Delivering Amidst Consolidation: Implications of Hospital Mergers on Newborn and Maternal Outcomes in the US Medicaid Program

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Abstract

Hospital mergers raise private health insurance prices, widening the gap between private and public payments. We use national Medicaid claims and natality data to study how within-market hospital mergers affect labor and delivery (L&D) care access and outcomes in Medicaid. After mergers, birth admissions in target hospitals decline by 37%. This effect is amplified among hospitals in more concentrated markets and in rural counties. Though there is suggestive evidence that the rate of Cesarean deliveries without a medical indication increases in merged hospitals, maternal complication rates do not worsen. At the county level, newborn outcomes improve in areas with acquirer hospitals. Overall, hospital mergers reduce hospital options for Medicaid beneficiaries while yielding modest improvements in birth outcomes for those who receive L&D care. (*JEL* I11, I18, L13)

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1. Introduction

The US hospital industry has rapidly consolidated in the past two decades, with more than 1,600 hospital mergers since 2000 (Gaynor, 2021). These mergers and acquisitions (M&As) have led to a US hospital market environment where over 90% of metropolitan statistical areas (MSAs) have "highly-concentrated" hospital markets (Fulton, 2017). Along with this trend of increasing hospital market concentration, there has been a rise in maternity ward closures and related reductions in labor and delivery (L&D) service availability, a relatively higher proportion of which have occurred in recently merged hospital systems.[1] Despite extensive research on hospital mergers and their impact on market power and prices, little is known about how consolidation interacts with the tiered US health care financing system, particularly how it shapes hospitals' incentives to treat the approximately 80 million lower-margin Medicaid beneficiaries in the United States. There is also little focus on understanding how target and acquirer firms might differentially impact their consumers and broader markets. A particularly salient setting for examining this question is L&D services. Despite approximately 1.5 million annual births in the US being financed by Medicaid (over 40% of all US births) the access and quality effects of hospital consolidation on this population remain understudied (Desai et al., 2023). In this paper, we examine the implications of hospital consolidation for mothers with Medicaid who seek L&D services.

In addition to increasing their market power with private insurers, hospitals may alter their service mix after a merger by restricting access for Medicaid beneficiaries, since private health insurance (PHI) reimbursements are much more lucrative than Medicaid inpatient reimbursements. The average inpatient L&D reimbursement is more than $15,000 for births covered by PHI and only about $6400 for Medicaid births (Valencia et al., 2022). Additionally, there is wide variation in PHI childbirth reimbursements between geographical regions, ranging from just above $10,000

[1] See Table 1. In the descriptive sample, target hospital-years are more likely than never-treated hospital-years to drop to zero births (9.5% vs. 3.9%; two-sided Welch test: $t = 4.18$, $p < 0.001$). For example, in April of 2020, following a merger in February of 2018 (Ballad Health, 2018), Ballad Health relocated its L&D services in Holston Valley Medical Center to a different facility within its hospital system, aligning "with longer-term preliminary recommendations by the Visioning Committee in Kingsport to enhance service offerings for women and newborns" (Ballad Health, 2020).

in Arkansas to more than $21,000 in California and Alaska (Valencia et al., 2022). Medicaid reimbursements range from about $3000 in Florida to approximately $11,000 in Washington D.C. (Valencia et al., 2022).[2] In some states, average childbirth payments under PHI can be more than five times those under Medicaid. Furthermore, the estimated hospital input costs of prenatal care often exceed the revenue generated from Medicaid reimbursements (Baker et al., 2021).

When hospitals merge, the resulting hospital system can leverage its increased bargaining and monopsony power to negotiate higher reimbursement rates from PHI firms (Cooper et al., 2019; Gaynor, 2021; Gowrisankaran et al., 2015), reduce various medical supply costs (Craig et al., 2021), and lower labor (Andreyeva et al., 2024; Arnold & Whaley, 2024; Prager & Schmitt, 2021) and financing costs (Andreyeva et al., 2024). Because there is no corresponding increase in Medicaid rates, the average price difference between insurance types can increase. With control over more facilities, a system can reallocate and centralize L&D services in better-resourced hospitals while closing relatively unprofitable obstetric units (Arnold et al., 2025; Dranove et al., 2025). Such restructuring can reduce the local supply of L&D care in markets where obstetric capacity is already declining (Kozhimannil et al., 2025). This consolidation process reduces the system's exposure to unprofitable service lines and directs mothers affected by these supply reductions to local market competitors (Desai et al., 2023, 2025; Dranove et al., 2025) or to facilities within the system that may require mothers to travel longer distances for care (Ballad Health, 2020). Overall, this behavior could result in less L&D volume and more highly reimbursed care provision at consolidated systems. These access impacts may be particularly acute among Medicaid beneficiaries (Desai et al., 2023, 2025). Yet, national-level empirical estimates of these effects are not known.

The overall impact of hospital mergers on health outcomes is ambiguous. Despite the potential prioritization of economic incentives in hospital system mergers, such mergers may also lead to the integration of clinical protocols, more robust staffing models, and increases in care coordination and efficiency (J. R. Clark & Huckman, 2012), yielding health benefits for patients. Mothers with Medicaid who are admitted to consolidated systems can receive higher-quality care due to the

[2]This range excludes the Medicaid payment for New York state, which is a clear outlier from the $3000 to $11,000 range at $18,000 per New York Medicaid childbirth visit (Valencia et al., 2022).

integration of protocols and reduction in hospital birth volume (Fischer et al., 2024), allowing for more resources per admitted mother. On the other hand, acquirer systems may implement more profitable care practices, leading to care that prioritizes system profit over maternity care services (Dranove et al., 2025). This paper addresses this ambiguity through an empirical analysis of hospital behavior and its effects on Medicaid birth outcomes. Our study provides a comprehensive national-level examination of the direct impacts of hospital mergers on L&D hospital care access, maternal and infant health outcomes, and geographic-level access and health effects for Medicaid beneficiaries.

We first analyze the national 2016-2021 Transformed Medicaid Statistical Information System Analytic files (Centers for Medicare & Medicaid Services, 2026) (hereafter, Medicaid TAF files), which are standardized data files for Medicaid, the primary source of publicly funded health insurance coverage for low-income individuals and families in the United States. With these data, we examine how hospital mergers impact hospital L&D access (e.g., L&D visit volume), birth procedures (e.g., C-sections without medical indication and induced labor), and maternal health outcomes during L&D visits. To do so, we first identify hospitals involved in a merger with another hospital or hospital system within the same hospital referral region (HRR) as defined by the Dartmouth Atlas Project (2024). We then separately examine the effects for the target hospitals and the acquirer hospitals to analyze possible differences in hospital behavior and birth outcomes depending on which side of the deal the hospital is on. We use doubly-robust staggered difference-in-differences models suggested by Callaway and Sant'Anna (2021). Our main treatment variable is the first instance of a within-HRR hospital merger.

We find that within-HRR target hospitals experience a statistically significant decline in Medicaid L&D volume after merger exposure. The post-treatment ATT implies approximately a 36.8% reduction in Medicaid births, equivalent to roughly 95 fewer Medicaid births from a pre-target mean of 257 births per hospital-year. This substantial effect is driven by hospitals operating in rural areas, consistent with prior literature examining rural maternity ward closures (Battaglia, 2025; Dranove et al., 2025; Fischer et al., 2024). In contrast, within-HRR acquirer hospitals experience a smaller

and statistically insignificant decline in Medicaid L&D volume. The corresponding estimate implies approximately a 17.6% reduction, or roughly 66 fewer Medicaid births from a pre-acquirer mean of 377 births per hospital-year. The combined target and acquirer effect (the "combined" or "consolidation" effect) implies approximately an 18.4% reduction in Medicaid births, equivalent to roughly 60 fewer Medicaid births from a pre-consolidation mean of 328 births per hospital-year.

We also find that hospital mergers in markets with HHI above the median at the beginning of our study period are associated with statistically significant reductions in both L&D volume and the number of unique admitting providers across all treatments. Hospital mergers in markets with HHI below the median at the beginning of our study period yield no statistically significant results. This result aligns with that of Prager and Schmitt (2021), who find that hospitals exercise their monopsony power after a merger in a highly concentrated market.

Turning to delivery intensity and maternal outcomes, we find limited, nonsignificant evidence that merger exposure worsens clinical care on average. Target hospitals experience a 1.50 percentage point increase in C-sections without medical indications, while the analogous estimates for acquirer hospitals and the combined consolidation group are smaller: 0.85 percentage points and 0.90 percentage points, respectively. We also find no statistically significant evidence that merger exposure increases maternal complications. Overall, our results suggest that the primary post-merger change is a reduction in Medicaid L&D volume concentrated among target hospitals, suggestive evidence of increases in C-sections without medical indications in target hospitals, and no statistically significant evidence of increases in maternal complications.

Because we find statistically significant reductions in L&D volume in target hospitals, in conjunction with these examples, we next analyze local market equilibrium adjustments by examining acquirers, using 2016-2022 birth certificate-derived data from the National Vital Statistics System (NVSS), maintained by the National Center for Health Statistics (National Center for Health Statistics, 2024). We analyze births covered by all insurance types and births covered by Medicaid. Our outcomes include the likelihood of a birth within a mother's county of residence ("same-county births") and infant Apgar scores (a standard measure of newborn health). We first identify counties

where at least one hospital is a within-HRR acquirer. We then link these hospitals to the mothers' counties of residence. We compare mothers' counties of residence that experience at least one local hospital acquirer to those with no hospital M&A activity to examine how ownership changes affect local L&D market dynamics.

For births covered by Medicaid, we find that counties with acquirer hospitals increase the likelihood of mothers giving birth within their county of residence, providing evidence of more consolidated L&D facilities where acquirer hospitals operate. We find decreases in the likelihood of C-section births and find no detrimental impacts on five-minute Apgar scores during the L&D visit. When coupled with our Medicaid claims-based analysis, we find evidence of access restrictions in consolidated hospitals but no detrimental impacts on birth quality for births exposed to local acquirer hospitals. Hence, overall, we find that Hospital mergers restructure L&D markets through reductions in L&D admissions driven by cost-cutting and efficiency gains by hospital systems.

The relatively lower profitability of L&D has already led to the closure of maternity wards in the US (Battaglia, 2025; Carroll et al., 2022; Hwang et al., 2024; Sonenberg & Mason, 2023), suggesting a strategy to restrict access to labor and delivery services for patients residing in areas with already-low levels of medical care access. This potential strategic shift is of particular concern due to the high levels of post-natal mortality in the United States that is driven by poor outcomes among individuals from lower socioeconomic statuses (Chen et al., 2016). As hospitals continue to consolidate, a margin to consider is the potential reduction in care provided by consolidated hospitals to mothers covered by Medicaid, relative to those with PHI.

Another key aspect of this framework is how multiple facilities distribute care. Given that more than 99% of births in our NVSS sample occur in a hospital setting, a decline in L&D services at one hospital implies that another local hospital will likely absorb displaced mothers. Thus, a potential mechanism linking hospital merger effects with birth outcomes is the change in the allocation of births within a local hospital market. A reallocation of births within markets could lead to changes in birth quality, especially if these reallocations divert mothers to better or worse hospitals (Dranove et al., 2025; Fischer et al., 2024).

We contribute the first comprehensive national-level analysis examining the direct impact of horizontal hospital mergers on the largest and one of the most vulnerable populations of mothers and newborns. Our analysis complements the growing literature on the impact of hospital ownership consolidation on access to care and care quality (Beaulieu et al., 2020; Desai et al., 2023; Hayford, 2012; Ho & Hamilton, 2000; Jiao, 2025). We also contribute to the literature on labor and delivery markets (Battaglia, 2025; Carroll et al., 2022; Dranove et al., 2025; Fischer et al., 2024). Although Battaglia (2025) does not specifically examine hospital M&As, they find evidence that maternity ward closures lead to further optimization of care delivery across L&D markets through the consolidation of L&D care within fewer facilities that employ more specialized and higher-quality care providers (Battaglia, 2025; J. R. Clark & Huckman, 2012; Fischer et al., 2024). Furthermore, recent findings by Dranove et al. (2025) show that rural counties with an acquired hospital are more likely to stop recording births after a merger. These findings suggest the existence of supply restrictions driven by new management strategies in rural settings (Dranove et al., 2025; Fischer et al., 2024).

There is currently no comprehensive medical claims-based national evidence that examines the behavior of both target and acquirer hospitals in their treatment of Medicaid beneficiaries. There is also no national evidence using birth certificate data on how hospital acquirers differentially impact market access and health outcome equilibria for mothers with Medicaid. Our findings provide new hospital- and market-level evidence on the impacts of hospital mergers on patients with low income. It also underscores the urgency with which hospital systems, health policy agencies, and antitrust agencies should consider the potential strains on health equity and hospital safety nets within rapidly consolidating hospital markets.

The rest of this paper is organized as follows: Section 2 describes the data and methods. Sections 3 and 4 discuss the results. Section 5 concludes.

2. Data and Descriptive Statistics

This section provides a brief overview of the key variables and data sources we use in our study. We implement two related sets of analyses: our first set examines the direct impact of hospital mergers using Medicaid claims data, and our second set examines post-merger acquirer-influenced equilibrium adjustments using the NVSS vital statistics data. Appendix Table A1 briefly summarizes each of the datasets that we use for this study.

Hospital Level Analysis with Medicaid Claims Data

We analyze Medicaid TAF files from 2016 to 2021 (Centers for Medicare & Medicaid Services, 2026). These are the official national standardized data files for Medicaid, the primary source of health insurance coverage for low-income populations in the United States. Our Medicaid TAF files do not contain predefined hospital facility identifiers, such as the American Hospital Association (AHA) Hospital ID or CMS Certification Number (CCN). Instead, they provide national provider identification (NPI) numbers for both the billing provider and the admitting provider. Hence, we use the National Bureau of Economic Research (NBER) NPI/CCN Crosswalk File (National Bureau of Economic Research, 2017) to link the billing NPI numbers on the TAF files to hospital facility CCNs.

To examine the potential contraction or expansion of L&D care access after mergers, we first analyze the following outcomes at the hospital/year level: volume of Medicaid births, number of hospital beds, and number of unique admitting L&D providers. We then examine the downstream impacts on C-sections. C-section rates are a measure of care quality used by the Centers for Medicare & Medicaid Services (CMS). Although CMS considers lower C-section rates a better quality outcome (Lotz et al., 2022), it is difficult to conclude the validity of this evaluation. We thus use various medical codes in the claims data to focus on both overall C-sections and C-sections without medical indication to analyze whether hospitals perform higher-intensity procedures after a merger. Because assessing the true medical necessity of C-sections is challenging, especially

when hospitals are incentivized to code patients as high-risk to obtain higher reimbursements (Crespin et al., 2024; Dafny, 2005), we study changes in all C-sections and C-sections without medical indication rates in the context of shifts in induced-labor rates, an intervention associated with substantial resource use and heightened clinical oversight (S. L. Clark et al., 2009), and in the context of their relationship with L&D care quality. Please see Appendix Table A2 to see our algorithm for identifying births, C-sections, C-sections without medical indication, induced labor, and maternal complications in the Medicaid TAF files.

Inpatient Claims

With Medicaid TAF data, we first identify inpatient visits that resulted in births. We then collapse the birth claims to the hospital-year level, resulting in a dataset of Medicaid birth volume per facility. We then take the log of this variable (plus one) for our volume analysis. To test the impact of hospital mergers on hospital L&D capacity, we take the log of hospital beds and the log of the number of unique admitting providers (plus one) per hospital facility. The latter two variables, while imperfect metrics, provide clues about how hospitals might change their personnel and care management capacity following a merger.

We restrict the analytical sample to general acute care hospitals that recorded at least 12 births (1 birth per month) during 2016, the first year of the study period. We make this restriction to ensure that we are comparing effects among hospitals that have meaningfully provided L&D care at the start of the study period.

Other factors could influence our estimated volume and capacity effects, including policy shocks such as the late Medicaid expansion from 2016 to 2021. Given the magnitude and significance of our volume and capacity target effects, we perform robustness checks to ensure that we can credibly claim these are a consequence of hospital mergers, not Medicaid expansion. For this analysis, we exclude hospitals from states that expanded Medicaid between 2016 and 2021: Maine, Montana, Louisiana, Virginia, Idaho, Utah, Nebraska, Oklahoma, and Missouri. We then run the same empirical models as above on this abridged sample. Another potential concern is that never-

treated hospitals located in counties with merger activity may themselves be affected by nearby consolidation events, violating the stable unit treatment value assumption (SUTVA). To assess this possibility, we re-estimate our hospital-year analysis after excluding never-treated hospitals from counties where a merger occurred.

For our analysis of birth outcomes, we maintain our data at the birth-admission level and examine the impact of hospital mergers on C-section rates (overall and without medical indication), induced-labor rates, and maternal complication rates. All of our birth outcome variables are binary. Our maternal complication variable is defined as whether a mother experiences at least one health complication as defined by the World Health Organization (2019) and AAPC (2025) guidelines. Please see Appendix Table A2 to review the ICD-10 codes used to identify these complications. The ICD-10 coding guidelines provided by World Health Organization (2019) and AAPC (2025) feature a broadly inclusive set of codes that span birth timing problems (e.g., preterm labor, prolonged labor), mechanics of labor (e.g., obstructed labor, malposition/malpresentation), intrapartum issues (e.g., abnormal uterine contractions, fetal distress, cord problems), and delivery events/complications (e.g., certain delivery complications and related conditions). We apply this broad definition of maternal complication to minimize the potential impact of strategic alterations to coding intensity that could otherwise disproportionately affect rates of narrowly defined complications.

While we analyze overall hospital-level birth volume to study L&D activity within hospitals, we restrict our analysis of birth outcomes to singleton births to reduce clinical heterogeneity and avoid estimating changes in multiple-gestation case mixes that could confound treatment effects. We implement individual-level controls for binned maternal age (24 and younger, 25-34, 35 and older) and the number of Elixhauser comorbidities for each mother. We also implement county-level controls for the unemployment rate in 2016 (Bureau of Labor Statistics, 2016), log median household income in 2016 (U.S. Census Bureau, 2016), and the percentage of individuals with at least a bachelor's degree in 2016 (OpenIntro, 2016).

Treatment Groups: Hospital-Level Merger Identification

We identify treatment at the hospital level, classifying each hospital as a target, acquirer, or part of the combined (or consolidated) treatment group following a merger. Treated hospitals are observed for up to four pre-treatment periods and three post-treatment periods (including the first treatment period, "0").

A target hospital is defined as a hospital that becomes newly affiliated with an acquiring system within the same HRR during a given year (i.e., it experiences a change in its system identifier from independent or another system to the acquirer's system). An acquirer hospital is a hospital that remains part of its original system but gains at least one new target hospital within its HRR during a given year. The combined group aggregates both within-HRR acquirer and target hospitals to capture the overall consolidation effect. Hospitals that do not experience changes in system affiliation during the study period (2016–2021) serve as never-treated controls.

To identify consolidation events, we use hospital-level data from the American Hospital Association (AHA) Annual Survey and the data provided by Cooper et al. (2019). Because the Cooper et al. (2019) dataset only extends through 2014, we emulate their annual identification procedure for 2016–2022, following the implementation in Arnold and Whaley (2024) and Arnold et al. (2025). Specifically, we track changes in each hospital's system identifier over time and flag a merger event whenever a hospital transitions to a different system. We implement three treatment specifications: (1) Hospitals as within-HRR targets; (2) hospitals as within-HRR acquirers; and (3) Combined (or Consolidated) effects of (1) and (2).

Our hospital target model (M1) examines the impact of being a hospital targeted by an acquiring system within the same hospital referral region (HRR) on our outcomes of interest. In this analysis, we exclude all hospitals that are part of a within-HRR acquiring system during our study period. We compare within-HRR target hospitals with never-treated hospitals, allowing us a more granular examination of the impacts of hospital mergers.

Our hospital acquirer model (M2) examines the impact of being on the acquiring side of a merger deal with a target hospital within the same HRR. In this analysis, we exclude all hospitals that are

within-HRR targets. We compare within-HRR acquirer hospitals with never-treated hospitals.

Our combined treatment model (M3) considers both within-HRR target and within-HRR acquirer hospitals as treated and having encountered a within-HRR consolidation event. We keep all hospitals in this sample. Figure 1 presents the spatial distribution and frequency of hospital merger activity across HRRs in our hospital sample from 2016 to 2021. To illustrate the difference in frequencies across types of consolidation events, Figure 2 shows the annual counts of within-HRR target, cross-HRR target, within-HRR acquirer, and cross-HRR acquirer hospitals. Cross-HRR consolidation events involve merger activity between hospitals in different HRRs. We specifically analyze within-HRR treatments in our empirical models to understand the impact of merger activity that can meaningfully shift the distribution of local market power. Because hospital mergers differ widely in timing and deal execution approaches, we expect substantial year-to-year variability in the frequency of within-HRR consolidation events.

Matching Strategy: 5-Nearest Neighbors

Because treatment and control hospitals differ substantially in several characteristics (please see Table 1 for pre-match descriptive statistics), we employ a 5-Nearest Neighbor (5-NN) matching strategy to compare treatment and control hospitals that are more comparable and likely to exhibit parallel trends in outcomes before treatment. Using probit models, we estimate the predicted probability of each hospital's involvement in a merger during our study period. Our predictor variables are not-for-profit status, county rurality, and the following values in 2016 for: baseline log Medicaid births, baseline log hospital beds, HRR Herfindahl-Hirschman Indices (HHI), log county median household income, county rate of bachelor degree holders, and county unemployment rate.

Table 2 displays the descriptive statistics of each treatment group after implementing our matching procedure. Although matching improves covariate balance between treated and comparison hospitals, the treatment groups still differ. Our identification strategy thus relies on the parallel trends assumption and we supplement our main estimates with the Rambachan and Roth Honest-DiD sensitivity analysis (Rambachan & Roth, 2023). This test uses the conventional parallel-trends

estimate as a benchmark and evaluates how robust our conclusions are to bounded deviations from parallel trends. Specifically, we assess how large deviations from parallel trends in the post-treatment period could be, relative to the largest observed pre-treatment deviation, before our substantive conclusions would no longer hold.

