Delivering Amidst Consolidation:

Implications of Hospital Mergers on Newborn and Maternal Outcomes

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**Abstract**

As hospital systems consolidate and negotiate higher private health insurance (PHI) reimbursements, the widening difference between PHI and public hospital payments could incentivize hospitals to restrict access to care for lower-margin Medicaid beneficiaries. We examine how hospital mergers affect labor and delivery (L&D) care for mothers covered by Medicaid. Using national Medicaid claims data and U.S. vital statistics data, we find that Medicaid birth admissions decline by an average of 29% following a within-market hospital merger. This reduction is largely attributed to a corresponding 12% decrease in the number of unique L&D providers at merged facilities. Rates of cesarean delivery show modest declines, with no measurable change in induced labor rates. At the county level, newborn health outcomes improve in areas served by hospitals that acquire other facilities. Overall, hospital consolidation appears to reduce access to within-market hospital options while yielding modest improvements in birth outcomes for mothers admitted for L&D care.

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# I. Introduction

The US hospital industry has consolidated rapidly 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). Despite extensive research on hospital mergers and their impact on market power and prices, little is known about how consolidation interacts with the tiered U.S. health care financing system—particularly how it shapes hospitals' incentives to treat lower-margin Medicaid beneficiaries. 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 remains critically understudied (Desai et al., 2023). In this paper, we examine the implications of consolidation for mothers with Medicaid seeking labor and delivery (L&D) services.

In addition to increasing their market power with private insurers, profit-motivated hospitals may alter their service mix after a merger by restricting access to Medicaid beneficiaries, since private health insurance (PHI) reimbursements are much more lucrative than Medicaid inpatient reimbursements. According to the Health Care Cost Institute (HCCI), the average reimbursement for a visit to deliver a baby was more than \$15,000 for births covered by PHI and only about \$6400 for Medicaid-covered births (Valencia et al., 2022). Additionally, there is wide variation in the childbirth reimbursements of PHI between geographical regions, ranging from just above \$10,000 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).[1] Delivering babies from privately insured mothers can generate up to five times more revenue for hospitals than delivering babies from mothers with Medicaid. Furthermore, the estimated hospital input costs of prenatal care often exceed the revenue generated from Medicaid reimbursements (Baker et al., 2021).

[1]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)

When hospitals merge, the resulting hospital system can leverage its increased bargaining power to negotiate higher reimbursement rates from PHI firms (Cooper et al., 2019; Gaynor, 2021; Gowrisankaran et al., 2015). Because there is no corresponding increase in Medicaid rates, the average price difference between insurance types can increase. With its control over more facilities, the system can consolidate its services, centralizing L&D care at more resourced facilities while closing obstetric units that were previously unprofitable (Arnold et al., 2025; Dranove et al., 2025). This consolidation process reduces the system's exposure to unprofitable service lines and routes mothers affected by these supply reductions to local market competitors (Desai et al., 2023; Dranove et al., 2025). Overall, this behavior could result in less L&D volume at consolidated systems. These access impacts may be particularly acute among Medicaid beneficiaries.

The overall impact of these care management dynamics on health outcomes is ambiguous. Hospital consolidation may lead to the integration of clinical protocols, more robust staffing models, and increases in care coordination and efficiency (J. R. Clark & Huckman, 2012), providing health benefits for patients. Mothers with Medicaid admitted to consolidated systems can receive higher-quality care due to the integration of protocols and the reduction of hospital L&D volume (Fischer et al., 2024), allowing more resources to be spent per admitted mother. On the other hand, acquirer systems may implement more profitable care practices, leading to care that prioritizes system profit over maternity services (Dranove et al., 2025). This paper addresses this ambiguity through an empirical analysis of hospital behavior and its effects on Medicaid birth outcomes.

We first analyze the 2016-2021 Transformed Medicaid Statistical Information System Analytic files (from here on: Medicaid TAF files), which are the standardized data files for the Medicaid program, the primary source of 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) and birth procedures (e.g., C-sections and induced labor). To do so, we first identify hospitals that were involved in a merger with another hospital or hospital system within the same hospital referral region (HRR). We then separately examine the effect 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 was on. We use staggered difference-in-differences models suggested by Callaway and Sant'Anna (2021). Our main treatment variable is the first instance of a hospital merger.

We find that target hospitals experience an average 31% decline in Medicaid L&D admissions––—equivalent to approximately 119 fewer Medicaid L&D admissions from a pre-target mean of 380 births per hospital-year——relative to hospitals that are not exposed to a merger. Acquirer hospitals experience an average 27% decline in Medicaid L&D admissions—equivalent to approximately 136 fewer Medicaid L&D admissions from a pre-acquirer mean of 505 births per hospital-year—relative to hospitals that are not exposed to a merger. The combined target and acquirer effect (the "combined" or "consolidation" effect) indicates an average 28% reduction in Medicaid L&D admissions—equivalent to approximately 130 fewer Medicaid L&D admissions from a pre-consolidation mean of 455 births per hospital-year—relative to hospitals that are not exposed to a merger. Across the sample of 554 hospitals observed over six years, the overall decline in Medicaid L&D admissions appears to be driven primarily by reductions in specialty staffing capacity at acquirer hospitals. We observe a 14% decrease in the number of unique admitting providers at acquirer hospitals, equivalent to approximately four fewer unique admitting providers for L&D admissions per hospital-year. This is evidence for strategic reductions in L&D staffing capacity.

Because most of our main effects are driven by acquirer effects, we analyze local market equilibrium adjustments influenced by 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 examine births covered by all insurance types and then births covered by Medicaid. These outcomes include the likelihood of a birth within a mother's county of residence ("same-county births"), infant mortality during the L&D visit, and infant Apgar scores (a standard measure of newborn health). To do so, we first identify counties where at least one hospital is a within-HRR acquirer. We then link these hospitals to the counties of residence of the mothers. We compare mothers' counties of residence that experienced at least one local hospital acquirer to those with no hospital M&A activity to examine how ownership changes affected local

L&D market dynamics.

For births covered by Medicaid, we find that counties with acquirer hospitals see increases in the likelihood of mothers giving birth within their county of residence, decreases in the likelihood of C-section births, and no detrimental impacts on infant mortality or 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, contractions in L&D staffing capacity in acquirer hospitals, and 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 that are driven by cost-cutting and efficiency gains by acquirer hospitals.

Several challenges exist when studying the impact of hospital mergers on birth outcomes. First, there are numerous data-related hurdles. No single dataset of medical claims spans the entire birth census in the US. Combining medical claims from across public-private payer sources across the entire US is virtually impossible due to the various restrictions introduced by data use agreements. We use Medicaid claims data from 2016 to 2021 to obtain comprehensive and granular hospital effect estimates, focusing on one of the most socioeconomically vulnerable populations in the US: mothers with Medicaid and their newborns. Second, the largest database of birth outcomes currently available is the National Vital Statistics System (NVSS) Birth Data (National Center for Health Statistics, 2024). A major limitation of these data is that they do not contain hospital facility-level identifiers that allow for direct hospital-level treatment effect estimation. The studies using these data are unable to directly link NVSS data to facilities in markets with multiple hospitals, limiting the conclusions that can be drawn about the direct impact of hospital behavior on patients. Third, though Medicaid is the largest insurer of births, the Medicaid program is run differently by state. This means that, depending on how much each state invests in the program, there are varying levels of data quality and completeness, necessitating multiple rounds of data diagnostic tests and the exclusion of certain outcomes due to data availability and reliability issues. Fourth, over 75% of Medicaid beneficiaries are now covered by comprehensive managed care programs where state Medicaid agencies pay capitated payments to health insurance companies that administer the plans. The insurer thus becomes an intermediary between the Medicaid agency and the care providers, making it less clear how hospitals might adjust their clinical protocols to increase profits following a merger. These challenges may be the reason why there is a limited number of papers that use Medicaid claims data to study hospital consolidation (Jiao, 2025; Oh et al., 2025), with Oh et al. (2025) examining general hospital (> 25 beds) mergers & acquisitions on all-cause Medicaid hospitalizations and Jiao (2025) focusing on the impact of private equity acquisitions of hospitals on maternal health outcomes.

According to the recent literature on the economics of L&D care, the unprofitability of labor and delivery has already led to closures 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 high levels of post-natal mortality in the United States being driven by poor outcomes within lower socioeconomic status individuals (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 private health insurance.

Another key aspect of this framework is how care is distributed between facilities. 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 that examines 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 care access and quality (Beaulieu et al., 2020; Desai et al., 2023; Hayford, 2012;

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). Most of this literature examines obstetric unit closures, akin to facility-level consolidation (Hayford, 2012), where hospital obstetric units may exit, consolidating labor and delivery care to fewer facilities. They also focus on the rural setting due to the lack of facility-level hospital identifiers in the NVSS data. Although Battaglia (2025) does not specifically examine hospital M&As, they find evidence that maternity ward closures lead to a reduction in C-sections with null effects on birth outcomes. This evidence suggests further optimization of care delivery across L&D markets through the consolidation of L&D care within fewer facilities employing 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 supply restrictions driven by new management and profit-driven hospital strategies in rural settings (Dranove et al., 2025; Fischer et al., 2024).

We provide the first evidence of within-hospital reductions in L&D staffing capacity using comprehensive sets of Medicaid birth data from two national-level sources. In particular, our findings reveal the influence of the hospitals on the acquiring side of a merger deal—a dimension that has not been extensively examined in the literature. As Medicaid administration and payment policies face changes by the Trump Administration (U.S. Congress, 2025) and health care markets continue consolidating after the COVID-19 pandemic (Barnett et al., 2020; Vogel, 2024), understanding whether these reallocations occur and whether they hurt or help mothers and newborn infants will inform antitrust policymakers, Medicaid agencies, and health systems on how they can best protect mothers with Medicaid and their infants.

The rest of this paper is organized as follows: Section 2 discusses the conceptual framework for the paper. Section 3 describes the data and methods. Sections 4 and 5 discuss the results. Section 6 discusses the role of public hospitals after local hospital mergers. Section 7 concludes.

# II. Theoretical Framework

We assume that hospitals act as profit-maximizing organizations subject to payer-mix, contracting, and moral constraints. While hospitals cannot explicitly deny care to lower-reimbursing patients, they can indirectly shape the mix of patients they attract and admit. They can do so through (i) capacity and scheduling choices, which we test via changes to bed and staffing capacity, (ii) demand-side positioning (brand, amenities, and selective marketing that may appeal more to privately insured patients (Huppertz et al., 2016)), and (iii) network and contracting decisions with payers (Cooper et al., 2019; Gaynor & Town, 2011).

Medicaid's growing dependence on managed care organizations (MCOs) further shapes incentives: under prospective payment and sub-capitated hospital/insurer contracts, hospitals have stronger reasons to manage the intensive margin by increasing their efficiency of care and the extensive margin by narrowing the Medicaid MCOs that they accept. As of 2022, comprehensive risk-based managed care organizations MCOs covered approximately three-quarters of all U.S. Medicaid enrollees, reflecting the dominant role of managed care in program administration. Most states report MCO penetration rates above 80%, though a subset maintained little or no comprehensive MCO presence (e.g., Alabama, Connecticut, Maine, Montana, South Dakota, Vermont, and Wyoming) (Kaiser Family Foundation, 2024).

We now present a stylized model that theorizes the conditions leading to increases in acquirer hospital quailty.

## Model: Quality & Quantity Before and After Hospital Mergers

### Setup

Let there be two hospitals: acquirer $A$ and target $T$. Each hospital $h$ chooses clinical quality $q_h \geq 0$ (reliability/appropriateness). We assume that aggregate demand is linear in quality for each type of hospital $h$ and its rival $-h$:

$$D_h = A_0 + B\, q_h - G\, q_{-h}, \qquad B, G > 0. \tag{1}$$

with $A_0$ capturing market size and network breadth (including Medicaid plan coverage). Patients (and MCO referrals) value higher quality hospitals ($B$) and substitute across hospitals ($G$). Thus, for each hospital $h$, the demand for their services decreases when the quality of their rival, $-h$, increases. We consider this phenomenon, "business stealing," which follows standard models of differentiated-product competition (Berry et al., 1995; Gaynor & Town, 2011; Werden & Froeb, 1994).

**Reimbursement.** Let $m_h$ denote the *average net margin per admission* (revenue minus non–quality-related variable cost), weighted by payer mix (including the Medicaid average reimbursement). We treat $m_h$ as *constant* with respect to $q_h$.

**Quality cost.** We assume that quality incurs convex resource costs: $\frac{1}{2} a_h q_h^2$ pre-merger and $\frac{1}{2} a'_h q_h^2$ post-merger, where $a'_h > 0$ may be lower after consolidation ($a'_h \leq a_h$) due to economies of scale and increased care delivery integration (J. R. Clark & Huckman, 2012; Schmitt, 2017). In other words, $a_h$ and $a_{h'}$ denote the cost of quality before and after a merger, respectively, and the hospital might increase its efficiency after a merger, resulting in a lower cost of quality.

### Objective Functions, Pre- and Post-merger

**Pre-merger (separate objectives).** For each hospital $h \in \{A, T\}$,

$$\max_{q_h \geq 0} \Pi_h^{\text{pre}}(q_h; q_{-h}) = m_h\, D_h(q_h, q_{-h}) - \tfrac{1}{2}\, a_h\, q_h^2, \qquad a_h > 0. \tag{2}$$

Each hospital has its own objective function that is impacted by its quality of care, $q_h$, and that of its rival, $q_{-h}$.

**Post-merger (joint objective).** When former rivals $A$ and $T$ merge, they now make joint decisions and there is no longer any "business stealing" between them. The consolidated system chooses quality $(q_A, q_T)$ such that:

$$\max_{q_A, q_T \geq 0} \Pi^{\text{post}}(q_A, q_T) = m_A D_A(q_A, q_T) + m_T D_T(q_A, q_T) - \tfrac{1}{2}\, a'_A\, q_A^2 - \tfrac{1}{2}\, a'_T\, q_T^2, \qquad a'_h > 0. \quad (3)$$

This maximization internalizes competitive externalities between $A$ and $T$ (Gaynor & Vogt, 2003; Werden & Froeb, 1994). In other words, the two hospitals no longer compete for the same patients, and any patient substitution that previously occurred between them is now managed within a single system.

### Best Responses and Closed Forms

**Pre-merger.** Using the linear demand function $D_h = A_0 + Bq_h - Gq_{-h}$, each hospital $h$ chooses quality $q_h$ to maximize $\Pi_h^{\text{pre}}$. The first-order condition (FOC) is

$$\frac{\partial \Pi_h^{\text{pre}}}{\partial q_h} = m_h B - a_h q_h = 0 \quad \Rightarrow \quad q_h^{\text{pre}} = \frac{m_h B}{a_h}.$$

(The rival's quality drops out of the FOC.)

**Post-merger.** The consolidated system maximizes joint profit with respect to both $q_A$ and $q_T$:

$$\frac{\partial \Pi^{\text{post}}}{\partial q_A} : m_A B - m_T G - a'_A q_A = 0 \quad \Rightarrow \quad q_A^{\text{post}} = \frac{m_A B - m_T G}{a'_A},$$

$$\frac{\partial \Pi^{\text{post}}}{\partial q_T} : m_T B - m_A G - a'_T q_T = 0 \quad \Rightarrow \quad q_T^{\text{post}} = \frac{m_T B - m_A G}{a'_T}.$$

Note that the decision to invest in quality is now centralized and may be heterogeneous, depending on the margins $m_A$ and $m_T$ and how much the cost of quality $a'_h$ changes from $a_h$ for each hospital $h$.

**When Does Acquirer Quality Rise After Merger?**

Comparing pre- and post-merger quality levels for the acquirer:

$$q_A^{\text{post}} > q_A^{\text{pre}} \quad \Longleftrightarrow \quad \frac{m_A B - m_T G}{a'_A} > \frac{m_A B}{a_A}.$$

Consolidation introduces two opposing forces on $q_A$: (i) **Economics of scale and scope** reduce the marginal cost of quality ($a'_A \downarrow$), increasing $q_A$; (ii) **Internalized business stealing** subtracts $m_T G$ from the demand-margin term, reducing $q_A$ by the marginal decrease in market competition introduced by the merger (in other words, they no longer need to compete with $T$ on quality). Hence, acquirer quality rises if the cost-synergy effect dominates the marginal disincentive to invest in quality. This component of the framework is consistent with theoretical merger models in differentiated hospital markets (Gaynor & Town, 2011; Gaynor & Vogt, 2003; Propper et al., 2004).

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**Realized Quantity with Network Narrowing and Capacity**

Observed admissions incorporate network inclusion and effective capacity:

$$Y_h = \pi_h \cdot \min\{D_h(q_h, q_{-h}), K_h\},$$

where $\pi_h \in [0, 1]$ is the plan-weighted probability that the hospital is in-network, and $K_h$ denotes effective hospital L&D capacity (beds and staffing). We assume that hospital $h$ realizes the minimum quantity demanded between $D_h(q_h, q_{-h})$ and $K_h$ due to the low margins realized for providing L&D care to mothers with Medicaid.

