry, where acquisitions often lead to significant operational changes at acquired facilities (Burns et al., 2015; Furukawa et al., 2020; Lee and Alexander, 1999). For example, Andreyeva et al. (2024) found that operating

[1]This chapter was published July 2025 in *Medical Research and Review* using the title “Mission vs. Margin: The Effects of Catholic Health System Ownership on Hospital Operations,” co-authored with myself, Becky Staiger, Hector P. Rodriguez, and Amanda L. Brewster (https://doi.org/10.1177/10775587251355541)

expenses declined after previously independent hospitals in New York state joined a health system, and these declines were primarily driven by a decrease in the number of employees concentrated in support functions. However, patient volume remained unchanged, suggesting that large systems were able to increase efficiency—reduce inputs while maintaining the same level of output (Andreyeva et al., 2024).

Contributions & Theory

A recent literature review found that, despite the significant role of Catholic providers within the U.S. health care system, there are relatively few studies on this topic (Thorne et al., 2019). Previous research on Catholic health care is mainly descriptive in nature and limited by the potential for selection bias and unobserved confounding. In this study, I advance the literature by applying rigorous quasi-experimental methods to analyze the impact of competing institutional logics within Catholic health systems. By comparing changes in hospital operations following acquisitions by different ownership types, I empirically assess how Catholic health systems navigate competing mission-driven and financial tensions differently than their non-Catholic counterparts. Primary hospital operation outcomes are mission-oriented services (e.g., chaplaincy, charity care) and obstetrics care—services that are typically not profitable, but Catholic systems may be more likely to provide given their religious doctrine (Haddad, 2023; Panicola and Hamel, 2015). Secondary hospital operation outcomes are measures of utilization, operating expenses, and employment, which may change with the financial pressures associated with health system acquisitions (Andreyeva et al., 2024; Burns et al., 2015).

I also make novel contributions in organizational theory by using the case of Catholic system acquisitions to illustrate how mission-driven organizations balance competing institutional logics, which can be defined as the organizing principles associated with different aspects of society that guide organizational behavior (Friedland and Alford, 1991; Singer et al., 2020). In recent decades, there has been increasing interest in extending neo-institutional theory to study the multiple (and oftentimes, competing) institutional logics that drive organizational behavior, especially during times of organization change (Franz et al., 2021; Goodrick and Reay, 2016; Thornton and Ocasio, 1999). Catholic acquisitions present an interesting case to study the impact of applying multiple institutional logics—for example, religious and financial logics—within newly acquired hospitals.

Not only do Catholic hospitals adhere to the ERDs, an explicit set of rules and values governing their operations, they also have organizational resources and incentives to follow these directives. When a hospital is acquired by a Catholic health system, hospital leadership and staff typically go through a series of trainings to ensure alignment with the system's Catholic values, brand identity, and operating model. Catholic systems also have a "Director of Mission Integration" who uses established tools, such as the Catholic Ministry Identity Assessment, to oversee the consistent implementation of the ERDs in practice (Panicola and Hamel, 2015; Santos, 2023; Smith, 2018). Moreover, Catholic health care systems often rely on support and funding from local communities and the Catholic Church (Gagliarducci, 2021; O'Loughlin, 2024). By consistently applying the shared rules and values outlined in the ERDs, Catholic hospitals signal to stakeholders—patients, the Catholic Church, and the broader community—that the organization remains faithful to its identity and is "deserving" of resources. This context informs my first hypothesis:

Hypothesis 1. *Hospitals are more likely to preserve or expand mission-oriented services and obstetrics care after joining Catholic systems compared to non-Catholic systems.*

At the same time, the U.S. health care landscape has become increasingly competitive and profit-driven in recent years, contributing to accelerated merger/acquisition trends to expand providers' market power and financial stability (Andreyeva et al., 2024; Burns et al., 2015; Lee and Alexander, 1999) Policymakers and regulators are increasingly scrutinizing hospital acquisitions due to concerns over profit-driven incentives and potential reductions in access to care, especially in rural and underserved areas (Furukawa et al., 2020; Fuse Brown and Gudiksen, nd). Today, four of the 10 largest U.S. health care systems are Catholic, including the single largest nonprofit system, CommonSpirit Health (Solomon et al., 2020). To remain competitive and financially viable, health systems—both Catholic and non-Catholic—must operate efficiently and look for opportunities to cut costs, especially within newly acquired hospitals. This context informs my second hypothesis:

Hypothesis 2. *Post-acquisition, hospitals implement cut-cutting measures, including lower operating expenses and decreased employment, while maintaining or increasing utilization. These effects are similar after both Catholic and non-Catholic system acquisitions.*

1.2 Data and variables

Study sample

I integrated five data sources to construct a 14-year (2009–2022) unbalanced panel dataset of U.S. hospitals. My primary data source was the American Hospital Association (AHA) survey, a comprehensive survey of U.S. hospitals is completed annually by hospital administrators and contains information on hospital location, system membership, size, entity type (government, for-profit, or nonprofit), utilization, expenses, employment, and service lines. Consistent data collection and survey processes are used to facilitate accurate time-series analyses (AHA Data & Insights, 2025). The inclusion of a unique, stable hospital identifier allows hospitals to be linked over time and analyzed longitudinally. 78% of hospitals in my sample had data for all 14 years (2009–2022). Hospital closures and openings mean that some hospitals were added or removed from the dataset each year. The survey typically achieves high response rates (above 75%). If a hospital was open, but did not respond to the survey in a given year, AHA imputes missing values using a structured estimation process based on historical data and peer hospitals, creating a high degree of continuity across years (AHA Data & Insights, 2025; CDC National Center for Health Statistics, 2024).

I use several secondary data sources to check and confirm data on the acquisition of hospitals by health systems, including Levin Associates (2024) database that tracks health care mergers and acquisitions, CMS (2024) Hospital Change of Ownership dataset, and Cooper et al. (2022) Health Care Pricing Project data on hospital acquisitions. I also used AHRQ (2024) Compendium of U.S. Health Systems to identify religiously-affiliated health systems. I then manually validated if each religiously-affiliated system was Catholic using hospital and health system websites, press releases, and local diocesan lists of affiliated organizations. The initial dataset included 86,571 total hospital-years. To construct the analytic sample, I excluded hospitals that were part of a health system throughout the study period (n = 49,854). It is necessary to exclude these "already-treated" hospitals to maintain a clean control group and ensure the validity of difference-in-differences (DID) estimates (Callaway and Sant'Anna, 2021). In addition, I excluded hospitals that switched in or out of a system more than once (n = 1,033), were part of both Catholic and non-Catholic systems over the study period (n = 181), and had less than 2 years of data before or after joining a system (n = 1,951). The final analytic sample included 2,722 unique hospitals and 33,552 hospital-years from 2009 to 2022. During the study period, there was a total of 707 first-time system acquisitions of previously independent hospitals, 76 (11%) by Catholic systems, and 631 (89%) by non-Catholic systems.

Measures

Treatment variable

Treatment is defined as the first year a previously independent hospital was acquired by a health system (a central organization that owns and/or manages health provider facilities or health-related subsidiaries). I only analyzed first-time acquisitions to ensure that any observed changes could be clearly attributed to the impact of joining a particular health system, without potential for contaminating effects from previous acquisitions. To explore if treatment effects differed depending on whether the acquiring health system was Catholic vs. non-Catholic, I further identified two treated sub-groups: "Catholic-acquired" (hospitals whose first acquisition was by a Catholic system) and "non-Catholic-acquired" (hospitals whose first acquisition was by a non-Catholic system). For all analyses, the control group was consistently defined as hospitals that were independent throughout the study period ("never-acquired" hospitals).

Primary outcomes

My primary hospital operation outcomes were measures of mission-oriented services and obstetrics care. Mission-oriented services were dichotomous variables indicating the provision of the following four services at the end of the fiscal year: (a) chaplaincy, defined as religious ministering and pastoral counseling services for hospital patients, their families, and staff; (b) charity care, defined as health care services provided free of cost or on a sliding scale for uninsured and underinsured persons, including free clinics subsidized by the sponsoring health care organization; (c) community outreach, defined as a hospital program that systematically interacts with the community to identify those in need of services, alerting persons and their families to the availability of services, locating needed services, and enabling persons to enter the service delivery system; and (d) linguistic/translation, defined as services to make health care more accessible to non-English speaking patients.

Considering Catholic hospitals' stated commitment to ensuring access to obstetrics care, alongside the recent trend of obstetric unit closures, I also examined changes in obstetrics care following acquisitions by Catholic and non-Catholic systems. An obstetrics unit was defined as a hospital unit that provided care, examination, treatment, and other services to women during pregnancy, labor, and postpartum. Other obstetrics care outcomes included obstetric bed count and proportion of obstetric beds/total hospital beds.

Secondary outcomes

Secondary hospital operation outcomes included measures of utilization, operating expenses, and employment. Bed count was defined as number of beds set up and staffed at the end of the fiscal year. Bed count could be both a predictor of acquisition and/or change significantly post-acquisition. Therefore, I normalized the subsequent outcomes by bed count to eliminate heterogeneity purely due to differences in hospital size. Utilization measures per bed included: total inpatient admissions and number of inpatient days with Medicaid as the primary payer and, separately, Medicare. I measured the following three types of operating expenses per bed: (a) total expenses, defined as all operating expenses during the fiscal year, including payroll, employee benefits, and all other expenses; (b) payroll expenses, which included all salaries and wages; and (c) employee benefit expenses, which included social security, group insurance, retirement benefits, workman's compensation, and unemployment insurance. Employment was measured as the number of full-time equivalents (FTEs) per bed on payroll, which accounted for both full-time and part-time employees. Doctors of Medicine (MDs) included physicians and dentists. Nurses included registered, licensed practical, and vocational nurses. Support staff included assistive nursing personnel, technicians (radiology, laboratory, pharmacy), respiratory therapists, and all other employees.

Covariates

My analyses controlled for Medicaid payer mix (defined as proportion of total inpatient days with Medicaid as the primary payer) for both conceptual and analytical reasons. I have conceptualized Medicaid payer mix as a confounder, given it may impact both treatment assignment (hospital acquisition) and study outcomes (mission-oriented services, obstetrics care, etc.). My statistical tests confirmed this variable was associated with lower odds of hospital acquisition and also changed differentially over time between the treatment and controls groups in the pre-period (e.g., non-parallel pretrends, see Appendix A). Therefore, I have included Medicaid payer mix as a covariate to more precisely isolate acquisition treatment effects. I did not control for Medicaid payer mix when analyzing Medicaid days per bed or Medicare days per bed due to collinearity.

1.3 Empirical Approach

My main analytical strategy aimed to estimate differences in hospital response to acquisitions by Catholic and non-Catholic systems. For my main analyses, I used a staggered DID specification to compare changes in trends at independent hospitals that were acquired by Catholic and non-Catholic systems (treated groups) vs. trends for independent hospitals that were never acquired (control group). Using a DID approach in which the "event" is the year of hospital acquisition, I compared changes in hospital operation measures between the 8 years before ("pre-period") and 8 years after ("post-period") acquisition, relative to trends in these measures among never-acquired hospitals over the same time period. My model is as follows:

$$\gamma_{ht} = \beta_1(\text{post*Catholic-aquired})_{ht} + \beta_2(\text{post*non-Catholic-acquired})_{ht} + \gamma X_{ht} + \theta_h + \theta_t + \varepsilon_{ht} \quad (1)$$

where γ_{ht} denotes the outcome of interest for hospital h in year t. β_1 and β_2 capture the average treatment effect on the treated (ATT) after joining Catholic and non-Catholic systems, respectively. X_{ht} denotes the time-varying hospital covariate, Medicaid payer mix. The model includes hospital fixed effects (θ_h) to adjust for time-invariant hospital characteristics that may be correlated with the outcome (e.g., geographic location), and year fixed effects (θ_t) to adjust for national shocks (e.g., economic conditions, COVID-19 pandemic). Standard errors were clustered at the hospital level. I "winsorized" each continuous outcome, meaning outlier observations below the first percentile and above the 99th percentile were replaced with the 1st and 99th percentile values, respectively, given these values were likely not feasible.

My preferred specification utilized the Borusyak et al. (2024) imputation-based DID approach, which is particularly well-suited to this empirical setting given its efficient handling of both time- and group-based heterogeneous treatment effects. Regarding time-based heterogeneity, hospital acquisitions occurred at different points over the study period (are "staggered"), so dynamic treatment effects may vary based on calendar year and/or relative time post-acquisition. Recent literature has highlighted limitations of using traditional two-way fixed effects estimators with staggered treatment timing (Johansson et al., 2024; Wang et al., 2024; Wing et al., 2024). By constructing imputed untreated outcomes for each treated unit (using pre-treatment data and the never-treated control group), the Borusyak et al. (2024) method accommodates complex patterns in treatment timing. This approach mitigates concerns with staggered timing and enhances both the precision and efficiency of my estimates. In addition, the Borusyak et al. (2024) approach allows me to assess the statistical significance of group-based heterogeneous effects (here, Catholic vs. non-Catholic system acquisitions), a capability not directly available with other estimation procedures that do not use an imputation approach. Last, the Borusyak et al. (2024) method requires a stronger parallel trends assumption compared to alternative approaches, which enhances the credibility of causal effects if such assumptions are met.

In this model, the identifying variation was based on the staggered timing of hospital acquisitions and the comparison of acquired and never-acquired hospitals in each relevant time period. To interpret β_1 and β_2 as the causal effect of system acquisition on outcome γ_{ht}, I must assume that the outcome trends of the acquired hospitals would have been parallel to the outcome trends of the never-acquired hospitals in the absence of treatment. I assessed pre-treatment trends both formally (using joint pretrends tests) and graphically (using event study plots) and confirm the parallel trends assumption was met for all study outcomes. In Appendix A, I present p-values from formal statistical tests of jointly significant pretrends for each outcome. Joint p-values of $> .05$ support the parallel pretrends assumption, as I are unable to reject the null hypothesis of no differences in trends between treatment and control groups prior to acquisition. Given the staggered nature of the treatment timing, I also present event study plots to observe how treatment effects vary in the years before and after joining a health system.

I ran several robustness checks to test the sensitivity to study design decisions. Specifically, I ran the following three analyses: (i) standard two-way fixed effects DID estimator rather than the imputation-based approach, (ii) limiting the pre- and post-periods to 6 years instead of 8, and (iii) and excluding Medicaid payer mix as a covariate.

1.4 Causal estimates

Primary outcomes

Table 1 presents results from Equation 1, the average treatment effects of Catholic (col. 1) and non-Catholic (col. 2) acquisitions for the primary outcomes. I also present the difference in effect magnitudes between Catholic vs. non-Catholic acquisitions (col. 3). Following acquisition, Catholic-acquired hospitals experienced a relative increase in mission-oriented services compared to hospitals that were never acquired, including a 10.41 percentage-point (pp) (95% CI 1.36, 19.46) relative increase in the provision of chaplaincy care and a 3.97 pp (95% CI 0.40, 7.54) relative increase in charity care. Non-Catholic-acquired hospitals did not experience a change in chaplaincy or charity care compared to never-acquired hospitals. The difference in treatment effect magnitudes between Catholic-acquired and non-Catholic-acquired hospitals for chaplaincy and charity care was statistically significant (Table 1, col. 3). Figures 1 and 2 show the dynamic effects for trends in chaplaincy and charity care, respectively, over time, which align with these DID results. These event study plots, along with Appendix A, confirm the parallel trends assumption was met. I do not observe any statistically significant changes in community outreach or linguistic/translation services after Catholic or non-Catholic acquisitions.

Regarding obstetrics care, non-Catholic-acquired hospitals were relatively 3.45 pp (95% CI −5.70, −1.21) less likely to operate an obstetrics unit after acquisition than never-acquired hospitals, while there was no significant change in probability among Catholic-acquired hospitals. In addition, obstetric bed count decreased after non-Catholic acquisitions (ATT −0.87, 95% CI −1.30, −0.44) compared to never-acquired hospitals, but there was no significant change after Catholic acquisitions. The proportion of obstetric beds to total beds did not change at Catholic-acquired or non-Catholic-acquired hospitals relative to never-acquired hospitals. Figures 3 and 4 show the decrease in probability of operating an obstetrics unit and obstetric bed count, respectively, after non-Catholic acquisitions.

Secondary outcomes

Table 2 presents the average treatment effects of Catholic and non-Catholic acquisitions for the secondary outcomes. Total bed count decreased by 12.41 (95% CI −18.44, −6.37) and 4.31 (95% CI −7.95, −0.68) for Catholic-acquired and non-Catholic-acquired hospitals, respectively, relative to never-acquired hospitals; these treatment effect magnitudes differed by −8.09 (95% CI −14.86, −1.32). The significant treatment effects for bed count reinforce the importance of adjusting for bed count in the subsequent outcomes.

Efficiency of bed use, as measured by admissions per bed, increased by 1.39 after non-Catholic acquisitions (95% CI 0.52, 2.25) relative to never-acquired hospitals. Admissions per bed decreased after Catholic acquisitions, although this result is not statistically significant. The difference in treatment effect magnitudes for admissions per bed in Catholic-acquired vs. non-Catholic-acquired hospitals was −2.73 (95% CI −4.88, −0.59). Medicaid days per bed increased by over 4 days after both Catholic and non-Catholic acquisitions relative to never-acquired hospitals, although only the change at non-Catholic-acquired hospitals was significant (ATT 4.14; 95% CI 1.86, 6.42). Medicare days per bed increased by 4.10 (95% CI 1.50, 6.70) after non-Catholic acquisitions but decreased by 6.28 (95% CI −11.96, −0.60) after

Catholic acquisitions relative to never-acquired hospitals. The difference in Medicare days per bed effect magnitudes between Catholic-acquired vs. non-Catholic-acquired hospitals was −10.37 (95% CI −16.45, −4.30).

In contrast to these differing effects between Catholic-acquired vs. non-Catholic-acquired hospitals, changes in operating expenses were similar (although generally not statistically significant) after both Catholic and non-Catholic acquisitions. The one statistically significant result was employee benefit expenses per bed, which decreased by $4,890.66 (95% CI $-8,235.41$, $-1,545.91$) at non-Catholic-acquired hospitals compared to never-acquired hospitals. There were no significant differences in treatment effect magnitudes after Catholic vs. non-Catholic acquisitions for any operating expense measure.

Similar to operating expenses, changes in employment were similar after both Catholic and non-Catholic acquisitions. Total FTEs per bed decreased after both Catholic and non-Catholic acquisitions relative to never-acquired hospitals, although this change was only significant at non-Catholic-acquired hospitals (ATT −0.26, 95% CI −0.43, −0.08). MD FTEs per bed decreased after both Catholic (−0.03, 95% CI −0.06, −0.01) and non-Catholic acquisitions (−0.02, 95% CI −0.03, −0.006) relative to never-acquired hospitals. Support staff FTEs per bed also decreased after acquisition, although this change was again only significant at non-Catholic-acquired hospitals compared to never-acquired hospitals (ATT −0.24, 95% CI −0.37, −0.11). The largest changes in employment post-acquisition were cuts to support staff, suggesting a large portion of the change in total FTEs were driven by a decrease in support staff. Differences in the changes after Catholic vs. non-Catholic acquisitions were not significant for any employment measure.

Robustness checks

In Appendix B Tables 2-4, I present the average treatment effects of Catholic and non-Catholic acquisitions for the following three robustness checks: (i) two-way fixed effects estimator instead of imputation-based approach, (ii) 6 years pre- and post-period instead of 8 years, and (iii) excluding Medicaid payer mix as a covariate. Overall, results from these robustness checks were similar to the main DID models, although there were some divergences. When using the two-way fixed effects estimator (Table 2), changes in chaplaincy and charity care were no longer statistically significant at Catholic-acquired hospitals compared to never-acquired hospitals. However, the probability of community outreach decreased by 3.34 pp (95% CI −6.13, −0.56) at non-Catholic-acquired hospitals compared to never-acquired hospitals. The only change when using 6 years pre- and post-period instead of 8 years (Table 3) was the difference in treatment effect magnitudes between Catholic-acquired vs. non-Catholic-acquired hospitals was no longer statistically significant for charity care. When removing Medicaid payer mix as a covariate (Table 4), the only change was support staff FTEs per bed decreased by 0.41 (95% CI −0.80, −0.01) at Catholic-acquired hospitals compared to never-acquired hospitals.