Heterogeneity & Mechanism Tests

To test potential mechanisms that drive our effects, we examine whether the volume and capacity effects vary across three dimensions of heterogeneity: market concentration, not-for-profit status, and geographic setting. First, we test whether merger effects differ between hospitals located in more versus less concentrated markets. Mergers in already concentrated markets may move hospital systems closer to monopoly/monopsony-level control, potentially creating incentives for strategic service reductions that differ from mergers occurring in more competitive markets. To assess this possibility, we calculate HHIs at the HRR level in 2016 and split the hospital sample at the median HHI. We then estimate our empirical models separately for hospitals located in HRRs above the 2016 median HHI and hospitals located in HRRs at or below the median.

In a similar vein, we test whether not-for-profit hospitals yield treatment effects that are different from our main effects. We use hospital control designations provided by the Medicare Provider of Service files ("Provider of Services File: Hospital/Non-Hospital Facilities", 2023). We examine this dimension of heterogeneity to assess whether there is evidence of not-for-profit hospitals (over 80% of our sample) behaving differently from for-profit hospitals. For example, given the negative margins associated with the average Medicaid L&D visit, if there were differences in behavior by ownership type, we would expect not-for-profit hospitals to show more muted effects.

Finally, we test hospitals in urban and rural subsamples to assess whether effects differ by geographic setting. We use urban/rural county designations provided by the United States Department of Agriculture's Economic Research Service (U.S. Department of Agriculture, Economic Research Service, 2025) to aid in this heterogeneity test. This also serves as a mechanism test because of the different market contexts presented by urban and rural settings. Multiple instances

of maternity ward closures in rural settings are well documented in prior literature (Dranove et al., 2025; Durrance et al., 2023; Fischer et al., 2024; Sager, 2026) and in recent media reports (Muoio, 2025; Olsen, 2025; Sager, 2026). Examining potential differential effects using Medicaid claims data gives us the advantage of also testing for health effects in addition to access effects.

County Level Acquirer Analysis

Based on our hospital-level findings, acquirer hospitals influence most of the care management changes after a merger. To study within-county equilibrium adjustments of Medicaid births influenced by acquirer hospitals, we use 2016-2022 data from the National Center for Health Statistics Restricted National Vital Statistics System (NVSS) Database, which provides comprehensive birth certificate-derived data on birth outcomes in the United States (National Center for Health Statistics, 2024). The data include detailed information such as five-minute Apgar scores (a standard metric for infant health immediately after birth) and the county of the birth facility. Although the NVSS's privacy and deidentification procedures do not allow for the identification of the facilities where each birth occurs, the data files contain the most comprehensive set of certified information on individual births and various birth outcomes during delivery.

Key variables

The first outcome we examine is the number of Medicaid births per 1,000 women at the county level. We begin with a county-year-level analysis to test whether there is a change in the extensive margin for births in counties with at least one acquirer hospital.

Next, we perform analyses at the birth level. We test the county-level impact of hospital acquirers on access to L&D care with the following outcome variables: prenatal visit volume (continuous) and same-county birth (binary; defined as a birth that occurred in the same county as the mother's residence county). Our health outcome variables include 5-minute Apgar scores (a standardized assessment of health status immediately after birth, scored from 0-10 with 10 being healthiest) and C-section birth (binary).

Our control variables are binned versions of maternal age (19 and younger; 20-34; 35-39; 40 and older) and total birth order (first birth; second birth; third or fourth births; fifth birth or later).

Treatment Groups: County-Level Acquirer Exposure

We define two comparison groups. Our treatment group consists of births in counties with a hospital that acquired a target hospital within the same HRR. Our control group consists of births in counties where there is neither a within-HRR target hospital nor a within-HRR acquirer hospital (never-treated counties). Figure 3 provides a geographic visual for the frequency of hospital acquirer activity in our NVSS sample from 2016 to 2022. For treated counties, we restrict the analysis to observations within an event-time window of up to five pre-treatment periods and four post-treatment periods (including the treatment period "0"). We exclude counties containing a within-HRR target hospital to isolate the effect of acquirer exposure. Figure 4 shows the annual counts of counties that gain exposure to an acquirer hospital, distinguished by whether the hospitals made a within-HRR or cross-HRR acquisition. We specifically analyze within-HRR acquirer exposure in our empirical models to understand the impact of merger activity that can meaningfully shift the distribution of local market power.

Table 5 provides the descriptive statistics for never-treated counties and counties with acquirers. We observe that, besides same-county birth rates, prenatal visit volume, and maternal transfers, most variables show balance across each treatment group.

Empirical Methods

We estimate dynamic treatment effects using the doubly-robust staggered-adoption difference-in-differences estimator of Callaway and Sant'Anna (2021). This estimator accommodates heterogeneous treatment effects across hospitals and over time, avoiding biases that can arise in conventional two-way fixed-effects models when treatment timing differs across units (Callaway & Sant'Anna, 2021; Rios-Avila et al., 2021; Roth et al., 2023). We define each hospital's treatment cohort according to the first year it becomes part of a merged system and estimate cohort-specific average

treatment effects for each post-treatment period. Formally, let $G_i = g$ denote the first year in which hospital i is exposed to a merger, and let $G_i = 0$ denote hospitals that are never treated during the study period. For each treated cohort g, we define $Y_{it}(g)$ as the potential outcome for hospital i in year t if first treated in year g, and $Y_{it}(0)$ as the potential outcome under no merger exposure. Hence, our group-time average treatment effect on the treated is:

$$ATT(g,t) = E\left[Y_{it}(g) - Y_{it}(0) \mid G_i = g\right], \qquad t \geq g. \tag{1}$$

We write the observed outcome as the untreated potential outcome plus the treatment effect realized by treated hospitals in their post-treatment years:

$$Y_{it} = Y_{it}(0) + \sum_{g \in \mathcal{G}} \mathbf{1}\{G_i = g\}\, \mathbf{1}\{t \geq g\}\left[Y_{it}(g) - Y_{it}(0)\right]. \tag{2}$$

We then aggregate our cohort-specific estimates into event-time effects, weighting cohorts by their relative size. Let $e = t - g$ denote event time. Our event-study estimand is:

$$\theta(e) = \sum_{g \in \mathcal{G}_e} w_g(e)\, ATT(g, g+e), \tag{3}$$

where $\mathcal{G}_e$ is the set of treatment cohorts observed at event time e, and $w_g(e)$ are cohort-size weights that sum to one. Our aggregated effects summarize the average post-merger change in outcomes among treated hospitals relative to the untreated potential outcome. Our effect identification comes from comparing outcomes at hospitals that have experienced a merger with outcomes at hospitals that are either not yet treated or never treated during the study period, depending on the specification.

We perform similar analyses for our county-level treatment models, except our Callaway and Sant'Anna (2021) estimators provide different interpretations from our hospital-level treatment models. First, our treatment group consists of births that occur in mothers' residence counties that are exposed to at least one hospital that acquires another hospital within the same HRR. This means that we are estimating how exposure to an acquirer hospital within a specific geographic area can impact where births occur and the outcomes of those births, instead of estimating the

direct impact of a hospital merger itself. We are thus examining the reduced-form equilibrium adjustments influenced by acquirer hospitals on births within markets.

We first test whether mothers' residence county exposure to at least one acquirer hospital shifts births across counties. This test assesses whether the hospital-level volume changes we find are attributable to county population shifts. Once we establish stability in county Medicaid births after exposure to an acquirer hospital, we can more plausibly attribute changes in our other outcomes to hospital behavioral responses to consolidation, rather than shifts in the underlying population of mothers giving birth.

For valid effect identification, there must be no anticipation effects nor differential conditional pre-trends before treatment. Oh et al. (2025) find that a substantial proportion of hospital merger announcements occur at least a year before deal completion. Because hospitals and health systems may begin adjusting operations after a merger is announced but before the recorded completion date, event-study estimates for the period immediately preceding treatment may partly reflect anticipatory behavior rather than pre-existing trends. To avoid conflating these anticipatory responses with the parallel trends assumption, we exclude the year immediately preceding treatment from the event-study reference period.[3] This choice changes the normalization of the event-study estimates rather than eliminating anticipatory behavior, so we interpret coefficients near the treatment date with caution. For our claims-based treatment models, we compare treated versus never-treated hospitals. Callaway and Sant'Anna (2021) difference-in-differences estimators allow for a "not-yet-treated" estimator option, which, in addition to never-treated hospitals, includes hospitals that have not yet been treated by a given year (but may be treated later), and are excluded from the control group once they become treated. We forgo the "not yet treated" option for our hospital-level treatment models due to the risk of later-treated hospitals being affected by the strategic decisions of nearby earlier-treated hospitals within similar markets.[4] We allow for "not yet treated" estimation for our

[3]We estimate our dynamic treatment effects using the `csdid` Stata package (Rios-Avila et al., 2021). Following Callaway and Sant'Anna (2021), we implement the `long2` option, which establishes a universal baseline using the period immediately preceding treatment ($t = -1$, or $g - 1$) as the constant reference period. This standardizes the pre-trends relative to a single fixed anchor, aligning the output with traditional event-study visualizations where the coefficient at $t = -1$ is normalized to zero.

[4]Activating the "not yet treated" option yielded near-identical results. Please see Table A3.

birth certificate-based county treatment models to maximize identification power using later-treated counties as valid comparators before their own treatment. This increases pre-treatment support and precision. We also assess conditional parallel trends in each of our models. A hospital's decision to merge is non-random and endogenous to hospital-, market-, and population-specific factors. Because treatment assignment is non-random, covariate means are likely to differ between treated and control groups. We therefore assume that, conditional on our covariates, if outcomes follow parallel trends between these treatment groups before treatment, post-treatment divergence can be attributed to the treatment itself. The visible evidence for conditional parallel trends in our statistically significant event study graphs validates our estimation procedures.

3. Hospital-Level Results

We now present the results of our empirical analysis. We summarize aggregated post-merger effect estimates in Tables 6 through 9 and show the dynamic event-study graphs for our main results below.

Effect on Hospital L&D Capacity

We first discuss the impact of hospital mergers on hospital Medicaid birth volume and whether there are potential shifts in hospital care management strategies that are specific to L&D care.

Effect on Hospital L&D Volume

Hospitals exposed to a merger during the study period, as either a target or acquirer, had an average of 328 Medicaid L&D births before their merger. We find that consolidated hospitals decreased their birth volume by approximately 18.4%, equivalent to roughly 60 fewer births per hospital-year after their mergers (Table 6). This combined effect is driven primarily by the target-hospital effect as shown in Figure 5. Target hospitals had an average of 257 Medicaid births before their merger and experienced a statistically significant 36.8% decline, equivalent to roughly 95 fewer Medicaid

births per hospital-year. This finding is consistent with prior literature documenting maternity ward closures among target hospitals and in rural settings following hospital mergers (Dranove et al., 2025). In contrast, acquirer hospitals had an average of 377 Medicaid births before their mergers and experienced a smaller, statistically insignificant 17.6% decline, equivalent to roughly 66 fewer Medicaid births per hospital-year. The event-study plots in Figure 5 show that the decline in L&D volume is most pronounced for target hospitals, while the acquirer effect is smaller and not statistically significant. Consistent with this, 6.1% of target hospitals in our matched sample permanently dropped to zero births, our proxy for maternity ward closure, during the study period, compared with 3.8% of non-target hospitals, contributing to the effect. As discussed further below, the decline in Medicaid L&D volume among target hospitals is driven primarily by rural hospitals and hospitals located in more concentrated markets. Because this is our core access result, we assess its sensitivity to violations of parallel trends using the Rambachan and Roth (2023) HonestDiD sensitivity analysis for the target and combined effects, as shown in Appendix Figure A1. In each panel, we report robust confidence sets that allow period-over-period post-treatment violations of parallel trends to be up to $\bar{M}$ times the maximum corresponding violation observed in the pretreatment period, for $\bar{M}$ ranging from 0 to 0.5. At $\bar{M} = 0$, both confidence sets exclude zero. The negative target effect remains statistically distinguishable from zero when post-treatment violations are allowed to be up to approximately 35 percent as large as the maximum pretreatment violation ($\bar{M} \approx 0.35$), while the combined effect has a breakdown value of approximately $\bar{M} = 0.2$. Thus, although the event-study estimates provide no statistically detectable evidence of differential pretreatment trends, the sensitivity analysis further shows that our substantive conclusions remain robust to modest departures from parallel trends.

Effect on Hospital Bed Capacity

We find little evidence that merger exposure reduces overall hospital bed capacity (Table 6 and Figure 6). Target hospitals experience a statistically insignificant 3.2% decline from a pre-target mean of 220 beds, equivalent to roughly 7 fewer beds after merger exposure. Acquirer hospitals

show essentially no change in bed capacity: the estimated effect is a statistically insignificant 0.5% decline, equivalent to roughly two fewer beds from a pre-acquirer mean of 310 beds. The combined consolidation estimate is similarly small and statistically insignificant, corresponding to approximately a 1.7% decline in bed capacity, or roughly five fewer beds from a pre-consolidation mean of 273 beds. Total hospital beds are an imperfect measure of L&D-specific physical capacity, however, so these estimates do not rule out reductions in maternity-specific capacity implied by the sizable decline in birth volume among target hospitals.

Effect on Unique Admitting NPIs

We measure L&D staffing capacity using the number of unique admitting National Provider Identifiers (NPIs) observed at each facility (Table 6 and Figure 7). The estimates show declines in unique admitting NPIs across target, acquirer, and consolidated hospitals. Target hospitals experience a statistically significant 14.6% decline in unique admitting NPIs, equivalent to roughly three fewer admitting providers from a pre-target mean of approximately 17 providers. Acquirer hospitals experience a statistically significant 11.8% decline, equivalent to roughly three fewer admitting providers from a pre-acquirer mean of approximately 27 providers. The combined consolidation effect is a statistically significant 10.4% decline, equivalent to roughly two fewer providers from a pre-consolidation mean of approximately 23 providers. Overall, our results indicate that the most robust post-merger change is a decline in Medicaid L&D volume among target hospitals, accompanied by reductions in L&D admitting-provider capacity, while overall bed capacity shows little change.

Robustness and Heterogeneity Test Results: Access Outcomes

Our heterogeneity analyses suggest that post-merger reductions in Medicaid L&D capacity are concentrated in hospital markets with less competition (high HHIs) and among rural target hospitals; Table 7 summarizes the aggregated estimates for each subsample. In HRRs with above-median baseline HHI, consolidated hospitals experience a 0.401 log-point decline in Medicaid births,

equivalent to a 33.0% reduction, while target and acquirer hospitals experience declines of 37.6% and 34.5% (the latter marginally significant), respectively (Figure A4). These high-HHI markets also show meaningful reductions in unique admitting NPIs, with declines of 17.0% for targets, 26.7% for acquirers, and 20.1% for consolidated hospitals (Figure A6), along with a marginally significant 5.3% decline in hospital beds among targets (Table 7). In contrast, hospitals in lower-HHI markets show no statistically significant declines in births, hospital beds, or unique admitting NPIs. We also find that the target hospital volume effect is particularly pronounced in rural areas: rural target hospitals experience a 0.801 log-point decline in Medicaid births, corresponding to a 55.1% reduction, along with a statistically insignificant 9.5% decline in hospital beds. Notably, rural acquirer hospitals experience a statistically significant 34.2% *increase* in Medicaid births, consistent with the delivery volume shifting from rural targets toward nearby acquirers. Figure A8 exhibits these estimates. This result is consistent with literature focusing on maternity ward restrictions in rural markets (Battaglia, 2025; Dranove et al., 2025).

Urban hospitals show smaller and statistically insignificant changes across these capacity measures (Table 7), aside from a marginally significant 11.2% decline in unique admitting NPIs among consolidated hospitals. Finally, among not-for-profit hospitals, target hospitals experience a smaller and marginally significant 0.293 log-point decline in births, equivalent to a 25.4% reduction, while acquirer and consolidated birth estimates are not statistically significant. Overall, these results suggest that the main reductions in Medicaid L&D volume and related capacity are concentrated in high-concentration markets and rural target hospitals, consistent with merger-related service reductions being most salient where competitive pressure is weaker or where maintaining L&D capacity may be financially vulnerable.

Our main target-hospital results remain robust to two additional checks. First, the estimated decline in Medicaid L&D volume persists when excluding states that expanded Medicaid during the 2016–2021 study period: target hospitals in non-expansion states experience a 0.551 log-point decline in Medicaid births, equivalent to a 42.3% reduction, along with a statistically significant 10.0% decline in hospital beds and a marginally significant 14.3% decline in unique admitting NPIs.

Second, to assess whether local spillovers could contaminate the never-treated comparison group, we re-estimate the models after dropping never-treated hospitals located in counties exposed to a merger. In this SUTVA check, target hospitals experience a 0.412 log-point decline in Medicaid births, compared with a 0.459 log-point decline in the main specification. The analogous estimates for acquirer hospitals and the combined consolidation group are smaller: a statistically insignificant −0.105 log points and a statistically significant −0.204 log points, respectively. These findings suggest that the main decline in Medicaid L&D volume among target hospitals is not driven by contamination of the never-treated comparison group. Table 9 presents the aggregated difference-in-differences estimates from each robustness check.

Effect on Hospital Care Delivery and Birth Outcomes

Next, we discuss the effects of mergers on hospital care delivery and birth outcomes. Panel B of Table 6 summarizes the aggregated post-merger estimates for each outcome and treatment arm. We first examine the downstream impact of hospital mergers on C-sections, C-sections without a documented medical indication, induced labor, and maternal complications. We study these outcomes because they differ in clinical management, scheduling flexibility, and regulatory oversight. Elective deliveries before 39 weeks, including both vaginal inductions and Cesarean deliveries, are monitored through national quality measures, such as the Joint Commission's PC-01 "Elective Delivery" measure and the Centers for Medicare & Medicaid Services (CMS) Quality ID #335 "Maternity Care: Elective Delivery (Without Medical Indication) at <39 Weeks" (Centers for Medicare & Medicaid Services, 2024; The Joint Commission, 2024).

Elective C-sections are planned surgical deliveries with predictable timing, while elective labor inductions artificially initiate labor and involve greater uncertainty in progression. Among first-time mothers or those with less favorable clinical conditions, inductions can increase the likelihood of requiring a C-section due to failed labor progression (S. L. Clark et al., 2009; Laughon et al., 2012; The Joint Commission, 2024). Zhang et al. (2010) further show that spontaneous labor progression varies widely across women, implying that induced labor requires greater clinical oversight and

resource use. Accordingly, we test whether merger effects differ across procedures that vary in clinical management intensity and scheduling constraints.

C-section rates: Overall and without medical indication

Overall C-section rates show little evidence of decline after merger exposure (Table 6). Target hospitals experience a marginally significant 1.10 percentage-point increase in overall C-section rates, while acquirer and consolidated hospitals experience statistically insignificant declines of 0.84 percentage points and 0.39 percentage points, respectively. Figure 8 displays the event studies and dynamic coefficients for these results. The pattern is more pronounced when we focus specifically on C-sections without a documented medical indication. Target hospitals experience a statistically significant 1.50 percentage-point increase in C-sections without a documented medical indication. The corresponding estimates for acquirer and consolidated hospitals are also positive: a marginally significant 0.85 percentage-point increase and a statistically significant 0.90 percentage-point increase, respectively (Table 6, Panel B). By contrast, C-sections with a documented medical indication decline significantly among acquirer and consolidated hospitals (1.69 and 1.29 percentage points, respectively). Thus, we have suggestive evidence that merger exposure is associated with a compositional shift in C-section provision toward more non-medically-indicated C-sections. This effect is most evident among target hospitals. Figure 9 displays the event studies and dynamic coefficients for these results.

We run the Rambachan and Roth (2023) HonestDiD sensitivity analysis for our target treatment result. We show in Appendix Figure A10 that the statistical significance of this estimate is not robust to violations of parallel trends: the robust confidence sets include zero once post-treatment violations are permitted to exceed roughly 5 percent of the largest pre-treatment deviation ($\bar{M} \approx 0.05$). In contrast to the birth-volume effect, which survives violations up to $\bar{M} \approx 0.35$, this result rests on the parallel-trends assumption holding closely in the post-treatment period. We therefore interpret the compositional shift in C-section provision as suggestive rather than definitive evidence.

Induced labor rates

The induced labor results are concentrated among target hospitals (Table 6 and Figure 10). Target hospitals experience a statistically significant 5.31 percentage-point decline in induced labor rates. This decline is consistent with the broader reduction in Medicaid L&D volume among target hospitals and may reflect reductions in scheduled or capacity-sensitive delivery services after merger exposure. In contrast, acquirer hospitals experience a small and statistically insignificant 0.75 percentage-point increase in induced labor rates, while consolidated hospitals experience a statistically insignificant 1.03 percentage-point decline. Overall, the induced labor results suggest that the reduction in labor-management intensity is concentrated among target hospitals rather than acquirer hospitals.

Maternal Complications

Finally, we find no evidence that hospital mergers increase maternal complications conditional on giving birth; if anything, complications decline (Table 6 and Figure 11). Target hospitals experience a statistically insignificant 0.31 percentage-point decrease in maternal complications, acquirer hospitals experience a statistically significant 3.90 percentage-point decrease, and consolidated hospitals experience a statistically insignificant 2.00 percentage-point decrease. Taken together, the birth outcome results suggest that mergers are associated with changes in delivery practice patterns, including suggestive evidence for a shift toward C-sections without documented medical indication and a decline in induced labor among target hospitals, but do not provide evidence of increased maternal complications among Medicaid-covered mothers who continue to deliver at these hospitals.