- **Network narrowing:** Post-merger, insurers may accept narrower networks due to their relative loss in bargaining power (Capps et al., 2003; Ho, 2009; Town et al., 2006), so the plan-weighted probability of the target hospital being in-network, $\pi_T$, decreases (and sometimes the plan-weighted probability of the acquirer hospital being in-network, $\pi_A$, remains flat or decreases).

This means that the choice set available to a Medicaid beneficiary narrows, negatively impacting their welfare if they must travel farther distances to receive care or if they receive care at an overcrowded hospital.

- **Capacity rationalization:** Systems streamline staffing, implying that capacity for the target hospital, $K_T$, decreases and, if efficiency-focused, capacity for the acquirer hospitals, $K_A$, can decrease as well.

Even when $q_A^{\text{post}} \geq q_A^{\text{pre}}$, total L&D admissions at consolidated hospitals can decline ($Y_A \downarrow, Y_T \downarrow$) if $\pi_h$ and/or $K_h$ contract. Aggregate HRR births may remain stable if other hospitals absorb displaced mothers.

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#### Empirical Mapping and Predictions

Considering the findings of Desai et al. (2023), Arnold et al. (2025), and Dranove et al. (2025), we hypothesize that L&D visit volume will decrease in consolidated hospitals relative to non-consolidated hospitals, via restrictions to hospital L&D capacity. We do not anticipate aggregate market demand for hospital L&D care to be impacted (there is nothing stopping a baby about to be born). The impact on quality is more ambiguous and necessitates a thorough empirical treatment.

## III. 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 2016-2021. These are the official national standardized data files for the Medicaid program, the primary source of health insurance coverage for low-income populations in the United States. Our Medicaid TAF files do not come with pre-defined hospital facility identifiers like the American Hospital Association (AHA) Hospital ID or CMS Certification Number (CCN). Instead, it comes with 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 L&D admissions, number of hospital beds, and number of unique admitting L&D providers. We then examine the downstream impacts on C-section rates, 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. Assessing the true medical necessity of C-sections is challenging, especially when hospitals are incentivized to code mothers as high-risk patients to obtain higher reimbursements. We thus study changes in C-section 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 birth quality outcomes in our county-level analyses. Please see Appendix Table A2 to see our algorithm for identifying birth, C-section, and induced labor admissions 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 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 analytic sample to hospitals that consistently provide labor and delivery (L&D) services by excluding hospitals that record fewer than 12 births per year (e.g. 1 birth per month) prior to their merger (for treated hospitals) or fewer than 12 births per year throughout the entire study period (for control hospitals). This restriction ensures that comparisons are made among hospitals that, absent a merger, consistently provide L&D care.

Other factors could be influencing our estimated volume and capacity effects, including policy shocks such as the late Medicaid expansion from 2016 to 2021 by several states and shocks due to the COVID-19 pandemic which took hold of the US starting in 2020. Given the magnitude and significance of our volume and capacity results, we perform robustness checks to ensure that we can credibly declare our treatment effects a result of hospital mergers and not these external shocks. For our robustness check to ensure that Medicaid expansion was not an influence on our treatment, 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. For our robustness check to ensure that COVID-related hospital procedures and policies were not drivers of our estimated effects, we run the same empirical models as above after excluding data from the years 2020 and 2021.

Furthermore, we test our volume and capacity effects along three different dimensions of heterogeneity: Market Concentration, Not-for-Profit Status, and Geographic Setting (Urban vs. Rural). Hospital mergers occurring in already-concentrated markets move the system even closer to monopoly-level control. This could lead to strategic behavior that is distinct from hospitals merging in less concentrated markets. We calculate Hirfindahl-Hirschman Indices (HHIs) in 2016 using the following equation:

$$HHI_{\text{sys},\,r} = \sum_{k \in r} \left( \frac{\text{Beds}_{k,r}}{\sum_{l \in r} \text{Beds}_{l,r}} \right)^2 \tag{4}$$

*Equation 4 Notes:* The Herfindahl–Hirschman Index (HHI) is computed at the HRR $r$ using system-level bed shares in 2016. $\text{Beds}_{k,r}$ denotes staffed inpatient beds for system $k$ within HRR $r$. Values closer to 1 indicate higher concentration. We use the hospital system designations provided by the American Hospital Association (AHA) Annual Hospital Survey dataset (“American Hospital Association Annual Survey Data”, 2014).

Within our hospital sample, we then compute the median HHI in 2016 and divide the sample into hospitals in HRRs that have an HHI above the median and hospitals in HRRs that have an HHI at or below the median. We run our empirical analysis on each subsample.

In a similar vein, we test for whether not-for-profit hospitals show 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).

Finally, we test hospitals in urban and rural subsamples to assess whether there are differential effects depending on the 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).

For our analysis of birth outcomes, we maintain our data at the birth admission level and examine the impacts of hospital mergers on C-section and induced labor rates.

**Treatment Groups: Hospital-Level Merger Identification**

We identify treatment at the hospital level, classifying each hospital as either a target, acquirer, or part of the combined (or consolidated) treatment group following a merger. Treatment hospitals are required to have up to five pre-treatment periods and three post-treatment periods (including the first treatment period, “0”) to ensure adequate pre- and post-merger observation windows.

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 acquirer and target hospitals to capture the overall consolidation effect. Hospitals that do not experience any system-affiliation change 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 extends only 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) Hospital as targets; (2) Hospitals as 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 and not-yet-treated hospitals, allowing us a more granular look at hospital merger impacts.

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 and not-yet-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 2016–2021.

Table 1 shows the characteristics of the hospitals in our sample. Table 2 provides the descriptive statistics of the characteristics of the birth sample by treatment status. We can see that mothers who give birth in consolidated hospitals are more likely to be white (43% vs. 40%) and sicker (0.47 Elixhauser comorbidities vs. 0.43) than mothers who give birth in never-treated hospitals.

## County Level Acquirer Analysis

Based on our hospital-level findings, acquirer hospitals seem to 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 includes detailed information such as five-minute Apgar scores (a standard metric for infant health five minutes after birth), county of birth facility, and infant mortality during the L&D admission. Although the NVSS's privacy and deidentification procedures do not allow for the identification of the facilities where each birth occurred, the data files contain the most comprehensive set of certified information on individual births and various birth outcome indicators.

### 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 labor and delivery care through the following 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 are 5-minute Apgar scores (a standardized assessment of health status immediately after birth, scored from 0-10 with 10 being healthiest), cesarean (C-section) birth (binary), and infant mortality (binary).

Our control variables are binned versions of maternal age (19 and younger; 20-34; 35-39; 40 and older) and of 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 2 provides a geographic visual for the frequency of hospital acquirer activity in our NVSS sample from 2016 to 2022. Treatment counties are required to have up to five pre-treatment periods and four post-treatment periods (including the first treatment period "0") to ensure adequate pre- and post-merger observation windows.

Table 3 shows the descriptive statistics for never treated and counties with acquirers. We observe that besides same county birth rates, prenatal visit volume, and maternal transfers, most variables showed balance across each treatment group.

## Empirical Methods

We estimate dynamic treatment effects using the staggered difference-in-differences framework developed by Callaway and Sant'Anna (2021), which allows for treatment effect heterogeneity between groups and over time. Specifically, we estimate event-study-type average treatment effects by time, relative to a hospital's first exposure to a merger.

Let $G_h$ denote the quarter in which hospital $h$ is first treated. For each relative event time period $k$, we estimate the average treatment effect on the treated (ATT) for hospitals treated $k$ years earlier, comparing them to hospitals that have not yet been treated or never treated. Equation 5 presents the dynamic effects of treatment:

$$\text{ATT}(k) = \mathbb{E}[Y_h(1) - Y_h(0) \mid G_h = y - k], \tag{5}$$

where $Y_h(1)$ and $Y_h(0)$ represent potential outcomes with and without treatment, respectively, for hospital $h$ in year $y$. We implement the doubly robust approach, applying both propensity score weighting and regression adjustment, allowing for weaker functional form assumptions in our modeling and estimation procedure. We aggregate these group-time ATTs to produce the event-study plot of ATT$(k)$ over event time $k$.

We achieve effect identification by comparing births in hospitals that experience a merger, with hospitals that have not yet or never been exposed. The Callaway and Sant'Anna estimator aggregates group-time average treatment effects in a way that avoids the biases present in traditional two-way fixed effects models (Callaway & Sant'Anna, 2021; Roth et al., 2023).

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 checks whether the hospital-level changes in volume we find are a function of 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 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. Hence, to minimize the impact of anticipation effects, we exclude effects for the year right before treatment. 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 prior to treatment, post-treatment divergence can be attributed to the treatment itself. For our analysis of claim-level data, we control for mothers' age and Elixhauser comorbidity counts.

# IV. Hospital-Level Results

We now present the results of our empirical analyses. We summarize aggregated post-merger effect estimates in Tables 4 and 7 and show the dynamic event-study graphs in the figures referenced 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 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, see an average of 455 L&D admissions before their merger. We find that consolidated hospitals decreased their birth volume by approximately 29% (or 130 fewer births) after their mergers, driven mostly by the acquirer effect. Acquirer hospitals saw an average of 505 L&D admissions before their mergers and experienced an average 27% decline (or 136 fewer births) after their mergers. Target hospitals saw an average of 380 L&D admissions before their merger and experienced a larger relative reduction of 30.6% (or 119 fewer births), though the aggregated effect was not statistically significant ($p = 0.097$). Each event study plot in Figure 3 shows a clear decrease in L&D volume for all effects. Though the aggregated target effect is not statistically significant, we observe a consistent downward trend in volume among target hospitals with the standard errors likely increasing due to the smaller quantity of post-treatment target hospitals at those event times.

### Effect on Hospital Bed Capacity

There are no significant effects of hospital mergers on hospital bed capacity. Although beds are an imperfect measure of L&D capacity, this signals that merging hospitals are likely to keep their overall physical capacity stable. The event study plots in Figure 4 show no statistically significant changes in hospital bed capacity over time.

### Effect on Unique Admitting NPIs

Hospitals that merge during our study period decrease labor and delivery (L&D) staffing capacity after their mergers, which we measure using the number of unique admitting National Provider Identifiers (NPIs) per facility. Merging hospitals employ an average of 26 unique providers before their mergers and reduce their number of unique admitting providers by 12.5%, on average, equivalent to roughly three fewer OB/GYN or L&D admitting providers per hospital-year. This effect is driven primarily by acquiring hospitals, which employ approximately 30 unique providers before their mergers, on average, and experience a 14.0% decline, corresponding to approximately four fewer admitting providers per hospital-year. The event-study plots in Figure 5 show a clear and persistent post-merger decline in unique admitting NPIs for acquirers. These findings suggest that acquirer hospitals make strategic adjustments to L&D staffing following consolidation. This provides evidence consistent with strategic reductions in staffing capacity among acquirer hospitals, potentially serving as a profit-oriented justification for lowering Medicaid L&D volume.

## Robustness and Heterogeneity Test Results

Our results remain robust when excluding late Medicaid expansion states and when restricting the analysis to the pre-COVID period. Table 5 presents the aggregated difference-in-difference coefficients of each test, while Appendix Figures A1—A6 show the event study plots.

Table 6 presents the results of our heterogeneity tests. We find that the main results are primarily driven by hospitals in the upper half of HHI markets, as further illustrated in Appendix Figures

A7— A12. This implies that mergers in more concentrated markets lead to a greater ability for consolidated hospital systems to restrict access to L&D services for mothers with Medicaid via reductions to staffing capacity as shown in Appendix Figure A11. We also see that in Appendix Figures A17—A21 non-profit hospitals (approximately 90% of hospitals in our sample) yield effects that mirrored our full sample effects. Hospitals in urban counties mirrored our main effects more than hospitals in rural counties, reflecting urban hospitals' larger size, closer proximity to system facilities (and thus, a better ability to consolidate services), and greater representation within our hospital sample.

## Effect on Birth Outcomes

We now examine the downstream impact of hospital mergers on birth outcomes, focusing on Cesarean deliveries (C-sections) and induced labor. We study these procedures because they differ in clinical management and in the regulatory oversight associated with reimbursement eligibility. Elective deliveries before 39 weeks—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 initiate labor artificially and involve greater uncertainty in progression. Among first-time mothers or those with less favorable clinical conditions, inductions substantially 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 shows 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 hospitals with varying degrees of clinical management stringency and scheduling constraints.

We find that, overall, C-section rates decrease by 1.1 percentage points relative to a pre-merger mean of 28.3%, corresponding to roughly four fewer C-sections per hospital-year. This effect is driven primarily by acquirer hospitals, which reduce their C-section rates by 1.0 percentage point (again, about four fewer C-sections per hospital-year). Event-study plots in Figure 6 show a steady post-merger decline among acquirer hospitals. Target hospitals also experienced a 0.8 percentage-point reduction, though this estimate is not statistically significant. We observe mostly flat induced-labor rates in Figure 7 following mergers. Taken together, our findings suggest that consolidated hospitals selectively reduce C-sections that may not have been medically necessary, while continuing similar labor induction protocols as before. We explore this interpretation further in the next section, which discusses our reduced-form acquirer-influenced equilibrium adjustment results.

# V. County-Level Results

Since acquirer hospitals seem to influence most decisions related to personnel shifts and care procedures, we now discuss adjustments in equilibrium due to acquirer hospitals, looking at 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 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.

## 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. Because we identify acquirer hospitals within mothers' counties of residence, this indicates that mothers in counties with acquiring hospitals are likely to give birth at a different hospital within the same county as the acquirer hospital. When assessing Figure 8, we observe flat effects after county acquirer exposure. While there is a statistically significant pre-treatment estimator five years before treatment occurs, we are confident that this effect is not a result of anticipatory market behavior prior to county merger activity.

## 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 9 and 10, 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 11. The same-county birth effect for Medicaid births is similar to the overall effect and is statistically significant ($p = 0.029$), 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%. This translates to roughly 5400 fewer C-sections within our sample attributable to the county acquirer effect. We observe no corresponding changes in infant mortality during the L&D admission, nor any detrimental effects on five-minute Apgar scores, as shown in Figure 13. Taken together, these findings suggest that, at the market level, hospitals reduce resource utilization following mergers without adverse impacts on birth outcomes.

# VI. The Role of Public Hospitals After Hospital Mergers

Public hospitals play a crucial role in caring for the most underprivileged patients. Their mission is to prioritize and serve the public good without maximizing profit. As such, they provide comprehensive medical care to anyone, regardless of their ability to pay; take on leadership roles in community public health initiatives, such as the fight against COVID-19 (Ross et al., 2023); train the next generation of health care providers with invaluable interactions with the most socioeconomically vulnerable patients; and steward public funds with efficiency and wisdom in relatively under-resourced environments. Given the merger-induced reductions we find in L&D admissions in hospitals, regardless of which side of the deal they stand on, we examine whether mothers with Medicaid are more likely to give birth at public hospitals after a local merger.

Revisiting our Medicaid L&D visit claims data from our main analysis, we define a county-level treatment similar to our equilibrium adjustment models: births within a county that are exposed to a hospital merger (encompassing both targets and acquirers). We restrict our sample to Medicaid births in counties that contain a public hospital, excluding births in counties exposed to a merger directly involving a public hospital. This is to avoid biasing the treatment effect estimates toward post-merger public hospital usage. We end up with a sample of 394,037 births, 9884 of which are exposed to a local within-county hospital merger. Figure 15 displays the percentage point increases in the probability of seeking L&D care at a public hospital after the merger occurs, going up to a point estimate of 0.156 percentage points (standard error: 0.085 percentage points) four years post-merger. We observe a progressively increasing probability that public hospitals local to a hospital merger absorb a significant proportion of displaced births. These findings highlight the importance of public hospitals in providing maternal health care, especially in the face of consolidating hospital markets.

## VII. 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. 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 the potential adjustments in equilibrium influenced by acquirer hospitals as they assume control over a larger network of facilities. We find that hospital mergers restrict access to L&D care for mothers covered by Medicaid, while modestly improving hospital resource utilization and birth outcomes, as reflected in reduced C-section rates with no obvious negative consequences to birth outcomes.

The decline in Medicaid L&D admissions is driven by acquiring hospital systems strategically reducing their staffing capacity for L&D care, reflected by the significant decrease in unique L&D admitting providers among acquirer hospitals. While human capital is affected in acquirer hospitals, physical capital, represented by beds, remain steady after mergers. For mothers who are admitted for L&D care post-merger, we find decreases in C-section rates with no changes in induced labor.