1.5 Discussion

In recent decades, health system acquisitions have become a defining feature of the U.S. health care landscape, often driven by financial pressures and the need for operational efficiency. While prior research has highlighted the cost-cutting strategies commonly associated with acquisitions, less is known about the role of Catholic-owned systems—which constitute 4 of the 10 largest U.S. health systems—in shaping post-acquisition outcomes (Andreyeva et al., 2024; Solomon et al., 2020). This study leverages a national sample of U.S. hospitals over a 14-year period to provide new insights into the operational impacts of Catholic and non-Catholic health systems' acquisitions. In doing so, I provide rigorous empirical evidence to inform ongoing state policy debates related to the expansion of Catholic health care and also make novel theoretical and methodological contributions.

I found that hospitals were more likely to provide some mission-oriented services, including chaplaincy and charity care, after being acquired by a Catholic vs. non-Catholic system (relative to the control group of never-acquired hospitals). However, there was no change in the likelihood of offering community outreach or linguistic/translation services after being acquired by a Catholic or non-Catholic system. In addition, hospitals acquired by non-Catholic systems were less likely to operate an obstetric unit and had lower obstetric bed count relative to never-acquired hospitals, while hospitals acquired by Catholic systems had no significant change in obstetric services after acquisition. These results generally support *Hypothesis 1* and align with previous literature finding Catholic ownership was associated with providing more mission-oriented services (Weisman et al., 1999; White et al., 2006; Zare and Gabow, 2023).

My results provide mixed support for *Hypothesis 2*. Aligning with expectations, changes in operating expenses and employment were generally similar after both Catholic and non-Catholic acquisitions. These results align with previous research showing reductions in operating costs, primarily by reducing employees in support functions, after previously independent hospitals join a health system (Andreyeva et al., 2024). In addition, these findings align with previous literature highlighting growing similarities between Catholic and non-Catholic hospitals in managerial/operating strategies (Homan and White, 2021) and quality measures (Kutney-Lee et al., 2014). However, contrary to my expectations, I found differential changes in some utilization outcomes (e.g., admissions per bed) after Catholic vs. non-Catholic acquisitions. Future research should investigate the organizational behavior driving these heterogeneous utilization results.

I take multiple steps to assess the validity of these estimates. I formally test and are unable to reject the null hypothesis of no differences in pretrends at treated (acquired) hospitals compared to the control group (never-acquired hospitals), implying the former were not on track to experience the observed changes absent treatment (see Appendix A). In addition, I confirm the robustness of the results to multiple study design and specification checks, increasing my confidence in the causal interpretation of findings.

This study has several limitations. First, although Catholic systems are the largest provider of religiously-affiliated care in the United States, they still only operate a minority of total hospitals. The relatively small number of Catholic acquisitions compared to non-Catholic acquisitions (76 vs. 631 hospitals) may reduce statistical power, leading to wider confidence intervals and potentially limiting the detection of significant effects. Second, this study examined hospital-level operations and does not utilize clinical data to verify the actual use or quality of services provided, which limits my ability to assess patient-level impacts. Third, my analytic sample compares first-time acquisitions with never-acquired hospitals to ensure a clean comparison and valid identification of post-acquisition effects. While this design enhances internal validity, it narrows generalizability by excluding hospitals with prior health system affiliations. Fourth, this analysis focuses on quantitative outcomes, leaving qualitative aspects of hospital operations, such as leadership decisions or cultural integration, unexplored. Finally, unobserved factors such as market conditions or leadership changes may confound some results despite the inclusion of covariates and fixed effects.

Implications

My findings provide practical evidence on the effects of health system ownership for policymakers and health care leaders. Health care consolidation faces increasing public scrutiny given the growing role of for-profit entities and industry incentives for profit-maximization, often prompting calls for enhanced government oversight (Furukawa et al., 2020; Fuse Brown and Gudiksen, nd). Catholic health system acquisitions present an interesting dynamic, as my findings show they have the potential to preserve or even expand some mission-oriented services. However, this also raises key questions about equitable access to services prohibited by Catholic doctrine, such as abortion and contraception. In the wake of the *Dobbs v. Jackson Women's Health Organization* Supreme Court decision, access to comprehensive reproductive health care is particularly important. Future research should explore the impact of Catholic ownership on access to comprehensive reproductive health care, especially in areas where Catholic hospitals have significant market share.

In addition, my study makes novel theoretical and methodological contributions. Using the case of Catholic system acquisitions, I demonstrate how conflicting institutional logics interact and evolve over time within newly acquired hospitals. In some cases, mission-oriented logics outweigh financial logics—for example, expanding charity care at acquired hospitals. Aligned with institutional theories and organizational identity, the explicit nature of the ERDs and human/financial resources dedicated to their consistent implementation may play a role in the preservation of mission-oriented services (Besharov and Smith, 2014; David et al., 2019; White et al., 2006). Furthermore, this study utilizes an advanced imputation-based DID estimator, which is particularly well-suited for settings with staggered treatment timing and heterogeneous treatment effects. By employing this method, I provide robust causal inference and greater precision in estimating group-specific effects (e.g., Catholic vs. non-Catholic acquisitions). This approach advances the econometric literature by addressing limitations of traditional two-way fixed effects models and demonstrates the importance of evaluating ownership-based heterogeneity in system acquisitions.

Conclusion

This study uses rigorous quasi-experimental methods, informed by organizational theory, to examine how health care providers balance conflicting institutional pressures. I find that, while acquisition effects broadly align with the cost-containment strategies observed in prior research, there were heterogeneous effects based on system ownership. Some mission-oriented services, such as chaplaincy and charity care, were more likely to be provided after Catholic vs. non-Catholic acquisitions. In addition, non-Catholic systems were more likely to cut obstetric services at newly acquired hospitals. Taken together, these results suggest that system ownership plays a role in shaping hospital priorities post-acquisition and highlights the importance of disaggregating effects by ownership type. As health system consolidation continues, policymakers and health care leaders should consider the implications of these differing approaches, particularly in balancing financial sustainability with equitable access to care.

1.6 Chapter 1 Tables

Table 1: Treatment Effects After Catholic and Non-Catholic Acquisitions for Primary Outcomes

	(1) Catholic-acquired	(2) Non-Catholic-acquired	(3) Difference (1)-(2)
Mission-oriented services			
Chaplaincy	0.10**	0.0008	0.10**
	(0.05)	(0.01)	(0.05)
	[0.01, 0.19]	[-0.03, 0.03]	[0.01, 0.20]
Charity care	0.04**	-0.002	0.04**
	(0.02)	(0.01)	(0.02)
	[0.004, 0.08]	[-0.02, 0.02]	[0.002, 0.08]
Community outreach	-0.005	-0.02	0.02
	(0.04)	(0.02)	(0.04)
	[-0.09, 0.08]	[-0.05, 0.006]	[-0.07, 0.11]
Linguistic/translation services	0.02	0.01	0.01
	(0.04)	(0.02)	(0.04)
	[-0.06, 0.11]	[-0.02, 0.04]	[-0.08, 0.10]
Obstetrics			
Obstetrics unit	-0.04	-0.03**	-0.01
	(0.03)	(0.01)	(0.03)
	[-0.09, 0.01]	[-0.06, -0.01]	[-0.06, 0.05]
Obstetric bed count	-0.79	-0.87***	0.07
	(0.42)	(0.22)	(0.47)
	[-1.62, 0.04]	[-1.30, -0.44]	[-0.84, 0.99]
Proportion obstetric beds	-0.005	-0.001	-0.004
	(0.004)	(0.002)	(0.005)
	[-0.01, 0.004]	[-0.005, 0.002]	[-0.01, 0.006]

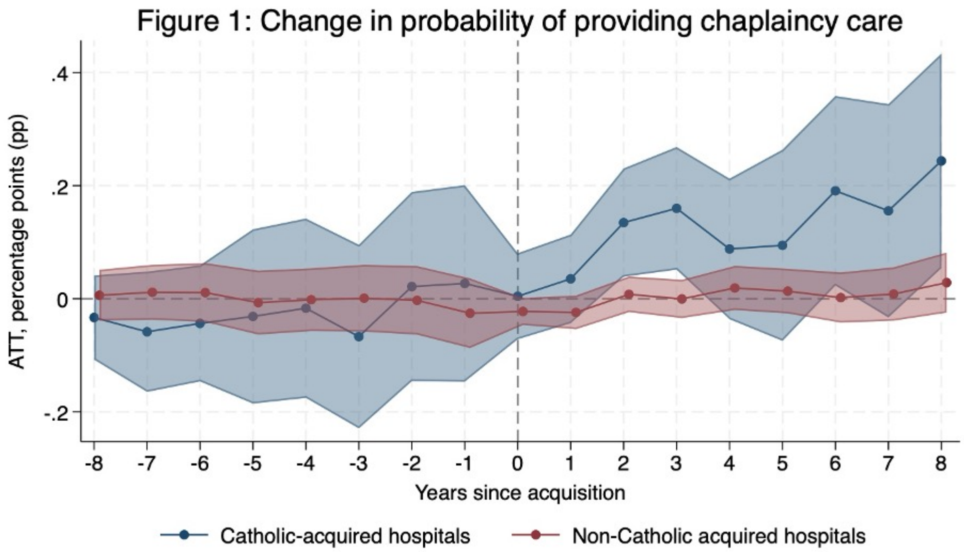
Notes: Table 1 presents results from Equation 1, which estimates the average treatment effects of Catholic (col. 1) and non-Catholic (col. 2) acquisitions, for our primary outcomes. Column (3) uses an imputation approach to estimate the difference between Catholic-acquired (col. 1) and non-Catholic-acquired (col. 2) treatment effects. The model includes hospital and year fixed effects and controls for Medicaid payer mix. All outcomes presented meet the parallel trends assumption. Standard errors are clustered at the hospital level and presented in parentheses. 95% confidence intervals are presented in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Treatment Effects After Catholic and Non-Catholic Acquisitions for Secondary Outcomes

	(1) Catholic-acquired	(2) Non-Catholic-acquired	(3) Difference (1)-(2)
Utilization			
Bed count	-12.41***	-4.31**	-8.09**
	(3.08)	(1.85)	(3.45)
	[-18.44, -6.37]	[-7.95, -0.68]	[-14.86, -1.32]
Admissions	-1.35	1.39***	-2.73**
	(1.02)	(0.44)	(1.09)
	[-3.35, 0.66]	[0.52, 2.25]	[-4.88, -0.59]
Medicaid days	4.64	4.14**	0.50
	(2.49)	(1.16)	(2.66)
	[-0.24, 9.52]	[1.86, 6.42]	[-4.71, 5.70]
Medicare days	-6.28**	4.10***	-10.37***
	(2.90)	(1.32)	(3.10)
	[-11.96, -0.60]	[1.50, 6.70]	[-16.45, -4.30]
Operating expenses			
Total expenses	30818.84	25767.52	5051.32
	(39038.36)	(16483.95)	(40805.85)
	[-45694.93, 107332.60]	[-6540.43, 58075.47]	[-74926.68, 85029.32]
Payroll expenses	-6057.64	-5285.82	-771.81
	(15270.27)	(6281.09)	(15885.04)
	[-35986.81, 23871.54]	[-17596.54, 7024.89]	[-31905.92, 30362.29]
Employee benefit expenses	-3496.23	-4890.66**	1394.43
	(4298.94)	(1706.54)	(4452.17)
	[-11922.00, 4929.55]	[-8235.41, -1545.91]	[-7331.67, 10120.53]
Employment			
Total FTEs	-0.39	-0.26***	-0.13
	(0.27)	(0.09)	(0.27)
	[-0.91, 0.13]	[-0.43, -0.08]	[-0.67, 0.40]
MD FTEs	-0.03**	-0.02***	-0.02
	(0.01)	(0.006)	(0.01)
	[-0.06, -0.01]	[-0.03, -0.006]	[-0.04, 0.009]
Nurse FTEs	0.01	0.02	-0.01
	(0.02)	(0.02)	(0.07)
	[-0.03, 0.07]	[-0.03, 0.07]	[-0.14, 0.013]
Support staff FTEs	-0.36	-0.24***	-0.12
	(0.20)	(0.07)	(0.21)
	[-0.77, 0.03]	[-0.37, -0.11]	[-0.53, 0.28]

Notes: Table 2 presents results from Equation 1, which estimates the average treatment effects of Catholic (col. 1) and non-Catholic (col. 2) acquisitions, for our secondary outcomes. Column (3) uses an imputation approach to estimate the difference between Catholic-acquired (col. 1) and non-Catholic-acquired (col. 2) treatment effects. All outcomes reported per bed. The model includes hospital and year fixed effects and controls for Medicaid payer mix. All outcomes presented meet the parallel trends assumption. MD, Doctor of Medicine (includes physicians and dentists); FTE, full-time equivalents. Standard errors are clustered at the hospital level and presented in parentheses. 95% confidence intervals are presented in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

1.7 Chapter 1 Figures



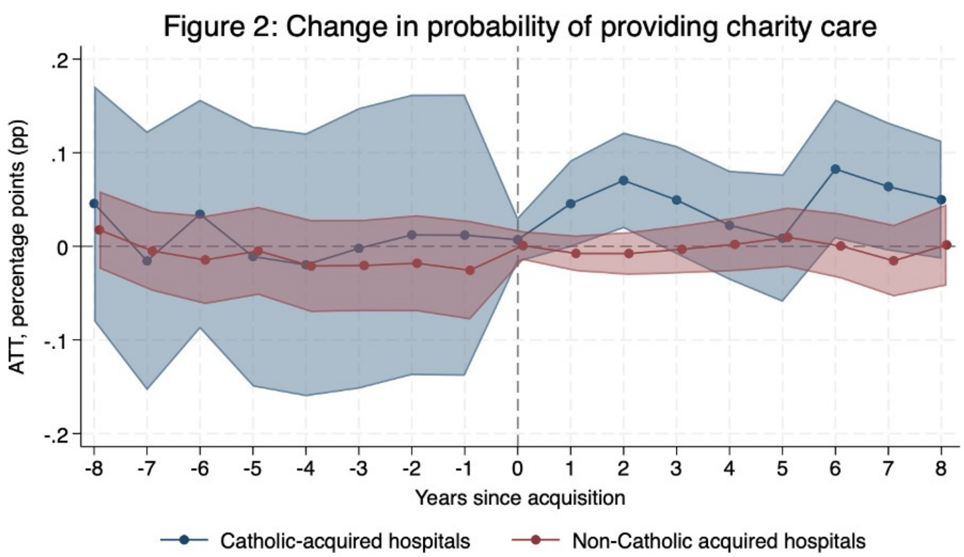


Figure 3: Change in probability of operating an obstetrics unit

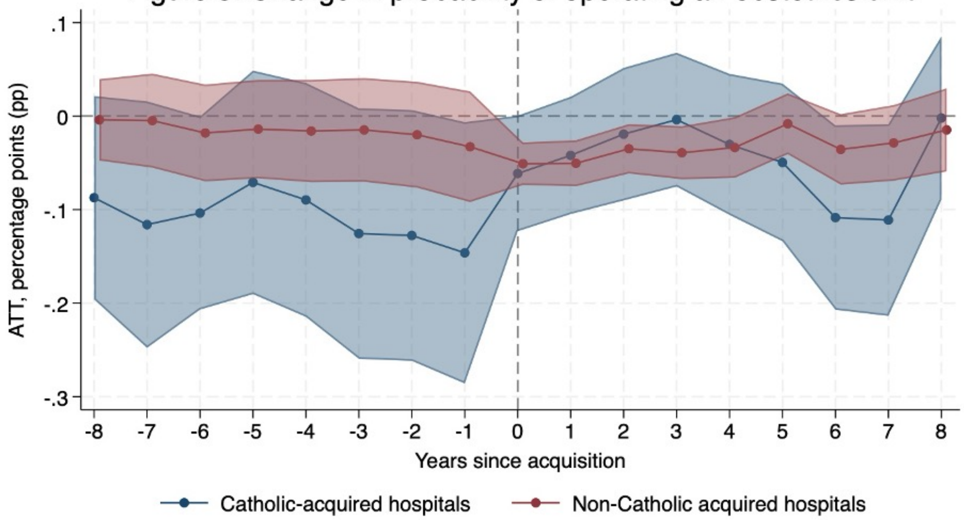
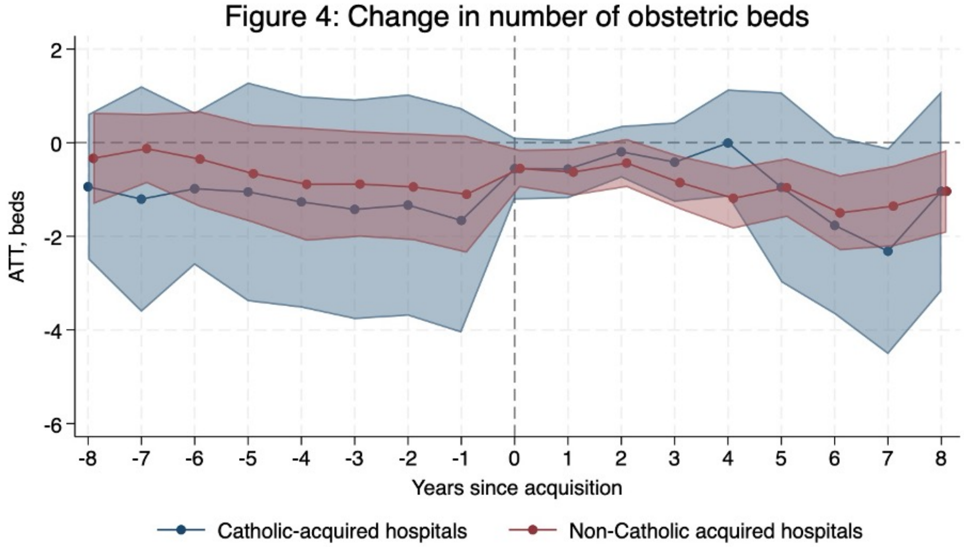


Figure 4: Change in number of obstetric beds



2 Chapter 2

Postpartum Contraception and Short-Interval Pregnancies: Differences at Catholic vs. non-Catholic Hospitals

2.1 Introduction

A growing, yet understudied, tension exists in the U.S. healthcare landscape: as demand for immediate postpartum contraception rises, especially post-*Dobbs*,[2] 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 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

[2] *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.

not provided. Table 3 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, I seek to fill this gap by analyzing the impact of hospital ERD adherence on contraception provision during the birth hospitalization and subsequent pregnancy spacing. The primary empirical challenge is patient selection: individuals who prefer not to use contraception may be more likely to seek care at ERD-adherent hospitals,[3] which would bias naive estimates. To circumvent this issue, I instrument for a patient's admission to an ERD-adherent hospital using the differential distance from a patient's residence to the nearest ERD-adherent vs. non-ERD-adherent hospital. This approach leverages quasi-random variation in geographic proximity to isolate the causal effect of being treated at a facility with religious service restrictions. I estimate the relationship between instrumented hospital admission and the outcomes of interest using two-stage least squares (2SLS) with a dataset of over 9 million birth hospitalizations from 2010-2023, constructed using 11 state inpatient databases from the Healthcare Cost and Utilization Project. The 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 these estimates particularly relevant to ongoing policy debates.

I first find that admission to an ERD-adherent hospital causes substantial reductions in the likelihood of receiving immediate postpartum contraception. Specifically, giving birth at an ERD-adherent hospital reduces the likelihood of receiving permanent contraception (e.g., sterilization) by 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). I 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.

Contributions

This study makes three key contributions to the literature. First, I provide the first causal evidence linking hospital religious restrictions to downstream health outcomes. Most prior research on this

[3] I use "ERD-adherent" rather than "Catholic" hospital for specificity, as not all hospitals owned by a Catholic health system are required to adhere to the ERDs. See Section 2.3 for details.