Heterogeneity Test Results: Hospital Care Delivery and Birth Outcomes

Because rural and high-concentration settings are major drivers of our access effects, we perform heterogeneity tests along these dimensions for our hospital care delivery and birth outcomes; Table 8

summarizes the aggregated estimates for each subsample. Notably, we observe a 5.3 percentage point increase in induced labor in merged hospitals within rural settings, implying more intensive L&D care. Appendix Figures A11, A12, and A13 show that a merged rural hospital system increased their rate of labor induction without evidence of changing their rates of C-sections. This effect goes the opposite direction as the main result. This suggests that hospital mergers in rural areas not only lead to a redistribution of births within the hospital system, but also to significant changes in L&D care protocols specific to their financial realities and market environments.

Urban hospitals display a distinct pattern (Appendix Figures A15–A18). Urban target hospitals experience a statistically significant 3.8 percentage-point decline in induced labor rates, going the same direction as the main target-hospital result, while urban acquirer and consolidated hospitals experience statistically significant declines in overall C-section rates of 1.1 and 1.0 percentage points, respectively, with no accompanying change in C-sections without medical indication. The only evidence of potentially worsening outcomes is a marginally significant 1.5 percentage-point increase in maternal complications among urban target hospitals. Taken together, these results indicate that the post-merger shift toward more intensive labor management is specific to rural markets, whereas mergers in urban markets are instead associated with modest reductions in procedure intensity.

4. County-Level Acquirer Treatment Results

Acquirer hospitals are on the "buyer" side of a merger transaction, allowing them to influence most decisions related to personnel shifts, care procedures, and insurer contracting (Cooper et al., 2019). We now discuss adjustments in equilibrium due to acquirer hospitals, examining the overall census of births in our NVSS sample of births (N = 14,893,580) and our Medicaid subsample (N = 6,037,451) from 2016 to 2022. Because our background and empirical research suggest that target hospitals are likely to reduce L&D volume, either through maternity ward closures or reductions in L&D volume overall, we focus on acquirer hospitals for our county-level analysis. Because

our treatment is defined at the county level and treatment counties may also contain never-treated hospitals, we expect smaller effect sizes than those observed with our hospital-level treatment models. We present our aggregated ATT estimates in Table 10.

County-Level Effect of Acquirers on Medicaid Birth Volume

We find that at the county level, on average, acquirer hospitals have no obvious impact on county birth volume. When assessing Figure 12, we observe flat effects after county acquirer exposure. While there is a statistically significant pre-treatment estimate five years before treatment, an isolated estimate five years before a merger is unlikely to reflect anticipation.

County-Level Effect of Acquirers on Access to Care

We find that counties with an acquirer hospital show a small but statistically significant 0.098-visit increase (or 0.7% relative increase) in prenatal visits and a 0.7 percentage point increase (or 1.1% relative increase) in the probability of a same-county birth (this effect just missed statistical significance with $p = 0.062$). Across 3.7 million post-treatment births, this translates to roughly 360,000 additional prenatal visits and 25,000 additional same-county births attributable to the county-acquirer effect. When assessing Figures 13 and 14, we observe no differential pre-trends and see clear increases in dynamic effect sizes. Whether a birth is attended by a physician remained flat after a merger as shown in Figure 15. The same-county birth effect for Medicaid births is similar to the overall effect and is statistically significant, as mothers with Medicaid are 0.9 percentage points (or 1.3%) more likely to give birth in the same county where they live after exposure to an acquirer hospital within their county of residence. Across 1.5 million post-treatment observations for Medicaid births, this translates to roughly 14,000 additional same-county Medicaid births that are attributable to the county-acquirer effect. Our findings indicate that overall access equilibrium is not impacted much by acquirer hospitals, though local access to care is slightly improved for mothers with Medicaid, given their statistically significant increase in same-county birth probability and an increase of 0.09 prenatal visits per birth (this effect just missed statistical significance with

$p = 0.085$). This translates to roughly 140,000 additional prenatal visits across 1.5 million post-treatment births that are attributable to the county-acquirer effect.

County-Level Effect of Acquirers on Birth Outcomes

We find that births in counties with acquiring hospitals experience a statistically significant 0.4 percentage-point decrease (or 1.3% relative decrease) in C-section rates among Medicaid births, relative to a baseline mean of 31.3% (Figure 16). This translates to roughly 5400 fewer C-sections within our sample attributable to the county acquirer effect. We observe no detrimental effects on five-minute Apgar scores, as shown in Figure 17. Taken together, these findings suggest that, at the market level, hospitals reduce resource utilization following mergers without adverse impacts on birth outcomes.

5. Discussion

Hospital mergers are a primary mechanism through which hospital systems consolidate market power, enhancing their bargaining leverage in negotiations with insurers. Consolidation may also reduce hospitals' incentives to invest in quality by eliminating marginal competitors. However, mergers can simultaneously generate efficiency gains through economies of scope and scale (Andreyeva et al., 2024; Arnold & Whaley, 2024; J. R. Clark & Huckman, 2012; Craig et al., 2021; Schmitt, 2017). In this study, we examine how hospital mergers affect access to labor and delivery (L&D) care and birth outcomes for Medicaid beneficiaries using claims data. We also explore potential market-level adjustments influenced by acquirer hospitals as they assume control over a larger network of facilities. We find that hospital mergers reduce Medicaid L&D volume at target hospitals, particularly in rural areas and more concentrated markets. At the same time, we find limited evidence of adverse changes in measured maternal or newborn outcomes among mothers who continue to give birth in these markets.

The decline in Medicaid L&D admissions is driven primarily by rural target hospitals and

hospitals in more concentrated markets. Target hospitals experience sizable reductions in Medicaid birth volume after merger exposure. When examining the rural setting, we find that acquirer hospitals absorb displaced births from the delivery reductions in target hospitals.

The estimated changes in unique L&D admitting providers are negative across treatment groups but not consistently statistically significant in the main specification, on average. However, in more concentrated hospital markets, our results are consistent with the evidence presented by Prager and Schmitt (2021), who find evidence of merged hospitals exercising their market power. In particular, the statistically significant reductions in the number of unique admitting providers across all treatment groups suggest that hospitals with less competition can more readily use indirect methods of restricting L&D care access. One plausible mechanism is through the reduction of staff available to provide L&D services.

Consolidated systems may reallocate capacity away from L&D services and toward other inpatient uses. Although these reductions in L&D access are consistent with financial incentives to reduce lower-margin service lines, the quality implications are more nuanced. There is suggestive evidence that C-section rates decline modestly overall when considering the impact of acquirers on overall hospital markets, while C-sections without documented medical indication increase among target hospitals. We do not find statistically significant increases in maternal complications. These findings suggest that mergers alter delivery practice patterns and L&D service availability, but they do not provide evidence of systematic deterioration in measured maternal health outcomes among Medicaid-covered mothers who continue delivering at these hospitals.

Hospital mergers have heterogeneous impacts on care delivery and quality depending on the geographic setting of the market. Our results suggest that whether a hospital is in a rural or urban setting shapes not only who delivers care but also how hospitals deliver that care. In rural markets, where target hospitals experience the sharpest volume losses and acquirer hospitals absorb displaced births, consolidated systems increase induced labor without corresponding changes in C-section rates. This pattern is consistent with rural systems concentrating their deliveries and L&D personnel in fewer, higher-volume facilities that rely on scheduled labor management. This

is a plausibly rational response to longer travel differences for mothers and less L&D staffing. It is also consistent with the evidence that hospital consolidation of L&D care into fewer facilities can alter care protocols (Battaglia, 2025; Fischer et al., 2024).

Urban markets exhibit the opposite adjustment. Target hospitals reduce their rate of induced labor and consolidated hospitals mostly reduce their overall C-section rates. This is evidence for de-intensification of L&D care where nearby alternatives are more readily available than in rural markets.

Importantly, neither setting shows meaningful increases in maternal complications, so these protocol changes appear to reflect a reorganization of care delivery rather than a worsening of care quality. Taken together with our access results, our findings imply that the consequences of hospital consolidation for Medicaid L&D care are mediated by local market structure: in rural and highly concentrated markets, mergers reallocate births across facilities and intensify labor management within the surviving units, whereas in urban markets they primarily trim procedure intensity at the margin. Policy responses to hospital consolidation should therefore weigh not only the access losses concentrated among rural target hospitals but also the accompanying shifts in clinical practice at facilities that continue to deliver care.

When examining the overall market-level implications of acquirer exposure, we find an increased likelihood that mothers with Medicaid give birth within their county of residence and a slight increase in the number of prenatal visits per birth. C-section rates decrease for mothers with Medicaid, with no detrimental impacts on five-minute Apgar scores. These county-level results suggest that, even as target hospitals reduce Medicaid L&D volume, local markets may partially adjust by reallocating births across facilities. The observed increase in same-county births among births exposed to acquirer hospitals is consistent with births being absorbed by other nearby facilities, including acquirer or non-merging hospitals.

These findings suggest that acquirer hospitals in merger deals may influence local L&D market organization without generating detectable adverse changes in measured newborn outcomes on average. This pattern is consistent with some degree of care reallocation within local markets.

However, the hospital-level decline in target-hospital Medicaid L&D volume indicates that these market adjustments occur alongside meaningful reductions in where Medicaid-covered mothers can deliver. Thus, even when measured outcomes do not worsen, hospital mergers may still reduce patient choice and alter the distribution of L&D access across facilities.

These results indicate that antitrust scrutiny of hospital mergers should consider distributional effects across payer groups and service lines. While there are legal restrictions that prevent hospitals from overtly refusing emergency L&D care to a mother in labor (Centers for Medicare & Medicaid Services, 2019), hospitals can still reduce L&D access through service-line contraction, maternity ward closures, or reductions in delivery capacity. Medicaid agencies and antitrust policymakers should therefore monitor whether consolidation leads to reductions in Medicaid-serving obstetric capacity, particularly in rural and already concentrated markets. Even in the absence of detectable increases in maternal complications, reductions in L&D availability may have important implications for access, travel burden, and local safety net capacity.

We acknowledge several limitations. First, there were some states where we could not reliably link facility identifiers to billing NPI numbers. We therefore do not have a complete nationally representative sample for our hospital-level treatment models. Second, we limit our dynamic treatment effects to three years post-treatment for our hospital-level treatment models and four years post-treatment for our county-level treatment models, giving us a short-run view of post-merger effects. Third, we do not specifically examine full hospital closures, but instead focus on hospitals that survived throughout our study period (some of which closed their maternity wards). Fourth, we do not study cross-market mergers; instead, we restrict attention to mergers that plausibly affect local market power within a hospital's HRR. Fifth, our claims-based measure of C-sections without documented medical indication depends on diagnosis and procedure codes, and therefore captures the absence of coded indications rather than the absence of true clinical indication. Finally, we do not incorporate analyses of vertical integration, meaning that we do not directly examine the outpatient care dynamics involved with prenatal care.

In sum, coupling hospital-derived Medicaid claims with birth-level vital records suggests that

hospital mergers reduce Medicaid L&D volume at target hospitals, especially in rural and more concentrated markets. These reductions are accompanied by modest changes in delivery practice patterns, including suggestive evidence for lower overall C-section rates and higher C-sections without documented medical indication among target hospitals, but no statistically significant increase in maternal complications. At the county level, acquirer exposure is associated with small improvements in local access measures and no deterioration in newborn outcomes. Together, these findings suggest that hospital mergers reshape L&D markets by reducing delivery volume at target hospitals while inducing local market adjustments that may maintain birth quality.

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Table 1: Hospital Characteristics by Treatment Status

Variable	**Full Sample**	**Consolidated**	**Targeted**	**Acquirer**	**Never Treated**
N (hospital-year observations)	*5,754*	*1,110*	*504*	*642*	*4,644*
Hospital beds	241.669 (228.515)	273.845 (225.779)	222.609 (179.381)	308.508 (246.129)	233.979 (228.518)
Unique admitting NPIs	19.446 (22.814)	23.610 (26.154)	19.540 (23.481)	26.327 (27.194)	18.451 (21.827)
Total births	386.655 (697.399)	413.368 (596.790)	337.331 (450.693)	463.223 (673.080)	380.270 (719.277)
Total C-section admits	112.001 (210.173)	121.278 (180.396)	100.065 (148.154)	135.234 (197.007)	109.784 (216.644)
System affiliated	0.617 (0.486)	0.818 (0.386)	0.762 (0.426)	0.852 (0.355)	0.569 (0.495)
Hospitals in system	8.154 (10.816)	7.597 (9.282)	7.472 (10.102)	7.657 (8.606)	8.287 (11.149)
For-profit	0.151 (0.358)	0.104 (0.305)	0.131 (0.338)	0.086 (0.280)	0.163 (0.369)
Public facility	0.164 (0.370)	0.076 (0.265)	0.131 (0.338)	0.037 (0.190)	0.185 (0.388)
Rural county	0.383 (0.486)	0.254 (0.436)	0.262 (0.440)	0.243 (0.429)	0.413 (0.493)
Maternity ward closure	0.045 (0.207)	0.070 (0.256)	0.095 (0.294)	0.047 (0.211)	0.039 (0.193)

Notes: This table reports hospital-year means with standard deviations in parentheses. **Full Sample** covers all hospital-year observations in the analytic file after restricting to hospitals observed in all six years from 2016 through 2021. **Consolidated** covers hospital-year observations for hospitals that experience a within-HRR consolidation event during the analysis period, pooling targets and acquirers. **Targeted** covers hospital-year observations for hospitals that serve as targets in a within-HRR consolidation event during the analysis period. **Acquirer** covers hospital-year observations for hospitals that serve as acquirers in a within-HRR consolidation event during the analysis period. **Never Treated** covers hospital-year observations for hospitals with no consolidation exposure during the analysis period. Binary variables, including system-affiliated, for-profit, public facility, rural county, and dropped to zero births, are reported as proportions. The **Targeted** and **Acquirer** columns are not mutually exclusive in general; hospitals can appear in both categories if they serve as a target in one event/year and an acquirer in another event/year.

Table 2: Hospital Characteristics by Treatment Status (5-NN PSM Matched Sample)

	By Consolidation Status		**By Target Status**		**By Acquirer Status**	
Variable	**Consolidated**	**Never-Treated**	**Targeted**	**Non-Target**	**Acquirer**	**Non-Acquirer**
N (hospital-year obs.)	*760*	*2,682*	*321*	*3,121*	*470*	*2,972*
Hospital beds	268.757 (217.213)	253.512 (223.803)	210.243 (148.666)	261.674 (228.142)	302.347 (242.655)	249.687 (218.231)
Unique admitting NPIs	22.272 (24.319)	19.440 (20.990)	17.100 (17.873)	20.370 (22.141)	25.266 (26.876)	19.243 (20.767)
Total births	356.378 (526.430)	395.543 (705.537)	269.047 (378.964)	399.016 (692.203)	406.826 (588.133)	383.743 (682.364)
Total C-section admits	106.663 (164.105)	116.852 (221.247)	81.620 (136.150)	117.995 (215.891)	121.291 (174.872)	113.545 (215.028)
System affiliated	0.793 (0.405)	0.510 (0.500)	0.695 (0.461)	0.560 (0.496)	0.847 (0.361)	0.530 (0.499)
Hospitals in system	5.474 (5.302)	4.715 (6.022)	5.299 (6.227)	4.840 (5.841)	5.536 (4.560)	4.779 (6.055)
For-profit	0.089 (0.286)	0.116 (0.321)	0.153 (0.360)	0.106 (0.308)	0.049 (0.216)	0.120 (0.325)
Public facility	0.088 (0.284)	0.201 (0.401)	0.181 (0.385)	0.176 (0.381)	0.032 (0.176)	0.199 (0.399)
Rural county	0.257 (0.437)	0.342 (0.475)	0.274 (0.447)	0.328 (0.470)	0.240 (0.428)	0.336 (0.473)
Maternity ward closure	0.050 (0.218)	0.036 (0.186)	0.050 (0.218)	0.038 (0.191)	0.047 (0.211)	0.038 (0.190)

Notes: This table reports hospital-year means with standard deviations in parentheses for our propensity-score-matched analysis sample. The sample is constructed via five-nearest-neighbor (5-NN) propensity score matching: each merger-exposed hospital is matched to its five nearest never-treated hospitals on a probit propensity score estimated from baseline (2016) log Medicaid births, log hospital beds, not-for-profit status, rural location, system-level HRR HHI, and county-level log median household income, bachelor's-degree share, and unemployment rate. Each pair of columns splits the matched sample by a different treatment definition. **By Consolidation Status** compares hospital-year observations at hospitals exposed to any within-HRR consolidation event (targets or acquirers) with matched never-treated hospitals. **By Target Status** and **By Acquirer Status** compare target (acquirer) hospital-year observations with all other observations in the matched sample; the **Non-Target** and **Non-Acquirer** columns therefore include both matched never-treated hospitals and hospitals treated under the other definition. Binary variables, including system-affiliated, for-profit, public facility, rural county, and maternity ward closure, are reported as proportions. Maternity ward closure indicates that a hospital permanently dropped to zero Medicaid births during the study period. The **Targeted** and **Acquirer** columns are not mutually exclusive in general; hospitals can appear in both categories if they serve as a target in one event/year and an acquirer in another event/year.

Table 3: L&D Admission Descriptive Statistics by Treatment Status

Variable	Entire Sample	Consolidated	Targeted	Acquirer	Never-Treated
N (L&D admissions)	*2,071,255*	*459,080*	*185,529*	*282,533*	*1,612,175*
Hospital beds	424.886 (333.328)	445.080 (276.916)	360.415 (243.323)	493.433 (283.485)	419.136 (347.507)
Elixhauser Comorbidities	0.431 (0.717)	0.463 (0.733)	0.455 (0.720)	0.470 (0.744)	0.422 (0.712)
C-section delivery rate	0.287 (0.452)	0.289 (0.453)	0.284 (0.451)	0.294 (0.455)	0.286 (0.452)
C-section without medical indication	0.060 (0.237)	0.060 (0.238)	0.057 (0.231)	0.063 (0.242)	0.060 (0.237)
C-section with medical indication	0.227 (0.419)	0.229 (0.420)	0.227 (0.419)	0.231 (0.421)	0.226 (0.418)
Induced labor rate	0.215 (0.411)	0.229 (0.420)	0.212 (0.409)	0.238 (0.426)	0.211 (0.408)
Maternal complication	0.649 (0.477)	0.653 (0.476)	0.659 (0.474)	0.648 (0.478)	0.648 (0.478)
White	0.390 (0.488)	0.425 (0.494)	0.458 (0.498)	0.402 (0.490)	0.381 (0.486)
Black	0.200 (0.400)	0.221 (0.415)	0.197 (0.397)	0.241 (0.428)	0.194 (0.395)
American Indian/Alaska Native	0.019 (0.135)	0.013 (0.114)	0.017 (0.130)	0.010 (0.101)	0.020 (0.141)
Asian	0.023 (0.149)	0.017 (0.131)	0.012 (0.110)	0.020 (0.141)	0.024 (0.154)
Hispanic	0.140 (0.347)	0.099 (0.299)	0.108 (0.310)	0.093 (0.290)	0.152 (0.359)

Notes: Cells report means with standard deviations in parentheses. Units are L&D admissions. **Entire Sample** covers all L&D admissions in the analytic file after excluding critical access hospitals. **Consolidated** pools admissions at hospitals exposed to a within-HRR consolidation event during the analytic period (2016–2021), combining targets and acquirers. **Targeted** covers admissions at hospitals serving as targets in a consolidation event during the analytic period. **Acquirer** covers admissions at hospitals serving as acquirers in a consolidation event during the analytic period. **Never-Treated** covers admissions at hospitals never exposed to a consolidation event during the analytic period. Proportion variables, including delivery method, complications, and race/ethnicity indicators, are on a 0–1 scale. C-section without medical indication refers to C-section deliveries not flagged as medically indicated. C-section with medical indication is defined as C-section deliveries not classified as C-section without medical indication. The **Targeted** and **Acquirer** columns are not mutually exclusive in general; a hospital and its admissions can appear in both categories across different events/years.

The maternal complication variable is binary and equal to 1 if a claim contains any of the following ICD-10-CM codes, per AAPC (2025) and World Health Organization (2019): `O60-O77` (complications of labor and delivery). This code range captures diagnoses including preterm labor (`O60`), failed induction of labor (`O61`), abnormalities of forces of labor (`O62`), long labor (`O63`), obstructed labor due to fetal malposition/malpresentation (`O64`) or maternal pelvic abnormality (`O65`), other obstructed labor (`O66`), intrapartum hemorrhage (`O67`), abnormalities of fetal acid-base balance (`O68`), umbilical cord complications (`O69`), perineal laceration during delivery (`O70`), other obstetric trauma (`O71`), postpartum hemorrhage (`O72`), retained placenta and membranes without hemorrhage (`O73`), complications of anesthesia during labor and delivery (`O74`), other complications of labor and delivery (`O75`), abnormal fetal heart rate and rhythm complicating labor and delivery (`O76`), and other fetal stress complicating labor and delivery (`O77`).