With fewer births and less L&D staff, and considering Oh (2025)'s findings of minimal impacts of hospital mergers on all-cause Medicaid admissions within markets, consolidated hospitals may use beds that were previously for L&D admissions on other types of inpatients. Although these strategic restrictions in L&D care access are consistent with profit-maximizing behavior by the acquirer hospital, we find evidence that gains in efficiency from the merger may have compensated for the decreased incentive to compete on quality. C-section rates decrease in consolidated hospitals without changes in induced labor rates, consistent with more efficient resource utilization, while maintaining health outcome quality.

When examining the equilibrium adjustment implications of hospital acquirers, we find increased likelihood of mothers with Medicaid eventually giving birth within their county of residence and a slight increase in the number of prenatal visits per birth. C-section rates go down for Medicaid beneficiaries with no changes in infant mortality and no detrimental impacts on 5-minute Apgar scores.

These findings suggest that acquirer hospitals in merger deals promote births that are closer to home for Medicaid beneficiaries and reduce C-section rates without the cost of infant mortality during birth admissions. This signals an increase in L&D care process efficiency without obvious harm to birth outcomes. When combined with our findings from our hospital-level treatment models, we can conclude that mothers with Medicaid are diverted to non-merging hospitals within their local market, and especially to public hospitals if they are nearby.

These results suggest that antitrust scrutiny of hospital mergers should consider distributional effects across payer groups. Our evidence indicates that consolidated hospitals reduce their L&D volume by adjusting their human capital decisions. While there are many legal restrictions to hospitals (Centers for Medicare & Medicaid Services, 2019) that prevent overt refusal to provide L&D care to a mother in labor, hospitals can proactively restrict L&D access by reducing their capacity for L&D care. This type of staffing behavior should be monitored by Medicaid agencies and antitrust policymakers. In addition, decreases in L&D care volume with increases in care efficiency and without accompanying detrimental effects to birth outcomes suggest that policies that prevent overcrowding in hospitals could increase welfare for mothers giving birth.

Mothers with Medicaid who live in counties exposed to a hospital merger and with a local non-merging public hospital are likely to access L&D care at the public hospital with time. This suggests the need for policymakers to ensure that public hospitals are well-funded and supported, especially in hospitals markets with merger activity.

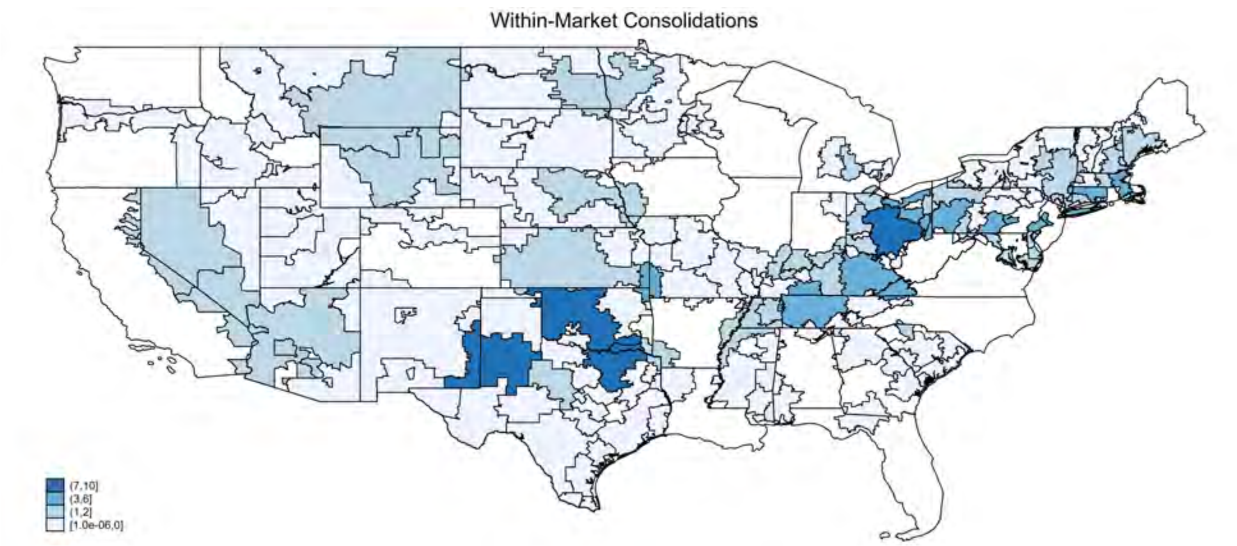
We acknowledge some limitations to our study. First, there were some states where we could not reliably link facility identifiers to their billing NPI numbers. We thus do not have a complete nationally representative sample for our hospital-level treatment models. Second, we limit our dynamic treatment effects to 3 years post-treatment for our hospital-level treatment models and 4 years post-treatment for our county-level treatment models, giving us more of a short-run outlook of effects. Third, we do not specifically examine hospital closures, but focus on hospitals that survived our study period. Only one hospital in our sample dropped to zero births per year after treatment, while still remaining open. Finally, we do not incorporate analyses of vertical integration, meaning that we do not discuss the outpatient care dynamics involved with prenatal care.

In sum, coupling hospital-derived Medicaid claims with population-level vital records suggests that hospital mergers lead to reductions in L&D staffing, translating to reductions in L&D admissions at consolidated hospitals. Hospital mergers may also nudge care practice toward lower procedure intensity and modestly improved birth outcomes for Medicaid births through less crowded birth facilities (Fischer et al., 2024) and retention of fewer, but skilled staff.

# Figures & Tables

## Descriptive Statistics

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



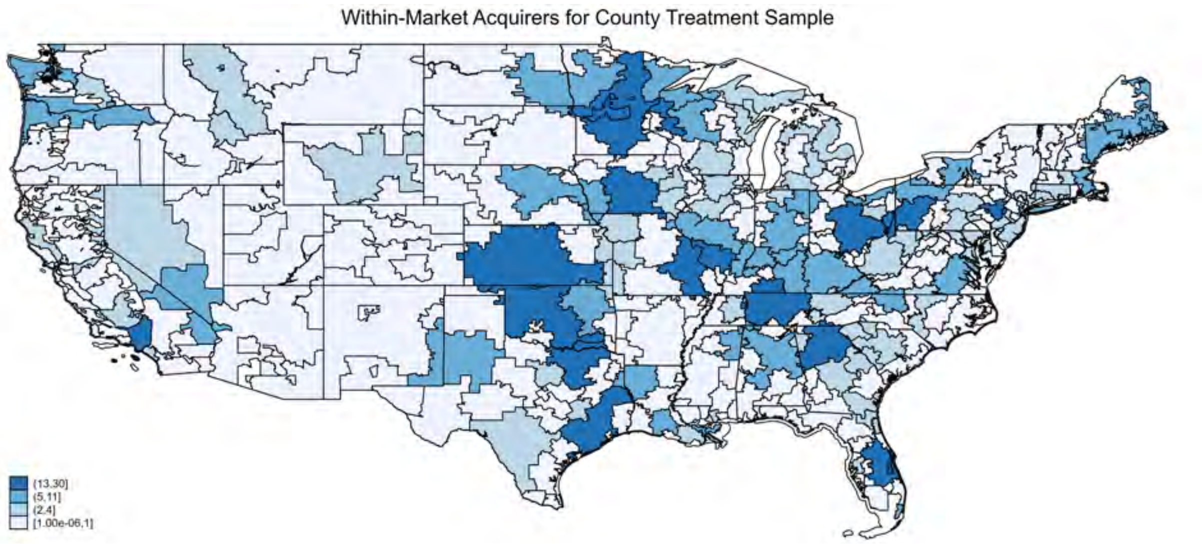

*Notes:* This figure displays the frequency of hospital consolidations by hospital referral region (HRR) within our Medicaid claims-derived hospital sample. Shaded areas indicate HRRs with one or more within-market consolidation events. We show state boundaries and HRR borders for reference.

Table 1: Hospital Characteristics by Treatment Status

| **Variable** | **Full Sample** | **Consolidated (Treated)** | **Control** |
|---|---|---|---|
| *N (hospital-year observations)* | *3,324* | *684* | *2,640* |
| Consolidated in period | 0.206 (0.404) | 1.000 (0.000) | 0.000 (0.000) |
| Acquirer in period | 0.126 (0.332) | 0.614 (0.487) | 0.000 (0.000) |
| Target in period | 0.083 (0.276) | 0.404 (0.491) | 0.000 (0.000) |
| Hospital beds | 246.595 (221.146) | 286.389 (246.620) | 236.285 (212.893) |
| Unique admitting NPIs | 20.485 (22.528) | 24.092 (24.476) | 19.550 (21.903) |
| Total births | 441.073 (755.033) | 477.013 (648.710) | 431.761 (780.062) |
| Total C-section admits | 127.647 (222.466) | 142.602 (199.694) | 123.772 (227.870) |
| System affiliated | 0.533 (0.499) | 0.731 (0.444) | 0.482 (0.500) |
| Hospitals in system | 4.044 (4.456) | 4.148 (3.583) | 4.017 (4.655) |
| For-profit | 0.107 (0.309) | 0.038 (0.191) | 0.125 (0.331) |
| Public facility | 0.164 (0.370) | 0.069 (0.253) | 0.189 (0.391) |
| Rural county | 0.387 (0.487) | 0.231 (0.422) | 0.427 (0.495) |
| Treated in period | 0.206 (0.404) | 1.000 (0.000) | 0.000 (0.000) |

*Notes:* Cells report means with standard deviations in parentheses. Units are hospital-level observations. "Treated" indicates hospitals exposed to a merger (combined target + acquirer) during the analysis period; "Control" indicates never-treated hospitals. Sample sizes: total $N = 3{,}324$; treated = $684$ (20.58%); control = $2{,}640$ (79.42%). Binary variables (e.g., system affiliated, for-profit, public facility, rural county, treated in period, within-HRR indicators) are reported as proportions.

Figure 2: 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.

Table 2: L&D Admission Descriptive Statistics by Treatment Status

| **Variable** | **Full Sample** | **Consolidated** | **Never-Treated** |
|---|---|---|---|
| *N (L&D admissions)* | *1,315,269* | *290,839* | *1,024,430* |
| Elixhauser Comorbidities | 0.437 (0.726) | 0.468 (0.737) | 0.428 (0.723) |
| C-section delivery rate | 0.288 (0.453) | 0.294 (0.456) | 0.287 (0.452) |
| Induced labor rate | 0.215 (0.411) | 0.231 (0.422) | 0.210 (0.407) |
| Transfer-out rate | 0.002 (0.040) | 0.002 (0.040) | 0.002 (0.041) |
| White | 0.400 (0.490) | 0.426 (0.494) | 0.392 (0.488) |
| Black | 0.212 (0.409) | 0.237 (0.425) | 0.206 (0.404) |
| American Indian/Alaska Native | 0.022 (0.148) | 0.016 (0.127) | 0.024 (0.153) |
| Asian | 0.020 (0.140) | 0.021 (0.142) | 0.020 (0.139) |
| Hispanic | 0.143 (0.350) | 0.095 (0.294) | 0.156 (0.363) |

*Notes:* Cells report means with standard deviations in parentheses. Units are L&D admissions. "Treated/Consolidated" hospitals are those exposed to a merger event during the analytic period (2016–2021); "Never-Treated" hospitals were never exposed. Proportion variables (e.g., delivery method, transfers, race shares) are on a 0–1 scale. Counts reflect the full analytic sample: $N = 1{,}315{,}269$; treated $= 290{,}839$ ($22.11\%$); control $= 1{,}024{,}430$ ($77.89\%$).

Table 3: Descriptive Statistics by Treatment Group: Vital Statistics

| **Variable**<br>**Number of Births** | **Full Medicaid Sample**<br>**(N = 6,037,451)** | **Acquirer in County**<br>**(N = 1,637,607)** | **Never Treated**<br>**(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) |
| Infant mortality | 0.003 (0.052) | 0.003 (0.054) | 0.003 (0.052) |

*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 mergers or acquisitions between 2016 and 2021. All continuous variables are averaged across births and measured at the individual level.

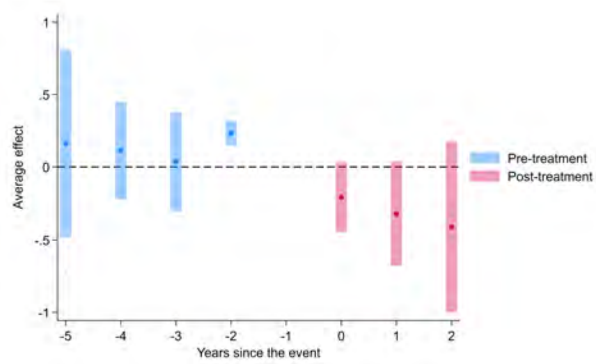
## Hospital Treatment Models

Table 4: Hospital-Level Aggregated Effects of Mergers on L&D Volume and Capacity

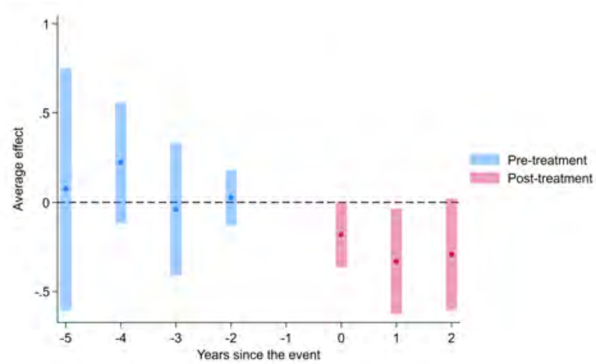
| | **Target** | **Acquirer** | **Combined** |
|---|---|---|---|
| *Changes in Volume & Capacity (N)* | *2874* | *2987* | *3231* |
| Volume: $\log(\text{Births})$ | -0.312* (0.188) | -0.269** (0.114) | -0.286*** (0.097) |
| Physical Capacity: $\log(\text{Beds})$ | -0.015 (0.016) | 0.014 (0.015) | 0.006 (0.012) |
| Staffing: $\log(\text{Unique Admitting NPIs})$ | -0.114 (0.076) | -0.140** (0.061) | -0.125*** (0.048) |
| *Changes in Clinical Outcomes (N)* | 1,107,350 | 1,189,321 | 1,276,023 |
| C-section | -0.008 (0.006) | -0.010* (0.006) | -0.011** (0.005) |
| Induced Labor | -0.009 (0.012) | 0.006 (0.008) | 0.005 (0.007) |

*Notes:* Entries report aggregated event-study estimates from Callaway and Sant'Anna (2021) Difference-in-Differences models using hospital-year data. Each coefficient represents the average post-merger effect for target hospitals (Target), acquiring hospitals (Acquirer), or both combined (Combined). Outcomes capture hospital delivery volume, physical capacity, staffing, and delivery method. Standard errors are cluster-robust at the hospital level and reported in parentheses. Estimates on this table correspond to Figures 3 to 7.
*Legend:* * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

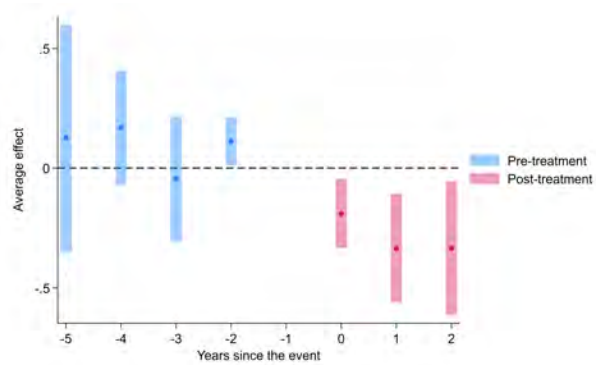
Figure 3: Hospital L&D Volume log(Births): Target, Acquirer, & Combined Effects




(a) Target Effect



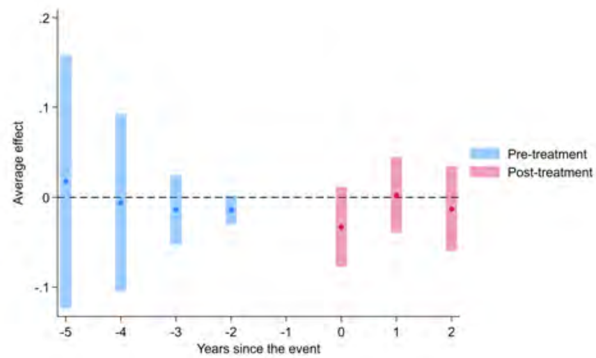

(b) Acquirer Effect



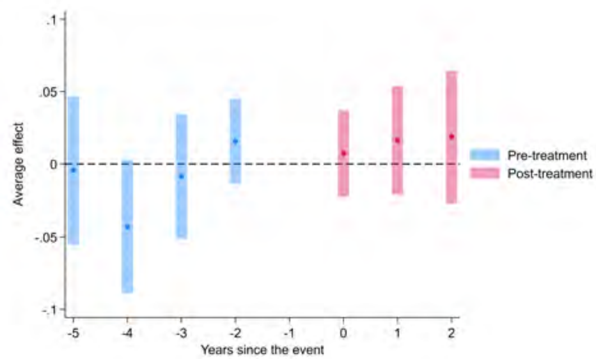

(c) Combined Effect

Notes: Estimates are derived from a Callaway and Sant'Anna (2021) difference-in-differences framework using hospital-year-level data aggregated from Medicaid TAF claisms. Treatment is defined as the year a hospital is first exposed to a mreger during the period 2016-2021. In addition to never-treated units, pre-treatment periods also serve as controls for treated units. We cluster standard errors at the hospital level.

