Mergers With Intermediation: The Price Effect of Distant Hospital Mergers[*]

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Abstract

A substantial and increasingly salient class of M&A activity occurs across seemingly disconnected markets. Such cross-market or cross-geography mergers are of growing concern for competition authorities, yet evidence on them is scarce. We study cross-geography hospital mergers between 2011 and 2020 using private insurance data and show that acquisitions are associated with a 4.6% price increase at target hospitals. Using data on employer-sponsored health insurance enrollment and claims, we show that merger effects are strongly associated with overlapping employers across merging markets. We find no price increases when employer overlap is low and significant increases when overlap is high, a finding that persists even when merging parties serve disjoint healthcare markets. We provide evidence that integrated systems exert control over insurers' ability to win valuable employer contracts and secure higher payment rates. We find that cross-geography mergers reduce staffing, admissions, and costs, and increase markups at acquired hospitals.

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1 Introduction

A substantial and increasingly salient class of M&A activity occurs across seemingly disconnected markets. Buyers and targets in pharmaceutical (Feng et al., 2026), banking (Bird et al., 2024), education (Russell, 2021), and energy markets (Demirer and Karaduman, 2026) routinely serve different consumer bases across different geographies and product markets. Standard competition theory suggests that mergers may increase prices when the consolidated ownership structure controls more of the options relevant to consumers (Farrell and Shapiro, 1990; Whinston, 2007). Under this view, mergers across markets are often presumed harmless, especially when the merging parties have no common consumers. Not surprisingly, these mergers rarely face regulatory scrutiny (Godwin et al., 2023). In this work, we show that one particularly prevalent class of cross-geography mergers—hospital acquisitions—produces sizable price effects. We attribute this to overlapping employers across merging markets, allowing us to predict which cross-market hospital mergers are most likely to lead to large price increases.

American hospital markets have undergone drastic consolidation over the last few decades. Since 2000, there have been over fifteen hundred hospital merger events, and the number of independent hospitals has decreased by nearly a third. Over 57% of hospitals acquired during this period were bought by systems located more than 30 miles away; in contrast, fewer than 7.5% of patients drive more than this distance for hospital care. In the majority of these cross-geography acquisitions, the affected hospitals are sufficiently distant that they share no meaningful common patient demand. Even so, prior research documents that they have led to price increases (Lewis and Pflum, 2017; Dafny et al., 2019; Brand et al., 2023; Arnold et al., 2025a). Accordingly, authorities have grown increasingly concerned about these acquisitions. The latest merger guidelines explicitly state that the agencies will review whether a merged firm can extend its dominant position from one market into a related market (U.S. Department of Justice and Federal Trade Commission, 2023; Shapiro, 2025), and a recent joint DOJ-FTC-HHS portal cites "cross-market mergers" in healthcare as potentially harmful (U.S. Department of Justice, Antitrust Division, 2024). Despite this, to our knowledge, no hospital merger has yet been fully litigated or formally challenged by the FTC on cross-market grounds.

Using data from UnitedHealthcare, a large national health insurer, we show that cross-geography acquisitions increase prices at target hospitals by an average of 4.6% relative to similar, unaffected hospitals in our sample. The effect of cross-geography mergers increases over time: it is 4.1% two years after acquisition, 5.7% three years after, and 10.0% four years after. We show that these price increases are strongly associated with the presence of large employers that overlap the merging geographies and procure insurance on behalf of employees in these markets. We develop a simple metric of employer overlap across merging markets and find that a one-standard-deviation increase in overlap raises post-acquisition prices by about 3.5%. The effect of employer overlap persists over vast distances and is robust to changes in costs, labor, and capacity at acquired hospitals. Moreover, we find that costs at acquired hospitals fall by about 6.5% following acquisition, indicating an overall increase in markups. An analysis of acquisition decisions reveals that acquirers are more likely to target hospitals with high employer overlap, suggesting that anticipated price increases influence target choice.

The role of employer overlap in cross-geography mergers is not coincidental. Employer-sponsored health insurance is the largest source of insurance in the country, covering 53.8% of all Americans. Crucially, many firms employ workers who reside in more than one local healthcare market. In our data, employers covering between 100 and 300,000 beneficiaries have employees in over a dozen local health service areas (HSAs) on average. Hospital systems, in turn, cover an average of seven different HSAs. These geographies overlap considerably: the share of employees who have a coworker in a different healthcare market, such that both share at least one hospital owned by the same system, is 81.5%.

A long-standing hypothesis holds that hospital mergers across geographic regions provide hospitals with additional leverage in price negotiations when firms employ workers in both the acquiring and target hospital markets (Vistnes and Sarafidis, 2013; Lewis and Pflum, 2017; Dafny et al., 2019; Brand and Rosenbaum, 2019). We formalize the employer overlap hypothesis using two distinct models of hospital-insurer price negotiations. In the first, prices are negotiated following the workhorse Nash-in-Nash framework (Gowrisankaran et al., 2015; Ho and Lee, 2017; Collard-Wexler et al., 2019), where Dafny et al. (2019) document a sufficient condition for cross-market mergers to increase prices. We show that this condition emerges naturally when employers overlap across the merging markets and can substitute labor in one market with labor in the other. If employees trade wages for better local hospital access, then, under mild conditions, the insurers will perceive hospitals as substitutes in the value they generate for employer contracts. Because they are substitutes, hospitals across markets compete on price, and their merger softens this competition. In contrast, our second model does not require employers to substitute labor between markets and, more broadly, fails to satisfy the property uncovered in Dafny et al. (2019). Instead, it supposes that the target hospital is within a narrow local network and is either subject to a threat of replacement by a local rival or is used as the replacement threat for a rival hospital, as in Ho and Lee (2019). We show that if an employer overlaps markets, requires coverage in both, and the acquiring hospital is sufficiently dominant in its local market, then the acquisition will eliminate the replacement threat and increase prices. We use these models—both of which require employer overlap between markets—as our guiding theories for the empirical analysis.

We study merger effects by comparing changes in the prices experienced by millions of insured individuals at acquired hospitals with those at non-acquired hospitals from 2011 to 2020. We focus on mergers in which at least one member hospital of the acquiring system is in the same state as the target hospital and in which the hospitals involved are part of no other local merger within a five-year window centered on the merger event.[1] The requirement of an untreated event window is fundamental to isolating the merger effect from other local forces and highlights a pattern of serial acquisitions by cross-geography acquirers. In particular,

[1]The focus on within-state mergers is motivated by the organization of contract negotiation in the industry and follows prior work on hospital mergers. In particular, Dafny et al. (2019) find cross-market merger effects on acquirers only for within-state mergers. Our results, however, extend to cross-state mergers.

systems that engage in cross-geography acquisitions pursue local and distant acquisitions at a frequency that makes it nearly impossible to disentangle which of their mergers produced which effect. As a consequence, we focus on targets, which complements prior evidence on acquirers (Dafny et al., 2019; Arnold et al., 2025a). Based on these filters, we match, for each acquired hospital, a set of control hospitals that are in the same state but in a different local market, that are never part of the acquiring or target hospital system, and that experience no mergers within a seven-year window centered on the event. We use these to form balanced, stacked event panels and to study merger effects using a difference-in-differences design.

We find significant merger effects on prices from both local and distant hospital acquisitions. Local acquisitions increase prices by about 6% relative to controls, while cross-geography acquisitions increase prices by 4.6%. In both cases, price increases rise over time. To assess the effect of employer overlap, we use employer-sponsored plan data from our insurance data provider and identify firms whose employees are present in both the acquirer's and target's local markets. For a given employer, we measure its exposure to a merger as the minimum of its employee counts across the acquirer and target markets. We define the *employer overlap share* of a merger as the sum of all employers' merger exposure over the total employee population covered by an insurer. This metric is naturally zero for mergers with no employer overlap and reaches a maximum of 50%, attainable only for insurers that cover employers equally divided between the merging markets. Intuitively, at its maximum, the integrated system has monopolistic control over the network relevant to the insurer's demand. The metric uses a common scale across mergers for a single insurer and can thus be compared across mergers and over time.

The average cross-geography acquisition in our sample has an employer overlap equal to 0.2% of our data provider's entire covered employer-sponsored population, or approximately 31,000 covered lives. Mergers vary considerably in overlap, with mean overlap in the bottom and top quartiles being 0.002% and 0.83%, respectively. A more intuitive quantification of overlap counts beneficiaries covered by firms that significantly overlap both markets. This measure captures the potential covered lives the insurer stands to lose if exclusion of the merged system caused those employers to switch insurers. Under this measurement, the mean overlap in the bottom quartile covers 321 lives, while that in the top quartile covers 293,589 lives.

We find that almost all cross-geography merger effects occur in above-median overlap mergers. In the first quartile of overlap, mergers exhibit small, negative, and statistically insignificant effects at any distance. In contrast, in the fourth quartile of employer overlap, target hospitals experience price increases of 15.2% if their nearest acquirer is within 18 miles (the 25th percentile of merger distance) and 10.9% if the nearest acquirer is located at least 66 miles away (the 75th percentile of merger distance), with positive effects throughout the range of distances. For comparison, fewer than 1.41% of patients ever travel more than 66 miles for care, and the acquiring hospitals account for none of the hospital-to-hospital referrals at target hospitals acquired at that distance. In addition, our results show that employer overlap has an increasing but non-linear association with merger effects and that a linear employer-overlap effect explains about half of the average merger effect in the sample. While we document significant heterogeneity in merger effects across hospitals, we find that the connection between employer overlap and price increases is persistent, slightly smaller for larger target hospitals and broader acquiring systems, but significant wherever we can measure meaningful post-acquisition price changes.

Cross-geography acquisitions significantly affect hospitals' healthcare production. We show that following the acquisition, target hospital bed capacity falls by 3% relative to controls, with a nearly 15% decrease in key specialty beds. Targets are less likely to own CT scanners or MRI machines, operate emergency rooms, or offer oncology, mammography, or hemodialysis services. These changes are paired with large reductions in labor, with nurses and doctors per bed falling by 12% and 21%, respectively. These changes, however, appear to have limited effects on local wages for healthcare workers, suggesting that target hospitals are small relative to their local labor markets. Moreover, these reductions in labor, capacity, and capital appear to decrease quality only mildly. However, we find that hospitals' cost per admission decreases by about 6.5%, consistent with prior evidence by Schmitt (2017). Together with higher prices, this implies a substantial change in hospital markups.

Using data on admissions and enrollment, we find significant responses by employers and patients to the changes hospitals experience post-acquisition. We document that employers' contract procurement decisions are highly responsive to the prices their employees experience and to the extent that their local hospitals are within the insurance network. Following acquisition, admissions from patients covered by our data provider fall by about 20%. Employer enrollment also declines modestly, and aggregate admission patterns suggest that many patients retain access after switching insurers.

We use our estimates to distinguish among the alternative theories that could generate cross-geography merger price effects. We provide evidence against simple theories, such as acquiring systems forcing higher, uniform prices on targets, or the presence of lucrative patients willing to travel long distances for care who can be steered between acquirer and target hospitals. Given this evidence and the fact that merger effects extend over vast distances when employer overlap is present, our results are unlikely to be explained by common demand among the merging parties. The evidence is also inconsistent with merger effects being driven by the application of common network adequacy standards or with bargaining power being unilaterally extended from acquirers to targets. Additionally, we present evidence against our second model of cross-geography price effects. In particular, we show that—consistent with this theory—acquirers that are more dominant in their local markets obtain larger price increases in cross-geography acquisitions than less dominant acquirers. However, these effects occur even when the target is not in a narrow-network arrangement, which is inconsistent with this theory.

Throughout the analysis, the connection between employer overlap and merger effects persists, suggesting that any underlying theory must assign it a role. Thus, we see our evidence as indirectly supporting the theory of employer intermediation and labor substitution. Nevertheless, our data do not allow us to directly test labor substitution effects or their connection to healthcare preferences, and, as far as we know, data containing the necessary components for this test do not exist. However, the evidence suggests that overlapping employers could successfully be used to determine market boundaries and the potential for harm when examining distant mergers. Thus, these distant mergers are not happening across markets—they link disconnected health service geographies via a unified insurance market.

Our findings are robust to a variety of changes to our empirical strategy. Crucially, we show that employer overlap remains a key determinant of cross-geography price effects when the local geography is modified to be a 15-mile radius around a hospital, the hospital's county, a Hospital Referral Region, and even an entire state. In all cases, we find that hospital prices increase post-acquisition when employers overlap the merging geographies. In addition, we show that our findings are robust to measuring employer exposure using network adequacy standards from Medicare Advantage markets, adjusting prices to account for changes in the units of care delivered, and studying outpatient prices. We note that although our evidence comes from a single insurer and our theory and some of our evidence suggest that other insurers might experience different price effects, the insured population we study is large enough (nearly 50 million unique individuals over 2011–2020) to be directly relevant.

Our work contributes to a growing literature on cross-market and cross-geography mergers (Vistnes and Sarafidis, 2013; Schmitt, 2017; King and Fuse Brown, 2017; Policarpo Garcia and Furquim de Azevedo, 2019; Russell, 2021; Fulton et al., 2022; King et al., 2023; Navathe and Connolly, 2023; Bird et al., 2024; Feng et al., 2026; Shapiro, 2025; Arnold et al., 2025a; Demirer and Karaduman, 2026). We build on the employer-overlap and holes-in-network theory of Vistnes and Sarafidis (2013), which posits that a cross-market acquisition could increase prices by leveraging employers' demand for coverage across employee geographies. Early evidence on price effects from Lewis and Pflum (2017) and Dafny et al. (2019) supported price increases following cross-market acquisitions but reached opposing conclusions regarding the role of employers. Crucially, prior literature relied on commuting patterns and insurer presence to proxy for geographic overlap. To our knowledge, we are the first to use employer data to directly measure overlap, offering an accurate accounting of these geographical patterns and providing evidence that overlap might matter even when regions are too far apart to share meaningful commuting patterns. Further, our theory shows that employer overlap can generate the conditions for price effects if labor is substitutable across markets, even if workers themselves do not move between locations. Our evidence on the role of employer overlap in post-acquisition price increases holds across vast distances, even for mergers that occur across state lines. This reconciles prior mixed evidence on the connection between distance and merger effects (Lewis and Pflum, 2017; Cooper et al., 2019; Dafny et al., 2019; Arnold and Whaley, 2020; Brand et al., 2023; Arnold et al., 2025a), as we find that at long distances, merger effects attenuate but remain persistent and strong whenever employer overlap is sufficient.

In addition to providing a measure of employer overlap, our evidence complements prior work by combining private insurance price data with merger data meticulously assembled and validated by Brot et al. (2024). As noted by Brand and Rosenbaum (2019) and Vistnes (2024), data quality has been a key issue in studying distant hospital mergers. In particular, access to national-level private insurance prices is limited, and the foundational articles on cross-market mergers inferred private prices from revenues reported in the Healthcare Cost Report Information System (HCRIS) database. Prior research has shown that these proxy prices are correlated with true private prices at 0.45 (Cooper et al., 2019), and our sample yields a similar correlation of 0.42. However, most of this correlation is cross-sectional, with the time-series correlation at only 0.12. In practice, this means that HCRIS-based prices are extremely limited in their ability to capture how any given hospital's prices vary over time, which is precisely the object of interest in retrospective merger analysis. During our analysis period, which is broader and more recent than in prior HCRIS-based work, we find no within-state cross-geography merger effects using HCRIS-based prices, despite clear increases in the true price data. We caution, however, that this does not mean that prior evidence was incorrect. In practice, the wedge between HCRIS-based and private prices has likely increased over time due to the growing complexity of hospital revenue structures. It does, however, highlight the value of revisiting these prior findings, particularly given antitrust authorities' growing interest in cross-market and cross-geography mergers (Federal Trade Commission, 2016; U.S. Department of Justice, Antitrust Division and Federal Trade Commission, 2020). This underscores the importance of recent evidence using private insurance prices, particularly Brand et al. (2023) and Arnold et al. (2025a), who draw on data from insurers other than our data provider to study price changes at local and distant mergers. Our work is highly complementary: this article provides evidence from a different population and, most importantly, measures employer overlap and quantifies the importance of the intermediation channel.

Broadly, this work contributes to the literature on hospital consolidation and provider market power. Retrospective and structural analyses have repeatedly found that hospital mergers—especially local mergers among close substitutes—raise negotiated commercial prices (Town and Vistnes, 2001; Capps et al., 2003; Gaynor and Vogt, 2003; Dafny, 2009; Haas-Wilson and Garmon, 2011; Tenn, 2011; Thompson, 2011; Gowrisankaran et al., 2015; Cooper et al., 2019; Brand et al., 2023; Brot et al., 2024) and result in declines or negligible changes in quality (Ho and Hamilton, 2000; Mutter et al., 2011; Beaulieu et al., 2020; Jiang et al., 2021). Related to our findings on cost changes, prior evidence is mixed and case-specific, with some articles finding meaningful reductions at acquired or newly corporatized hospitals (Alexander et al., 1996; Connor et al., 1998; Dranove and Lindrooth, 2003; Schmitt, 2017; Craig et al., 2021; Andreyeva et al., 2024). Our work is also related to a broader agenda on provider consolidation that examines its harms to consumers and workers through patient selection, labor-market monopsony, vertical foreclosure, and downstream wage or employment effects (Capps et al., 2018; Prager and Schmitt, 2021; Duggan et al., 2023; Brot-Goldberg et al., 2024; Cuesta et al., 2024; Setzler, 2025; Cooper et al., 2025). With few exceptions, research has focused on local consolidation, where substitution-based theories of harm are most direct. Our findings show that similar patterns can arise for cross-geography mergers that consolidate employer rather than health-service markets.

Our study of employer overlap connects to the literature on employer-sponsored benefits and their consequences. Extensive research has shown that health benefits respond to employee demand and workforce composition (Dranove et al., 2000; Bundorf, 2002; Gruber and Lettau, 2004; Dey and Flinn, 2005) and create mobility frictions and job lock (Madrian, 1994; Gruber and Madrian, 1994; Fang and Gavazza, 2011). Our work highlights another important consequence of this market organization. By grouping patient demand, employers may be able to procure insurance at lower premiums, but this aggregation of demand increases the stakes for insurers in having broad geographic networks, giving leverage to merging hospitals. These forces can create merger incentives for hospitals. The consequences of intermediation are likely not unique to healthcare markets. Our work thus connects to growing evidence on the economics of intermediation (Gavazza and Lizzeri, 2021; Salz, 2022; Li et al., 2026; Allende et al., 2026), highlighting nontrivial competitive externalities associated with it. Finally, this article presents evidence of direct effects of seemingly multimarket mergers, linking to a large literature on multimarket contact and offering an alternative theory of price effects via increased leverage over intermediaries (Bernheim and Whinston, 1990; Parker and Röller, 1997; Ciliberto and Williams, 2014; Schmitt, 2018).

The rest of this article is organized as follows. Section 2 discusses alternative theories of cross-geography mergers and price effects. Our empirical analysis begins in Section 3, which describes our data and setting. Our main evidence on price effects is found in Section 4. Section 5 shows how mergers impact healthcare production, while Section 6 shows the impact on insurance and hospital demand. Section 7 discusses the extent to which employer overlap predicts mergers. Section 8 revisits the merger theory and presents evidence against certain hypotheses. Finally, we provide robustness analyses in Section 9 and conclude in Section 10.

2 Theoretical Framework

We begin by presenting the theoretical framework underlying our empirical analysis.[2] We introduce the current standard model for hospital price negotiations, establish basic conditions under which a merger can increase prices within the framework, explain why cross-market merger effects might be difficult to rationalize, and show how cross-geography merger effects can arise both within and outside the framework.[3] The development of this section is largely self-contained, such that readers interested exclusively in the empirical evidence can skip it and proceed to the next section at minimal loss of coherence.

[2]This section builds extensively on Dafny et al. (2019), who establish the submodularity condition and show that mergers across markets do not raise prices when insurer payoffs are additive across markets. We formalize their observation that submodularity typically arises when hospitals are substitutes and provide a microfoundation based on employer demand. We then present examples consistent with their intuition that nevertheless fail to generate submodularity or cross-geography price effects, as well as an example that generates merger price effects without satisfying submodularity. Dafny et al. (2019) also discuss alternative explanations for cross-market merger effects that are outside our scope, so the two treatments are complementary.

[3]In the remainder of the article, we refrain from using the term "cross-market" unless necessary to link to the previous literature. While the mergers we study might occur across healthcare service markets, the merging parties overlap in insurance markets. Thus, labeling them as cross-market suggests an erroneous lack of connection between the merging parties.

Hospitals and insurers engage in bilateral negotiations to establish transaction prices for care. In current practice, these negotiations are modeled using the Nash-in-Nash framework (Crawford and Yurukoglu, 2012; Gowrisankaran et al., 2015; Ho and Lee, 2017; Collard-Wexler et al., 2019). In this framework, hospitals negotiate simultaneously with insurers, each negotiation following a Nash-bargaining protocol with passive beliefs about rival outcomes, as in Horn and Wolinsky (1988). Negotiations are conducted conditional on others' equilibrium actions, thereby reaching an overall Nash equilibrium among all active negotiations. In the framework, one must specify the threat point for each negotiating party, indicating their payoff if the negotiation were unsuccessful. In the hospital setting, evidence points to all-or-nothing threats on both sides: upon disagreement, the hospital system withholds all its member hospitals from the insurer's network, and the insurer, in turn, drops the system from all its plans.

Using this framework, we can establish the basic conditions for merger effects, following Dafny et al. (2019). Hospitals in the economy are organized into a collection of systems, $\mathcal{S}$, such that each system $s \in \mathcal{S}$ is a set of hospitals that is disjoint from all other systems. Throughout, we focus on the effects on a single insurer, holding rival insurance prices and networks constant, for simplicity. We denote by $\mathcal{N}$ the equilibrium network of the focal insurer, and by $V(\mathcal{N})$ the profit the insurer derives from selling insurance plans to employers and individuals, given its assembled network of providers, gross of any payment to hospitals. The framework establishes that the overall negotiated transfer between the insurer and any hospital system s solves

$$\max_{t} \ (V(\mathcal{N}) - t - V(\mathcal{N} \setminus s))^{1-\lambda_s} (t - c_s - \Delta\pi_s)^{\lambda_s} \tag{1}$$

where $V(\mathcal{N} \setminus s)$ is the insurer's threat point: the profit of the insurer if it excludes the hospital system from its network. The cost of serving the insurer's enrollees is c_s, while the hospital's threat point is given by $\Delta\pi_s$, which corresponds to the change in profit from other insurance sources when the system and the insurer disagree. This final term includes the hospital's ability to recapture profits upon disagreement and is often considered small and independent of the agreed-upon transfer (Ho and Lee, 2017). Finally, the exponent, $\lambda_s \in (0, 1)$, denotes the hospital system's relative bargaining skill. We note that the form above is particularly simple, as it leans heavily on the evidence that hospital demand in the US is largely inelastic (Prager, 2020).[4] The solution to the bargaining problem equals

$$t_s = (1 - \lambda_s)(c_s + \Delta\pi_s) + \lambda_s(V(\mathcal{N}) - V(\mathcal{N} \setminus s)), \tag{2}$$

implying that system s's profit is a share λ_s of the gains from trade: $V(\mathcal{N}) - V(\mathcal{N} \setminus s) - c_s - \Delta\pi_s$.

The transfer above holds for any hospital system and thus, in particular, for any merger. Therefore, for any two systems $s, r \in \mathcal{S}$, we say that their merger into a unified system $s \cup r$ yields a (positive) price effect if $t_{s\cup r} > t_s + t_r$. Despite the simplicity of the model above,

[4] Assuming risk-neutral and symmetrically informed firms, this implies that whether hospitals and insurers negotiate prices for a list of procedures or for a single transfer is, ex ante, equivalent for both hospitals and insurers. Direct transfers are easier to work with in theory, whereas prices are typically better measured empirically, which explains the distinct approaches in this section and the rest of the article.

without further restrictions on how mergers affect model primitives, anything can happen. For example, if s and r have very low bargaining weights before merging, and their merger unlocks much higher bargaining skill, then transfers will tend to increase. Alternatively, if bargaining skills are equal and unchanged, but the merger makes both hospitals significantly less efficient, then prices will tend to increase as well. The standard analysis therefore holds bargaining weights, costs, and recapture fixed and asks how the merger changes the system's value to the insurer. Following Dafny et al. (2019), we take $\lambda \equiv \lambda_s = \lambda_r = \lambda_{s\cup r}$ and assume that costs and recapture are additive across merging systems. Assuming negligible recapture, and denoting $\Delta_{s'} V \equiv V(\mathcal{N}) - V(\mathcal{N} \setminus s')$ for a generic system s', the merger will yield price increases if and only if

$$\begin{aligned} t_{s\cup r} > t_s + t_r &\iff (1-\lambda)(c_s + c_r) + \lambda \Delta_{s\cup r} V > (1-\lambda)c_s + \lambda \Delta_s V + (1-\lambda)c_r + \lambda \Delta_r V \\ &\iff \Delta_{s\cup r} V > \Delta_s V + \Delta_r V \end{aligned} \tag{3}$$

which is precisely the (local) submodularity condition of Dafny et al. (2019). This condition requires that the loss in insurer profit from removing the merged system $s \cup r$ be strictly greater than the sum of the losses from removing each system independently. Equivalently, this is simply a statement about decreasing returns to adding hospitals to the network, which means that submodularity is closely related to the concavity of the insurer's objective.

The submodularity condition arises naturally when the merging systems are substitutes for consumers because consumer surplus is typically submodular. To see this, denote by $v_i(\mathcal{H}) = \mathbb{E}[\max_{h\in\mathcal{H}} u_{ih}]$ the expected value of a network $\mathcal{H}$ to a consumer i, where u_{ih} is the (potentially random) utility of choice h. Let $\underline{\mathcal{H}} \subseteq \mathcal{H}$ be a subset of the network and $h^* \notin \mathcal{H}$ a new network addition, and define the random variable $u^*_{\mathcal{H}} = \max_{h\in\mathcal{H}} u_{ih}$ capturing the maximum choice utility in network $\mathcal{H}$ and similarly $u^*_{\underline{\mathcal{H}}}$. Then,

$$v_i(\underline{\mathcal{H}} \cup \{h^*\}) - v_i(\underline{\mathcal{H}}) = \mathbb{E}[\max\{u_{ih^*} - u^*_{\underline{\mathcal{H}}}, 0\}] \geq \mathbb{E}[\max\{u_{ih^*} - u^*_{\mathcal{H}}, 0\}] = v_i(\mathcal{H} \cup \{h^*\}) - v_i(\mathcal{H}) \tag{4}$$

where the inequality follows from every choice in $\underline{\mathcal{H}}$ being contained in $\mathcal{H}$. The inequality becomes strict whenever the added hospital is chosen with positive probability from the smaller network while some hospital in $\mathcal{H} \setminus \underline{\mathcal{H}}$ would have been chosen otherwise.[5] The condition in Equation (4) is equivalent to the submodularity used above, as it states that consumer surplus increases more when a hospital is added to a smaller network than when it is added to a strictly larger one.[6] This is intuitive, since each additional equivalent substitute increases surplus by less than the previous one. Hence, any pricing protocol that would reward the insurer with a weakly concave, non-decreasing function of the consumer surplus it offers to the market

[5] This would happen, for example, if the random utilities include a fully supported iid shock. In the logit-demand framework, the surplus of individual i for a candidate network $\mathcal{H}$ is proportional to $\ln(\sum_{h\in\mathcal{H}} e^{\delta_{ih}})$, where δ_{ih} is the expected indirect utility of visiting hospital h. Every hospital introduced into the local network adds an exponential term to the concave log function. Submodularity follows directly from this.