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." I build upon Hill et al. (2019) in three key areas: i) I examine rural-urban heterogeneity and trace effects through to downstream reproductive health outcomes, demonstrating that hospital service restrictions increase the risk of short-interval pregnancy, particularly for rural patients; ii) I use an instrumental variable design to estimate effects for patients who choose hospitals based on geographic convenience ("compliers"), a particularly policy-relevant population given they would be most harmed if consolidation eliminated non-religious alternatives; and iii) I introduce a precise, time-varying measurement of ERD adherence (see Appendix 4), distinguishing hospitals officially required to follow religious directives from those merely affiliated with Catholic systems.[4]

Second, I highlight institutional religious restrictions as an overlooked mechanism—distinct from policy, legal, or financial barriers—that impacts reproductive health access and outcomes. Prior research has established causal links between legal and financial barriers and reproductive healthcare (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. This study provides causal evidence of this mechanism in the context of reproductive healthcare specifically, but the ERDs also restrict providers' ability to offer evidence-based care in other domains, such as end-of-life care. Understanding this hospital-level mechanism is especially urgent amid broader shifts in the U.S. healthcare landscape: Catholic health systems are expanding; obstetric services are contracting, particularly in rural regions; options for abortion and contraception care have narrowed post-*Dobbs*; and impending Medicaid funding cuts threaten rural hospitals' financial stability. Together, these trends heighten the need for recognition and transparency of institutional restrictions as a distinct determinant of patient care.

Third, this 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). I 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. These 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.

[4] 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.

This finding adds important nuance to the debate on the social benefit of not-for-profit status in healthcare.

2.2 Background

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).[5] 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).[6]

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 (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,

[5] 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).

[6] 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.

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).[7] 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 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.

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.[8] Most, but not

[7] 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.

[8] 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).

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).[9] 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, five of the ten largest U.S. health systems (by net patient revenue) are Catholic, and approximately one in seven U.S. hospitals is Catholic (Solomon et al., 2020; Schulte et al., 2025; Popowitz, 2025). In terms of geographic presence, the market share of Catholic-affiliated hospitals varies based on locality (Solomon et al., 2020; Drake et al., 2020) but, on average, Catholic-affiliated hospitals are equally likely to be located in rural vs. urban areas.[10]

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.

2.3 Data and variables

My 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 this study given that ERD adherence is most consistently enforced within the hospital facility (Guiahi, 2018; Freedman, 2023). HCUP-SIDs contain hundreds of clinical and non-clinical variables for each hospital discharge, including patient demographics; admission type; patient, provider, and hospital identifiers; International Classification of Diseases (ICD) 9/10 diagnosis and procedure codes; Current Procedural Terminology (CPT) codes; and Diagnosis Related Groups (DRG) codes.

I merge HCUP-SID hospital discharge records with hospital characteristics from the American Hospital Association (AHA) Annual Survey, linked using unique hospital identifiers that are

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

[10] 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.

consistent across both databases (AHA, 2025). As described in Chapter 1, the AHA dataset provides comprehensive information on hospital location, system affiliation, organizational structure, ownership status, operational characteristics, and available service lines.

Study sample

The 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 4). The sample includes years 2010-2023, but not all years were available for each state. State-years in this 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 this sample may contain multiple hospitalizations for the same patient.

I arrived at this sample by making several exclusions. First, from the sample of all births (n=10,925,859), I exclude records missing patient county or zip code (14.4%), then records missing hospital location (1.7%), as these variables are required for my empirical strategy. Second, I exclude 0.75% of observations where the patient's state of residence differs from the hospital state, assuming these cases represent atypical care-seeking patterns (e.g., tourism or travel).[11] I 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. I further discuss this analytical decision in Section 2.4.

For the secondary analyses of short-interval pregnancy, I create a sub-sample of five states (Arkansas, Florida, Maryland, New York, and Washington) with unique patient identifiers that enables linkage of hospitalizations by the same patient over time (Table 5). In this sub-sample, I drop index birth hospitalizations in the last two years for each state to allow sufficient time to observe short-interval pregnancies.

Treatment: Identifying ERD-adherent hospitals

My empirical strategy requires accurately characterizing ERD-adherent hospitals, meaning those that are officially recognized by the local bishop as Catholic, and are therefore expected to fully comply with the ERDs. 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

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

expected to adhere to the ERDs.[12]

To construct precise, time-varying indicators of hospital ERD adherence, I implemented a rigorous multi-step validation process (detailed in Appendix C). To briefly summarize: I first cross-referenced AHA Catholic-affiliation variables against six secondary data sources.[13] In cases of conflicting information, I then conducted a targeted search of hospital websites and historical news reports. In 44 cases where adherence to the ERDs remained inconclusive, I conducted "secret shopper" calls (with a research assistant) to the hospitals to clarify the implementation of religious directives. During these secret shopper calls, we identified ourselves 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.

Primary and secondary outcomes

The primary outcome of interest is provision of postpartum contraception (permanent and non-permanent) during the birth hospitalization. I 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.

The secondary outcome of interest is short-interval pregnancy, defined as a pregnancy within 18 months 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, I 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 D Tables 5 and 6 provide the complete list of codes used to define the study sample and outcomes.

[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.

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

Covariates

I include a rich set of patient, hospital, and market-level covariates. Patient-level covariates include both sociodemographic and clinical traits. For sociodemographic characteristics, I consider age (in bins that capture ages ranging from 12-17, 18-25, 26-30, 31-36, and 37-49), race/ethnicity (White, Black, Asian, Hispanic, other/missing), primary payer (private insurance, Medicaid, other/missing), marital status (binary), and patient residence (urban vs. rural). In the main analyses, I use the National Center for Health Statistics (NCHS) urban-rural county classification. I 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., 2013). I also construct a summary measure of patient comorbidity burden using the van Walraven Elixhauser comorbidity index (van Walraven et al., 2009; Li et al., 2008; Quan et al., 2005).[16]

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

[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.

[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. I measured hospital concentration using the Herfindahl-Hirschman Index (HHI) with annual birth volumes per hospital service area (HSA). HSAs are defined by the Dartmouth Atlas Project.

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

2.4 Empirical Approach

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

Naive OLS estimation

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

$$Y_{ihmt} = \beta_0 + \beta_1 \mathrm{ERD}_{ihmt} + \mathbf{X}'_{ihmt}\boldsymbol{\beta}_2 + \delta_{st} + \varepsilon_{ihmt} \tag{2}$$

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 2.3; and ε_{ihmt} is the error term. δ_{st} are state-by-year fixed effects, which control for time-varying, state-specific factors (e.g., changes in data collection protocols, Medicaid policies, and market dynamics) and are particularly important given the unbalanced panel dataset. Standard errors are clustered at the patient's county of residence to account for intra-county correlation in unobserved factors specific to where a patient lives.

[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)).

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

Differential distance instrument

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

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

One potential concern with the DD instrument is that patients facing extreme travel trade-offs may

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

[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."

not represent a population with meaningful choice, potentially violating the common support assumption required for instrument validity. To address this, I exclude observations where the absolute DD exceeds 100 miles (approximately 2.5 to 3 hours of driving time).[21] This excludes about 1% of the sample, focusing the analysis on patients for whom both types of hospitals are reasonably accessible. As shown in Figure 1, the closest non-ERD-adherent hospital is 14.6 miles closer to the patient zip code centroid, on average, than the closest ERD-adherent hospital. As a robustness check, I re-estimate the primary outcomes using the untrimmed DD measure. Results are consistent, although with a lower first-stage F statistic, especially for the rural sub-population.

2SLS-IV model

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

$$\text{ERD}_{ihmt} = \alpha_0 + \alpha_1 DD_{izt} + \mathbf{X}'_{ihmt}\boldsymbol{\alpha}_2 + \delta_{st} + \mu_{ihmt} \tag{3}$$

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

I present the first-stage relationship in Figure 2 to confirm the relevance assumption needed for a valid 2SLS-IV design. The binned scatterplot visually confirms the strong, negative first-stage relationship between the instrument and ERD-adherent hospitalization.[22] The coefficient of -0.00537 (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, I estimate the causal effect using only the exogenous variation from the first stage:

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

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

$$Y_{ihmt} = \gamma_0 + \gamma_1 \widehat{\text{ERD}}_{ihmt} + \mathbf{X}'_{ihmt}\boldsymbol{\gamma}_2 + \delta_{st} + \nu_{ihmt} \tag{4}$$

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

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

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 this 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, I provide both conceptual and empirical evidence—specifically, balance and negative control outcome (NCO) tests—supporting the plausibility of both assumptions.

Conceptual support

From a conceptual standpoint, several features of the 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

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

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 my instrument captures quasi-random differences in which hospital type happens to be more geographically convenient.

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

Balance tests

To empirically provide support for my identification strategy, I conduct balance tests that examine whether the instrument predicts patient characteristics that might be correlated with the outcomes. The logic of these tests is analogous to randomization checks in experimental settings: if the instrument captures quasi-random variation in geographic access to hospitals, it should not systematically predict patient characteristics associated with contraception use. Finding that the instrument is unrelated to *observed* patient characteristics provides evidence that it is also unlikely to be correlated with *unobserved* characteristics that could threaten the independence assumption. I implement these balance tests in two steps using linear regression models. First, I construct predicted outcomes for each patient by regressing the outcome Y_{ihmt} on patient-level demographic and clinical covariates (described in Section 2.3) and obtaining the fitted value $\widehat{Y}_{ihmt}$. This predicted outcome, or "risk score," captures the component of the outcome explained by observable patient characteristics—specifically, the likelihood that a patient would seek or use contraception based on their observed traits. By aggregating information across all patient covariates into a single summary measure, this approach provides a more powerful test of balance than examining each covariate separately. Second, I residualize both $\widehat{Y}_{ihmt}$ and the instrument DD_{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.

Negative Control Outcome (NCO) tests

To further assess the validity of the identification strategy, I conduct 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). I estimate reduced-form regressions of six NCOs on differential distance, using the same controls as in the main specification. NCOs were selected to test for specific sources of potential selection bias. I first test *marital status*, which serves as a proxy for individual religiosity.[26] Second, I 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] Table 6 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 the instrument is not capturing unobserved patient characteristics that would threaten the validity of the design. Overall, these null results strengthen confidence that the large, significant effects I observe for the main study outcomes reflect the causal impact of hospital ERD adherence rather than patient selection or other confounding factors.

[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.

[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 the longitudinal subsample with unique patient identifiers. These variables capture births observed in the 2010–2023 data; for women whose first birth preceded 2010, I observe age at first *observed* birth, introducing potential left-censoring. This measurement error would only threaten my conclusions if correlated with differential distance, which is unlikely given the instrument's quasi-random variation.

Limitations

Although the 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 I control for a wide range of observable hospital characteristics to mitigate this concern, I cannot rule out the possibility that the estimates capture both the direct effect of ERD adherence and these correlated organizational attributes.

This limitation has important implications for interpretation and policy application. From a patient perspective, these results accurately characterize the consequences of giving birth at an ERD-adherent hospital as they currently exist: institutions that, as a package, include both religious service restrictions and the organizational characteristics that accompany them. As long as these organizational attributes continue to coincide together in practice, the estimates reflect the treatment effect of giving birth at an ERD-adherent hospital. However, the results may not predict the effects of external policy changes that alter ERD status independently, such as state mandates requiring service provision. Such interventions would change religious restrictions without necessarily changing underlying organizational characteristics, and thus could have different effects than I estimate. This caveat does not diminish the relevance of my findings for understanding current patient experiences, but suggests caution in extrapolating to counterfactual policy scenarios.

2.5 Descriptive results

Sample characteristics

Table 7 presents summary statistics of the analytic sample, stratified by hospital type (ERD-adherent vs. not). Panel A reports patient characteristics. In general, patients appear to be similar on observable demographic and clinical characteristics across hospital type. Mean age is consistent (29, SD: 6), 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 the dataset had an obstetrics unit and more than a third of total admissions were for obstetrics care—proportions that were similar for both types of hospitals. 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, I find that patients discharged from ERD-adherent hospitals came from counties with slightly lower per capita Catholic adherents, on average, compared to patients discharged from non-ERD-adherent hospitals. While counterintuitive, this finding strengthens my identification strategy by suggesting that patient religiosity is unlikely to be an unobserved confounder affecting both hospital proximity and reproductive health outcomes. In addition, patients discharged from ERD-adherent hospitals live in counties with slightly fewer OBGYNs per capita (12 vs. 13) and fewer abortion clinics (3 vs. 5) compared to patients who went to non-ERD-adherent hospitals.

The sample includes 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 the study sample by ERD-adherence. Visual inspection suggests that ERD-adherent hospitals are dispersed throughout the states in the sample rather than geographically clustered, supporting the plausibility of my identification strategy.

In Table 8 I explore differences in the occurrence of the 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.

Complier characteristics

The 2SLS-IV approach identifies treatment effects for compliers—patients whose hospital choice is determined by geographic proximity (as compared to those willing or able to travel for care that better meets their preferences). This complier population is most harmed when hospital M/A eliminates local non-religious alternatives, a key concern in current policy debates regarding Catholic healthcare expansion. In Table 9, I characterize the complier population compared to "always-takers" (patients who deliver at ERD-adherent hospitals regardless of proximity) and "never-takers" (patients who deliver at non-ERD-adherent hospitals regardless of proximity). To do so, I use a binary version of the instrument with the method developed by Marbach and Hangartner (2020), which builds on the principal stratification framework of Angrist et al. (1996).[30]

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

[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), I can identify

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 my 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 the LATE is identified—are precisely those most harmed when mergers or acquisitions eliminate local non-religious hospital alternatives.

2.6 Causal estimates

Immediate postpartum contraception

Table 10 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 the 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 my confidence that the relationship is robust. For permanent contraception (Panel A), I find that admission to 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), I 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 mean of any pretreatment covariate for each compliance type. This method requires a binary instrument; I therefore create an indicator for whether a patient resides closer to an ERD-adherent vs. non-ERD-adherent hospital from the continuous differential distance measure. While the main analysis exploits continuous variation in differential distance, the binary instrument identifies all patients whose hospital choice is distance-sensitive. The marginal compliers in the continuous specification—those whose choices respond to incremental differential distance changes—are likely a subset of the binary compliers I characterize here, concentrated among patients facing the strongest distance constraints. The patterns I document therefore likely represent lower bounds on the characteristics of marginal compliers in the main estimates.

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.

Heterogeneity by patient geography

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, I explore heterogeneity in the effects of ERD-adherent hospital admission on contraception provision by patient location, estimating separate models for rural and urban sub-groups. The main model defines rural vs. urban using NCHS definitions, and I 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 11. 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. I 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 E Table 9, I present the LATE of ERD-adherent hospitalization on provision of postpartum contraception using two alternative rural-urban definitions. First, using the OMB definition, I find nearly identical patterns to the 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, I split the sample by market competition using the Herfindahl-Hirschman Index (HHI)—areas with low competition have fewer providers, analogous to rural areas. I 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

[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.

vulnerability of rural populations to ERD-adherent hospital service restrictions.

Heterogeneity by patient insurance

Insurance coverage policies may mediate the relationship between hospital religious restrictions and contraception access. Throughout the study period, Medicaid coverage of postpartum contraception evolved, especially coverage of immediate postpartum LARC.[32] While the state-by-year fixed effects absorb most variation from these policy changes, differential exposure could remain if ERD-adherent hospitals serve different patient populations. Descriptively, ERD-adherent hospitals do serve a slightly lower share of Medicaid patients (42% vs. 45%, Table 7). I therefore estimate separate models for privately-insured and Medicaid patients to assess whether insurance coverage policies interact with religious restrictions in ways that the main specification does not fully capture.

Table 12 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.

Short-interval pregnancy

A critical downstream consequence of barriers to contraception is an increased risk of short-interval pregnancy (SIP). I investigate this relationship by examining the effect of delivering at an ERD-adherent hospital vs. non-ERD-adherent on the risk of SIP. Table 13 presents the LATE for SIP occurring within 6 months (Panel A), 12 months (Panel B), and 18 months (Panel C) of the index birth. For these analyses, I use a subsample of patients with unique identifiers that I can follow across hospitals over time (n=5,217,931). In the full longitudinal sample (Column 1), I find small and statistically insignificant effects of delivering at an ERD-adherent hospital on the likelihood of a subsequent SIP. The point estimates are positive, suggesting a potential increase, but not precisely estimated. However, when stratifying by geographic location, a clear pattern emerges that aligns with the previous postpartum contraception findings. Rural patients show

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

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 the contraception findings, where rural patients experienced the largest reductions in postpartum contraceptive provision at ERD-adherent hospitals. These results provide compelling evidence of a causal chain: hospital religious restrictions reduce contraception provision, which subsequently increases the likelihood of short-interval pregnancies among rural patients who have limited alternative sources of reproductive healthcare.

Appendix E Table 10 presents estimates of the effect of ERD-adherent hospitalization on SIP rates using two alternative rural-urban classifications, confirming the robustness of the 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 for urban patients are small and statistically insignificant across all panels. The consistency of these findings across alternative definitions of rurality provides strong evidence that the main findings are robust.

Additional robustness checks

I present three additional robustness checks in Appendix E. First, I define treatment as giving birth at a hospital that adheres to the ERDs *or* is affiliated with a Catholic health system. Related literature uses this definition for "Catholic" hospitals (Hill et al., 2019; Solomon et al., 2020; Rodriguez et al., 2023; Menegay et al., 2022; Caldwell et al., 2022; Liu et al., 2025), but as discussed in Section 2.3, ERD-adherent hospitals and Catholic-affiliated hospitals are not perfectly analogous. Appendix E Table 11 shows that results are consistent, but slightly lower in magnitude, which is expected given the less strict treatment definition. For example, in the main analyses, ERD-adherent hospitalization caused a 63% decrease in likelihood of permanent contraception provision, compared to 61% decrease for Catholic-affiliated hospitalization.

Second, I re-estimate the 2SLS specification using the untrimmed DD instrument. In the main analyses, I excluded 1% of observations where DD > |100| miles (approximately 2.5 to 3 hours of driving time), which helps ensure the common support assumption is met. Appendix E Table 12 reports estimated effects using the untrimmed sample that includes DD up to a maximum of 206.8 miles. Results are qualitatively similar, indicating that my findings are robust to DD trimming decisions. Notably, the first stage F-statistics are lower compared to the 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, I present 2SLS estimates with covariates selected by the machine learning LASSO-orthogonalized algorithm (Chernozhukov et al., 2015), which is further described in Appendix F. 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. Results from this robustness check are shown in Appendix E Table 13. The LASSO estimates are consistent with and slightly larger in magnitude than the 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 I do not present a robustness check using the two-stage residual inclusion (2SRI) control function method. While 2SRI models are sometimes employed for binary outcomes (typically by incorporating residuals from a linear first stage into a nonlinear second stage), linear 2SLS models are generally preferred when outcomes are rare, as is the case in this study. Nonlinear specifications such as probit frequently encounter separation problems (perfect prediction) when applied to rare events. In addition, previous econometric research demonstrates that linear 2SLS estimates are qualitatively identical to probit 2SRI marginal effects with rare outcomes. Moreover, the predicted probabilities generated by 2SLS rarely exceed the logical $[0, 1]$ bound when baseline rates are low, mitigating the primary practical concern motivating nonlinear specifications (Angrist and Pischke, 2009; Chiburis et al., 2012; Terza et al., 2008).

2.7 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.

I 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, I 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, my 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 previously, the complier population is composed of more low-income and racially diverse patients compared to the full sample, which is consistent with other evidence demonstrating less advantaged families are more affected by distance constraints (Card et al., 2023; Fontenot et al., 2024). While it is unclear whether the estimates generalize to planned deliveries or higher-SES patients, they suggest that Catholic hospital expansions may disproportionately impact vulnerable patients with fewer resources (e.g., time or money). In addition, the 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.

I 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.

These 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)), this 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 the 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, these 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, I 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).

Implications

These 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. These 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, these 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). This 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); these findings support extending this scrutiny to healthcare service availability.

In addition, these 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 Table 3). 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, the 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.

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, these 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.

