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ABSTRACT

Between 2000 and 2020, the share of US hospital bed capacity under multi-unit firms (systems) increased from 58% to 81% – a rapid corporatization of a sector with $1.3 trillion in annual spend. However, little is known about how system ownership affects hospital profitability and quality. We combine novel, patient-level transaction price data from a large commercial insurer, Medicare claims, and New York hospital discharges between 2012 and 2018 to study changes at over 100 independent hospitals that transition to system ownership. The targets obtain differentially higher prices than a matched comparison group, but the operating cost reductions, primarily obtained by reducing employees in support functions, capital, and financing costs, are far greater and exhibit significant economies of scale with acquirer firm size. In contrast, we detect small and statistically insignificant effects on operating costs at 135 system-owned hospitals acquired by other systems, suggesting that the switch to system ownership is the key to achieving these savings. However, corporatization may worsen quality of care on some dimensions.

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1 Introduction

Do large, multi-unit firms increase operating efficiency while maintaining quality of care? This question lies at the heart of the debate over the rapid growth of large multi-unit chains throughout the US healthcare sector (Dranove and Burns 2021). Prior evidence on chain ownership has shown that standalone establishments gain several benefits from joining chains, such as a reputable brand name, access to capital, knowledge of best practices, and improved chances of survival (Ingram 1996; Kalnins and Mayer 2004; Foster, Haltiwanger, and Krizan 2006). An opposing view holds that large firms leverage their market power to increase prices, but do not commensurately improve efficiency or quality, and may even reduce quality while pursuing cost reductions (Gaynor and Town 2012; Eliason et al. 2020). Since product quality is less transparent in healthcare than in hotels or restaurants, the rise of chains deserves greater scrutiny in this sector. Surprisingly, there is limited recent evidence on the causal effects of chain ownership in healthcare. This paper begins to fill this gap in the context of the US hospital industry.

The hospital industry is an excellent setting to study this question. First, multi-unit firms, known as hospital "systems," have rapidly expanded and now dominate the industry – the share of national bed capacity under system control increased from 58% in 2000 to 81% by 2020 (Figure 1 Panel (a)), with similar growth in the share of employment.[1] They have done so primarily by acquiring independent hospitals, a phenomenon we refer to as "corporatization."[2] Hence, we can study a large number of events in which independent hospitals enter systems. The median acquirer in our sample already owns six hospitals, with an aggregate bed capacity that is more than ten times that of the independent hospital it targets. Formerly independent hospitals experience a large change in operations when they enter a system across a broad set of domains, including price setting, procurement, personnel, and clinical care (Bazzoli et al. 2002; Burns et al. 2015).

Second, our understanding of the effects of *system* ownership remains limited. Data on negotiated hospital prices has typically been unavailable for research, making it nearly impossible to examine price effects accurately until recently. Several studies have quantified cost or quality economies of scale with respect to hospital size or when two standalone hospitals merge, but not due to variation in the parent's size (Connor, Feldman, and Dowd 1998; Dranove 1998; Gaynor, Seider, and Vogt 2005; Gowrisankaran, Ho, and Town 2006; Harrison 2011). Alternately, studies have quantified the effects of hospital consolidation while remaining agnostic to the nature of the transition, thus obtaining an average effect across myriad disparate changes (Schmitt 2017; Cooper et al. 2019; Beaulieu et al. 2020; Craig, Grennan, and Swanson 2021).[3] Finally, most

1. There is sometimes confusion between the terms health "system" and health "network." Both refer to alliances or contractual affiliations of hospitals and other organizations (e.g., nursing homes). The key distinction between the two is that systems have unified asset ownership of all affiliated entities, while networks typically have diversified ownership (Bazzoli et al. 1999). We focus exclusively on systems and follow this interpretation.

2. We use this term to signal the change in sophistication of management and operations at the target hospital once it joins a system, and not potential changes in its profit status. Only about 5% of the transitions involve a change in profit status.

3. Studies have typically pooled together deals as disparate as individual hospitals merging into one facility, individual hospitals coming together to create new systems, existing systems acquiring or merging with other systems,

studies have focused on one type of outcome – price, cost, or quality – and thus have not been able to examine the trade-offs involved between cost and quality or the pass-through from cost to price. This paper attempts to overcome all of these limitations, as we describe below.

We use patient-level claims data from the second largest private health insurer in the US, Elevance Health, which allows us to observe patient-level transaction prices for hospital inpatient services in 20 large states over 2012–18. Further, we combine data from three other complementary sources to study the effects on costs and quality. We use annual surveys from the American Hospital Association (AHA) to track changes in hospital ownership, capital and labor inputs, operating expenses, and service portfolio breadth. To ensure a broad examination of changes in hospital quality, we also deploy Medicare fee-for-service claims and all-payer hospital discharge data from New York state over the same period, in addition to our base sample of commercial insurer claims. We supplement these principal data sources with various public use files made available by the federal government.

To estimate causal effects of system ownership, we exploit the panel nature of the data and study 101 acquisitions of independent hospitals by systems from 2013 to 2017. We use a staggered difference-in-differences research design and compare trends for acquired facilities to those for hospitals that remained independent.[4] There is substantial variation in target, acquirer, and deal attributes, allowing us to also examine heterogeneity along several dimensions of interest. This approach is standard in the literature on ownership and consolidation in healthcare (Schmitt 2017; Eliason et al. 2020). That being said, we recognize the limitations of the design and proceed with caution, paying careful attention to the presence of differential trends at the acquired hospitals prior to the deal year. We strengthen the design further by using only a subset of the comparison hospitals that match the acquired hospitals closely on key attributes prior to the acquisition.[5] Finally, we contrast these results against those obtained from a companion analysis for previously system-owned hospitals acquired by other systems. These hospitals also experienced a transfer of ownership to a larger acquirer, but did not experience corporatization since they already enjoyed the benefits of system ownership. Comparing the two sets of results helps us disentangle the effects of corporatization from those due to a change in ownership alone.

We estimate an increase of about 6% in mean reimbursement per inpatient stay for commercially insured patients following system ownership. Our models control granularly for changes in procedure intensity, so we interpret this effect as the increase in price for performing a similar procedure. This average effect masks substantial variation across service lines. The price increase varies from negligible to 11% across the top 7 specialties by volume, with stays for res-

and systems acquiring one or more independent hospitals. As we discuss below, the effects of these deals can differ substantially.

4. We use multiple sources of data to identify transitions to system ownership, which we then also manually validate. We exclude cases of "network affiliations" and only consider those cases where the independent hospital's *ownership* changes hands. We also exclude cases where two hospitals merge into one legal entity and where multiple independent hospitals join forces to create a new system.

5. We match hospitals using coarsened exact matching and data from the year prior to the acquisition on number of beds, rural county location, non-profit status, Medicaid and Medicare share of patient admissions, and total operating expenses. For operating expenses, we match on values in the last two years prior to acquisition.

piratory, central nervous system, and cardiac diseases experiencing the largest increases. There is little evidence of heterogeneity in price effects by type of target or acquirer. Consistent with economic theory and prior evidence, within-market deals lead to about a 25% greater increase in price than the average, though the difference is not statistically significant. Assuming this estimate is representative of price increases across all private insurers, we estimate an increase in inpatient hospital revenue of about $11,700 per bed by applying the corresponding average number of commercially insured stays and bed size.[6]

Total operating expenses decline by about $48,300 per bed (about 4.8%) at the acquired hospital following system ownership, without any offsetting increases at the acquirer. There are two main drivers. First, we detect a large reduction in capital and financing costs, accounting for about 20% of the total savings, though they contribute only 8% to the cost base. Second, we detect a reduction in personnel expenses (salaries and benefits), which accounts for about 60% of the total decline in cost. Again, this is disproportionate to their share of the cost base of 50%. The decline in personnel spend is explained mostly by a reduction in employment. A key mechanism appears to be a reduction in the number of employees in support and back office functions, though we also find a modest decline among clinical staff.[7] Patient volume at the acquired hospitals remains unchanged despite the reductions in labor inputs, implying system ownership enables the target hospitals to maintain service levels while restructuring. This pattern is consistent with theories of management predicting that multi-unit firms enjoy scale and scope economies, leading to cost and marketing advantages (Dranove and Shanley 1995). In further evidence supporting scale economies, non-parametric evidence suggests a nearly linear relationship between estimated cost savings and acquirer size.

Considering the effects on revenue and expenses together, we estimate an increase in hospital operating profit of about $60,000 per bed per year, about 6% of the operating expense for the average targeted hospital. The price changes in our estimation affect only commercially insured patients, while changes in labor and capital costs affect all patients. Notwithstanding such large cost savings, average prices for commercial patients increase following system acquisitions, prompting the question of whether consumers benefit from cost savings at all.[8] We explore the interaction between cost savings and prices and detect a modest negative correlation between the two across the 101 deals, consistent with pass-through of savings to insurers. Specifically, our estimate implies that for every $100 more in operating expense reduction, hospitals accept a lower price increase equivalent to $8 in lost commercial revenue. Perhaps reflecting the fact that labor costs account for much of the total saving, the correlation is much stronger between

6. This may be a conservative estimate of the increase in revenue for at least two reasons. First, prices may also increase for outpatient care, which is not accounted for here. Second, the hospital may change its service or payer focus toward more remunerative patients.