Table 4: L&D Admission Descriptive Statistics by Treatment Status (5-NN PSM Hospital-Matched)

Variable	**Entire Sample**	**Consolidated**	**Targeted**	**Acquirer**	**Never-Treated**
N (L&D admissions)	*1,109,790*	*145,471*	*53,581*	*96,815*	*964,319*
Hospital beds	417.473 (259.497)	472.185 (297.170)	374.772 (252.002)	513.825 (305.819)	409.220 (252.301)
Elixhauser Comorbidities	0.441 (0.724)	0.518 (0.767)	0.432 (0.701)	0.569 (0.800)	0.430 (0.717)
C-section delivery rate	0.291 (0.454)	0.288 (0.453)	0.279 (0.448)	0.295 (0.456)	0.291 (0.454)
C-section without medical indication	0.060 (0.238)	0.058 (0.234)	0.058 (0.234)	0.058 (0.233)	0.061 (0.239)
C-section with medical indication	0.230 (0.421)	0.230 (0.421)	0.220 (0.414)	0.238 (0.426)	0.230 (0.421)
Induced labor rate	0.219 (0.413)	0.246 (0.430)	0.238 (0.426)	0.245 (0.430)	0.215 (0.411)
Maternal complication	0.646 (0.478)	0.654 (0.476)	0.631 (0.483)	0.668 (0.471)	0.645 (0.479)
White	0.381 (0.486)	0.452 (0.498)	0.474 (0.499)	0.435 (0.496)	0.370 (0.483)
Black	0.222 (0.416)	0.278 (0.448)	0.248 (0.432)	0.307 (0.461)	0.214 (0.410)
American Indian/Alaska Native	0.021 (0.143)	0.012 (0.109)	0.006 (0.077)	0.015 (0.120)	0.022 (0.147)
Asian	0.024 (0.153)	0.012 (0.108)	0.009 (0.093)	0.013 (0.113)	0.026 (0.159)
Hispanic	0.133 (0.339)	0.077 (0.266)	0.077 (0.266)	0.074 (0.261)	0.141 (0.348)

Notes: Cells report means with standard deviations in parentheses. Units are L&D admissions. **Entire Sample** covers all L&D admissions at hospitals in the five-nearest-neighbor propensity-score-matched sample, after excluding critical access hospitals. **Consolidated** pools admissions at hospitals exposed to a within-HRR consolidation event during the analytic period (2016–2021), combining targets and acquirers. **Targeted** covers admissions at hospitals serving as targets in a consolidation event during the analytic period. **Acquirer** covers admissions at hospitals serving as acquirers in a consolidation event during the analytic period. **Never-Treated** covers admissions at hospitals never exposed to a consolidation event during the analytic period. Proportion variables, including delivery method, complications, and race/ethnicity indicators, are on a 0–1 scale. C-section without medical indication refers to C-section deliveries not flagged as medically indicated. C-section with medical indication is defined as C-section deliveries not classified as C-section without medical indication. The **Targeted** and **Acquirer** columns are not mutually exclusive in general; a hospital and its admissions can appear in both categories across different events/years.

The maternal complication variable is binary and equal to 1 if a claim contains any of the following ICD-10-CM codes, per AAPC (2025) and World Health Organization (2019): `O60–O77` (complications of labor and delivery). This code range captures diagnoses including preterm labor (`O60`), failed induction of labor (`O61`), abnormalities of forces of labor (`O62`), long labor (`O63`), obstructed labor due to fetal malposition/malpresentation (`O64`) or maternal pelvic abnormality (`O65`), other obstructed labor (`O66`), intrapartum hemorrhage (`O67`), abnormalities of fetal acid-base balance (`O68`), umbilical cord complications (`O69`), perineal laceration during delivery (`O70`), other obstetric trauma (`O71`), postpartum hemorrhage (`O72`), retained placenta and membranes without hemorrhage (`O73`), complications of anesthesia during labor and delivery (`O74`), other complications of labor and delivery (`O75`), abnormal fetal heart rate and rhythm complicating labor and delivery (`O76`), and other fetal stress complicating labor and delivery (`O77`).

Table 5: Descriptive Statistics by Treatment Group: Vital Statistics

Variable **Number of Births**	**Full Medicaid Sample** **(N = 6,037,451)**	**Acquirer in County** **(N = 1,637,607)**	**Never Treated** **(N = 4,399,844)**
Medicaid	1.000 (0.000)	1.000 (0.000)	1.000 (0.000)
Hospital birth	0.992 (0.088)	0.993 (0.086)	0.992 (0.089)
Same county birth	0.705 (0.456)	0.756 (0.430)	0.693 (0.461)
5-minute Apgar score	8.755 (0.864)	8.757 (0.858)	8.754 (0.866)
Log birth weight	8.047 (0.238)	8.042 (0.245)	8.049 (0.235)
Birth weight (grams)	3198.036 (597.101)	3185.976 (605.870)	3202.768 (593.610)
Physician attended	0.883 (0.322)	0.871 (0.335)	0.887 (0.317)
Gestation (weeks)	38.449 (2.646)	38.427 (2.735)	38.458 (2.611)
Log gestation	3.647 (0.075)	3.646 (0.078)	3.647 (0.074)
Prenatal visits	12.665 (14.025)	13.124 (15.907)	12.500 (13.255)
C-section	0.316 (0.465)	0.324 (0.468)	0.313 (0.464)
Assisted ventilation (all)	0.053 (0.224)	0.053 (0.224)	0.053 (0.224)
Induced labor	0.291 (0.454)	0.295 (0.456)	0.289 (0.453)
Maternal transfer	0.007 (0.082)	0.005 (0.073)	0.007 (0.085)

Notes: Table reports means with standard deviations in parentheses. The sample includes all Medicaid-covered live births from the National Vital Statistics System (NVSS), 2016–2021, matched to maternal county of residence. "Acquirer in County" indicates births in counties that contain at least one hospital belonging to a system that acquired another hospital during the study period. "Never Treated" counties had no hospitals involved in a merger between 2016 and 2021. All continuous variables are averaged across births and measured at the individual level.

Table 6: Hospital-Level Aggregated Effects of Mergers on L&D Volume, Capacity, and Outcomes

	Target	**Acquirer**	**Consolidated**
Panel A. Hospital-year outcomes (N = hospital-year observations)			
Sample N (hospital-year observations)	*1,598*	*2,179*	*3,442*
Volume: log(Births)	−0.459*** (0.150)	−0.194 (0.118)	−0.204** (0.097)
Physical Capacity: log(Beds)	−0.032 (0.025)	−0.005 (0.017)	−0.017 (0.014)
Staffing: log(Unique Admitting NPIs)	−0.158** (0.068)	−0.126** (0.062)	−0.110** (0.050)
Panel B. L&D admissions outcomes (N = admissions)			
Sample N (admissions)	*1,012,667*	*1,054,469*	*1,109,790*
C-section	0.011* (0.007)	−0.008 (0.006)	−0.004 (0.006)
C-section without medical indication	0.015*** (0.005)	0.009* (0.005)	0.009** (0.004)
Induced Labor	−0.053*** (0.016)	0.008 (0.012)	−0.010 (0.013)
Maternal Complication	−0.003 (0.011)	−0.039** (0.019)	−0.020 (0.014)

Notes: Entries report aggregated post-merger effects from Callaway & Sant'Anna (2021) Difference-in-Differences event-study models estimated on our five-nearest-neighbor (5-NN) propensity-score-matched sample, in which each merger-exposed hospital is matched to its five nearest never-treated hospitals on baseline (2016) volume, capacity, ownership, rurality, market concentration, and county socioeconomic characteristics. Panel A uses **hospital-year** data (unit: hospital-year) to measure delivery volume, physical capacity, and staffing. Panel B uses **L&D admission-level** data (unit: admission) restricted to births at matched hospitals to measure clinical outcomes. Standard errors are cluster-robust at the hospital level and reported in parentheses. Estimates correspond to Figures 5–11.

C-section without medical indication is defined as a C-section admission without diagnosis codes indicating a clear medical indication. C-section with medical indication is defined as the complementary medically indicated C-section measure based on the indication algorithm described in Appendix Table A2.

The maternal complication variable is binary and equal to 1 if a claim contains any of the following ICD-10-CM codes, per AAPC (2025) and World Health Organization (2019): `O60–O77` (complications of labor and delivery). This code range captures diagnoses including preterm labor (`O60`), failed induction of labor (`O61`), abnormalities of forces of labor (`O62`), long labor (`O63`), obstructed labor due to fetal malposition/malpresentation (`O64`) or maternal pelvic abnormality (`O65`), other obstructed labor (`O66`), intrapartum hemorrhage (`O67`), abnormalities of fetal acid-base balance (`O68`), umbilical cord complications (`O69`), perineal laceration during delivery (`O70`), other obstetric trauma (`O71`), postpartum hemorrhage (`O72`), retained placenta and membranes without hemorrhage (`O73`), complications of anesthesia during labor and delivery (`O74`), other complications of labor and delivery (`O75`), abnormal fetal heart rate and rhythm complicating labor and delivery (`O76`), and other fetal stress complicating labor and delivery (`O77`). *Legend:* * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 7: Aggregated Post-Treatment Effects Across Heterogeneity Dimensions

Group	**Target**	**Acquirer**	**Consolidation**
HHI – Upper Half			
log(Births)	−0.472** (0.192)	−0.423* (0.246)	−0.401*** (0.147)
log(Hospital Beds)	−0.055* (0.033)	−0.025 (0.058)	−0.037 (0.034)
log(Unique NPIs)	−0.186** (0.090)	−0.311** (0.128)	−0.225*** (0.076)
HHI – Lower Half			
log(Births)	−0.146 (0.233)	0.074 (0.145)	0.032 (0.125)
log(Hospital Beds)	−0.038 (0.030)	0.023 (0.018)	0.004 (0.017)
log(Unique NPIs)	0.002 (0.092)	0.036 (0.074)	0.029 (0.061)
Not-for-Profit Hospitals			
log(Births)	−0.293* (0.160)	−0.041 (0.116)	−0.120 (0.097)
log(Hospital Beds)	−0.029 (0.023)	0.028* (0.014)	0.002 (0.013)
log(Unique NPIs)	−0.090 (0.070)	−0.077 (0.066)	−0.080 (0.051)
Urban Hospitals			
log(Births)	−0.229 (0.183)	−0.203 (0.153)	−0.184 (0.123)
log(Hospital Beds)	−0.031 (0.025)	0.006 (0.021)	−0.009 (0.016)
log(Unique NPIs)	−0.112 (0.091)	−0.144 (0.095)	−0.119* (0.071)
Rural Hospitals			
log(Births)	−0.801** (0.320)	0.294** (0.150)	−0.183 (0.180)
log(Hospital Beds)	−0.100 (0.077)	0.008 (0.024)	−0.039 (0.030)
log(Unique NPIs)	−0.136 (0.130)	0.033 (0.057)	−0.044 (0.067)

Notes: Entries are aggregated post-treatment effects (average of event-time coefficients $t \geq 0$) from Callaway & Sant'Anna (2021) DiD event-study models estimated on the five-nearest-neighbor propensity-score-matched sample; standard errors shown in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. HHI split is based on the 2016 median HHI. The Not-for-Profit Hospitals section excludes for-profit facilities.

Table 8: Aggregated Post-Treatment Effects on L&D Care Quality Across Heterogeneity Dimensions

Group	**Target**	**Acquirer**	**Consolidation**
HHI – Upper Half			
C-section	−0.004 (0.009)	−0.008 (0.029)	−0.008 (0.005)
C-section w/o Medical Indication	0.010** (0.005)	0.010 (0.013)	0.007** (0.003)
Induced Labor	−0.019 (0.022)	0.011 (0.034)	−0.000 (0.013)
Maternal Complications	0.004 (0.009)	−0.009 (0.034)	−0.007 (0.007)
Urban Hospitals			
C-section	−0.008 (0.006)	−0.011** (0.005)	−0.010*** (0.004)
C-section w/o Medical Indication	0.005 (0.004)	−0.003 (0.006)	−0.001 (0.004)
Induced Labor	−0.038*** (0.011)	0.008 (0.008)	−0.006 (0.008)
Maternal Complications	0.015* (0.008)	−0.005 (0.015)	0.002 (0.010)
Rural Hospitals			
C-section	−0.019 (0.037)	−0.000 (0.013)	−0.004 (0.012)
C-section w/o Medical Indication	0.006 (0.020)	0.005 (0.007)	0.008 (0.007)
Induced Labor	0.063* (0.037)	0.044** (0.021)	0.053*** (0.020)
Maternal Complications	−0.023 (0.023)	0.012 (0.015)	−0.002 (0.013)

Notes: Entries are aggregated post-treatment effects (average of event-time coefficients $t \geq 0$) from Callaway & Sant'Anna (2021) DiD event-study models estimated on Medicaid TAF claims-derived L&D admission-level data, restricted to hospitals in the five-nearest-neighbor propensity-score-matched sample; standard errors, clustered at the hospital level, are shown in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Outcomes are binary indicators measured at the birth level, so coefficients are interpretable as changes in rates. HHI split is based on the 2016 median HHI.

Table 9: Robustness Checks: Aggregated Post-Treatment Effects

A. No Late Medicaid Expanders			
	Target	**Acquirer**	**Consolidation**
log(Births)	−0.551*** (0.208)	−0.203 (0.249)	−0.341** (0.169)
log(Beds)	−0.105** (0.052)	−0.011 (0.041)	−0.054 (0.034)
log(Unique Admitting NPIs)	−0.155* (0.086)	−0.115 (0.128)	−0.105 (0.079)
B. SUTVA Check: Drop Never-Treated Hospitals in Merger Counties			
	Target	**Acquirer**	**Consolidation**
log(Births)	−0.412*** (0.152)	−0.105 (0.124)	−0.204** (0.101)
log(Beds)	−0.049* (0.025)	0.013 (0.016)	−0.012 (0.014)
log(Unique Admitting NPIs)	−0.133** (0.068)	−0.103 (0.073)	−0.108** (0.054)

Notes: Entries are aggregated post-treatment effects from Callaway & Sant'Anna (2021) doubly robust staggered difference-in-differences event-study estimations on the five-nearest-neighbor propensity-score-matched sample. Standard errors, clustered at the hospital level, are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Approximate percent changes can be calculated as $100 \times (e^{\hat{\beta}} - 1)$. Panel A excludes states that expanded Medicaid during 2016–2021. Panel B assesses potential SUTVA violations by dropping never-treated hospitals located in counties where a merger occurred.

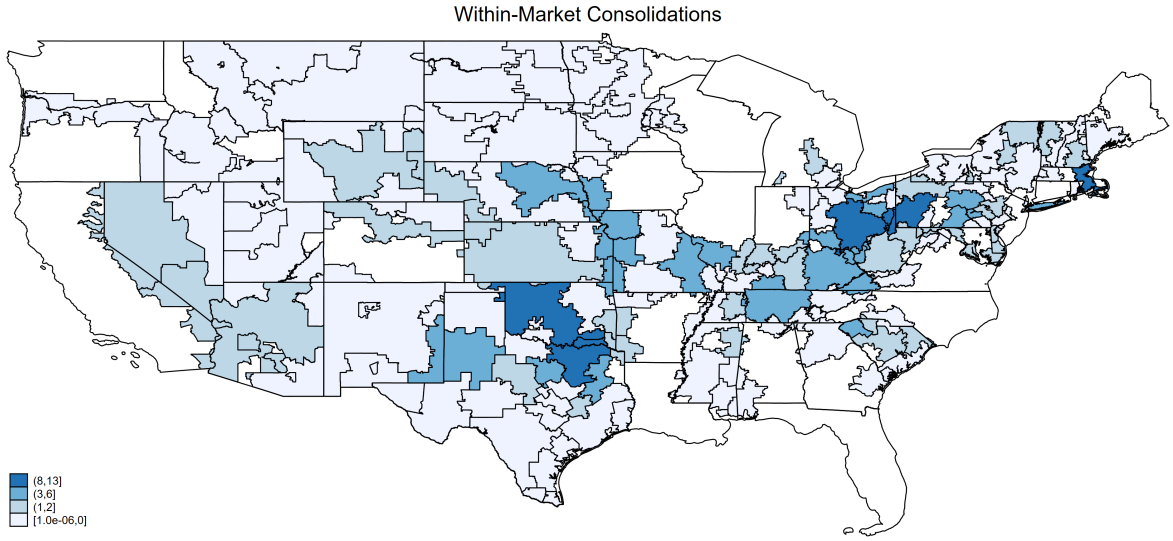
Table 10: Equilibrium Adjustment Effects of Acquirers: Aggregated Estimates by Outcome

	Overall Births	**Medicaid**
N (County-years – Volume)	13,511	13,511
Medicaid births per 1,000 women	-0.043 (0.106)	-0.019 (0.087)
N (Births — Access)	13,053,447	5,300,767
Same-county birth	0.007* (0.004)	0.009** (0.004)
Physician-attended birth	-0.002 (0.003)	-0.001 (0.004)
Prenatal visits	-0.100** (0.047)	0.093* (0.054)
N (Births — Outcomes)	13,053,447	5,300,767
Cesarean delivery	-0.003* (0.002)	-0.004** (0.002)
5-minute Apgar score	0.014* (0.008)	0.011 (0.008)

Notes: Each entry reports aggregated coefficients estimated from Callaway and Sant'Anna Difference-in-Differences (DiD) models, capturing average post-acquisition changes in county-level birth outcomes. Estimates correspond to Figures 12 to 17, which present dynamic effects by outcome and insurance group. Standard errors (in parentheses) are cluster-robust at the county level. We exclude counties that contain within-HRR target hospitals.
Outcomes are grouped into three conceptual categories: (1) overall fertility (births per 1,000 women), (2) access measures (same-county births, physician-attended births, prenatal visits), and (3) perinatal outcomes (5-minute Apgar scores, and C-sections).
Legend: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

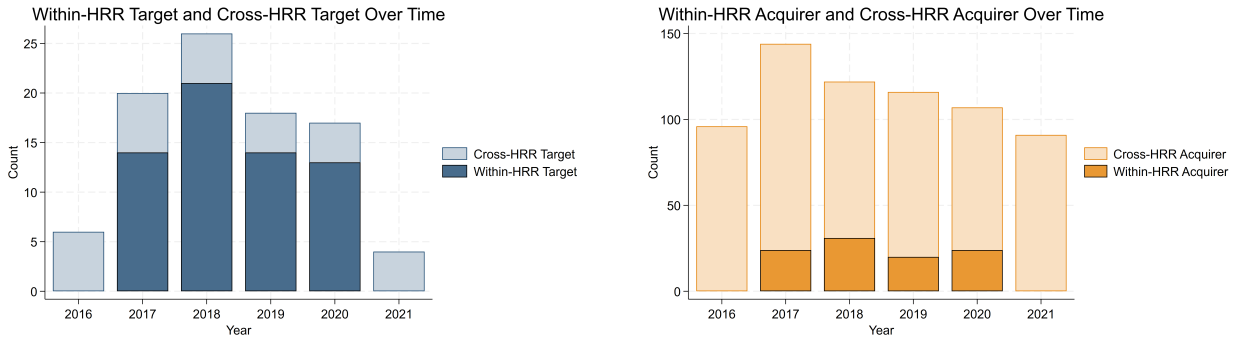
Figures

Figure 1: Within-Market Consolidations by HRR (Hospital Sample)



Notes: This figure displays the frequency of hospital consolidations by hospital referral region (HRR) within our Medicaid claims-derived 5-NN matched hospital sample. Shaded areas indicate HRRs with one or more within-market consolidation events. We show state boundaries and HRR borders for reference.

Figure 2: Counts of Treated Hospitals, 2016–2021

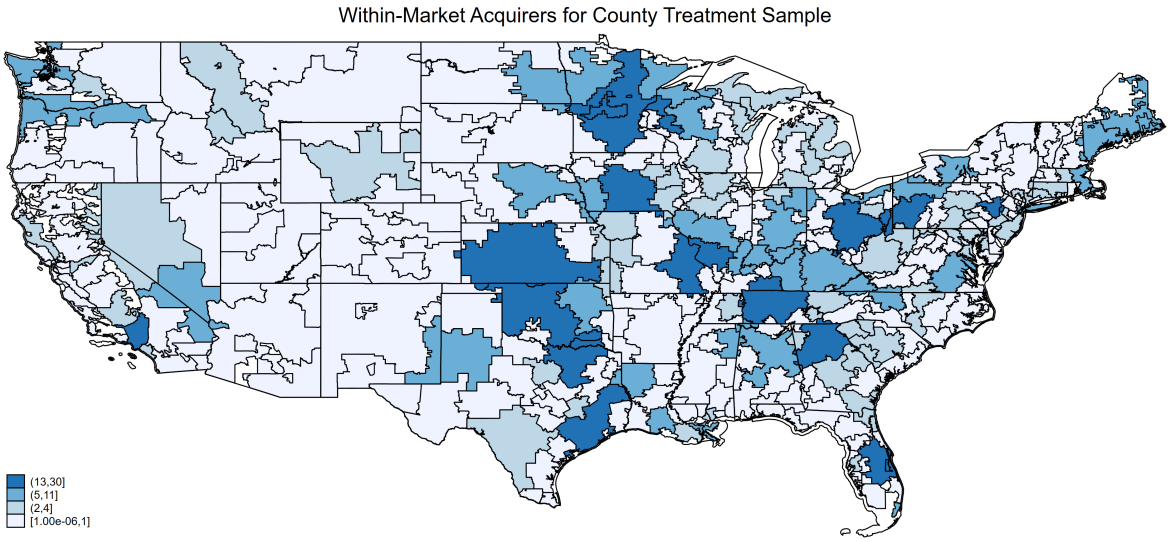


(a) Target Hospitals

(b) Acquirer Hospitals

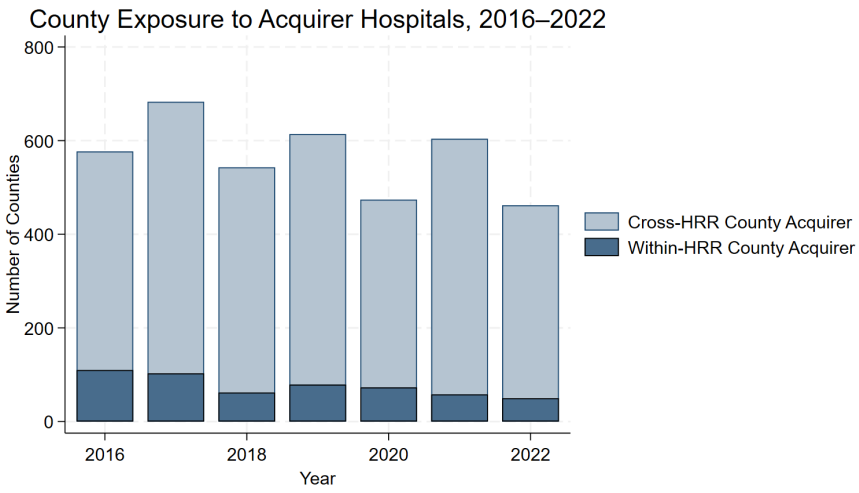
Note: Bars show the number of hospitals experiencing a merger each year within our 5-NN matched hospital sample. Within-HRR counts represent the treatments for our study. A hospital is considered a "within-HRR" target or acquirer if the partner hospital is located in the same Hospital Referral Region (HRR), indicating a local merger deal. A hospital is considered a "cross-HRR" target or acquirer if the partner hospital is located outside its HRR.