2.8 Chapter 2 Tables

Table 3: State Legislation Related to Reproductive Healthcare in Catholic Hospitals

State	Name	Description	Status
New York	Hospital Transparency Act, Senate Bill S1003A / Assembly Bill A733A (S3486/A3862)	Requires hospitals to fill out a form regarding which reproductive health services they provide and post on both their website and the Department of Health webpage	Passed both chambers but vetoed twice (2024, 2025); reintroduced in 2025-26 session
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 Senate but not the House in 2024, ongoing discussions
Connecticut	House Bill 5424 / 5516	Protects clinicians who work in Catholic institutions and provide referrals or counseling on reproductive healthcare	Proposed in 2026
New Mexico	Health Care Consolidation Oversight Act, House Bill 586	Increased government oversight of healthcare mergers/acquisitions to ensure access to comprehensive services, including reproductive healthcare	Made law in 2025
New Jersey	Senate Bill 3275 / Assembly 4829	Launched state information portal with increased transparency on patient rights and reproductive healthcare options	Made law in 2023
Colorado	Patients' Right to Know Act, House Bill 23-1218	Requires every hospital/health facility to publicly disclose services (including reproductive health care) they do not provide for non-medical reasons, such as religious directives	Made law in 2023
California	Health Care Quality and Affordability Act, Senate Bill 184	Established the Office of Health Care Affordability (OHCA) with greater oversight to evaluate M/A transactions on competition, cost, and access	Made law in 2022
Oregon	House Bill 2362	Increased government oversight of healthcare mergers/acquisitions to ensure access to comprehensive services, including reproductive healthcare	Made law in 2021
Washington	Protecting Pregnancy and Miscarriage-Related Care, Senate Bill 5140	Prohibits hospitals from interfering with providers' ability to manage pregnancy complications where the patient's health or life is at risk	Made law in 2021
Washington	Senate Bill 5602	Requires hospitals to fill out a form regarding which reproductive health services they provide and post on both their website and the Department of Health webpage	Made law in 2019

Table 4: Study Sample of Birth Hospitalizations (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 5: Sub-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

Table 6: 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.

Table 7: 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 8: 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 9: 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.

Table 10: 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 11: 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 12: 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.

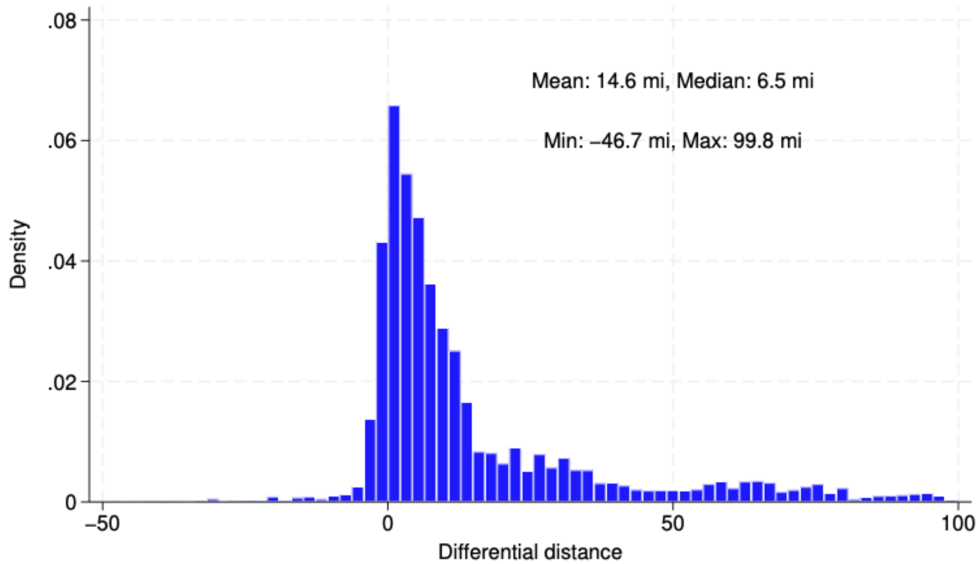
Table 13: The Effect of ERD-adherent Hospitalization on Short-Interval Pregnancy, 2SLS Estimates

	(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.

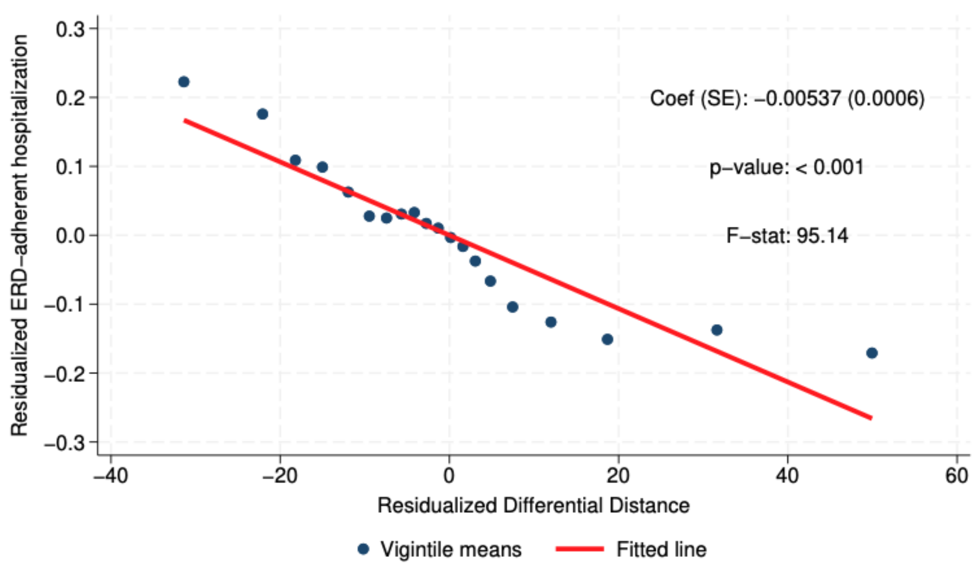
2.9 Chapter 2 Figures

Figure 1: DD instrument distribution



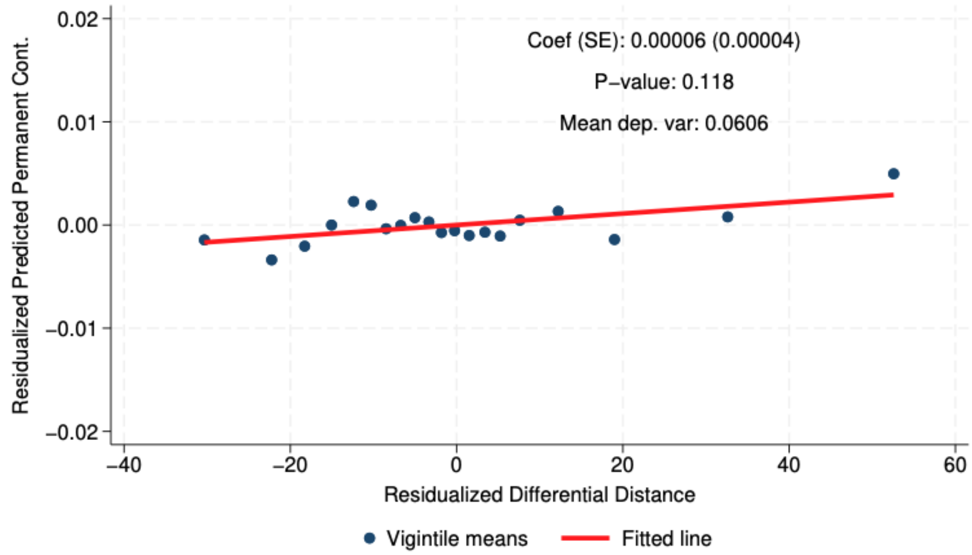
Notes: This figure shows the distribution of the differential distance (DD) instrument for the main analytic sample, which is restricted to observations where the absolute DD is less than 100 miles to ensure common support. The instrument is calculated as the distance from a patient's ZIP code centroid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

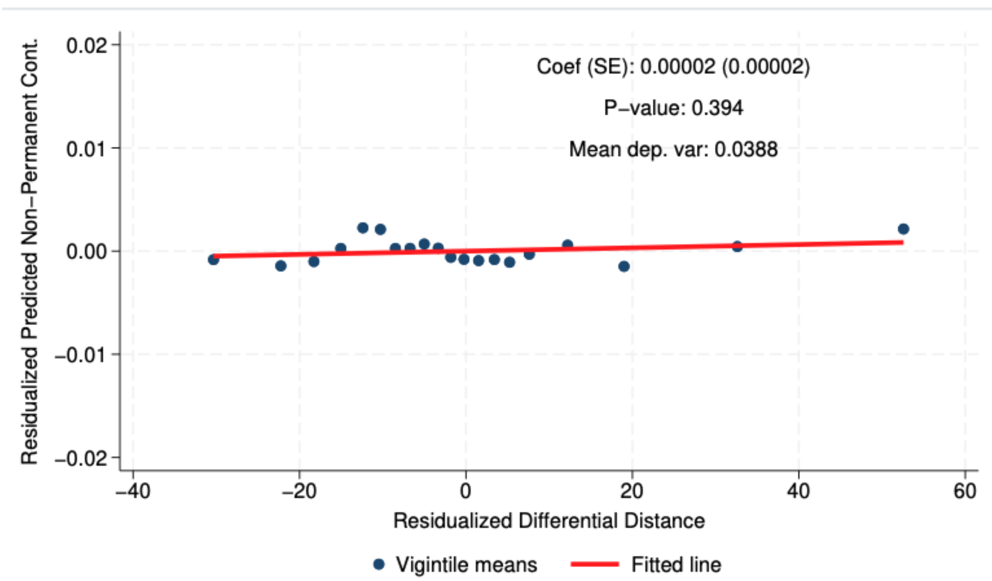


Notes: This figure presents a binned scatterplot illustrating the first-stage relationship between the differential distance (DD) instrument and admission to an ERD-adherent hospital. Both the treatment variable (admission to an ERD-adherent hospital) and the differential distance instrument are residualized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



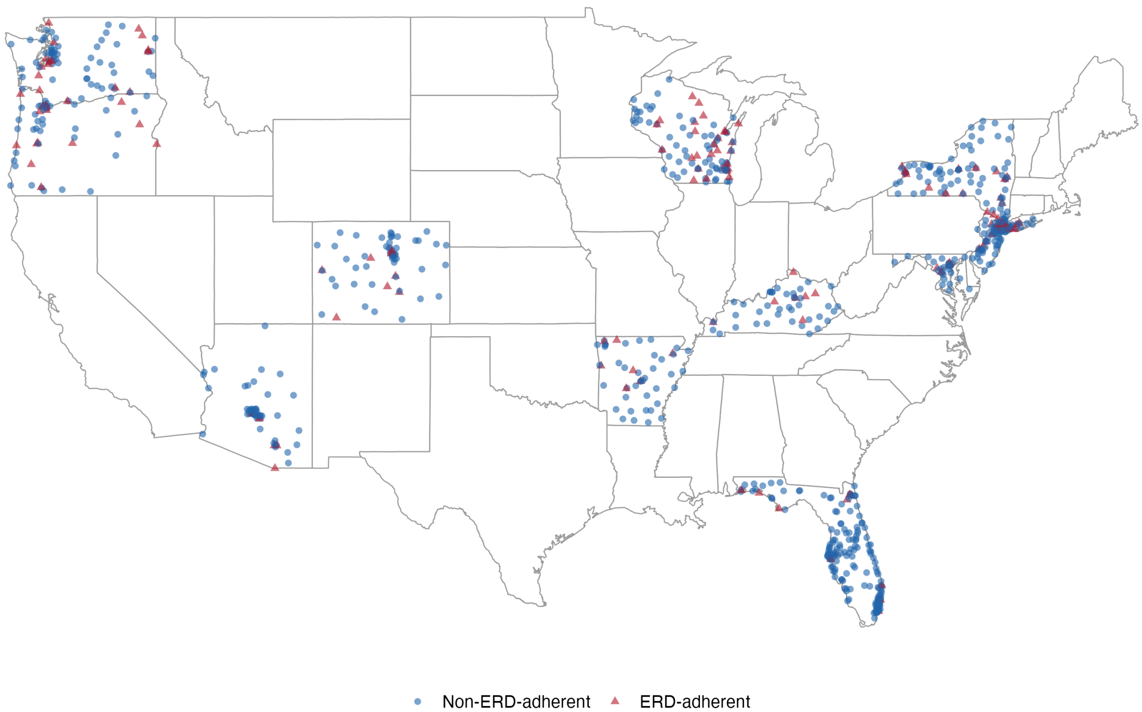
(a) Permanent Contraception



(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.

3 Chapter 3

Abortion Provision and Related Complications: Differences at Catholic vs. non-Catholic Hospitals

3.1 Introduction

Management scholars increasingly recognize that modern organizations rarely pursue singular objectives, but instead must navigate multiple, often competing, demands. These tensions typically arise from conflicting institutional logics—the socially constructed, historically grounded principles that guide organizational behavior (Friedland and Alford, 1991; Thornton et al., 2012). Such conflicts are particularly salient in "hybrid organizations": those that have committed to two or more logics, embedding multiple (and often contradictory) values into their core identity and mission (Battilana and Dorado, 2010; Jay, 2013). Hybrid organizations are especially prevalent in healthcare, where providers must simultaneously pursue clinical excellence, financial sustainability, and, in many cases, religious or social missions—challenges that are becoming increasingly difficult with the corporatization of care (Cutler and Huckman, 2025). Nonprofit hospitals, physician groups, medical schools, and accountable care organizations are all examples of hybrid organizations navigating this kind of institutional complexity (Greenwood et al., 2011; Reay and Hinings, 2009; Dunn and Jones, 2010; Goodrick and Reay, 2016).

Prior management research on conflicting institutional logics has focused on internal dynamics: how organizations maintain identity, legitimacy, and operational efficiency while navigating competing tensions (Battilana and Dorado, 2010; Besharov and Smith, 2014; Schad et al., 2016; Putnam et al., 2016). A related body of work examines employee "workarounds"—the informal practices that frontline workers use when formal processes fail to reconcile conflicting demands—and their consequences for internal efficiency and organizational culture (Halbesleben et al., 2008; Tucker et al., 2020). Yet, both literatures focus predominantly on consequences *within* the organization, leaving largely unexamined how these internal balancing acts affect external stakeholders such as customers, patients, and caregivers.

In this study, I fill this gap by providing the first large-scale causal evidence of how hybrid organizations manage competing institutional logics and the consequences for external stakeholders. I ask three related research questions: i) What intraorganizational and interorganizational strategies are used to manage conflicting institutional demands? ii) How, if at all, are external stakeholders impacted? and iii) Do management strategies and stakeholder effects differ across environmental contexts?

I conduct this investigation in a setting where conflicting institutional logics are particularly salient: U.S. Catholic hospitals. Catholic hospitals must strictly adhere to the Ethical and Religious Directives (ERDs), which prohibit several types of standard healthcare, including abortion, while simultaneously fulfilling professional medical obligations to patient safety and healthcare quality (U.S. Conference of Catholic Bishops, 2018). This creates a structural tension that cannot be resolved: success as a Catholic hospital requires simultaneously honoring both the "Catholic" and "hospital" identities. When these logics collide—as they frequently do in urgent pregnancy-related situations—clinicians are forced to choose between doctrinal compliance and medical necessity. Catholic healthcare is also large and growing: the number of U.S. Catholic hospitals increased by over 20% in the last two decades and now five of the ten largest health systems (by net patient revenue) are Catholic (Solomon et al., 2020; Popowitz, 2025).

I employ a mixed-methods design to analyze the effects of religious restrictions on abortion care and related complications during urgent, pregnancy-related hospitalizations. For my quantitative analyses, I use a dataset of over 1 million hospitalizations across ten U.S. states (2010-2022). I construct a precise, time-varying treatment indicator for ERD-adherent[33] hospitalizations by cross-referencing multiple proprietary datasets and conducting "secret shopper" calls to resolve conflicting classifications. Because patients may sort into or away from religious institutions based on unobservable personal preferences, naive estimates are susceptible to selection bias. To overcome this endogeneity, I use a differential distance instrument to quasi-randomly assign patients to hospitals based on geographic proximity, enabling rigorous causal inference. To complement this quasi-experimental analysis, I conducted 27 semi-structured qualitative interviews with clinicians, administrators, and researchers with experience in Catholic hospitals. These interviews were essential not only for contextualizing and interpreting the quantitative results, but also for developing a grounded understanding of the decision-making pressures facing clinicians and managers. This early engagement and integration helps ensure my research offers practical insights for those navigating conflicting institutional tensions—in Catholic hospitals and hybrid organizations more broadly.

Relevant to my first research question, quantitative and qualitative evidence illuminates several organizational strategies used to manage conflicting logics, including separating the tension across time (temporal separation) and space (spatial sorting). Clinicians frequently employ *temporal separation* to align medical necessity with religious directives. Rather than intervening when a complication arises, providers are often forced to wait until the patient's condition deteriorates sufficiently—for example, until fetal cardiac activity ceases or severe infection sets in—so that the procedure can be reclassified as directly treating a "serious pathological condition" (with pregnancy termination as an "indirect" effect). In addition, institutions utilize *spatial sorting* to displace the tension geographically. When prohibited care is required, Catholic hospitals often transfer patients to secular facilities, effectively outsourcing the violation of the ERDs while preserving their organizational identity.

Second, I find that these internal management strategies have external consequences, namely reduced access to care and worse quality, for patients and their caregivers. Patients admitted to an ERD-adherent hospital during urgent, pregnancy-related situations are 0.93 percentage points (pp) less likely to be induced for an abortion procedure (179% reduction relative to the mean).[34] Care quality, measured by abortion-related complication rates, initially appears similar across ERD-adherent and non-ERD-adherent hospitals. However, this aggregate finding masks substantial heterogeneity: Hispanic patients experience 0.57 pp increase in general abortion

[33] I use "ERD-adherent" rather than "Catholic" hospital for specificity, as not all hospitals owned by a Catholic health system are required to adhere to the ERDs. See Section 2.3 for details.

[34] In this study, percent changes relative to the mean often exceed 100% because hospital-based abortion care is rare (as discussed in Section 2.3)—even modest absolute reductions translate into large relative changes. Additionally, the 2SLS-IV model identifies effects for compliers, a subpopulation with above-average baseline outcome rates, which further amplifies the relative magnitude of the estimate.

complication rates at ERD-adherent hospitals (67% increase relative to the mean), while patients with complex pregnancies, such as ectopic/molar pregnancies or premature rupture of membranes (PROM), also experience higher complication rates.

Third, I observe substantial heterogeneity in organizational responses based on environmental context, specifically degree of market concentration. Both quantitative and qualitative evidence reveals that religious restrictions reduce induced abortion rates primarily in low concentration (urban) settings, where spatial sorting—via hospital transfers or patient self-selection—is feasible. In contrast, I do not observe a decrease in induced abortions at ERD-adherent hospitals in highly concentrated (rural) markets, where there is a lack of alternative providers. These results demonstrate that organizational management strategies are not uniformly available but depend on organizational capabilities and environmental resources.

3.2 Literature and contributions

Institutional logics and workarounds

Modern organizations increasingly operate across fragmented institutional spheres, forcing them to grapple with multiple, often conflicting, institutional logics. Hybrid organizations can be defined as those that formally embed two or more logics into a single organizational identity, necessitating the continuous balancing of persistent tensions (Battilana and Dorado, 2010; Greenwood et al., 2011; Jay, 2013). Previous research documents strategies, both formal and informal, to manage these conflicting tensions. For example, hybrid organizations often set up formal mechanisms to manage conflict across time and space: *temporal separation*, allocates competing demands to sequential time periods, and *spatial separation*, allocates competing demands to distinct physical locations or organizational units (Poole and Van de Ven, 1989; Smith and Lewis, 2011; Schad et al., 2016). In addition, establishing strong intra-organizaitonal relationships for cross-department collaboration and handoffs is an effective way to manage conflicting logics (Reay and Hinings, 2009; Tucker et al., 2014).