[6] To see why the two submodularity expressions are equivalent, redefine $\mathcal{H} = \mathcal{N} \setminus s$, $\underline{\mathcal{H}} = \mathcal{N} \setminus (s \cup r)$ and replace $\{h^*\}$ in the argument with s. Then the statement is $v_i(\mathcal{N} \setminus r) - v_i(\mathcal{N} \setminus (s \cup r)) \geq v_i(\mathcal{N}) - v_i(\mathcal{N} \setminus s)$. Subtracting $v_i(\mathcal{N} \setminus r) - v_i(\mathcal{N})$ from both sides of the inequality yields the desired expression.

would preserve submodularity, thereby generating positive merger price effects. For example, suppose that r and s are in a single market m, where a single employer procures insurance to maximize $v_m(\mathcal{N}) - p$, where p denotes the insurance premium and $v_m(\mathcal{N})$ the total surplus of its employees. If the insurer and employer negotiate premiums à la Nash, with an insurer bargaining weight $\tau \in (0, 1)$, then the insurer's profit would be exactly $V(\mathcal{N}) = \tau(v_m(\mathcal{N}) - v_m(\emptyset))$, which is submodular, yielding an increased transfer following the merger of two local systems that offer substitute services.

While it is simple to rationalize the strict submodularity of payoffs for local mergers, this property rapidly breaks down in mergers that cross market boundaries. For example, suppose that s is located in a market m and r in a market n. Consumers in each market value only access to local hospitals. If insurance markets are independent (e.g., the sets of employers in the two markets are disjoint), then the insurer's payoff will be additive across markets, violating strict submodularity. Regardless of the size or importance of either merging party, their joint transfer will equal the sum of their pre-merger transfers (assuming no changes in costs or bargaining skills). Intuitively, the merged system does not hold more leverage over the insurer's profits than the sum of its parts, and thus cannot obtain a higher aggregate payment.

The lack of submodularity is not easily resolved by introducing a common insurance market. For example, if the same Nash-bargaining employer now overlaps both markets m and n and negotiates a premium to maximize the total surplus of its employees, the insurer's profit would simply be $V(\mathcal{N}) = \tau\,(v_m(\mathcal{N} \cap \mathcal{H}_m) - v_m(\emptyset) + v_n(\mathcal{N} \cap \mathcal{H}_n) - v_n(\emptyset))$, where v_n is the surplus of employees in market n and $\mathcal{H}_m, \mathcal{H}_n$ are the sets of hospitals in each market. This objective is additive in network additions across markets, yielding no price effects from cross-geography mergers. In Appendix A.2.1, we show that even if one considers more active competition for employer contracts, it is challenging to unambiguously obtain submodular insurance payoffs. For example, we show that submodularity is not recovered if the employer selects the insurer with the highest surplus and negotiates with the threat of replacing it with the next-best offer. Neither is it unambiguously resolved when insurers compete in an exchange-like market, nor when employers run an optimal procurement mechanism.[7] In fact, for many scenarios, these procurement models yield exactly the opposite effect, predicting that hospitals across markets are complements from the perspective of the insurer, since each network addition increases both the probability that the employer buys the insurer's contract and the insurer's profit conditional on winning the procurement.[8] Intuitively, when hospitals are complementary, they each obtain a rent from unlocking the complementarity value of their network partners. However, when hospitals merge, this externality is internalized, and the associated rents disappear.

Despite these challenges, the cross-geography merger puzzle can be resolved via a richer model of the role of insurance benefits in labor markets, in the style of Lamadon et al. (2022). Suppose again that a common employer operates in markets m and n. Each worker i in market m has an

[7] In some scenarios, submodularity emerges under specific assumptions about the distribution of rival values and costs and specific procurement rules.

[8] Ide (2026) develops an alternative theory based on conglomerate effects, which can produce submodularities.

indirect utility of working at the cross-market firm given by $\gamma_m \ln(w_m) + \nu_m x_m + \xi_m + \epsilon_{im}$, where w_m is the wage offered by the firm in the market, x_m is an indicator of whether the employer offers insurance that includes access to system s, and ξ_m is the additional amenity value of working for the cross-market employer. The idiosyncratic preference shock, ϵ_{im}, is assumed to follow a type-I extreme-value distribution. Assuming that demand is equivalent in market n and that workers prefer higher wages and better healthcare access, the employer faces two upward-sloping labor supply curves. Local network access acts as a wage subsidy, changing the slope in each market. The cross-market employer produces output using a Cobb-Douglas production technology of the form $\Omega K^{\alpha}(L_m + L_n)^{\beta}$, where Ω is total factor productivity, K is capital, and L_m, L_n are the labor hired in each market. In Appendix A.2.2, we show that holding rival employers' wage offers fixed, if the employer has decreasing returns to scale (i.e., $\alpha+\beta < 1$), then the employer's profit is strictly submodular in cross-geography network additions. Thus, any pricing protocol rewarding the insurer with an increasing and weakly concave function of these profits—like the bargaining outcome—yields strictly submodular payoffs and thus cross-geography merger effects.

The intuition behind this result is simple. The employer's marginal wage bill in each market decreases as it expands access to local providers. Because labor from the two markets is substitutable, better hospital access in one market reduces the marginal value of access in the other. Before the merger, this substitution limits each hospital's contribution to the employer contract; common ownership internalizes that competition. The merger forces the insurer and employer to take the combined network as a whole or not at all.

If one is willing to deviate from the basic framework, cross-geography merger effects can be obtained under mechanisms that do not require submodularity. For example, in Appendix A.2.3, we show an example of tying, in which a dominant hospital in one market can extend its power to acquired hospitals in other markets. The key distinction is that in this model, the acquired hospital faces a threat of replacement in the sense of Ho and Lee (2019).[9] The acquirer serves as a must-have provider, eliminating the target's replacement threat and enabling the merged system to secure a larger transfer. This is perhaps most intuitive when the target is excluded prior to the merger; in that case, being linked to a must-have in an all-or-nothing negotiation forces the insurer to include the target in the network. More broadly, the replacement mechanism does not require submodularity, as it operates by changing the insurer's threat point. However, it still requires an agent akin to a common employer who must procure coverage in both markets. Moreover, under this framework, if the acquirer is easily replaceable, a cross-market acquisition should be transfer-neutral, providing a testable prediction.

Overall, the theory behind cross-geography merger effects is varied. When networks and threat points are fixed, the requirement for merging hospitals to act as substitutes within the insurer's objective is—except in a few special cases—a necessity. This substitution channel is often difficult to argue for and has proven challenging to show empirically. We note that all

[9]Concurrent work by Kleiner et al. (2026) uses a similar replacement threat framework to study local merger effects.

of the employer-centered models discussed above are variants of a *holes-in-network* argument as suggested by Vistnes and Sarafidis (2013), only some of which make merging hospitals substitutes within the insurer's objective. Our model of labor demand offers this substitution, and the empirical development that follows shows that common employers span the relevant merger geographies and that this overlap is linked to cross-geography merger effects. After presenting this evidence, we revisit alternative merger theories in Section 8 and show that the common-employer labor demand hypothesis appears most plausible.

3 Setting and Data

The last few decades have seen a major transformation of the American healthcare system. There were over fifteen hundred hospital merger events between 2000 and 2020, and a similar number of openings and closures.[10] No state has been spared from hospital mergers, and almost all Americans (91.6%) have experienced at least one hospital merger in their county during this time.[11] The number of stand-alone general acute care hospitals decreased from 2,304 to 1,628, and the number of multi-hospital systems decreased from 522 to 490. Perhaps unsurprisingly, this period saw average hospital prices increase by approximately 76.1% and total spending on healthcare per capita increase by 208%, or by 21.5% and 112.5% after adjusting for inflation, respectively.[12] National health expenditure on hospital care grew from 4.05% to 5.89% of GDP. This transformation has spurred extensive research on the effects of hospital consolidation, which, with few exceptions, has focused on mergers between hospitals competing for the same patient base. However, 57.6% of all hospital acquisitions are by hospitals and systems located over 30 miles away from their targets; in contrast, less than 7.3% of patients drive further than 30 miles for inpatient hospital care.[13]

This class of cross-geography mergers has been a growing source of concern for regulatory authorities. In a 2016 comment on a proposed cooperative agreement between two large health systems, the FTC noted the potential for larger systems to command higher prices by virtue of their size, even beyond their effects on local markets (Federal Trade Commission, 2016). A joint DOJ-FTC submission to the OECD in 2020 similarly noted explicitly that hospitals may still be substitutes for insurers constructing provider networks, especially when employers are present in both areas—precisely the mechanism explored in this article (U.S. Department of Justice, Antitrust Division and Federal Trade Commission, 2020). The same concerns are reflected directly in the joint DOJ-FTC-HHS HealthyCompetition.gov portal, which explicitly cites "cross-market mergers" in healthcare as potentially harmful (U.S. Department of Justice,

[10]When not stated otherwise, figures on merger activity come from our data, which will be described in the following subsection. Our data register 2,779 hospital acquisitions, 1,519 closures, and 1,634 openings between 2000 and 2020. These data are, however, incomplete and might exclude some relevant M&A activity. For example, they include only the contiguous United States.

[11]Merger exposure is computed as the 2019-population-weighted share of counties with at least one hospital merger during 2000–2020.

[12]The change in healthcare spending was computed using the National Health Expenditure Accounts published by CMS (Centers for Medicare & Medicaid Services, 2026b). The change in hospital prices was computed using the U.S. Bureau of Labor Statistics' price index for the hospital industry (U.S. Bureau of Labor Statistics, 2026c).

[13]Conditional on admissions within 100 miles from home zip code. The unconditional share is 7.5%.

Antitrust Division, 2024).

Our analysis focuses on the role of employer geographies in explaining the effect of these cross-geography mergers. Employer-sponsored coverage accounts for 53.8% of all health insurance coverage in the US (U.S. Census Bureau, 2025a). This outsized role is due to a tax advantage dating back at least to 1943 (Congressional Budget Office, 1994) and the well-documented impact of insurance benefits on the ability to attract and retain workers (Madrian, 1994; Dranove et al., 2000; Bundorf, 2002; Gruber and Lettau, 2004).[14] Due to their size and the geography of American cities, employers often purchase insurance on behalf of groups of individuals who collectively reside in multiple distinct local healthcare markets.[15]

We center our analysis on hospital mergers that occurred between 2011 and 2020 in the continental USA. Our primary interest is in identifying changes in inpatient prices because inpatient care constitutes the bulk of hospital spending. While we report some evidence for within-geography mergers, we seek to provide new evidence on cross-geography acquisitions.

3.1 Data

Our work relies on four data sets: individual-level hospital claims data, individual-level enrollment files, a panel of hospital mergers, and a panel of hospital characteristics. We obtained the first two data sets from a large national health insurance company.[16] The claims data cover consumers in employer-sponsored commercial plans across all 50 states and DC from 2011 through 2020. We observe over six hundred billion dollars in hospital payments from nearly two hundred million claims covering nearly fifty million individuals.[17] The claims data include detailed diagnostic and procedure codes, admission and discharge timestamps, and a breakdown of charges and payments across services. For tracking mergers, we use the panel constructed by Brot et al. (2024).

Crucially, our data include the total payment to providers for each inpatient hospitalization. This allows us to measure the effects of mergers on prices experienced by commercially insured patients and construct a standard index price for each hospital in our sample, as in Gowrisankaran et al. (2015) and Cooper et al. (2019). Following the literature, our index price accounts for factors that may affect the case complexity of admitted patients, including their age and gender, and is indexed to the average price for a vaginal delivery for a woman aged 31 to 35.[18] In contrast, most previous evidence on cross-market merger effects has relied on

[14] In addition, the 2010 Affordable Care Act's employer shared-responsibility provisions require most employers with an average of at least 50 full-time-equivalent employees to provide minimum essential insurance or face financial penalties (United States Congress, 2010; U.S. Department of the Treasury and Internal Revenue Service, 2014). This regulation does not impose provider-network breadth or adequacy requirements.

[15] The average one-way commute time in the US is 27.2 minutes (U.S. Census Bureau, 2025b), with many workers driving dozens of miles to work every day. This pattern is often attributed to low-density suburbanization and the decentralization of both households and employment resulting from post-1945 highway construction, federally subsidized single-family housing financing, and local land-use regulation (White, 1994; Baum-Snow, 2007).

[16] Numbers specific to our data provider's population or prices are rounded.

[17] Throughout the article, payments and prices are constructed from *allowed amounts*: the combined obligation of the insurer and patient to the hospital, as recorded in the claims data. We do not observe whether patients paid their portion of the bill; hence, our measure of hospital payments is an upper bound.

[18] See Appendix A.4.4 for further details on the price index construction.

HCRIS-based index prices (Lewis and Pflum, 2017; Dafny et al., 2019), which are constructed as the average revenue from all non-Medicare patients, including Medicaid patients, self-pay patients, and uninsured patients, and which may be affected by issues in collection and delays in billing. In our sample, the overall correlation between HCRIS-based and commercial prices is 0.42, closely matching the 0.45 found by prior research using older data (Cooper et al., 2019). However, the bulk of this correlation stems from cross-sectional comparisons, and the time-series correlation within a hospital is only 0.12. Since the key question in merger retrospectives concerns how a given hospital's prices changed over time—driven by the time-series variation—this underscores the importance of revisiting prior evidence on distant hospital mergers. Consistent with this weak time-series correlation, a merger analysis using HCRIS-based prices fails to reject the null of no price effects for within-state cross-geography mergers, despite the large changes in our private-price measure. Appendix A.3 presents further evidence on this topic.

Our linked enrollment files allow us to track patient plan participation over time. The data include the five-digit ZIP code of each enrollee's residence and a group identifier unique to each firm and plan. We develop an algorithm that leverages enrollees' movements between groups to identify all plans offered by the same firm. We describe this strategy in detail, as well as the construction of all other datasets, in Appendix A.4.

Finally, our panel of hospital characteristics includes information on hospital services, organizational structure, staffing, costs, admissions, accreditation, beds, and location, drawn from the American Hospital Association (AHA) Hospital Survey. We link the resulting panel with provider-level data from our data provider. This includes information on the types of services provided at each hospital, as well as identifiers linking different hospital units that might be reported to the AHA as separate entities. We use supplemental data from Irving Levin to classify hospitals involved in mergers as acquirers or targets. In the majority of cases covered by these data, the larger of the two systems involved in a merger (in terms of member hospital count) serves as the acquirer, a pattern we extend to all mergers in our sample. Thus, for the remainder of the article, the terms "acquirer" and "target" should be interpreted as a classification of the system size involved in a merger, rather than as a classification of who paid whom in the acquisition.[19]

Table 1 shows the characteristics of hospitals involved in mergers in our sample, split between acquirers and targets, and between those involved in within- and cross-geography mergers. A Health Service Area (HSA) is our preferred notion of the local healthcare market, while we present robustness checks using alternative definitions.[20] As shown in the table, most merger activity occurs across healthcare markets and involves larger hospitals (by bed count) acquiring

[19]In 1.7% of records, both merging parties are equally sized. In those cases, we classify both as targets and acquirers. Because most of our analysis focuses on the target, these hospitals are included. Our results are robust to their exclusion.

[20]Using HSAs is convenient from a methodological standpoint as they are defined based on demand patterns and create a clear delineation between markets. In contrast, a radius-based market definition typically implies that all hospitals form part of the same connected network of markets, and thus, in principle, could all be subject to spillover effects from mergers.

Table 1: Descriptive Statistics of the Merger Sample

	Within-HSA		**Cross-HSA**	
	Target	Acquirer	Target	Acquirer
Hospitals involved in merger	227	776	1,489	13,710
Systems involved in merger	96	211	588	329
# Affected Markets	124	386	961	1,734
Average Bed Size	95.943	212.871	124.476	149.240
Average System Members	30.565	36.236	14.990	21.284
Share Stand-Alone	0.463	0.106	0.583	0.031
Share Rural	0.076	0.133	0.278	0.207
Share Critical Access	0.040	0.062	0.137	0.117
Share General Care	0.498	0.762	0.748	0.655
Share For-Profit	0.526	0.384	0.307	0.539
Average Nurses/Bed	0.116	0.116	0.131	0.126
Average Doctors/Bed	0.094	0.097	0.102	0.088
Average Occupancy Rate	0.501	0.589	0.522	0.580
Miles to nearest counterpart				
25th Percentile	1.888	2.111	15.474	14.470
Median	5.065	6.161	28.377	26.441
75th Percentile	10.018	14.109	60.411	57.599

Notes: This table presents descriptive statistics for hospitals grouped by their merger status, for the years 2011–2020. The number of systems and system members, as well as stand-alone and for-profit status, are computed based on the status of the merging hospitals in the year prior to the merger. Distance to the nearest counterpart reflects the minimum distance between the target (acquirer) hospital and its nearest acquirer (target).

smaller ones. The average cross-geography merger involves a larger for-profit system acquiring one or more hospitals from a nonprofit system. Hospitals acquired in cross-geography mergers are 3.7 times as likely to be rural as those acquired in within-geography mergers, and in over 75% of events lie more than 15.5 miles from their nearest acquirer. This implies that while we focus on HSAs as local markets, the vast majority of our mergers would also qualify as cross-geography under other standard market definitions used in the literature, such as a 15-mile radius or a 30-minute drive (Brot et al., 2024).

Our private insurance sample covers the majority of national merger activity.[21] Appendix Table A.1 reports statistics comparable to those in Table 1 for the subset of hospitals for which we have prices. Our effective sample covers 66.5% of all hospitals acquired within geography and 71.9% of cross-geography-acquired hospitals, and overrepresents larger target hospitals (131 beds relative to a national mean of 124). The table shows that within-geography acquirers have, on average, a 12.8% higher price than targets in the year before the merger, while cross-geography acquirers have an 18.9% higher price.[22] The table shows the first evidence of price changes: a year after their merger, hospital prices are 12.5% and 9.1% higher at acquirer and

[21] The coverage is limited since our data provider does not contract with all hospitals in the country.

[22] Throughout, we use the log-point approximation when reporting percentage effects for ease of readability. Conversion to exact percentage effects is immediate.

Table 2: Employer Characteristics and Local Hospital Market Coverage

Statistic	Value
Average employees per employer-year	200.03
Average number of covered local hospital markets, by employer-size quartile	
First Quartile	3.41
Second Quartile	5.74
Third Quartile	9.28
Fourth Quartile	46.06
Average number of local hospital markets per employer-year	16.02
Average number of local hospital markets per multi-hospital system	6.998
Employees with a coworker in a different local hospital market connected by a common hospital system	81.55%

Notes: This table summarizes employer characteristics in the analytic sample constructed from the insurance enrollment data. A local hospital market is defined as a Health Service Area (HSA). An employer is said to cover a local hospital market if it has at least one employee residing in that market. A hospital system covers an area if it has at least one member hospital in the area.

target hospitals involved in within-geography mergers, and 10.4% and 11.2% higher for those in cross-geography mergers. For comparison, the average yearly healthcare inflation during the period was 2.94%.[23]

Table 2 shows the characteristics of employers in our sample. For privacy reasons, our data contain no information about employers, except for encrypted, persistent member and group identifiers. We exclude employers covering fewer than 100 or more than 300,000 enrollees, resulting in a sample of nearly 150,000 employers and over 150 million beneficiary-years.[24] In our analysis below, we do not attempt to distinguish between employees and their dependents and thus refer to the population covered by employer-sponsored contracts broadly as employees. The average employer-year has 200 covered lives residing across 16 HSAs. When employers are grouped into size quartiles, average geographic coverage ranges from 3.41 to 46.06 markets per employer. For comparison, the average multi-hospital system in our sample covers 7 HSAs. These two geographies overlap considerably: 81.55% of employees have a coworker residing in a different HSA who shares at least one hospital system.

4 Merger Effects on Prices

We now present our main empirical analysis of cross-geography merger effects, focusing on their impact on prices. The strategy and tools developed in this section form the basis for the remainder of the article's analysis. We follow a standard difference-in-differences approach,

[23] Annual inflation calculated using the Consumer Price Index for Medical Care from the Bureau of Labor Statistics (U.S. Bureau of Labor Statistics, 2026a).

[24] Small employers are unlikely to overlap merger markets or procure actively on behalf of enrollees and are more likely to switch to the ACA exchanges. Extremely large employers are likely to provide additional health services beyond the scope of our data and may negotiate preferential prices with providers, making it difficult to assess the effects of hospital mergers on them using commercial data.

forming stacked balanced panels that contrast hospitals undergoing mergers with other unaffected hospitals.[25] Given that cross-geography targets are not assigned at random and are likely selected because they are expected to yield the greatest returns, our findings should be interpreted as the average treatment effect on the treated (acquired) hospitals.

In what follows, we say that a hospital is *treated* in period t if it is involved in a merger that year, and is *local to a merger* if it is located in the same local healthcare market as any treated hospital at time t. We classify a merger as cross-geography when the nearest merging counterpart is in a different local market. For our main analysis, we additionally restrict cross-geography mergers to within-state transactions, following Dafny et al. (2019). We require hospitals to be fully observed (with private prices) within a five-year window centered on their merger event and to be subject to no other local merger within that window. For each treated hospital, we identify control hospitals that meet strict requirements. Specifically, control hospitals are never part of the same system as the treated hospital, are not involved in any kind of merger activity within a seven-year window centered on the merger, and are in the same state as the treated hospital but in a different local market. Overall, for our main local market definition of HSA, these requirements reduce the sample from 12,348 treated hospitals (1,221 acquired and 11,127 acquirer hospitals) with observed prices to 4,254 hospitals (807 acquired and 3,447 acquirer hospitals) for which we can study merger effects. Appendix Table A.2 compares the included hospitals to the full sample of hospitals, and Appendix Table A.3 compares treated and control units for our main cross-geography acquisition panel. We note that the filters are particularly strict on acquirers, which are typically involved in multiple consecutive mergers. As a result, our focus is on the effects of mergers on target hospitals. We present the key evidence on acquirers in Appendix A.5.

The rest of this section is organized as follows. First, we present the main estimates of merger effects on acquired hospitals, comparing local and cross-geography mergers. Second, we present the main evidence on employer overlap and its predictive effect on merger outcomes. Third, we show how employer overlap and the distance between merging parties interact to shape merger effects, demonstrating that employer overlap effects persist even in very distant mergers. We conclude the section by exploring how heterogeneity in hospital types, local competition, and local geography shapes merger price effects.

4.1 Local and Cross-Geography Mergers

We begin with a basic analysis of merger effects, which serves as the basis for all subsequent results. Using our stacked event panels, we conduct the following regression:

$$\ln(p_{het}) = \alpha \times \text{Post}_{et} \times \text{Treated}_{he} + \boldsymbol{x}'_{het}\boldsymbol{\beta} + \delta_{et} + \gamma_{he} + \epsilon_{het} \tag{5}$$

where p_{het} is the price of hospital h associated with merger event e at event time t. Here, t denotes event time, and Post_{et} indicates that $t \geq 0$. The event-by-event-time fixed effects, δ_{et}, absorb

[25] This strategy is robust to common issues in two-way fixed effects (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021).

shocks common to treated and control hospitals in each event stack and event-time period; the hospital-by-event fixed effects, γ_{he}, absorb time-invariant differences across hospitals within each event stack. In some regressions, we also include time-varying factors $\boldsymbol{x}_{het}$ associated with the hospital. Throughout our analyses that rely on the stacked merger event panels as used here, we cluster standard errors at the hospital level to adjust for the repeated use of control hospitals across events. We also weight control units following the approach of Wing et al. (2024), which, in our analysis, ensures that event stacks with more control units do not contribute mechanically more to the estimated coefficient.[26] We restrict attention to the 5 years preceding and following the merger.

Table 3 presents the resulting estimates for within- and cross-geography mergers, focusing exclusively on acquired hospitals. Column (1) shows the effect of local mergers, indicating a mean price increase of 6.0% following the merger. This estimate is broadly consistent with the literature.[27] Columns (2)–(4) incrementally control for potentially changing bed capacity, staffing levels, and cost per admission. The results show a negligible impact on the estimated effects, with price increases declining slightly once changes in costs are accounted for. Columns (5)–(8) report the cross-geography specifications: column (5) reports the baseline estimate (4.4%), and column (6) adds bed-capacity controls and yields a slightly higher estimate (4.5%). Our preferred specification in column (7) additionally controls for staffing levels and yields the headline estimate of 4.6%. Column (8) further controls for cost per admission and yields an identical estimate. Thus, changes in capacity, staffing, and cost explain little of the variation and slightly amplify the estimated merger effect.[28]

Figure 1 shows the event-study estimates for the within- and cross-geography mergers. Focusing on the cross-geography mergers, the figure shows no pretrends in the seven-year window centered on acquisition, within which we impose our sample filters, and a steady price increase following acquisition. We note that hospital-insurer contracts typically last between three and five years, suggesting that not all hospitals can immediately exercise additional market power following a merger. Thus, the slope shown following the merger might reflect an averaging between hospitals that negotiated new contracts post-acquisition and those that have yet to do so. In addition, after our filter window ends three years post-acquisition, the acquired hospital might experience additional price effects from the steady stream of acquisitions that cross-geography acquirers engage in. We note that event time zero corresponds to the year in which the merger is consummated, but full changes in ownership and control can take several months; thus, immediate responses to mergers are unlikely, consistent with our findings. To put these numbers into perspective, the estimates indicate that between the year prior to the merger and two years after the merger, the price of a vaginal delivery for a woman aged 31–35

[26]Given our balanced design, the sample weights become one for the focal treated unit in each event and $1/N^c_{et}$, where N^c_{et} is the number of control hospitals in event e in period t.

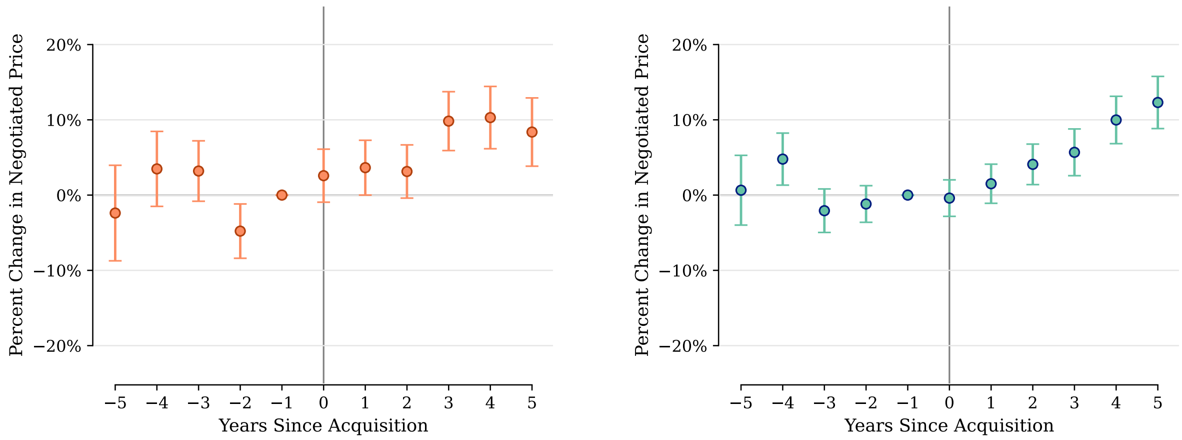
[27]Cooper et al. (2019) find a price effect for acquired hospitals of 1–6%, decreasing by distance and increasing by years following the merger event. Dafny et al. (2019) find a price effect of 7–10% for acquiring systems following cross-market mergers. Brot et al. (2024) find a composite price effect of 1.6% for both acquired and acquiring hospitals.