7. We detect a 14% reduction in licensed practical nurses (LPNs), as well as a decline in a composite group that includes social workers, technicians, and aides. We find no change in the use of contract workers. We also estimate a decline in mean compensation per employee, but it is not statistically significant.

8. We only consider the increase in commercial inpatient revenue in this calculation. If we assume a similar increase in commercial outpatient prices (which we do not observe), we predict an increase in commercial revenue of about $26,000 per bed instead. However, cost savings would still account for more than half of the aggregate increase in profitability.

reductions in personnel expenses and lower commercial prices (about 0.3). This relationship is also remarkably linear across different levels of cost savings.

Hospital quality is multi-dimensional and notoriously difficult to measure. We examine standard performance measures from the literature and used by the federal government to incentivize hospitals — short-term readmissions, mortality rates, and patient satisfaction scores. We find a robust and statistically significant increase in readmission rates following system ownership, but we fail to reject null effects on mortality rates and patient satisfaction scores. Reassuringly, these patterns are consistent across different patient samples, implying the results reflect hospital-wide changes in quality. Deal-specific estimates suggest that target hospitals experiencing larger cuts in staff also experience greater increases in readmission rates. This evidence may not imply causality, but it suggests that the cuts in staff can explain about 40% of the estimated increase in readmission rates for the commercial patients. This is consistent with prior evidence in health economics linking staff inputs, including non-clinical staff, to readmission rates (Boutwell, Johnson, and Watkins 2016; Jenq et al. 2016; Evans et al. 2021).

Our estimates remain similar in magnitude when subjected to a battery of robustness checks to assess concerns about identification and alternate explanations. A companion exercise examining the effects for previously system-owned hospitals helps put in perspective the changes discussed above for independent hospitals. We find a similar magnitude price increase following these "non-corporatization" deals, suggesting that system ownership does not confer an unusual price negotiation ability on independent hospitals, and that the price effect may be driven more by changes in market power. In contrast, we find small and statistically insignificant effects on operating costs for this sample, including no effect on employment or personnel spending. This is consistent with the interpretation that transitioning to system membership for the first time, and not just transitioning ownership, enables the large reduction in labor inputs. Consistent with the hypothesis that the reduction in labor inputs is an important driver behind the increase in readmission rates for the main sample, we find no effect on readmissions for these targets.

This article contributes to three distinct literatures. We add to the growing literature on corporatization in the healthcare sector. The recent growth of large multi-unit firms and new forms of financing and delivery of medical care have attracted considerable attention, mainly due to the potential for harm to patients in the drive for profit maximization (Eliason et al. 2020; Gandhi, Song, and Upadrashta 2020; Gupta et al. 2021; La Forgia 2023; Richards and Whaley 2023). Although the growth of chains has been a defining feature of the hospital industry, it has been understudied. To our knowledge, the last study to quantify the effects of hospital system ownership on prices, costs, and quality examined deals ending in 2000 (Cuellar and Gertler 2005).[9] Besides providing a much needed update, our main contribution is to demonstrate (with

9. An active literature in the 1990s and early 2000s studied the effects of system ownership. However, the studies suffered from several limitations including cross-sectional designs, limited geographic scope, and/or lack of data on transaction prices, patient-level utilization, and health. Several prior studies on system ownership focused on California: Dranove and Shanley (1995), Lynk (1995), Dranove, Durkac, and Shanley (1996), and Dranove and Ludwick (1999) studied system behavior cross-sectionally; Young, Desai, and Hellinger (2000) studied price effects due to deals over 1990–95; Krishnan (2001) studied price effects of across-market acquisitions over 1994–95; and Ho and Hamilton (2000) studied quality effects for deals over 1992–95. Menke (1997) performed a cross-sectional

nuances) the mechanisms that systems use to realize cost savings, such as reducing employment in support functions and lowering capital and financing costs. These mechanisms lend themselves to scale economies, and we show that the savings increase in magnitude nearly linearly with the acquirer's size. These patterns suggest that hospital systems are akin to the "superstar" firms hypothesized by Autor et al. (2020), with low labor shares and high profit margins. Accordingly, their growth has coincided with a large reduction in the aggregate labor share for the hospital industry.

A large adjacent literature has examined the effects of horizontal consolidation in hospital markets.[10] Although voluminous, the literature has typically focused on one-to-one hospital "mergers," defined by Dranove and Lindrooth (2003) as deals in which two or more hospitals merge into a single operating license and report finances jointly (e.g., Connor, Feldman, and Dowd 1998; Dafny 2009; Gaynor, Laudicella, and Propper 2012), or has estimated an average effect pooling across multiple deal types (e.g., Spang, Arnould, and Bazzoli 2009; Schmitt 2017; Cooper et al. 2019; Beaulieu et al. 2020; Craig, Grennan, and Swanson 2021). In contrast, our analysis suggests policy-relevant heterogeneity in treatment effects even within acquisitions by systems. For example, we find that system acquisitions of independent hospitals lead to reductions in labor intensity and associated cost savings, but that these benefits are not repeated when a system-owned hospital experiences another acquisition. Thus, the latter type of deal lacks even the *potential* to lower costs for consumers and may therefore warrant greater scrutiny from antitrust regulators. This corroborates the conclusions of Gaynor et al. (2021), who examine a single large deal involving the acquisition of one system by another. We also test for and directly confirm the presence of pass-through of these cost savings toward prices. To our knowledge, this is the first study to document feedback between cost savings and prices in the hospital industry.

More broadly, our results extend the literature studying the effects of multi-unit firms across sectors, which has tended to focus on sectors such as retail, hotels, and restaurants (Kalnins and Mayer 2004; Foster, Haltiwanger, and Krizan 2006; Kosová and Lafontaine 2012; Arora et al. 2023). This literature has previously emphasized process standardization and the use of technology, particularly in franchised chains, to drive productivity improvement. Our results suggest important roles for centralizing support functions away from individual establishments to reduce employment and leveraging scale to lower financing and capital costs for system-owned facilities. We hesitate to interpret the cost savings as evidence of productivity enhancement due to the effects on readmission rates.

The paper is organized as follows. Section 2 provides a brief background on the hospital industry. Section 3 describes the data and preliminary evidence. Section 4 presents the empirical strategy and descriptive evidence. Sections 5 and 6 present the results on profitability and quality, respectively. Section 7 tests robustness. Section 8 discusses the implications of our results and concludes.

analysis using national data from 1990.

10. See Gaynor, Ho, and Town (2015) or Handel and Ho (2021) for comprehensive reviews.

2 Background

2.1 Hospital Spending and System Expansion

Hospital care is the largest segment in the $4.3 trillion US healthcare sector, with $1.3 trillion in annual spending. Despite a decline in inpatient volume over the last decade, hospital spending has remained stable as a share of total spending at about 31%, suggesting offsetting rapid growth in prices.[11] Data from the Bureau of Labor Services (BLS) confirms that hospital care experienced the highest growth in consumer prices not only within the healthcare sector, but also across *all* sectors of the economy over the last two decades (see Figure A.1 Panel (a)). Consumer prices for hospital care grew much faster than in other segments of healthcare: nearly 60% faster than prices for prescription drugs and twice as fast as those for physician services. BLS data combines prices paid by both private and public insurers and therefore understates the growth in prices faced by privately insured patients.[12]

Two important and complementary trends in hospital markets may have contributed to the rapid growth in hospital prices. First, multi-unit firms or hospital systems have rapidly expanded their footprint by acquiring independent hospitals. It is important to define terminology here. The terms health systems and health "networks" are sometimes used interchangeably. However, the AHA and the management literature observe an important distinction between the two. Both refer to strategic and contractual affiliations of hospitals and other organizations (e.g., nursing homes, ambulatory surgical centers). However, while networks typically have diversified ownership of assets and can be mere collaborations, systems have unified asset ownership over all affiliated entities (Bazzoli et al. 1999). Figure 1 Panel (a) presents the national aggregate share of bed capacity and total employment under the control of systems over 2000–20 using data from the AHA annual survey files. Systems controlled 58% of total US beds in 2000, increasing to 81% in 2020. Their share of total hospital employment is slightly lower but has increased at a similar pace. These statistics showcase how hospital capacity has been rapidly brought under the control of large corporate firms. We refer to the growth of systems through the acquisition of independent hospitals as "corporatization."

Figure 1 Panel (b) captures this phenomenon through a different lens, highlighting its geographic reach. The proportion of hospital markets without a *single* independent hospital increased from 7% to 25% over this period (blue diamonds). We assign hospitals to markets using Hospital Referral Regions (HRRs), as defined by the Dartmouth Atlas. By 2020, 75% of markets had over half of their hospital bed capacity controlled by their two largest systems, nearly double the rate in 2000 (green circles). The trends in each of these series accelerated after 2010, coinciding with the acceleration in hospital price growth reported by the BLS.[13] Corporatization

11. Source: National health expenditure table for calendar year 2021, reported by the department of Health and Human Services (HHS).

12. Public insurers unilaterally set prices based on costs and inflation, while private insurers negotiate prices with hospitals, subjecting them to market power and other bargaining considerations. Private insurers experienced faster growth in hospital spending relative to Medicare, even though private insurance enrollment remained flat (NHE 2020). Hence, price growth for private insurers alone exceeded the sum of price and volume growth for Medicare.