Researchers have studied informal employee "workarounds," strategies to achieve an operational goal when confronted with organizational barriers. In healthcare, operational researchers have found that workarounds can waste up to 10% of staff time and propose a more formal, process-focused approach to reduce workarounds and related negative outcomes (Halbesleben et al., 2008; Tucker et al., 2020, 2014; Alter, 2014). From a sociological and qualitative perspective, Freedman (2023) has studied workarounds in Catholic hospitals. She describes workarounds as "idiosyncratic and informal," a stopgap solution to a much larger problem: the irreconcilable tension between religious and medical logics in Catholic hospitals.

When the ERDs conflict with medical necessity, clinicians are forced to choose between institutional compliance and patient welfare—and many resort to informal workarounds.[35] Qualitative studies document OB/GYNs in Catholic hospitals providing medications without

[35] In her book "Bishops and Bodies," sociologist Dr. Lori Freedman defines a workaround as "a goal-driven adaptation that allows a worker to overcome, bypass, or minimize the impact of a variety of constraints on achieving the work goal." She explicitly states the constraint to be "worked-around" in Catholic hospitals are the ERDs, which cause resource limitations and barriers to accessing necessary healthcare (Freedman, 2023).

documentation, "stretching the truth" to secure ethics committee approval, offering services off-site, and discussing options with patients "behind closed doors" (Freedman and Stulberg, 2013; Stulberg and Freedman, 2012; Foster et al., 2011; Freedman, 2023). According to the ERDs, not only is abortion provision prohibited in Catholic hospitals, but so is counseling discussions that include abortion as a potential option. In addition, while working in a Catholic hospital, clinicians are contractually obligated to follow the ERDs, so these workarounds not only jeopardize patient safety, but also risk clinicians' employment termination.

Crucially, environmental conditions shape how organizations experience and respond to conflicting logics. Environmental scarcity—whether characterized by a lack of alternative market options, geographic isolation, or limited operational resources—intensifies the salience of tensions by constraining the range of available responses and forcing direct confrontation between competing demands (Smith and Lewis, 2011). Under conditions of scarcity, organizations are typically forced to confront the tension, as they can no longer evade the friction between their conflicting mandates. Conversely, organizations operating in resource-rich environments typically have more degrees of freedom to manage tensions through displacement and outsourcing rather than direct resolution (Schad et al., 2016; Raisch and Birkinshaw, 2008; Smith and Tushman, 2005).

Across these literatures, the focus has been on *internal* processes and outcomes. To my knowledge, there has been no large-scale, causal research analyzing how organizational strategies to manage conflicting logics impact *external* stakeholders. I fill this gap by showing that the strategies used to manage conflicting internal tensions, such as temporal delays and spatial sorting, shift friction costs onto external stakeholders. In my empirical setting, a Catholic hospital's "successful" maintenance of its dual religious-medical identity comes at the expense of patient access and quality, with disproportionate effects on patients lacking the time, social capital, and/or health literacy to navigate institutional complexity.

Healthcare consolidation

In recent decades, there has been increasing consolidation and corporatization in the American healthcare sector (Gaynor, 2018; Fulton, 2017; Cutler and Huckman, 2025). Previous literature has focused on price and quality effects, finding that consolidation reduces competition, which leads to higher prices and usually worse quality of care (Dafny et al., 2016; Gaynor, 2018; Cooper et al., 2019; Eliason et al., 2020). These effects often stem from the perverse incentive structure embedded in the American healthcare system: despite the moral and economic case for keeping people healthy, the system still allocates resources primarily toward treatment rather than prevention, rewarding volume over value (Cutler and Huckman, 2025; Eliason et al., 2020). However, there remains a critical gap in understanding how consolidation and the resulting loss of competition impact the availability of essential services.

One particularly consequential—and underexamined—aspect of this consolidation trend is the expansion of Catholic health systems. The number of Catholic hospitals grew by over 20% in the last two decades; today, five of the ten largest U.S. health systems by net patient revenue are Catholic and overall about one in seven U.S. hospitals is Catholic[36] (Solomon et al., 2020;

[36]Catholic hospitals tend to be larger and generate higher revenues than their non-Catholic counterparts.

Popowitz, 2025; Schulte et al., 2025). When a Catholic system acquires a previously secular hospital, patients who once had access to comprehensive reproductive healthcare services may suddenly face restrictions, often without their knowledge or choice (Guiahi et al., 2014; Wascher et al., 2018; Takahashi et al., 2019; Freedman, 2023). In this paper, I find that religious restrictions imposed in Catholic hospitals substantially alter the availability of services (as evidenced by reduced access to abortion care) and results in worse quality, particularly for patients with less time and resources to navigate institutional complexity.

These findings have implications beyond Catholic healthcare. Consolidation, both within the healthcare industry and more broadly, can meaningfully impact the availability of essential services. For instance, insurance mergers or private equity acquisitions of physician practices can reduce access to certain drugs or providers through changes in formularies, provider referral networks, or covered services. Similar dynamics arise outside healthcare, for example in banking, media, and telecommunications, where consolidation often results in the elimination of local branches, coverage gaps, or the removal of products and services that served specific populations.[37] Taken together, my findings provide evidence that regulators and antitrust authorities should incorporate assessments of how consolidation will affect the availability of essential services (alongside price and quality impacts), especially in rural markets where a single merger or acquisition can effectively eliminate access to key services.

3.3 Data and variables

Study sample

I use the same primary data sources, HCUP-SIDs and AHA, as described in Chapter 2. My analytic sample for Chapter 3 comprises all urgent/emergent inpatient pregnancy-related hospital admissions for females aged 12-49 at general medical and surgical hospitals across ten geographically diverse states: Arkansas, Arizona, Colorado, Florida, Kentucky, Maryland, New Jersey, New York, Oregon, and Washington (n=1,051,745; see Table 14). The sample spans 2010-2022, though not all years are available for each state. I selected state-years based on data availability and quality criteria, including availability of unique hospital identifiers, encrypted AHA linkage files, completeness of patient demographic variables, and sufficient variation in ERD-adherent hospital market share. The unit of analysis is the hospitalization; my sample may contain multiple admissions for the same patient.

I applied several systematic exclusions to construct this sample. First, my study focuses on the time period before the *Dobbs v. Jackson Women's Health Organization* Supreme Court decision, so I exclude all admissions after June 2022.[38] Second, I restrict to urgent and emergent pregnancy-related admissions, as inpatient abortions typically occur in acute medical situations

[37] For example, banking acquisitions may result in branch closures or the elimination of certain financial products. Telecommunications mergers can reduce rural coverage through tower closures. In media, consolidation can force subscription models on previously standalone products or remove content from certain platforms entirely.

[38] It is important to note that my study period (2010–2022) precedes the Supreme Court's *Dobbs v. Jackson Women's Health Organization* decision, so these results capture the baseline effect of religious directives in an environment of federal abortion protection. As state policies increasingly restrict access to reproductive healthcare, the mechanisms identified here—temporal separation and spatial sorting—will likely become even more salient, exacerbating the negative outcomes related to providing healthcare under religious directives.

rather than elective contexts (elective abortions are performed in outpatient settings, most commonly at Planned Parenthood clinics). Third, I exclude admissions resulting in normal birth or delivery, as patients who have a live delivery have progressed beyond the point of viability for abortion procedures. HCUP-SIDs do not contain gestational age, so I am not able to make exclusions using this information.[39] Fourth, I exclude records missing patient county or zip code (14.4% of the initial sample) and hospital location (1.7%), as these geographic identifiers are essential for my IV strategy. Fifth, I exclude 0.75% of observations where the patient's state of residence differs from the hospital state, as these cases likely represent atypical care-seeking patterns (e.g., during tourism or travel).[40]

I also construct a subsample of complex pregnancies, which includes ectopic pregnancies (occurring outside the uterus, typically in the fallopian tube), molar pregnancies (involving abnormally fertilized eggs), and premature rupture of membranes (PROM). I focus on these conditions because they represent high-risk clinical scenarios where prompt pregnancy termination is often the indicated medical intervention to prevent severe maternal mortality or morbidity (e.g., sepsis or hemorrhage), thereby placing secular standards of care in direct tension with religious prohibitions (JAMA, 2022; Foster et al., 2011; Freedman and Stulberg, 2013). All pregnancy admission types were identified using ICD-9/10 diagnosis and procedure codes, CPT codes, and DRG codes based on established clinical definitions from prior literature (Zhu et al., 2023; Pollack et al., 2022; Meille and Monnet, 2024; Stulberg et al., 2017). Appendix D provides the complete list of codes used to define my study sample.

Variables

I use the same treatment indicator (ERD-adherent hospitalization) and patient-, hospital-, and market-level covariates as described in Chapter 2. In Chapter 3, my primary outcome of interest is *induced* abortion, which captures any procedure to terminate pregnancy, including dilation and curettage (D&C), dilation and evacuation (D&E), and medication abortion. I also measure *spontaneous* abortion (miscarriage) as a validation check: because miscarriage is an unintended biological process unaffected by hospital religious policy, a null effect across hospital types provides evidence that my identification strategy successfully addresses patient selection.

Secondarily, to evaluate the quality and safety implications of managing conflicting logics, I measure two types of complications. First, I identify general abortion-related complications, which include the most common abortion-related complications such as pelvic infection, obstetric sepsis, shock, hemorrhage, and need for blood transfusion (Wall and Yemane, 2022; Freedman et al., 2008; Freedman, 2023). This general measure captures whether temporal separation (delays while assessing medical necessity) and/or spatial sorting (coordination and transfer delays) related to abortion care increase overall maternal morbidity. I also measure complications arising from

[39]The point of viability is typically around 24 weeks of gestation. Before the *Dobbs* Supreme Court decision, abortion was federally protected before the point of viability. My study only includes years pre-*Dobbs*, 2010-2022. In my sample, I retain preterm and stillbirths as these represent pregnancy outcomes where abortion may have been medically indicated.

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

abortion procedures induced during that hospitalization specifically, which helps distinguish between harms arising from delays in care provision (captured in the general complication measure) versus harms from the abortion procedure itself. For example, if ERD-adherent hospitals have higher rates of general abortion complications but similar or lower rates of induced abortion-specific complications, this pattern would be theoretically informative. It suggests that complications arise from the management process itself (e.g., delays and transfers associated with temporal separation and spatial sorting), rather than poor procedural quality. In other words, patients are harmed during the waiting period, not during the procedure itself.

Although hospital-based abortion care is rare, it is the appropriate focus here for two reasons. First, ERD adherence is most consistently enforced in inpatient settings (Guiahi, 2018; Freedman, 2023). Second, abortions indicated during urgent pregnancy-related admissions typically involve emergency situations with significant physical, emotional, and clinical consequences. Therefore, these outcomes represent rare but important events where institutional policy has the greatest potential to impact patient care.

Outcomes were measured as a binary indicators for the presence of relevant ICD-9/10 diagnosis or procedure codes, CPT codes, or DRG codes based on established clinical definitions from prior literature (Zhu et al., 2023; Pollack et al., 2022; Meille and Monnet, 2024; Hill et al., 2019). Appendix D provides the complete list of codes used to construct these outcomes.

3.4 Empirical approach

My empirical goal is to estimate the causal relationship between going to an ERD-adherent hospital (vs. not) for urgent/emergent pregnancy-related reasons and the likelihood of abortion provision and related complications. Similar to Chapter 2, there is potential for patient selection and unobserved confounding to bias the relationship between treatment outcomes. For example, patients may select hospitals based on unobservable factors—such as individual religiosity, pregnancy intentions, or unmeasured clinical severity—that are also correlated with abortion provision and complications. Therefore, I use the same approach as Chapter 2 (2SLS model with differential distance, DD, instrumenting for hospital choice) to more plausibly estimate the causal effect of ERD-adherent hospitalization on abortion outcomes.

The empirics are the same as previously described in Chapter 2, but I present 3 additional figures here to confirm the validity of the DD instrument when applied to the Chapter 3 sample. First, Figure 5 presents the distribution of my DD instrument. On average, the closest non-ERD-adherent hospital is 15.5 miles closer to the patient zip code centroid than the closest ERD-adherent hospital, which aligns with Chapter 2 estimates. Second, Figure 6 presents the first stage relationship, confirming the strong, negative relationship between my instrument and ERD-adherent hospitalization. In addition, the F-statistic is 67.60, well above conventional thresholds for weak instrument concerns (e.g., $F > 10$). Third, Figure 7 shows a flat relationship between DD and predicted induced abortion, providing support for the outcome independence assumption.

3.5 Descriptive results

Sample characteristics

Table 15 presents summary statistics of my analytic sample, stratified by hospital type (ERD-adherent vs. not). Panel A reports patient characteristics. Patients treated at ERD-adherent hospitals were more likely to be White (48% vs. 37%) and have private insurance (41% vs. 38%), and were less likely to live in urban counties (67% vs. 77%). These demographic differences suggest some patient selection into ERD-adherent facilities, motivating my instrumental variable approach. Clinical characteristics are balanced across hospital types: patients exhibit similar rates of chronic conditions and average Elixhauser comorbidity scores.

Panel B reports hospital characteristics. The vast majority of ERD-adherent hospitals were non-profit (94%), compared to 68% of non-ERD-adherent hospitals, while government ownership was virtually absent among ERD-adherent facilities (0.3%). ERD-adherent hospitals were also less likely to be teaching hospitals (3% vs. 37%). In terms of market concentration, ERD-adherent hospitals operated in slightly more concentrated markets (mean HHI of 0.5 vs. 0.4), meaning they were more likely to be in rural areas with lower provider density and fewer alternatives for care. These organizational differences underscore the importance of my instrumental variable approach and rich set of covariates to isolate the causal effect of ERD adherence from potentially confounding hospital characteristics.

Panel C reports patient county characteristics. Importantly, I find that patients hospitalized at ERD-adherent facilities came from counties with slightly lower per capita Catholic adherents (22 vs. 23) and fewer Catholic congregations (36 vs. 53), on average, compared to patients who went to non-ERD-adherent hospitals. While counterintuitive, this finding strengthens my identification strategy by suggesting that patient religiosity is unlikely to be an unobserved confounder affecting both hospital proximity and inpatient abortion provision. Patients treated at ERD-adherent hospitals resided in counties with slightly lower access to reproductive healthcare resources, including fewer OB/GYNs per 100,000 residents (12 vs. 13) and fewer abortion clinics (3 vs. 5), on average, compared to those treated at non-ERD-adherent facilities. In total, the sample includes 1,051,745 hospitalizations, 12% from ERD-adherent facilities, a proportion that aligns with national estimates (Solomon et al., 2020; Meille and Monnet, 2024).

In Table 16 presents unadjusted outcome rates stratified by hospital type. In terms of service provision (Panel A), the raw prevalence of induced abortion at ERD-adherent hospitals was 0.31%, almost one-half of the 0.55% rate observed at non-ERD-adherent facilities. In contrast, rates of spontaneous abortion (miscarriage) were comparable across hospital types (2.22% vs. 2.20%), suggesting similar baseline volumes of pregnancy loss. Regarding complication rates (Panel B), ERD-adherent hospitals exhibited slightly lower unadjusted rates of both general abortion-related complications (0.79% vs. 0.86%) and complications specific to induced abortion (0.18% vs. 0.27%). These descriptive patterns suggest ERD restrictions reduce induced abortion access, while lower complication rates at ERD-adherent hospitals may reflect patient selection or differential transfer patterns.

Transfer rates

Table 17 provides descriptive evidence of spatial sorting by describing patient transfer rates stratified by hospital religiosity and market concentration. In the full sample, ERD-adherent hospitals transfer patients at modestly higher rates than their non-ERD counterparts (Panel A: 2.23% vs. 2.08%). Hospitals operating in highly concentrated, rural markets exhibit high transfer rates (Panel B: 2.93% vs. 1.54% in urban areas), likely reflecting their role as stabilization and referral centers for complex emergency admissions. To isolate the effect of religious restrictions from these standard rural referral patterns, Panel C restricts the analysis to urban markets. In these low-concentration areas with ample alternatives for timely care, ERD-adherent facilities continue to exhibit elevated transfer rates compared to non-ERD-adherent hospitals (1.77% vs. 1.51%). This divergence likely reflects an institutional reliance on spatial sorting, where ERD-adherent hospitals transfer cases that conflict with religious directives to nearby secular facilities (when doing so is logistically feasible in urban markets).

Crucially, because my study sample is composed of inpatient admissions, these transfer rates only capture "reactive transfers"—cases where a patient was admitted (likely for expectant management) and subsequently transferred, potentially due to clinical deterioration or denied ethics approval. These rates do not include "proactive transfers" that occur prior to admission. Consequently, these results likely underestimate the total volume of spatial separation, measuring only the most burdensome cases where a patient is admitted then later transferred to a different facility.

3.6 Causal estimates

Abortion provision and related complications

Table 18 presents the impact of ERD-adherent hospitalization on abortion provision and complications. Results from the OLS model (Column 1) show that ERD-adherent hospitalization is associated with a 0.28 percentage point (pp) reduction in induced abortion, a 0.13 pp reduction in general abortion-related complications, and a 0.11 pp reduction in complications related to induced abortion specifically (all $p < 0.01$). However, these OLS estimates may reflect patient selection patterns rather than the treatment effect of religious restrictions. For example, if religious patients with a lower baseline preference for abortion disproportionately sort into Catholic facilities, the OLS model will underestimate the restrictive impact of the ERDs. To address this endogeneity, Column 2 presents results from the 2SLS-IV model, where the estimated reduction in induced abortion increases substantially in magnitude. Being admitted to an ERD-adherent hospital caused a 0.93 pp decrease in the probability of induced abortion provision ($p < 0.05$), a 179% decrease relative to the sample mean of 0.52%. There was no difference in the likelihood of spontaneous abortion (miscarriage) at ERD-adherent hospitals (vs. not) in the OLS or 2SLS-IV models.

In terms of complications, the 2SLS-IV estimates reveal no significant effect on general abortion-related complications (Panel C), but a statistically significant 0.29 pp reduction in complications from induced abortion specifically (Panel D, $p < 0.05$), which represents a 110% decrease relative to the mean of 0.26%. The pattern of reduced induced abortion alongside reduced complications likely reflects both selective transfer of high-risk cases to non-Catholic facilities and careful execution of the abortion procedures that Catholic hospitals do perform. I further explore these mechanisms in my interviews with clinicians and administrators at Catholic hospitals, as described in Section 3.7.

Heterogeneity by hospital concentration

Next, I examine heterogeneity in effects across market concentration, as ERD restrictions may bind differently depending on provider density and transfer capacity. In rural areas with more concentration and fewer alternative hospitals, ERD-adherent facilities may be compelled to provide necessary care despite restrictions, whereas in urban areas with less concentration, patients can more easily select non-ERD-adherent hospitals or be transferred when care conflicts with directives. Table 19 reveals a striking divergence in induced abortion provision and complications: while ERD-adherent hospitalization significantly reduced the probability of induced abortion by 2.00 pp in urban settings (357% relative to the mean, $p < 0.01$), there was no statistically significant difference at rural hospitals. These findings suggest that when ERD-adherent hospitals have high concentration (typically in rural areas), environmental scarcity constrains spatial sorting options, compelling these hospitals to provide medically necessary abortions despite religious restrictions. Urban ERD-adherent hospitals also show a significant 0.57 pp reduction in induced abortion complications ($p < 0.01$), likely reflecting both selective transfer of complex cases to non-Catholic facilities and increased oversight for procedures performed locally. In contrast, rural hospitals show no significant difference in induced abortion complications, consistent with limited transfer capacity causing local provision of both routine and complex cases. Consistent with the main analysis, I see no significant difference in the probability of miscarriage diagnoses at ERD-adherent vs. non-ERD-adherent hospitals across high and low market concentration.

Appendix Table 14 presents a robustness check, defining hospital concentration by birth count (rather than admissions, as used in the main analyses). The results are highly consistent with the main specification in Table 19. ERD-adherent hospitalization significantly reduces induced abortion in low concentration (urban) settings by 2.31 pp, representing a 461% reduction relative to the mean ($p < 0.01$), with no significant effect in rural settings. In addition, ERD-adherent hospitals with low market concentration have lower rates of induced abortion complications (-0.68 pp, $p < 0.01$), while ERD-adherent hospitals with high concentration (typically in rural areas) show no significant difference. This consistency across alternative measures of market concentration strengthens my confidence that the observed heterogeneity reflects genuine differences in management strategies and environmental scarcity rather than sensitivity to how concentration is defined.