[28]Notably, these increases are above and beyond the average price growth at control hospitals, which averages 3.26% per year (see Appendix Table A.4).

Table 3: Merger Effects on Log Negotiated Prices for Acquired Hospitals

	Within-Geography				**Cross-Geography**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treated (α)	0.060***	0.060***	0.060***	0.058***	0.044***	0.045***	0.046***	0.046***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.012)	(0.012)	(0.012)	(0.012)
Bed Controls		✓	✓	✓		✓	✓	✓
Staffing Controls			✓	✓			✓	✓
Cost Controls				✓				✓
Mean dep. var.	-0.505	-0.505	-0.505	-0.527	-0.592	-0.592	-0.592	-0.615
Observations	233,338	233,338	233,338	224,731	370,810	370,810	370,810	356,354
R^2	0.813	0.813	0.813	0.815	0.798	0.798	0.798	0.800

Notes: Bed controls account for total staffed beds. Staffing controls account for doctors per bed and nurses per bed. Cost controls account for logged costs per admission, as reported in HCRIS. The mean dependent variable is measured in the year before the merger event. Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions include hospital-event and year-event fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$



(a) Within-Geography Mergers

(b) Cross-Geography Mergers

Figure 1: Merger Effects on Log Negotiated Prices for Acquired Hospitals

Notes: These panels present the treatment (acquisition) effect on prices over time, corresponding to Equation (5). Error bars show 95 percent confidence intervals. Standard errors are clustered at the hospital level. Regressions use analytic event-stack weights following Wing et al. (2024).

increased by about $460 more at acquired hospitals than at similar hospitals.

4.2 Employer Overlap

We now turn to the main hypothesized channel for the cross-geography effects—employers. We begin by defining a simple metric of employer overlap between merging markets, which we call the *employer overlap share*. For a merger event e occurring in year t, denote by $\mathcal{M}_e^{acquirer}$ the collection of local markets in which acquiring hospitals are located and by $\mathcal{M}_e^{target}$ the collection of local markets for target hospitals. For any cross-geography merger, these two are disjoint by construction. Let $\mathcal{F}_t$ denote the set of employers, and for any firm $f \in \mathcal{F}_t$ let $N_{fm,t}$ denote the number of employees the firm has in market m. The employer overlap share of the merger

Table 4: Summary Statistics by Employer-Overlap Quartile

Quartile	Employer Overlap (%)	Target-Market Employees	Total Covered Lives of Overlapping Employers		
			1% Threshold	5% Threshold	10% Threshold
Q1	0.002 [0.000; 0.006]	6,689 [36; 445,247]	3,351 [0; 29,681]	321 [0; 4,419]	94 [0; 1,146]
Q2	0.017 [0.006; 0.032]	6,604 [898; 72,514]	34,482 [2,479; 147,477]	3,458 [0; 36,165]	970 [0; 6,567]
Q3	0.091 [0.033; 0.232]	35,916 [6,077; 242,458]	262,647 [52,823; 955,024]	27,065 [462; 137,164]	7,010 [40; 58,652]
Q4	0.825 [0.237; 1.739]	311,962 [61,050; 462,823]	1,916,131 [585,473; 5,608,128]	293,589 [9,325; 1,431,283]	94,705 [350; 582,474]

Notes: The table presents the mean of each column, with the range (minimum, maximum) in brackets. Employer overlap is shown as a percentage of the insurer's covered employer-sponsored population. Target-market employees correspond to the total number of covered employees across all target markets in the merger, summed across employers. The total-covered-lives columns report the combined population of qualifying employers with a significant share of employees in both the acquirer and target markets. At an n% threshold, qualifying firms have at least n% of their covered lives in each market group; the column reports their combined covered lives. A value of zero indicates that at least one merger in the quartile has no employer satisfying the relevant threshold. The full distribution of employer overlap for cross-HSA mergers is shown in Appendix Figure A.1.

event e is defined as

$$O_e = \frac{\sum_{f \in \mathcal{F}_{t-1}} \min\{\sum_{m \in \mathcal{M}_e^{acquirer}} N_{fm,t-1}, \sum_{m \in \mathcal{M}_e^{target}} N_{fm,t-1}\}}{\sum_{f \in \mathcal{F}_{t-1}} \sum_{m \in \mathcal{M}_{t-1}} N_{fm,t-1}} \tag{6}$$

where $\mathcal{M}_{t-1}$ is the collection of all local markets served in the year prior to the merger.[29]

The expression above can be understood as the ratio of potentially exposed employees to the total number of covered employees before the merger. The numerator sums over employers and counts the number of their employees who live in the local markets of acquirers and targets. The exposure is taken to be the minimum of the two populations, which serves two purposes: first, if the employer is absent from the acquirers' markets or from the targets' markets, its contribution to the metric is zero (i.e., the employer does not span both sides of the merger); second, it is a conservative lower bound on the number of potentially affected employees if the integrated system drops out of the network. The numerator then sums this exposure across employers. Dividing by the insurer's total covered population puts all merger events on a common scale. Measurements are taken in the year before the merger to capture the likely set of employer contracts that the merging system could threaten. The metric is always non-negative and has a theoretical ceiling of one-half. It is zero for any merger that does not overlap employers at all and never double-counts employees, even for local mergers.

Table 4 reports the distribution of the employer overlap share and its connection to market size and overlapping employer size. The target-market employees column displays the size

[29]This construction is similar in spirit to the *insurer overlap* metric developed by Dafny et al. (2019), which they use to determine if insurers overlap merger geographies based on their local market shares. Our data do not allow us to construct insurer overlap within the same geography as our employer data. Thus, we cannot assess if the finding of Dafny et al. (2019) that merger effects load on insurer overlap holds. However, because our data come from a single large insurer, some insurer overlap is present across all of our observations.

of markets containing target hospitals, as measured by the total number of employees in each hospital's HSA. As shown, greater employer overlap is associated with acquiring hospitals in larger markets. The remaining columns report the combined membership of employers meeting each overlap threshold, representing the number of covered lives that might be at risk for the insurer if a disagreement with the merged system leads overlapping employers to switch insurers. Designating employers as overlapping requires taking a stance on how large an employer's footprint has to be in each market for it to consider local access to care significant. The table reports three thresholds for this classification, based on the minimum share of employees that the employer has in the target and acquirer markets. For example, if system A acquires system B, then we compute for each employer the share of lives covered across all markets served by system A hospitals, the share of all lives covered in all markets served by system B hospitals, and then take the minimum. Under a 5% threshold, the firm is considered overlapping if it has at least 5% of its covered lives in each of these market groups. Thus, a merger might have positive overlap but zero covered lives by overlapping employers if no single firm meets the threshold. We find that mergers in the highest quartile of overlap affect between nearly 100,000 and nearly two million covered lives, depending on the threshold. These thresholds are used only for quantifying the extent of overlap and play no role in our empirical analysis.

Table 5 shows the key estimated effects of the interacted regression:

$$\ln(p_{het}) = \alpha \times \text{Post}_{et} \times \text{Treated}_{he} + \alpha^{overlap} \times O_e \times \text{Post}_{et} \times \text{Treated}_{he} + \boldsymbol{x}'_{het}\boldsymbol{\beta} + \delta_{et} + \gamma_{he} + \epsilon_{het} \quad (7)$$

where $\alpha^{overlap}$ captures the effect of overlapping employers. The first column shows a positive and statistically significant association between employer overlap and the post-acquisition price effect at target hospitals. The estimate indicates that a one-standard-deviation increase in employer overlap increases post-acquisition prices by about 3.5%. Moreover, the estimates indicate that the baseline acquisition effect is marginally significant and equals about half (52.2%) of its original magnitude. The following columns show the effect persists after controlling for changes in beds, staffing levels, and costs.

The estimates above support the hypothesis that employer overlap drives cross-geography merger effects. However, there is no single way of expressing overlap. In our explorations, we have found that measuring overlap by employee counts or by its share of the state-specific employer population often produces coefficients with the expected sign, but the estimates are weaker than those from our proposed metric. Intuitively, whether an overlap is relevant to an insurer's decisions depends on its scale relative to the insurer's entire population, which is why our main metric is divided by the total covered population. Alternatives based on local demand models also appear to work well in practice, but require stronger assumptions about patients' choice sets and competition, as well as access to admission data. In contrast, the employer overlap share can be constructed solely from data on employer insurance coverage, which is publicly reported in the Department of Labor's Form 5500, and on employee geography,

Table 5: Overlap Effect on Log Negotiated Price for Acquired Hospitals

	(1)	(2)	(3)	(4)
Post × Treated (α)	0.023*	0.023*	0.024*	0.024*
	(0.013)	(0.013)	(0.013)	(0.013)
Post × Treated × Overlap ($\alpha^{overlap}$)	8.656***	8.637***	8.673***	8.692***
	(2.021)	(2.018)	(2.018)	(1.992)
Bed Controls		✓	✓	✓
Staffing Controls			✓	✓
Cost Controls				✓
Mean dep. var.	-0.592	-0.592	-0.592	-0.615
Mean overlap	0.002	0.002	0.002	0.002
SD overlap	0.004	0.004	0.004	0.004
Observations	369,793	369,793	369,793	355,368
R^2	0.790	0.790	0.790	0.791

Notes: Bed controls account for total staffed beds. Staffing controls account for doctors per bed and nurses per bed. Cost controls account for logged costs per admission, as reported in HCRIS. The mean dependent variable is measured in the year before the merger event. Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions include hospital-event and year-event fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

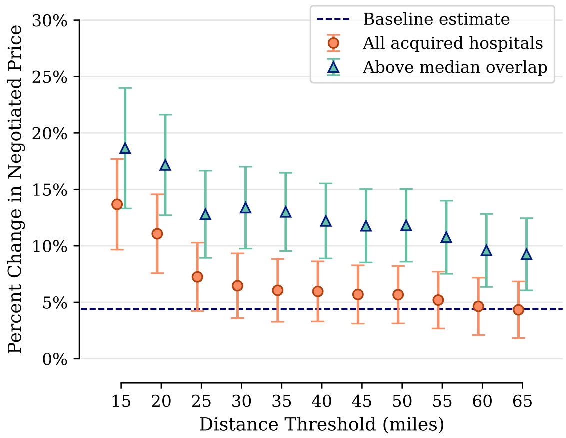
which regulatory authorities may find easier to obtain or proxy using commute patterns and employer size. In Section 9.2, we present an alternative measure based on network-adequacy rules for Medicare Advantage plans that performs similarly to employer overlap and is also simple to compute. This alternative metric yields estimates similar to those shown above and supports the employer overlap hypothesis.

4.3 Distance and Common Demand

Our definition of local markets is convenient but narrow. A natural concern is that we are erroneously labeling local mergers as distant, and that our employer-overlap metric is simply measuring the affected population. To alleviate this concern, we show how merger effects attenuate with distance. Figure 2a shows the estimated effects from regression (5) when restricting attention to the sample of hospitals acquired within a certain distance of their nearest acquirer. As the distance cutoff expands to include more distant acquisitions, the estimate approaches the full-sample estimate from Section 4.1; the above-median-overlap estimate remains larger even in bins extending to 65 miles. This figure is designed to be comparable to Figure 10 from Cooper et al. (2019), which provided key evidence of local merger effects as a function of distance based on data from 2007 and 2011. The two figures are consistent, and our figure additionally separates acquisitions by employer overlap.[30]

Despite the evidence from Figure 2a, it is still possible that conditioning on high overlap implicitly selects the closest merging hospitals. Overlap and distance are related, but mergers

[30] We note that Cooper et al. (2019) find no merger effects past around 25 miles. Our analysis differs from theirs in at least three key ways: first, we focus on merger targets and within-state acquisitions; second, we cover a longer period, including more recent years; third, we differ in our strategies for identifying control units and maintaining a stable cohort of controls over time.



(a) Distance effects

		Distance Quartiles				
		Q_1^D	Q_2^D	Q_3^D	Q_4^D	Avg.
Overlap Quartiles	Q_1^O	-0.048 (0.048)	-0.044 (0.036)	-0.045 (0.030)	-0.041 (0.039)	-0.045** (0.021)
	Q_2^O	0.115*** (0.024)	0.021 (0.028)	0.002 (0.024)	-0.043 (0.042)	0.030* (0.016)
	Q_3^O	0.185*** (0.034)	0.079*** (0.029)	-0.027 (0.048)	0.076** (0.031)	0.092*** (0.019)
	Q_4^O	0.152*** (0.030)	0.065** (0.029)	0.050* (0.028)	0.109*** (0.031)	0.094*** (0.020)
	Avg.	0.117*** (0.018)	0.024 (0.017)	-0.008 (0.018)	0.050*** (0.019)	0.044*** (0.012)

(b) Estimated Cross-Effects

Figure 2: Acquisition Effects on Log Negotiated Prices by Proximity to Acquirer

Notes: Panel (a) presents the acquisition effect on prices, corresponding to Equation (5), when the sample is restricted to hospitals whose nearest acquirer is within x miles, for increasing x. This figure uses 95% confidence intervals. Panel (b) shows the estimated cross-effects of overlap and distance from Equation (8). The final column reports the average estimated effect within each employer-overlap quartile across distance quartiles, while the final row reports the average estimated effect within each distance quartile across employer-overlap quartiles. These marginal estimates are weighted by the number of merger events in each overlap-distance cell. The bottom-right entry reports the corresponding average across all merger events. Appendix Table A.5 displays summary statistics for each distance quartile, while Table 4 displays summary statistics for each employer-overlap quartile. In both panels, standard errors are clustered at the hospital level. Regressions use analytic event-stack weights following Wing et al. (2024). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

at every distance exhibit substantial overlap, allowing us to test their effects jointly.[31] For that, we codify quartiles of distance and employer overlap between targets and acquirers, and run the regression:

$$\ln(p_{het}) = \sum_{r=1}^{4}\sum_{q=1}^{4} \alpha_{r,q} \times \text{Post}_{et} \times \text{Treated}_{he} \times \mathbb{1}\{O_e \in Q_r^O\} \times \mathbb{1}\{D_{e,0} \in Q_q^D\} + \delta_{et} + \gamma_{he} + \epsilon_{het} \quad (8)$$

where $D_{e,0}$ is the shortest distance between merging parties in event e, as measured in the year of the merger; Q_r^O is the r-th quartile set of overlap; and Q_q^D is the q-th quartile set of distance. We run this regression exclusively on cross-HSA acquisitions, as in the majority of our analysis.

Panel (b) of Figure 2 reports $\alpha_{r,q}$ from Equation (8) for each cell defined by employer-overlap quartile r (rows) and merger-distance quartile q (columns), as well as pooled average estimates associated with each quartile of overlap or distance, independently. Each coefficient is the post-acquisition treated-control price difference for mergers in that overlap-distance cell. The results show a clear, strong effect in the highest-quartile overlap row, which persists even in the most distant mergers. In fact, for the highest-overlap quartile, the effect of distance appears non-monotonic: the two largest estimates appear for the closest and most distant mergers, with the largest in the closest-distance quartile. Looking at the pooled average column, we note that the overlap effect is monotonic but non-linear, with the largest increase in merger effects happening at the median overlap and the incremental effect of overlap being positive but small on average. This suggests hospitals have decreasing returns to employer overlap,

[31] Appendix Table A.6 shows overlap by distance.

which appears connected to the submodularity of network additions hypothesized in Section 2. Notably, mergers with below-median employer overlap show small to negative and statistically insignificant price changes in almost all cells, with the pooled first quartile of overlap showing a 4.5% decrease in prices post-acquisition.

The evidence above clearly shows that mergers in which employers overlap considerably across geographies lead to significant price increases, even when the merging hospitals are very distant. In the fourth quartile of distance, merger hospital pairs are more than 66 miles apart, implying negligible common demand between the hospitals. In our data, fewer than 1.4% of patients ever travel further than this distance for care. Moreover, our data show that acquiring hospitals account for less than 0.4% of all hospital-to-hospital referral volume at target hospitals, with the share being exactly zero for those in the fourth quartile of distance. Thus, these merger effects are unlikely to stem from niche common hospital demand. Rather, they appear driven by intermediation forces, as the overlap metric highlights.

4.4 Heterogeneous Effects

Having established the main evidence, we now turn our attention to heterogeneity. We ask whether cross-geography merger effects or employer-overlap interactions are particularly salient in certain markets or hospitals. That is, for a given heterogeneity variable $H_{he,-1}$ measured in the year prior to the merger, we run the regression

$$\begin{aligned} \ln(p_{het}) = &\alpha \times \text{Post}_{et} \times \text{Treated}_{he} + \theta \times \text{Post}_{et} \times \text{Treated}_{he} \times H_{he,-1} \\ &+ \alpha^{overlap} \times O_e \times \text{Post}_{et} \times \text{Treated}_{he} \\ &+ \theta^{overlap} \times O_e \times \text{Post}_{et} \times \text{Treated}_{he} \times H_{he,-1} \\ &+ \delta_{et} + \gamma_{he} + \epsilon_{het} \end{aligned} \tag{9}$$

In Equation (9), θ is the coefficient on Post × Treated × H and measures how the post-acquisition treatment effect varies with H. The coefficient $\theta^{overlap}$ is on Overlap × Post × Treated × H and measures how the overlap interaction varies with H.

Table 6 shows the key estimated effects. The first column shows that larger hospitals, as measured by their number of beds, face larger baseline merger effects but a smaller incremental effect of employer overlap. This is consistent with larger hospitals having more pre-existing leverage and smaller incremental gains in power from acquisition. The second column shows that a similar effect exists for hospitals acquired by larger systems, which is consistent with the non-linear effect of overlap on negotiated prices documented in the previous section. These two observations suggest that acquirers' ability to leverage overlap may exhibit decreasing returns to scale. The third column shows that acquisitions made by the five largest multi-state systems have a negligible baseline effect and a significantly smaller incremental employer-overlap effect. Appendix Table A.7 further supports this by showing the overlap effect estimated separately for this group rather than as an additive term. This observation is consistent with anecdotal evidence that the largest systems negotiate nearly uniform rates across their members, limiting

Table 6: Cross-Geography Merger Effects on Prices (Heterogeneity)

	Log Negotiated Price								
	Beds (100s) (1)	Acquirer Size (2)	Largest Systems (3)	HHI (4)	Non-Gov. Hospitals (5)	Share Gov. Hospitals (6)	Rural (7)	Critical Access (8)	Low Income (9)
Post × Treated	-0.045***	-0.022	0.018	0.047*	0.011	0.038***	0.040***	0.045***	0.036**
	(0.017)	(0.015)	(0.013)	(0.028)	(0.014)	(0.014)	(0.014)	(0.013)	(0.015)
↪ × H	0.050***	0.001***	0.008	-0.030	0.006**	-0.089***	-0.046**	-0.130***	-0.031
	(0.007)	(0.000)	(0.043)	(0.034)	(0.002)	(0.032)	(0.023)	(0.031)	(0.023)
↪ × Overlap	17.118***	53.187***	18.347***	9.864***	8.346***	5.102**	9.592***	6.872***	11.076***
	(2.715)	(9.680)	(2.409)	(3.216)	(2.316)	(2.101)	(2.042)	(2.058)	(2.225)
↪ × Overlap × H	-6.599***	-0.419***	-15.039***	-2.813	-0.261	43.021***	-5.837*	8.660	-5.061*
	(1.542)	(0.087)	(4.735)	(4.217)	(0.159)	(11.993)	(3.208)	(6.065)	(2.850)
Mean dep. var.	-0.592	-0.592	-0.592	-0.592	-0.592	-0.592	-0.592	-0.592	-0.592
Mean *H* pre-merger	1.256	38.181	0.003	0.696	5.621	0.349	0.439	0.297	0.400
Observations	358,919	369,501	358,919	369,793	369,494	369,494	352,414	369,793	352,287
R^2	0.789	0.787	0.789	0.790	0.790	0.790	0.789	0.790	0.790

Notes: Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions include hospital-event and year-event fixed effects. Heterogeneity variables vary by column as indicated. Acquirer size is defined as the number of hospitals in the acquiring system in the year before the merger. Largest Systems is an indicator for the five largest systems in our sample, defined by the average number of hospitals over time. The Herfindahl–Hirschman Index (HHI) is defined as the sum of squared admission shares within an HSA. "Non-Gov. Hospitals" counts the number of non-government hospitals at the HSA level, while "Share Gov. Hospitals" measures the share of government hospitals within an HSA. Rural and Critical Access are hospital-level indicators of whether a hospital is in a rural area or is designated as a critical access hospital. "Low Income" is a county-level indicator for whether the hospital's county has a lower median household income than the sample median. The mean dependent variable is measured in the year before the merger. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

their ability to leverage heterogeneous employer-overlap gains. We note that for this group of acquisitions, we find overall small price changes in our sample, as inferred from the small baseline effect (2.6%).

Columns four to six study heterogeneity based on the local level of competition in the target market. Accounting for the target's local HHI does not meaningfully change the estimates, while more local non-government competitors appear to increase the baseline merger effect without materially altering overlap effects. Target markets where a larger share of hospitals is government-owned have significantly smaller baseline merger effects and significantly larger employer overlap effects. This last observation appears particularly relevant because it suggests that government-based competition disciplines baseline merger effects; however, since public hospitals are not often popular among the privately insured employer-sponsored population, having a larger share of local government hospitals suggests fewer relevant local substitutes for the target hospital and thus a larger role for it in local employer networks.

The final set of columns studies heterogeneity by the local geography of the acquired hospital, supported by binary analysis in Appendix Table A.7. Column seven shows that the baseline merger effect for acquired rural hospitals is small, negative, and statistically insignificant, and their employer-overlap effects are small, positive, and statistically insignificant. In contrast, acquired critical access hospitals show a negative baseline effect but a considerably larger employer-overlap effect. These findings are consistent with these hospitals being, on average, not heavily frequented by employer-sponsored enrollees in our sample. Yet, because these are often isolated and locally important hospitals, when employer overlap is present, they are likely very important for employer networks. Finally, the last column shows the effect of controlling for whether the county where the target hospital is located has a median household income above or below the median among target-hospital counties. The estimates indicate a stronger overlap effect—and thus a stronger cross-geography merger effect—in higher-income counties.

Overall, the results indicate significant heterogeneity in cross-geography mergers. Baseline merger effects are larger for larger hospitals, those acquired by larger systems, those in urban areas, non-critical-access hospitals, and those facing less local competition from government hospitals. In addition, we find that employer overlap is positively and significantly associated with price increases for almost all acquisitions, with smaller overlap effects for larger hospitals and those acquired by larger systems, but larger effects for those with more local competition from public hospitals and those in urban and high-income areas.

5 Effects on Healthcare Production

Having established conditions under which cross-geography mergers yield price increases, we now examine how these mergers shape hospital care production. We focus on how mergers affect capital, staffing, quality, and overall cost per admission. In what follows, we use the same regression strategy as in (5), but with different hospital attributes as outcomes.

Panel A of Table 7 shows that hospital bed capacity falls significantly following cross-geography acquisition. The average acquired hospital loses 4 beds, or about 3% of its total capacity, and a comparatively larger fraction of its specialty care beds, with cardiac care beds declining by 14.3% relative to pre-merger levels. Columns 6 and 7 of the same panel show that, in addition to reduced capacity, hospital staffing levels decline, with full-time equivalent nurses and doctors per bed down 11.9% and 21.3%, respectively. Concurrent work by Setzler (2025) documents similar labor changes following local mergers.

In Appendix Table A.8, we explore how these changes are associated with a hospital's local environment and type. We find that the decline in nurses per bed and doctors per bed is not statistically different in more concentrated, rural, or lower-income markets. However, we find that more staffed beds are lost in more concentrated markets, and the decline in full-time-equivalent doctors per licensed bed is nearly twice as large at acquired critical access hospitals. This suggests that local competition may discipline capacity reductions and that some labor changes may be the merged entity's response to the costs of running a critical access hospital. In addition, we explore whether these changes in labor demand are reflected in local wages, as reported by the Bureau of Labor Statistics (U.S. Bureau of Labor Statistics, 2026b).[32] Appendix Table A.9 shows no meaningful changes in wages for licensed practical nurses, registered nurses, internists, OB/GYNs, primary care physicians, or physical therapists. These observations suggest that while these staffing changes may be large relative to the size

[32]This analysis is the sole exception to our standard error clustering strategy. Because the wage data are reported at the Core-Based Statistical Area (CBSA) level, we cluster standard errors at that level.

Table 7: Effects on Healthcare Production

	Panel A: Beds and Staff per Bed						
	Total Beds	Cardiac Beds	Obstetric Beds	Pediatric Beds General	Pediatric Beds Intensive	Nurses per Bed	Doctors per Bed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × Treated	-4.223***	-0.361***	-0.234*	-0.463***	-0.148***	-0.030***	-0.032***
	(1.019)	(0.092)	(0.125)	(0.119)	(0.038)	(0.004)	(0.005)
Mean dep. var.	140.623	2.517	8.452	6.402	0.991	0.252	0.150
Observations	681,006	591,367	591,367	591,367	591,367	680,934	680,934
R^2	0.972	0.920	0.965	0.980	0.956	0.742	0.859

	Panel B: Capital and Services						
	CT Scanner	MRI Machine	Emergency Room	Dialysis	Mammo-graphy	Oncology	Outpatient Surgery
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × Treated	-0.036***	-0.026***	-0.021***	-0.021**	-0.034***	-0.040***	-0.026***
	(0.005)	(0.007)	(0.005)	(0.009)	(0.007)	(0.008)	(0.005)
Mean dep. var.	0.838	0.626	0.845	0.318	0.654	0.476	0.794
Observations	681,006	681,006	681,006	681,006	681,006	681,006	681,006
R^2	0.880	0.852	0.896	0.832	0.866	0.861	0.896

	Panel C: Quality and Log Costs Per Admission						
	Readmits	Recommend	Score	Cost	Cost	Cost	Cost
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × Treated	0.000	-0.011***	-0.010***	-0.063***	-0.071***	-0.066***	-0.066***
	(0.000)	(0.002)	(0.002)	(0.010)	(0.010)	(0.011)	(0.011)
Bed Controls					✓	✓	✓
Staffing Controls						✓	✓
Service Controls							✓
Mean dep. var.	0.157	2.661	2.624	10.233	10.248	10.248	10.248
Observations	498,594	491,923	435,491	611,364	550,749	550,749	550,749
R^2	0.841	0.879	0.868	0.840	0.851	0.852	0.852

Notes: In Panel A, total beds correspond to total staffed beds, as reported in the AHA survey. Staffing is measured in full-time equivalents per licensed bed. In Panel B, outcomes are as reported in the AHA survey. In Panel C, all-cause readmission rates are obtained from CMS (Centers for Medicare & Medicaid Services, 2026a), while "recommend" and "score" come from HCAHPS (Centers for Medicare & Medicaid Services, 2025). The "recommend" column corresponds to the average response to "would recommend to others (1–3)", while "score" corresponds to the overall score a patient provides to the hospital, which ranges from 1 to 3. In Panel C, bed controls include the total number of staffed, obstetric, and pediatric beds. Staffing controls include full-time equivalent doctors, nurses, and workers per bed, as well as total payroll costs. Service controls additionally include access to a CT scanner, an MRI machine, an emergency room, hemodialysis, mammography, oncology, surgical, and psychiatric services. Across the table, all regressions include hospital-event and year-event fixed effects. The mean dependent variable is measured in the year before the merger event. Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

of target hospitals, they are likely small relative to local demand for medical professionals.[33]

Panel B of Table 7 shows that capacity and staffing changes are accompanied by significant changes to capital and service offerings. Acquired hospitals are less likely to have a CT scanner or MRI machine post-acquisition than controls. They are also less likely to operate an open-access emergency room or offer mammography, oncological care, hemodialysis, or

[33]The results also show a modest—in size and significance—connection between overlap and wages for OB/GYNs and physical therapists, which might suggest that some of the increase in revenue in areas that saw the largest price increases was passed through to these workers.

outpatient surgery. These findings highlight a significant reduction in the variety of services at acquired hospitals, suggesting that cross-geography acquisitions might have particularly important effects on access to care beyond what our data can measure.