13. This analysis almost surely understates the extent of corporatization since patients likely do not consider hos-

has not yet run its course. There were about 500 independent privately owned hospitals in the US in 2020 per the AHA; they remain targets for future acquisitions.

Second, concentration in hospital markets also increased over this period. Figure 1 Panel (c) presents the mean Herfindahl-Hirschman index (HHI) across all HRRs over 2000–20.[14] Mean HHI increased by more than 1,000 points over this period. This is approximately equivalent to the average market transitioning from seven equally sized competitors to four. By the end of our sample period, the average hospital market would be considered highly concentrated per merger guidelines issued by the federal antitrust agencies (DOJ 2010). Like the growth in hospital prices and corporatization, this trend also accelerated after 2010. Note that corporatization does not necessarily increase market concentration. Half of the deals in our sample involved the acquisition of an independent hospital located in a different HRR from the acquiring system and therefore did not affect concentration in the target hospital's market. We exploit this source of variation in our empirical analyses.

2.2 Prior Evidence on System Ownership

Economic theory predicts that system ownership should increase hospital prices. Lewis and Pflum (2015) examine the effects of system ownership on hospital profitability using a Nash-in-Nash bargaining model. Under this framework, systems can increase negotiated prices, holding quality constant, through two channels. When a system acquires a hospital that competes for the same patients, it strengthens the acquirer's bargaining position or leverage. The insurer can no longer use the target hospital as a substitute for the acquirer's other hospitals and is therefore willing to accept a greater equilibrium price. This is the canonical concern in antitrust actions that seek to block horizontal consolidation. Prior studies have typically assumed that hospitals compete for the same patients only when they are located in the same market. However, Dafny, Ho, and Lee (2019) argue and show evidence to support the hypothesis that acquiring a hospital in a different market may also improve a system's bargaining position with an insurer, if there are large employers with employee presence in both markets that negotiate contracts considering the insurer's network across markets.

System ownership may also increase the target hospital's bargaining power or weight. Systems may possess larger and more skilled contract negotiating teams and pool information about the insurer across multiple markets (Benko 2003; Colias 2006). They may be less risk averse, for example being willing to threaten to terminate negotiations (Lowes 2008). These factors tend to increase the target hospital's bargaining power, allowing it to extract a higher proportion of the joint surplus from the contract, implying higher prices. This channel operates even when the target hospital is located in a different market without any patient or employer overlap.

While there is substantial empirical evidence on the price effects of hospital consolidation,

pitals over such a large area for acute care or routine procedures. HRRs are very large and, on average, span 10 counties and contain about 20 hospitals.

14. If an independent hospital is bought by an out-of-market firm, then the market HHI will not change since firm-level concentration in the market has not changed. Following similar logic, if a hospital is sold by an existing system to an out-of-market firm then concentration could decrease.

very few studies have focused specifically on the effects of system ownership. To our knowledge, Lewis and Pflum (2017) is the only recent study to quantify the effects of acquisitions of independent hospitals by systems. However, since their focus was on cross-market acquisitions, they primarily characterize the effects following those deals. They report large price effects of 10% or more following acquisitions over 2000–10. A key limitation is that they use aggregate data obtained from hospital cost reports filed with the federal government and do not observe negotiated prices between hospitals and private insurers. Previously, Cuellar and Gertler (2005) examined the effects of system ownership on prices and detected an increase of 4–8%. However, they also lacked transaction data and relied on hospital chargemaster rates.[15] Recent work has shown that such imputed prices are only weakly correlated with true transaction prices (Darden, McCarthy, and Barrette 2023).

Being able to negotiate higher commercial prices is only one of the changes a hospital experiences when it enters a system and may not be the most important one. In theory, membership in a large, multi-hospital firm could confer benefits in reputation, managerial and clinical practices, access to capital and technology, the ability to attract and retain personnel, and a more efficient cost structure due to economies of scale and scope (Dranove and Shanley 1995; Burns et al. 2015). The wider literature on the effects of multi-unit firms or chains in other sectors suggests chains improve labor productivity, expand the use of information technology, standardize processes, and reduce logistics costs in retail trade, hotels, restaurants, and other service industries (Baum and Ingram 1998; Kalnins and Mayer 2004; Foster, Haltiwanger, and Krizan 2006; Holmes 2011; Kosová and Lafontaine 2012). Chains may increase the efficiency of labor inputs in hospitals, resulting in a decline in spending on labor, or the labor share. The hospital industry has, indeed, experienced a substantial decline in the average labor share over this period, which we measure as the share of total operating expenses contributed by spending on personnel and benefits. Figure A.1 Panel (b) presents the trend in the mean labor share across all hospitals over 2000–20. It was relatively stable over 2000–10, decreasing only by 1 percentage point. However, it dropped further by 4 percentage points over the next 10 years. The rapid decline of the labor share coincided with the period of growth for systems, as seen in Figure 1 Panel (a).

Surprisingly, we know little about the effects of corporatization for the target hospital on both operating costs and quality of care. An active literature in the 1990s examined this question and reached mixed conclusions, but it was hampered by lack of data, limited geographic coverage, or cross-sectional research designs (Dranove and Shanley 1995; Lynk 1995; Dranove, Durkac, and Shanley 1996; Dranove and Ludwick 1999; Menke 1997). In general, studies have primarily focused on deals involving two independent hospitals in the same local market either merging into one entity or forming a new system, (e.g., Connor, Feldman, and Dowd (1998), Dranove (1998), Dranove and Lindrooth (2003), and Harrison (2011)). Consequently, the key mechanisms hypothesized, such as reorganizing hospital departments to reduce excess capacity, may not apply when systems (often not operating in the local market) acquire a hospital.

15. Young, Desai, and Hellinger (2000) and Krishnan (2001) are two older studies that also examined price effects of system acquisitions using similar imputed prices.

More recent studies have broadened the scope to include multiple types of deals, including acquisitions by systems, but have not specifically reported the effects of corporatization. Among these, Schmitt (2017) finds a 4-7% reduction in operating costs on average following hospital consolidation, but does not explore the mechanisms producing these efficiencies; Craig, Grennan, and Swanson (2021) study the effects of consolidation on the costs of medical devices and supplies and find a modest reduction.

The effect on quality is a critical parameter for policymakers considering whether to counter further expansions of system ownership. Even if the effect on prices could be countered through other policy tools (e.g., price regulation), consumers could be worse off if hospital quality suffers. However, the evidence on the effects of system ownership on hospital quality is limited, with greater focus on mergers of two facilities into one. Ho and Hamilton (2000) find an increase in readmission rates for cardiac care patients following system acquisitions.

For completeness, we also summarize the arguments made by industry participants in favor of corporatization. Based on our review of press releases and trade articles on the deals studied in this paper, three main benefits are claimed. First, independent hospitals expect that that they will obtain easier access to capital for capacity, service expansions, and upgrades (including the adoption of sophisticated IT platforms) once they are part of a larger corporate entity. Second, they anticipate reducing operating costs by leveraging the system's scale. This is particularly relevant in the case of procurement costs (e.g., medical supplies and devices). Third, they believe that they will benefit from having access to a larger and potentially better pool of managerial and clinical talent at the system. These claims largely mirror those previously reported by surveys of hospital administrators (Bazzoli et al. 2002).

As an illustrative case study, consider the 2015 acquisition of Northern Westchester hospital (NWH) in Westchester county, New York, by Northwell, the largest hospital system and private employer in the state. This deal is representative of the "average" corporatization deal in our sample based on scale, intent, and other dimensions.[16] NWH had been run independently for nearly a century at the time of the deal. Based on federal tax filings, it was performing well financially with growing patient revenue (approximately $245 million), stable expenses, and a positive net margin of around 3-4% (NWH 2014). Still, the deal was justified as necessary to improve NWH's financial performance and quality of care. For example, the CEO described the deal saying, "NWH will directly benefit from stronger clinical information systems, economies of scale, access to capital, and the sharing of knowledge available from a system of community hospitals anchored by well-established academic medical centers" (Ellison 2014). NWH leaders anticipated that Northwell's scale would help it adapt to the "monumental changes currently underway in the healthcare industry" (Donnelly 2014). Northwell is a larger organization with vastly more resources than NWH. In 2014, Northwell earned $7.4 billion in revenue (about 30x NWH) and managed eight general acute care hospitals (NSUH 2014; LIJMC 2014; Northwell

16. The deal was particularly appealing as an example since both the acquiring system and target independent hospital are similar in size to the median values of hospitals in our sample in terms of beds and number of hospitals, respectively. The deal was cross-market. Finally, the target hospital is located in New York, an important state in our analysis.

2015).

However, publicly available evidence of the acquisition's expected benefits is inconclusive. The anticipated capital infusions from Northwell did not materialize in a significant way, although NWH did receive access to expert physicians employed at Northwell's academic medical centers.[17] Financial filings suggest that system ownership quickly improved mean reimbursement rates at NWH, but the effects on quality and efficiency are not obvious. This type of ambiguous evidence has led to a debate over the role of hospital systems, and more generally of corporatization, in healthcare.