Heterogeneity by patient race/ethnicity

In Table 20, I examine whether ERD restrictions affect patients differently by race and ethnicity, as organizational management strategies—such as delays in care or transfer—may interact with structural inequities to differentially affect patient populations. Panel A indicates that religious restrictions reduce access to abortion across all patient populations, with the largest reductions for Black patients (1.76 pp decrease, 266% relative to the mean). In addition, complication rates in Panels C and D highlight racial disparities. White patients experience a significant reduction in induced abortion complications at ERD-adherent hospitals (Panel D: -0.25 pp, $p < 0.05$), likely reflecting successful transfers of high-risk cases and effective advocacy during determination delays. In contrast, Hispanic patients experience a significant increase in general abortion-related complications (Panel C: 0.57 pp, $p < 0.10$) yet show no increase in complications from induced abortion procedures themselves (Panel D). This pattern is consistent with temporal separation imposing differential costs: Hispanic patients, facing language barriers, lower health literacy, and implicit bias, likely experience longer delays while hospitals determine medical necessity, during which pregnancy complications develop. More broadly, these results reveal that minority populations are disproportionately affected, as the organizational strategies (temporal separation and spatial sorting) that enable ERD-adherent hospitals to sustain their dual mission require patient resources that are unequally distributed across racial groups.

Subset of complex pregnancies

Last, I examine medically complex pregnancies where ERD restrictions are most salient: ectopic and molar pregnancies and premature rupture of membranes (PROM). These conditions typically require pregnancy termination to prevent serious maternal morbidity or mortality, placing evidence-based clinical guidelines in direct conflict with religious directives (JAMA, 2022; Foster et al., 2011; Freedman and Stulberg, 2013). Table 21 presents 2SLS-IV estimates for these complex subsamples. I find no significant differences in induced abortion rates for either ectopic/molar pregnancies or PROM (Panel A), suggesting ERD-adherent hospitals do provide abortions when they can be framed as necessary treatment for a "serious pathological condition." However, the complication patterns reveal the costs of organizational management strategies. ERD-adherent hospitalization increases general abortion-related complications (Panel C), particularly for PROM cases (0.17 pp, $p < 0.10$), yet decreases complications from induced abortion procedures (Panel D), particularly for ectopic/molar pregnancies (-0.87 pp, $p < 0.05$). This pattern aligns with the institutional mechanisms of temporal separation and spatial sorting. Ruptured membranes create a time-sensitive infection risk that compounds during periods of institutional delay, as providers have to wait for fetal cardiac activity to cease or sufficient maternal deterioration. Conversely, the decrease in induced abortion complications for ectopic and molar pregnancies aligns with spatial sorting: ERD-adherent hospitals likely transfer the highest risk or clinically ambiguous patients to secular facilities, while competently providing terminations for the remaining (relatively low-risk) cases that clearly meet the ethics committee criteria. By outsourcing the most complex cases, ERD-adherent hospitals effectively suppress their own procedural complication rates while externalizing the highest acuity risks to neighboring institutions.

Robustness checks

I present four additional robustness checks in Appendix Table 15 Columns 2-5.[41] First, I define treatment as going to a hospital that adheres to the ERDs **or** is affiliated with a Catholic health system. Related literature uses this definition for "Catholic" hospitals (Solomon et al., 2020; Hill et al., 2019; Rodriguez et al., 2023; Menegay et al., 2022), but as discussed in Section 2.3,

[41] All robustness checks include the same patient-, hospital-, and county-level controls as described in Section 2.3, state and year fixed effects, and cluster standard errors by patient county.

ERD-adherent hospitals and Catholic-affiliated hospitals are not perfectly analogous. Column (2) shows that results are consistent but slightly attenuated in magnitude, which is expected given the less strict treatment definition. For example, the estimate for induced abortion is -0.88 pp at Catholic-affiliated hospitals (vs. -0.93 pp in the main specification). These results confirm that the findings are robust to alternative definitions of Catholic hospital status while demonstrating that the stricter ERD-adherent treatment definition provides more precise estimates.

Second, I re-estimate results using the untrimmed DD instrument. In the main analyses, I exclude observations in the top 1% of the sample (DD >= 104.3 miles, representing approximately 2.5 to 3 hours of driving time), which helps ensure the common support assumption is met. Column (3) reports estimated effects using the untrimmed sample that includes DD up to a maximum of 190 miles. Results remain substantively unchanged: ERD-adherent hospitalization reduces induced abortion by 0.97 pp (vs. 0.93 pp in the main specification, both $p < 0.01$) and reduces induced abortion complications by 0.30 percentage points (vs. 0.29 pp in the main specification, both $p < 0.05$), with no significant effects on spontaneous abortion or general complications. The first-stage F-statistic decreases slightly from 67.60 to 63.77 when including extreme DD values. These consistent results demonstrate that my findings are robust to trimming decisions and not driven by observations with extreme DD values.

Third, I present results 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. I describe this approach in detail in Appendix F. Results of the robustness check are shown in Column (4), indicating that the LASSO estimates are consistent with (and in some cases, stronger than) results from the main specification. For example, the estimated reduction in induced abortion at ERD-adherent hospitals increases modestly in magnitude (-1.07 pp vs. -0.93 pp in the main specification). Notably, the LASSO model shows an increase in general abortion-related complications at ERD-adherent hospitals (Panel C: 0.52 pp, $p < 0.05$), an effect that was not statistically significant in the main specification. This likely reflects the LASSO algorithm identifying and controlling for additional confounders (e.g., clinical comorbidities) that attenuate the complication effect in the main specification. Note that Hispanic patients did experience an increase in general abortion-related complications at ERD-adherent hospitals in the main specification. Taken together, the consistency—and in some cases, strengthening—of results with machine learning covariate selection reinforce the credibility of my main causal estimates.

Fourth, I present average marginal effects (AME) from a recursive bivariate probit model estimated via full information maximum likelihood (FIML) in Column (5).[42] This FIML approach is similar to a two-stage residual inclusion (2SRI) model in that it uses a nonlinear model to estimate binary outcomes, but is preferred because it exploits the full joint likelihood to estimate the binary treatment and outcome equations simultaneously, improving asymptotic efficiency. Furthermore, applying 2SRI (originally designed for continuous treatments) to a binary treatment may introduce first-stage specification error. As shown in Column (5), results are consistent, though slightly lower in magnitude. For example, the reduction in induced abortion is

[42] I report AMEs to facilitate direct comparison with the linear probability estimates in Columns (1)–(4).

estimated as -0.46 pp ($p < 0.01$), compared to -0.93 in the main specification. This is expected given that 2SLS identifies the LATE for compliers, while the FIML bivariate probit averages effects across the full population. Despite these modeling differences, the consistency in sign and significance across specifications reinforces the primary findings. Overall, these four alterative specifications provide evidence that my results are robust to modeling decisions and strengthen the credibility of the causal estimates.

3.7 Qualitative Interviews

Methods

To better understand the organizational mechanisms and decision-making processes driving my quantitative results, I conducted key informant interviews with 23 clinicians, administrators, and researchers with experience working in Catholic healthcare. I recruited participants through a combination of targeted outreach to my professional network and snowball sampling. Of the 31 individuals contacted, 23 agreed to participate (74% response rate). Four participants were interviewed twice to clarify emerging themes, resulting in a total of 27 interviews. Interviews were conducted via Zoom between August 2024 and December 2025, lasting 45-60 minutes. Participants received a $150 Amazon gift card as compensation for their time. A list of de-identified interviewee roles and affiliations appears in Table 22.

Prior to each interview, I obtained verbal consent for participation and assured participants they could decline to answer any questions. All interviews were de-identified to protect participant confidentiality. I used a semi-structured interview guide that varied slightly based on participant role (clinician, administrator, or researcher) but consistently covered four key domains: (1) professional experience in Catholic healthcare settings and familiarity with ERD implementation, (2) institutional processes for communicating and enforcing ERDs, (3) protocols for handling prohibited procedures, such as abortion, and (4) the role of ethics committees in adjudicating medical necessity. Rather than audio recording, I took detailed (de-identified) notes during interviews, which allowed for more candid discussions of sensitive topics while maintaining participant confidentiality. Therefore, the qualitative findings below are presented as a thematic summaries, illustrated with brief quotations rather than lengthy verbatim quotes.

Through systematic comparison of interview notes, several key themes emerged consistently across participants, regardless of their professional role or institutional affiliation. These themes—particularly the mechanisms of temporal separation and spatial sorting—appeared repeatedly and independently across interviews, providing qualitative validation for the quantitative patterns observed in the inpatient hospital discharge data. Below I further describe my qualitative findings and describe representative examples from interviews. I am careful to maintain participant anonymity while conveying the substantive insights that emerged from interviews.

Findings

Qualitative evidence reveals that Catholic hospitals manage the religious-medical tension through care pathway reconfiguration—systematically restructuring clinical processes to satisfy both logics internally, while shifting costs to patients through delays, transfers, and uncertainty. This care pathway reconfiguration primarily operates through two interconnected mechanisms: *temporal separation*, choosing one pole of a tension at one point in time and then switching after some threshold has been met, and *spatial sorting*, where organizations allocate competing demands to distinct physical locations or units to prevent direct conflict between opposing logics (Poole and Van de Ven, 1989). I further describe each mechanism below, highlighting themes from qualitative interviews.

Temporal separation

My findings suggest that ERD-adherent hospitals navigate the tension between religious and medical logics by sequencing behavior over time. Rather than integrating these conflicting demands simultaneously, hospitals choose one pole (religious adherence) during the early stages of a complication and switch to the opposing pole (medical intervention) once conditions meet doctrinal thresholds for medical necessity. This strategy is codified in the ERDs, which permit procedures resulting in fetal death only for a "proportionately serious pathological condition" that "cannot be safely postponed." Ethics committees provide the institutional mechanism for determining when the threshold has been crossed, as the ERDs mandate that "an ethics committee or some alternate form of ethical consultation should be available to assist by advising on particular ethical situations" (U.S. Conference of Catholic Bishops, 2018).

Multiple interviewees described well-established, formal protocols enabling this temporal switching. One physician noted that obtaining ethics committee approval for medically necessary procedures was "surprisingly straightforward" once clinical deterioration became evident. However, this temporal sequencing is not instantaneous. Determining when a patient's condition becomes "proportionately serious" requires careful observation, clinical consultation, and ethics committee review—processes that introduce delays between the onset of complications and the provision of necessary care. These delays represent the friction costs of maintaining organizational equilibrium: while the hospital successfully navigates its dual identity internally, patients bear the burden externally.

Qualitative interviews also shed light on why induced abortion complications appear lower at ERD-adherent hospitals. If a patient is discharged after being denied an abortion and later returns to an emergency department with a complication, she is likely to seek care at a non-Catholic facility where she knows there will not be religious restrictions. Also, when an ERD-adherent hospital stabilizes a patient but transfers her to a secular facility prior to completing a termination, any subsequent complications—such as sepsis or hemorrhage—are coded at the receiving institution rather than the ERD-adherent hospital. In both cases, the complication is attributed to the second institution in administrative data, not to the ERD-adherent hospital where the initial denial or delay occurred. In addition, when ERD-adherent hospitals do induce abortions, these procedures receive significant institutional oversight, which likely reduces procedural complications for the selected cases performed locally. Together, these mechanisms suggest that lower induced abortion complication rates at ERD-adherent hospitals reflect case selection and institutional practices rather than superior procedural quality—and therefore should be interpreted with appropriate caution.

Spatial sorting

Consistent with Poole and Van de Ven (1989)'s theory of *spatial sorting*, I find through my qualitative interviews that ERD-adherent hospitals often allocate competing demands to distinct physical locations to prevent direct confrontation between opposing logics. When a pregnancy complication requires intervention that conflicts with religious directives (e.g., abortion), hospitals transfer patients to non-Catholic facilities, effectively externalizing the tension rather than resolving it internally. Clinicians described the difficulty of informing patients (often in an urgent situation) that they must be transferred because the necessary care cannot be provided at the ERD-adherent hospital. This geographic displacement allows the Catholic institution to maintain doctrinal compliance while ensuring patients can theoretically access medically necessary care—albeit elsewhere, and with significant costs.

Notably, interview data supports the idea that this spatial sorting is sustained by *organizational opacity*, a classic defensive mechanism described in the management literature (Berti and Simpson, 2021; Smith and Lewis, 2011). Multiple clinicians emphasized that ERD-adherent hospitals are not transparent about their Catholic ownership or the implications for reproductive healthcare access. For example, one clinician stated: "They don't advertise it. They don't want to scare patients away." By obscuring their religious affiliation and associated care restrictions, organizations reduce their own internal tension but shift the cognitive and operational burden onto patients, who must navigate a restricted care environment without full information.

In addition, qualitative interviews reveal how *environmental scarcity* further shapes spatial sorting. Clinicians at urban Catholic hospitals described routine transfers to nearby secular facilities, with one administrator noting "strong relationships" enabling relatively quick transfers. In contrast, rural clinicians emphasized that geographic isolation significantly changes their clinical decision-making given the long drive (or in some cases, helicopter ride) that would be necessary to transfer patients in a timely manner. For example, a clinician working at a rural Catholic hospital said she was more likely to "ask for forgiveness rather than permission" when providing time-sensitive medically necessary care that conflicted with religious doctrine. However, these procedures were kept "under the radar" to maintain legitimacy. Overall, the geographic heterogeneity demonstrates that management strategies are not uniformly available across organizational contexts but depend on environmental resources, transfer infrastructure, and the availability of alternative care sites.

3.8 Discussion

Hybridity is a defining feature of the modern healthcare landscape, where organizations must navigate multiple—and often conflicting—institutional logics to maintain their legitimacy and identity. However, previous literature has focused on *internal* processes and outcomes of balancing conflicting logics within hybrid organizations. In this study, I extend the literature by analyzing impact of these management strategies on external stakeholders. I find that temporal separation and spatial sorting are the key organizational mechanisms used to manage conflicting logics. I then provide the first large-scale causal evidence demonstrating that these management strategies have significant consequences for external stakeholders, with costs falling disproportionately on those with the least time, resources, and social capital to navigate institutional complexity. I further show that these effects are shaped by environmental context: the degree of market concentration determines which management strategies are available, producing systematic heterogeneity in how costs are distributed across stakeholder populations.

I develop these arguments in a setting where organizational tensions are particularly acute: U.S. Catholic hospitals, which must simultaneously adhere to Ethical and Religious Directives prohibiting abortion and fulfill professional medical obligations to patient welfare. Using a mixed-methods design that combines a quasi-experimental 2SLS-IV analysis of over one million urgent pregnancy-related hospitalizations with 27 qualitative interviews, I find that managing the religious-medical tension results in decreased access to care and worse quality. Specifically, when patients have an urgent, pregnancy-related admission at a hospital that must follow religious directives, they are 0.93 pp less likely to be induced for an abortion procedure (179% decrease relative to the mean). In addition, vulnerable populations, specifically Hispanic patients and those with complex pregnancies, experience lower quality of care at hospitals with religious restrictions.

Quantitative and qualitative evidence reveal that ERD-adherent hospitals manage the religious-medical tension through *temporal separation*, in which hospitals delay intervention until clinical deterioration satisfies doctrinal thresholds for medical necessity, and *spatial sorting*, in which hospitals transfer cases that conflict with religious doctrine to secular facilities rather than resolving the contradiction internally. These strategies stabilize the organization's dual identity but externalize friction costs onto patients. Consistent with theoretical propositions that environmental scarcity constrains management options (Smith and Lewis, 2011; Raisch and Birkinshaw, 2008), I find these mechanisms are primarily used in urban markets with available secular alternatives. Crucially, when spatial sorting is unavailable—as in rural markets with few alternative providers—hospitals do not default to the religious logic. Instead, they prioritize the medical logic, providing necessary care despite religious restrictions. More broadly, the reliance on spatial sorting as a management strategy is problematic because it depends on the existence and capacity of secular referral sites, which then have the responsibility of caring for high-acuity patients denied or delayed care at ERD-adherent facilities. These dynamics also reinforce the systemic marginalization of abortion care and contribute to the broader stigma experienced by both patients and providers.

Finally, this study extends the operational literature on workarounds by identifying the boundary conditions of process-based improvement. Prior research recommends reducing workarounds through formal, process-focused approaches to operational design, on the assumption that the underlying operational failure is correctable (Tucker et al., 2020). However, my findings suggest that in hybrid organizations where tensions stem from irreconcilable institutional mandates—such as the ERDs—the underlying conflict cannot be resolved through better process design alone. While formal mechanisms like ethics committee reviews are intended to manage these tensions, they often prioritize religious logic over medical necessity, leading to the delays and complications.

Prior research has also found that strong *intra*-organizational relationships are important mechanisms for reducing informal workarounds and improving patient outcomes (Reay and Hinings, 2009; Tucker et al., 2014). I build upon this by showing the importance of formal *inter*-organizational relationships (e.g., patient transfer/referral networks) in reducing the need for informal workarounds. However, reliance on these inter-organizational relationships is not a panacea: secular referral partners may not exist in rural markets, and transfers cause delays that can worsen outcomes for time-sensitive conditions.

I should note a few limitations of this study. First, in terms of data, I can only count "reactive" (rather than "proactive") hospital transfers, and therefore underestimate the total volume of spatial sorting. My data also lack gestational age, preventing direct viability assessment, though sample restrictions make pre-viability cases overwhelmingly likely and coding decisions were confirmed with clinical experts. In addition, statistical power is constrained in subgroup analyses given the rarity of outcomes. Second, this study focused on the formal mechanisms hybrid organizations use to manage conflicting logics. Informal, provider-level workarounds to manage tensions in ERD-adherent hospitals has been the subject of previous qualitative research (Freedman, 2023). However, quantifying the frequency and variation in these informal workarounds should be the subject of future research. Third, the qualitative component of my research has limitations. Because the interviews were intended to provide contextual depth to the quantitative results (rather than serve as standalone qualitative analysis), I utilized a single-coder, iterative thematic analysis. While this approach effectively surfaced organizational mechanisms, it lacks the inter-rater reliability checks typical of formal qualitative research. Additionally, to encourage candor regarding the sensitive nature of this topic, interviews were not audio-recorded. While this protocol likely increased data fidelity regarding sensitive behaviors, it precluded the use of extended verbatim quotes and introduces potential recall bias.

Implications

This research has important implications for managers and leaders of hybrid organizations—both within the healthcare sector and beyond. Nonprofit hospitals, physician groups, medical schools, accountable care organizations, and religiously affiliated health systems all navigate competing institutional logics related to financial performance, clinical excellence, and social or religious missions (Greenwood et al., 2011; Reay and Hinings, 2009; Dunn and Jones, 2010; Goodrick and Reay, 2016). My findings suggest that the management of these tensions creates friction costs that are often not visible in traditional performance metrics, but rather manifest in the experiences of external stakeholders who may lack the appropriate platform to share their experiences. Leaders of hybrid organizations should therefore build relevant feedback mechanisms into governance and accountability structures, ensuring that the costs of internal balancing acts remain visible to those making strategic decisions. More broadly, this study cautions cross-sector leaders that organizational performance must be evaluated not only by internal sustainability, but by its impact on those outside the organization.

My findings also underscore the role of formal, process-based approaches to managing conflicting logics. Without such mechanisms, employees are more likely to rely on informal workarounds that degrade operational efficiency and often result in worse outcomes (Halbesleben et al., 2008; Tucker et al., 2020, 2014). However, formal processes are not always sufficient: when the underlying tension is structural and irreconcilable—as in the case of ERD-adherent hospitals—internal process improvements alone cannot eliminate the conflict. In these settings, broader policy intervention is necessary.