On the positive side, Panel C shows that these changes appear to have only modest effects on quality. In particular, all-cause readmissions do not materially change post-acquisition. Beyond all-cause readmissions, however, measures of mortality, condition-specific readmissions, and hospital-acquired infections are too sparsely reported in the sample to be examined at scale. A better-populated alternative is the Hospital Consumer Assessment of Healthcare Providers and Systems (HCAHPS) survey, which records patients' experiences with hospital services and quality. The second column of Panel C shows that acquisitions slightly decrease the score patients give hospitals when asked whether they would recommend them for future care. The third column of Panel C shows a similar decrease in the overall HCAHPS score for hospitals, which summarizes various features such as doctor communication, pain management, and cleanliness as experienced by patients. The decline in both measures is modest, amounting to about a 0.4% decrease relative to the pre-merger baseline. This moderate decline in quality contrasts with larger effects found by Setzler (2025) following local mergers but is consistent with evidence on cross-market acquirers reported by Arnold et al. (2025a).

In total, cross-geography acquisitions appear to yield significant changes in hospital operations. Perhaps the most striking of these changes is expressed in the overall cost per admission, as shown in the last four columns of Panel C of Table 7. Consistent with prior evidence (Schmitt, 2017), we find that hospital costs fall by over 6%, an effect that is only magnified when accounting for changes in capital and staffing. The limited evidence on quality suggests that this decrease in costs is not due to lower-quality service, and the evidence presented in the next section will support the hypothesis that these cost changes are not due to a reduction in the complexity of admitted patients. In Appendix Table A.10, we show that cost reduction and labor changes are most pronounced at distant high-overlap cross-geography mergers, while capacity reductions are most pronounced in proximal low-overlap cross-geography mergers. Put together, the evidence suggests that capacity reductions are occurring at a different set of hospitals than where labor and cost reductions are materializing. We observe that admissions from our data provider decline after acquisition while privately insured admissions from other payers increase. The shift toward privately insured patients covered by other insurers, whose services may yield higher margins, provides one possible explanation for the joint decline in labor and costs. However, because labor changes do not explain the entire decline in costs, part of that decline may reflect reduced administrative costs, consistent with concurrent evidence on the effect of local mergers on this margin (Arnold et al., 2025b).

This evidence indicates that hospital markups might have increased by substantially more than price evidence alone would suggest. Appendix Table A.11 shows the change in the hospital's per-admission markup, defined as the ratio of revenue per admission from our data provider to the HCRIS-reported cost per admission, minus one. We find that markups increased by about 3.8 percentage points following cross-geography acquisitions, with significantly larger effects at hospitals that are neither located in rural areas nor designated as critical access hospitals. We note, however, that this evidence is suggestive, as we measure cost using HCRIS data, which are known to have data-quality issues.[34] As the next section will show, acquired hospitals experience substantial changes in demand, implying that the ratio of cost per privately insured patient to the average hospital admission cost may have changed significantly post-acquisition.

6 Merger Effects on Demand for Insurance and Care

We now turn to the effect of cross-geography mergers on demand for insurance and care. We begin by establishing basic patterns of employer-sponsored demand, showing that employers care about the access their employees have to their local network and the prices they face for care. We then test whether employer enrollment changes after acquisition. Linking these changes to hospital demand, we show that admissions fall following acquisition, with a net decrease broadly consistent with the hospital's shrinking capacity. We describe demand patterns that are suggestive of strong recapture effects by hospitals.

For each employer in our data, we define the *Network Coverage* of the data provider's network as the share of local hospitals included in the network. Specifically, for each employee, we compute the share of general acute care hospitals in their HSA that are within our data provider's network. The network coverage for the employer-year is defined as the average of these shares across all of its employees. Similarly, we define the average price of the network faced by each employer as the admission-weighted average negotiated price of all in-network hospitals that are local to each employee.[35] Using these two metrics, we estimate the regression:

$$y_{ft} = \alpha N_{ft} + \beta \ln(\bar{p}_{ft}) + \gamma_f + \epsilon_{ft} \tag{10}$$

where y_{ft} is an enrollment indicator which is equal to one when the employer is under contract with the data provider and zero otherwise, N_{ft} is the network coverage, $\bar{p}_{ft}$ is the average price in the network for the employer, and γ_f is an employer fixed effect.

Panel A in Table 8 shows the key estimates. The first two columns estimate the regression using all employers ever observed in our enrollment data, showing a positive effect of network coverage and a negative effect of prices. However, a large fraction of the employers in our sample have been persistently under contract, leaving little within-employer variation and likely attenuating the estimates. Accordingly, columns 3 and 4 of Panel A show significantly stronger estimates when the sample is restricted to the subset of employers for which we observe a cycle: an initial period in which they are not under contract, a period under contract, and one or more subsequent periods out of contract. We view these employers as active

[34] A large collection of articles and policy reports document reporting errors, inconsistencies in reporting, selective revision of reports, endogenous reporting, and gaps in oversight of submissions to the HCRIS dataset (Kane and Magnus, 2001; Medicare Payment Advisory Commission, 2004; Lamboy-Ruiz et al., 2019; U.S. Department of Health and Human Services, Office of Inspector General, 2025; Chalmers et al., 2026). The gaps are often specific to certain fields or hospitals; thus, the data are still broadly valuable, but evidence from them should be interpreted with caution.

[35] We weight the average price by the overall admissions of the hospital from all sources, as reported in the AHA.

Table 8: Cross-Geography Merger Effects on Insurance and Hospital Demand

Panel A: Insurance Demand

	Employer Under Contract				Contract Drop		Log Enrollment	
	All	All	Cycle	Cycle	All	Cycle	Employees	Employers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Network Coverage	0.207***	0.692***	0.532***	1.443***				
	(0.007)	(0.009)	(0.023)	(0.032)				
Log Avg. Price		-0.649***		-1.186***				
		(0.005)		(0.025)				
Δ Network Coverage					-0.040***	-0.045***		
					(0.004)	(0.011)		
Post × Treated							-0.013	-0.009**
							(0.008)	(0.004)
Mean dep. var.	0.731	0.731	0.540	0.539	0.311	0.543	8.020	5.764
Observations	1,066,539	1,063,238	217,006	216,500	922,427	184,166	540,090	540,090
R^2	0.464	0.533	0.187	0.322	0.078	0.339	0.991	0.997

Panel B: Hospital Demand

	Admissions					HSA Total Admissions		Admitted DRG Weight	
	Data Provider	Medicaid	Medicare	Non-public	All	All	Data Provider	Mean	Variance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post × Treated	-28.152***	-28.395**	-211.040***	7.295	-232.139***	472.828*	-167.654***	-0.005	0.027
	(5.176)	(12.578)	(20.286)	(31.219)	(44.644)	(259.425)	(36.423)	(0.009)	(0.017)
Mean dep. var.	142.711	1,105.521	1,917.096	1,935.114	4,957.730	54,316.876	1,472.545	1.404	0.809
Observations	370,810	681,006	681,006	681,006	681,006	681,006	681,006	370,810	343,793
R^2	0.933	0.958	0.974	0.947	0.981	0.992	0.852	0.802	0.662

Notes: In Panel A, the Cycle subsample refers to firms that enter and exit our enrollment data at least once between 2011 and 2019. "Employer Under Contract" regressions include firm fixed effects. "Contract Drop" regressions include year fixed effects. In Panel B under admissions, "Data Provider" refers to admissions from our insurance data provider, while "All" refers to all-source admissions as reported in the AHA. "Non-public" equals AHA total facility admissions minus Medicare and Medicaid discharges; it includes admissions covered by our data provider and is therefore not restricted to commercially insured patients. Columns 1–6 of Panel A are estimated on the firm panel, are unweighted, and have standard errors clustered by firm. Columns 7–8 of Panel A and all columns of Panel B are estimated on the event panel with analytic event-stack weights following Wing et al. (2024) and standard errors clustered by hospital. Log Enrollment regressions in Panel A and all regressions in Panel B include hospital-event and year-event fixed effects matching the design of Equation (5). The mean dependent variable is measured in the year before the merger event. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

choosers and can interpret the estimates as indicating that they are significantly more likely to be under contract with our data provider in years when local networks are broader and prices are lower. To reinforce this interpretation, columns 5 and 6 redefine the outcome variable to indicate whether the employer drops coverage in the next year and regress this outcome on the change in network coverage, defined as next-year coverage minus current-year coverage. The estimates indicate that employers are more likely to drop coverage when the local network narrows.

Having established that employers consider network coverage and prices in contracting decisions, we now return to merger effects. Using the enrollment files and the difference-in-differences strategy in Equation (5), we document a modest decline in enrollment following acquisition. In particular, column 8 of Panel A in Table 8 shows that acquisition leads to a 0.9% decrease in the number of firms under contract with employees within the same HSA as the target hospital. The overall magnitude of the effect is small, since it counts all local firms, even those with minuscule local employment. In addition, Appendix Table A.12 shows that cross-geography acquisitions are associated with a lower likelihood of exclusion from insurance networks relative to controls. Thus, the network coverage effects do not appear to materially affect enrollment post-merger.

Using the same strategy, we now turn to hospital admissions at acquired hospitals. The first column of Panel B in Table 8 shows that hospitals acquired across geographies experienced 28.2 fewer admissions from employees covered by our data provider, amounting to a 19.7% loss. Columns (2) to (5) show that these hospitals significantly shrank their overall pool of admitted patients, experiencing declines equivalent to an 11% decrease in admissions from Medicare patients and a 4.7% overall decrease. This overall decrease is consistent with the average decrease in capacity: multiplying 232 fewer annual admissions by a five-day average stay and dividing by 365 yields roughly 3.2 beds of reduced capacity demand, comparable to the loss of 4.2 documented in Table 7.[36] As noted above, the decrease in services provided to Medicare patients might be a strategic response to increased revenue from services provided to the employer population. A reallocation of resources and restructuring of services in favor of higher margins might rationalize these decreases in Medicare services and the need for fewer beds.

The patterns in privately insured demand for care are striking. Subtracting the changes in Medicare and Medicaid admissions from the change in total admissions yields the change in non-Medicare/non-Medicaid admissions, which still includes patients covered by our data provider. Subtracting the observed change in admissions covered by our data provider from that residual yields the implied change in admissions covered by other non-Medicare/Medicaid payers. This implied increase more than offsets the loss of admissions covered by our data provider, consistent with recapture through other coverage. Columns (6) and (7) show further evidence of this recapture, as overall admissions occurring in HSAs experiencing a cross-geography merger remain largely unchanged, yet from the perspective of our data provider, admissions in these markets drop by 167.7, or 11.4% of pre-merger levels.

The aggregate admission patterns are consistent with patients replacing coverage from our data provider while retaining access to the hospital. We cannot distinguish whether this reflects employers switching insurers, firms leaving the affected labor markets, or workers changing jobs. The welfare implications therefore remain uncertain because each mechanism changes who bears the merger's costs. In addition, even if substitution is happening and, to some degree, mitigating the harms from these mergers, the number of remaining patients is still very large. Significant evidence on enrollment inertia also suggests that selection into switching might not necessarily protect those most harmed by price increases (Dafny, 2010; Craig, 2022;

[36]The length of stay used for this back-of-the-envelope calculation is based on Medicare admissions (KFF, 2023), which is comparable to the average in our sample of 4.5 days per inpatient admission. We note that patient crowdout might be counterintuitive given the limited occupancy ratios presented in Table 1. However, low average occupancy does not imply abundant usable capacity. Because admissions are stochastic, and beds are differentiated by service line, staffing, and timing, hospitals maintain reserve capacity to absorb demand surges, and aggregate occupancy can remain below full physical capacity even when the relevant operational capacity is locally binding (Gaynor and Anderson, 1995; Green, 2002; Rodríguez-Álvarez et al., 2012; Song et al., 2020). Capacity strain can worsen patient outcomes and increase treatment costs, while inducing hospitals to accelerate discharge, reduce treatment intensity, or, in some settings, tighten admission thresholds (Eriksson et al., 2017; Woodworth, 2020; Woodworth and Holmes, 2020; Hoe, 2022; Gowrisankaran et al., 2026).

Gao et al., 2023). Quantifying this harm, however, would require claims or discharge data from multiple identifiable insurers, with consistent employer identifiers, private prices, and identifiable hospitals, the combination of which does not exist as of yet in any dataset available to researchers, as far as we can tell.

Finally, despite reduced demand and the documented change in production technology, the hospital's case composition does not change materially. Using each admission's DRG weight as a summary measure of its resource intensity, columns (8) and (9) in Panel B of Table 8 show no significant change in the mean or variance of resource usage of admitted patients. Given the cost evidence presented earlier, this suggests that hospitals did not reduce admissions for lower-revenue services—which are often tied to DRG weights—but might instead have selectively reduced high-cost service offerings. This evidence is consistent with a growing literature documenting the closure of service lines in hospital care following mergers (Dranove et al., 2025).

7 Overlap and Predicted Mergers

The evidence above established the potential harm of cross-geography mergers through a retrospective analysis of prices, admissions, and services. In this section, we instead ask whether it is possible to predict which cross-geography mergers will occur. In particular, we focus on the ability of employer overlap to predict cross-geography acquisitions, considering hospital acquisitions with no additional system members in the state and the acquirers that bought those hospitals. For each acquirer, we build a choice panel by assuming they could have bought any such stand-alone hospital within the target state that does not overlap with members of the acquiring system in the same HSA. For each such potential acquisition, we compute the employer overlap share and the minimum distance between the potential target and the potential acquirer. Given our sample restrictions, the resulting panel contains 282 potential choice events. We use this panel to estimate a multinomial choice model of target acquisition.

For each acquiring system s in year t, we model the expected benefit of acquiring hospital h among its choice set $\mathcal{H}_{st}$ as

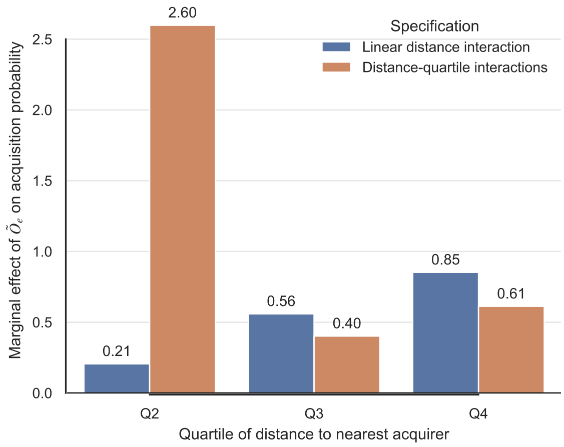
$$\pi_{hst} = v(O_{sh}, D_{sh}) + \boldsymbol{x}'_{ht}\boldsymbol{\beta} + \epsilon_{hst} \quad \forall s, t, h \in \mathcal{H}_{st} \tag{11}$$

where $v(\cdot, \cdot)$ is a function of the employer overlap and distance between merging parties, measured in the year prior to the potential merger event, and $\boldsymbol{x}_{ht}$ includes other hospital attributes such as the hospital type, staffing ratios, and certification and affiliation status. The error, ϵ_{hst}, is assumed to follow a type-I extreme-value distribution. In estimation, we measure O_{sh} in units of the population standard deviation (0.004) for interpretability.

Panel (a) of Figure 3 shows the estimates for two different specifications of $v(\cdot, \cdot)$. In the first, distance and overlap enter linearly, including an interaction term. In the second, the effect of employer overlap is allowed to change by quartiles of distance to the nearest acquirer. Both

	Chosen Target Hospital	
	(1)	(2)
Employer overlap	-0.889	
	(0.606)	
↪ × distance	0.015***	
	(0.005)	
↪ × distance Q1		-0.966
		(0.719)
↪ × distance Q2		1.609*
		(0.880)
↪ × distance Q3		0.743
		(1.187)
↪ × distance Q4		1.666***
		(0.484)
Observations	12,833	12,833
Pseudo R^2	0.192	0.201
Overlap elasticity	0.043	0.026

(a) Employer Overlap and Target Choice



(b) Marginal Effects of Overlap

Figure 3: Predicted Cross-Geography Merger Acquisitions

Notes: Panel (a) of Figure 3 shows the coefficients estimated from the multinomial logit model, with standard errors in parentheses, clustered by source merger. The dependent variable equals one for the chosen target hospital within a pseudo-merger choice set. Employer overlap share is measured in standard-deviation units. Distance is measured as miles to the nearest acquirer hospital, capped at 300 miles. Both regressions include hospital-type fixed effects, controls for doctors, nurses, and overall workers per bed, an indicator for medical school affiliation, an indicator for Medicare certification, a critical-access-hospital indicator, total staffed beds, and an indicator for having no acquirer within 300 miles. Overlap elasticity corresponds to the standard demand elasticity with respect to the employer overlap share. See Appendix Table A.13 for the full set of estimates. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. To interpret these estimates, Figure 3b shows the mean marginal effect of overlap on choice probabilities in percentage points. The first quartile is omitted as the estimates are statistically indistinguishable from zero and largely uninformative.

show a significant interaction between overlap and distance. The estimates indicate that as potential targets are located farther from potential acquirers, employer overlap becomes a more dominant factor in choice probability, consistent with the intuition that local hospital demand forces become negligible with distance. The implied elasticity of overlap is positive, indicating that employer overlap meaningfully influences the probability that acquirers choose the observed targets. Figure 3b presents the implied marginal effect of a one-standard-deviation increase in overlap by distance quartile, excluding the most local one in which distance is overwhelmingly the predominant force. Under the preferred linear specification, a one-standard-deviation increase in overlap in the fourth quartile of distance increases acquisition probability by 0.85 percentage points. For comparison, the average predicted choice probability in the linear specification is 2.2%. The quartile specification is nonmonotonic: the marginal effect of overlap peaks at an intermediate distance. One interpretation is that local substitution weakens with distance while the relevance of employer overlap strengthens.

This analysis offers a different perspective on the two forces that have dominated discussions of merger effects: distance and overlap. In both predicted acquisition and price effects, local and distant mergers behave differently. Local mergers play to local market concentration, consolidating power over local demand for care and leveraging that in negotiations with insurers for increased pay. The potential for greater payments from consolidation incentivizes local market concentration, which has been the historical focus of antitrust enforcement in healthcare markets. More distant mergers, however, can also have considerable merger effects by leveraging scale against the employer-sponsored insurance market. In distant mergers, employer overlap dominates the price effect and the predicted reason for acquisitions. This suggests that merger evaluations in this space require a different perspective. Employer overlap share is a simple metric for assessing a merger's potential harm, particularly in distant acquisitions.

8 Revisiting the Merger Theories

Using the evidence established thus far, we can attempt to rule out particular theories for why cross-geography mergers might be increasing prices. So far, we have shown that cross-geography mergers are associated with higher prices, even when acquirers are far away. We have shown that those price increases are associated with the degree to which employers overlap merging markets, consistent with the theory of Section 2. However, because we lack data to determine whether employees hired in one market are substitutes or complements in their employer's production, we are unable to fully test our main microfoundation for submodularity, as described in Section 2.[37] Thus, we are left with the possibility of alternative theories that might coexist with an employer overlap channel. This section aims to provide evidence for or against some of these key alternatives.

The first, simplest alternative is that acquiring systems force a common price across all their hospitals. For example, systems might make take-it-or-leave-it offers to insurers, offering them a single price in exchange for access. However, the price variance across geographies for acquiring systems is sufficiently large to rule out a uniform price, as we discuss further in Appendix A.6. Nevertheless, this lack of uniformity might be driven by contracting frictions that resolve over time, yielding a price path that slowly but surely converges to a single price. To test this, we adapt our main regression in Equation (5) and replace the outcome variable with the squared difference between a hospital's log price and the acquiring system's average log price for the given merger event. That is, we compute

$$\Delta^2 \ln(p_{het}) \equiv \left(\ln(p_{het}) - \frac{1}{|\mathcal{H}_e^{acquirers}|} \sum_{h' \in \mathcal{H}_e^{acquirers}} \ln(p_{h' Y(e,t)}) \right)^2 \tag{12}$$

where $\mathcal{H}_e^{acquirers}$ is the set of hospitals that acquired the target hospital, which defines event e, and $Y(e,t)$ is the year corresponding to t years after the merger event e. That is, $\Delta^2 \ln(p_{het})$ measures the squared gap between a hospital's log-negotiated price and the average log-negotiated price across all acquiring hospitals. If acquired hospital prices converge to those of

[37] Moreover, to our knowledge, the data needed to fully test this do not exist. It would require, at a minimum, detailed data on workers' occupations linked to firms, their insurance information, and information about their medical demand, together with sufficient identifiers to link with hospital mergers. Privacy and patient protection regulations in healthcare markets make the formation of such a dataset unlikely.

their acquirers, we would expect the treatment effect to be negative and large enough to close the pre-acquisition gap. Columns one to three of Table 9 show small but statistically significant effects, consistent with our findings that both acquirer and target prices increase post-merger relative to control hospitals. However, the effect is far too small, representing only 7.0% of the pre-acquisition squared price gap. Thus, targets do not appear to be converging to their acquirers' prices post-merger.

An alternative hypothesis is that, despite the distance, there remains an important pool of shared demand between the merging hospitals. Good connectivity between the merging geographies or linked economic sectors might explain both the employer overlap and some shared hospital demand. If patients who travel longer distances are particularly lucrative, then even a small pool of common consumers could generate the same effects observed for local mergers, even across large distances. Appendix Table A.14 compares the distribution of spending between patients who travel more than 66 miles (the last quartile cut in our distance analysis in Section 4.3) and those who do not. On average, far-traveling patients spend about 60.0% more on care but account for only 2.4% of overall demand. Despite the small volume, the higher revenue from these patients might help explain the merger effects and would be consistent with a theory of common patient demand. If the merging parties can channel demand towards the acquired hospital, the hospital's importance to the insurer's enrollee population might increase with the acquisition, thereby explaining the higher prices.

We test this by exploring whether acquired hospitals experience an increase in admissions or revenue from distant patients. In particular, we divide target hospitals into quartiles by distance to their nearest acquirer and ask whether demand or revenue from patients residing beyond the quartile thresholds changed relative to control hospitals.[38] We focus here exclusively on the fourth quartile of merger distance, where we know that price effects exist when overlap is present, and present the full set of estimates in Appendix Tables A.15 and A.16. Column four of Table 9 shows that for hospitals targeted by acquirers 66 miles or more away, demand from patients at 66 miles or more slightly declined relative to controls, by about one admission per year. Column five shows a positive change in demand exactly where we observe price increases. However, the mean overlap is 0.003 in this quartile, with a standard deviation of 0.005, which implies that a merger event one standard deviation above the mean in overlap generates about 0.61 additional admissions per year from distant patients. Column six shows negligible effects on distant revenue. Finally, column seven shows that target hospitals with overlap one standard deviation above the mean would experience an increase of about $35,532 per year in revenue from distant patients, which combines the small gain in demand with a relatively large increase in prices. Thus, given the magnitude of the changes, it is unlikely that these few additional patients account for the merger effects; it appears implausible that the insurer would be willing to accept significantly higher prices for all admissions at the acquired

[38] We note that we observe almost no patient transfers between acquiring and target hospitals, with the volume falling to an exact zero rapidly as distance grows. We also observe almost no demand flowing from the acquirers' HSAs or 15-mile radii to targets, especially at long distances. We therefore test the common-demand hypothesis with a broad radius rather than with market-to-market flow patterns, which are almost entirely negligible in the data.

Table 9: Alternative Merger Hypotheses

	Convergence			Distant Demand		Distant Revenue	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × Treated	-0.059**	-0.060**	-0.063**	-0.943*	-2.344***	0.523	-2.201
	(0.027)	(0.027)	(0.027)	(0.566)	(0.671)	(1.622)	(1.666)
↪ × Overlap					369.840***		719.275**
					(104.028)		(326.267)
Bed Controls		✓	✓				
Staffing Controls			✓				
Mean dep. var.	0.897	0.897	0.897	4.944	4.944	13.696	13.696
Observations	359,556	359,556	359,556	98,030	97,783	98,030	97,783
R^2	0.632	0.632	0.632	0.893	0.892	0.892	0.892

Notes: Convergence measures the squared difference between a hospital's log negotiated price and the acquirer's average log negotiated price. Distant demand and revenue correspond to those from the fourth distance quartile, measured in patient counts and tens of thousands of dollars, respectively. Tables for revenue and demand from patients of different distances are provided in Appendix Tables A.15 and A.16. Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions include hospital-event and year-event fixed effects matching the design of Equation (5). The mean dependent variable is measured in the year before the merger event. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

hospital, given the leverage the hospital gains from fewer than one additional admission per year.

An alternative theory that might yield merger effects is one of tying, as developed in Section 2. This theory requires an overlapping employer, as in the labor substitution hypothesis, but operates by reshaping exclusion threats in the target market by leveraging the acquirer's dominant position in another market. This hypothesis is particularly compelling because many acquirers in our sample are near-monopolists in their local markets, and extensive evidence supports narrow networks (Ho and Lee, 2019; Ghili, 2022; Kleiner et al., 2026). We conduct a series of tests to assess this hypothesis. First, we test whether acquirers that have a more dominant position in their local markets obtain higher prices at target hospitals. Appendix Table A.17 shows that targets acquired by systems containing at least one local monopolist experience a positive but statistically insignificant additional post-acquisition price increase. However, using the maximum HHI the acquirer has among its local markets yields a positive and significant association with prices post-acquisition. To further test this connection between acquirer dominance and price effects, we measure the weighted average of the acquirer's HHI, weighted by the share of the insurer's overall employee population found in each market. This accounts for the fact that acquirers that are particularly dominant in their local markets also tend to be located in very small markets, which would be of limited relevance to the insurer. Using this market-size-adjusted metric, we find a statistically significant effect of acquirer dominance on post-acquisition prices: a standard-deviation increase in the weighted HHI mean of the acquiring party increases post-acquisition prices at targets by about 4.7%.