3 Data

3.1 Data Sources

Our analysis primarily relies on four data sources. First, we use administrative claims data over 2012–18 from Elevance Health, one of the largest health insurers in the US. In 2018, Elevance, then known as Anthem Inc., served approximately 40 million enrollees or members across employer-sponsored (ESI), individual, and public insurance plans (such as Medicaid and Medicare). We focus on individuals enrolled in ESI and Medicare Advantage (MA) plans, which account for about 80% of enrollees (Anthem Inc 2018).

The data are very rich in detail and available at the level of each healthcare encounter, similar to other standard insurer claims data sources used by researchers. Unique IDs for each enrollee and hospital allow us to follow patients over time across different hospitals. Crucially, the data contain the aggregate payment amount made to the provider as well as its components. We focus on the amounts paid by the insurer for inpatient stays. We sometimes refer to this as the price per inpatient stay.[18]

Elevance markets health insurance plans in 14 states where it builds and maintains provider networks and directly negotiates prices. To provide services to members located in other states, it leverages its association with partner firms in the Blue Cross Blue Shield (BCBS) association.[19] Our analysis sample includes members located in all 14 states and the 6 external states with at least 300,000 members annually during the study period. Collectively, these 20 states include 9 of the 10 largest US states by population; they account for more than two-thirds of total US population and over half of US hospital markets (173 out of 306 HRRs). The states

17. NWH opened a new surgery center in 2016 with funding from several sources such as the state, debt, NWH funds, philanthropy, and Northwell (Mullin 2016). The center may have been planned prior to the acquisition since hospitals have to obtain certificate of need approval from the state. NWH further opened a $4 million cardiac catherization laboratory in 2020 (Mullin 2020). The lab is led by physicians from Northwell's academic medical center, suggesting that access to physicians is perhaps as important as that of capital.

18. For each healthcare encounter, we observe the healthcare provider, patient demographics, dates of service, diagnosis and procedure codes, patient age and gender, and billing codes such as DRG and CPT. We also observe plan enrollment and zip code of residence. We observe the amount paid to the hospital as the facility fee, which represents about 85% of the total allowed amount, and the corresponding amount paid to physicians as professional fees. Table A.1 lists the top DRGs in the sample.

19. In these states, the commercial insurer abides by the contract terms negotiated by its partner firms with their network of healthcare providers.

represent all 9 Census divisions and span the spectrum of regulatory preferences, as reflected in the cost of doing business.[20]

Commercial insurer claims are perfectly suited to study hospital price setting, but are limited when the objective is to study hospital quality. ESI members are typically healthier than other patient segments such as Medicare and Medicaid and contribute a relatively small proportion of a typical hospital's patient volume. To study quality, we therefore complement commercial claims with Medicare claims and New York all-payer hospital discharge data. Both sources have been frequently used in the literature to study hospital quality (Chandra et al. 2016; Silver 2021).

Traditional Medicare is the single largest payer of hospital admissions in the US, accounting for about 30% of all hospital admissions.[21] We use a 100% sample of hospital inpatient claims for fee-for-service Medicare beneficiaries over 2009–17. We limit the sample to the 20 states in the commercial insurer data to make the results relatable to those in the price analysis. Medicare claims are the only data source that allow us to examine the effects on mortality beyond the hospital stay.

The New York data allow us to observe all hospital admissions across payers and ages, providing a comprehensive picture of hospital quality, albeit for a comparatively small sample of hospitals. New York state is also uniquely suited to our analysis. Elevance is an important player in New York's commercial insurance market, creating substantial overlap between the claims data and hospital discharge files. New York also experienced the second highest number of hospital deals during this period among the states in our sample, maximizing statistical power to estimate precise effects of system ownership. Across the different patient files, we organize the analysis sample around "index" inpatient stays. We observe patients' medical utilization history in the year leading up to the index hospital stay, as well as for 90 days following their discharge.

The last principal data source is annual survey data from the American Hospital Association (AHA). We use these files to obtain information on hospital location, owner type (public, for-profit, or non-profit), system membership, size, service portfolio, finances (e.g., operating expenses, depreciation), and personnel over 2010–18. The data on service portfolio, capital spending, and personnel has been extensively used to study changes in hospital performance (Finkelstein 2007; Acemoglu and Finkelstein 2008; Prager and Schmitt 2021). A key limitation

20. The 14 plan states are California, Colorado, Connecticut, Georgia, Indiana, Kentucky, Maine, Missouri, Nevada, New Hampshire, New York, Ohio, Virginia, and Wisconsin. The 6 external states are Florida, Illinois, New Jersey, North Carolina, Pennsylvania, and Texas. All 9 Census divisions are represented: New England (CT, ME, NH), Mid-Atlantic (NY, NJ, PA), South Atlantic (NC, VA, GA, FL), East South Central (KY), West South Central (TX), East North Central (OH, IN, IL, WI), West North Central (MO), Mountain (CO, NV), and Pacific (CA). As an example of the diversity in regulatory preferences, see https://www.cnbc.com/2021/07/13/americas-top-states-for-business.html. Ohio is ranked 2^{nd} on the costs of doing business among all states, while California is ranked 47^{th}. These subjective rankings differ considerably across different sources; for example, see here, but suggest these states span the spectrum of regulatory preferences.

21. According to the Agency for Healthcare Research and Quality (AHRQ), there were 36 million total hospital discharges in the US in 2012. The Medicare 100% claims data recorded about 11 million hospital admissions by fee-for-service beneficiaries in 2012, or 30% of the total. AHRQ also reports that all private insurers collectively had 11.2 million discharges.

of these data is that, while we can observe price and quality measures by specialty in the claims data (e.g., cardiac care), inputs can only be observed at the hospital level. Although the AHA survey is national, we limit the sample to hospitals in the 20 commercial insurer data states, so that estimates on all key dimensions — price, cost, and quality — pertain to the same hospital sample.

We use information recorded in the AHA surveys to infer changes in a hospital's system ownership over time. As we noted previously, the AHA records system ownership distinctly from network affiliation. We focus exclusively on system ownership.[22] We aim to study corporatization, which we define as the acquisition of independent hospitals by existing systems. Accordingly, we exclude deals which do not meet this description. This includes cases where one hospital operationally merges into another, or where two or more independent hospitals join forces to create a new system. We augment this information with deals reported by Irving Levin, a market research firm that compiles a proprietary database of M&A deals in the healthcare industry. Both AHA and Irving Levin are frequently used for this specific purpose (Lewis and Pflum 2017; Cooper et al. 2019). Finally, we manually validated each deal in our sample through Internet searches of public (hospital websites, press releases, and news articles) and proprietary sources (American Hospital Directory). The validation exercise helped correct the deal year in several cases, but largely helped confirm the veracity of AHA data. Appendix A.1 provides more details on the data sources and sample construction.

Apart from these principal data sources, we also deploy public use data released by the Centers for Medicare and Medicaid Services (CMS). We obtain data on subjective patient assessments of hospital quality from surveys recorded in the Hospital Consumer Assessment of Healthcare Providers and Systems (HCAHPS) files available on the federal Hospital Compare portal. We also use information on hospital revenue and overhead staff from the Healthcare Cost Report Information System (HCRIS), often known as the Medicare cost reports. Finally, we use information from the US Census to develop county-level covariates.

3.2 Descriptive Evidence

We begin by describing the variation in our key outcomes of interest across hospitals by the size of their system in 2012, the first year we observe most hospitals. We consider three outcomes: mean price for an inpatient stay paid by the commercial insurer, total operating cost per bed, and the 90-day readmission rate among commercially insured patients following an inpatient stay for cardiac care. This descriptive exercise helps visualize patterns in the raw data relatively non-parametrically. It also has the virtue that we can use the cross-section of our entire sample of nearly 1,650 hospitals observed in the commercial claims data: about 1,150 system-owned hospitals and 500 that were independent at the start of the sample. To control for differences in market-level factors and patient mix across hospitals, we first estimate the

22. We disregard network affiliations since these may reflect collaborations, rather than ownership. We also disregard consulting or management firms that help independent hospitals improve operations and are sometimes incorrectly reported as systems in the AHA (e.g., Quorum Health Resources).

following predictive models for each outcome using hospital-level data:

$$(1) \qquad Y_h = \alpha_m + X_h'\delta + \xi_h,$$

where h and m denote hospital and market (HRR), respectively. Y_h is the outcome of interest. We obtain the residuals, ξ_h, after accounting for differences in outcome levels between markets, α_m, and observed differences in patient mix and treatment intensity, X_h. Market fixed effects eliminate differences that may creep in because system-owned hospitals are disproportionately located in higher priced or cost markets (e.g., urban markets). Similarly, the vector X helps eliminate differences in patient risk. We then use the residuals, ξ_h, in a second step to investigate the relationship between adjusted hospital performance and firm size, measured by the number of member hospitals (independent hospitals are assigned a firm size of 1). To facilitate interpretation of the coefficients, we express them as a percentage of the standard deviation of the outcome variable.