My findings speak directly to two policy levers. First, regarding consolidation and antitrust oversight: my results provide strong evidence to state and federal antitrust authorities that service availability—alongside price and quality—should be a key consideration when evaluating the potential impact of mergers and acquisitions (M/A). Several states have already moved in this direction, passing or proposing laws that give regulators greater oversight in proposed M/A deals with the goal of maintaining access to essential services (see Table 3 for a summary of related bills). The experience of Dignity Health, a large Catholic health system, provides a blueprint for how government oversight (in this case, the state of California) can be used to protect patient access without requiring full divestiture.[43] Second, this research supports the expansion of transparency requirements to reduce the information asymmetry between patients and religiously-affiliated hospitals. Many patients remain unaware of a hospital's Catholic identity or the associated clinical restrictions. Policies like those recently passed in Colorado and Washington (or currently proposed in New York) require hospitals to publicly disclose restricted services. While not a permanent resolution to the underlying institutional conflict, these policies offer an actionable short-term solution to improve patient awareness and choice.

Conclusion

This study provides the first large-scale causal evidence that organizational strategies used to manage competing institutional logics impose significant costs on external stakeholders, with the heaviest burden falling on those least equipped to navigate institutional complexity. As hybrid organizations proliferate across healthcare and other sectors, these findings caution managers and policymakers alike: organizational sustainability and stakeholder welfare are not the same thing, and strategies to achieve the former can systematically undermine the latter.

[43] As a condition of approving the 2019 merger—which created CommonSpirit Health, now the largest nonprofit hospital system in the U.S.—the California Attorney General required the new entity to maintain existing levels of reproductive health and emergency care for five years, and structured the non-Catholic Dignity hospitals under a separate system that preserved their ability to provide services prohibited under the ERDs (Littlefield, 2018). Before that, Dignity Health restructured in 2012 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. The hospitals that are not expected to fully comply with ERDs instead operate under a "Statement of Common Values" (Vizient, Inc. and JD Healthcare, Inc., 2018).

3.9 Chapter 3 Tables

Table 14: Study sample by state and year (n=1,051,745)

State	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022
AR	1,276	1,239	1,162	1,104	–	3,219	2,129	–	–	–	–	–	–
AZ	13,767	12,947	12,247	11,288	11,259	10,710	10,252	9,914	10,908	–	–	–	–
CO	4,363	4,320	4,062	3,763	4,241	4,708	5,071	4,907	–	–	–	–	–
FL	28,007	28,917	28,620	27,507	28,273	29,515	30,837	31,353	32,217	–	–	–	–
KY	–	–	–	–	–	–	–	5,104	–	–	–	–	–
MD	–	–	–	–	–	–	–	–	8,687	8,225	7,704	7,678	3,920
NJ	10,598	10,302	9,992	9,369	8,944	8,271	7,988	7,812	8,340	8,467	8,009	9,356	4,841
NY	30,677	33,703	31,177	29,218	29,279	29,111	30,689	31,158	29,847	28,212	25,481	26,821	12,766
OR	4,426	4,181	4,215	4,105	3,894	3,881	4,195	4,208	–	–	–	–	–
WA	6,344	6,750	6,917	6,162	6,044	6,131	6,750	7,331	7,016	6,703	6,331	7,010	3,303
Total	99,458	102,359	98,392	92,516	91,934	95,546	97,911	101,787	97,015	51,607	47,525	50,865	24,830

Table 15: Sample Summary Statistics

	Hospital Type		
	ERD-adherent	Non-ERD-adherent	All Hospitals
Panel A. Patient characteristics			
Mean age (SD)	28 (6)	28 (6)	28 (6)
Race/ethnicity (%)			
White	48	37	38
Black	20	24	24
Hispanic	20	24	24
Asian/PI	4	5	5
Other/missing	8	10	9
Insurance type (%)			
Private	41	38	38
Medicaid	50	54	53
Self pay/other	9	9	9
Married (%)	15	16	16
Living in urban area (%)	67	77	76
Clinical characteristics			
Diabetes (%)	3	3	3
Hypertension (%)	1	2	2
Obesity (%)	7	7	7
Alcohol/drug abuse (%)	6	5	5
Mean Elixhauser index (SD)	0.2 (3)	0.3 (3)	0.3 (3)
Panel B. Hospital characteristics			
Ownership type (%)			
Government	0.3	19	16
Non-profit	94	68	71
For-profit	5	13	12
Teaching hospital (%)	3	37	33
Presence of obstetrics unit (%)	90	87	87
Mean HHI concentration (SD)	0.5 (0.4)	0.4 (0.3)	0.4 (0.3)
Mean total annual admissions (SD)	20,950 (8,990)	23,710 (11,092)	23,372 (10,894)
Mean obstetric volume (SD)	0.4 (0.1)	0.3 (0.1)	0.3 (0.1)
Panel C. Patient county characteristics			
Mean Catholic adherents (SD)	22 (14)	23 (10)	23 (11)
Mean Catholic congregations (SD)	36 (28)	53 (36)	51 (36)
Mean OB/GYNs per 100,000 (SD)	12 (6)	13 (8)	13 (8)
Mean abortion clinics (SD)	3 (3)	5 (5)	5 (5)
Total discharges	128,707 (12%)	923,038 (88%)	1,051,745 (100%)
Total hospitals	121 (14%)	728 (86%)	849 (100%)

Notes: Hospital concentration measured using the Herfindahl-Hirschman Index (HHI) with annual admissions per hospital service area. Obstetric volume defined as the proportion of annual admissions for obstetric care.

Table 16: Outcome Summary Statistics

	Hospital Type		
	ERD-adherent	Non-ERD-adherent	All Hospitals
Panel A. Abortion Provision			
Induced	0.31%	0.55%	0.52%
Spontaneous (i.e., miscarriage)	2.22%	2.20%	2.20%
Panel B. Abortion-related complications			
General	0.79%	0.86%	0.85%
Induced abortion specifically	0.18%	0.27%	0.26%

Notes: Induced abortion captures any procedure to terminate pregnancy, including dilation and curettage (D&C), dilation and evacuation (D&E), and medication abortion. Spontaneous abortion is also called miscarriage. Outcomes were measured as a binary indicators for the presence of relevant ICD-9/10 diagnosis or procedure codes, CPT codes, or DRG codes on the discharge record.

Table 17: Discharge Status and Transfer Rates

	(1) Routine discharge	(2) Transfer	(3) Other
Panel A. By hospital religiosity			
ERD-adherent	94.38	2.23	3.39
Non-ERD-adherent	93.47	2.08	4.45
Panel B. By market concentration			
Low concentration (urban)	93.87	1.54	4.59
High concentration (rural)	93.14	2.93	3.93
Panel C. Subset of urban hospitals			
ERD-adherent	94.64	1.77	3.59
non-ERD-adherent	93.78	1.51	4.71

Notes: ERD, Ethical and Religious Directives. These rates reflect only patients who were admitted prior to transfer, likely undercounting the total volume of patients transferred or turned away prior to admission. Urban hospitals are those below the average level of concentration (measured using the Herfindahl-Hirschman Index based on the hospital service area and number of admissions), indicating more opportunity to transfer patients in a timely manner. Rural hospitals are above the average. "Other" category includes discharge to home healthcare, law enforcement, or death.

Table 18: The Effect of ERD-adherent Hospitalization on Abortion Provision and Complications

	(1) OLS	(2) 2SLS
Panel A. Induced Abortion		
ERD-adherent	-0.00284***	-0.00933**
	(0.000592)	(0.00372)
Mean dep. var.	0.0052	0.0052
Panel B. Spontaneous Abortion (Miscarriage)		
ERD-adherent	-0.00131	-0.00505
	(0.00179)	(0.00646)
Mean dep. var.	0.0220	0.0220
Panel C. General Abortion-Related Complication		
ERD-adherent	-0.00129***	0.00138
	(0.000494)	(0.00231)
Mean dep. var.	0.0085	0.0085
Panel D. Induced Abortion Complication		
ERD-adherent	-0.00111***	-0.00285**
	(0.000267)	(0.00136)
Mean dep. var.	0.0026	0.0026
Observations	1,051,745	1,051,745
First stage F-stat		67.60

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. Column (1) reports coefficients from the OLS regression estimating the association between ERD-adherent hospitalization and probability of each outcome. Column (2) reports the Local Average Treatment Effect (LATE) from the Two-Stage Least Squares (2SLS) model estimating the causal effect of ERD-adherent hospitalization for the complier population.

Table 19: The Effect of ERD-adherent Hospitalization on Abortion Provision and Complications by Hospital Concentration, 2SLS Estimates

	(1) High concentration (Rural)	(2) Low concentration (Urban)
Panel A. Induced Abortion		
ERD-adherent	-0.00108	-0.0200***
	(0.00188)	(0.00519)
Mean dep. var.	0.0047	0.0056
Panel B. Spontaneous Abortion (Miscarriage)		
ERD-adherent	-0.00154	-0.00275
	(0.00842)	(0.00909)
Mean dep. var.	0.0256	0.0195
Panel C. General Abortion-Related Complication		
ERD-adherent	-0.000745	0.00268
	(0.00320)	(0.00318)
Mean dep. var.	0.0091	0.0082
Panel D. Induced Abortion Complication		
ERD-adherent	-0.000933	-0.00571***
	(0.00125)	(0.00170)
Mean dep. var.	0.0025	0.0026
Observations	425,037	626,708
First stage F-stat	36.68	36.17

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. Coefficients report the Local Average Treatment Effect (LATE) from the Two-Stage Least Squares (2SLS) model. Rural hospitals are those above the average level of concentration (measured using the Herfindahl-Hirschman Index based on the hospital service area and number of admissions), indicating fewer alternatives for care and less opportunity to transfer patients in a timely manner. Urban hospitals are below the average.

Table 20: The Effect of ERD-adherent Hospitalization on Abortion Provision and Complications by Patient Race/Ethnicity, 2SLS Estimates

	(1) White Patients	(2) Black Patients	(3) Hispanic Patients
Panel A. Induced Abortion			
ERD-adherent	-0.00760***	-0.0176**	-0.00746*
	(0.00259)	(0.00887)	(0.00388)
Mean dep. var.	0.0048	0.0066	0.0054
Panel B. Spontaneous Abortion (Miscarriage)			
ERD-adherent	-0.00634	-0.0121	-0.00979
	(0.00485)	(0.0151)	(0.0122)
Mean dep. var.	0.0178	0.0286	0.0255
Panel C. General Abortion-Related Complication			
ERD-adherent	0.000982	0.00392	0.00569*
	(0.00251)	(0.00498)	(0.00314)
Mean dep. var.	0.0083	0.0099	0.0085
Panel D. Induced Abortion Complication			
ERD-adherent	-0.00254**	-0.00622	0.000201
	(0.00119)	(0.00379)	(0.00169)
Mean dep. var.	0.0023	0.0036	0.0026
Observations	403,504	248,465	249,819
First stage F-stat	97.31	22.47	52.78

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. Coefficients report the Local Average Treatment Effect (LATE) from the Two-Stage Least Squares (2SLS) model.

Table 21: The Effect of ERD-adherent Hospitalization on Abortion Provision and Complications, Subset of Complex Pregnancies, 2SLS Estimates

	(1) Ectopic/Molar	(2) PROM
Panel A. Induced Abortion		
ERD-adherent	-0.0189	-0.00604
	(0.0125)	(0.00504)
Mean dep. var.	0.0108	0.0024
Panel B. Spontaneous Abortion (Miscarriage)		
ERD-adherent	-0.00659	0.00404
	(0.00978)	(0.00353)
Mean dep. var.	0.0143	0.0055
Panel C. General Abortion-Related Complication		
ERD-adherent	0.0233	0.00170*
	(0.0224)	(0.00103)
Mean dep. var.	0.0618	0.0011
Panel D. Induced Abortion Complication		
ERD-adherent	-0.00868**	-0.00242
	(0.00438)	(0.00223)
Mean dep. var.	0.0033	0.0011
Observations	43,349	127,222
First stage F-stat	74.59	62.86

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. Coefficients report the Local Average Treatment Effect (LATE) from the Two-Stage Least Squares (2SLS) model. An ectopic pregnancy is one that happens outside of the uterus, typically in the fallopian tube. A molar pregnancy is one where an abnormally fertilized egg implants in the uterus. PROM is premature rupture of membranes. These complex pregnancies are more likely to need an induced abortion to preserve the life or health of the mother.

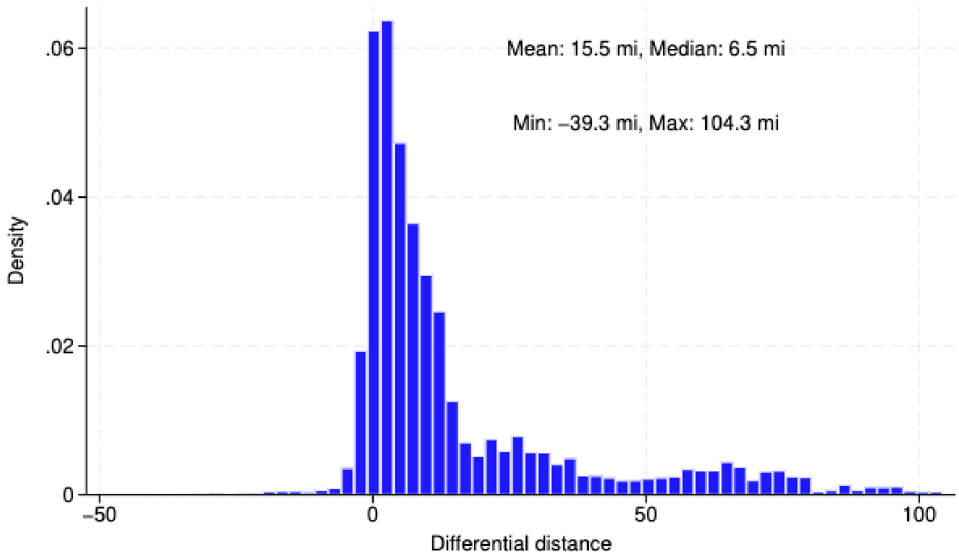
Table 22: Qualitative Interviews (n=27)

Type	Role	Organization	Gender
Administration	Director, Formation Design	Catholic health system	Female
Administration	SVP, Population Health	Catholic health system	Male
Administration	SVP, Community Health	Catholic health system	Male
Administration	Medical Director	Catholic hospital	Male
Administration	President & CEO	Catholic hospital	Male
Administration	President & CEO	Catholic hospital	Male
Administration	SVP, Mission Integration	Catholic health system	Male
Administration	SVP, Theology & Ethics	Catholic health system	Female
Clinical	OBGYN Physician (2)	Catholic hospital	Male
Clinical	OBGYN Physician (2)	Catholic hospital	Female
Clinical	OBGYN Physician (2)	Catholic hospital	Male
Clinical	OBGYN Physician (2)	Catholic hospital	Female
Clinical	OBGYN Physician	Catholic hospital	Male
Clinical	OBGYN Physician	Catholic hospital	Female
Clinical	OBGYN Physician	Catholic hospital	Female
Clinical	OBGYN Physician	Catholic hospital	Female
Research	Assistant Professor	University	Female
Research	Professor	University	Male
Research	Director, Women's Health	Nonprofit	Female
Research	Professor	University	Female
Research	Professor	University	Male
Research	Professor	University	Female
Research	Professor	University	Male

Notes: All participants were de-identified to protect confidentiality. I interviewed 4 of the OBGYN physicians twice (to further contextualize and clarify emerging themes) for a total of 27 interviews.

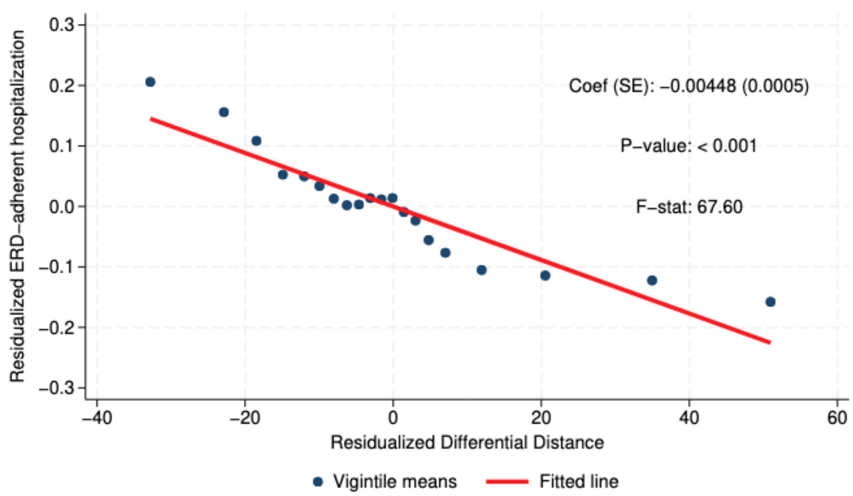
3.10 Chapter 3 Figures

Figure 5: DD Instrument Distribution



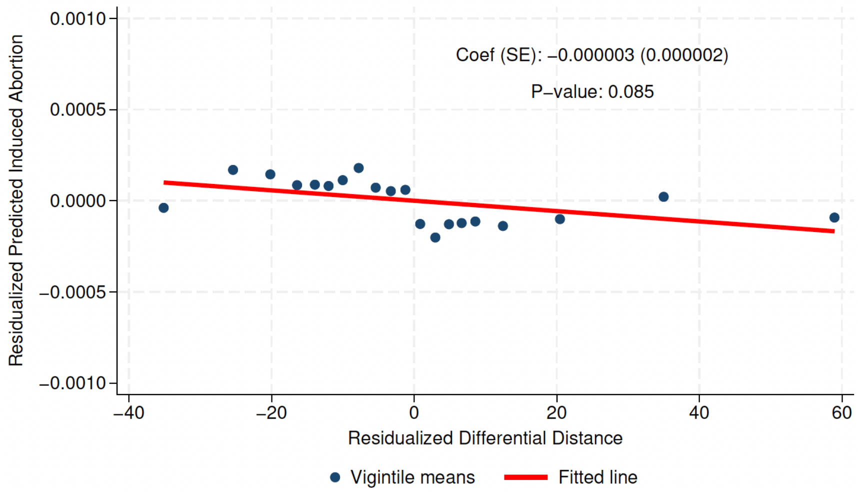
Notes: This figure shows the distribution of the differential distance (DD) instrument, which is calculated as the distance from a patient's zip code centroid to the nearest ERD-adherent hospital minus the distance to the nearest non-ERD-adherent hospital. Positive values indicate the nearest hospital is non-ERD-adherent.

Figure 6: 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 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.

Figure 7: Balance Test: Induced Abortion



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 fixed effects.

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4 Appendices

Appendix A: Pre-trends Test

Appendix Table 1 presents p-values from formal statistical tests of jointly significant pre-trends for each outcome. We compare trends in the pre-period between the control group ("never-treated" independent hospitals) and various treatment groups: hospitals later acquired by any system type (col. 1), hospitals later acquired by Catholic systems (col. 2), and hospitals later acquired by non-Catholic systems (col. 3). Joint p-values of > 0.05 support the parallel pre-trends assumption, as we are unable to reject the null hypothesis of no differences in trends between treatment and control groups prior to acquisition. We control for Medicaid payer mix in our main difference-in-differences (DID) analyses given that trends were not parallel between the treatment and control groups as shown below

Table 1: Pre-trends joint p-values to test parallel trends assumption

	(1) Any acquisition	(2) Catholic-acquired	(3) Non-Catholic-acquired
Mission-oriented services			
Chaplaincy	0.85	0.40	0.69
Charity care	0.43	0.58	0.39
Community outreach	0.14	0.43	0.09
Linguistic/translation services	0.32	0.66	0.12
Obstetrics			
Obstetrics unit	0.74	0.08	0.93
Obstetric bed count	0.56	0.68	0.55
Proportion obstetric beds/total beds	0.92	0.27	0.86
Utilization			
Bed count	0.30	0.41	0.19
Admissions per bed	0.64	0.66	0.73
Medicaid days per bed	0.04**	0.09	0.05
Medicare days per bed	0.44	0.10	0.46
Expenses			
Total expenses per bed	0.68	0.29	0.83
Payroll expenses per bed	0.63	0.43	0.74
Employee benefit expenses per bed	0.27	0.15	0.32
Employment			
Total FTEs per bed	0.76	0.23	0.76
MD FTEs per bed	0.79	0.12	0.85
Nurse FTEs per bed	0.77	0.63	0.57
Support staff FTEs per bed	0.27	0.53	0.35
Covariates			
Medicaid payer mix	0.01**	0.65	0.002***

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix B: Chapter 1 Robustness Checks

Appendix B presents tables for the following robustness checks of Chapter A: standard two-way fixed effects difference-in-differences (DID) estimator rather than Borusyak et al. DID imputation-based approach (Table 1), limiting the pre- and post-periods to 6 years instead of 8 (Table 2), excluding Medicaid payer mix as a covariate (Table D3), and including hospital entity type (government, nonprofit, for-profit) as a covariate (Table D4). These Tables present the average treatment effects of Catholic (col. 1) and non-Catholic (col. 2) acquisitions. If present, column (3) uses an imputation approach to estimate the difference between Catholic-acquired (col. 1) and non-Catholic-acquired (col. 2) treatment effects.