Figure 3: Acquirer Hospitals



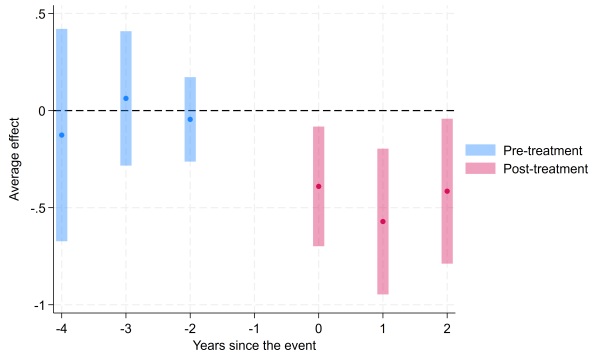
Notes: This figure displays the frequency of hospital acquirers by hospital referral region (HRR) within our vital statistics-derived NVSS birth sample. Shaded areas indicate HRRs with one or more within-market consolidation events. We show state boundaries and HRR borders for reference.

Figure 4: County Exposure to Acquirer Hospitals, 2016–2022

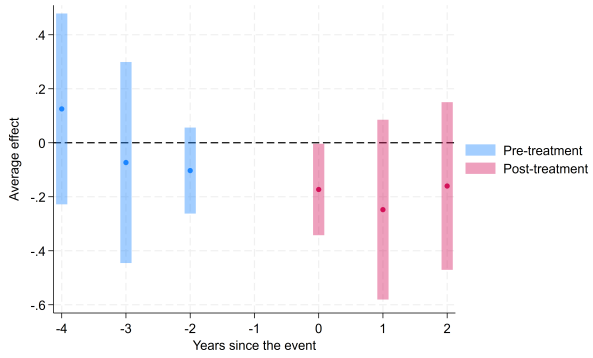


Note: "Within-HRR County Acquirer" indicates the subset of counties where the acquiring hospital acquired another hospital within the same HRR, representing direct local consolidation exposure. We use the Within-HRR variable as the treatment for our empirical models. Cross-HRR County Acquirer indicates the subset of counties where the acquiring hospital acquired another hospital outside of its HRR.

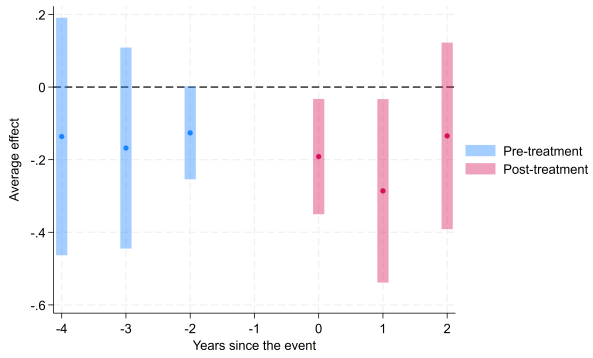
Figure 5: Hospital L&D Volume – log(Births): Target, Acquirer, & Combined Effects



(a) Target Effect



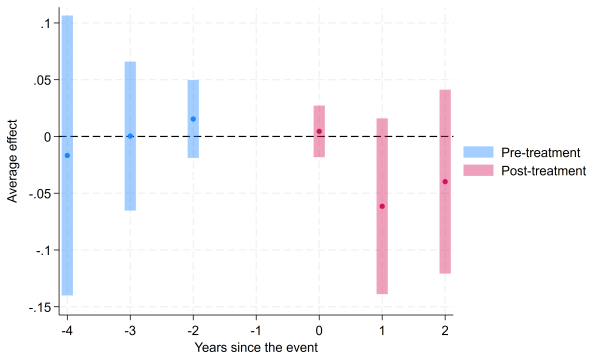
(b) Acquirer Effect



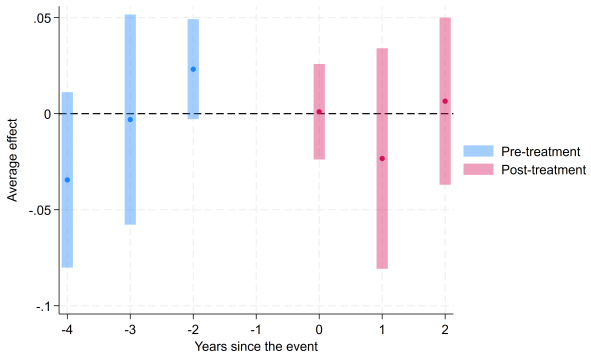
(c) Combined Effect

Notes: Estimates are derived from a Callaway and Sant'Anna (2021) difference-in-differences framework using hospital-year-level data aggregated from Medicaid TAF claims. The sample is a propensity-score-matched sample constructed via five-nearest-neighbor (5-NN) matching: each merger-exposed hospital is matched to its five nearest never-treated hospitals on a probit propensity score estimated from baseline (2016) log Medicaid births, log hospital beds, not-for-profit status, rural location, system-level HRR HHI, and county-level log median household income, bachelor's-degree share, and unemployment rate. Treatment is defined as the year a hospital is first exposed to a merger during the period 2016–2021. The control group consists of matched never-treated hospitals. We cluster standard errors at the hospital level.

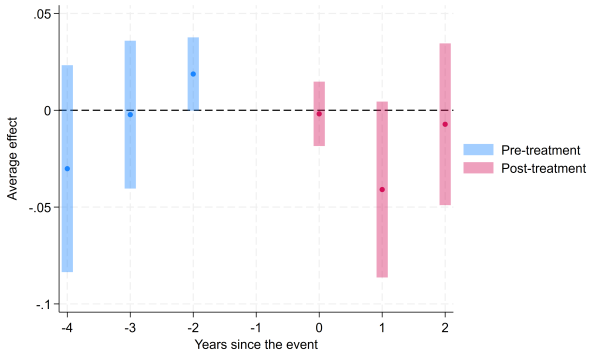
Figure 6: Hospital Physical Capital – log(Hospital Beds): Target, Acquirer, & Combined Effects



(a) Target Effect



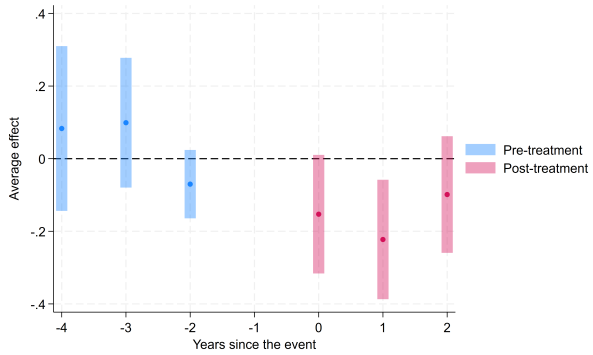
(b) Acquirer Effect



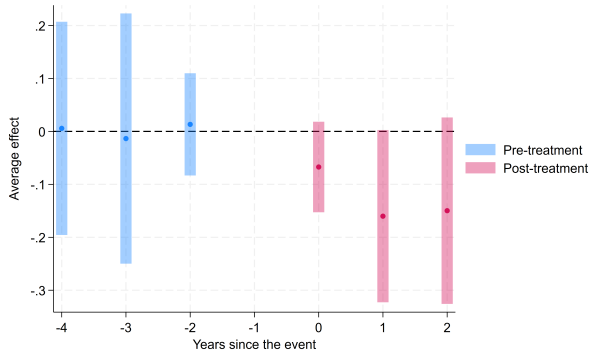
(c) Combined Effect

Notes: Estimates are derived from a Callaway and Sant'Anna (2021) difference-in-differences framework using hospital-year data. The outcome is the natural logarithm of hospital beds from the American Hospital Association dataset. The sample is our five-nearest-neighbor (5-NN) propensity-score-matched sample, in which each merger-exposed hospital is matched to its five nearest never-treated hospitals on baseline (2016) volume, capacity, ownership, rurality, market concentration, and county socioeconomic characteristics. Treatment is defined as the year a hospital is first exposed to a merger during 2016–2021. The control group consists of matched never-treated hospitals. We cluster standard errors at the hospital level.

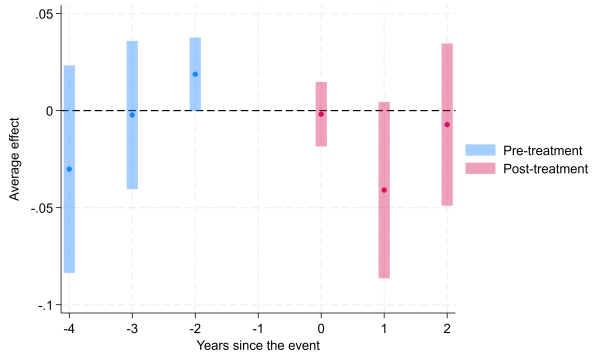
Figure 7: L&D Staffing Capacity – log(Unique Admitting NPIs): Target, Acquirer, & Combined Effects



(a) Target Effect



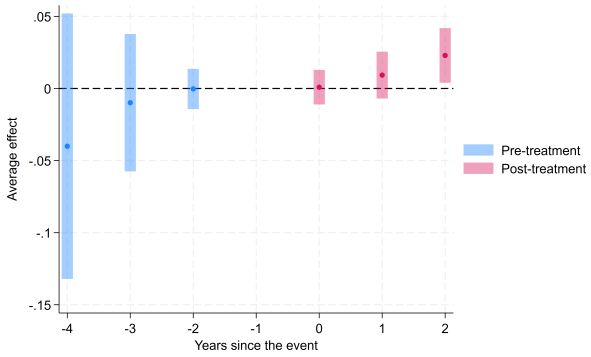
(b) Acquirer Effect



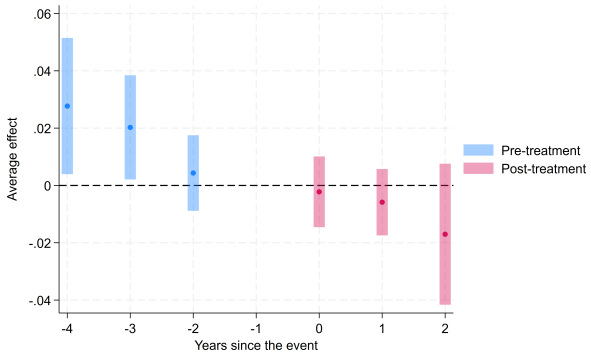
(c) Combined Effect

Notes: Estimates are derived from a Callaway and Sant'Anna (2021) difference-in-differences framework using hospital-year-level data aggregated from Medicaid TAF claims. The outcome variable is the natural logarithm of the number of unique admitting National Provider Identifiers (NPIs), reflecting hospital-level OB/GYN staffing capacity. The sample is our five-nearest-neighbor (5-NN) propensity-score-matched sample, in which each merger-exposed hospital is matched to its five nearest never-treated hospitals on baseline (2016) volume, capacity, ownership, rurality, market concentration, and county socioeconomic characteristics. Treatment is defined as the year a hospital is first exposed to a merger during 2016–2021. The control group consists of matched never-treated hospitals. We cluster standard errors at the hospital level.

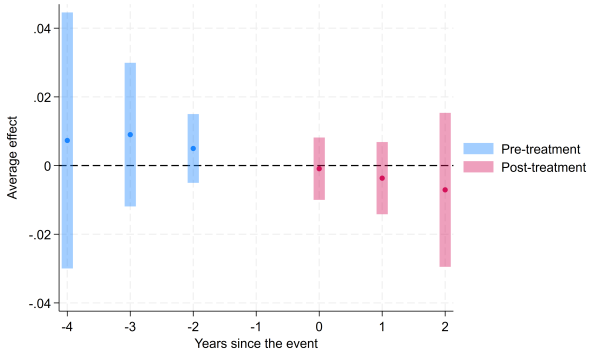
Figure 8: L&D Care Quality – C-section: Target, Acquirer, & Combined Effects



(a) Target Effect



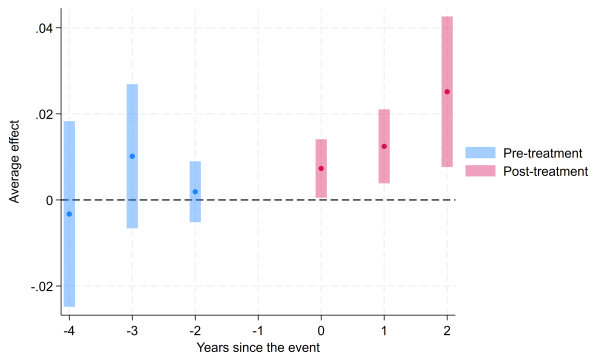
(b) Acquirer Effect



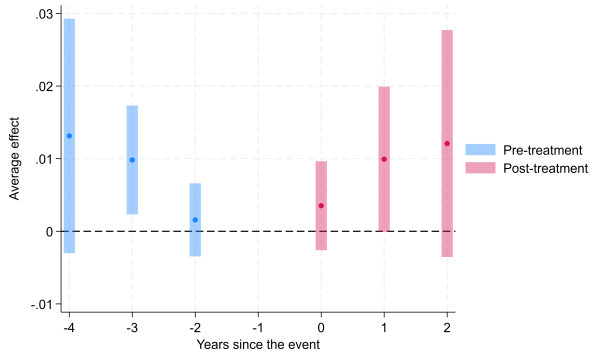
(c) Combined Effect

Notes: Estimates are derived from a Callaway and Sant'Anna (2021) difference-in-differences framework using Medicaid TAF claims-derived L&D admission-level data. The outcome variable is the rate of Cesarean (C-section) deliveries among all live births, a common indicator of labor and delivery (L&D) care quality and practice intensity. Births are restricted to hospitals in our five-nearest-neighbor (5-NN) propensity-score-matched sample, in which each merger-exposed hospital is matched to its five nearest never-treated hospitals on baseline (2016) volume, capacity, ownership, rurality, market concentration, and county socioeconomic characteristics. Treatment is defined as the year a hospital is first exposed to a merger during 2016–2021. The control group consists of births in matched never-treated hospitals. We cluster standard errors at the hospital level.

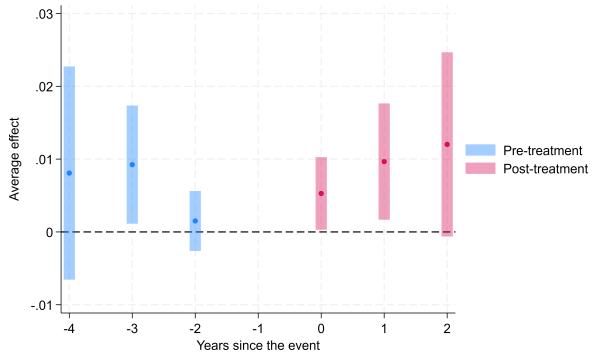
Figure 9: L&D Care Quality – C-section Without Medical Indication: Target, Acquirer, & Combined Effects



(a) Target Effect



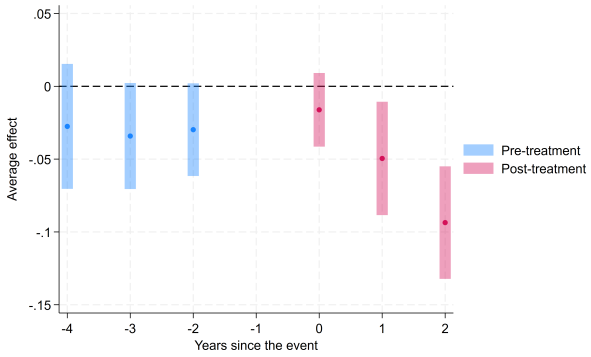
(b) Acquirer Effect



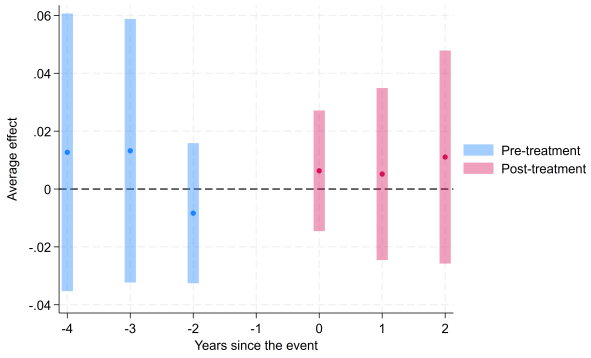
(c) Combined Effect

Notes: Estimates are derived from a Callaway and Sant'Anna (2021) difference-in-differences framework using Medicaid TAF claims-derived L&D admission-level data. The outcome variable is the rate of Cesarean (C-section) deliveries without medical indication among all live births. Births are restricted to hospitals in our five-nearest-neighbor (5-NN) propensity-score-matched sample, in which each merger-exposed hospital is matched to its five nearest never-treated hospitals on baseline (2016) volume, capacity, ownership, rurality, market concentration, and county socioeconomic characteristics. Treatment is defined as the year a hospital is first exposed to a merger during 2016–2021. The control group consists of births in matched never-treated hospitals. We cluster standard errors at the hospital level.

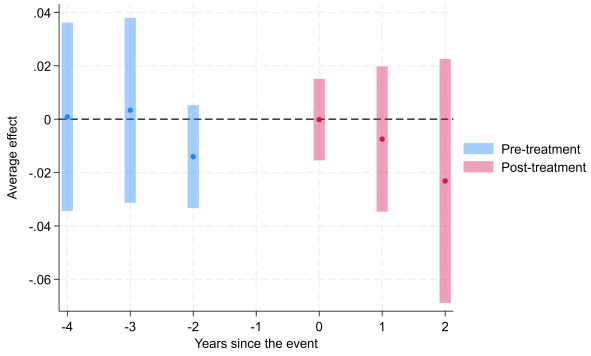
Figure 10: L&D Care Quality – Induced Labor: Target, Acquirer, & Combined Effects



(a) Target Effect



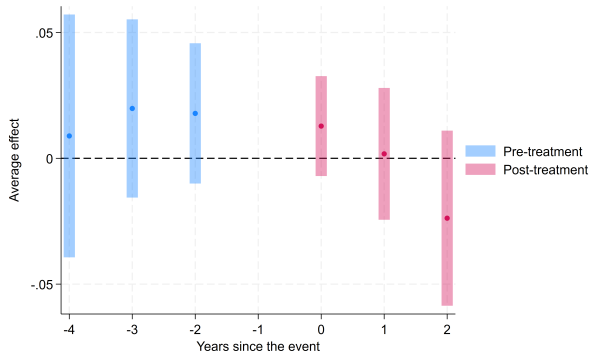
(b) Acquirer Effect



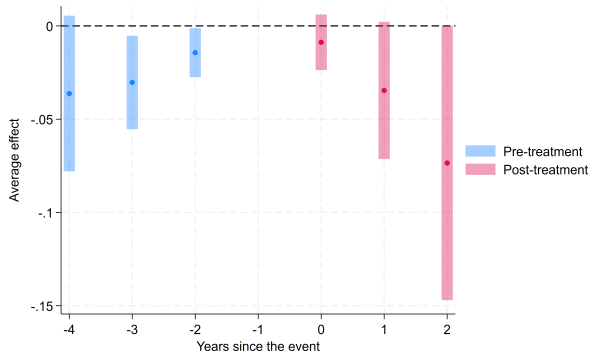
(c) Combined Effect

Notes: Estimates are derived from a Callaway and Sant'Anna (2021) difference-in-differences framework using Medicaid TAF claims-derived L&D admission-level data. The outcome variable is the rate of induced labor among all live births, an indicator of L&D practice patterns and care management intensity. Births are restricted to hospitals in our five-nearest-neighbor (5-NN) propensity-score-matched sample, in which each merger-exposed hospital is matched to its five nearest never-treated hospitals on baseline (2016) volume, capacity, ownership, rurality, market concentration, and county socioeconomic characteristics. Treatment is defined as the year a hospital is first exposed to a merger during 2016–2021. The control group consists of births in matched never-treated hospitals. We cluster standard errors at the hospital level.