Figure 1 Panels (d)–(f) depict the association between the hospital residuals for each outcome and system size, measured by the number of hospitals in the system. Each figure plots the means of the outcome residuals in decile bins on the Y-axis against the corresponding mean firm size values on the X-axis. There is a right skew in system size, but the median system owns only 4 hospitals. Therefore, we plot the system size on a log scale for expositional clarity. Since there is a mass of hospitals at $n = 1$, values are plotted only for eight distinct bins. The figures also overlay a linear fit obtained from a linear regression estimated on the underlying hospital-level data to formally illustrate the underlying correlation. The corresponding slope coefficient and standard errors are also presented. To account for estimation error in the first step, we bootstrap over both steps to obtain standard errors. We exclude the two largest systems from the sample for this exercise since they are outliers in firm size.[23]

Figure 1 Panel (d) presents the association between mean hospital price and system size. The figure suggests prices are modestly higher for hospitals belonging to larger systems, but the slope is not statistically significant. The slope coefficient implies that a unit of a large system with 34 hospitals (about the 90^{th} percentile) enjoys a mean price 0.09 s.d. greater than a standalone hospital (34 x 0.27 = 9.2%).

Panel (e) presents the association between operating expenses per bed and firm size. In contrast to Panel (d), the downward sloping relationship here is clearly noticeable — hospitals in larger systems enjoy lower operating costs per bed on average. The relationship appears to be non-linear, with the largest systems having disproportionately lower costs per bed. As we will discuss below, system-owned hospitals are similar in size to the standalone hospitals on average (see Table 1), so this is not capturing establishment returns to scale. Following the same thought experiment as above, operating costs for a hospital in a system at the 90^{th} percentile in size are 0.24 s.d. lower than for a standalone hospital (34 x -0.71 = -24.1%).

23. They have 133 and 120 hospitals, respectively, while the next largest system has 62 hospitals. The mean system size drops from 15 to 11 and the 90^{th} percentile from 46 to 34 after imposing this restriction.

Panel (f) presents the association between readmission rates and system size. The slope coefficient is modestly positive but statistically insignificant. The coefficient implies that a hospital in a system at the 90^{th} percentile in size is predicted to have a 0.08 s.d. higher readmission rate than a standalone hospital (34 x 0.24 = 8.2%).

Overall, the descriptive patterns suggest that hospitals in larger firms enjoy modestly higher prices and substantially lower operating costs, implying higher profitability. These patterns suggest that system ownership may improve operating efficiency, consistent with evidence on chain ownership from other sectors (Foster, Haltiwanger, and Krizan 2006). However, the positive association between firm size and readmissions suggests caution when interpreting reduced costs as improved productivity, since quality of care may decline. Next, we present our research design to quantify the causal effects of transitioning to system ownership.

4 Empirical strategy

4.1 Research Design

To estimate causal effects of system ownership on hospital performance, we zoom in on the changes in patterns at hospitals that are acquired by systems during the sample period and compare them to the patterns for similar hospitals that did not experience a change in ownership during the sample period and thus offer a counterfactual trend.[24] This difference-in-differences research design follows the approach used by recent studies examining hospital ownership and consolidation (Lewis and Pflum 2017; Dafny, Ho, and Lee 2019; Craig, Grennan, and Swanson 2021). This design relies on the identifying assumption that the acquired and comparison hospitals would continue to progress on parallel trends in the absence of the acquisitions. As in the previous literature, we acknowledge the potential for selection of acquired hospitals and interpret the estimated effects as the average treatment effects on the treated (ATT), i.e., they do not necessarily generalize to the average hospital.

We build on this standard approach in two ways in order to further strengthen our ability to make causal interpretations. First, we examine the effects of system ownership separately for two different types of acquired hospitals. The group of primary interest is formerly standalone hospitals that become a member of a system for the first time. These hospitals experience a change in ownership that includes the benefits of being part of a much larger firm for the first time, the "corporatization" phenomenon we wish to study. Intuitively, we compare their trends to other standalone hospitals that remain independent throughout the sample period. In addition, there are a substantial number of deals during this period where systems acquire one or more hospitals from other hospital systems. These hospitals offer an interesting contrast to the primary group since they experience a change in ownership without experiencing a discontinuous change in firm size. Since these facilities were already system-owned at the start of the sample period, we compare their trends to other system-owned hospitals that do not experience a change

24. Hospitals that exit the sample are retained in the comparison group. Conceptually, exit is also a valid counterfactual to acquisition.

in ownership during the sample period. Throughout, we refer to these as "non-corporatization" deals; we present a companion set of results on effects on the hospitals acquired in these deals in the appendix, using the same format as our main exhibits. Considering the two sets of results together helps us move closer to quantifying the effect of corporatization separately from a change in ownership.

Second, our baseline models limit the comparison hospitals to a subset that match the acquired hospitals closely on key characteristics prior to the acquisition. These attributes include the number of beds, total operating cost per bed in the two years prior to the deal, profit type, patient share of Medicaid and Medicare, and rural county status. We identify the matched comparison subset using the non-parametric coarsened exact matching algorithm (Iacus, King, and Porro 2012).[25] Appendix Table A.2 presents evidence on balance across several key attributes between the acquired and comparison hospitals using data from the year prior to the merger. We use the standardized difference between the acquired and comparison hospitals as our metric of balance. Columns 1 and 2 present the values for the full sample and matched subsample, respectively, for the corporatization deals. Columns 3 and 4 present corresponding values for the non-corporatization deals. The table clearly shows that matching weakly improves balance between the acquired and comparison hospitals on all attributes and that the matched samples are very well balanced. Matching is more important in the case of non-corporatization deals since the standardized difference in the full sample approaches 0.1 – a threshold conventionally used to signal imbalance – on multiple important attributes (Austin 2011). For completeness, the robustness section presents the corresponding coefficients obtained using the full sample.

Equation 2 below presents our baseline specification. Y_{ht} denotes the outcome of interest for hospital h in year t. We model the outcome as a function of hospital and year fixed effects, α_h and α_t, respectively, and covariates X_{ht}, a vector that controls for observed differences in patient mix and hospital and market attributes, including differences in lagged local market concentration.[26] The key regressor of interest, D_{ht}, is a time-varying indicator variable that is equal to one starting in the year the hospital is acquired and zero otherwise. Finally, ϵ_{ht} denotes unobserved time varying factors. When studying patient-level outcomes such as readmission or mortality, we estimate the model at the patient- rather than the hospital-level so we can granularly control for differences in patient risk. We cluster standard errors by hospital to account for the potential correlation between outcomes across patients or over time for the same hospital, which is the unit of treatment.

25. We use the Stata command "cem" to implement coarsened exact matching (CEM). Each target hospital is matched to never acquired hospitals in the year before the deal. We match on the following variables: bed size (above/below median), rural county, non-profit status, Medicare and Medicaid share (above/below median), expenses and lagged expenses per bed (quartiles).

26. We include the following covariates. Patient: female, age, Elixhauser co-morbidity scores, previous year count of hospital stays, plan attributes such as the product type (HMO, PPO, CDHP, POS, EPO, other), relationship to subscriber (self, spouse, child, parent), individual exchange, individual non-exchange, fully insured, and DRG weights. Hospital: number of beds, teaching status, Medicare and Medicaid shares of patients. Market: rural, white, college, unemployed, poverty, elderly, Medicaid expansion, and lagged HHI. Covariates that are at the patient level reflecting binary variables are aggregated to the hospital level and are transformed in percent.

$$Y_{ht} = \alpha_h + \alpha_t + \beta\, D_{ht} + X'_{ht}\,\delta + \epsilon_{ht}. \tag{2}$$

While our approach follows the prior literature, we recognize that acquisitions are not randomly assigned, nor is there credible quasi-experimental variation leading to changes in ownership. Hence, one should interpret the coefficient of interest, β, with caution. However, our specifications control for the most important potential confounders. For example, hospital fixed effects eliminate persistent unobserved differences between hospitals, an important source of selection. To assess the validity of the parallel trend assumption, we consider the dynamic effects on target hospital outcomes around the year of the acquisition by estimating the event study model in Equation 3 for each outcome.

$$Y_{ht} = \alpha_h + \alpha_t + \sum_{s\neq -1} \beta_s\, D_{h,t+s} + X'_{ht}\,\delta + \epsilon_{ht}. \tag{3}$$

A lack of differential trends in the years prior to the acquisition is consistent with the identifying assumption. We present *p*-values from formal statistical tests of jointly significant pre-trends for each outcome. Reassuringly, we find no evidence of such pre-trends visually or in the formal tests. We consider the evidence to be supportive of causal effects on hospital performance only when we find noticeable changes in the trajectory of the outcome following soon after system ownership. We can follow hospitals post-acquisition for three years on average and for a maximum of 5 years. Hence, we interpret the coefficients as the medium-run effects of system ownership.

We also run a battery of robustness checks to test the stability and significance of the estimates to varying the covariates, the comparison group (e.g., using hospitals acquired later in the sample as an alternate comparison group), the specification (e.g., different functional forms), and clustering at different levels. Recognizing the recent concerns about the consistency of DD estimates in staggered treatment designs, we also report coefficients using the estimator proposed by Callaway and Sant'Anna (2020), which is consistent for the ATT in the presence of treatment effect heterogeneity and staggered treatment.