The alternative theory also stipulates that effects would materialize in markets where the target could face a threat of replacement. To examine this, we determine whether a comparable out-of-network hospital exists in the target's local market, treating the target as in a narrow (local) network when such a hospital exists. We use the hospital specialty type (if any) and divide hospitals into those above and below the median number of beds by their type and year to assess comparability. Appendix Table A.18 shows the estimated effect, separating it by the acquirer's dominance to match the theory. Consistent with the theory of Section 2, we find no effect on prices when the acquirer's market-size-adjusted HHI is below the median. However, price changes are large and significant both when the target is not in a narrow network arrangement (6.9%) and when it is (10.5%). Moreover, when the acquirer is dominant in its local markets and the target is in a narrow network, the merger effect loads strongly on employer overlap. These two observations run counter to elements of our tying theory, suggesting that it cannot fully explain the observed price changes. However, the finding that dominant acquirers and targets in narrow networks see larger price increases following mergers presents a relevant degree of heterogeneity for our main theory. We note that acquirer dominance effects can be easily incorporated into the labor substitution theory, as a more dominant acquirer increases the employer's losses from not offering health benefits in those markets. The effect of narrow networks, however, falls outside that model and thus suggests that mergers' effects on threats of replacement might further increase price effects from acquisitions.[39]

Finally, some theories stipulate that merger effects are driven by increases in bargaining sophistication (Lewis and Pflum, 2017). From a purely theoretical perspective, within the standard Nash-in-Nash framework, this hypothesis cannot be tested without further assumptions or additional data. Bargaining parameters operate largely as a residual, which, if allowed to vary freely over time, can explain almost any variation in prices. However, our evidence points against certain types of changes in bargaining power. First, it does not align with effects being driven exclusively by targets obtaining the bargaining power of their acquirers, because acquirers also experience price increases, as documented in Appendix A.5. Second, the evidence also does not appear consistent with greater sophistication arising when targets convert from nonprofit to for-profit status; Appendix Table A.20 shows that for-profit cross-geography targets also experience price effects. Finally, unless bargaining sophistication changes happen to scale with employer overlap for some unspecified reason, it would be difficult for such a model to reproduce the evidence in Section 4.2.

Overall, the evidence clearly points to a role for employer markets but leaves room for a potential channel of replacement threats. Accommodating these threats within the main framework is feasible and would tend to predict higher price effects. However, the evidence above does not support replacement threats as the sole channel for cross-geography price effects, since these are also measured when local networks are not narrow and employer overlap is present. The limited impact of distant demand on revenue and the large distances between targets and acquirers in our sample suggest a limited role for common demand in explaining the effect. The lack of price uniformity or meaningful convergence to a common system price suggests

[39] Appendix Table A.19 shows that these findings are robust to using hospital referral regions (HRR) as the local market definition for computing HHI and narrow networks, rather than HSAs.

that single-system contracting hypotheses are unlikely to explain the data well. In Section 9.2 below, we discuss one additional theory based on network adequacy standards, which yields an alternative way to measure employer overlap but which also does not appear to present a consistent theory for cross-geography effects.

9 Robustness

In this section, we present some additional evidence on the robustness of our findings. We start with the key challenge of local market definitions, then explore alternative overlap metrics, price measures, and outpatient price data.

9.1 Local Market Definition

The key assumption underlying our main analysis is that Health Service Areas (HSAs) appropriately define local healthcare markets. The market definition determines which mergers we classify as cross-geography, which acquired hospitals are free from the effects of recent local mergers, and, similarly, which non-acquired hospitals can serve as controls. The evidence in Section 4.3 has already shown that our findings persist even when the target and acquirer are far apart, especially when employer overlap is substantial. However, that evidence does not speak to the effect of local market definitions on the sample under analysis.

To highlight the robustness and limitations of our approach, we repeat our sample construction strategy, defining the local market as a 15-mile radius around the acquired hospital, the hospital's county, the hospital's Hospital Referral Region (HRR), or the hospital's entire state.[40] We focus on the price effects, as they are the leading evidence and source of concern for competition authorities. Panel A of Table 10 reports estimates from Equation (5), while Panel B reports estimates from Equation (7).

The results are broadly consistent with our main analysis. Throughout, we find a positive and statistically significant effect of mergers on prices across all four geographic definitions. In addition, for all definitions, we find that employer overlap is associated with a positive and significant post-acquisition price increase. The within-state overlap coefficients are similar in sign and significance and range from 8.5 to 10.6, compared with 8.7 under the HSA definition. Comparing sample sizes to our main analysis, we observe that the 15-mile radius includes more merger events than our HSA-based definition, likely introducing small, nearly local mergers of negligible effect. In contrast, the HRR-based market definition reduces the sample by 58.4%, vastly limiting the set of mergers that inform the estimated price effects. We note that the theoretically correct notion of a local healthcare market should be based on demand patterns, as it should capture the immediate substitutes of the acquired or acquiring hospitals. Because HSAs and HRRs were developed from demand patterns, they are likely better suited to this

[40]When considering the state as the local market, we focus on control hospitals located in states contiguous to the treated hospital's state. In this analysis, we remove the requirement that the acquirer and target be present in the same state. To keep the analysis simple, we do not impose further restrictions on the control-matching strategy, resulting in an enormous number of control units per event, which explains the large sample size shown in the table.

Table 10: Merger Effects on Log Negotiated Prices by Geographic Definition

	Log Negotiated Price							
	Panel A: Baseline				Panel B: With Overlap			
	15 Miles (1)	County (2)	HRR (3)	State (4)	15 Miles (5)	County (6)	HRR (7)	State (8)
Post × Treated	0.025**	0.031**	0.044***	0.037***	0.001	0.005	0.011	0.026***
	(0.011)	(0.012)	(0.014)	(0.009)	(0.013)	(0.013)	(0.016)	(0.009)
Post × Treated × Overlap					8.539***	9.915***	10.612***	5.612***
					(2.319)	(2.150)	(2.679)	(0.901)
Mean dep. var.	-0.604	-0.623	-0.599	-0.593	-0.605	-0.623	-0.599	-0.593
Mean overlap	-	-	-	-	0.003	0.003	0.003	0.002
SD overlap	-	-	-	-	0.004	0.004	0.004	0.003
Observations	396,664	316,170	154,201	10,005,123	394,725	315,273	153,734	9,996,780
R^2	0.798	0.794	0.808	0.805	0.778	0.786	0.800	0.797

Notes: Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions include hospital-event and year-event fixed effects. The mean dependent variable is measured in the year before the merger event. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

purpose. The fact that our strategy appears consistent for both market definitions gives us confidence in the evidence we have uncovered.

9.2 Overlap Measurement

In our main analysis, we measured employers' exposure to a merger using our employer overlap share metric. This metric is simple, but it does not meaningfully capture how important a given hospital is to a patient population. Thus, whether an acquirer purchases one out of many local hospitals or acquires the only hospital within the local market, the overlap metric will measure the same amount. In Appendix A.7, we present an alternative approach based on network adequacy. There, we use the Medicare Advantage guidelines as a pre-specified criterion to evaluate whether a provider network is adequate for a population, and to determine whether our data provider's network is adequate for any given employer in the data. Using these guidelines, we define adequacy leverage as the share of the insurer's covered employees whose employer's network changes from adequate to inadequate when the merged system is removed. The measure is computed in the year before the merger. In the appendix, we show that a merged system's leverage on network adequacy is positively associated with post-merger price increases and shares many of the same patterns as our employer overlap metric.

In addition, we compute how the merged system's leverage on network adequacy changes relative to the sum of its merging parties' leverage. Theoretically, if employers make enrollment decisions based on network adequacy, this differenced measure will dictate the merged system's additional leverage relative to that of its merging parties, and thus the price effect of the merger. That is, this change in adequacy captures how much additional leverage the merger generated. In the appendix, we show that this measure is also positively associated with post-merger price effects and yields an estimated effect similar in magnitude to that of our employer overlap metric. However, the results show that the effect operates in a single direction: systems that lose leverage do not seem to obtain lower prices post-merger. Thus, the evidence does not appear consistent with network adequacy rules—at least those determined for Medicare Advantage—being the governing theory for merger effects. Nevertheless, the measured change in adequacy leverage presents an alternative approach for measuring the connection between mergers and employer footprint. However, we see employer overlap as the simpler metric for evaluation, since network adequacy requires broader data on each insurer's provider network and is more challenging to compute.

9.3 Robustness to Changes in Units of Care

The analysis above focused on inpatient care prices paid per admission, normalized by care type and patient characteristics. However, hospitals might respond to a merger by providing more or less care within a given inpatient admission, which could make prices incomparable across time, even within a hospital and condition group. Appendix Table A.21 shows the estimated effects on unit-adjusted inpatient price indices and on outpatient prices, as estimated in Equation (7). Unit-adjusted inpatient prices account for the units of care delivered to each patient, as well as patient characteristics and diagnoses. They are constructed using the same procedure as for negotiated prices, as described in Appendix A.4.4, but replacing the overall expected payments to the hospital (insurer payments plus patient obligations) with the ratio of these payments to the units of care delivered. The estimates in the table indicate that unit-adjusted prices follow the same pattern as negotiated prices, increasing by about 3.9% post-acquisition. As in our main results, the association between employer overlap and prices following acquisition is large and significant, with the residual baseline term being marginally significant at 2.3%. We see this evidence as supportive of the conclusion that our findings are not driven by how negotiated prices were constructed, but rather reflect changes to the underlying high-dimensional service prices.

9.4 Outpatient Prices

Our analysis of price effects has focused exclusively on inpatient prices. However, hospitals might change outpatient prices in ways that offset or augment the effects on inpatient prices. For example, the acquiring system might reach an agreement with the insurer for higher inpatient prices in exchange for lower outpatient prices. Such price redistribution might harm some patients and benefit others, posing a new challenge for regulatory authorities to discern the harms of cross-geography mergers.

Appendix Table A.21 shows two outpatient price indices, one constructed using Ambulatory Payment Classification (APC) codes and one using payment per Relative Value Unit (RVU). The APC-based approach is identical to our construction of negotiated inpatient prices, replacing inpatient DRG codes with outpatient APC codes. However, about 20% of outpatient visits contain no APC code. RVUs, in contrast, are available for almost all outpatient procedures and provide a single index that operates similarly to units of care. Using this, we compute the price paid per outpatient visit per RVU and use these prices in our analysis. Under both variants, we find substantial price increases following acquisitions. APC-based prices increase by 6%, while the price per RVU increases by about 8.2%. Moreover, as with inpatient prices, outpatient price increases appear strongly associated with employer overlap. However, the fraction of the merger effect on outpatient prices that is explained by a linear overlap interaction is smaller than for inpatient prices. This is perhaps because our data are not optimized to study effects on outpatient care. In particular, our merger panel does not include acquisitions of ambulatory care centers that might compete with the inpatient-care hospitals under examination or their control units.[41] Thus, whether employer overlap would play a different role in a more comprehensive analysis of the effect of mergers on outpatient care remains an important open question.

10 Conclusions

This article studies the effects of cross-geography hospital acquisitions. These acquisitions account for over half of the merger volume yet remain largely unchallenged by regulatory authorities. Using data from a large national health insurance company, we provide new evidence that these hospital mergers increased prices, decreased costs per admission and full-time-equivalent nurses and doctors per licensed bed, and reduced overall admissions at acquired hospitals. Our data allow us to test a longstanding hypothesis that these merger effects are driven by the employer-insurance market, which spans broader geographies than local hospital care markets. We develop a simple yet effective metric of employer overlap that measures the extent to which an insurer's employer population is exposed to cross-geography mergers. We show that this metric is closely linked to post-acquisition price increases, providing strong support for the employer-overlap hypothesis.

Our findings guide future enforcement and research on cross-geography hospital mergers. For hospital analyses, our results provide a clear way to measure potential harm from distant mergers via the presence of overlapping employers. The metric can be readily computed from either employer-level data or the enrollment records of a single large insurer, as in our case. As we note in our results, high employer overlap appears to predict meaningful price increases at any distance between targets and acquirers.

Our results speak to broader research on cross-market mergers in pharmaceutical, financial, and education markets. When consumers purchase products or services via demand aggregators (e.g., procurement platforms), adding layers of intermediation can create merger effects that did not exist before. Merging service providers can threaten intermediaries' ability to sell to demand aggregators, leading to higher prices and stronger incentives to merge. While healthcare markets are prone to these effects because of multiple layers of intermediation and localized geographies of care, it seems plausible that similar effects exist in other intermediated markets.

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[41] To our knowledge, no comprehensive dataset tracks mergers and acquisitions of outpatient care centers.

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A Appendix

A.1 Additional Figures and Tables

Table A.1: Descriptive Statistics of Merger Hospitals Observed in Private Insurance Data

	Within-HSA		Cross-HSA	
	Target	Acquirer	Target	Acquirer
Hospitals involved in merger	151	622	1,070	10,505
↪ coverage	66.5%	80.2%	71.9%	76.6%
Systems involved in merger	58	143	395	275
↪ coverage	60.4%	67.8%	67.2%	83.6%
# Affected Markets	85	315	752	1,452
↪ coverage	68.5%	81.6%	78.3%	83.7%
Average Bed Size	93.841	212.017	131.235	147.385
Average System Members	34.761	40.711	16.840	23.141
Share Stand-Alone	0.418	0.092	0.536	0.022
Share Rural	0.062	0.127	0.280	0.207
Share Critical Access	0.046	0.063	0.141	0.116
Share General Care	0.556	0.768	0.802	0.672
Share For-Profit	0.512	0.393	0.300	0.551
Average Nurses/Bed	0.111	0.117	0.128	0.119
Average Doctors/Bed	0.105	0.089	0.105	0.078
Average Occupancy Rate	0.485	0.592	0.523	0.577
Average log price pre-merger	-0.269	-0.141	-0.352	-0.163
Average log price post-merger	-0.178	-0.016	-0.240	-0.059
Miles to nearest counterpart				
25th Percentile	2.040	2.346	15.847	15.847
Median	5.247	6.246	28.489	29.745
75th Percentile	11.582	14.653	59.945	68.010

Notes: This table presents descriptive statistics for the subsample of hospitals with private insurance price data, grouped by their merger status for the years 2011–2020. The number of systems and system members, as well as stand-alone and for-profit status, are computed based on the status of the merging hospitals in the year prior to the merger. Negotiated prices are relative to a vaginal delivery for a woman aged 31 to 35, the level of which is kept masked for confidentiality reasons. Pre-merger prices are in the year prior to the merger, while post-merger prices are in the year after the merger.

Table A.2: Hospitals Involved in Mergers

	Within-Geography (HSA)		Cross-Geography (HSA)	
	Target	Acquirer	Target	Acquirer
Full panel	227	776	1,489	13,710
Full panel with prices	151	622	1,070	10,505
↪ coverage	(66.5%)	(80.2%)	(71.9%)	(76.6%)
Event-study panel	97	372	710	3,075
↪ coverage	(64.2%)	(59.8%)	(66.4%)	(29.3%)

Notes: This table summarizes sample coverage across merger-hospital samples. The full panel includes all merger hospitals in the original merger data. The full panel with prices restricts the sample to merger hospitals observed in the insurance claims-based price data. The event-study panel further restricts the sample to hospitals included in the stacked DiD event-study panel under the HSA market definition. Coverage for the full panel with prices is computed relative to the full panel. Coverage for the event-study panel is computed relative to the full panel with prices.

Table A.3: Baseline Characteristics of Treated and Control Hospitals

	Treated	**Control**
Average Bed Size	131.452	140.729
Average System Members	21.583	6.635
Share Stand-Alone	0.439	0.681
Share Rural	0.330	0.391
Share Critical Access	0.161	0.252
Share General Care	0.831	0.768
Share For-Profit	0.317	0.146
Average Nurses/Bed	0.155	0.253
Average Doctors/Bed	0.096	0.150
Average Occupancy Rate	0.535	0.528
Hospital-event observations	797	69,565

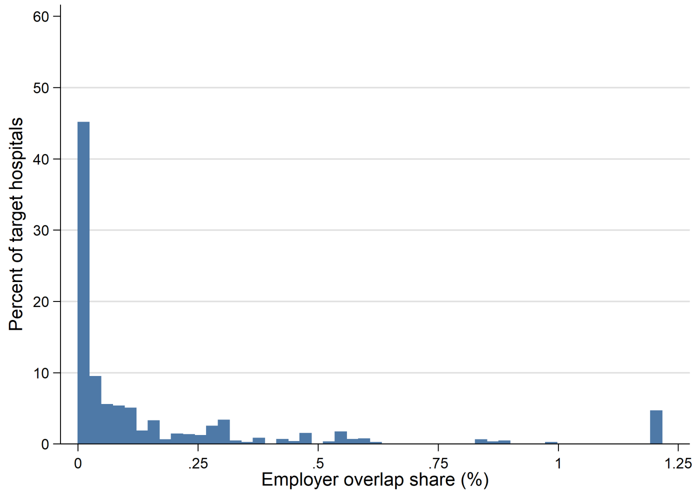
Notes: This table presents baseline characteristics of treated and control hospitals in the cross-HSA merger event-study panel. The sample includes all hospitals passing the filters for inclusion in our stacked and matched panel but is not restricted to hospitals with prices available in our private insurance data. Hospital characteristics are measured in the year prior to the merger event ($t = -1$). Treated hospitals are hospitals acquired in a cross-HSA merger, while control hospitals are the corresponding hospitals in the stacked event-study comparison groups.

Table A.4: Annual Negotiated Price Growth for Control Hospitals (Cross-HSA)

Event-Time Transition	Mean $\Delta \log p$	Mean log growth (%)	SE($\Delta \log p$)	SD($\Delta \log p$)	N
$-5 \rightarrow -4$	0.0322	3.2229	0.0049	0.5247	11,543
$-4 \rightarrow -3$	0.0498	4.9850	0.0041	0.5252	16,811
$-3 \rightarrow -2$	0.0346	3.4570	0.0034	0.5134	23,029
$-2 \rightarrow -1$	0.0273	2.7291	0.0032	0.5371	27,582
$-1 \rightarrow 0$	0.0387	3.8669	0.0030	0.5316	31,401
$0 \rightarrow 1$	0.0315	3.1475	0.0029	0.5438	35,222
$1 \rightarrow 2$	0.0330	3.2963	0.0026	0.5423	43,174
$2 \rightarrow 3$	0.0227	2.2735	0.0027	0.5305	39,742
$3 \rightarrow 4$	0.0330	3.2998	0.0027	0.5274	38,617
$4 \rightarrow 5$	0.0333	3.3252	0.0029	0.5372	33,922
Overall average	0.0326	3.2589	0.0010	0.5331	301,043

Notes: This table reports annual negotiated-price growth among control hospitals in the cross-HSA merger event-study panel. Each observation is a hospital-event pair observed in two consecutive event years. $\Delta \log p$ is the within-hospital-event change in log negotiated price. Mean log growth (%) equals $100 \times E[\Delta \log p]$.

Figure A.1: Distribution of Employer Overlap in Cross-HSA Mergers



Notes: This figure reports the distribution of employer overlap shares for target hospitals involved in cross-HSA mergers. Employer overlap is measured in the year prior to the merger. For each employer, exposure is the smaller of its covered employee counts in the acquirer and target market groups; employer overlap sums those minima across employers and divides by the insurer's total covered population. The histogram is constructed at the target-hospital level using 50 bins, trimming observations outside the 1st and 99th percentiles of the overlap distribution. Employer overlap is multiplied by 100 in this figure and shown in percentage points.

Table A.5: Summary Statistics on Distance to Acquirer by Quartile

	Min	Mean	Max
Q1	0.9330	11.5709	18.4500
Q2	18.4863	25.1291	33.9022
Q3	34.0635	45.9332	65.5544
Q4	66.0738	132.0057	669.9901

Notes: This table reports summary statistics for the distance cutoffs used in Figure 2.

Table A.6: Distribution of Employer Overlap by Merger Distance

	Distance between target and acquirer hospitals				
	0–15 miles	15–30 miles	30–45 miles	45–60 miles	60+ miles
Panel A: All mergers					
N	101	156	107	54	163
Mean	0.002	0.002	0.002	0.003	0.003
SD	0.004	0.004	0.004	0.005	0.004
p25	0.0001	0.00004	0.00004	0.00002	0.0001
p50	0.0004	0.0002	0.0002	0.0003	0.0005
p75	0.001	0.001	0.002	0.003	0.003
Panel B: Above-median employer overlap					
N	51	79	55	27	82
Mean	0.004	0.003	0.004	0.006	0.006
SD	0.005	0.005	0.005	0.005	0.005
p25	0.0007	0.0004	0.0003	0.0006	0.002
p50	0.001	0.001	0.002	0.003	0.003
p75	0.005	0.006	0.006	0.012	0.012
Panel C: Below-median employer overlap					
N	50	77	52	27	81
Mean	0.0001	0.00006	0.00004	0.00006	0.0002
SD	0.0001	0.00006	0.00004	0.00007	0.0002
p25	0.00003	0.00001	0.00001	0.00001	0.00005
p50	0.0001	0.00004	0.00003	0.00002	0.0001
p75	0.0002	0.00008	0.00007	0.00009	0.0003

Notes: This table reports summary statistics for employer overlap across merger-distance bins. Distance is measured between the target and acquirer hospitals. Employer overlap is reported on the 0–1 share scale; for example, 0.002 equals 0.2% of the insurer's covered population. Above- and below-median employer overlap are defined using the median employer overlap for each distance bin.

Table A.7: Cross-Geography Merger Effects on Prices (Binary Heterogeneity)

	Log Negotiated Price			
	Largest Systems	Rural	Critical Access	Low Income
	(1)	(2)	(3)	(4)
Post × Treated	0.018	0.040***	0.045***	0.036**
	(0.013)	(0.014)	(0.013)	(0.015)
$\hookrightarrow \times H$	0.008	-0.046**	-0.130***	-0.031
	(0.043)	(0.023)	(0.031)	(0.023)
$\hookrightarrow \times$ Overlap $\times(1-H)$	18.347***	9.592***	6.872***	11.076***
	(2.409)	(2.042)	(2.058)	(2.225)
$\hookrightarrow \times$ Overlap $\times H$	3.308	3.755	15.532**	6.015**
	(4.083)	(3.352)	(6.113)	(2.666)
Mean dep. var.	-0.592	-0.592	-0.592	-0.592
Mean H pre-merger	0.003	0.439	0.297	0.400
Observations	358,919	352,414	369,793	352,287
R^2	0.789	0.789	0.790	0.790

Notes: Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions include hospital-event and year-event fixed effects. Heterogeneity variables vary by column as indicated. Largest Systems is an indicator for the 5 largest systems in our sample, defined by the average number of hospitals over time. Rural and Critical Access are hospital-level indicators for whether the hospital is in a rural area or designated as a critical access hospital. "Low Income" is a county-level indicator for whether the hospital's county has a lower median household income than the sample median.