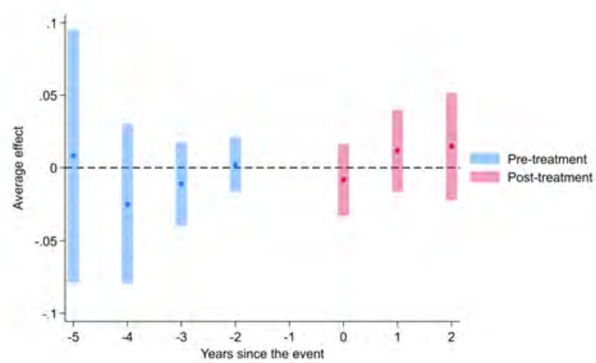
Figure 4: Hospital Physical Capital – $\log(\text{Hospital Beds})$: Target, Acquirer, & Combined Effects




(a) Target Effect



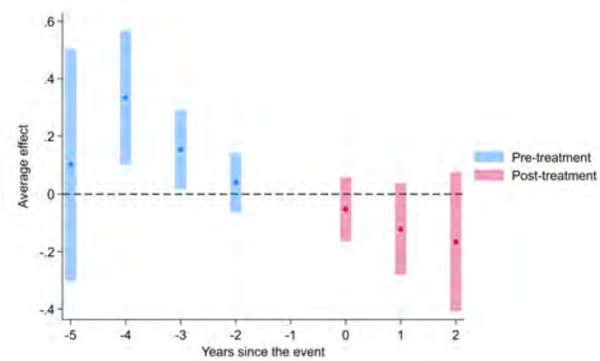

(b) Acquirer Effect



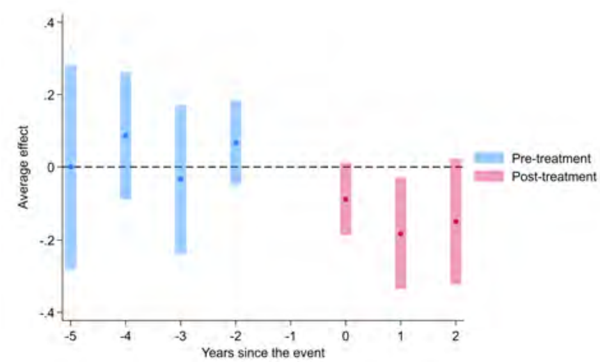

(c) Combined Effect

*Notes:* Estimates are derived from a Callaway and Sant'Anna (2021) difference-in-differences framework using hospital-year data. We get the log(hospital) outcome from the American Hospital Association dataset. Treatment is defined as the year a hospital is first exposed to a merger during 2016–2021. In addition to never-treated units, pre-treatment periods also serve as controls for treated units. We cluster standard errors at the hospital level.

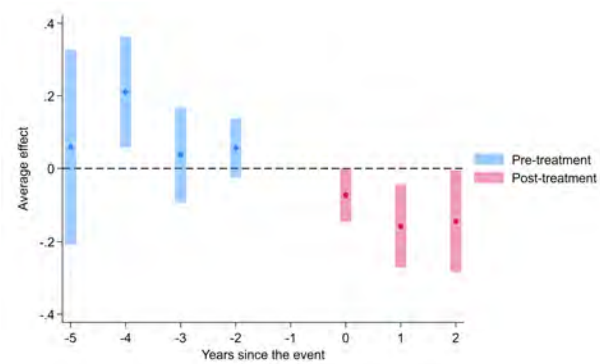
Figure 5: L&D Staffing Capacity – $\log(\text{Unique Admitting NPIs})$: Target, Acquirer, & Combined Effects




(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. Treatment is defined as the year a hospital is first exposed to a merger during 2016–2021. In addition to never-treated hospitals, pre-treatment periods for treated hospitals serve as controls. We cluster standard errors at the hospital level.

Table 5: Robustness Checks: Aggregated Post-Treatment Effects

| **A. Non-Expansion States (Exclude Late Medicaid Expansion States)** | | | |
|---|---|---|---|
| | **Target** | **Acquirer** | **Consolidation** |
| log(Births) | $-0.252$ (0.192) | $-0.384^{***}$ (0.119) | $-0.336^{***}$ (0.102) |
| log(iHOSPBD) | $-0.019$ (0.017) | $0.012$ (0.017) | $0.003$ (0.012) |
| log(Unique Admitting NPIs) | $-0.096$ (0.077) | $-0.180^{**}$ (0.072) | $-0.141^{***}$ (0.053) |
| **B. Pre-COVID Sample (2016–2019)** | | | |
| | **Target** | **Acquirer** | **Consolidation** |
| log(Births) | $-0.654^{**}$ (0.307) | $-0.342^{**}$ (0.170) | $-0.441^{***}$ (0.152) |
| log(iHOSPBD) | $-0.020$ (0.026) | $0.023$ (0.022) | $0.014$ (0.018) |
| log(Unique Admitting NPIs) | $-0.223$ (0.140) | $-0.149$ (0.092) | $-0.170^{**}$ (0.076) |

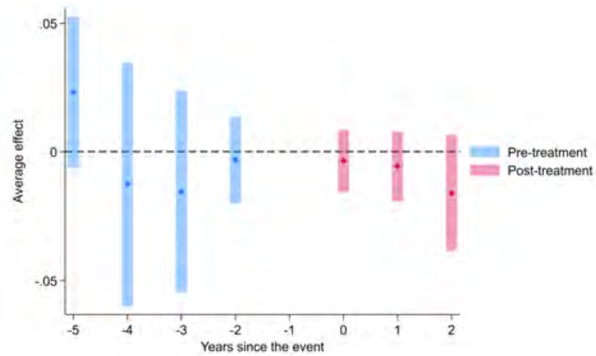
*Notes:* Entries are aggregated post-treatment effects from Callaway & Sant'Anna (2021) DiD event-study estimations; standard errors in parentheses, clustered at the hospital level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Approximate percent change $\approx 100 \times (e^{\hat{\beta}} - 1)$. Panel A excludes states that expanded Medicaid during 2016–2021; Panel B restricts the sample to 2016–2019.

Table 6: Aggregated Post-Treatment Effects Across Heterogeneity Dimensions

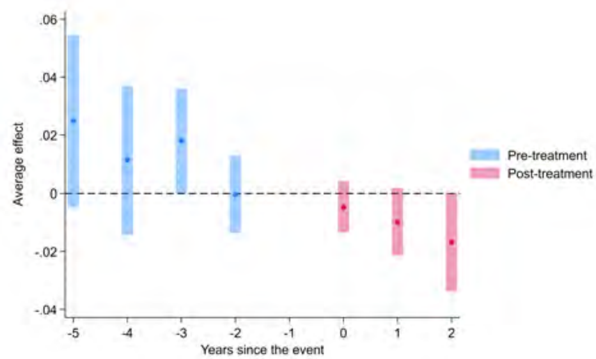
| Group | Target | Acquirer | Consolidation |
|---|---|---|---|
| *HHI – Upper Half* | | | |
| log(Births) | $-0.592^{**}$ (0.242) | $-0.626^{***}$ (0.229) | $-0.608^{***}$ (0.168) |
| log(Beds) | $-0.017$ (0.024) | 0.005 (0.026) | $-0.006$ (0.018) |
| log(Unique NPIs) | $-0.254^{**}$ (0.112) | $-0.327^{***}$ (0.124) | $-0.291^{***}$ (0.086) |
| *HHI – Lower Half* | | | |
| log(Births) | 0.166 (0.188) | $-0.023$ (0.082) | $-0.001$ (0.078) |
| log(Beds) | 0.004 (0.010) | 0.017 (0.018) | 0.014 (0.014) |
| log(Unique NPIs) | 0.097 (0.043) | 0.009 (0.049) | $-0.031$ (0.038) |
| *Not-for-Profit Hospitals* | | | |
| log(Births) | $-0.309$ (0.204) | $-0.230^{**}$ (0.110) | $-0.258^{***}$ (0.099) |
| log(Beds) | $-0.000$ (0.009) | 0.018 (0.016) | 0.011 (0.011) |
| log(Unique NPIs) | $-0.136$ (0.083) | $-0.128^{**}$ (0.060) | $-0.124^{**}$ (0.049) |
| *Urban Hospitals* | | | |
| log(Births) | $-0.295$ (0.249) | $-0.443^{***}$ (0.155) | $-0.390^{***}$ (0.130) |
| log(Beds) | $-0.006$ (0.010) | 0.018 (0.018) | 0.011 (0.012) |
| log(Unique NPIs) | $-0.140$ (n.s.) | $-0.179$ (0.085) | $-0.155^{**}$ (0.065) |
| *Rural Hospitals* | | | |
| log(Births) | $-0.501^{**}$ (0.235) | 0.116 (0.140) | $-0.097$ (0.141) |
| log(Beds) | $-0.054$ (0.057) | $-0.034$ (0.030) | $-0.040$ (0.028) |
| log(Unique NPIs) | $-0.102$ (0.101) | $-0.064$ (0.076) | $-0.076$ (0.062) |

*Notes:* Entries are aggregated post-treatment effects from Callaway & Sant'Anna (2021) DiD event-study models; standard errors shown in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Approximate percent change $\approx 100 \times (e^{\hat{\beta}} - 1)$. HHI split based on 2016 median HHI (0.1849); Not-for-Profit subsample excludes government and for-profit facilities.

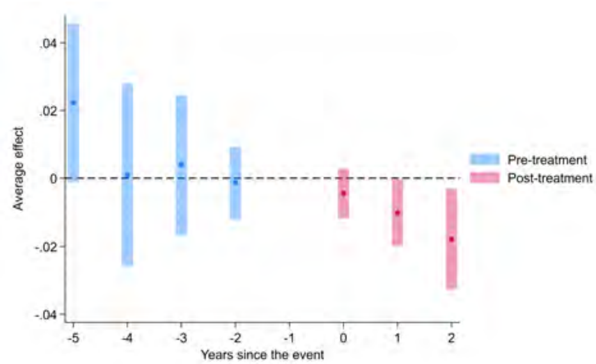
Figure 6: L&D Care Quality – C-section: Target, Acquirer, & Combined Effects




(a) Target Effect



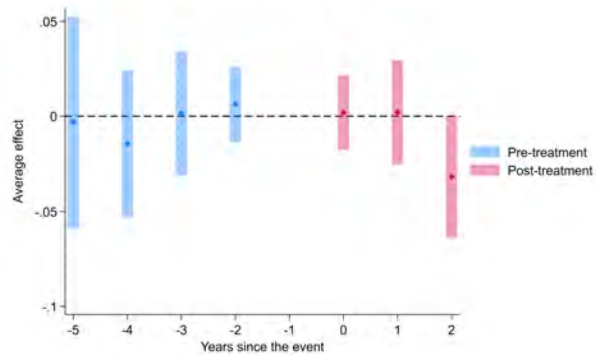

(b) Acquirer Effect



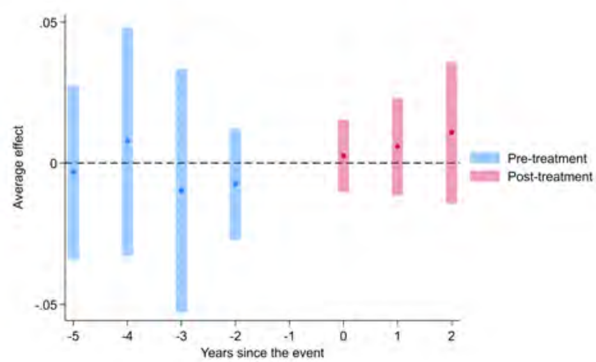

(c) Combined Effect

*Notes:* Estimates are derived from a Callaway and Sant'Anna (2021) difference-in-differences framework using Medicaid TAF claims-dervied 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. Treatment is defined as the year a hospital is first exposed to a merger during 2016–2021. In addition to never-treated hospitals, pre-treatment periods for treated hospitals serve as controls. We cluster standard errors at the hospital level.

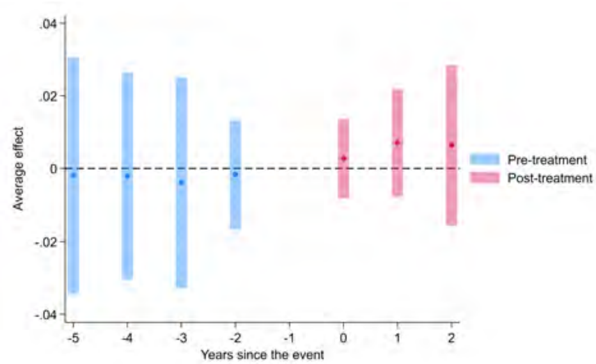
Figure 7: L&D Care Quality – Induced Labor: Target, Acquirer, & Combined Effects




(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-dervied 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. Treatment is defined as the year a hospital is first exposed to a merger during 2016–2021. In addition to never-treated hospitals, pre-treatment periods for treated hospitals serve as controls. We cluster standard errors at the hospital level.

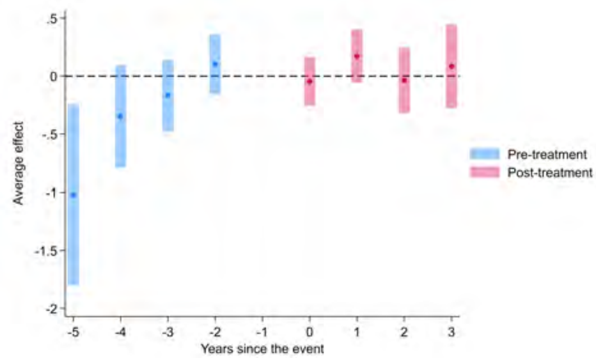
## County Treatment Models

Table 7: Equilibrium Adjustment Effects of Acquirers: Aggregated Estimates by Outcome

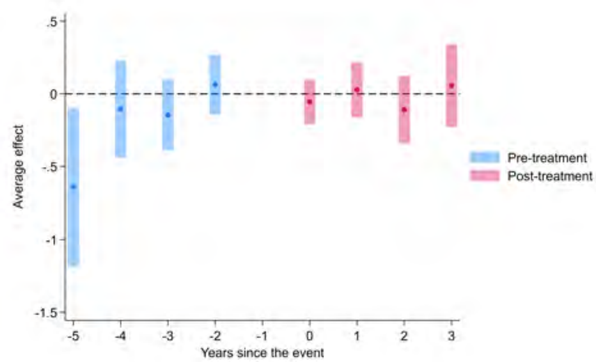
| | **Overall Births** | **Medicaid** |
|---|---|---|
| **N (County-years – Volume)** | 13,511 | 13,511 |
| Medicaid births per 1,000 women | -0.043 (0.106) | -0.019 (0.087) |
| **N (Births — Access)** | 13,053,447 | 5,300,767 |
| Same-county birth | 0.007* (0.004) | 0.009** (0.004) |
| Physician-attended 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) |
| Infant mortality | -0.000 (0.000) | -0.0003 (0.0002) |
| 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 8 to 14, which present dynamic effects by outcome and insurance group. Standard errors (in parentheses) are cluster-robust at the county level.
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 (infant mortality, 5-minute Apgar score, and C-sections).
*Legend:* * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Figure 8: County-Level Fertility – Medicaid births per 1,000 Women: Acquirer Effects (Overall & Medicaid)



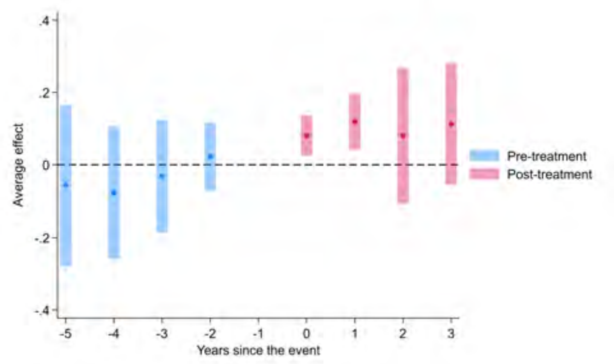

(a) Overall (All Payers)



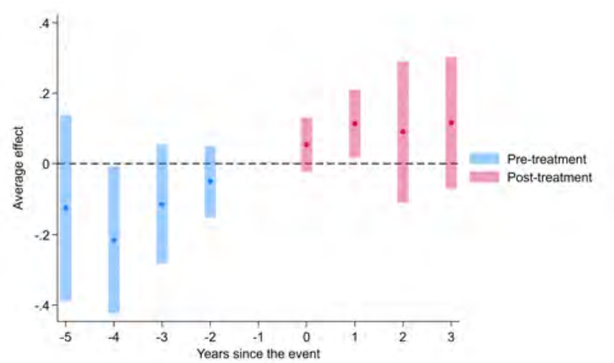

(b) Medicaid

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



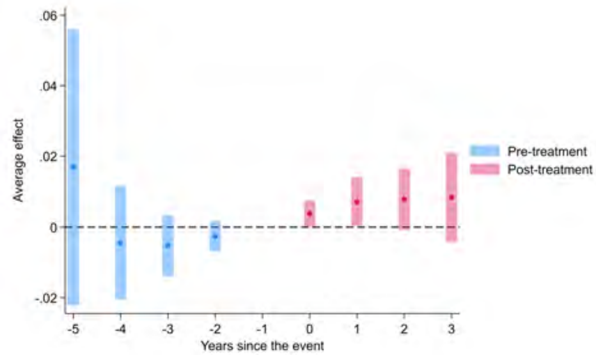

(a) Overall (All Payers)



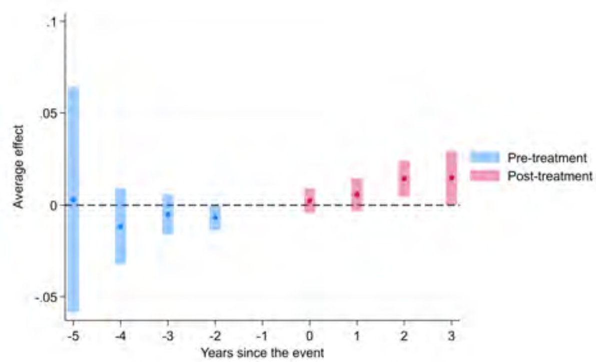

(b) Medicaid

*Notes:* Event-study estimates from Callaway and Sant'Anna (2021) difference-in-differences using birth data. The outcome is the average 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. Never-treated counties and pre-treatment periods for treated births serve as controls. We cluster standard errors at the county level.