4.2 Sample construction

We focus on hospital deals that closed between 2013 and 2017 in order to ensure that we observe each acquired hospital for at least one year before and after treatment.[27] 107 hospitals were acquired in corporatization deals over this period and can be compared against a pool of 348 independent hospitals. After matching, we are left with a sample of 101 acquired and 251

27. It is difficult to clearly identify the exact date when the ownership change was executed. The process of executing the contractual agreement sometimes takes more than a year after the deal is first announced. In some cases, antitrust or regulatory agencies undertake lengthy reviews even after the deal is consummated and the operational status during that period is unclear. We assume the year of treatment is year the deal was contractually executed, which we manually validated for all deals. This is year zero in the event study analyses.

matched comparison hospitals. In the case of non-corporatization deals, we begin with a pool of 173 and 205 acquired and comparison hospitals, respectively. After matching, the sample reduces to 135 and 128 hospitals, respectively. We use the same CEM algorithm and matching attributes in both sets of deals.

Figure 2 Panel (a) presents the number of acquired hospitals separately in each year from 2013 through 2017 in the corporatization and non-corporatization deals. No particular deal year dominates the sample. Panel (b) illustrates the geographic coverage of the corporatization deals by plotting the location of the acquired and comparison (never acquired) hospitals. The figure shows that every state in the sample experienced a deal during this period, with a greater concentration of target hospitals in the mid-Atlantic and Midwestern states. Appendix Figure A.2 presents the corresponding geographic distribution of the non-corporatization deals.

Table 1 describes key aspects of the markets (Panel A), hospitals (Panel B), prices and inputs (Panel C), and quality of care (Panel D) observed in our analysis sample as of 2012. For brevity, we report four key outcomes of interest: mean price for an inpatient stay, total operating expenses per bed, and two measures of quality: 90-day readmission and mortality rates. Column 1 presents mean values across all 615 hospitals in the matched sample. Of these, 251 hospitals were not acquired throughout the study period (column 2) and are the comparison hospitals for the 101 standalone hospitals acquired by a system during the study period (column 3). Collectively, these hospitals comprise the sample to study corporatization deals. The acquired hospitals are smaller in terms of bed capacity and had slightly lower mean price levels and operating costs per bed in 2012, but were not systematically better or worse on quality measures. Both sets of hospitals had similar negative average profit margins at baseline.[28]

Table 1 columns 4 and 5 present mean values across the hospitals involved in the non-corporatization deals. Columns 4 and 5 present means for the comparison and acquired hospitals, respectively. Acquired system-owned hospitals are less likely to be rural and non-profit than the comparison hospitals. Among outcomes, they do not differ systematically on prices and operating costs, but tend to have slightly worse performance at baseline on quality.

The raw data suggests that system-owned hospitals (cols. 4 and 5) were more likely to be located in urban markets, were slightly larger, and commanded higher prices than independent hospitals at the start of the sample (cols. 2 and 3). However, they were similarly placed on operating costs and quality measures. System-owned hospitals enjoyed a much higher profit margin than independent hospitals, on average, perhaps reflecting their higher price levels.

4.3 Transition to system ownership

Table 2 characterizes the ownership transitions in our sample and presents descriptive statistics on the acquirers (Panel A), acquired or "target" hospitals (Panel B), and characteristics of the markets in which target hospitals are located (Panel C). We present these statistics for the two types of deals discussed previously. Columns 1 and 2 present the mean and median values,

28. The profit margin measure was computed using data from HCRIS. It only accounts for revenue and costs pertaining to patient care services. For example, it excludes income from investments and concessions.

respectively, for the 101 deals in which independent hospitals are acquired by a system. These are the transitions of primary interest. As a contrast, columns 3 and 4 present the mean and median values, respectively, for the 67 deals in which previously system-owned hospitals are acquired by another system.

The average corporatization deal involves a large increase in firm scale for the new member. The average target has approximately 230 beds and serves about 10,000 admissions per year. In contrast, the average acquiring system has nearly 3,900 beds across 17 hospitals and serves about 177,000 patients annually — nearly a 17x multiple on both beds and patient volume. The enormity of the increase in operational scale for the target hospital is highlighted further by contrasting it against the corresponding change in scale in the average non-corporatization deal. Here, a system with 18 hospitals sells 2 of its member units to an acquirer with about 45% more hospitals and 65% more beds.

Based on this evidence, most of the gains in scale and sophistication are realized when a hospital joins a system for the first time. Subsequent changes in ownership may lead to an increase in firm size, but the differences are relatively minor. In both types of deals, the target hospitals are likely to be non-profit, and about 50% of the deals involve a target located in the same HRR. Intuitively, "within-market" deals have greater potential for clinical and non-clinical synergies since greater proximity facilitates optimizing service lines, staffing, and patient flows across the facilities. We therefore investigate this possibility in our analysis. One concern with using HRRs to define hospital markets is that "cross-market" does not automatically imply a large distance between the target and acquirer. However, very few hospitals in our sample are located close to an acquirer while being assigned a different market. For example, only 6 independent targets within 15 miles of the closest acquirer (out of 23) are assigned a different HRR.

5 System ownership and profitability

5.1 Prices

5.1.1 Average effects

Table 3 presents the estimated effects on hospital-level average reimbursement per stay obtained by estimating Equation 2 on unweighted hospital-level data.[29] We present the effect on the mean price across our sample (column 1) and for each of the top 7 service lines by volume (columns 2–8). The regressions include patient, hospital, market, and plan controls discussed in Section 4. We control for procedure intensity by including the mean DRG weight as a covariate. We find that system ownership leads to a price increase of 6% overall, and the effect ranges between 0% (musculoskeletal) and 11% (central nervous system) across the different service lines. Relatively large price increases are observed in deliveries, cardiac care, and respiratory

29. Estimating the models at the patient-level generated similar results, without gains in precision.

care.[30]

Figure 3 Panel (a) presents the dynamic effects on mean prices in the commercial insurer sample following system ownership. The event study pattern indicates no differential pre-trends at the acquired hospitals, which we confirm formally using joint tests of significance and report the corresponding p-values. We find a gradual increase in prices at acquired hospitals following the transition, which is intuitive since hospital contracts with insurers are renegotiated on expiry, and contract renewal schedules would not immediately coincide with the change in ownership. Figure 3 Panel (b) presents the estimated change in price in % terms for the acquired hospitals by service line.

The increase in prices cannot be explained by changes in the patient risk profile. Specifically, we do not detect changes in observed patient risk factors such as DRG weight, age, length of stay, and Elixhauser score. These results are presented for the commercial insurer's patient sample in Appendix Table A.3 Panel A. The estimates are precise enough to rule out even modest changes in patient risk levels. For example, we can reject a change in the mean age of more than 0.60 years (0.094 + 0.252x2) and in the mean Elixhauser score of more than 0.045 (-0.003 + 0.024x2), both less than 5% of the corresponding mean levels. In Panel B we present the corresponding results for the subset of commercial patients admitted with cardiac conditions. Overall, we find similarly unchanging risk levels, but we do detect a marginally significant 3% increase in mean DRG weight. Another piece of evidence supporting the lack of change in patient mix is that we detect no effect on patient volume at the acquired hospitals. Appendix Table A.4 presents the estimated effects on total patient volume (cols. 1 and 2) as well as on commercially insured cardiac care patients (cols. 3 and 4). The coefficients tend to be small and statistically insignificant.[31]

5.1.2 Potential mechanisms

In this section, we examine heterogeneity in the estimated price effects to assess the importance of different potential contributing mechanisms. Table 4 presents the corresponding results. Columns 1 and 2 present results corresponding to prices, while columns 3 and 4 present results corresponding to operating expenses, which we discuss in the next section. We first present the baseline average effect across all deals as a benchmark for comparison. Panels 1, 2, and 3 highlight heterogeneity across attributes of the targets, acquirers, and by the predicted change in concentration, respectively. Column 1 presents the DD coefficients estimated for the subset of acquired hospitals that meet the criteria stated in each row. Column 2 displays the interaction

30. Employer sponsored patients tend to be younger and healthier than the average hospital patient. Thus, the commercial insurer sample overemphasizes deliveries and orthopedic procedures relative to what we observe in the universe of hospital discharges in New York. We therefore apply the sample weights observed in the New York discharge data for every DRG when we estimate our models to make the results representative of the average hospital stay. Reassuringly, models with and without this weighting scheme yield similar results.

31. We also investigate whether the share of total inpatient volume changes across 12 disease groups within acquired hospitals using the discharge data in New York. We classified patient volume into 12 different logical groupings of Major Diagnostic Categories, and the only statistically significant effect is a decline in patient volume for the "pregnancy and reproductive" group. This is mainly driven by a reduction in labor and delivery, which we discuss later on.

coefficients from triple difference models estimated on the full sample. For example, column 1 in row A of Panel 1 presents the DD effect on price for targets that had below-median price levels at baseline. Column 2 presents the interaction term from a triple difference model testing whether these targets experienced a differential change in price following system ownership. Each cell therefore presents results from a different regression model. This presentation format allows us to discuss both the average effect for the subset of interest as well as whether the estimate differs from that in the remaining deals in statistical significance. Appendix Table A.5 provides further details by presenting the complete set of results from the triple difference models, including the effects on readmissions.

The coefficients in Panel 1 Row A imply that the increase in price following an acquisition is negatively correlated with the baseline price level. Targets that had lower than median prices obtain, on average, a greater increase in price than across all deals. The triple difference coefficient in Column 2 is large, about $550, but it is imprecisely estimated. This pattern is consistent with convergence in prices between the target and its acquirer. Row B shows that the increase in price is also somewhat negatively correlated with the size of the acquired hospital, consistent with the hypothesis that smaller targets obtain greater benefits in price negotiations due to system membership. Based on the triple difference model, the price increase for smaller target hospitals is $265 greater than for large targets (col. 2).