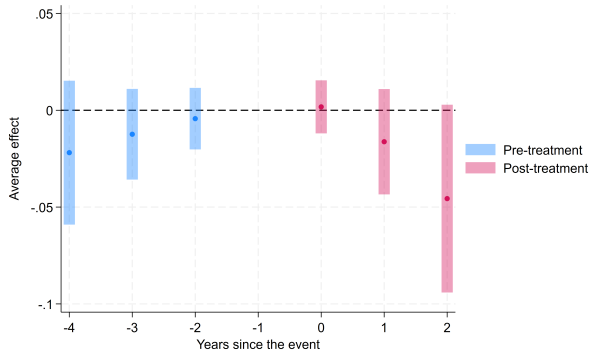
Figure 11: L&D Care Quality – Maternal Complications: Target, Acquirer, & Combined Effects



(a) Target Effect



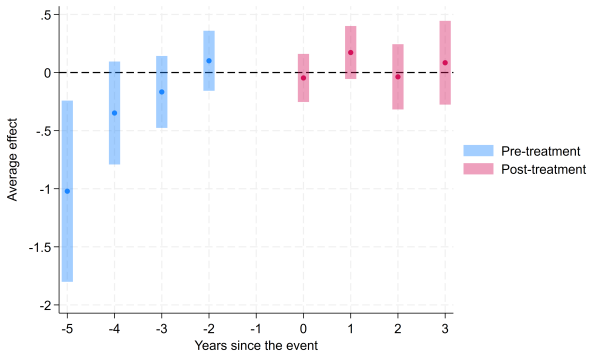
(b) Acquirer Effect



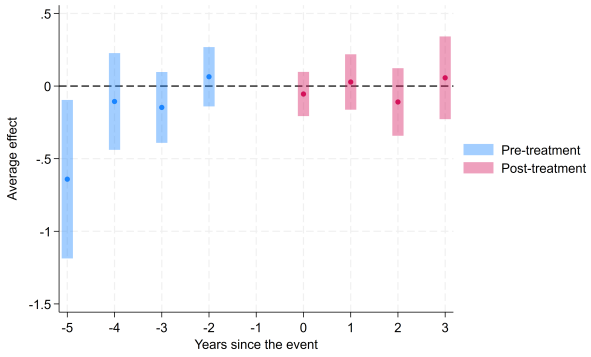
(c) Combined Effect

Notes: Estimates are derived from a Callaway and Sant'Anna (2021) difference-in-differences framework. The outcome variable is the rate of maternal complications among all live births. Births are restricted to hospitals in our five-nearest-neighbor (5-NN) propensity-score-matched sample, in which each merger-exposed hospital is matched to its five nearest never-treated hospitals on baseline (2016) volume, capacity, ownership, rurality, market concentration, and county socioeconomic characteristics. Treatment is the year a hospital is first exposed to a merger during 2016–2021. The control group consists of births in matched never-treated hospitals. We cluster standard errors at the hospital level. The maternal complication variable is binary and equal to 1 if a claim contains any of the following ICD-10-CM codes, per AAPC (2025) and World Health Organization (2019): `O60–O77` (complications of labor and delivery). This code range captures diagnoses including preterm labor (`O60`), failed induction of labor (`O61`), abnormalities of forces of labor (`O62`), long labor (`O63`), obstructed labor due to fetal malposition/malpresentation (`O64`) or maternal pelvic abnormality (`O65`), other obstructed labor (`O66`), intrapartum hemorrhage (`O67`), abnormalities of fetal acid-base balance (`O68`), umbilical cord complications (`O69`), perineal laceration during delivery (`O70`), other obstetric trauma (`O71`), postpartum hemorrhage (`O72`), retained placenta and membranes without hemorrhage (`O73`), complications of anesthesia during labor and delivery (`O74`), other complications of labor and delivery (`O75`), abnormal fetal heart rate and rhythm complicating labor and delivery (`O76`), and other fetal stress complicating labor and delivery (`O77`).

Figure 12: County-Level Fertility – Medicaid births per 1,000 Women: Acquirer Effects (Overall & Medicaid)



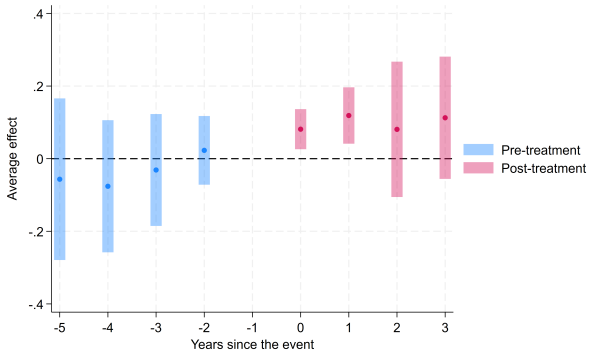
(a) Overall (All Payers)



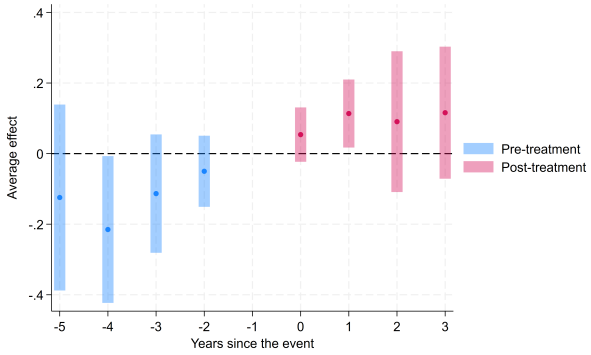
(b) Medicaid

Notes: Event-study estimates from Callaway and Sant'Anna (2021) difference-in-differences using county-year data. Outcomes are births per 1,000 women of reproductive age (top: all payers; bottom: Medicaid). "Acquirer effects" reflect exposure based on counties that have at least one hospital that is part of an acquiring system. Treatment is the first year a county is exposed to an acquirer hospital during 2016–2021. Never-treated counties and pre-treatment periods for treated counties serve as controls. We cluster standard errors at the county level.

Figure 13: County-Level Care Process – Prenatal Visits: Acquirer Effects (Overall & Medicaid)



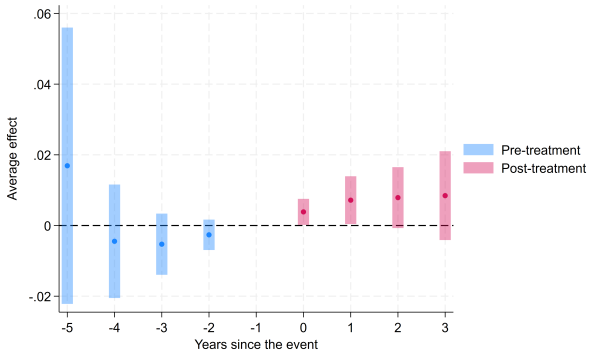
(a) Overall (All Payers)



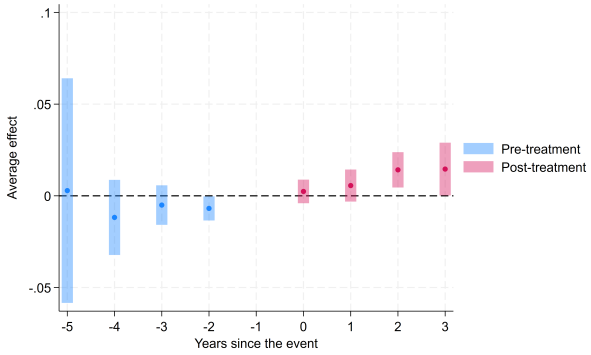
(b) Medicaid

Notes: Event-study estimates from Callaway and Sant'Anna (2021) difference-in-differences using birth data. The outcome is the number of prenatal care visits per birth, reported for all payers (top) and for Medicaid-covered births (bottom). "Acquirer effects" reflect exposure based on counties served by hospitals that become part of an acquiring system. Treatment is the first year a county is exposed to an acquirer hospital during 2016–2021. Births in never-treated counties and births during pre-treatment periods for treated counties serve as controls. We cluster standard errors at the county level.

Figure 14: County-Level Access – Same-County Birth: Acquirer Effects (Overall & Medicaid)



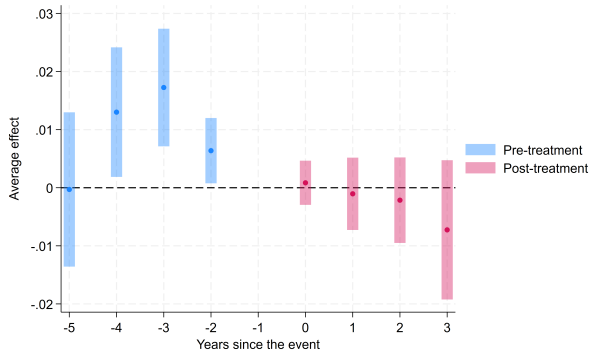
(a) Overall (All Payers)



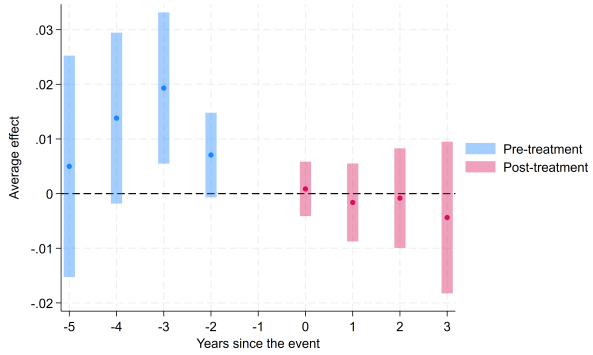
(b) Medicaid

Notes: Event-study estimates from Callaway and Sant'Anna (2021) difference-in-differences using birth data. The outcome is the share of births occurring in the mother's county of residence ("same-county birth"), reported separately for all payers (top) and Medicaid (bottom). "Acquirer effects" reflect exposure based on counties served by hospitals that become part of an acquiring system. Treatment is the first year a county is exposed to an acquirer hospital during 2016–2021. Births in never-treated counties and births during pre-treatment periods for treated counties serve as controls. We cluster standard errors at the county level.

Figure 15: County-Level Care Setting – Physician-Attended Births: Acquirer Effects (Overall & Medicaid)



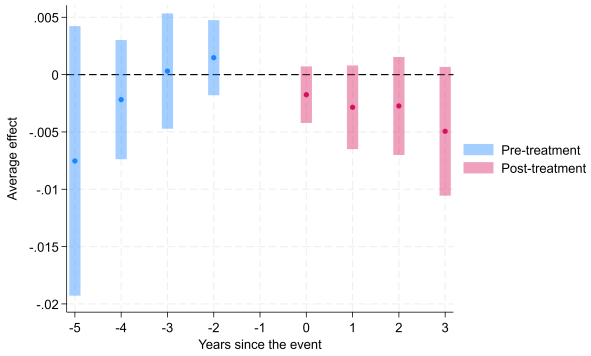
(a) Overall (All Payers)



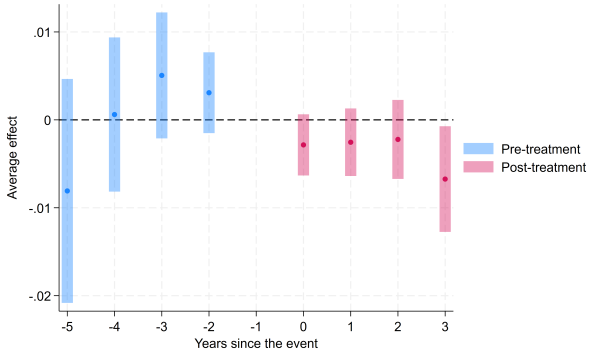
(b) Medicaid

Notes: Event-study estimates from Callaway and Sant'Anna (2021) difference-in-differences using birth data. The outcome is the share of live births attended by a physician, as opposed to certified nurse-midwife or other attendant, reported for all payers (top) and Medicaid (bottom). "Acquirer effects" reflect exposure based on counties served by hospitals that become part of an acquiring system. Treatment is the first year a county is exposed to an acquirer hospital during 2016–2021. Births in never-treated counties and births during pre-treatment periods for treated counties serve as controls. We cluster standard errors at the county level.

Figure 16: County-Level L&D Care Quality – C-section Rate: Acquirer Effects (Overall & Medicaid)



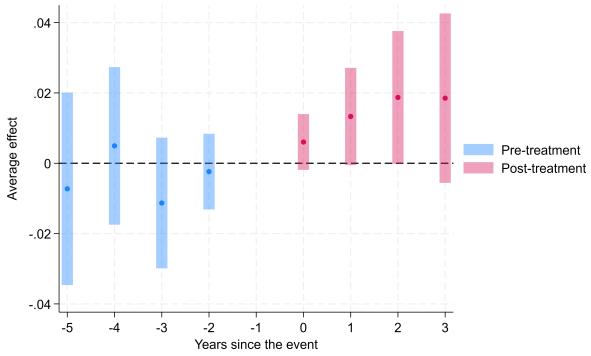
(a) Overall (All Payers)



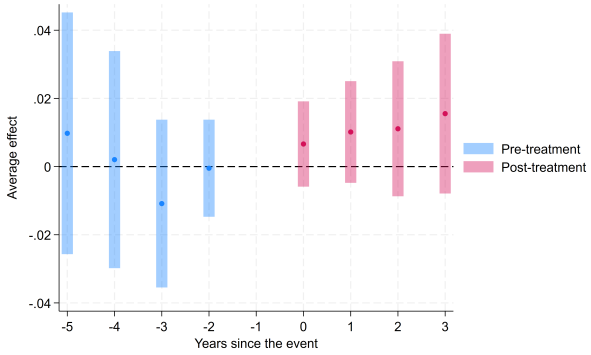
(b) Medicaid

Notes: Event-study estimates from Callaway and Sant'Anna (2021) difference-in-differences using birth data. The outcome is the share of live births delivered by Cesarean section (C-section), reported for all payers (top) and for Medicaid-covered births (bottom). "Acquirer effects" reflect exposure based on counties served by hospitals that become part of an acquiring system. Treatment is the first year a county is exposed to an acquirer hospital during 2016–2021. Births in never-treated counties and births during pre-treatment periods for treated counties serve as controls. We cluster standard errors at the county level.

Figure 17: County-Level Outcomes – Five-Minute Apgar Score: Acquirer Effects (Overall & Medicaid)



(a) Overall (All Payers)



(b) Medicaid

Notes: Event-study estimates from Callaway and Sant'Anna (2021) difference-in-differences using birth data. The outcome is the average five-minute Apgar score among live births, reported for all payers (top) and Medicaid-covered births (bottom). "Acquirer effects" reflect exposure based on counties served by hospitals that become part of an acquiring system. Treatment is the first year a county is exposed to an acquirer hospital during 2016–2021. Births in never-treated counties and births during pre-treatment periods for treated counties serve as controls. We cluster standard errors at the county level.

Appendix

Table A1: Data Sources Used for Analyzing the Impact of Hospital Mergers on Birth Outcomes

Data Source	**Years**	**Description**
Medicaid TAF Claims	2016–2021	Claims to build obstetric cohorts and outcomes; includes provider identifiers for linking to facilities/providers.
American Hospital Association (AHA) Annual Survey	2016–2022	Hospital characteristics, including ownership, beds, teaching status, and system affiliation, used to classify facilities/systems.
Medicare Provider of Service (POS) Files	2016–2022	Facility roster with ownership/control type, addresses, and CCNs; used for hospital attributes and CCN validation.
NVSS Natality (Birth) Data	2016–2022	Vital statistics on births, including maternal and infant characteristics and outcomes, used for general equilibrium analyses.
OpenIntro County Complete Data	2016	County-level education measures used to construct baseline socioeconomic controls; matched to analytic files using county FIPS codes. Source: OpenIntro.
Bureau of Labor Statistics Local Area Unemployment Statistics (BLS LAUS)	2016	County-level unemployment measures used to construct baseline local labor market controls; matched to analytic files using county FIPS codes. Source: BLS LAUS.
Census Small Area Income and Poverty Estimates (SAIPE)	2016	County-level household income measures used to construct baseline socioeconomic controls; matched to analytic files using county FIPS codes. Source: Census SAIPE 2016.
USDA ERS Urban/Rural Classifications (RUCC / UIC / RUCA)	Harmonized to study period	Rural / urban measures used to stratify counties/areas; matched via county FIPS.

Abbreviations: AHA = American Hospital Association; BLS LAUS = Bureau of Labor Statistics Local Area Unemployment Statistics; CCN = CMS Certification Number; FIPS = Federal Information Processing Series code; NVSS = National Vital Statistics System; POS = Provider of Service; RUCC/UIC/RUCA = USDA ERS rural/urban classification schemes; SAIPE = Small Area Income and Poverty Estimates; TAF = Transformed Medicaid Statistical Information System Analytic Files.

Table A2: Algorithms for Identifying Births, Cesarean Deliveries, Induced Labor, Maternal Complications, and Medically Indicated Cesarean Deliveries in Claims

Condition	DRG Codes	Diagnosis Codes (ICD-10-CM)	Procedure Codes (ICD-10-PCS)
Birth (any delivery encounter)	765–768, 774, 783–788, 795–798, 805–807	Curated list of delivery/obstetric codes (e.g., O10–O9A, Z37x, Z38x, Z39x)	0Q820ZZ, 0Q823ZZ, 0Q824ZZ, 0Q830ZZ, 0Q833ZZ, 0Q834ZZ, 0U7C7ZZ, 0W8NXZZ, 10D00Z0–10D07Z8, 10E0XZZ, 10S07ZZ, 10S0XZZ
Cesarean Delivery (C-section)	765–766, 783–788, 540 (APR-DRG)	O82, O75.82	10D00Z0–10D00Z2
C-section with medical indication	Same as C-section algorithm above	C-section delivery with at least one diagnosis code indicating a potential medical indication for cesarean delivery: malpresentation, disproportion, obstructed labor, fetal distress, cord/vasa complications, placenta previa or placental abruption, uterine rupture, placenta accreta spectrum, multiple gestation, fetal demise, preterm labor, prior uterine scar or prior C-section, or gestational age below 37 weeks. Codes matched include prefixes O32, O33, O64, O65, O66, O68, O69, O44, O45, O711, O432, O433, O364, O365, O30, O31, O60, O342, and Z3A00–Z3A36.	Same as C-section algorithm above
C-section without clear medical indication	Same as C-section algorithm above	C-section delivery with no diagnosis code matching the medical-indication list above. Operationally, this flag equals 1 when a C-section is identified and no indication code is found across diagnosis fields.	Same as C-section algorithm above
Induced Labor	–	O610, O611, O618, O619	Oxytocin 3E033VJ; prostaglandins/cervical ripening 3E0P7GC; mechanical dilation 0U7C7ZZ (legacy 0U7C7DZ)
Maternal Complications (labor & delivery)	–	O60–O77 (matched via pattern O6x or O7x through O77)	–

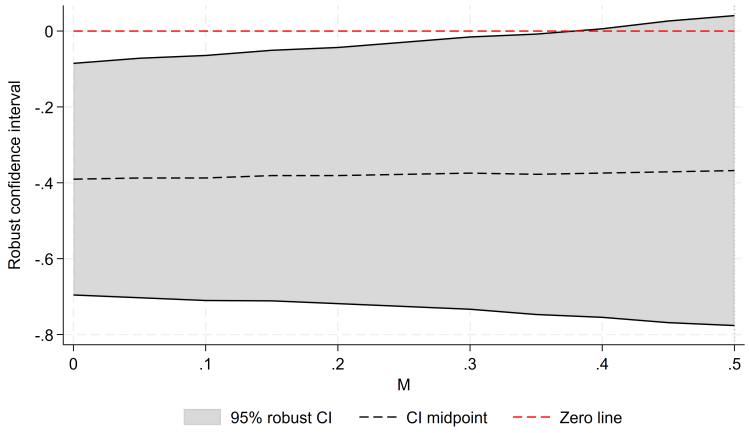
Note: Codes are matched case-insensitively across diagnosis (DGNS_CD_1–DGNS_CD_12), procedure (ICD_PRCDR_CD1–ICD_PRCDR_CD6), and DRG (CLM_DRG_CD) fields. Cesarean deliveries are first identified using ICD-10-PCS procedure codes 10D00Z0–10D00Z2, DRG codes 765–766, 783–788, or APR-DRG 540, and supportive diagnosis codes O82 or O75.82. C-sections with medical indication are defined as C-section admissions with at least one diagnosis code indicating a potential clinical indication, including malpresentation, obstructed labor, fetal distress, placental conditions, uterine rupture, multiple gestation, fetal demise, preterm labor, prior uterine scar or prior C-section, or gestational age below 37 weeks. C-sections without clear medical indication are defined as C-section admissions without any of these indication codes.
Maternal complications are defined using ICD-10-CM codes O60–O77 (complications of labor and delivery) and are flagged if any diagnosis code matches the regular expression ^O(6[0-9]|7[0-7]). This code range captures diagnoses including preterm labor (O60), failed induction of labor (O61), abnormalities of forces of labor (O62), long labor (O63), obstructed labor due to fetal malposition/malpresentation (O64) or maternal pelvic abnormality (O65), other obstructed labor (O66), intrapartum hemorrhage (O67), abnormalities of fetal acid-base balance (O68), umbilical cord complications (O69), perineal laceration during delivery (O70), other obstetric trauma (O71), postpartum hemorrhage (O72), retained placenta and membranes without hemorrhage (O73), complications of anesthesia during labor and delivery (O74), other complications of labor and delivery (O75), abnormal fetal heart rate and rhythm complicating labor and delivery (O76), and other fetal stress complicating labor and delivery (O77).

Table A3: Robustness to Control Group Definition: Never-Treated vs Not-Yet-Treated Hospitals

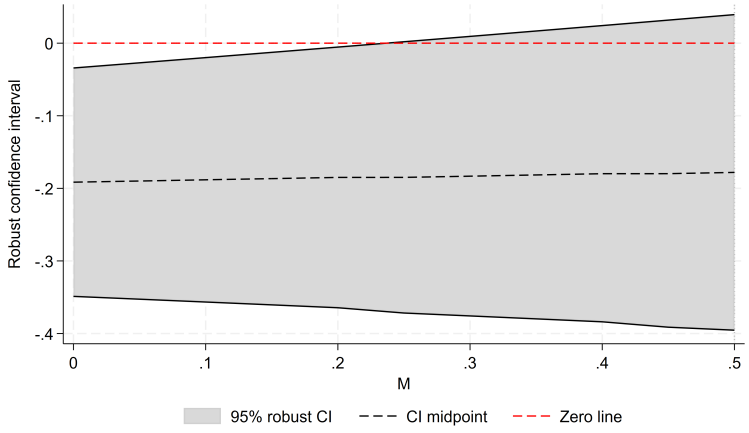
	Target	**Acquirer**	**Consolidated**
Panel A. Hospital-year outcomes (aggregated post-merger effect; N = hospital-year observations)			
Sample N (hospital-year observations)	*1,598*	*2,179*	*3,442*
Control group: Never-treated hospitals			
Volume: log(Births)	−0.459*** (0.150)	−0.194 (0.118)	−0.204** (0.097)
Physical Capacity: log(Beds)	−0.032 (0.025)	−0.005 (0.017)	−0.017 (0.014)
Staffing: log(Unique Admitting NPIs)	−0.158** (0.068)	−0.126** (0.062)	−0.110** (0.050)
Control group: Not-yet-treated hospitals			
Volume: log(Births)	−0.467*** (0.144)	−0.188 (0.119)	−0.215** (0.095)
Physical Capacity: log(Beds)	−0.033 (0.025)	−0.007 (0.017)	−0.017 (0.014)
Staffing: log(Unique Admitting NPIs)	−0.152** (0.066)	−0.124** (0.062)	−0.107** (0.049)

Notes: Entries report aggregated post-merger effects from Callaway & Sant'Anna (2021) difference-in-differences event-study models estimated using `csdid` with `method(dripw)` and the `long2` option on the propensity-score-matched sample. Standard errors are cluster-robust at the hospital level and reported in parentheses. **Never-treated** controls are hospitals that are never exposed to the relevant treatment during the sample window. **Not-yet-treated** controls include hospitals that have not yet been treated by a given year but may be treated later, and are excluded from the control group once they become treated. *Legend:* * ($p < 0.10$); ** ($p < 0.05$); *** ($p < 0.01$).