Figure 10: County-Level Access – Same-County Birth: Acquirer Effects (Overall & Medicaid)



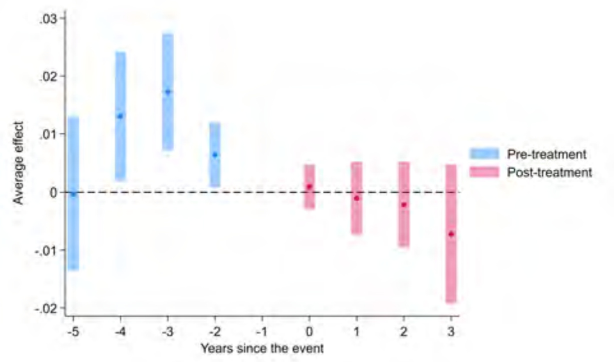

(a) Overall (All Payers)



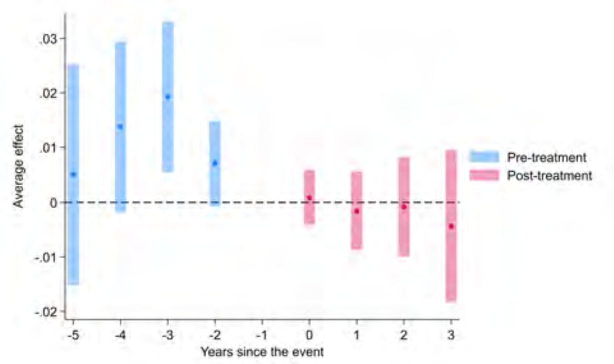

(b) Medicaid

*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. Never-treated counties and pre-treatment periods for treated births serve as controls. We cluster standard errors at the county level.

Figure 11: County-Level Care Setting – Physician-Attended Births: Acquirer Effects (Overall & Medicaid)



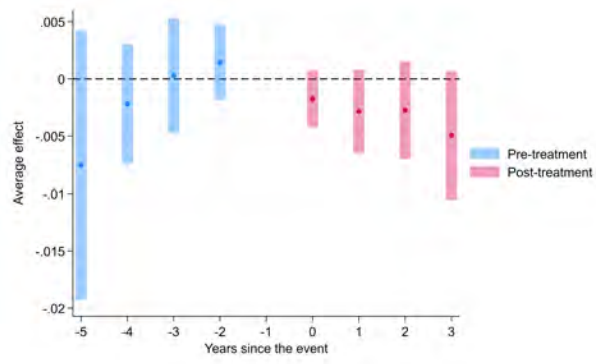

(a) Overall (All Payers)



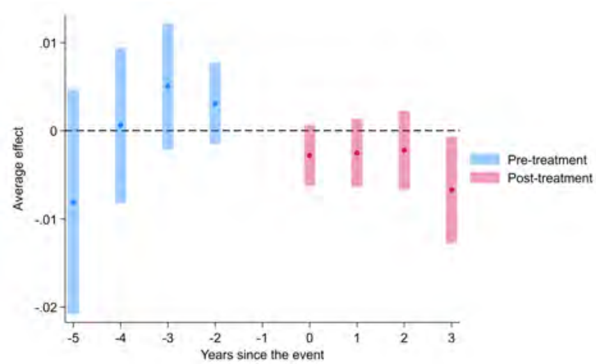

(b) Medicaid

*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. Never-treated counties and pre-treatment periods for treated births serve as controls. We cluster standard errors at the county level.

Figure 12: County-Level L&D Care Quality – C-section Rate: Acquirer Effects (Overall & Medicaid)



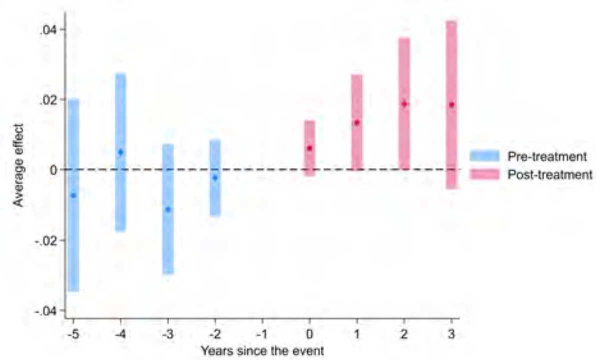

(a) Overall (All Payers)



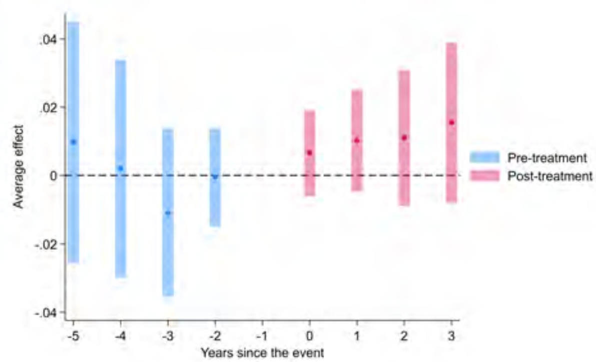

(b) Medicaid

*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. Never-treated counties and pre-treatment periods for treated births serve as controls. We cluster standard errors at the county level.

Figure 13: County-Level Outcomes – Five-Minute Apgar Score: Acquirer Effects (Overall & Medicaid)



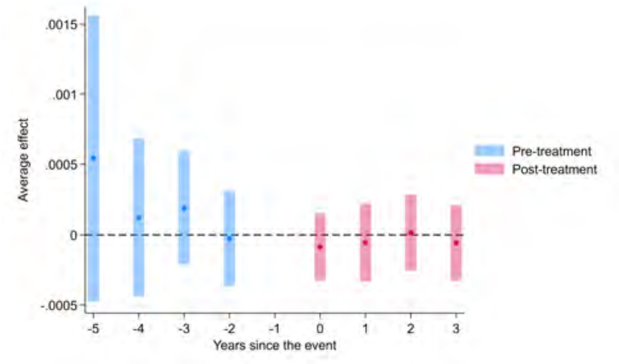

(a) Overall (All Payers)



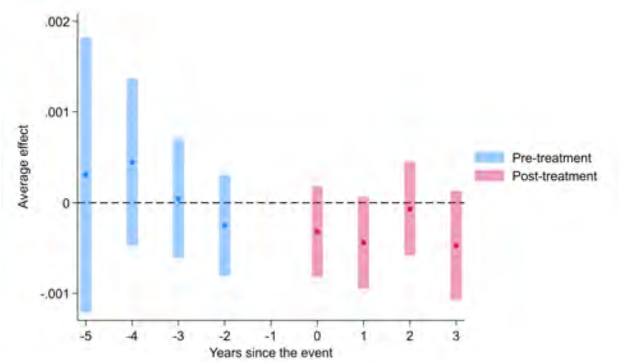

(b) Medicaid

*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. Never-treated counties and pre-treatment periods for treated births serve as controls. We cluster standard errors at the county level.

Figure 14: County-Level Outcomes – Infant Mortality: Acquirer Effects (Overall & Medicaid)



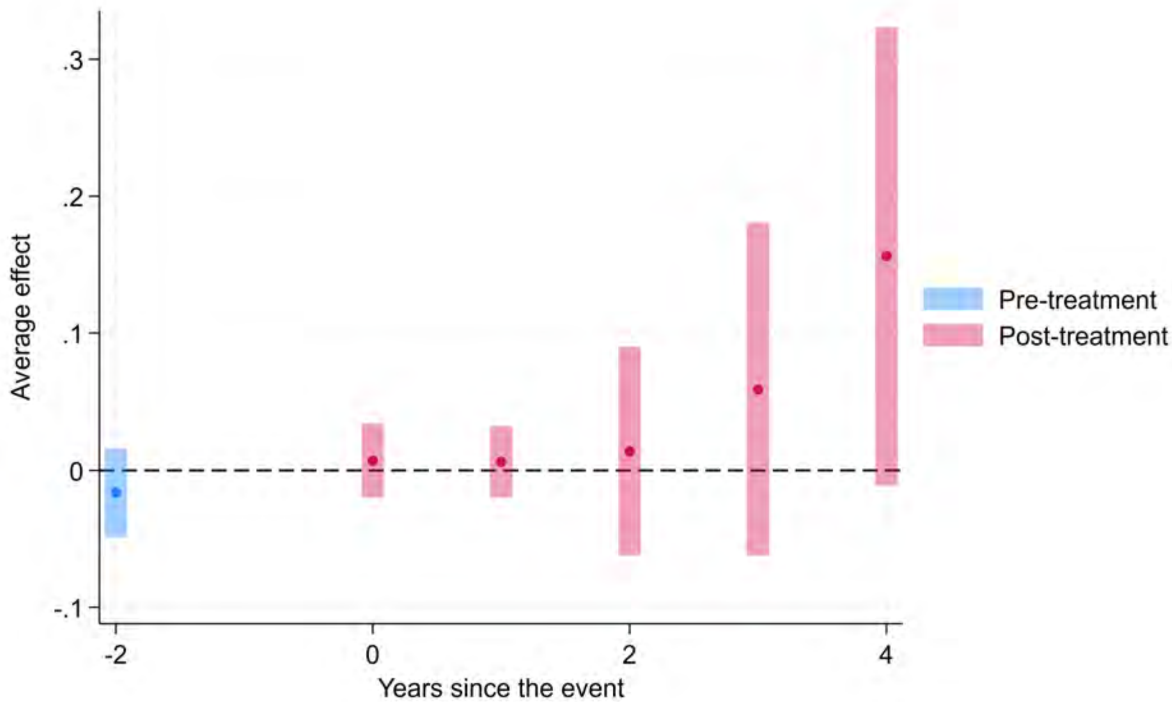

(a) Overall (All Payers)




(b) Medicaid

*Notes:* Event-study estimates from Callaway and Sant'Anna (2021) difference-in-differences using birth data. The outcome is the infant mortality rate (deaths before age 1 per 1,000 live births), 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. Never-treated counties and pre-treatment periods for treated births serve as controls. We cluster standard errors at the county level.

Figure 15: Mechanism Check: Public Facilities (County Merger Exposure)




*Notes:* Event-study estimates from a Callaway and Sant'Anna (2021) DiD. Outcome: indicator for whether birth occurred at a public facility. We analyze Medicaid TAF L&D claims data and defined our treatment as the first county exposure to a hospital consolidation event. Never-treated births and pre-treatment periods serve as control births We adjust for mother's age and Elixhauser comorbidities. Standard errors clustered at the county level.

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# APPENDIX

# 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 (ownership, beds, teaching status, 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 (maternal/infant characteristics and outcomes) used for general equilibrium analyses. |
| 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:* CCN = CMS Certification Number; FIPS = Federal Information Processing Series code; RUCC/UIC/RUCA = USDA ERS rural/urban classification schemes.

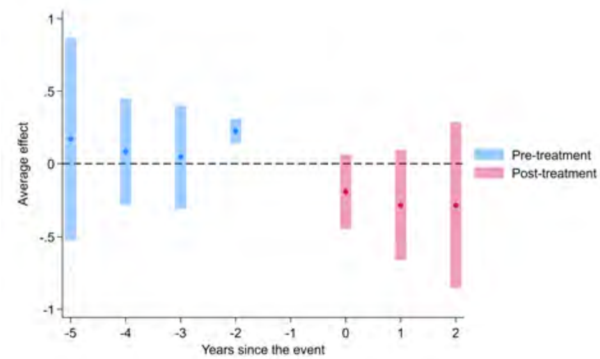
Table A2: Algorithms for Identifying Births, Cesarean Deliveries, and Induced Labor in Claims

| **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 |
| **Induced Labor** | – | O610, O611, O618, O619 | Oxytocin 3E033VJ; Prostaglandins/cervical ripening 3E0P7GC; Mechanical dilation 0U7C7ZZ (legacy 0U7C7DZ) |

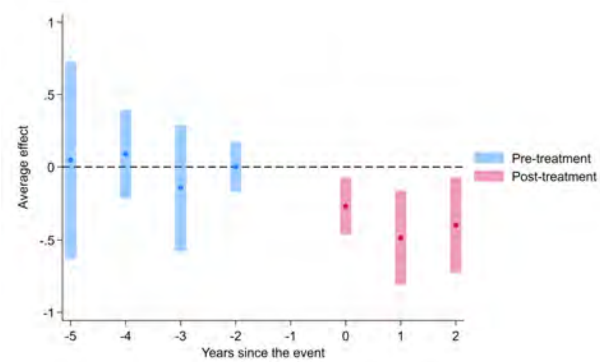
*Note:* Codes are matched case-insensitively across diagnosis (DGNS_CD_1–DGNS_CD_12), procedure (ICD_PRCDR_CD1–ICD_PRCDR_CD6), and DRG (CLM_DRG_CD) fields.