The specifications are reparameterizations of the corresponding binary heterogeneity specifications in Table 6. Post × Treated × Overlap $\times(1-H)$ reports the effect of employer overlap for hospitals for which the listed heterogeneity indicator equals zero, while Post × Treated × Overlap $\times H$ reports the effect for hospitals for which the indicator equals one. Thus, the difference between the two overlap coefficients corresponds to the Post × Treated × Overlap $\times H$ interaction reported in Table 6. The mean dependent variable is measured in the year before the merger.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Heterogeneity in Effects on Staffing and Capacity

	HHI			Rural		
Outcome	Nurses (1)	Doctors (2)	Staffed Beds (3)	Nurses (4)	Doctors (5)	Staffed Beds (6)
Post × Treated	-0.039***	-0.041***	0.812	-0.033***	-0.030***	-3.648***
	(0.008)	(0.009)	(2.031)	(0.004)	(0.006)	(1.225)
Post × Treated × H	0.012	0.012	-6.557***	0.011	-0.005	-1.166
	(0.009)	(0.009)	(2.473)	(0.008)	(0.006)	(1.683)
Mean dep. var.	0.252	0.150	140.623	0.252	0.150	140.099
Mean H	0.675	0.675	0.675	0.390	0.390	0.390
Observations	680,934	680,934	681,006	630,992	630,992	631,064
R^2	0.742	0.859	0.972	0.745	0.858	0.971
	Critical Access			**Low Income**		
Outcome	Nurses (1)	Doctors (2)	Staffed Beds (3)	Nurses (4)	Doctors (5)	Staffed Beds (6)
Post × Treated	-0.030***	-0.028***	-4.516***	-0.026***	-0.033***	-3.530***
	(0.004)	(0.005)	(1.131)	(0.005)	(0.006)	(1.214)
Post × Treated × H	0.003	-0.022**	1.739	-0.008	0.004	-1.446
	(0.012)	(0.009)	(1.279)	(0.007)	(0.006)	(1.734)
Mean dep. var.	0.252	0.150	140.623	0.252	0.150	140.095
Mean H	0.251	0.251	0.251	0.389	0.389	0.389
Observations	680,934	680,934	681,006	630,878	630,878	630,950
R^2	0.742	0.859	0.972	0.745	0.858	0.971

Notes: The table reports heterogeneity in merger effects on healthcare production. The dependent variables are full-time equivalent nurses per bed, full-time equivalent doctors per bed, and total facility beds set up and staffed. H denotes the heterogeneity measure listed at the top of each column. All regressions include hospital-event and year-event fixed effects. HHI is computed using hospital admission shares at the HSA level. Rural and Low Income are county-level indicators; Critical Access is a hospital-level indicator. The mean dependent variable and mean H are measured in the year before the merger event. Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Merger Effects on Local Healthcare Labor Markets

	Log Wages					
	Internist Wage		LPN Wage		OB/GYN Wage	
	Baseline	Overlap	Baseline	Overlap	Baseline	Overlap
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated	-0.002	-0.008	0.000	-0.001	-0.002	-0.015
	(0.011)	(0.015)	(0.002)	(0.002)	(0.010)	(0.013)
↪ × Overlap		2.562		0.113		4.057**
		(2.422)		(0.298)		(1.724)
Mean dep. var.	12.147	12.147	10.684	10.684	12.228	12.229
Observations	211,708	210,939	317,053	315,917	170,836	170,238
R^2	0.856	0.847	0.979	0.977	0.872	0.864

	Log Wages					
	PCP Wage		PT Wage		RN Wage	
	Baseline	Overlap	Baseline	Overlap	Baseline	Overlap
	(7)	(8)	(9)	(10)	(11)	(12)
Post × Treated	0.010	0.006	0.003	-0.002	0.002	0.001
	(0.007)	(0.008)	(0.004)	(0.005)	(0.002)	(0.002)
↪ × Overlap		1.651		1.237*		0.471
		(1.452)		(0.648)		(0.374)
Mean dep. var.	12.108	12.108	11.326	11.326	11.125	11.126
Observations	287,606	286,555	313,548	312,426	314,499	313,388
R^2	0.798	0.786	0.872	0.857	0.978	0.975

Notes: We exclude control hospitals found in the same county as target hospitals for these regressions. This is done to avoid mechanically biasing the estimates towards zero by using the same wage for treated and control hospitals. Standard errors in parentheses, clustered by BLS Occupational Employment and Wage Statistics (OEWS) wage area (CBSA). All regressions include hospital-event and year-event fixed effects. Overlap columns additionally include the interaction between Post × Treated and overlap. The mean dependent variable is measured in the year before the merger. Regressions use analytic event-stack weights following Wing et al. (2024). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Merger Effects on Production and Costs by Median Distance

	Healthcare Production and Costs							
	Log Staffed Beds		Log Nurses per Bed		Log Doctors per Bed		Log Cost per Admission	
	Below (1)	Above (2)	Below (3)	Above (4)	Below (5)	Above (6)	Below (7)	Above (8)
Post × Treated	-0.127***	-0.055**	-0.036	-0.027	-0.115**	0.141***	0.020	0.035
	(0.021)	(0.025)	(0.042)	(0.041)	(0.048)	(0.054)	(0.025)	(0.033)
↪ × Overlap Q2	0.064**	0.020	-0.187***	-0.057	0.073	0.001	-0.072**	-0.087**
	(0.026)	(0.031)	(0.060)	(0.062)	(0.064)	(0.074)	(0.030)	(0.036)
↪ × Overlap Q3	0.135***	0.011	-0.110*	-0.015	0.187***	0.031	-0.076***	-0.215***
	(0.025)	(0.030)	(0.059)	(0.057)	(0.070)	(0.087)	(0.029)	(0.053)
↪ × Overlap Q4	0.049	0.091***	-0.155**	-0.304***	0.048	-0.268***	-0.044	-0.177***
	(0.030)	(0.031)	(0.062)	(0.054)	(0.096)	(0.070)	(0.029)	(0.039)
Mean dep. var.	4.418	4.179	-1.955	-1.777	-2.276	-2.296	10.223	10.247
Observations	340,829	338,139	320,286	314,449	244,285	217,771	305,360	304,045
R^2	0.972	0.973	0.841	0.850	0.853	0.858	0.863	0.794

Notes: Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions include hospital-event and year-event fixed effects. Columns split the sample by whether the treated hospital's distance to the acquirer is below or above the sample median. The omitted category for overlap quartiles is Q1. The mean dependent variable is measured in the year before the merger.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Effects on Markup

	Markups							
	Base Regressions				**Heterogeneity**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
H					HHI	Rural	Critical Acc.	Low Income
Post × Treated	0.038***	0.036***	0.034***	0.034***	0.064***	0.057***	0.049***	0.038***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.010)	(0.005)	(0.005)	(0.005)
Post × Treated ×H					-0.033***	-0.053***	-0.069***	-0.001
					(0.012)	(0.007)	(0.006)	(0.007)
Mean Markup	-0.708	-0.708	-0.708	-0.708	-0.708	-0.708	-0.708	-0.708
Mean H					0.715	0.459	0.302	0.415
Bed Controls		✓	✓	✓				
Staffing Controls			✓	✓				
Service Controls				✓				
Observations	337,960	308,095	308,095	308,095	337,960	321,550	337,960	321,447
R^2	0.894	0.904	0.904	0.904	0.894	0.893	0.894	0.893

Notes: The dependent variable is markup. Columns (1)–(4) report base specifications with progressively added controls. Columns (5)–(8) report heterogeneity specifications, where H is listed at the top of each column. Bed controls account for total staffed beds, obstetric beds, and pediatric beds. Staffing controls include full-time equivalent doctors, nurses, and workers per bed, as well as total payroll costs. Service controls additionally include access to a CT scanner, an MRI machine, an emergency room, hemodialysis, mammography, oncology, surgical, and psychiatric services. All regressions include hospital-event and year-event fixed effects. Markups are trimmed at the 2.5th and 97.5th percentiles. The mean markup is measured in the year before the merger event. Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Merger Effects on Network Exclusion

	Dropped From Network	
	(1)	(2)
Post × Treated	-0.032***	-0.030***
	(0.005)	(0.006)
Post × Treated × Overlap		-0.459
		(0.824)
Share dropped	0.053	0.053
Mean overlap	–	0.002
SD overlap	–	0.004
Observations	330,037	330,037
R^2	0.499	0.499

Notes: Column (1) estimates Equation (5); column (2) estimates Equation (7), with hospital exclusion from the network as the outcome. Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions include hospital-event and year-event fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.13: Predicted Acquisition Choice Estimates

	Chosen Target Hospital	
	(1)	(2)
Employer overlap	-0.889	
	(0.606)	
$\hookrightarrow \times$ distance	0.015***	
	(0.005)	
$\hookrightarrow \times$ distance Q1		-0.966
		(0.719)
$\hookrightarrow \times$ distance Q2		1.609*
		(0.880)
$\hookrightarrow \times$ distance Q3		0.743
		(1.187)
$\hookrightarrow \times$ distance Q4		1.666***
		(0.484)
Distance to acquirer	-0.026***	-0.018***
	(0.003)	(0.006)
Distance Q1		0.532
		(1.074)
Distance Q2		-0.354
		(0.895)
Distance Q3		-0.605
		(0.650)
No acquirer within 300 miles	3.671***	1.993*
	(1.275)	(1.122)
Critical access hospital	-0.765***	-0.744***
	(0.195)	(0.195)
Medicare certified	-0.127	-0.172
	(0.773)	(0.767)
Medical school affiliation	-0.044	-0.055
	(0.183)	(0.185)
Total staffed beds	-0.001*	-0.001
	(0.001)	(0.001)
Workers per bed	0.002	0.001
	(0.026)	(0.026)
Nurses per bed	-1.511***	-1.541***
	(0.470)	(0.481)
Doctors per bed	-0.015	-0.019
	(0.325)	(0.333)
Observations	12,833	12,833
Pseudo R^2	0.192	0.201
Overlap elasticity	0.043	0.026
Hospital-type fixed effects	Yes	Yes
Merger Clusters	247	247
Choice Sets	282	282

Notes: This table reports conditional logit estimates of target-hospital choice. Standard errors in parentheses, clustered by source merger-year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.14: Distribution of Allowed Inpatient Spending by Patient-Provider Distance

Statistic	Distance ≤ 66 miles	Distance > 66 miles
Mean	17,148	27,430
Standard deviation	31,169	47,587
Minimum	4.43	6.64
25th percentile	3,879	5,337
Median	8,522	12,821
75th percentile	17,882	29,480
Maximum	1,636,098	1,606,933
Observations	8,586,826	213,540

Notes: This table reports the distribution of allowed inpatient spending, separately for patients traveling no more than 66 miles and patients traveling more than 66 miles, where 66 miles is the fourth-quartile threshold for distance between merging hospitals.

Table A.15: Effect on Total Revenue by Distance Quartiles

	Total Revenue							
	Baseline				With Overlap			
	Revenue Q1 (1)	Revenue Q2 (2)	Revenue Q3 (3)	Revenue Q4 (4)	Revenue Q1 (5)	Revenue Q2 (6)	Revenue Q3 (7)	Revenue Q4 (8)
Post × Treated	-23.617***	-6.076***	-2.220	0.523	-20.502***	-6.124***	-3.956**	-2.201
	(4.752)	(1.925)	(1.572)	(1.622)	(4.973)	(2.000)	(1.676)	(1.666)
Post × Treated × Overlap					-1235.812*	18.568	560.480**	719.275**
					(700.051)	(349.009)	(225.752)	(326.267)
Mean outcome	208.418	70.273	31.624	13.696	219.458	73.089	31.728	12.401
Mean overlap	–	–	–	–	0.002	0.003	0.003	0.003
SD overlap	–	–	–	–	0.004	0.004	0.005	0.005
Observations	341,988	251,824	172,395	98,030	340,999	251,097	171,920	97,783
R^2	0.974	0.966	0.948	0.892	0.974	0.966	0.948	0.892

Notes: Standard errors, reported in parentheses, are clustered by hospital. Revenue is computed using allowed amounts and is measured in tens of thousands of dollars. Columns Q1–Q4 restrict the sample to mergers with target-to-acquirer distances at or above the respective quartile lower bounds (0.933, 18, 34, and 66 miles) and measure revenue from patients traveling at least the corresponding distance. All regressions include hospital-event and year-event fixed effects and use analytic event-stack weights following Wing et al. (2024). Mean outcomes in the baseline columns are measured at $t = -1$, while those in the overlap columns are measured at $t = +1$; this timing convention is intentional and explains why the Q4 overlap-column mean of 12.401 differs from the $t = -1$ mean of 13.696 reported in Table 9. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.16: Effect on Total Admissions by Distance Quartiles

	Total Admissions							
	Baseline				With Overlap			
	Admissions Q1 (1)	Admissions Q2 (2)	Admissions Q3 (3)	Admissions Q4 (4)	Admissions Q1 (5)	Admissions Q2 (6)	Admissions Q3 (7)	Admissions Q4 (8)
Post × Treated	-22.013***	-1.738	-1.847***	-0.943*	-25.899***	-2.992**	-3.524***	-2.344***
	(5.018)	(1.178)	(0.678)	(0.566)	(5.645)	(1.218)	(0.781)	(0.671)
Post × Treated × Overlap					1541.051***	481.527**	541.414***	369.840***
					(554.493)	(194.687)	(100.288)	(104.028)
Mean outcome	128.516	33.698	12.933	4.944	127.362	33.270	12.590	4.269
Mean overlap	–	–	–	–	0.002	0.003	0.003	0.003
SD overlap	–	–	–	–	0.004	0.004	0.005	0.005
Observations	341,988	251,824	172,395	98,030	340,999	251,097	171,920	97,783
R^2	0.933	0.954	0.945	0.893	0.929	0.953	0.945	0.892

Notes: Standard errors, reported in parentheses, are clustered by hospital. Columns Q1–Q4 use the same target-to-acquirer merger-distance lower bounds as the corresponding revenue columns and measure admissions by patients traveling at least the corresponding distance. All regressions include hospital-event and year-event fixed effects and use analytic event-stack weights following Wing et al. (2024). Mean outcomes in the baseline columns are measured at $t = -1$, while those in the overlap columns are measured at $t = +1$; this timing convention is intentional and explains why the Q4 overlap-column mean of 4.269 differs from the $t = -1$ mean of 4.944 reported in Table 9. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.17: Cross-Geography Merger Effects on Prices — Acquirer Dominance Heterogeneity

	Log Negotiated Price							
	Monopoly Market		Max HHI		Weighted Mean HHI		Weighted Mean HHI (Adj.)	
	Baseline (1)	Overlap (2)	Baseline (3)	Overlap (4)	Baseline (5)	Overlap (6)	Baseline (7)	Overlap (8)
Post × Treated	0.017	-0.026	-0.088**	-0.169***	0.058***	0.010	0.009	0.009
	(0.026)	(0.031)	(0.045)	(0.053)	(0.019)	(0.021)	(0.013)	(0.014)
↪ ×H	0.032	0.054*	0.139***	0.199***	-0.037	0.035	2.937***	1.975***
	(0.027)	(0.032)	(0.046)	(0.054)	(0.039)	(0.043)	(0.591)	(0.727)
↪ × Overlap		33.177***		241.273***		26.020***		0.514
		(10.497)		(74.285)		(5.766)		(5.308)
↪ × Overlap ×H		-25.567**		-233.587***		-64.427***		169.143
		(10.598)		(74.363)		(21.936)		(169.918)
Mean dep. var.	-0.592	-0.592	-0.592	-0.592	-0.592	-0.592	-0.592	-0.592
Mean H pre-merger	0.799	0.799	0.918	0.918	0.335	0.335	0.012	0.012
SD H pre-merger	0.401	0.401	0.208	0.208	0.229	0.229	0.016	0.016
Observations	310,668	310,365	310,668	310,365	310,668	310,365	310,668	310,365
R^2	0.791	0.791	0.791	0.791	0.791	0.791	0.791	0.791

Notes: Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions include hospital-event and year-event fixed effects. Heterogeneity variables vary by column as indicated. Baseline columns include interactions between Post × Treated and the listed heterogeneity variable. Overlap columns additionally include interactions with cross-geography overlap and the triple interaction between Post × Treated, Overlap, and the heterogeneity variable. Monopoly Market indicates whether the acquiring system has at least one monopolistic market. Max HHI is the maximum HHI for the acquiring system. Weighted Mean HHI averages the HHI for the acquiring system, weighted by the total number of employees in the insurer's network located in the acquiring hospital HSAs. Weighted Mean HHI (Adj.) uses a similar weighted average but applies it across the entire employee population. The mean dependent variable is measured in the year before the merger. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.18: Effects on Log Negotiated Price — Narrow Network and Adjusted HHI (HSA-Level)

	Log Negotiated Price							
	Not Narrow Network				Narrow Network			
	Below Median HHI		Above Median HHI		Below Median HHI		Above Median HHI	
	Baseline (1)	Overlap (2)	Baseline (3)	Overlap (4)	Baseline (5)	Overlap (6)	Baseline (7)	Overlap (8)
Post × Treated	-0.002	-0.036*	0.069***	0.055***	-0.005	0.024	0.105***	-0.030
	(0.017)	(0.020)	(0.014)	(0.016)	(0.045)	(0.056)	(0.034)	(0.046)
↪ × Overlap		271.090***		3.659*		-192.975		30.741***
		(63.314)		(2.094)		(163.001)		(6.691)
Mean dep. var.	-0.512	-0.512	-0.610	-0.610	-0.581	-0.581	-0.684	-0.684
Mean overlap		0.000137		0.003852		0.000153		0.004041
Observations	88,700	88,446	148,108	148,108	34,228	34,179	39,632	39,632
Events	216	216	246	246	114	114	79	79
R^2	0.791	0.789	0.796	0.796	0.763	0.767	0.792	0.793

Notes: Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions include hospital-event and year-event fixed effects. Columns split the sample by narrow network status and whether the acquiring system's adjusted HHI is above its median value. A target is in a narrow network when its HSA-year comparison group contains a comparable out-of-network hospital, where comparability uses hospital type and the side of the year-by-hospital-type bed median; adjusted HHI is the acquiring system's employee-weighted HHI numerator divided by the insurer's entire employee population. Overlap columns additionally include the interaction between Post × Treated and overlap. The mean dependent variable is measured in the year before the merger. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.19: Effects on Log Negotiated Price — Narrow Network and Adjusted HHI (HRR-Level)

	Log Negotiated Price							
	Not Narrow Network				Narrow Network			
	Below Median HHI		Above Median HHI		Below Median HHI		Above Median HHI	
	Baseline (1)	Overlap (2)	Baseline (3)	Overlap (4)	Baseline (5)	Overlap (6)	Baseline (7)	Overlap (8)
Post × Treated	0.033	-0.015	0.060***	0.033	0.006	0.000	0.067***	0.036*
	(0.025)	(0.029)	(0.019)	(0.023)	(0.018)	(0.020)	(0.016)	(0.019)
↪ × Overlap		324.344***		5.195*		17.561		9.395***
		(95.056)		(2.776)		(20.316)		(2.694)
Mean dep. var.	-0.549	-0.549	-0.564	-0.564	-0.504	-0.504	-0.656	-0.656
Mean overlap		0.000150		0.004835		0.000261		0.003399
Observations	30,585	30,486	52,459	52,459	87,503	87,299	140,121	140,121
Events	68	68	90	90	255	255	242	242
R^2	0.803	0.803	0.790	0.790	0.782	0.781	0.792	0.793

Notes: Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions include hospital-event and year-event fixed effects. Columns split the sample by narrow network status and whether the acquiring system's adjusted HHI is above its median value. A target is in a narrow network when its HRR-year comparison group contains a comparable out-of-network hospital, where comparability uses hospital type and the side of the year-by-hospital-type bed median; adjusted HHI is the acquiring system's employee-weighted HHI numerator divided by the insurer's entire employee population. Overlap columns additionally include the interaction between Post × Treated and overlap. The mean dependent variable is measured in the year before the merger. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.20: Merger Effects on Log Negotiated Prices: Full Sample and For-Profit Targets

	Log Negotiated Prices			
	Full Sample		For-Profit Targets	
	Baseline (1)	With Overlap (2)	Baseline (3)	With Overlap (4)
Post × Treated	0.044***	0.023*	0.071***	0.023
	(0.012)	(0.013)	(0.020)	(0.027)
Post × Treated × Overlap		8.656***		7.616***
		(2.021)		(2.613)
Mean dep. var.	-0.592	-0.592	-0.691	-0.691
Observations	370,810	369,793	137,059	136,669
R^2	0.798	0.790	0.804	0.792

Notes: This table reports the effects of hospital mergers and employer overlap on negotiated prices. Columns (1) and (2) report estimates using the full sample of acquired hospitals and their corresponding control hospitals. Columns (3) and (4) restrict treated target hospitals to those classified as for-profit in the year before acquisition while retaining the original control hospitals in the corresponding merger-event stacks. Columns (1) and (3) report the baseline difference-in-differences specification. Columns (2) and (4) additionally interact Post × Treated with employer overlap. Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions include hospital-event and year-event fixed effects. The mean dependent variable is measured in the year before the merger event. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.21: Merger Effects on Inpatient and Outpatient Prices

	Log Prices					
	Baseline			With Overlap		
	Inpatient (Unit-Adjusted) (1)	Outpatient (APC) (2)	Outpatient (RVU) (3)	Inpatient (Unit-Adjusted) (4)	Outpatient (APC) (5)	Outpatient (RVU) (6)
Post × Treated	0.039***	0.060***	0.082***	0.023*	0.050***	0.070***
	(0.011)	(0.008)	(0.008)	(0.013)	(0.008)	(0.009)
Post × Treated × Overlap				6.740***	4.605***	5.356***
				(1.901)	(1.584)	(1.733)
Mean dep. var.	8.420	5.438	4.825	8.420	5.438	4.825
Mean overlap	–	–	–	0.002	0.002	0.002
SD overlap	–	–	–	0.004	0.004	0.004
Observations	370,810	364,713	365,164	369,793	363,863	364,309
R^2	0.808	0.908	0.909	0.801	0.905	0.907

Notes: This table reports the merger effects on a unit-adjusted inpatient price index and two outpatient price indices for acquired hospitals. Columns (2) and (5) report results using outpatient prices calculated by collapsing outpatient services at the CMS Ambulatory Payment Classification (APC) level. Columns (3) and (6) report results using outpatient prices standardized using Medicare relative value units (RVUs). Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions include hospital-event and year-event fixed effects. The mean dependent variable is measured in the year before the merger event. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.2 Theoretical Appendix

In this appendix, we present additional analysis to support Section 2 in the main text. We first provide stylized examples of employer-procurement models that illustrate how merging hospitals might threaten the insurer's ability to win lucrative employer contracts. We show that, despite the intuition that this should allow merging hospitals to obtain larger post-merger transfers, the theory can often point in the opposite direction. We then formalize the statements made in the main text regarding the labor-market model and how employers' use of health insurance to offset wages can generate the submodularity condition. Finally, we present an alternative theory based on the threat of replacement, demonstrating how mergers across markets can yield price increases outside the standard Nash-in-Nash framework.

A.2.1 Employer procurement models. A common intuition in the cross-market merger literature is that merging hospitals might threaten insurers' ability to win employer contracts. If employers procure coverage by choosing among insurers the one that provides their employees with the largest surplus, then expanding hospital networks in markets relevant to the employers can increase both the insurer's probability of winning and the profit it derives conditional on winning. Logic would then suggest that if two hospitals relevant to these employers merge, even if across markets, they should control more of the insurer's value than either would individually.

There is, of course, no single way of specifying such a procurement environment. Here, we provide a couple of simplified examples showing how this logic might fail and produce results that are either ambiguous or directly contrary to those predicted by the argument above. To simplify the exposition, we consider the case in which systems s and r are in distinct markets, m and n, and assume a single large employer overlaps both markets. The employer solicits coverage offers from a collection of insurers. From the perspective of the focal insurer in our analysis, $F(\cdot)$ is the distribution of the maximum network surplus offered to the employer by rival insurers, which we assume has strictly positive density over the real line.[42] We assume that equilibrium networks are unaffected by the merger and that transfer negotiations between insurers and hospitals occur before this procurement round, making the transfer cost sunk. We further assume that the employer simply seeks to maximize its employees' net surplus, as in the main text. To reduce notation, we adopt the shorthand $v_m = v_m(\mathcal{N} \cap \mathcal{H}_m)$ and use v_n analogously for the other market. We recall that we denote by $V(\mathcal{N})$ the insurer's procurement-stage payoff, gross of hospital transfers.

Suppose first that the employer procures coverage by choosing the highest-surplus insurer and then negotiates with it, à la Nash, subject to the threat of replacing it with the next-best offer. In this case, the focal insurer wins with probability $F(v_n + v_m)$, and obtains a payment that solves

$$\max_p ((v_n + v_m) - p - (w - p^o))^{1-\tau} p^\tau \implies p = \tau(v_n + v_m - w + p^o)$$

[42] Surplus offers can be considered relative to some fixed outside option of the employer, which rationalizes the relative surplus taking on negative values.

where w is the best alternative surplus and p^o is the payment to the alternative under disagreement, which depends on the threat protocol. For example, under Nash-in-Nash with Threat of Replacement (NNTR) (Ho and Lee, 2019), $p^o = 0$, and under standard fall-back Nash bargaining with an employer outside option $v^o < w$, we have $p^o = \tau(w - v^o)$. Taking the NNTR case for simplicity, we get that the insurer's payoff is

$$V(\mathcal{N}) = \int_{-\infty}^{v_n+v_m} \tau(v_n + v_m - w)dF(w)$$

This payoff structure has the intuitive properties described above: a greater surplus in either market increases both the probability that the insurer wins and the payoff the insurer derives conditional on winning. However, this objective is strictly supermodular. To see this, note that the function $g(x) = \int_{-\infty}^{x} \tau(x - w)dF(w)$ is convex, with second derivative $g''(x) = \tau f(x)$, and thus the composition of the additive $v_n + v_m$ with this convex function yields a strictly supermodular function. Hence, under the main text's assumptions of equal bargaining weights, additive costs, and fixed networks, the merger between s and r will be predicted to decrease rather than increase total transfers.

Changing the fallback bargaining protocol does not unambiguously recover submodularity. Under Nash-fallback, the composing function is $g(x) = \int_{v^o}^{x} \tau(x - w + \tau(w - v^o))dF(w)$, whose second derivative is $g''(x) = \tau(1 + \tau)f(x) + \tau^2 f'(x)(x - v^o)$.[43] Thus, under special distributions of rival values, the composing function can be concave and generate submodularity. However, its second derivative generally has no determinate sign, leading to ambiguous predictions about whether mergers would increase or decrease overall prices.

A simple alternative is to consider a setting in which the employer buys a plan on an exchange. The employer's indirect utility from enrolling in the focal insurer's plan is

$$u = v_n + v_m - p + \epsilon$$

where ϵ is a type-I extreme-value idiosyncratic shock. Assume, for simplicity, that there is a collection of alternative insurance products of fixed values, with an exponential inclusive value equal to A^o, such that the probability that the employer enrolls in the focal insurer's plan is given by

$$s(p, \mathcal{N}) = \frac{\exp(v_n + v_m - p)}{A^o + \exp(v_n + v_m - p)}$$

and the payoff of the insurer is

$$V(\mathcal{N}) = \max_p p \cdot s(p, \mathcal{N})$$

Denote by $v = v_n + v_m$ the total surplus offered by the insurer to the employer. It is easy to

[43] Note that we change the support of F to $[v^o, \infty)$ to avoid cases with no gains from trade under fallback bargaining. It does not materially change the results.

see that $\partial p/\partial v = s(p, \mathcal{N}) > 0$, meaning that, again, higher surplus increases both the probability that the employer chooses the insurer and the profit the insurer obtains from the transaction. However, again, note that $g(v) := \max_p p \frac{e^{v-p}}{A^o + e^{v-p}}$ is convex in v. In particular,

$$g''(v) = s(1-s)^2 > 0$$

which again implies a supermodular objective, leading to lower post-merger hospital transfers.

There are, of course, many other alternative models. Optimal procurement mechanisms via reverse scoring auctions also yield complicated and ambiguous results. For example, it is easy to modify the model to one with multiple symmetric insurers, each with costs and network values that are correlated but drawn independently across insurers (for example, due to differences in bargaining skill and customer service). As long as network values are verifiable and contractible (i.e., the employer can determine the breadth of the network it is procuring), it is possible to derive the optimal direct mechanism. However, the payoff of any given insurer in the optimal mechanism is a rather complicated object, whose second derivative with respect to its network value depends on various factors such as the distribution of rival insurers' costs and network values. Except in special cases, it is also impossible to unequivocally say that an insurer's payoff would be submodular with respect to network additions across markets or even monotonic in the quality of its network.[44]

These examples illustrate the difficulty of establishing a coherent model of employer procurement that yields merger effects. In many cases, adding hospitals across markets increases both the likelihood that the insurer wins the employer contract and the price it obtains, yet this phenomenon introduces convexity rather than concavity to the insurer's objective, making hospitals complements to its product rather than substitutes. This complementarity implies that, by merging, hospitals lose the leverage they had over the insurer's ability to capture the gains from complementarity when selling to employers. Once the hospitals form a single system, this complementarity is internalized, and prices are lowered.

A.2.2 Labor market model. We now establish the submodularity result described in Section 2 of the main text. We begin by specifying the model's conditions more clearly. There are two labor and healthcare markets, m and n, and two hospital systems, s and r, such that s is located in m and r in n. In each market $l \in \{m, n\}$, worker i derives indirect utility from working for the large cross-market employer given by

$$u_{il} = \gamma_l \ln(w_l) + \nu_l x_l + \xi_l + \epsilon_{il}$$

where $\gamma_l, \nu_l > 0$ are the worker's preferences for (log) wages and access to the local system (s if $l = m$ and r if $l = n$). The worker derives additional value from work amenities, given by ξ_l, and experiences an idiosyncratic type-I extreme-value shock, ϵ_{il}. In this notation, we have defined

[44]We omit the extensive development required to establish the reverse auction variant. It is largely a textbook optimal direct mechanism, yielding an expected payoff for the insurer that is convoluted and not trivially sign-definite in its second derivative. A derivation of such an example is available upon request.

x_l as the indicator of whether the employer's health insurance network includes access to the local hospital in market l. Although this term is binary, we extend it to $[0, 1]$ for the derivations below, interpreting interior points as partial access. The submodularity condition evaluated below remains properly defined at the binary corners, as in the main text.