Figure A1: Rambachan and Roth (2023) Honest DiD Sensitivity Analysis: Hospital L&D Volume, log(Births)



(a) Target Effect

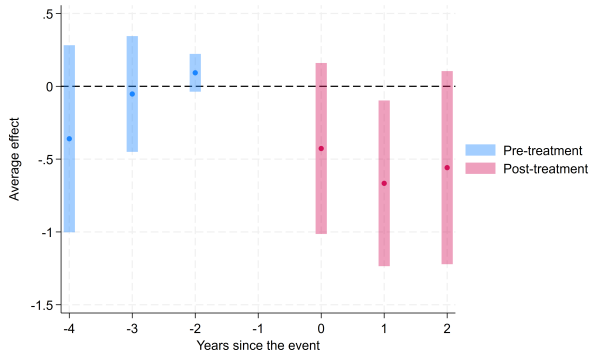


(b) Combined Effect

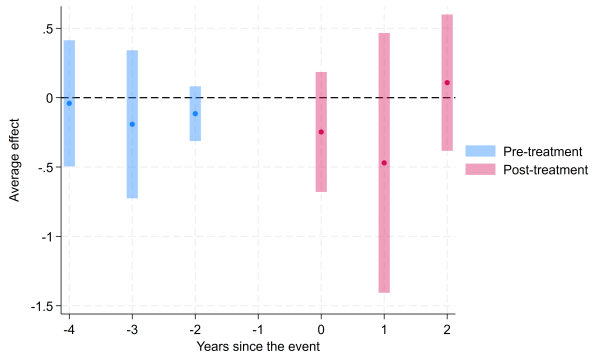
Notes: This figure reports Rambachan and Roth (2023) HonestDiD sensitivity analyses for the target and combined effects on log Medicaid births shown in Figure 5. The original confidence interval reproduces the Callaway and Sant'Anna (2021) post-treatment estimate from the five-nearest-neighbor propensity-score-matched sample. Subsequent intervals allow post-treatment violations of parallel trends up to $\bar{M}$ times the maximum pre-treatment violation, for $\bar{M} \in [0, 0.5]$. The breakdown value is the largest $\bar{M}$ for which the robust confidence set excludes zero.

Robustness—Exclude 2016–2021 Medicaid Expansion States

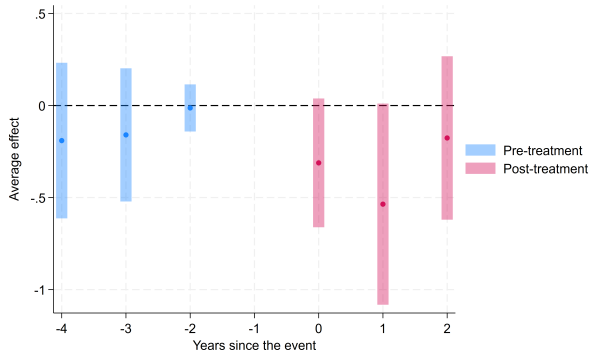
Figure A2: Robustness (Non-Expansion States): L&D Volume — log(Births) (Target, Acquirer, & Consolidation)



(a) Target Effect (Non-Expansion)



(b) Acquirer Effect (Non-Expansion)

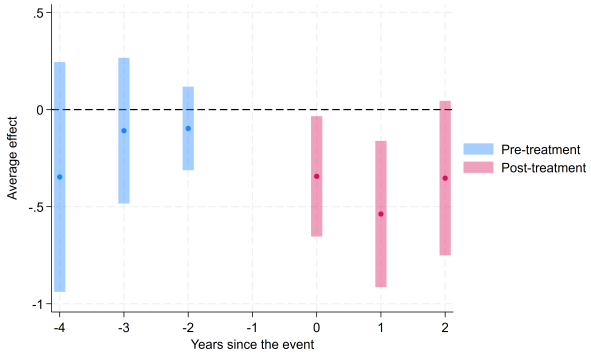


(c) Consolidation Effect (Non-Expansion)

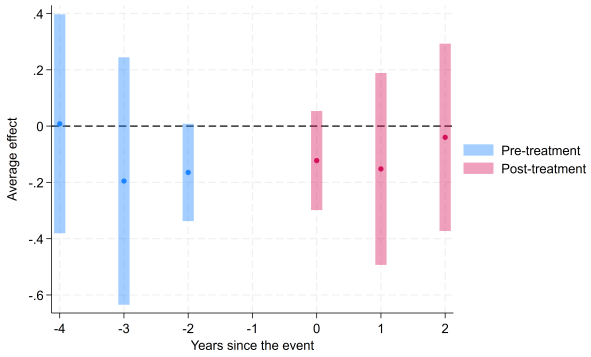
Note: Excludes states that expanded Medicaid between 2016–2021: ME, MT, LA, VA, ID, UT, NE, OK, and MO.

Robustness—SUTVA Check

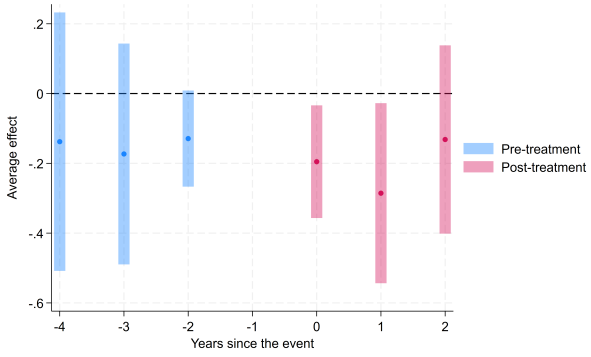
Figure A3: Robustness (SUTVA Check): L&D Volume — log(Births) (Target, Acquirer, & Consolidation)



(a) Target Effect (SUTVA Check)



(b) Acquirer Effect (SUTVA Check)

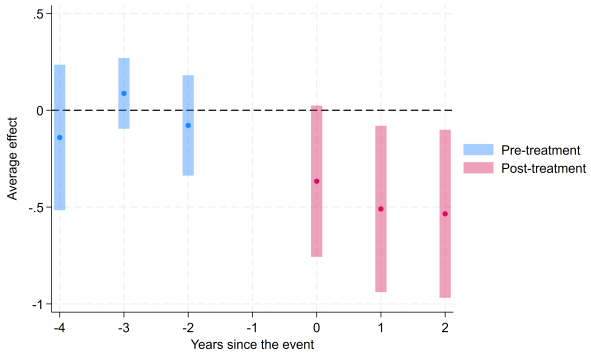


(c) Consolidation Effect (SUTVA Check)

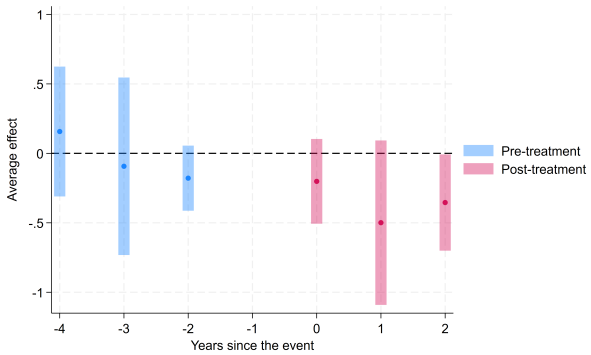
Note: SUTVA check drops never-treated hospitals located in counties where a merger occurred.

Heterogeneity—Upper and Lower Half of HHI

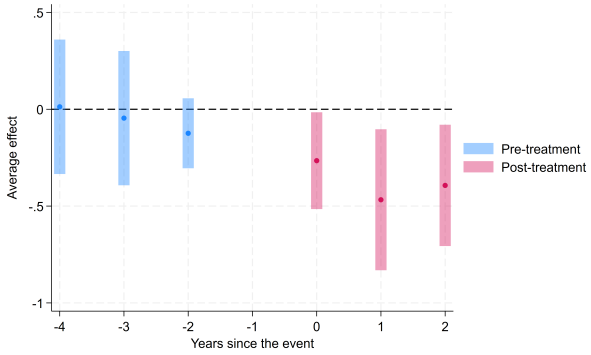
Figure A4: Upper HHI HRRs: L&D Volume — log(Medicaid Births) (Target, Acquirer, & Consolidation)



(a) Target Effect (Upper HHI)



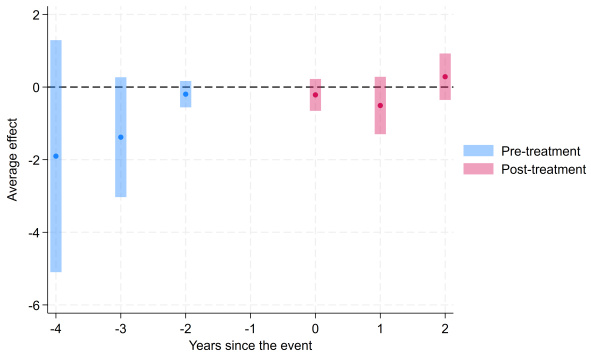
(b) Acquirer Effect (Upper HHI)



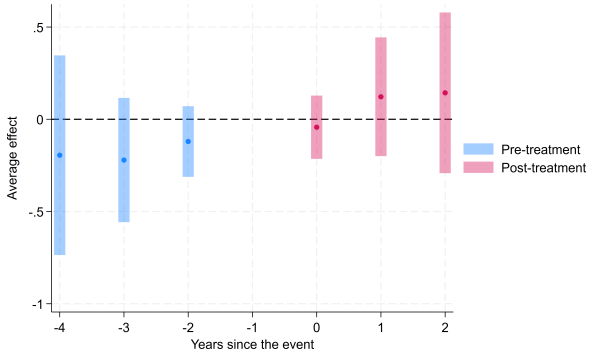
(c) Consolidation Effect (Upper HHI)

Notes: Main Callaway–Sant'Anna (2021) DiD specification. Subsample: HRRs with 2016 hospital system HHI > median. Controls: never-treated and pre-treatment periods. SEs clustered at the hospital level.

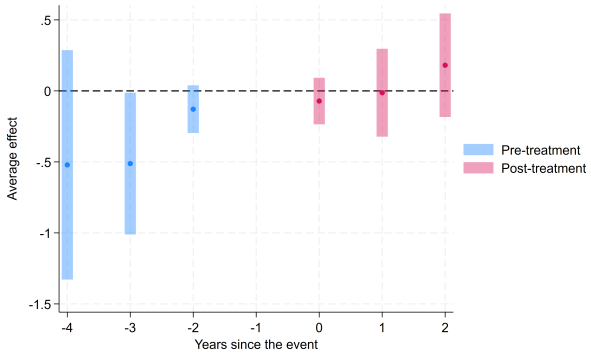
Figure A5: Lower HHI HRRs: L&D Volume — log(Medicaid Births) (Target, Acquirer, & Consolidation)



(a) Target Effect (Lower HHI)



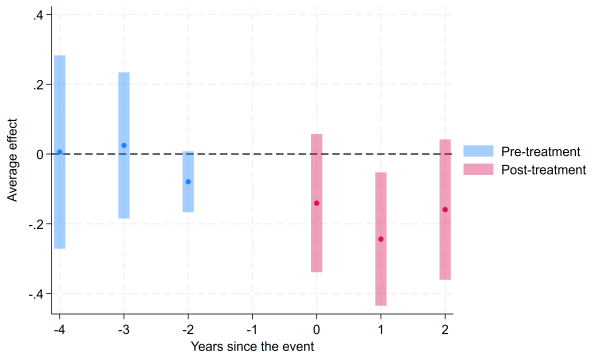
(b) Acquirer Effect (Lower HHI)



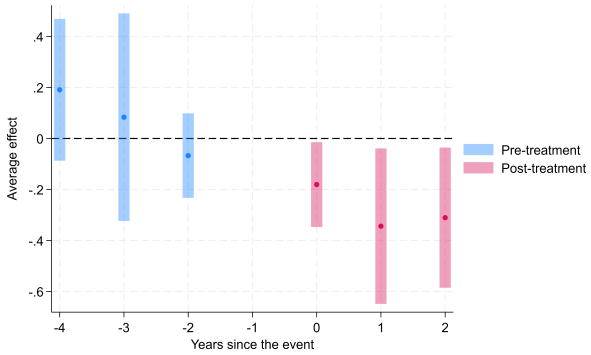
(c) Consolidation Effect (Lower HHI)

Notes: Same DiD setup as Figure A4, but for HRRs with 2016 hospital system HHI ≤ median.

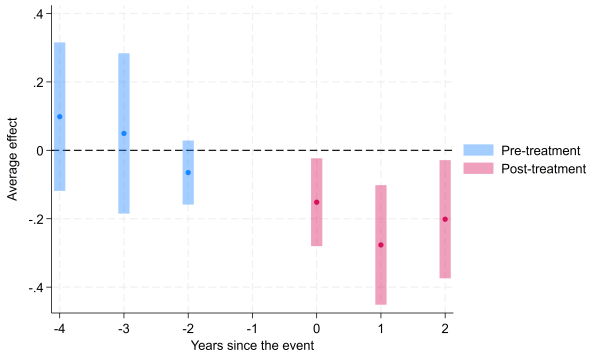
Figure A6: Upper HHI HRRs: L&D Staffing Capacity — log(Unique Admitting NPIs) (Target, Acquirer, & Consolidation)



(a) Target Effect (Upper HHI)



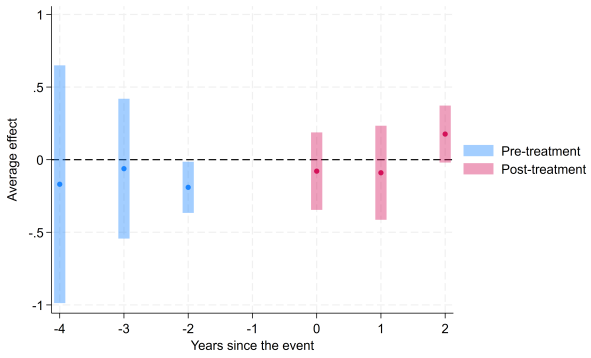
(b) Acquirer Effect (Upper HHI)



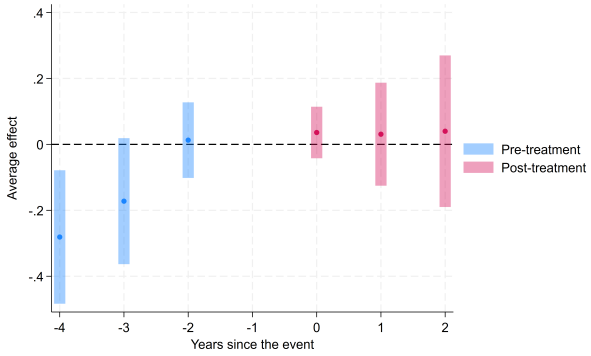
(c) Consolidation Effect (Upper HHI)

Notes: DiD per Callaway–Sant'Anna (2021); outcome is log(unique admitting NPIs) (Medicaid TAF). Subsample: HRRs with 2016 hospital system HHI > median.

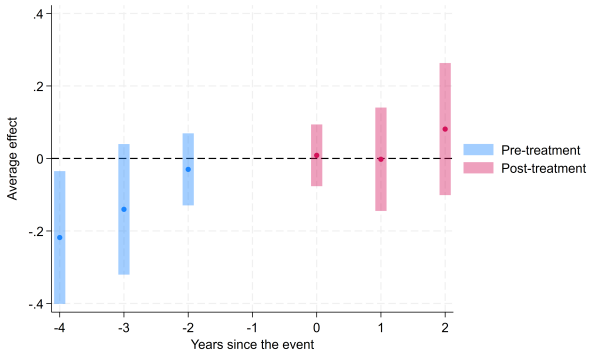
Figure A7: Lower HHI HRRs: L&D Staffing Capacity — log(Unique Admitting NPIs) (Target, Acquirer, & Consolidation)



(a) Target Effect (Lower HHI)



(b) Acquirer Effect (Lower HHI)

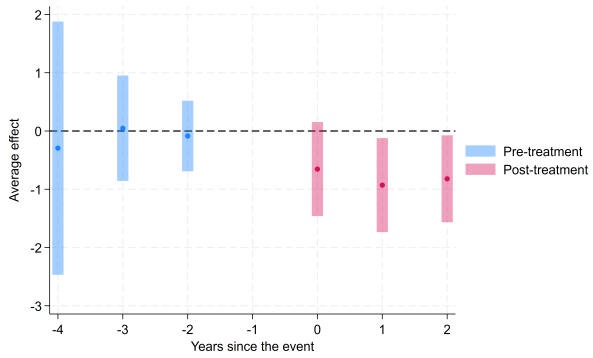


(c) Consolidation Effect (Lower HHI)

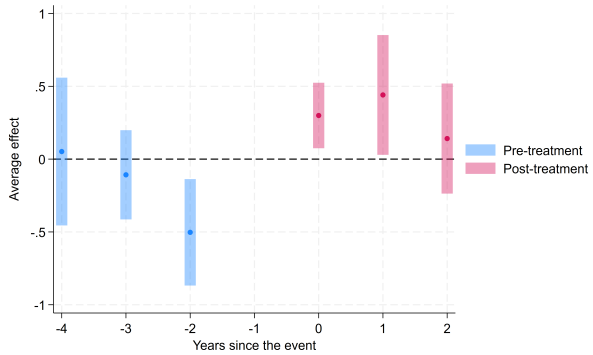
Notes: Same as Figure A6, but for HRRs with 2016 hospital system HHI ≤ median.

Heterogeneity—Urban vs. Rural: L&D Volume

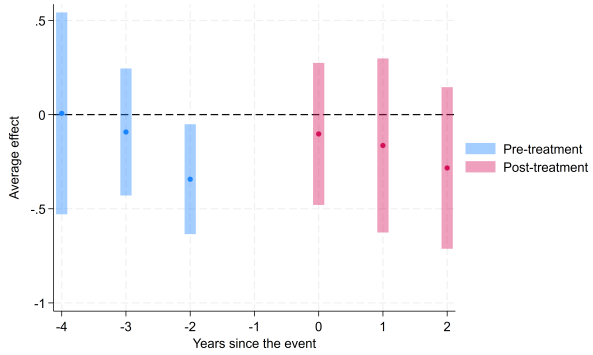
Figure A8: Rural Hospitals: L&D Volume – log(Births) (Target, Acquirer, & Combined Effects)



(a) Target Effect (Rural)



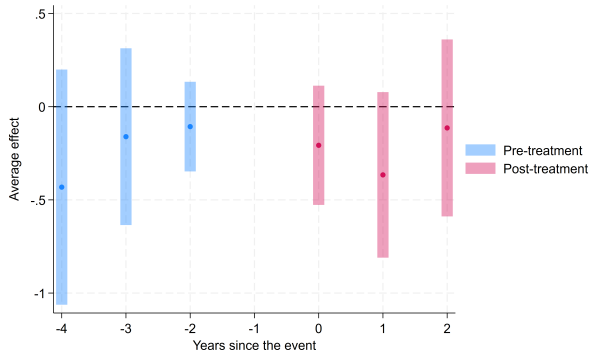
(b) Acquirer Effect (Rural)



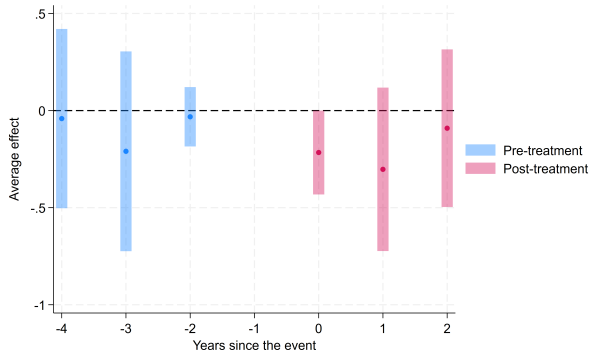
(c) Combined Effect (Rural)

Notes: Estimates come from a Callaway and Sant'Anna (2021) difference-in-differences design using hospital-year data aggregated from Medicaid TAF claims. The sample is our five-nearest-neighbor (5-NN) propensity-score-matched sample restricted to *rural* hospitals per the USDA ERS rural county indicator. Treatment is the first year a hospital is exposed to a within-HRR merger during 2016–2021. Matched never-treated hospitals and pre-treatment periods for treated hospitals serve as controls. We cluster standard errors at the hospital level.