# Robustness—Exclude 2016–2021 Medicaid Expansion States

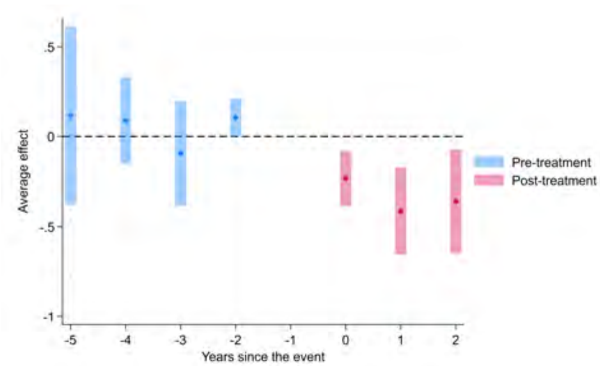
Figure A1: Robustness (Non-Expansion States): L&D Volume — $\log(\text{Births})$ (Target, Acquirer, & Consolidation)




(a) Target Effect (Non-Expansion)



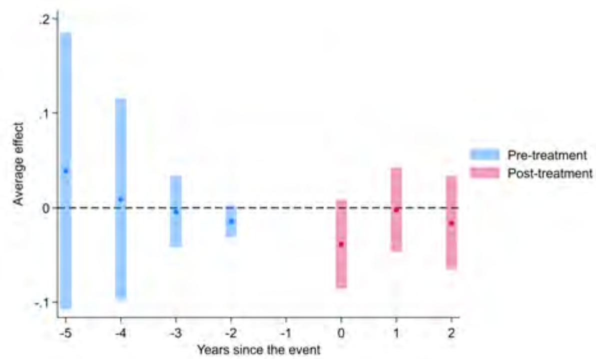

(b) Acquirer Effect (Non-Expansion)



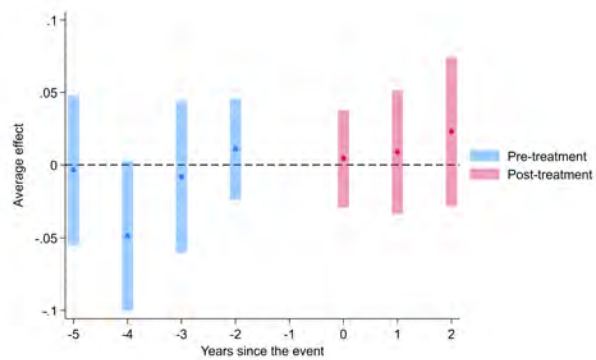

(c) Consolidation Effect (Non-Expansion)

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

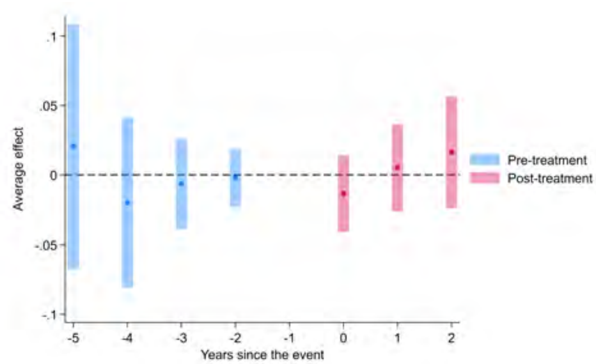
Figure A2: Robustness (Non-Expansion States): Bed Capacity — $\log(\text{Beds})$ (Target, Acquirer, & Consolidation)




(a) Target Effect (Non-Expansion)



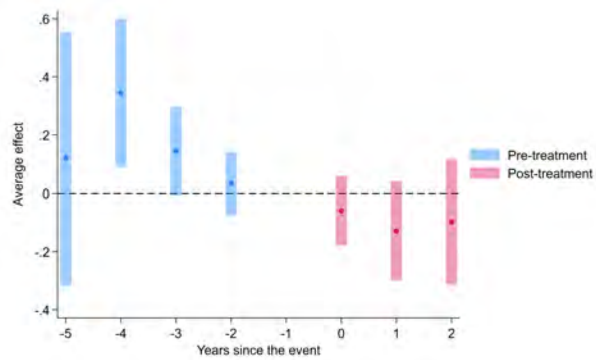

(b) Acquirer Effect (Non-Expansion)



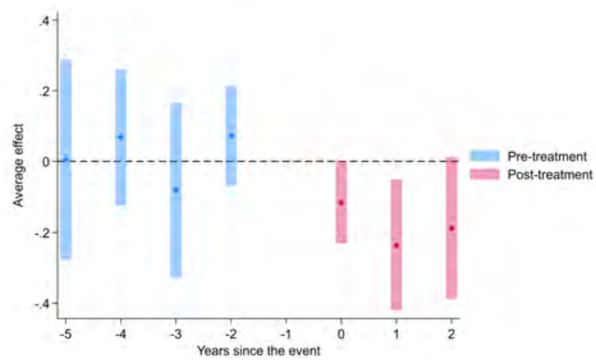

(c) Consolidation Effect (Non-Expansion)

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

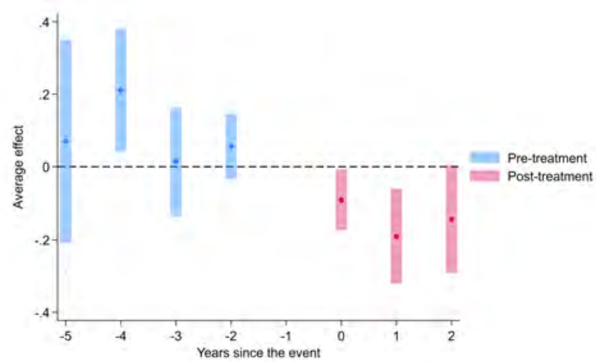
Figure A3: Robustness (Non-Expansion States): Staffing Capacity — $\log(\text{Unique Admitting Providers})$ (Target, Acquirer, & Consolidation)




(a) Target Effect (Non-Expansion)




(b) Acquirer Effect (Non-Expansion)

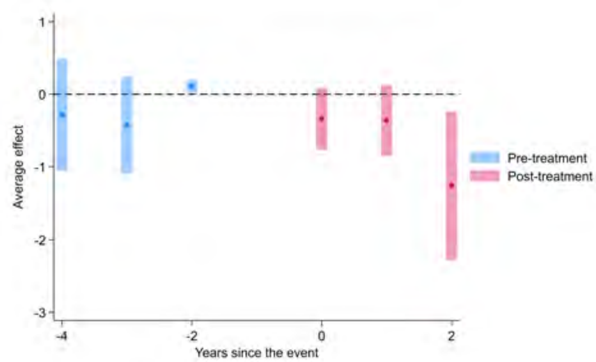



(c) Consolidation Effect (Non-Expansion)

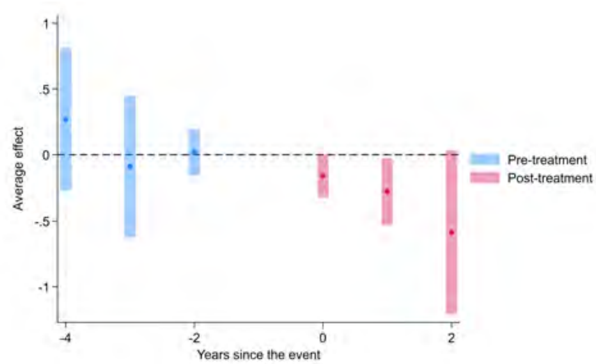
*Note:* Excludes states that expanded Medicaid between 2016–2021: ME, MT, LA, VA, ID, UT, NE, OK, and MO.

# Robustness—Pre-COVID Period (2016–2019)

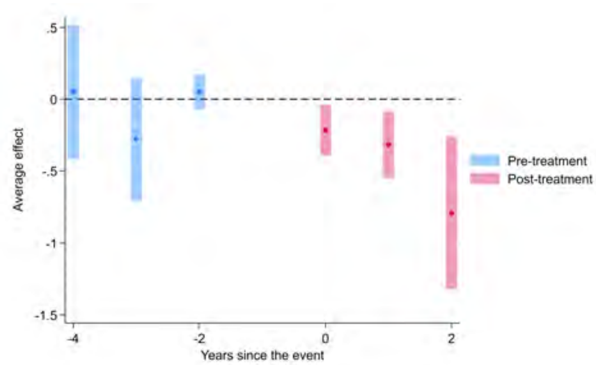
Figure A4: Robustness (Pre-COVID 2016–2019): L&D Volume — $\log(\text{Births})$ (Target, Acquirer, & Consolidation)




(a) Target Effect (Pre-COVID)



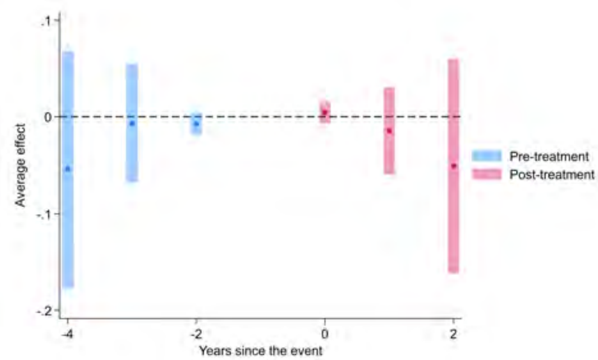

(b) Acquirer Effect (Pre-COVID)



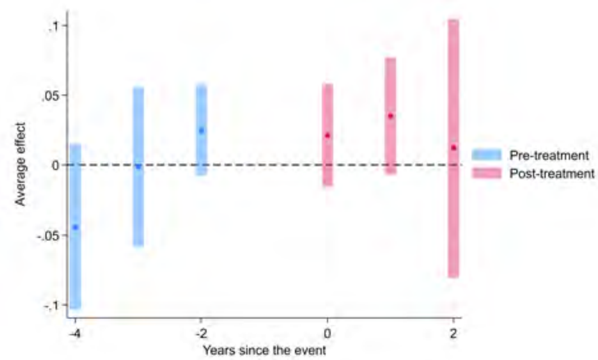

(c) Consolidation Effect (Pre-COVID)

*Note:* This robustness check restricts the analytic sample to the pre-COVID period (2016–2019), excluding observations from 2020 onward to ensure results are not influenced by pandemic-related disruptions.

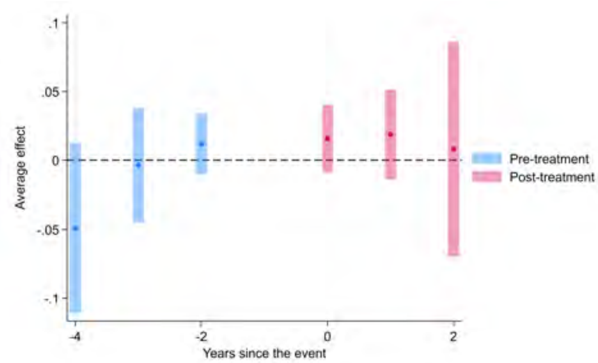
Figure A5: Robustness (Pre-COVID 2016–2019): Bed Capacity — $\log(\text{iHOSPBD})$ (Target, Acquirer, & Consolidation)




(a) Target Effect (Pre-COVID)



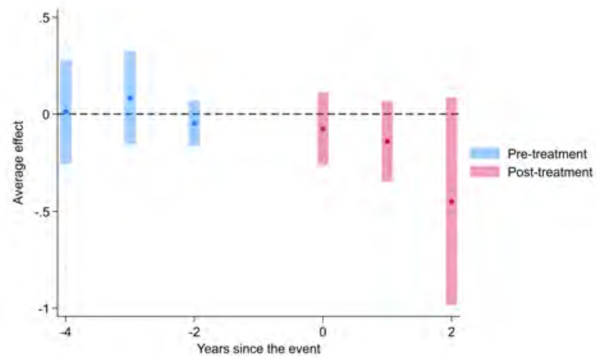

(b) Acquirer Effect (Pre-COVID)



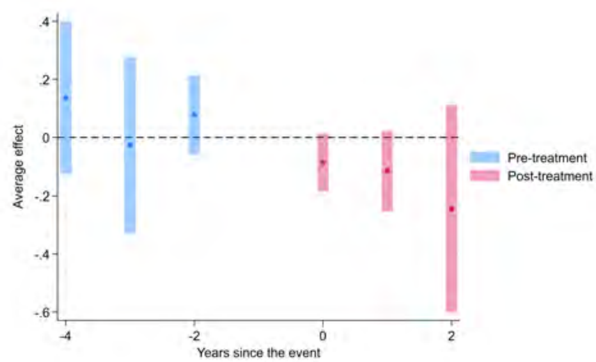

(c) Consolidation Effect (Pre-COVID)

*Note:* The sample is restricted to 2016–2019, prior to the onset of COVID-19, to isolate merger effects from pandemic-era capacity shocks.

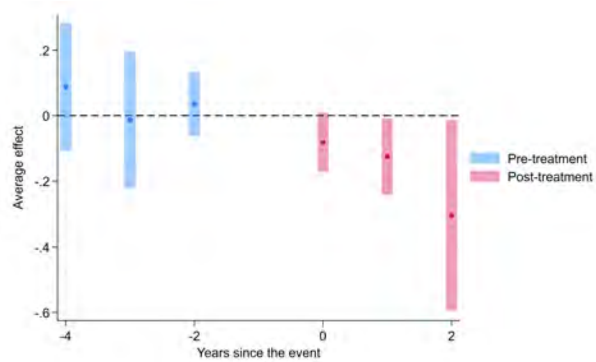
Figure A6: Robustness (Pre-COVID 2016–2019): Staffing Capacity — $\log(\text{Unique Admitting Providers})$ (Target, Acquirer, & Consolidation)




(a) Target Effect (Pre-COVID)




(b) Acquirer Effect (Pre-COVID)

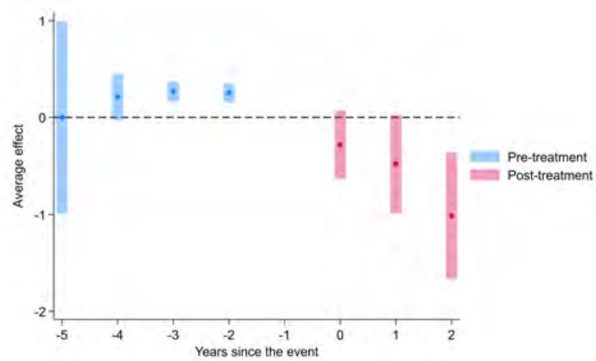



(c) Consolidation Effect (Pre-COVID)

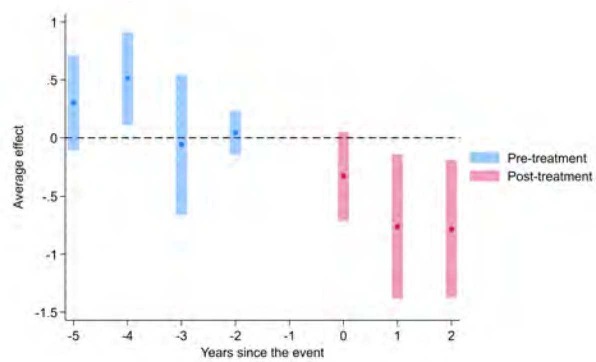
*Note:* Results are limited to the 2016–2019 pre-COVID period to verify that observed patterns are not driven by pandemic-induced changes in staffing or admission practices.

# Heterogeneity—Upper and Lower Half of HHI

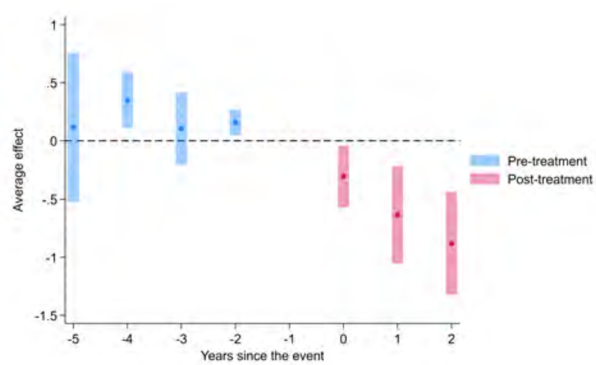
Figure A7: Upper HHI HRRs: L&D Volume — $\log(\text{Medicaid Births})$ (Target, Acquirer, & Consolidation)




(a) Target Effect (Upper HHI)



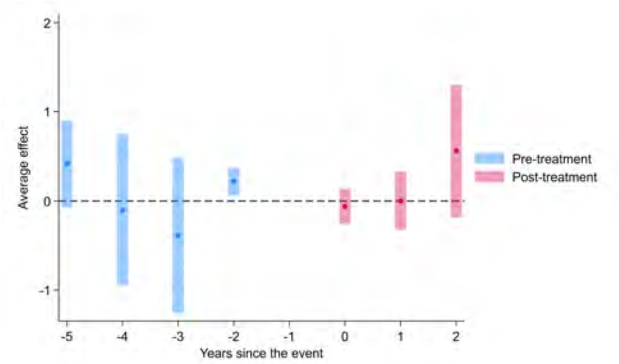

(b) Acquirer Effect (Upper HHI)



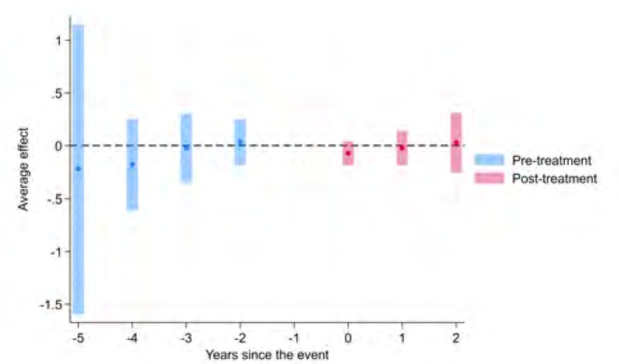

(c) Consolidation Effect (Upper HHI)

*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.