Each market contains a mass M_l of workers, who choose between working at the cross-market employer and a collection of alternative local opportunities (rival employers and non-employment). Under the extreme-value assumption, the labor supply the firm obtains from market l is the conditional-logit share

$$L_l(w_l, x_l) = M_l \frac{e^{\gamma_l \ln(w_l)+\nu_l x_l+\xi_l}}{I_l + e^{\gamma_l \ln(w_l)+\nu_l x_l+\xi_l}}$$

where $I_l > 0$ is the inclusive value of the alternative opportunities, which we hold fixed. In contrast to the analysis in Lamadon et al. (2022), we do not require the cross-market employer to be small relative to its local labor markets: fixing I_l is a statement that rival offers are held constant—the standard partial-equilibrium convention—rather than an approximation. The small-employer case corresponds to the limit $e^{\gamma_l \ln(w_l)+\nu_l x_l+\xi_l} \ll I_l$, in which the supply curve reduces to the isoelastic form $L_l = A_l e^{x_l \nu_l} w_l^{\gamma_l}$ with $A_l = M_l e^{\xi_l}/I_l$; every result below applies to that case as well.

Inverting the supply curve on $L_l \in (0, M_l)$ yields the upward-sloping inverse labor supply of the firm in each market,

$$W_l(L_l, x_l) = e^{-\frac{\nu_l x_l+\xi_l}{\gamma_l}} \left(\frac{I_l L_l}{M_l - L_l}\right)^{1/\gamma_l}.$$

Because access enters worker utility additively alongside log wages, it is equivalent to a proportional wage subsidy, and thus shifts the inverse supply curve multiplicatively. As a result, the firm's wage bill factors as

$$L_l W_l(L_l, x_l) = e^{-\frac{\nu_l x_l}{\gamma_l}} \varphi_l(L_l), \qquad \varphi_l(L_l) := \tilde{I}_l L_l^{\kappa_l}(M_l - L_l)^{-1/\gamma_l},$$

where $\kappa_l := (\gamma_l + 1)/\gamma_l > 1$ and $\tilde{I}_l := (I_l e^{-\xi_l})^{1/\gamma_l}$. Direct differentiation shows that $\varphi_l' > 0$ and $\varphi_l'' > 0$ on $(0, M_l)$: every term produced by the product rule is positive because $\kappa_l > 1$. Moreover, $\varphi_l'(L_l) \to 0$ as $L_l \to 0$ and $\varphi_l'(L_l) \to \infty$ as $L_l \to M_l$. Hence the wage bill is increasing and strictly convex in labor demand, L_l, and is strictly submodular in (L_l, x_l). In particular,

$$\frac{\partial^2}{\partial L_l \partial x_l} L_l W_l(L_l, x_l) = -\frac{\nu_l}{\gamma_l} e^{-\frac{x_l \nu_l}{\gamma_l}} \varphi_l'(L_l) =: -e_l < 0.$$

The cross-market employer has a Cobb-Douglas production function with decreasing returns to scale and chooses wages to offer in each market and the level of capital to procure. It solves

$$\Pi(x_m, x_n) = \max_{K, L_m, L_n} \Omega K^{\alpha}(L_m + L_n)^{\beta} - \rho K - \sum_{l \in \{m,n\}} L_l W_l(L_l, x_l)$$

where ρ is the rental rate of capital, $\alpha + \beta < 1$, $\alpha, \beta > 0$, and Ω is the product of the selling price of the firm's product or service and the total factor productivity, both of which we consider constant. Since the wage bill is convex in each market, the employer will seek to smooth hiring across the two markets. To focus on labor effects, we solve for the firm's optimal capital purchase and substitute it into the objective. In particular, we have that

$$K^*(L_m, L_n) = (\frac{\alpha\Omega}{\rho})^{1/(1-\alpha)}(L_m + L_n)^{\beta/(1-\alpha)}$$

Substituting this expression into the objective yields

$$\Pi(x_m, x_n) = \max_{L_m, L_n} \tilde{\Omega}(L_m + L_n)^{\theta} - \sum_{l \in \{m,n\}} L_l W_l(L_l, x_l)$$

where $\tilde{\Omega} = (1-\alpha)\Omega(\frac{\alpha\Omega}{\rho})^{\alpha/(1-\alpha)}$ combines all the constant terms and $\theta := \beta/(1-\alpha) \in (0,1)$ because $\alpha + \beta < 1$.

The reduced objective is strictly concave on $(0, M_m) \times (0, M_n)$: the revenue term is concave in (L_m, L_n) since $\theta < 1$, while each wage bill is strictly convex in its own argument. The solution is interior and unique: marginal revenue diverges as total labor vanishes while the marginal wage bill vanishes at $L_l = 0$; conversely, the marginal wage bill diverges as $L_l \to M_l$ while marginal revenue remains bounded. Since the Hessian of the objective is negative definite at the optimum, the implicit function theorem implies that the policy functions $L_l^*(x_m, x_n)$ are continuously differentiable on $[0,1]^2$, and thus Π is twice continuously differentiable by the envelope theorem.

We now note the key property that delivers the Dafny et al. (2019) condition: $\Pi(x_m, x_n)$ is strictly submodular. Writing the first-order conditions as $\theta\tilde{\Omega}(L_m + L_n)^{\theta-1} = e^{-\nu_l x_l/\gamma_l}\varphi_l'(L_l)$ for $l = m, n$, the Hessian of the objective takes the form

$$H = \begin{pmatrix} -q - d_m & -q \\ -q & -q - d_n \end{pmatrix}, \qquad q := \theta(1-\theta)\tilde{\Omega}(L_m + L_n)^{\theta-2} > 0, \quad d_l := e^{-\frac{\nu_l x_l}{\gamma_l}}\varphi_l''(L_l) > 0,$$

with $\det H = q(d_m + d_n) + d_m d_n > 0$. The negative off-diagonal terms formalize the sense in which labor in one market is a substitute for labor in the other; note that they are strictly negative precisely because $\theta < 1$. Since access lowers the marginal wage bill in its own market and does not enter the other market's first-order condition, differentiating the system with respect to x_m yields

$$\begin{pmatrix} \partial L_m^*/\partial x_m \\ \partial L_n^*/\partial x_m \end{pmatrix} = -H^{-1}\begin{pmatrix} e_m \\ 0 \end{pmatrix} = \frac{e_m}{\det H}\begin{pmatrix} q + d_n \\ -q \end{pmatrix},$$

so that expanded access in market m strictly increases hiring there and strictly decreases hiring in market n. By the envelope theorem, $\frac{\partial \Pi}{\partial x_n} = \frac{\nu_n}{\gamma_n} e^{-\nu_n x_n/\gamma_n} \varphi_n(L_n^*) > 0$, so the employer's payoff is strictly increasing in access. Differentiating this expression with respect to x_m, which operates only through L_n^*, delivers the cross-partial

$$\frac{\partial^2 \Pi(x_m, x_n)}{\partial x_m \partial x_n} = \frac{\nu_n}{\gamma_n} e^{-\frac{\nu_n x_n}{\gamma_n}} \varphi_n'(L_n^*) \frac{\partial L_n^*}{\partial x_m} = -\frac{q\, e_m e_n}{\det H} < 0$$

which holds for all $(x_m, x_n) \in [0,1]^2$. The expression makes the role of decreasing returns transparent: at constant returns to scale ($\alpha+\beta = 1$), $q = 0$ and the payoff becomes exactly additive across markets, eliminating cross-geography effects.[45] Finally, recall that strict submodularity means that

$$\Pi(1,0) + \Pi(0,1) > \Pi(1,1) + \Pi(0,0)$$
$$\Leftrightarrow \Pi(1,0) + \Pi(0,1) - \Pi(1,1) - \Pi(0,0) = -\int_0^1 \int_0^1 \frac{\partial^2 \Pi(a,b)}{\partial x_m \partial x_n}\, da\, db > 0$$

where the integral representation is valid because Π is twice continuously differentiable on $[0,1]^2$. This implies that the employer's indirect payoff is strictly submodular in local network access. As noted in the main text, since Π is increasing in (x_m, x_n), any nondecreasing and weakly concave transformation of the employer's payoff inherits its submodularity, strictly so whenever the transformation is strictly increasing.[46] In particular, any affine transformation with positive slope—as in the bargaining outcome—preserves strict submodularity.

A.2.3 Replacement threat model. We now consider a different model that can generate merger effects across markets. For simplicity, we consider the case in which each system is composed of a single hospital $s = \{h\}$ and $r = \{h'\}$, and the two hospitals are located in different markets. In market n, where r is located, there is an alternative hospital h^o. In market m, where s is located, there is a potential replacement h'', which may be an imperfect substitute for the acquirer: swapping h for h'' in any network lowers the employer's valuation by a quality gap $\delta \geq 0$. The case in which the acquirer is a local monopolist corresponds to the absence of h'' or, equivalently, to a prohibitively costly replacement; we make this nesting precise below. There is a single employer that overlaps the two markets, and it is required by law to provide adequate coverage in each market. Thus, the employer purchases the insurer's plan only if it offers coverage in both markets. The payoff to the insurer is v if $\{h, h'\}$ are in network, $\bar{v}$ if the network further includes h^o, and $\underline{v}$ if the network excludes h' but includes h^o; replacing h with h'' in any of these networks lowers the corresponding payoff by δ. The payoff is zero if market m has no hospital

[45] The derivation uses only that the wage bill takes the multiplicative form $e^{-\nu_l x_l/\gamma_l} \varphi_l(L_l)$ with φ_l increasing and strictly convex; the logit functional form is otherwise immaterial. Any labor supply system in which amenities act as a wage subsidy shares this structure.

[46] Let g be nondecreasing and concave, and let $a \leq b, c \leq d$ satisfy $a + d \leq b + c$, as delivered by the monotonicity and submodularity of Π evaluated at the four corners of the comparison. Concavity implies $g(a) + g(a+s+t) \leq g(a+s) + g(a+t)$ for all $s, t \geq 0$; taking $s = b - a$ and $t = c - a$ and using $d \leq b + c - a$ with g nondecreasing yields $g(a) + g(d) \leq g(b) + g(c)$. Monotonicity of the underlying payoff is required: concave transformations of non-monotone submodular functions need not be submodular.

or both h' and h^o are missing. We assume $\bar{v} > v > \underline{v} > \delta \geq 0$, so the employer prefers broader networks and every replacement network retains positive value. Let $c_h, c_{h'}, c_{h^o}, c_{h''}$ denote the cost of each hospital and assume zero recapture. We assume a common hospital bargaining weight, $\lambda \in (0, 1)$, across all hospitals. In addition, we assume that the insurer has limited liability: it pays hospitals their negotiated transfer fee only if the employer buys the contract, and it exercises a replacement threat only when doing so is profitable. Finally, we summarize the strength of the acquirer's position by its *replacement gap*,

$$R := \delta + c_{h''},$$

the total surplus the insurer sacrifices by swapping the acquirer out: the value lost to the quality gap plus the cost of compensating the replacement. We assume $R > c_h$, so that the acquirer is the efficient hospital to contract with in market m.[47]

Consider first the insurer's negotiation with the acquirer, in any network $\mathcal{H}$ containing h. Upon disagreement, the insurer's best option is to replace h with h'' at cost, provided this is profitable; the employer still purchases the plan in that event, so the committed market-n transfers, $t_r(\mathcal{H})$, remain payable and cancel on both sides of the negotiation. The insurer's disagreement payoff is therefore $D_h(\mathcal{H}) = \max\{V(\mathcal{H}) - \delta - c_{h''} - t_r(\mathcal{H}),\, 0\}$, where $V(\mathcal{H})$ is $\bar{v}$ if both h' and h^o are in network, $\underline{v}$ if only h^o is available, and v if only h' is available. The transfer to the acquirer solves

$$\begin{aligned} &\max_t (V(\mathcal{H}) - t - t_r(\mathcal{H}) - D_h(\mathcal{H}))^{1-\lambda}(t - c_h)^{\lambda} \\ \Longrightarrow \quad & t_h(\mathcal{H}) = (1-\lambda)c_h + \lambda \min\{V(\mathcal{H}) - t_r(\mathcal{H}),\ R\} \end{aligned}$$

so that the acquirer's markup is capped at $\lambda(R - c_h)$: a hospital can never extract more than a share λ of the surplus the insurer would sacrifice to replace it. The monopoly case is nested as the branch in which the cap is slack—formally, $R = +\infty$—recovering $t_h(\mathcal{H}) = (1-\lambda)c_h + \lambda(V(\mathcal{H}) - t_r(\mathcal{H}))$.

We first analyze the case of an *effectively must-have* acquirer, $R \geq \bar{v}$. Then every continuation payoff from replacing the acquirer—or, below, any merged system containing it—is negative, limited liability binds, and all threat points involving h'' equal zero. The model therefore coincides exactly with one in which h is a local monopolist: strict monopoly is sufficient for what follows but not necessary.[48] We maintain this assumption through the merger analysis and relax it at the end of the section.

In market n, the insurer has the option to contract with a single hospital and use the other as a replacement threat, or to contract with both. These negotiations are unaffected by the presence

[47] By the mirror-image argument, when $R > c_h$ there are no bilateral gains from trade between the insurer and h'' given that h is available as a replacement threat against it: the joint surplus of that pairing is $c_h - \delta - c_{h''} = c_h - R < 0$. Hence h'' is never in the equilibrium network and serves purely as a threat, which it is willing to do at cost, as in Ho and Lee (2019).

[48] The condition $R \geq \bar{v}$ is itself stronger than needed; it suffices that the replacement threats be unprofitable in the network configurations that arise on the equilibrium path. We use $R \geq \bar{v}$ because it frees the analysis from checking configurations case by case.

of h'', since h remains in the network at every threat point they involve. Assuming that the equilibrium network includes h, if the insurer contracts exclusively with h' and uses h^o to form an NNTR threat (Ho and Lee, 2019), the transfer solves

$$\begin{aligned}
&\max_t (v - t - (\underline{v} - c_{h^o}))^{1-\lambda}(t - c_{h'})^{\lambda} \\
\implies \quad & t_{h'}(\{h, h'\}) = (1-\lambda)c_{h'} + \lambda(v - \underline{v} + c_{h^o})
\end{aligned}$$

and similarly, if the insurer contracts only with h^o and uses h' as the threat, we obtain:

$$t_{h^o}(\{h, h^o\}) = (1-\lambda)c_{h^o} + \lambda(\underline{v} - v + c_{h'}).$$

In contrast, if it negotiates with both hospitals for inclusion, the transfers for h' and h^o solve

$$\begin{aligned}
&\max_t (\bar{v} - t - \underline{v})^{1-\lambda}(t - c_{h'})^{\lambda} && \implies t_{h'}(\{h, h', h^o\}) = (1-\lambda)c_{h'} + \lambda(\bar{v} - \underline{v}) \\
&\max_t (\bar{v} - t - v)^{1-\lambda}(t - c_{h^o})^{\lambda} && \implies t_{h^o}(\{h, h', h^o\}) = (1-\lambda)c_{h^o} + \lambda(\bar{v} - v)
\end{aligned}$$

Thus, the total transfer paid by the insurer in market n is

$$t_r(\mathcal{H}) = \begin{cases} (1-\lambda)c_{h'} + \lambda(v - \underline{v} + c_{h^o}) & \text{if } \mathcal{H} = \{h, h'\} \\ (1-\lambda)c_{h^o} + \lambda(\underline{v} - v + c_{h'}) & \text{if } \mathcal{H} = \{h, h^o\} \\ (1-\lambda)(c_{h'} + c_{h^o}) + \lambda(2\bar{v} - \underline{v} - v) & \text{if } \mathcal{H} = \{h, h', h^o\} \end{cases}$$

meaning that the total cost to the insurer in each scenario is

$$\bar{t}(\mathcal{H}) = \begin{cases} (1-\lambda)c_h + \lambda v + (1-\lambda)^2 c_{h'} + (1-\lambda)\lambda(v - \underline{v} + c_{h^o}) & \text{if } \mathcal{H} = \{h, h'\} \\ (1-\lambda)c_h + \lambda \underline{v} + (1-\lambda)^2 c_{h^o} + (1-\lambda)\lambda(\underline{v} - v + c_{h'}) & \text{if } \mathcal{H} = \{h, h^o\} \\ (1-\lambda)c_h + \lambda \bar{v} + (1-\lambda)^2(c_{h'} + c_{h^o}) + (1-\lambda)\lambda(2\bar{v} - \underline{v} - v) & \text{if } \mathcal{H} = \{h, h', h^o\} \end{cases}$$

This establishes three potential scenarios. The first possibility is that $\{h, h'\}$ is the optimal network. Gains from trade in market n require $c_{h'} < v - \underline{v} + c_{h^o}$, while gains from trade in market m require $v - [(1-\lambda)c_{h'} + \lambda(v - \underline{v} + c_{h^o})] \geq c_h$. A comparison of insurer profits shows that, given gains from trade, the arrangement $\{h, h'\}$ is optimal if either $\lambda \geq 1/2$ or $c_{h^o} \geq \bar{v} - v$. The second possibility is that $\{h, h^o\}$ is optimal, which has gains-from-trade requirements $c_{h'} > v - \underline{v} + c_{h^o}$ and $\underline{v} - [(1-\lambda)c_{h^o} + \lambda(\underline{v} - v + c_{h'})] \geq c_h$. Conditional on gains from trade, $\{h, h^o\}$ is optimal if $c_{h'} \geq \bar{v} - \underline{v}$ or $\lambda \geq 1/2$. Finally, the full network $\{h, h', h^o\}$ is optimal if both $c_{h'} < \bar{v} - \underline{v}$ and $c_{h^o} < \bar{v} - v$, and $\bar{v} - [(1-\lambda)(c_{h'} + c_{h^o}) + \lambda(2\bar{v} - \underline{v} - v)] \geq c_h$ and $\lambda \leq 1/2$.

From these observations, it is easy to construct scenarios in which the acquisition of h' by h across markets strictly increases prices. In particular, consider $\lambda > 1/2$ and suppose that the conditions for $\{h, h'\}$ being optimal pre-merger hold. If, in addition, it holds that

$$v - \underline{v} + 2c_{h^o} - (\bar{v} - v) < c_{h'} < v - \underline{v} + c_{h^o}$$
$$\bar{v} - [(1-\lambda)c_{h^o} + \lambda(\bar{v} - v)] \geq c_h + c_{h'}$$

then, post-merger, the insurer's optimal network will expand to include h^o, since excluding it is no longer helpful in keeping the price of h' low. Since the conditions above imply that $c_{h^o} < \bar{v} - v$, including the hospital is efficient for the insurer but increases total transfers to the merged party by:

$$t_{h,h'}(\{h, h', h^o\}) - [t_h(\{h, h'\}) + t_{h'}(\{h, h'\})] = \lambda(1-\lambda)[\bar{v} - 2v - 2c_{h^o} + \underline{v} + c_{h'}]$$

which is positive by the conditions above.

It is also easy to derive alternative conditions under which the pre-merger network is $\{h, h^o\}$ and the acquisition changes the insurer's optimal network to $\{h, h'\}$ or $\{h, h', h^o\}$, both of which automatically yield increases in total transfers to the system (as long as costs are non-negative). While plausible, these scenarios are slightly less interesting because our empirical evidence conditions on the set of hospitals in the network both before and after the acquisition; otherwise, we could not measure their price increase in the analysis.

We now relax the must-have assumption and show that it—rather than literal monopoly—is the essential ingredient. When $R < \bar{v}$, the replacement threat can bind against the merged system as well. Upon disagreement with the merged party in the full network, the insurer replaces the system with the bundle $\{h'', h^o\}$: it bears the quality gap δ, pays $c_{h''}$, continues to pay the committed transfer t_{h^o}, and the network value falls from $\bar{v}$ to $\underline{v} - \delta$. In the narrow network $\{h, h'\}$, the replacement bundle is the same, but h^o joins at cost c_{h^o}. The merged transfers become

$$t_{h,h'}(\{h, h', h^o\}) = (1-\lambda)(c_h + c_{h'}) + \lambda \min\left\{\bar{v} - t_{h^o}(\{h, h', h^o\}),\ (\bar{v} - \underline{v}) + R\right\}$$
$$t_{h,h'}(\{h, h'\}) = (1-\lambda)(c_h + c_{h'}) + \lambda \min\left\{v,\ (v - \underline{v} + c_{h^o}) + R\right\}$$

The caps reveal the bundling logic of the all-or-nothing threat: the merged system's replacement gap is the *sum* of its members' gaps—the acquirer's gap R plus the target's gap given its local replacement—because replacing the system forces the insurer to bear both value losses and both replacement costs at once.

Suppose then that the acquirer is easily replaced, in the sense that every replacement threat is profitable to exercise: $R \leq \min\{v - t_{h'}(\{h, h'\}),\ \underline{v} - t_{h^o}(\{h, h', h^o\}),\ \underline{v} - c_{h^o}\}$, a region that is non-empty when h'' is a close and cheap substitute for the acquirer while the market-n replacements are strong. Every min above then attains its capped branch, so every negotiating party—and the merged system—earns a markup equal to λ times its replacement cap net of its own cost. In the narrow network, define the acquirer's net replacement gap as $G_h := R - c_h$, the target's as $G_{h'} := v - \underline{v} + c_{h^o} - c_{h'}$, and the merged system's as $G_{h,h'} := v - \underline{v} + c_{h^o} + R - c_h - c_{h'}$. Because

$G_{h,h'} = G_h + G_{h'}$, the merger is exactly neutral. In particular,

$$t_{h,h'}(\{h,h'\}) - [t_h(\{h,h'\}) + t_{h'}(\{h,h'\})] = \lambda \left[G_{h,h'} - G_h - G_{h'}\right] = 0$$

and the same cancellation holds in the full network. Moreover, the insurer's network choice is unaffected by the merger in this region: excluding h^o retains its disciplining value against the merged system, since it cheapens the replacement bundle by $t_{h^o} - c_{h^o}$, and the comparison between $\{h,h'\}$ and the full network reduces to the sign of $(2\lambda - 1)(\bar{v} - v - c_{h^o})$ both before and after the merger. Hence no network expansion occurs, and cross-market acquisitions have no price effect. For intermediate values of R, the merger effect varies continuously between the two polar cases, and its magnitude depends on which replacement threats bind along the equilibrium path; we omit the full taxonomy. The economics, however, is robust: cross-market merger effects in this framework require the threat of replacing the acquirer to be slack, so that tying the target to the acquirer meaningfully worsens the insurer's disagreement position. Local monopoly is the simplest primitive delivering this slack, but any sufficiently dominant, hard-to-replace acquirer suffices. This yields a testable implication—effects should be concentrated among acquisitions by must-have systems.

Overall, this model allows us to capture anticompetitive tying in a clear and simple way. The employer's requirement to have coverage in market m is likely realistic for any employer with a large footprint in a market anchored by a must-have system, while the threat of replacement is a common rationale for the narrow networks observed in practice. Importantly, we note that submodularity is not necessary for the results above. For example, conditional on h^o not being in the pre-merger network, submodularity would require that the insurer's profit loss from removing $\{h,h'\}$ exceeds the sum of the losses from removing h and h' independently. Evaluating losses at the replacement-inclusive continuation values (gross of replacement costs), consistent with the threat protocol, this requirement becomes

$$v - 0 > v - \underline{v} + v - 0$$

or simply $\underline{v} > v$, which was intentionally ruled out here.[49] Instead, hospital prices increase because either the acquisition disables a threat of replacement and allows the insurer to broaden its network, thereby increasing transfers, or it disables the exclusion of the acquired hospital as a replacement threat, thereby increasing total transfers.

A.3 HCRIS Prices

This appendix section describes how we construct the HCRIS-based comparison prices and assess the correlation between these indices and our private prices. We conclude with a comparison of this article's key analysis, in which we replace our observed private prices with proxy metrics.

[49] Under the stricter convention in which the removal of h' leaves the network $\{h\}$ with zero continuation value, the requirement becomes $v > 2v$, which likewise fails. No value convention delivers submodularity in this example.

Construction We construct two common variants of the proxy price, one following Lewis and Pflum (2017) and the other following Dafny et al. (2019). The Lewis and Pflum (2017) measure is defined as

$$P_{ht}^{LP} = \frac{\widetilde{R}_{ht}^{IP}\left(1 - \frac{CA_{ht}}{\widetilde{R}_{ht}^{IP}+\widetilde{R}_{ht}^{OP}}\right) - MS_{ht}}{Q_{ht}^{LP}}. \tag{13}$$

Here, h indexes hospitals and t indexes years; $\widetilde{R}^{IP}$ denotes inpatient revenue net of ambulatory surgery center and hospice components; $\widetilde{R}^{OP}$ denotes the analogous outpatient revenue; and Q^{LP} denotes non-Traditional Medicare discharges. CA denotes contractual adjustments, and MS denotes Medicare settlement payments.

The second measure follows Dafny et al. (2019). For hospital h in year t, this measure can be written as

$$P_{ht}^{DHL} = \frac{\left(R_{ht}^{GEN} + R_{ht}^{INT} + R_{ht}^{ANC}\right)\left(1 - \frac{CA_{ht}}{TPR_{ht}}\right) - (MP_{ht} + MPA_{ht})}{D_{ht} - D_{ht}^{M}}. \tag{14}$$

Here, R^{GEN}, R^{INT}, and R^{ANC} denote general, intensive, and ancillary inpatient revenue, respectively. TPR denotes total patient revenue, MP denotes Medicare payments received, MPA denotes Medicare amounts payable, and D^{M} denotes Medicare discharges.

For both measures, when revenue, payment, or discharge components enter an additive expression, missing components are treated as zero. In addition, discharge denominators are set to missing whenever they equal zero, so the corresponding hospital-year price measure is not defined.

Correlation with Private Prices Table A.22 reports the results of regressing our private prices on the HCRIS index prices. The overall correlation between private and HCRIS-based index prices is computed as the square root of the within-R^2 estimate (adjusted for fixed effects) and signed according to the main coefficient. The reported overall correlation in the introduction corresponds to the first column, while the reported time-series correlation corresponds to the second column. Mechanically, we obtain the same number when looking at the simple correlation between private and index prices, before and after demeaning within a hospital. The estimates indicate that these index prices are limited in their ability to capture time-series variation in our private prices.

Merger effects We use the price index of Dafny et al. (2019) (DHL) and apply their empirical strategy to our sample to further illustrate the impact of using proxy prices. The first three columns of Table A.23 show the results from DHL, copied verbatim for comparison. The last three columns show the estimates for our data, applying the same market definitions, sample restrictions, and regression strategy as in the original article. In the language of DHL,

Table A.22: Correlation Between Commercial Prices and HCRIS Price Indices (in logs)

	Log Negotiated Price							
	Dafny et al. (2019) Index				Lewis and Pflum (2017) Index			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log price index	0.208***	0.173***	0.200***	0.170***	0.069***	-0.006	0.074***	0.043***
	(0.008)	(0.019)	(0.008)	(0.007)	(0.009)	(0.008)	(0.008)	(0.009)
Hospital FE		✓				✓		
Year FE			✓				✓	
State-Year FE				✓				✓
N	22,025	21,881	22,025	22,015	34,400	34,111	34,400	34,399
R-squared	0.172	0.766	0.219	0.405	0.008	0.721	0.043	0.151
Within R-squared	0.172	0.013	0.168	0.145	0.008	0.000	0.010	0.003
Correlation	0.415	0.115	0.410	0.381	0.090	-0.005	0.098	0.057

Notes: The dependent variable is our log negotiated price. Standard errors are clustered at the hospital level. Correlation reports the fixed-effect-adjusted correlation implied by the corresponding specification. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

the cross-market merger effect equivalent to our main estimates corresponds to "Adj Treated $*(t > 0)$", which is the price change on non-local, within-state hospitals. We stress that this is not a replication, as our samples have only two years in common. In fact, it is quite likely that as private and public insurance healthcare prices have diverged, the ability of HCRIS-based prices to detect meaningful changes in private prices has deteriorated.