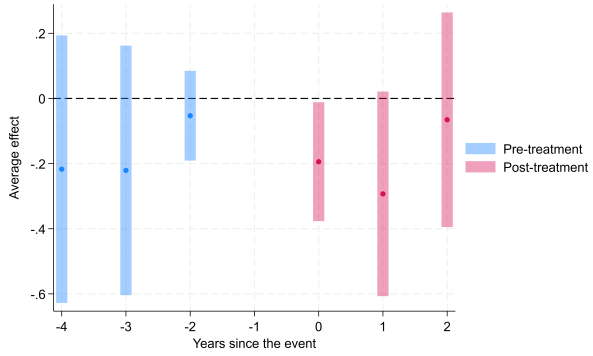
Figure A9: Urban Hospitals: L&D Volume – log(Births) (Target, Acquirer, & Combined Effects)



(a) Target Effect (Urban)



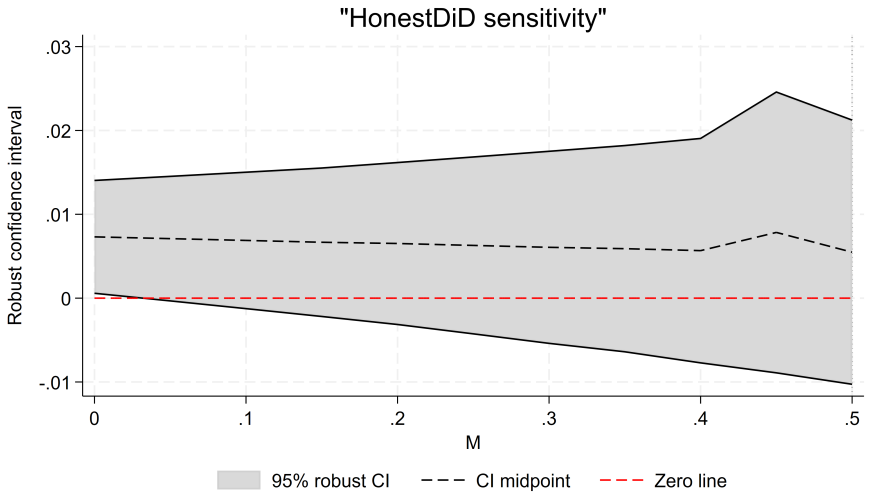
(b) Acquirer Effect (Urban)



(c) Combined Effect (Urban)

Notes: Same specification as Figure A8, restricted to *urban* hospitals in the matched sample. DiD per Callaway and Sant'Anna (2021); treatment defined as first within-HRR merger year (2016–2021); matched never-treated and pre-treatment periods serve as controls; standard errors clustered at the hospital level.

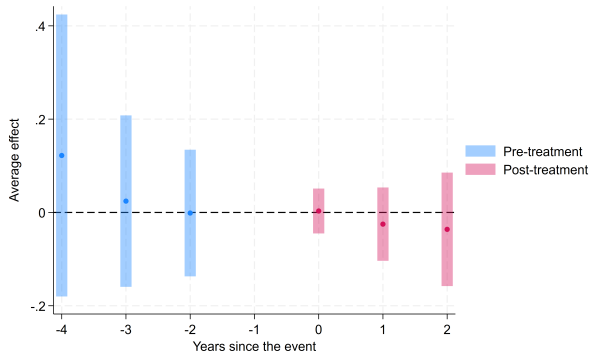
Figure A10: Rambachan and Roth (2023) Honest DiD Sensitivity Analysis: L&D Care Quality, C-Section Without Medical Indication—Target Effect



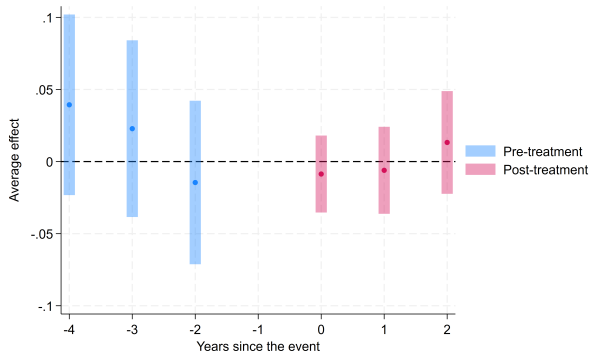
Notes: This figure reports a Rambachan and Roth (2023) HonestDiD sensitivity analysis for the target effect on the rate of Cesarean deliveries without medical indication shown in Figure 9. The original confidence interval reproduces the Callaway and Sant'Anna (2021) post-treatment estimate using Medicaid TAF claims-derived L&D admissions at hospitals in the five-nearest-neighbor propensity-score-matched sample. Subsequent intervals allow post-treatment violations of parallel trends up to $\bar{M}$ times the maximum pre-treatment violation, for $\bar{M} \in [0, 0.5]$. The breakdown value is the largest $\bar{M}$ for which the robust confidence set excludes zero.

Heterogeneity—L&D Care Quality, Rural Hospitals

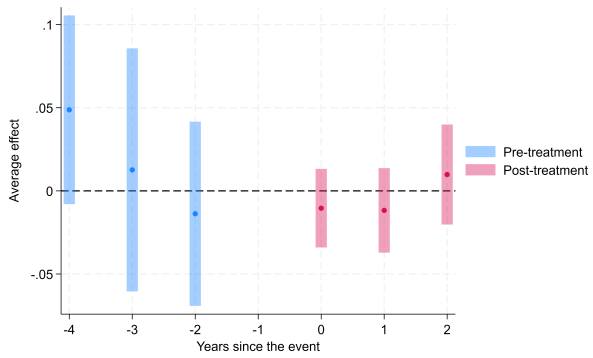
Figure A11: Rural Hospitals: C-section (Target, Acquirer, & Combined Effects)



(a) Target Effect



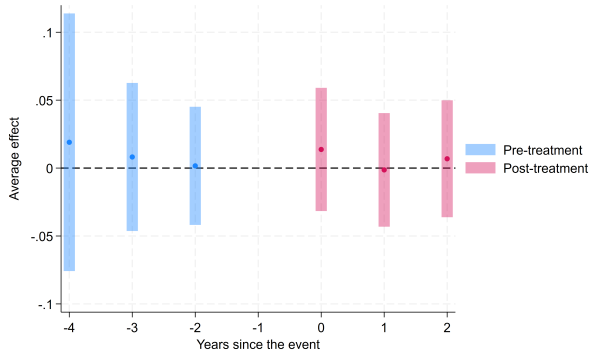
(b) Acquirer Effect



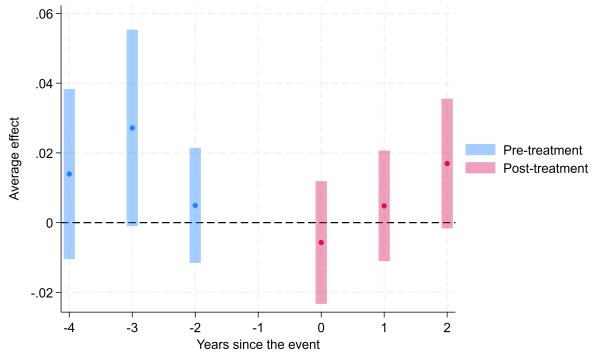
(c) Combined Effect

Notes: Estimates are derived from a Callaway and Sant'Anna (2021) difference-in-differences framework using Medicaid TAF claims-derived L&D admission-level data. The outcome is the rate of Cesarean (C-section) deliveries among all live births. The sample is restricted to *rural* hospitals within our five-nearest-neighbor (5-NN) propensity-score-matched sample, per the USDA ERS rural county indicator. Treatment is defined as the year a hospital is first exposed to a merger during 2016–2021. The control group consists of births in matched never-treated hospitals. We cluster standard errors at the hospital level.

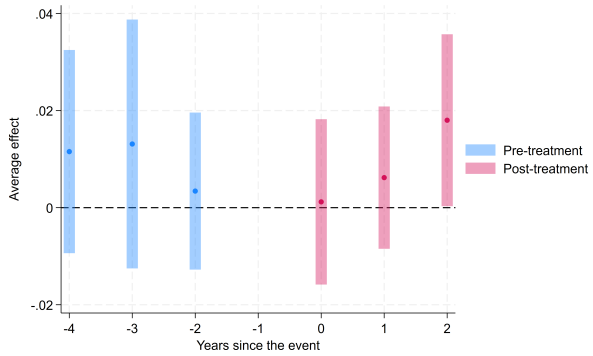
Figure A12: Rural Hospitals: C-section Without Medical Indication (Target, Acquirer, & Combined Effects)



(a) Target Effect



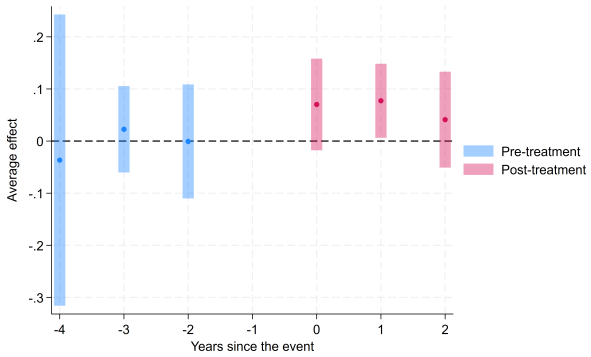
(b) Acquirer Effect



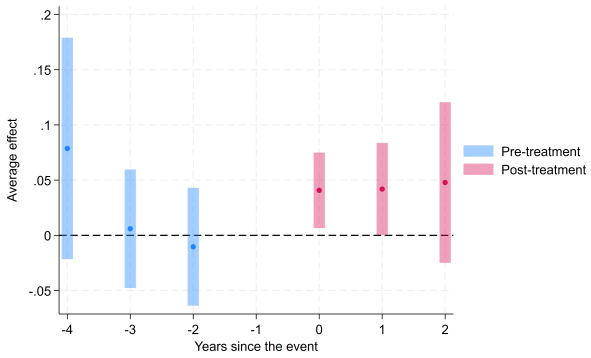
(c) Combined Effect

Notes: Same specification as Figure A11. The outcome is the rate of C-section deliveries without medical indication among all live births.

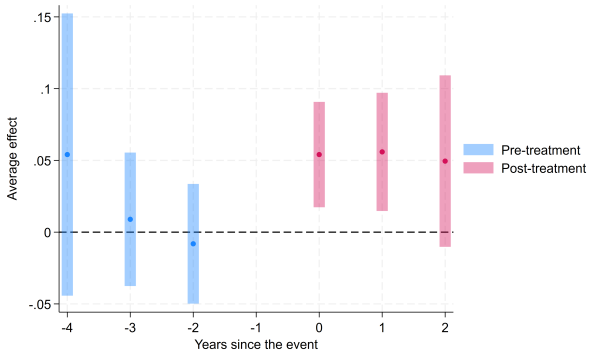
Figure A13: Rural Hospitals: Induced Labor (Target, Acquirer, & Combined Effects)



(a) Target Effect



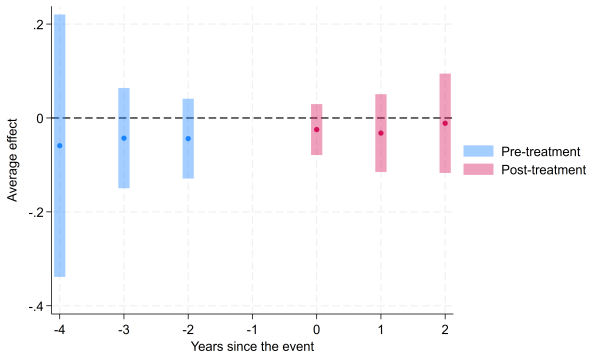
(b) Acquirer Effect



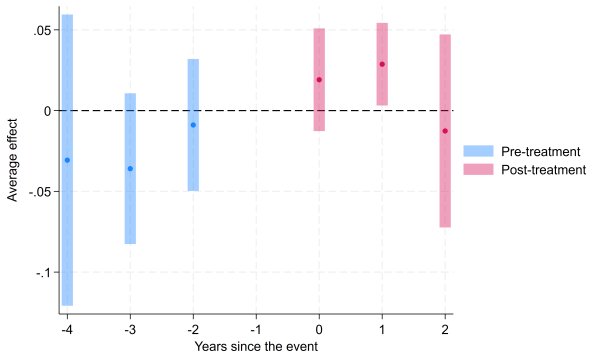
(c) Combined Effect

Notes: Same specification as Figure A11. The outcome is the rate of induced labor among all live births.

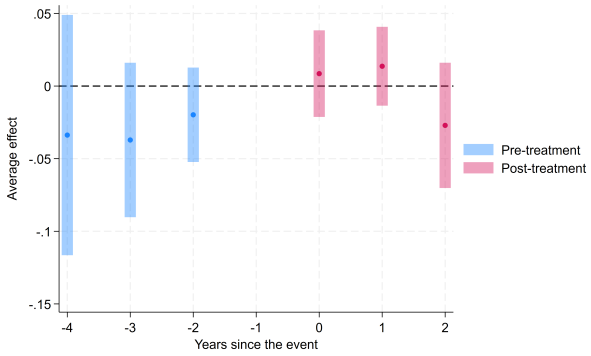
Figure A14: Rural Hospitals: Maternal Complications (Target, Acquirer, & Combined Effects)



(a) Target Effect



(b) Acquirer Effect

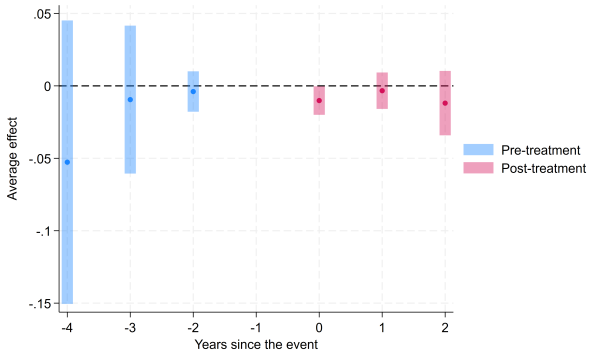


(c) Combined Effect

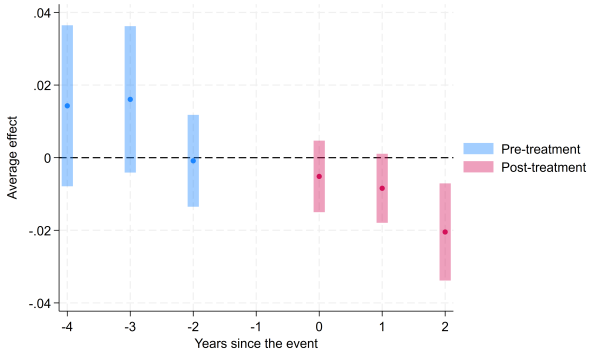
Notes: Same specification as Figure A11. The outcome is the rate of maternal complications (ICD-10-CM O60–O77) among all live births.

Heterogeneity—L&D Care Quality, Urban Hospitals

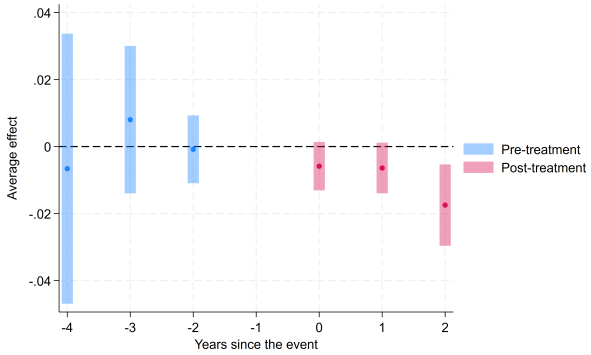
Figure A15: Urban Hospitals: C-section (Target, Acquirer, & Combined Effects)



(a) Target Effect



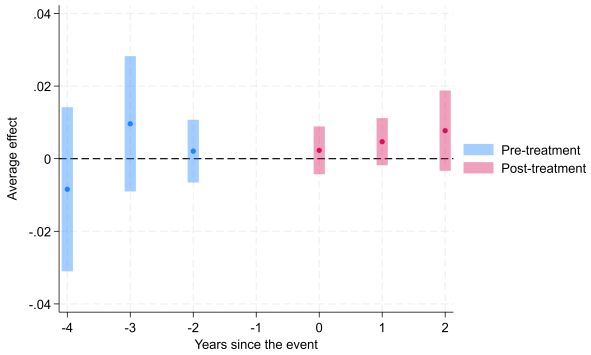
(b) Acquirer Effect



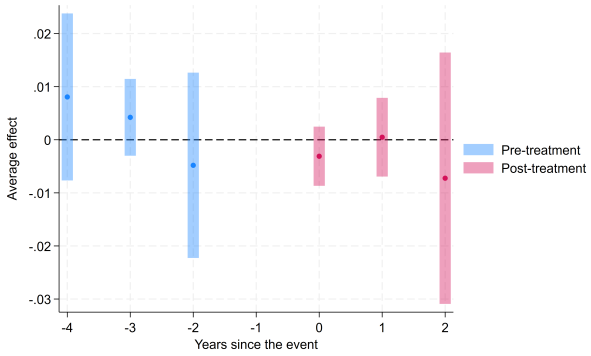
(c) Combined Effect

Notes: Same specification as Figure A11, restricted to *urban* hospitals in the matched sample per the USDA ERS rural county indicator. The outcome is the rate of Cesarean (C-section) deliveries among all live births.

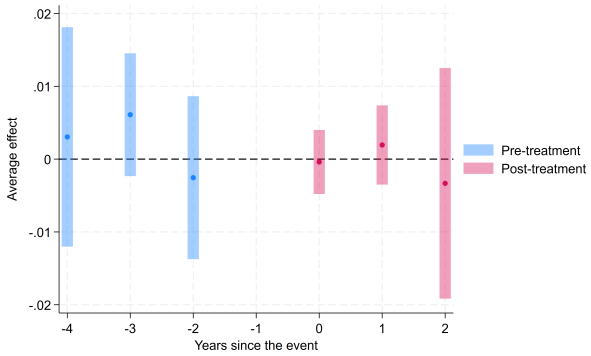
Figure A16: Urban Hospitals: C-section Without Medical Indication (Target, Acquirer, & Combined Effects)



(a) Target Effect



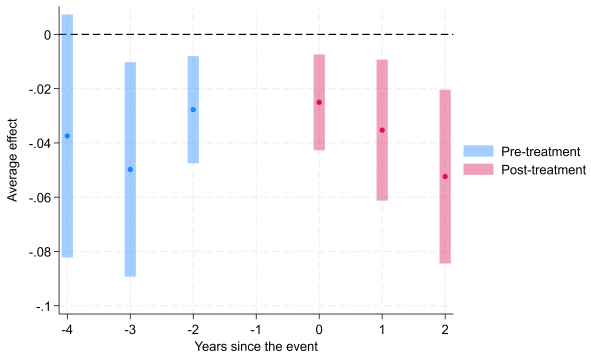
(b) Acquirer Effect



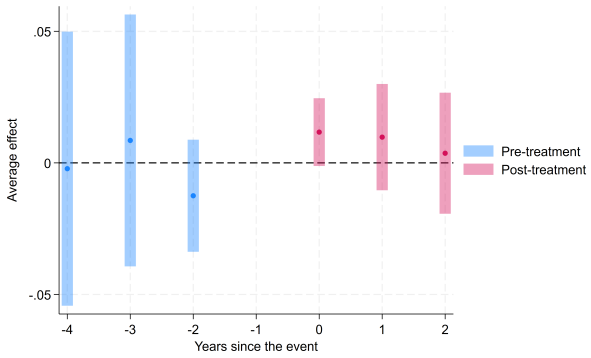
(c) Combined Effect

Notes: Same specification as Figure A15. The outcome is the rate of C-section deliveries without medical indication among all live births.

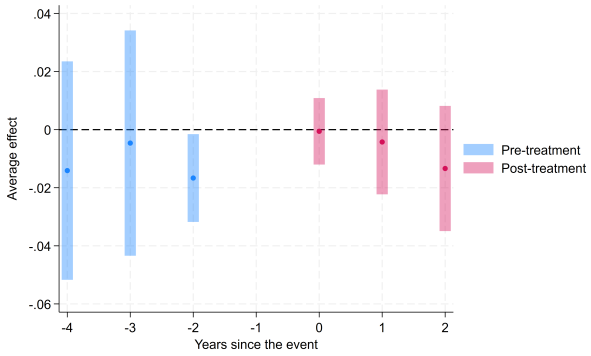
Figure A17: Urban Hospitals: Induced Labor (Target, Acquirer, & Combined Effects)



(a) Target Effect



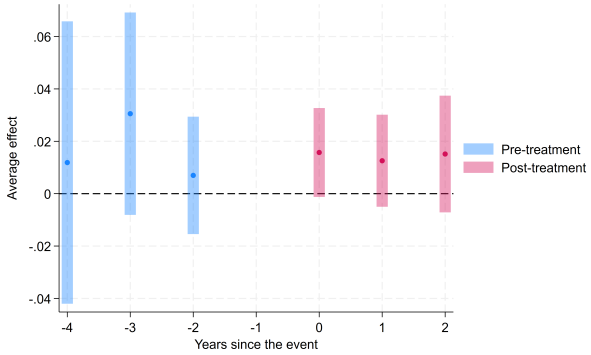
(b) Acquirer Effect



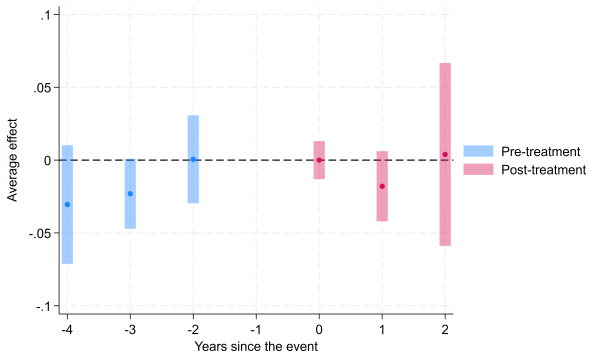
(c) Combined Effect

Notes: Same specification as Figure A15. The outcome is the rate of induced labor among all live births.

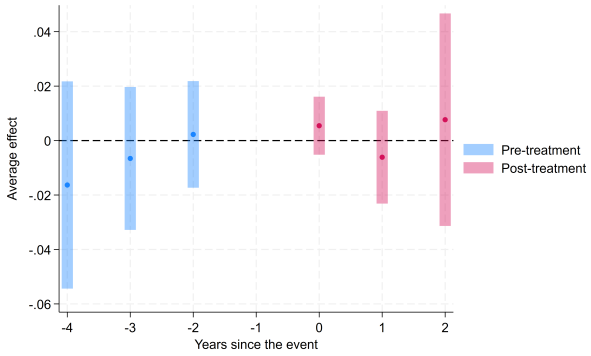
Figure A18: Urban Hospitals: Maternal Complications (Target, Acquirer, & Combined Effects)



(a) Target Effect



(b) Acquirer Effect



(c) Combined Effect

Notes: Same specification as Figure A15. The outcome is the rate of maternal complications (ICD-10-CM O60–O77) among all live births.