Table 2: Treatment Effects After Catholic and Non-Catholic Acquisitions Using Two-Way Fixed Effects DID Estimator

	(1) Catholic-acquired	(2) Non-Catholic-acquired
Mission-oriented services		
Chaplaincy	0.07	−0.004
	(0.04)	(0.01)
	[−0.01, 0.16]	[−0.03, 0.02]
Charity care	0.04	0.001
	(0.02)	(0.01)
	[−0.01, 0.09]	[−0.02, 0.02]
Community outreach	−0.04	−0.03**
	(0.04)	(0.01)
	[−0.12, 0.04]	[−0.06, −0.01]
Linguistic/translation services	−0.03	0.0003
	(0.04)	(0.01)
	[−0.11, 0.06]	[−0.03, 0.03]
Obstetrics		
Obstetrics unit	−0.06	−0.04***
	(0.03)	(0.01)
	[−0.12, 0.01]	[−0.07, −0.02]
Obstetric bed count	−0.67	−0.78***
	(0.38)	(0.22)
	[−1.42, 0.09]	[−1.20, −0.35]
Proportion obstetric beds	−0.005	−0.002
	(0.004)	(0.002)
	[−0.01, 0.004]	[−0.01, 0.001]

Notes: This table presents results using a standard two-way fixed effects DID estimator. The model includes hospital and year fixed effects and controls for Medicaid payer mix. Standard errors are clustered at the hospital level and presented in parentheses. 95% confidence intervals are presented in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Treatment Effects After Catholic and Non-Catholic Acquisitions with 6 Years Pre- and Post-Period

	(1) Catholic-acquired	(2) Non-Catholic-acquired	(3) Difference (1)-(2)
Mission-oriented services			
Chaplaincy	0.09**	−0.001	0.09**
	(0.04)	(0.01)	(0.04)
	[0.005, 0.17]	[−0.03, 0.02]	[0.004, 0.17]
Charity care	0.04**	−0.0008	0.04
	(0.02)	(0.01)	(0.02)
	[0.001, 0.07]	[−0.02, 0.02]	[−0.002, 0.08]
Community outreach	−0.02	−0.03	0.004
	(0.04)	(0.01)	(0.04)
	[−0.10, 0.05]	[−0.05, 0.0006]	[−0.08, 0.08]
Linguistic/translation services	0.009	0.01	0.002
	(0.04)	(0.01)	(0.04)
	[−0.07, 0.09]	[−0.02, 0.04]	[−0.08, 0.08]
Obstetrics			
Obstetrics unit	−0.04	−0.04***	−0.002
	(0.03)	(0.01)	(0.03)
	[−0.09, 0.01]	[−0.06, −0.01]	[−0.06, 0.05]
Obstetric bed count	−0.63	−0.79***	0.15
	(0.36)	(0.21)	(0.41)
	[−1.34, 0.07]	[−1.20, −0.37]	[−0.65, 0.95]
Proportion obstetric beds	−0.004	−0.001	−0.002
	(0.004)	(0.002)	(0.004)
	[−0.01, 0.004]	[−0.005, 0.002]	[−0.01, 0.006]

Notes: This table presents results limiting the pre- and post-periods to 6 years instead of 8. The model includes hospital and year fixed effects and controls for Medicaid payer mix. Standard errors are clustered at the hospital level and presented in parentheses. 95% confidence intervals are presented in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Treatment Effects After Catholic and Non-Catholic Acquisitions without Medicaid Payer Mix Covariate

	(1) Catholic-acquired	(2) Non-Catholic-acquired	(3) Difference (1)-(2)
Mission-oriented services			
Chaplaincy	0.10**	0.001	0.10**
	(0.05)	(0.01)	(0.05)
	[0.01, 0.19]	[−0.03, 0.03]	[0.01, 0.20]
Charity care	0.04**	−0.001	0.04**
	(0.02)	(0.01)	(0.02)
	[0.005, 0.08]	[−0.02, 0.02]	[0.002, 0.08]
Community outreach	−0.003	−0.02	0.02
	(0.04)	(0.02)	(0.04)
	[−0.09, 0.08]	[−0.05, 0.006]	[−0.07, 0.11]
Linguistic/translation services	0.03	0.01	0.01
	(0.04)	(0.02)	(0.04)
	[−0.06, 0.11]	[−0.02, 0.04]	[−0.07, 0.10]
Obstetrics			
Obstetrics unit	−0.04	−0.03***	−0.01
	(0.03)	(0.01)	(0.03)
	[−0.09, 0.01]	[−0.06, −0.01]	[−0.06, 0.05]
Obstetric bed count	−0.78	−0.86***	0.07
	(0.42)	(0.22)	(0.46)
	[−1.61, 0.05]	[−1.29, −0.43]	[−0.83, 0.99]
Proportion obstetric beds	−0.005	−0.001	−0.004
	(0.004)	(0.002)	(0.005)
	[−0.01, 0.004]	[−0.005, 0.002]	[−0.01, 0.006]

Notes: This table presents results excluding Medicaid payer mix as a covariate. The model includes hospital and year fixed effects and controls for Medicaid payer mix. Standard errors are clustered at the hospital level and presented in parentheses. 95% confidence intervals are presented in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix C: 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.

Appendix D: Medical Codes for Study Outcomes

Table 5: ICD, CPT, and DRG codes for identifying birth hospitalizations (Chapter 2)

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

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

Table 6: ICD, CPT, and DRG codes identify permanent and non-permanent contraception (Chapter 2)

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

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

Table 7: ICD, CPT, and DRG codes to identify birth- and pregnancy-related hospital admissions (Chapter 3)

Panel A. Any pregnancy- and birth-related hospitalizations	
ICD-9 DX	64000, 64001, 64080, 64081, 64100, 64101, 64110, 64111, 64130, 64131, 64200, 64201, 64300, 64301, 64410, 64411, 64420, 64421, 64440, 64441, 64500, 64501, 64503, 64510, 64511, 64513, 64520, 64521, 64523, 650, 65100, 65101, 65110, 65111, 65120, 65121, 65170, 65171, 65173, 65180, 65181, 65190, 65191, 65421, 65640, 65641, 65643, 66970, 66971, V220, V221, V222, V240, V241, V242, V270, V271, V272, V273, V274, V276, V277, V724, V7242, V9100, V9101, V9102, V9103, V9109, V9110, V9111, V9112, V9119, V9120, V9121, V9122, V9129, V9190, V9191, V9192, V9199
ICD-9 PR	740, 741, 742, 744, 7499
ICD-9 CCS	DX single: 185, 196; DX multiple: 11.03.05; PR single: 134; PR multiple: 13.02
ICD-10 DX	O364, O364XX0, O364XX1, O364XX2, O364XX3, O364XX4, O364XX5, O364XX9, O4202, O4212, O4292, O480, O481, O601, O6010, O6010X0, O6010X1, O6010X2, O6010X3, O6010X4, O6010X5, O6010X9, O6012, O6012X0, O6012X1, O6012X2, O6012X3, O6012X4, O6012X5, O6012X9, O6013, O6013X0, O6013X1, O6013X2, O6013X3, O6013X4, O6013X5, O6013X9, O6014, O6014X0, O6014X1, O6014X2, O6014X3, O6014X4, O6014X5, O6014X9, O6020X0, O6020X1, O6020X2, O6020X3, O6020X4, O6020X5, O6020X9, O6022X0, O6022X1, O6022X2, O6022X3, O6022X4, O6022X5, O6022X9, O6023X0, O6023X1, O6023X2, O6023X3, O6023X4, O6023X5, O6023X9, O80, O81, O82, O83, O84, P95, Z3400, Z3401, Z3402, Z3403, Z3480, Z3481, Z3482, Z3483, Z3490, Z3491, Z3492, Z3493, Z370, Z371, Z372, Z373, Z374, Z3750, Z3751, Z3752, Z3753, Z3754, Z3759, Z3760, Z3761, Z3762, Z3763, Z3764, Z3769, Z377, Z3800, Z3801, Z381, Z382, Z3830, Z3831, Z384, Z385, Z3861, Z3862, Z3863, Z3864, Z3865, Z3866, Z3868, Z3869, Z387, Z388, Z390
ICD-10 PR	10D00Z0, 10D00Z1, 10D00Z2, 10D07Z3, 10D07Z4, 10D07Z5, 10D07Z6, 10D07Z7, 10D07Z8, 10E0XZZ
CPT	59409, 59410, 59514, 59515, 59612, 59614, 59620, 59622
DRG	765, 766, 767, 768, 774, 775, 783, 784, 785, 786, 787, 788, 796, 797, 798, 805, 806, 807
Panel B. Normal birth or delivery	
DRG	765, 766, 767, 768, 774, 775, 783, 784, 785, 786, 787, 788, 796, 797, 798, 805, 806, 807
Panel C. Ectopic pregnancies	
ICD-9 DX	6330, 63300, 63301, 6331, 63310, 63311, 6332, 63320, 63321, 6338, 63380, 63381, 6339, 63390, 63391
ICD-9 PR	6662, 6911, 743
ICD-9 CCS	DX single: 180; DX multiple: 11.03.01; PR single: 122; PR multiple: 13.10
ICD-10 DX	O000, O0000, O0001, O001, O0010, O00101, O00102, O00109, O0011, O00111, O00112, O00119, O002, O0020, O00201, O00202, O00209, O0021, O00211, O00212, O00219, O008, O0080, O0081, O009, O0090, O0091, O3670X0, O3670X1, O3670X2, O3670X3, O3670X4, O3670X5, O3670X9, O3671X0, O3671X1, O3671X2, O3671X3, O3671X4, O3671X5, O3671X9, O3672X0, O3672X1, O3672X2, O3672X3, O3672X4, O3672X5, O3672X9, O3673X0, O3673X1, O3673X2, O3673X3, O3673X4, O3673X5, O3673X9
ICD-10 PR	10D20ZZ, 10D24ZZ, 10D27ZZ, 10D28ZZ, 10J20ZZ, 10J23ZZ, 10J24ZZ, 10J27ZZ, 10J28ZZ, 10J2XZZ, 10S20ZZ, 10S23ZZ, 10S24ZZ, 10S27ZZ, 10S28ZZ, 10T20ZZ, 10T23ZZ, 10T24ZZ, 10T27ZZ, 10T28ZZ
CPT	59120, 59121, 59130, 59135, 59136, 59150, 59151
DRG	777
Panel D. Molar pregnancies	
ICD-9 DX	630
ICD-10 DX	O010, O011, O019, O020, O021, O0281, O0289, O029
CPT	59100
Panel E. Premature rupture of membranes (PROM)	
ICD-9 DX	6581*, 7611*
ICD-10 DX	O4201*, O4211*, O4291*

Notes: This table presents the diagnosis (DX) and procedure (PR) codes used to identify birth- and pregnancy-related hospital admissions across five subsets. Asterisks (*) indicate wildcard prefixes. International Classification of Diseases, Ninth Revision (ICD-9) codes apply to admissions before Q4 2015, while ICD-10 codes apply to admissions from Q4 2015 onward. ICD Clinical Classifications Software (CCS) codes include both single-level and multi-level classifications. Current Procedural Terminology (CPT) and Diagnosis-Related Group (DRG) codes have no time restriction.

Table 8: ICD, CPT, and DRG codes to identify abortion-related outcomes (Chapter 3)

Panel A. Induced abortion	
ICD-9 DX	63500, 63501, 63502, 63510, 63511, 63512, 63520, 63521, 63522, 63530, 63531, 63532, 63540, 63541, 63542, 63550, 63551, 63552, 63560, 63561, 63562, 63570, 63571, 63572, 63580, 63581, 63582, 63590, 63591, 63592, 63600, 63601, 63602, 63610, 63611, 63612, 63620, 63621, 63622, 63630, 63631, 63632, 63640, 63641, 63642, 63650, 63651, 63652, 63660, 63661, 63662, 63670, 63671, 63672, 63680, 63681, 63682, 63690, 63691, 63692, 63700, 63701, 63702, 63710, 63711, 63712, 63720, 63721, 63722, 63730, 63731, 63732, 63740, 63741, 63742, 63750, 63751, 63752, 63760, 63761, 63762, 63770, 63771, 63772, 63780, 63781, 63782, 63790, 63791, 63792, 6380, 6381, 6382, 6383, 6384, 6385, 6386, 6387, 6388, 6389
ICD-9 PR	6901, 6951, 7491, 750
ICD-9 CCS	DX single: 178; DX multiple: 11.02.02; PR single: 126; PR multiple: 12.07
ICD-10 DX	O045*, O046*, O047*, O048*, O070*, O071*, O072*, O073*, O074*, Z332
ICD-10 PR	10A00ZZ, 10A03ZZ, 10A04ZZ, 10A07Z6, 10A07ZW, 10A07ZX, 10A07ZZ, 10A08ZZ
CPT	59840, 59841, 59850, 59851, 59852, 59855, 59856, 59857, S0199, S2260, S2265, S2266, S2267, S8055
DRG	770, 779
Panel B. Spontaneous abortion (Miscarriage)	
ICD-9 DX	63400, 63401, 63402, 63410, 63411, 63412, 63420, 63421, 63422, 63430, 63431, 63432, 63440, 63441, 63442, 63450, 63451, 63452, 63460, 63461, 63462, 63470, 63471, 63472, 63480, 63481, 63482, 63490, 63491, 63492
ICD-9 CCS	DX single: 177; DX multiple: 11.02.01
ICD-10 DX	O030*, O031*, O032*, O033*, O034*, O035*, O036*, O037*, O038*, O039*
Panel C. Abortion-related complication	
ICD-9 DX	63500, 63501, 63502, 63510, 63511, 63512, 63520, 63521, 63522, 63530, 63531, 63532, 63540, 63541, 63542, 63550, 63551, 63552, 63560, 63561, 63562, 63570, 63571, 63572, 63580, 63581, 63582, 63600, 63601, 63602, 63610, 63611, 63612, 63620, 63621, 63622, 63630, 63631, 63632, 63640, 63641, 63642, 63650, 63651, 63652, 63660, 63661, 63662, 63670, 63671, 63672, 63680, 63681, 63682, 63700, 63701, 63702, 63710, 63711, 63712, 63720, 63721, 63722, 63730, 63731, 63732, 63740, 63741, 63742, 63750, 63751, 63752, 63760, 63761, 63762, 63770, 63771, 63772, 63780, 63781, 63782, 6380, 6381, 6382, 6383, 6384, 6385, 6386, 6387, 6388, 6389
ICD-9 CCS	DX single: 179; DX multiple: 11.02.03
ICD-10 DX	O045*, O046*, O047*, O048*, O070*, O071*, O072*, O073*

Notes: This table presents the diagnosis (DX) and procedure (PR) codes used to identify abortion services and complications. Asterisks (*) indicate wildcard prefixes. 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 E: Chapter 2 Robustness Checks

Table 9: The Effect of ERD-adherent Hospitalization on Postpartum Contraception Provision (using alternative rural-urban definitions), 2SLS Estimates

	OMB definition		Competition (HHI)	
	Rural	Urban	Low, rural	High, urban
	(1)	(2)	(3)	(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.

Table 10: The Effect of ERD-adherent Hospitalization on Short-Interval Pregnancy (using alternative rural-urban definitions), 2SLS Estimates

	OMB definition		Competition (HHI)	
	Rural	Urban	Low, rural	High, urban
	(1)	(2)	(3)	(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.

Table 11: 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).

Table 12: 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.

Table 13: 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 F provides details about this model).

Appendix F: LASSO-orthogonalized Algorithm for Machine Learning Covariate Selection

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. 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).

Appendix G: Chapter 3 Robustness Checks

Table 14: The Effect of ERD-adherent Hospitalization on Abortion Provision and Complications by Hospital Concentration using birth count (rather than admissions)

	(1) High concentration (Rural)	(2) Low concentration (Urban)
Panel A. Induced Abortion		
ERD-adherent	0.00164	-0.0231***
	(0.00215)	(0.00492)
Mean dep. var.	0.0055	0.0050
Panel B. Spontaneous Abortion (Miscarriage)		
ERD-adherent	-0.00549	-0.000547
	(0.00910)	(0.00906)
Mean dep. var.	0.0271	0.0186
Panel C. General Abortion-Related Complication		
ERD-adherent	0.000378	0.00410
	(0.00369)	(0.00321)
Mean dep. var.	0.0094	0.0080
Panel D. Induced Abortion Complication		
ERD-adherent	0.000162	-0.00679***
	(0.00135)	(0.00176)
Mean dep. var.	0.0028	0.0024
Observations	420,718	631,027
First stage F-stat	30.02	35.11

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. ERD, Ethical and Religious Directives. Coefficients report the Local Average Treatment Effect (LATE) from the Two-Stage Least Squares (2SLS) model. Rural hospitals are those above the average level of concentration, indicating fewer alternatives for care and less opportunity to transfer patients in a timely manner. Urban hospitals are below the average. In this robustness check, we measure concentration with the the Herfindahl-Hirschman Index based on the hospital service area and number of births (rather than number of admissions in our main results.)

Table 15: The Effect of Catholic Hospitalization on Abortion Provision and Complications Across Four Robustness Checks

	(1) 2SLS (Main)	(2) 2SLS (Catholic-affiliated)	(3) 2SLS (Untrimmed DD)	(4) 2SLS (LASSO)	(5) FIML (Bivariate probit)
Panel A. Induced Abortion					
Catholic	-0.00933**	-0.00877**	-0.00965***	-0.01065***	-0.00464***
	(0.00372)	(0.00349)	(0.00346)	(0.00401)	(0.00076)
Mean dep. var.	0.0052	0.0052	0.0052	0.0052	0.0052
Panel B. Spontaneous Abortion (Miscarriage)					
Catholic	-0.00505	-0.00474	-0.00234	0.00449	0.01016
	(0.00646)	(0.00605)	(0.00637)	(0.00914)	(0.00719)
Mean dep. var.	0.0220	0.0220	0.0219	0.0220	0.0220
Panel C. General Abortion-Related Complication					
Catholic	0.00138	0.00130	0.00213	0.00517**	0.00254
	(0.00231)	(0.00216)	(0.00223)	(0.00252)	(0.00253)
Mean dep. var.	0.0085	0.0085	0.0085	0.0085	0.0085
Panel D. Induced Abortion Complication					
Catholic	-0.00285**	-0.00267**	-0.00303**	-0.00271**	-0.00186***
	(0.00136)	(0.00128)	(0.00127)	(0.00139)	(0.000482)
Mean dep. var.	0.0026	0.0026	0.0026	0.0026	0.0026
Observations	1,051,745	1,051,745	1,062,267	1,051,745	1,051,745
First stage F-stat	67.60	69.78	63.77	32.71	

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by patient county in parentheses. Column (1) reports results from our main specification, see Table 18. Column (2) uses a broader Catholic-affiliated treatment definition rather than the strict ERD-adherent definition. Column (3) includes observations in the top 1% of differential distance (DD), which were dropped in the main analysis to ensure common support. Column (4) reports results using covariates selected with a machine learning LASSO algorithm (Appendix F provides details about this model). Column (5) reports results using an endogenous probit model via full information maximum likelihood (FIML).