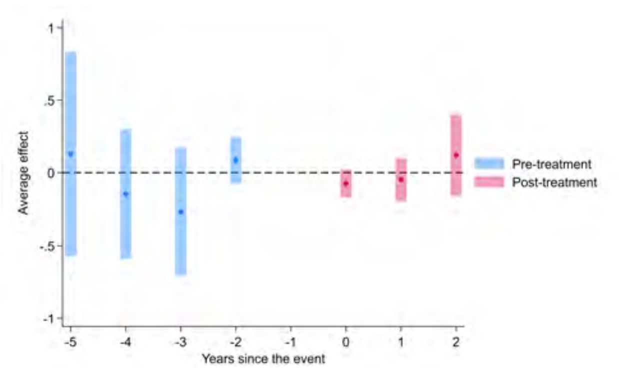
Figure A8: Lower HHI HRRs: L&D Volume — $\log(\text{Medicaid Births})$ (Target, Acquirer, & Consolidation)




(a) Target Effect (Lower HHI)



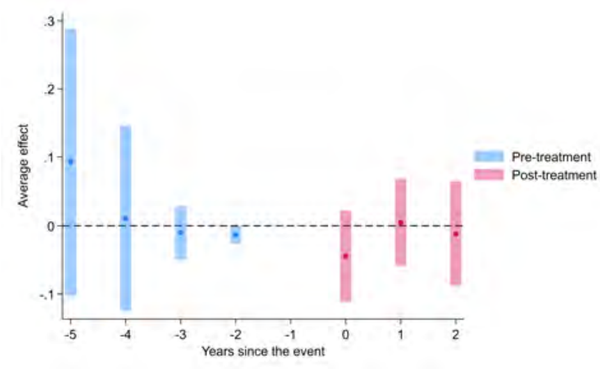

(b) Acquirer Effect (Lower HHI)



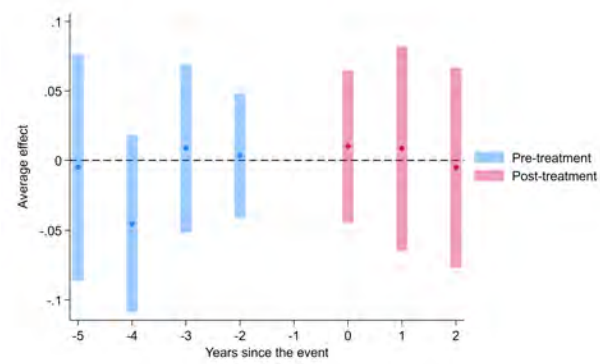

(c) Consolidation Effect (Lower HHI)

*Notes:* Same DiD setup as Figure A7, but for HRRs with 2016 hospital system HHI $\leq$ median.

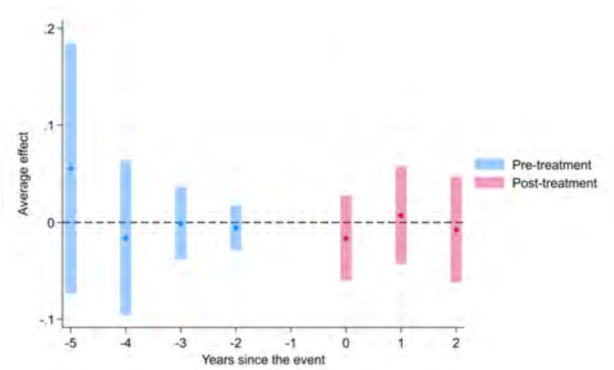
Figure A9: Upper HHI HRRs: Bed Capacity — $\log(\text{Beds})$ (Target, Acquirer, & Consolidation)




(a) Target Effect (Upper HHI)



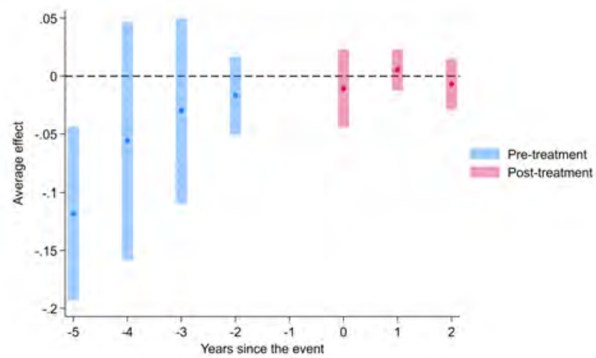

(b) Acquirer Effect (Upper HHI)



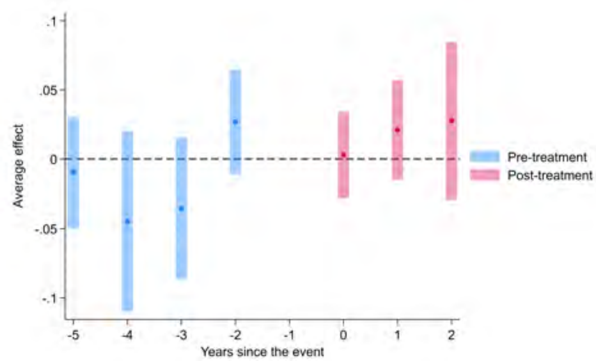

(c) Consolidation Effect (Upper HHI)

*Notes:* DiD per Callaway–Sant'Anna (2021). Outcome: $\log(\text{Beds})$ (AHA staffed inpatient beds). Subsample: HRRs with above-median 2016 hospital system HHI.

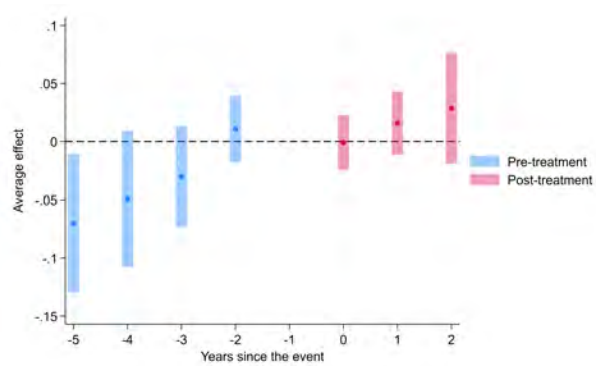
Figure A10: Lower HHI HRRs: Bed Capacity — $\log(\text{Beds})$ (Target, Acquirer, & Consolidation)




(a) Target Effect (Lower HHI)



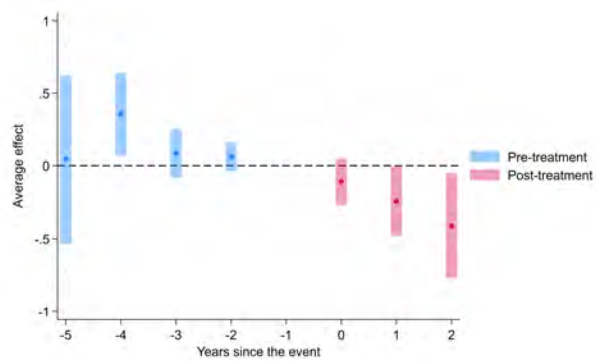

(b) Acquirer Effect (Lower HHI)



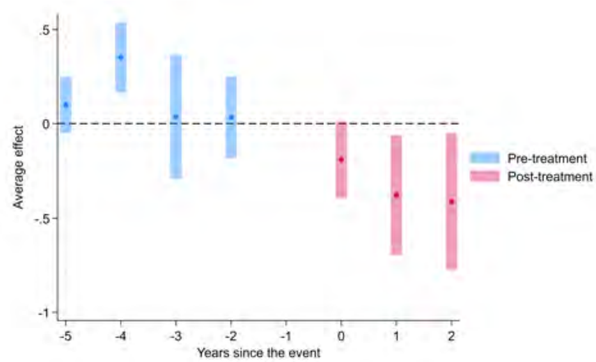

(c) Consolidation Effect (Lower HHI)

*Notes:* Same as Figure A9, but for HRRs with 2016 hospital system HHI $\leq$ median.

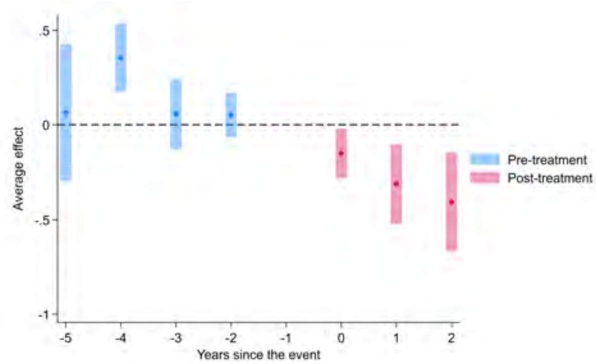
Figure A11: Upper HHI HRRs: L&D Staffing Capacity — $\log$(Unique Admitting NPIs) (Target, Acquirer, & Consolidation)




(a) Target Effect (Upper HHI)



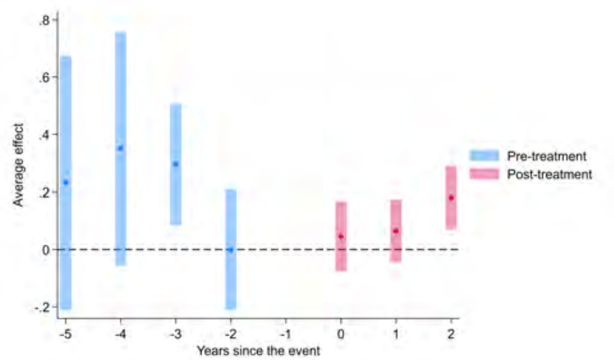

(b) Acquirer Effect (Upper HHI)



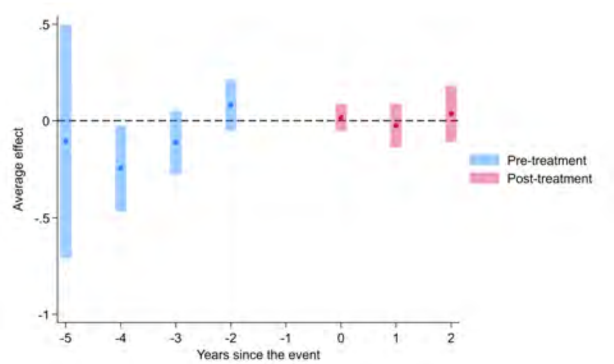

(c) Consolidation Effect (Upper HHI)

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

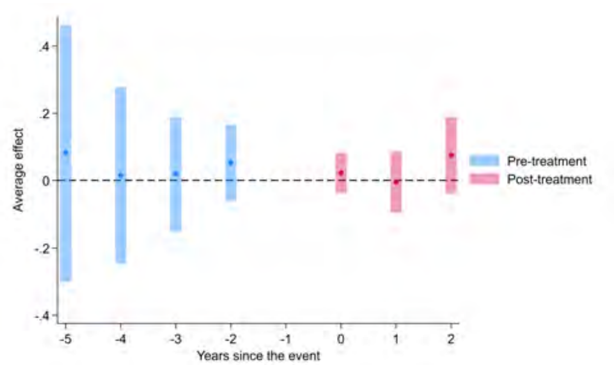
Figure A12: Lower HHI HRRs: L&D Staffing Capacity — $\log(\text{Unique Admitting NPIs})$ (Target, Acquirer, & Consolidation)




(a) Target Effect (Lower HHI)




(b) Acquirer Effect (Lower HHI)

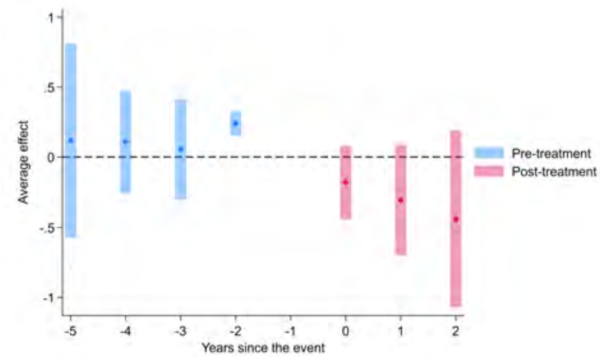



(c) Consolidation Effect (Lower HHI)

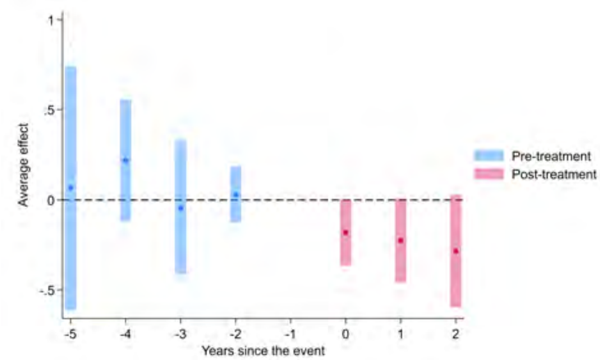
*Notes:* Same as Figure A11, but for HRRs with 2016 hospital system HHI $\leq$ median.

# Heterogeneity—Not-for-Profit Hospitals

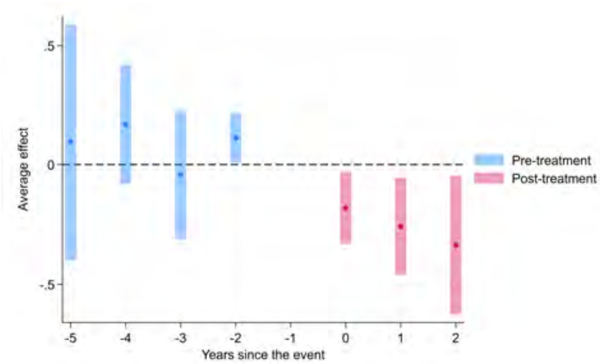
Figure A13: Not-for-Profit Hospitals: L&D Volume — $\log(\text{Births})$ (Target, Acquirer, & Consolidation)




(a) Target Effect (Not-for-Profit)

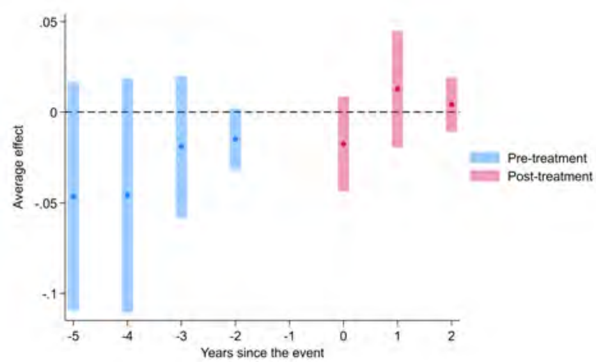



(b) Acquirer Effect (Not-for-Profit)

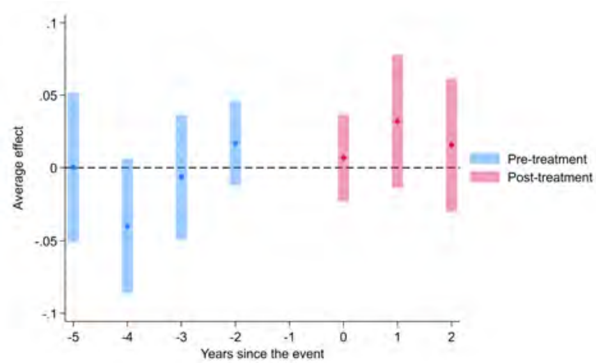



(c) Consolidation Effect (Not-for-Profit)

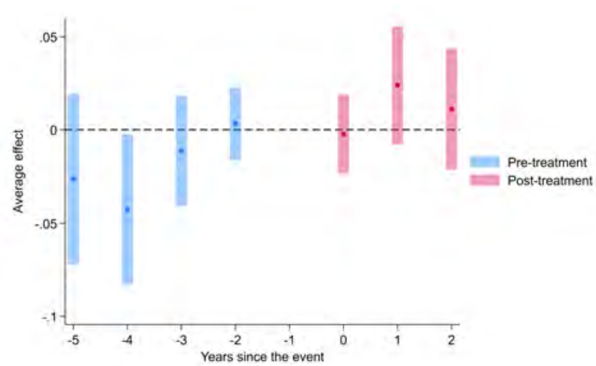
Figure A14: Not-for-Profit Hospitals: Bed Capacity — $\log(\text{Beds})$ (Target, Acquirer, & Consolidation)




(a) Target Effect (Not-for-Profit)

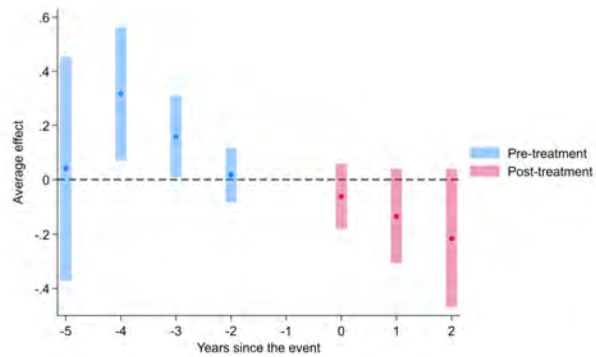



(b) Acquirer Effect (Not-for-Profit)

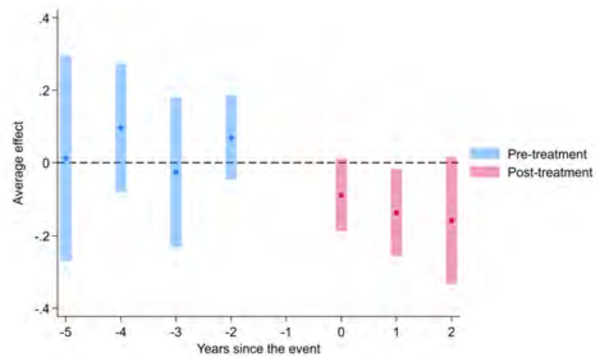



(c) Consolidation Effect (Not-for-Profit)