Panel 2 sheds light on heterogeneity based on attributes of the acquirer. Row A implies that larger acquirers do not obtain greater price increases for their targets than the average acquirer. Although the triple difference coefficient is statistically insignificant, it is negative, implying a lower price increase for hospitals acquired by larger systems. Row B suggests a similar pattern in the case of for-profit acquirers. We return to this issue in Section 5.4 when we discuss the effects for large acquirers on both prices and operating costs together.

Panel 3 tests if the price effects are greater in deals that lead to a greater increase in concentration in the target's market. Row A presents the effect for deals where the target and at least one hospital of the acquirer share the same HRR. By construction, these deals lead to a greater increase in market power than deals where the acquirer and targets do not share a market. The results imply larger price effects in such deals, with a large triple difference coefficient of about $440. Panel 3 Row B presents the effect for targets involved in the approximately 30 deals where we predict an increase in HHI of more than 100. We find a similar magnitude effect on price for this subset of deals, reinforcing the interpretation that deals involving a greater increase in concentration lead to larger price increases.

As noted previously, HRRs are very large geographically and may not accurately reflect patients' hospital choice sets. As an alternate approach, we examine variation in the price effect by the distance between the target and the closest facility of the acquirer. Appendix Figure A.3 Panel (a) shows how the estimated price effect (in %) changes as we allow this distance to increase and include an increasing number of deals in the estimation sample. The figure implies that the price effect remains stable in magnitude throughout the distance range, though we cannot rule out very large effects for targets in the lowest distance decile.

To summarize, the patterns support heterogeneity in price effects on two dimensions. Targets that were at a greater disadvantage at baseline (based on their size or their price level) appear to benefit more from system acquisitions. Secondly, within-market deals lead to greater price increases.

In addition to heterogeneity across deals involving standalone target hospitals, it is instructive to study the effects for system-owned hospitals after they transition to another system. Table 2 highlighted that independent hospitals experience a dramatically larger increase in firm size when they join a system, relative to the change experienced by system-owned hospitals. If improvements in bargaining ability track increases in firm size, then we should expect a greater relative price increase following corporatization deals, all else equal. Appendix Table A.6 column 1 presents the estimated effect on mean price for commercially insured patients at system-owned hospitals acquired by other systems, about 6%. This coefficient is similar in magnitude to the main result reported above for independent targets. Appendix Figure A.4 Panel (a) presents the dynamic effects on prices, showing an increase in price following the change in ownership. Table A.6 also presents the effect on the price for each of the top 7 service lines by volume (columns 2–8) using the same format as in Table 3. The overall effect on price is similar across the two types of deals, but the effects are concentrated in slightly different service lines. Similar to the case of independent hospitals, we find no evidence to support a change in the risk profile of patients in these deals (see Appendix Table A.7).

To determine if the estimated effects are statistically distinguishable across the two sets of deals, we performed bootstrapping analyses to independently generate empirical distributions of the price effect for both types of deals. The resulting distributions overlap substantially, and a two-sample t-test fails to reject the null hypothesis that the means of the distributions are the same. (see Appendix Figure A.5 Panel (a)). This evidence raises doubts whether system ownership increases bargaining ability materially and suggests that the price increase following system acquisitions may have more to do with increases in market power.

5.2 Operating costs and hospital inputs

5.2.1 Average effects

Table 5 Panel A presents the estimated effects on hospital operating expenses per bed.[32] We describe the results on total expenses as well as on mutually exclusive components of spending. Panel A column 1 presents the effect on total operating costs per bed, which implies a 4.8% reduction relative to the mean of about a million dollars per bed. Column 2 presents the effect on combined depreciation and interest rate costs. We bucket these together since they both relate to capital inputs for the hospital — the former is a measure of capital spending, while the latter reflects the cost of raising capital. We find a 13% reduction in this segment, implying a larger reduction in capital costs due to system ownership relative to other components. As a result,

32. We hold beds fixed at the value of the first year we observe the hospital, usually 2012. The results are qualitatively similar if we allow the beds to update each year.

although it is 8% of the total cost base, it accounts for 20% of the savings.[33]

Panel A column 3 reports the effect on personnel spend (salaries and benefits) per bed, which accounts for about 50% of total operating costs for the average independent hospital. We find a statistically significant 6% reduction in personnel spend, which accounts for about 60% of the total reduction in operating expenses. Finally, column 4 presents the collective effect on all remaining spending categories. We do not detect a statistically significant effect on these groups, which mainly consist of material costs (medical supplies and consumables). Spending decreases by a statistically insignificant 1.4%, similar to the 1.9% reduction in purchasing costs reported by Craig, Grennan, and Swanson (2021) using granular purchasing transaction data. Overall, these patterns suggest that systems drive cost reductions primarily by reducing labor inputs at the hospital. Since the target hospital continues to serve the same patient volume, we interpret this as a reduction in the labor intensity of care.

Figure 4 Panels (a) through (c) present the corresponding event study figures for the first three outcomes. Reassuringly, we do not observe differential trends at the acquired hospitals prior to the deal. The dynamic effects are consistent with the DD estimates and confirm a differential decline in these outcomes at acquired hospitals following the change in ownership. Note that the dynamic effects increase in magnitude over time, suggesting the long-run effects may be still larger.

The reduction in spending on labor may reflect a reduction in employment, wages, hours, or a combination of all three channels. To shed more light on the roles of these channels, we present the effects on employment in Table 5 Panel B. Employment is measured by the number of full-time equivalent (FTE) employees, which accounts for both full-time and part-time employees. We normalize FTE by the number of beds to eliminate heterogeneity purely due to differences in hospital size. Column 1 shows that personnel decline by about 0.29 FTE per bed, or 5% of the mean. Figure 4 Panel (d) presents the corresponding event study, showing a noticeable decline in personnel following the change in ownership. Assuming the reduction in personnel is not concentrated in any specific part of the wage distribution, the reduction in employment directly implies a reduction of about $24,000 in spending per bed (0.287 FTE per bed x 82,200 payroll spend per FTE = $23,534 spend per bed), which represents about 80% of the decline in personnel spend. The remaining decline in personnel spend, therefore, is due to a reduction in compensation per employee. In unreported results, we confirm that spending per employee declines by 0.7%, but this estimate is statistically insignificant. (The coefficient is -$556.1 with a standard error of $1,540). However, our data cannot distinguish between a reduction on the intensive margin in the number of hours worked versus a decline in wages.

The reductions in staffing appear to be concentrated among specific employee types. Panel B

33. A decrease in depreciation may reflect changes to the acquired hospital's accounting practices post-deal, rather than a real change in capital investment. Specifically, the acquired hospital may increase the "useful life" over which its capital stock is depreciated, artificially reducing its annual depreciation. To address this concern, we calculated the implied useful life for each hospital-year based on its average balance of plant, property, and equipment as reported in the AHA. The coefficients obtained by estimating Equation 2 with implied useful life as the dependent variable suggest that useful life *decreases* by 1.6 years (8%) on average following the transition in ownership, relative to the pre-deal mean of 21.3 years, suggesting our depreciation results are understated.

column 2 presents the result on employment among employees excluding physicians and nurses ("other"). We omitted the results for the latter two groups for the sake of brevity, since we find only small effects there, although they collectively account for about a third of all personnel.[34] The coefficient from other employees accounts for 85% of the total reduction in employment, disproportionate to their share of total employees. Panel (e) of Figure 4 presents the dynamic effects, which are consistent with the DD coefficient.

Unfortunately, the AHA does not provide a detailed breakdown of sub-categories or functions outside of physicians and nurses. To make progress in identifying which types of employees are let go, we turn to the Medicare cost report data (HCRIS), which provides more detail. Table 5 Panel B columns 3 and 4 report the results using data from HCRIS. Column 3 presents the effect on employment in overhead or support functions, which include categories such as employee benefits, general and administrative, maintenance, supply, pharmacy, and medical records. This group is a subset of the employees captured in column 2 and performs exclusively non-clinical roles. We find a relatively large reduction in overhead personnel of about 9%. Although this group only accounts for a third of total employment, it accounts for 61% of the reduction, implying new owners disproportionately cut headcount in these back office functions. Figure 4 Panel (f) presents the corresponding event study, which corroborates the DD estimate.

Panel B column 4 presents the effect on contracted staff. We estimate a small and statistically insignificant effect and can reject an increase of more than 0.002 FTE per bed (-0.018 + 2 x 0.010), negligible relative to the estimated reduction in employment. This helps rule out the explanation that systems convert former employees of their newly acquired hospital to contracted staff, artificially reducing headcount without a change in real labor inputs into hospital care. In results not reported here, we further tested and were unable to reject the null hypothesis of no change in employment at the acquirer. In other words, we find no evidence to suggest that the target's employees are reassigned to the parent firm. These results point to a picture of significant employee reductions at the target hospital, focused among support functions, consistent with the mechanism hypothesized by Dranove (1998).