A.4 Data Construction

This appendix section describes how we construct our different datasets and how certain key variables are computed. We first describe the restrictions and construction of the four main datasets and then explain how we construct the key variables.

A.4.1 Claims. To form our claims panel, we first select all paid facility claims that have a positive allowed amount (i.e., the total payment to a hospital) from any patient who has had any care delivered in an inpatient setting within the same year. We define those patients as those with claims from inpatient hospitals, inpatient psychiatric facilities, psychiatric residential treatment facilities, and comprehensive inpatient rehabilitation facilities. Additionally, we include all patients with claims for inpatient care, room and board, surgical procedures, and deliveries, as well as any claim with a DRG code. Having collected all facility claims for this population, we drop observations associated with DRGs that code "unknown conditions" and observations for services provided at skilled nursing facilities, nursing facilities, custodial-care facilities, or hospice facilities. We also exclude all hospice-related charges, regardless of their location. We mark a claim row as part of an inpatient event if it has a nonmissing DRG and records care provided at an inpatient hospital, inpatient psychiatric facility, psychiatric residential treatment facility, or comprehensive inpatient rehabilitation facility. From our provider data, we extract more accurate information about the type of facility involved in

Table A.23: Comparison with Dafny, Ho, and Lee (2019) Main Pre-Post Regression Results

	1996–2012 mergers (Dafny, Ho, and Lee (2019))			2011–2019 mergers (New)		
	(1) Control Group 1	(2) Control Group 2	(3) Control Group 2 No Targets	(4) Control Group 1	(5) Control Group 2	(6) Control Group 2 No Targets
Adj Treated*(t = 0)	0.032	0.035	0.040	-0.005	-0.031	-0.030
	(0.030)	(0.029)	(0.030)	(0.009)	(0.019)	(0.021)
Adj Treated*(t > 0)	0.102**	0.093**	0.101**	0.011	-0.007	-0.004
	(0.046)	(0.046)	(0.047)	(0.010)	(0.017)	(0.017)
Non-Adj Treated*(t = 0)	-0.017	-0.019	-0.019	-0.014	-0.042	-0.045
	(0.025)	(0.027)	(0.027)	(0.015)	(0.029)	(0.029)
Non-Adj Treated*(t > 0)	-0.031	-0.032	-0.028	0.026	0.051*	0.053**
	(0.030)	(0.034)	(0.034)	(0.016)	(0.028)	(0.027)
ln(Case Mix Index)	0.258***	0.213	0.213	0.118*	0.122	0.133
	(0.056)	(0.160)	(0.161)	(0.061)	(0.085)	(0.085)
ln(Beds)	0.092***	0.117*	0.115*	0.231***	0.191***	0.192***
	(0.018)	(0.067)	(0.067)	(0.027)	(0.037)	(0.037)
For-Profit	0.040**	0.072	0.069	-0.000	0.021	0.006
	(0.019)	(0.049)	(0.049)	(0.042)	(0.059)	(0.067)
% Medicaid	0.103**	0.164	0.166	0.011***	0.011***	0.011***
	(0.051)	(0.149)	(0.150)	(0.002)	(0.002)	(0.002)
Observations	40,994	4,422	4,392	13,855	6,455	6,396
Number of Hospitals	4,174	711	705	1,979	894	892
Within R^2	0.462	0.435	0.436	0.268	0.229	0.233

Notes: Columns (1)–(3) reproduce the published Dafny, Ho, and Lee (2019) estimates; columns (4)–(6) apply the same specifications to our sample. Treatment is defined as being part of a system involved in a cross-market merger in a given year. Directly affected hospitals (hospitals involved in the merger or located within a 30-minute drive of a target or acquirer) are excluded. Adjacent treatment is for hospitals in the same state as the target, while non-adjacent treatment is for hospitals in a different state from the target. As in Dafny, Ho, and Lee (2019), we control for case mix index, bed size, for-profit status, and percent Medicaid patients. We include hospital and year fixed effects, and cluster standard errors at the hospital level. Control Group 1 imposes no additional restrictions on the control group. Control Group 2 excludes hospitals within a 30-minute drive of any treated hospital in the 3 years following and including the merger year. Control Group 2, No Targets further excludes hospitals that were targets in any merger. We express prices in constant year-2000 dollars and trim the price index using the same criteria as Dafny, Ho, and Lee (2019). * p<0.10, ** p<0.05, *** p<0.01.

each claim. We select the subset of facilities registered as hospitals in that data and merge those with our claims data. We then drop any claim that has no rows marked as inpatient or contains no hospital services. In the extremely rare case of multiple facility providers per claim (the 1st and 99th percentiles of providers per claim are 1), we keep the provider that is paid the largest fraction of the bill. Unless stated otherwise, our analysis is also limited to claims flagged as in-network.

A.4.2 Enrollment and Firm Identifiers. We obtain data on all members of commercial plans associated with an employer. We exclude the few members with missing gender or age (less than 0.1%) and those that belong to firms that never (in the entire sample) cover more than 100 individuals, or that ever cover more than 300,000. These restrictions drop about 40% of all member records. We do so to exclude a few small firms that might be directly on the margin of switching to an ACA exchange plan, and a few extremely large employers. Those large employers might be able to negotiate special deals with hospitals directly, in addition to the insurance contracts captured in our data, and thus their exposure to merger effects might differ from that of the typical American firm.

To construct firm identifiers, we begin by forming a panel of movers across plans and flagging plans as either entering or exiting. We say that a plan is renamed, and link the old and new plans, if the original plan exits and, in the following year, a new plan contains more than half of the exiting plan's members while more than half of the new plan's members came from the exiting plan. We also apply the same logic to plans that exit the market in two years, but in the first year, they lose more than 90% of their enrollment. In that case, we combine the exiting plan with its renamed successor. We then iteratively apply the following linkage rule: we flag plan A and plan B as being of the same firm if (i) there are at least five movers in both directions in a year; (ii) plan A exits without being renamed and the majority of its members end up in plan B (or in plans of the same firm as plan B); or (iii) plan A is a new plan and it gets most of its enrollees from plan B (or from plans marked as being from the same firm as plan B). Finally, we perform a connected-component graph analysis to cluster all plans that have formed links via this strategy. The algorithm flags 95% of group-years as independent employers. Among the 38,700 firm-years with more than one group, the mean and median numbers of groups per firm are 2.2 and 2, respectively. Firms with multiple groups tend to be substantially larger: the mean and median number of beneficiaries for single-group firms are 174 and 33, while for multiple-group firms, they are 919 and 120.

A.4.3 Hospital panel. Our hospital panel consists of detailed information from the AHA survey linked with two additional sources. First, we link the AHA with the HCRIS data source. From this, we obtain reported expenses, admissions, and revenues. Second, we link the AHA with data on providers from our administrative data provider — the private national insurer. Crucially, the insurer keeps provider identifiers that are more detailed and more consistent over time than the AHA identifiers. In some cases, it identifies units of the same hospital that are registered as separate entities in the AHA. In others, it correctly detects that two AHA identifiers in different years belong to the same hospital, which had undergone restructuring, renaming, or a change of ownership.

A.4.4 Price index. We construct our price index following Cooper et al. (2019) and run the regression

$$\ln(p^*_{idht}) = \alpha_{Age(i)\times Fem(i)} + \delta_{ht} + \xi_d + \epsilon_{idht}$$

where i indexes patients, h indexes hospitals, d indexes diagnoses (DRG codes), and t indexes years. p^*_{idht} is the observed total allowed amount for the medical event, $\alpha_{Age(i)\times Fem(i)}$ is an age-group-by-gender fixed effect, δ_{ht} are hospital-year fixed effects, ξ_d are diagnosis fixed effects, and ϵ_{idht} is the residual. Age groups are defined as under 18, between 18 and 25, and in 5-year increments up to 65. We exclude members older than 65. We trim extreme payments by diagnosis cell: within each DRG-year sample, we drop medical events whose total allowed amount falls below the 1st percentile or above the 99th percentile of p^*_{idht} for that DRG. The estimated δ_{ht} constitutes our first price index, which we call the log negotiated price. We normalize the age-gender and diagnosis fixed effects relative to a regular vaginal delivery (DRG 768) for a woman aged 31 to 35, such that δ_{ht} is measured in prices relative to a birth. We find that this makes the prices easier to interpret than the usual approach of setting the mean of δ_{ht} to match the mean spending. Applying this alternative normalization, as in Cooper et al. (2019), does not affect any results or the predictive power of this index.

Distance measurements: Distance plays a key role in defining local care markets and merger types. We use two sources for measuring distance based on the level of detail we have on the location of agents. For hospitals, we know exact latitude and longitude, and so we use precise geodesic distances. For enrollees, however, we only observe five-digit ZIP codes. To determine their distance to hospitals, we use publicly available NBER tabulations of distances between ZIP-code centroids.

A.5 Evidence on Acquirers

Here we discuss briefly the evidence on acquiring hospitals, which was omitted from our main analysis. Following the regression in Equation (5), we estimate how within- and cross-geography acquisitions affect acquiring hospitals. The panel is constructed in the same way for acquirers as for targets, with the same standards applied for controls and treatment inclusion. Crucially, as shown in Appendix Table A.2, cross-geography acquirers tend to engage in frequent local acquisitions, which limits our ability to distinguish whether price effects stem from their cross-geography or within-geography mergers. Here, we focus on the narrow—and likely not representative—subsample of acquirers that engaged in no other local merger within the five-year windows centered on our target cross-geography acquisition.

Table A.24 shows the estimates. The price effects for acquirers are similar to those for targets, notably so for cross-geography acquisitions. Within-geography mergers are associated with a 7.7% increase in prices at acquirers, compared to a 6% increase at targets. Similarly, cross-geography effects are 4.7% at acquirers and 4.6% at targets. The final column of Table A.24 shows the effect of employer overlap share on acquirers' prices. As with targets, the effect is positive and significant. However, the effect is smaller in magnitude and explains only 16% of the merger effect. This is consistent with acquiring systems being significantly larger than targets, with the employer overlap channel being effective only for the subset of acquiring hospitals that overlap with the acquisition target. As integrated hospital systems are known to negotiate rates as a block, with the threat of removing their entire system from insurers' networks, even hospitals that are not in the direct overlap path can still benefit from it.

Table A.24: Merger Effects on Log Negotiated Prices for Acquiring Hospitals

	Within-Geography			**Cross-Geography**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post × Treated (α)	0.077***	0.077***	0.077***	0.046***	0.046***	0.047***	0.039***
	(0.012)	(0.012)	(0.012)	(0.009)	(0.009)	(0.009)	(0.009)
Post × Treated × Overlap							5.237***
							(1.414)
Total beds		0.000	0.000		0.000*	0.000**	0.000**
		(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Doctors per bed			0.028			0.019	0.018
			(0.026)			(0.019)	(0.020)
Nurses per bed			0.004			0.007	0.007
			(0.009)			(0.007)	(0.007)
Mean dep. var.	-0.567	-0.567	-0.567	-0.563	-0.563	-0.563	-0.563
Observations	371,161	371,161	371,161	2,193,665	2,193,665	2,193,665	2,188,996
R^2	0.819	0.819	0.819	0.809	0.809	0.809	0.803

Notes: Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions are estimated on acquiring hospitals and include hospital-event and year-event fixed effects. Column (7) adds the interaction between Post × Treated and employer overlap. The mean dependent variable is measured in the year before the merger event. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.25: Partial R^2 from Hospital Fixed Effects

Outcome	Restricted RSS	Unrestricted RSS	ΔR^2
Log Negotiated Price	6029.14	2197.49	0.636
Negotiated Price	6968.48	2692.18	0.614

Notes: Restricted Residual Sum of Squares (RSS) is computed from a model with system-year fixed effects only. Unrestricted RSS is computed from a model that additionally includes hospital fixed effects. The ΔR^2 is calculated as $(RSS_R - RSS_U)/RSS_R$.

A.6 Uniform Pricing Within Systems

From the perspective of the theory of mergers across markets, a critical question is whether system-member hospitals negotiate a single payment or transfer to the entire system. As we show in the theory, the standards for harm are much higher when systems negotiate a single transfer, because unifying the transfer helps resolve contracting frictions between hospitals and insurers. In practice, hospitals and insurers contract over prices rather than flat transfers, so to examine this empirically, we ask whether hospital prices are uniform within a hospital system.

We begin by examining prices for a few high-volume, profitable procedures: hip and knee replacements, and deliveries. These procedures account for a sizable fraction of hospital profits and, accordingly, are actively negotiated. We focus on hospital systems with at least 3 members and DRG codes with at least 3 observations per system-year. Figure A.2 shows the distribution of prices for these procedures in our data. In the left panels, we residualize prices on DRG-state-year fixed effects and recenter. Hence, the plotted distribution captures variation across hospitals within a state-year and does not account for system identifiers. If prices were uniform within a system, then residualizing prices on DRG-system-year fixed effects should collapse the distribution to a mass point, up to measurement error. The right panels show that this is not the case. In practice, prices are more homogeneous within a system than across hospitals, but they are far from uniform. Even within a system-year, prices for these procedures can vary by more than 100% across system hospitals. The center mass in the plots accounts for about 25% of the variation in hip and knee replacements and slightly less than 40% in deliveries.

To systematically expand the analysis to the rest of the data, we examine the variation in our negotiated price index explained by the system. Recall that the negotiated price controls for heterogeneity in conditions and population across hospitals, so it is more likely to reflect the true contracted price between the insurer and each hospital. Consider the following two regressions:

$$\ln(p_{ht}) = \delta_{s(h)t} + \epsilon_{ht} \quad (15)$$

$$\ln(p_{ht}) = \delta_{s(h)t} + \gamma_h + \tilde{\epsilon}_{ht} \quad (16)$$

The first regression projects prices on system-year fixed effects, while the second also includes a hospital fixed effect. Under the hypothesis that prices are uniform within a system, the ability of the second regression to fit the price data should be comparable to that of the first. In particular, we can define RSS_R as the residual sum of squares of Equation (15) and RSS_U as the residual sum of squares of Equation (16). We can then evaluate

$$\Delta R_s^2 = \frac{RSS_R - RSS_U}{RSS_R} \tag{17}$$

Under the hypothesis of uniform system-year pricing, $\Delta R_s^2 \approx 0$. If hospital fixed effects explain nearly all of the residual dispersion left after system-year fixed effects, then $\Delta R_s^2 \approx 1$. Table A.25 shows the results for negotiated prices measured in logs and levels. In both cases, $\Delta R_s^2 > 0.6$, which strongly suggests that prices are not uniform within a system. We note that in both of our explorations, we use the system identifiers as developed by Brot et al. (2024), which have been thoroughly validated in the prior literature.

A.7 Network Adequacy

This appendix section describes our construction of network adequacy and our key findings based on this alternative overlap metric. The evidence in this section complements that in the main text and serves as an alternative standard for measuring exposure and potential harm.

The network adequacy standard was introduced to Medicare Advantage (MA) markets in 2011 as a guideline for evaluating whether an insurer's network is sufficiently broad to meet patients' basic demand (Centers for Medicare & Medicaid Services, 2011). The MA network adequacy standard stipulates maximum distances and travel times between enrollees' residences and certain types of providers, with these distances and times varying by the rurality of the enrollee's locality. While, to our knowledge, no legal requirements tie employer-sponsored insurance to these adequacy standards, anecdotal evidence suggests that employers and insurers consult these guidelines when evaluating coverage. One reason for this use is that MA insurance is similar in structure and composition to employer-sponsored insurance, making the MA adequacy standard the closest regulatory standard for network adequacy in the US.

Given that our sample does not include all provider types specified in the MA network adequacy rules, we construct adequacy as follows. An employee is considered to have adequate coverage if they are within a maximum distance of an in-network acute inpatient hospital and an in-network critical care hospital. We compute the distance between patients and hospitals using their ZIP codes. We follow the maximum distance requirements laid out by CMS using the HSD Criteria Reference Tables from 2011.[50] A firm is considered to meet adequacy requirements if more than 90% of all employees living in Metro counties have adequate coverage, and if more than 85% of all employees living in other counties have adequate coverage. Table A.26 reports summary statistics for network adequacy in our sample.

Using network adequacy, we compute how removing a merged system from the data provider's network would affect the network's adequacy for each employer. We measure the *adequacy*

[50] https://web.archive.org/web/20120504005941/http://www.cms.gov/Medicare/Medicare-Advantage/MedicareAdvantageApps/index.html

Table A.26: Employer Network Adequacy Summary Statistics

Statistic	Full sample	≥ 50 employees	≥ 100 employees
Average network adequacy	97.0%	96.8%	96.7%
Share of employers with full adequacy	69.5%	46.5%	32.5%
Share of employers with adequacy < 95%	14.5%	17.2%	17.2%
Share of employers with adequacy < 90%	8.1%	7.4%	6.9%
Share of employers with adequacy < 85%	5.3%	4.3%	3.8%

Notes: Network adequacy is defined as the share of an employer's employees for whom adequacy requirements are met, given the providers included in the network. The average network adequacy row shows the average adequacy among employers. The share of employers with full adequacy measures the fraction of employer-years for which the insurance network is adequate for all employees. The remaining rows measure the share of employer-years for which the network is adequate for less than x% of their employee population, where x ranges from 85 to 95.

leverage of a merger as the share of overall covered employees in the year prior to the merger for whom removing the merged system would turn the data provider's network from adequate to inadequate for their employer. The first column of Table A.27 presents estimates obtained by replacing employer overlap in Equation (7) with adequacy leverage. The table shows that adequacy leverage, like employer overlap, is positively and significantly associated with post-merger price changes in the specification without heterogeneity controls (column (0)). The effect of leverage also decreases with target hospital bed size, acquirer size, the number of non-governmental hospitals, and in rural areas. As for employer overlap, the effect is larger when public hospitals make up a larger share of the target's market competition. The two metrics, however, differ in their connection with income and with the five largest systems in our sample.

In addition to this metric, we compute a Δ adequacy leverage, defined as the difference between the merged system's leverage and the sum of each party's leverage. Theoretically, this is the extent to which the joint system has more leverage on adequacy than the sum of its parts, which should govern its price effects. Table A.28 shows the estimated effect of this Δ adequacy leverage on cross-geography price effects. Since the adequacy leverage of the merged system can be smaller or larger than the sum of its parts, we also present estimates broken out by sign. The results are mixed: when Δ adequacy leverage is not weighted by the employee population, mergers that decrease leverage see larger price increases; in contrast, when weighted, mergers that increase leverage are associated with price increases. Thus, the evidence supports some role for network adequacy in determining prices, but leverage losses are not associated with price reductions, suggesting that adequacy alone does not govern negotiations. Weighted adequacy leverage nonetheless performs very similarly to our employer overlap in explaining merger price increases, offering an alternative measurement approach for practitioners.

Table A.27: Cross-Geography Merger Effects on Prices Using Adequacy Leverage

	Base	Heterogeneity (H)								
		Beds (100s)	Acquirer Size	Largest System	HHI	Non-Gov. Hospitals	Share Gov. Hospitals	Rural	Critical Access	Low Income
	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post × Treated	0.006	-0.065***	-0.021	0.007	0.069**	-0.017	0.019	0.015	0.031**	0.031*
	(0.013)	(0.018)	(0.016)	(0.014)	(0.033)	(0.016)	(0.015)	(0.015)	(0.013)	(0.016)
↪ × H		0.052***	0.000	-0.036	-0.077*	0.012***	-0.076**	-0.022	-0.135***	-0.059**
		(0.008)	(0.000)	(0.043)	(0.040)	(0.004)	(0.030)	(0.025)	(0.035)	(0.025)
↪ × Leverage	12.403***	18.929***	28.827***	13.293***	0.954	16.186***	8.227***	13.828***	9.869***	7.451**
	(2.885)	(4.336)	(6.472)	(3.466)	(5.924)	(3.589)	(3.120)	(3.011)	(2.945)	(3.444)
↪ × Leverage × H		-5.004***	-0.163***	1.998	14.290*	-1.688***	41.725***	-6.867	10.518	11.371**
		(1.534)	(0.059)	(6.998)	(7.930)	(0.551)	(11.254)	(6.065)	(8.940)	(5.190)
Mean dep. var.	-0.570	-0.570	-0.570	-0.570	-0.570	-0.570	-0.570	-0.570	-0.570	-0.570
Mean *H* pre-merger	-	1.337	37.811	0.004	0.711	4.944	0.344	0.435	0.298	0.398
Observations	245,446	244,505	245,172	244,505	245,446	245,379	245,379	245,418	245,446	245,283
R^2	0.792	0.792	0.789	0.792	0.792	0.792	0.792	0.792	0.792	0.792

Notes: Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions include hospital-event and year-event fixed effects. Heterogeneity variables vary by column as indicated. Leverage is the employee-weighted change in employers' adequacy status, as a share of the insurer's total covered population. The mean of Leverage is 0.003 with a standard deviation of 0.003. "Beds" is measured in hundreds. "Acquirer Size" is the number of hospitals in the relevant system. "Largest Systems" is an indicator for the five largest systems. HHI is defined as the sum of squared admission shares within an HSA. "Non-Gov. Hospitals" counts the number of non-government hospitals at the HSA level, while "Share Gov. Hospitals" measures the share of government hospitals within an HSA. Rural and Critical Access are hospital-level indicators of whether a hospital is in a rural area or is designated as a critical access hospital. "Low Income" is a county-level indicator for whether the hospital's county has a lower median household income than the sample median. The mean dependent variable is measured in the year before the merger. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.28: Effect of Δ Adequacy Leverage on Log Negotiated Price

	Log Negotiated Price					
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated	0.039***	-0.006	0.028**	-0.005	0.026**	-0.006
	(0.012)	(0.019)	(0.012)	(0.018)	(0.013)	(0.018)
↪ × Δ	0.007					
	(0.045)					
↪ × Negative Δ		0.059***				
		(0.020)				
↪ × Positive Δ		0.048				
		(0.034)				
↪ × Weighted Δ			44.216**		37.775*	
			(18.881)		(20.610)	
↪ × Negative Weighted Δ				0.036		0.036
				(0.031)		(0.031)
↪ × Positive Weighted Δ				0.062***		0.056***
				(0.020)		(0.020)
↪ × Overlap					1.668	1.928
					(2.757)	(2.635)
Mean dep. var.	-0.570	-0.570	-0.570	-0.570	-0.570	-0.570
Observations	245,446	245,446	245,446	245,446	245,446	245,446
R^2	0.792	0.792	0.792	0.792	0.792	0.792

Notes: Standard errors in parentheses, clustered by hospital. Regressions use analytic event-stack weights following Wing et al. (2024). All regressions include hospital-event and year-event fixed effects. Δ refers to the difference between the adequacy leverage of the merged system and the sum of the separately calculated adequacy leverages of the acquirer and target. "Weighted Δ" is the employee-population-weighted transformation of that measure, shown in columns (3)–(6). The mean dependent variable is measured in the year before the merger. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

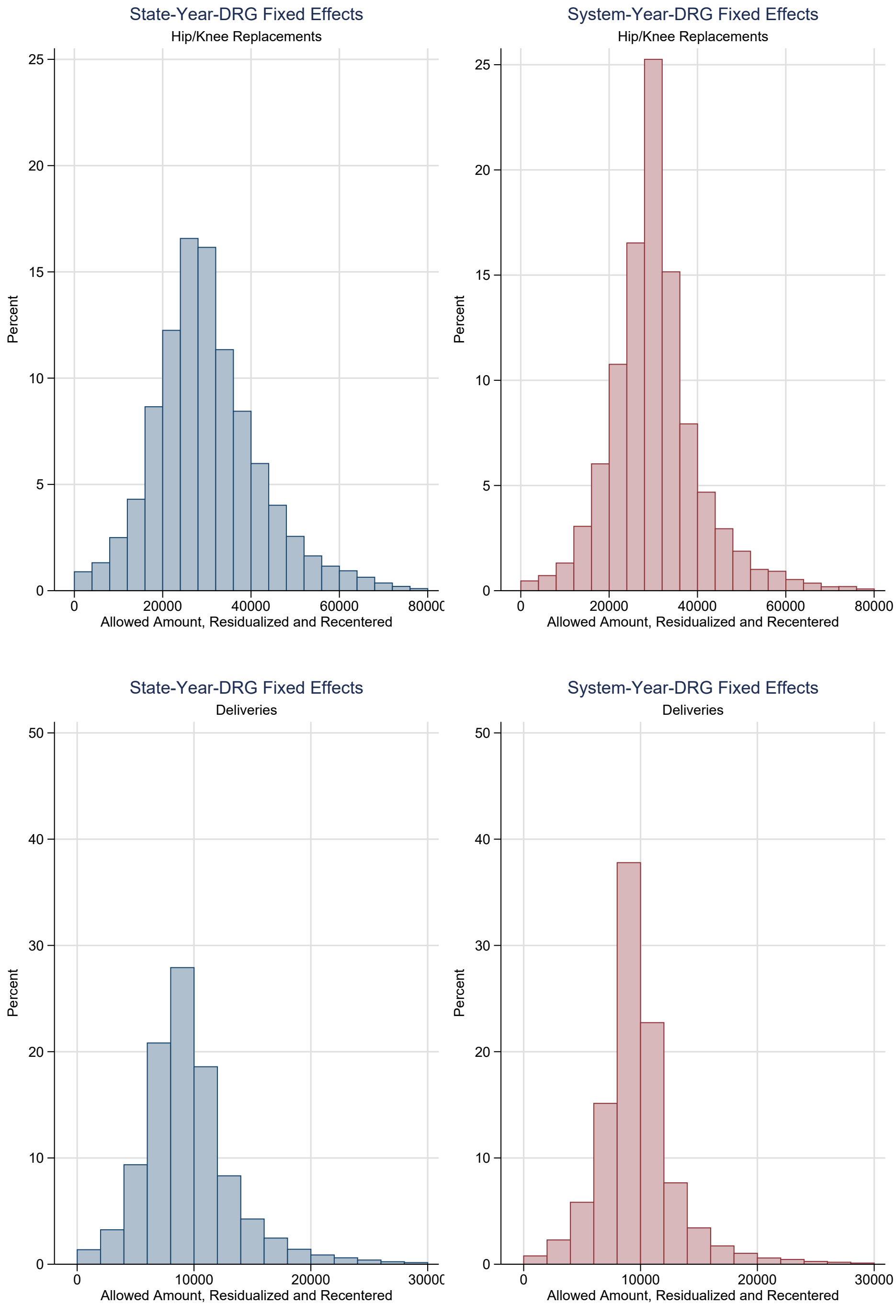


Figure A.2: Price Dispersion Within State-Year vs. System-Year

Notes: This figure reports the distributions of allowed amounts (prices) for hip/knee replacements and deliveries. The left panels show prices residualized and recentered after regressing on state-year-DRG fixed effects; the right panels show prices residualized and recentered after regressing on system-year-DRG fixed effects. We exclude systems with fewer than three member hospitals and DRG-year-system rows with fewer than three observations.