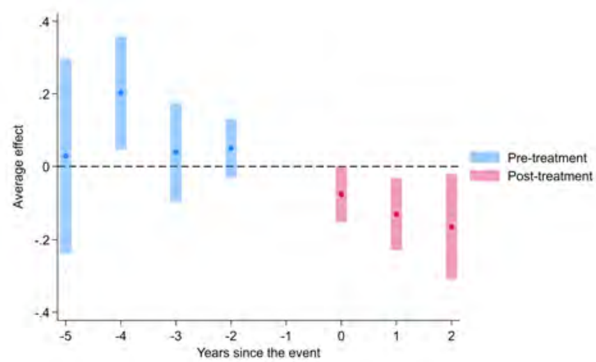
Figure A15: Not-for-Profit Hospitals: Staffing Capacity — $\log($Unique Admitting Providers$)$ (Target, Acquirer, & Consolidation)




(a) Target Effect (Not-for-Profit)



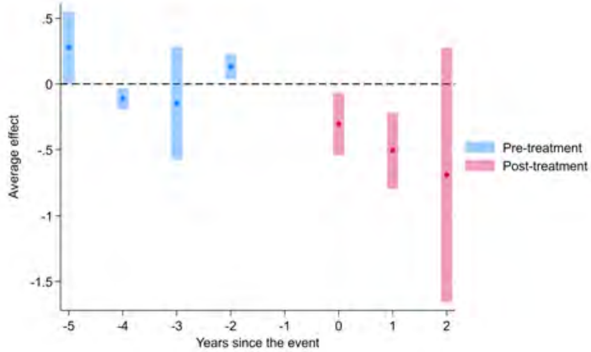

(b) Acquirer Effect (Not-for-Profit)



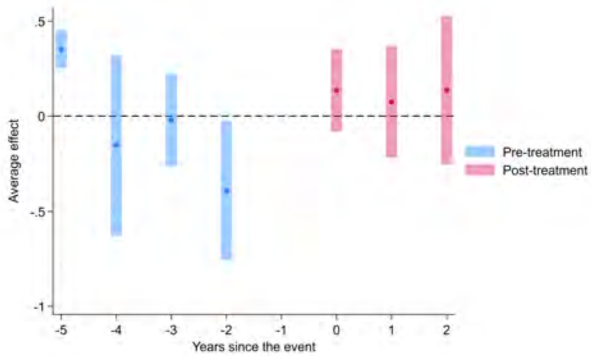

(c) Consolidation Effect (Not-for-Profit)

# Heterogeneity—Urban vs. Rural

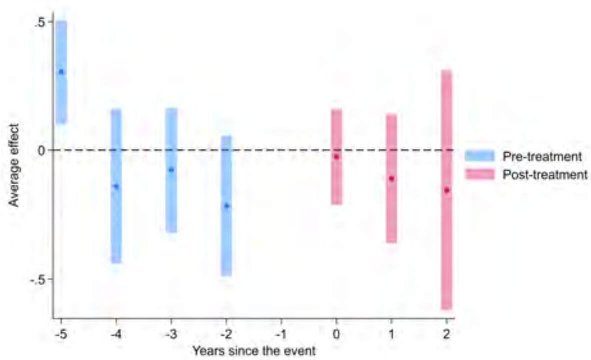
Figure A16: Rural Hospitals: L&D Volume $\log(\text{Births})$ (Target, Acquirer, & Combined Effects)




(a) Target Effect (Rural)



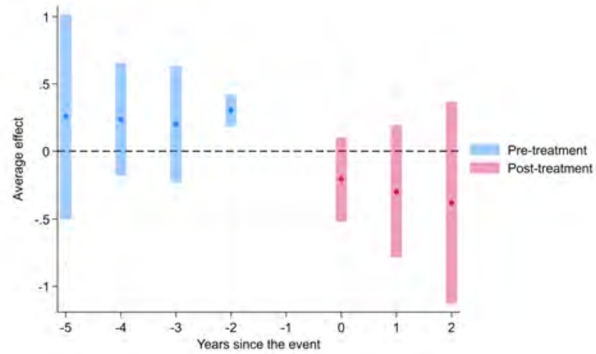

(b) Acquirer Effect (Rural)



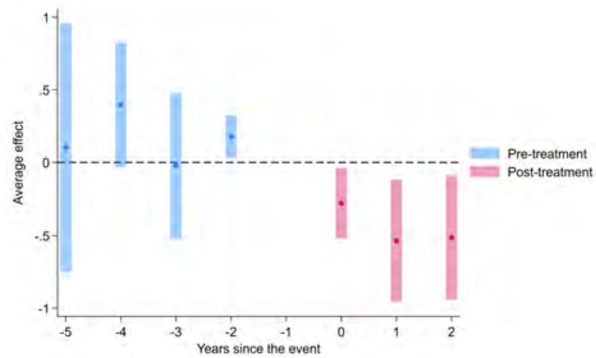

(c) Combined Effect (Rural)

*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 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. Never-treated hospitals and pre-treatment periods for treated hospitals serve as controls. We cluster standard errors at the hospital level.

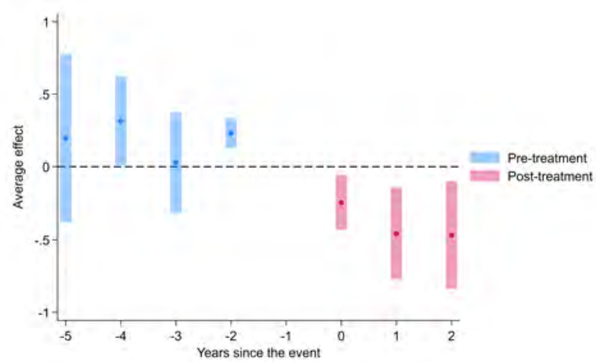
Figure A17: Urban Hospitals: L&D Volume $\log(\text{Births})$ (Target, Acquirer, & Combined Effects)




(a) Target Effect (Urban)



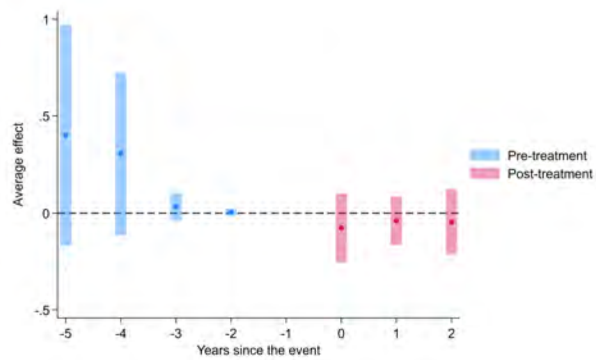

(b) Acquirer Effect (Urban)



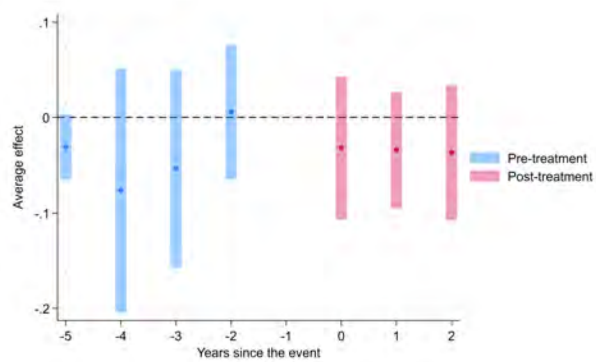

(c) Combined Effect (Urban)

*Notes:* Same specification as Figure A16, restricted to *urban* hospitals. DiD per Callaway and Sant'Anna (2021); treatment defined as first within-HRR merger year (2016–2021); never-treated and pre-treatment periods serve as controls; standard errors clustered at the hospital level.

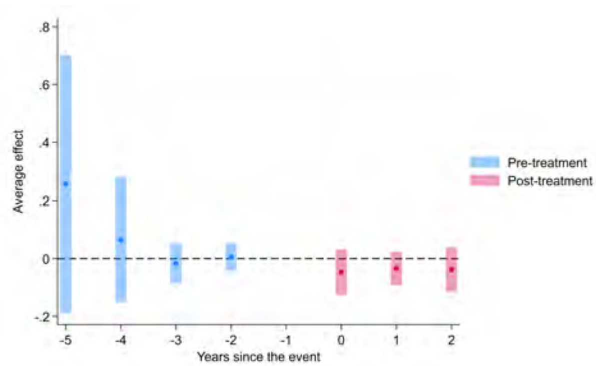
Figure A18: Rural Hospitals: Physical Capital $\log(\text{Hospital Beds})$ (Target, Acquirer, & Combined Effects)




(a) Target Effect (Rural)



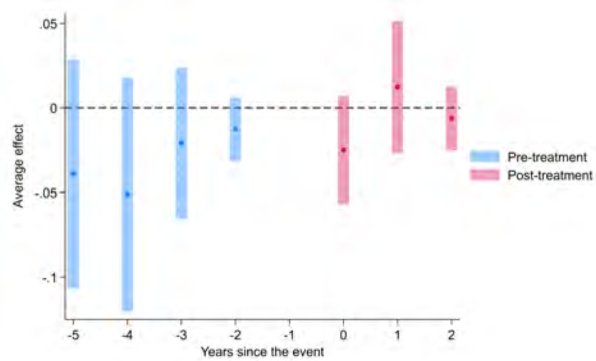

(b) Acquirer Effect (Rural)



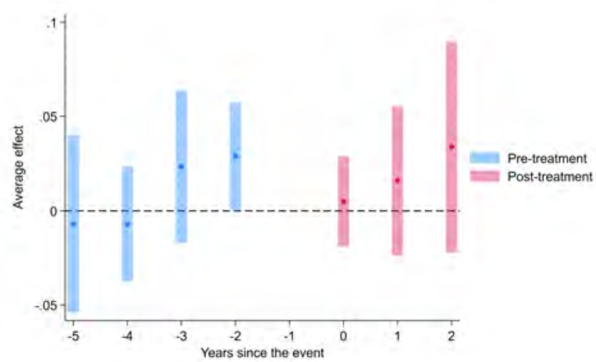

(c) Combined Effect (Rural)

*Notes:* Callaway and Sant'Anna (2021) DiD using hospital-year data; outcome is $\log(\text{beds})$ from the AHA Annual Survey; rural hospitals only. Treatment is first merger exposure (2016–2021). Controls include never-treated hospitals and pre-treatment periods; SEs clustered at the hospital level.

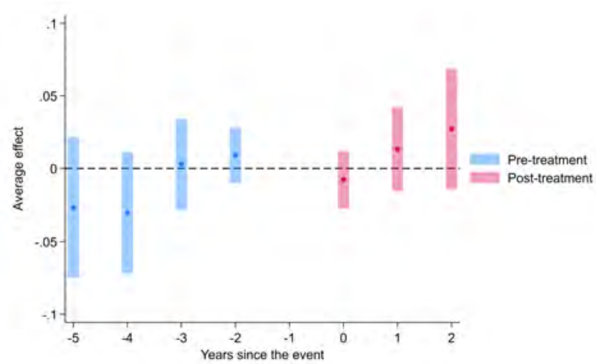
Figure A19: Urban Hospitals: Physical Capital $\log(\text{Hospital Beds})$ (Target, Acquirer, & Combined Effects)




(a) Target Effect (Urban)



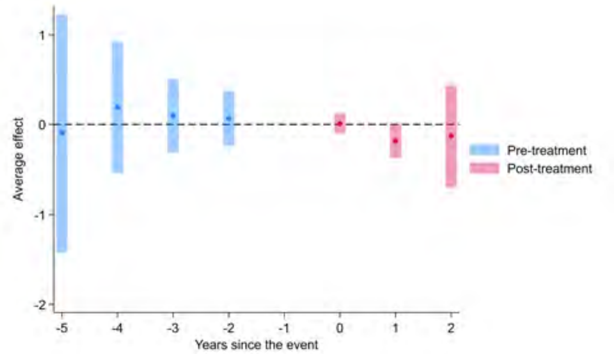

(b) Acquirer Effect (Urban)



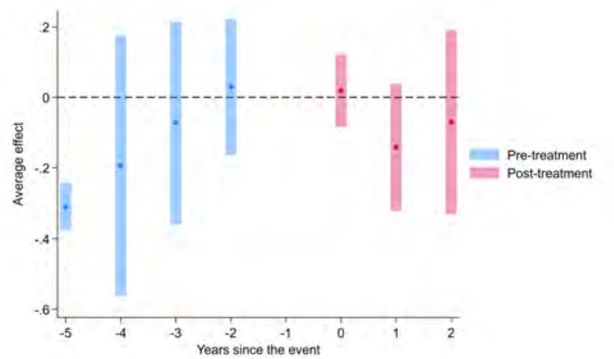

(c) Combined Effect (Urban)

*Notes:* Same DiD setup as Figure A18 but for *urban* hospitals. Outcome: AHA beds (log). Treatment timing, controls, and clustering as above.

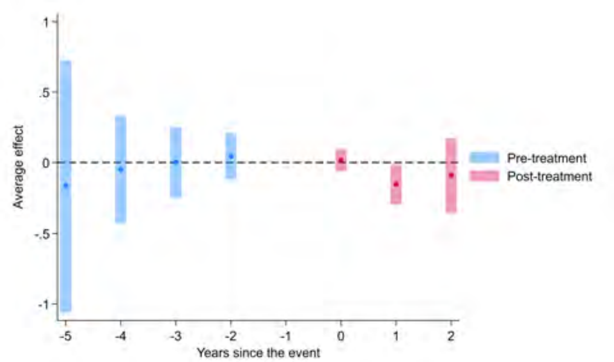
Figure A20: Rural Hospitals: L&D Staffing Capacity $\log($Unique Admitting NPIs$)$ (Target, Acquirer, & Combined Effects)




(a) Target Effect (Rural)



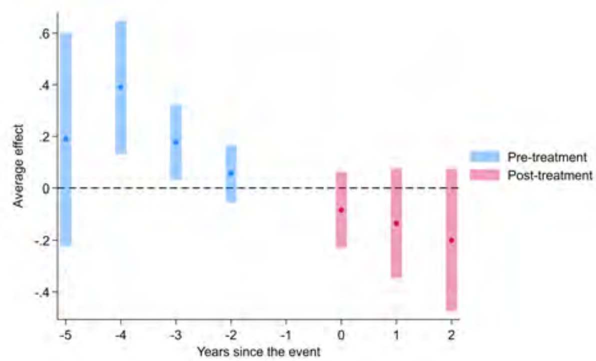

(b) Acquirer Effect (Rural)



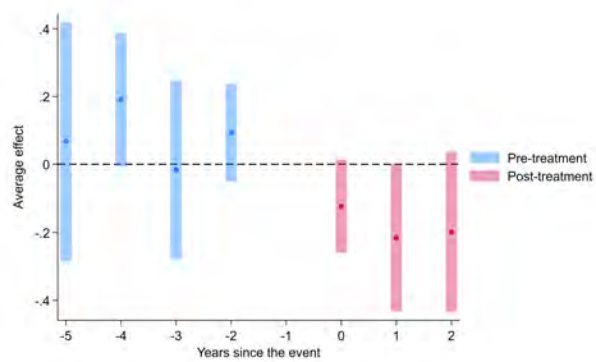

(c) Combined Effect (Rural)

*Notes:* Callaway and Sant'Anna (2021) DiD using hospital-year Medicaid TAF aggregates; outcome is $\log($unique admitting NPIs$)$ (OB/GYN staffing proxy); rural hospitals only. Treatment = first merger exposure (2016–2021). Never-treated and pre-treatment periods serve as controls; hospital-clustered SEs.

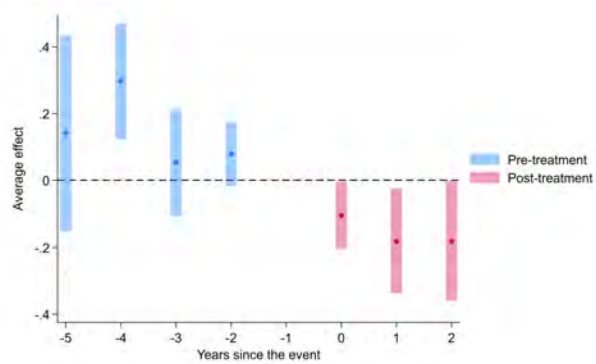
Figure A21: Urban Hospitals: L&D Staffing Capacity $\log(\text{Unique Admitting NPIs})$ (Target, Acquirer, & Combined Effects)




(a) Target Effect (Urban)




(b) Acquirer Effect (Urban)




(c) Combined Effect (Urban)

*Notes:* Same DiD specification as Figure A20 but restricted to *urban* hospitals. Outcome is $\log(\text{unique admitting NPIs})$; treatment timing, controls, and clustering identical.