The reduction in employment could also be driven by potential rationalization or optimization of the hospital's service portfolio. We evaluate whether the number of services offered by the acquired hospital declines following its change in ownership. We also specifically examine whether the hospital continues to provide cardiac and delivery care after the transition. Table 5 Panel C provides suggestive evidence of such rationalization. While we can rule out even modest changes in the number of services and technology-dependent services offered (cols. 1 and 2), as well as in the probability of offering cardiac care (col. 3), we do find a small net reduction in the probability of offering deliveries (col. 4) following the change in ownership. We decomposed the latter two estimates into the probabilities of offering a new cardiac or delivery

34. The distribution shares of physicians and nurses reported by the AHA closely match numbers reported by the BLS for the universe of hospitals. We find a reduction of 0.04 FTE per bed, which implies a 2% reduction among physicians and nurses. We find reductions in both physicians and nurses, with the largest decreases in physicians [10%, -0.012 (0.014)] and licensed practical nurses [14%, -0.016 (0.007)].

service and of stopping an existing service. We find movement in both directions for cardiac and delivery care, but the probability of exit increases much more in the case of deliveries. Panel (b) of Appendix Figure A.3 presents the corresponding dynamic effect coefficients, which illustrate clear changes on both margins. We also find that the extensive margin decline in delivery services is entirely concentrated among rural target hospitals.[35] A caveat in interpreting these results is that they represent an equilibrium outcome of negotiations between the hospital and insurers, so these optimizations could also be sought by the insurer.

5.2.2 Potential Mechanisms

This section returns to Table 4 to explore how the effect on operating cost varies by attributes of the target facility, the acquiring system, or the deal. Column 3 presents the DD coefficients estimated using the different subsets of deals based on the attribute of interest, while column 4 presents the interaction coefficient from triple difference models estimated on the analysis sample. The results in Panel 1 indicate there is little variation in the effect on costs by the target hospital's size or baseline price level. Similarly, the results in Panel 3 imply that the cost reductions are not materially different whether the target is located in the same market as the acquirer or not. In contrast, the reductions in cost vary substantially by the acquirer's attributes, and we discuss this evidence below.

Table 4 Panel 2 row A column 4 shows that operating costs decline at the target hospital by about $80,000 per bed more when it enters a system with more hospitals than the median acquirer (6), relative to entering a below-median size system. The subsample coefficient in Column 3 implies that larger systems generate about twice the reduction in costs at their acquired facility than in the average corporatization deal. This difference is economically meaningful and statistically significant.[36] Appendix Figure A.3 Panel (c) provides a visual representation of the variation in the effect on costs with acquirer size. We estimate the DD effect independently for each target against all matched comparison hospitals. The figure plots the binned mean estimated effect on operating cost in each decile of system size. We also plot a fitted line predicted using the slope coefficient from an OLS regression of the underlying deal-level estimates on system size. We report the slope coefficient with bootstrapped standard errors.[37] The fitted line has a negative slope with a statistically significant coefficient of -0.13. The decile means follow the fitted line closely suggesting the linear fit is a reasonable approximation. The slope implies that an acquirer with 1 s.d. larger size (28 more hospitals) obtains $36,000 per bed (28 x 0.13 = 3.6) greater cost reductions, nearly 75% larger than the mean effect. This finding also corroborates the descriptive analysis in Section 3.2.

35. The point estimate for pregnancy services at rural targets is -0.175 (0.070), while the corresponding coefficient for urban targets is -0.004 (0.035). Hence, there may be greater consolidation of services following acquisitions in rural markets versus their urban counterparts.

36. In unreported results, we find greater reductions in costs for larger acquirers across all the components discussed above. Hence, this effect is not disproportionately driven by any one segment.

37. The standard errors presented in parentheses in this figure and other such binned scatter plots throughout the paper are obtained by bootstrapping over both estimation steps and therefore account for estimation error in the first step where we obtain the DD effects for each deal.

We also find that for-profit acquirers obtain much larger reductions in operating cost at their targets. Table 4 Panel 2 row B column 4 shows that targets acquired by for-profit systems experience a differential reduction of about \$94,000 per bed relative to those acquired by non-profit acquirers. In light of this pattern, one concern arises whether the relationship between operating cost effects and system size is largely driven by for-profit status. To explore this, we examine whether the correlation with system size is also detected among non-profit acquirers. Figure A.3 Panel (d) presents the corresponding binned scatter plot limited to deals involving non-profit acquirers and shows that the negative correlation prevails with a similar slope. Hence, greater system size delivers economies of scale benefits independent of profit status.

We then contrast the effects on formerly standalone target hospitals with those estimated for system-owned hospitals involved in the non-corporatization deals. These hospitals presumably already realized the gains in capital and personnel costs under their previous owner. Hence, another change in ownership, even to a larger system, may not materially affect costs. Alternately, if there are constant or increasing returns to scale, then an increase in firm size even without changing the nature of the firm could produce cost reductions. Appendix Table A.8 presents the corresponding results on operating cost and its components. Overall, the coefficients imply a small and statistically insignificant effect on operating cost. Appendix Figure A.7 Panel (a) presents the corresponding event study, which suggests noticeably muted effects on total costs. Appendix Figure A.5 Panel (b) presents empirical distributions of the effect on total operating costs in both corporatization and non-corporatization deals obtained using bootstrapping. We can reject the null hypothesis of equal means across the two distributions (p <0.001).

To the extent there is a small decline in total cost for previously system-owned targets, it is driven by a statistically significant decline in interest payments and depreciation (Figure A.7 Panel (b)), while personnel expenses and FTE per bed show no change in trends (Panels c–f). The coefficients in Panel B of Table A.8 are consistent with the event studies and indicate negligible changes in personnel. Together, these patterns suggest that system ownership confers gains in labor intensity on the extensive margin, but not on the intensive margin, while financing and capital costs continue to decline on the intensive margin.

5.3 Profitability

We combine our estimated effects on prices, volume, and operating expenses to predict the net effect on the target hospital's operating margin. We find no effects on patient volume or composition (discussed previously), suggesting changes in revenue from commercial patients are driven entirely by price increases. Our baseline estimate implies mean prices grow by \$856 per admission per year (see Table 3 column 1). Assuming this is representative of all private insurer admissions, we estimate an increase in inpatient hospital revenue of \$11,656 per bed (\$856 x 13.62 admissions per bed). This may understate the real increase in prices if the commercial insurer in our sample is able to negotiate smaller price increases than other insurers due to its generally strong position in the markets where it operates. It may also be a conservative estimate of revenue increase for other reasons, for example, because we do not consider potential increases in prices for outpatient care.[38] On the expenses side, we apply the result from Table 5 Panel A column 1, which indicates that operating expenses decrease by $48,281 per bed per year. Taken together, these estimates imply an increase in operating profits of about $59,940 per bed per year, or about 6% of baseline operating expenses for the average acquired hospital.

As is immediately obvious from these estimates, cost reductions contribute much more to the change in profitability than the price increases. To put this in perspective, system ownership increases revenue for the average acquired hospital in our sample by about $2.7 million per year (231 beds x $11,656 per bed), but it decreases expenses by $11.2 million per year. Overall, system ownership increases surplus by $13.9 million per year for the average target hospital.

5.4 Pass-through of cost savings

The previous sections have established that system ownership enables higher negotiated prices from commercial insurers while also affording the acquired target a more efficient cost structure. Finding price increases on average despite such large cost reductions confirms that efficiencies alone do not prevent price increases in oligopolistic hospital markets. Still, a policy relevant question arises whether some of the cost savings are passed through to insurers in the form of lower prices. Although prices rise differentially following system ownership, they may have risen still more in the absence of the estimated cost changes. While undeniably important, a full blown pass-through analysis is out of scope for this paper since it requires the estimation of structural cost and price-setting parameters in order to predict hypothetical prices under a counterfactual scenario had the target hospital not achieved any cost savings (e.g., Miller and Weinberg 2017).

That being said, the present research design allows us to test for the presence of such a feedback from cost savings to prices in the spirit of Ashenfelter, Hosken, and Weinberg (2015), who demonstrated the link between merger efficiencies and lower prices following the merger of the Miller and Coors beverage companies in 2007. We adapt the test for our setting where we can directly observe the effects on prices and operating costs across a large number of deals. Therefore, we test for a negative correlation between the estimated effect on operating costs and on prices across the 101 deals in our sample. Note that the correlation may not be an unbiased estimate of the precise pass-through rate since a number of unobserved factors could vary across these deals, some of which may not average out.[39]

Figure 5 presents binned scatter plots of the deal-specific DD effect on commercial revenue on the Y-axis against the corresponding estimated effect on operating costs on the X-axis. We obtain these values by estimating our baseline DD specification separately for each target hospi-

38. We estimate the average treated hospital had 13.62 private insurer admissions per bed per year using total volume and commercial share from the AHA. We do not observe outpatient prices. However, if we apply our 6% estimated increase in price to outpatient revenue for the acquired hospitals and consider this as well, we project an increase of about $26,000 per bed.

39. For example, deals involving larger acquirer systems may systematically produce greater cost savings due to scale benefits. Managers at larger acquirers may also be systematically less focused on extracting price increases at their latest member facility due to other competing concerns. This pattern would bias upward the estimated negative correlation; other confounders could bias it toward zero.

tal against the full set of comparison hospitals.[40] We plot the mean values in each decile bin and the best linear fit estimated on the underlying deal-level values. The corresponding slope coefficient is presented along with bootstrapped standard errors in parentheses. Panel (a) contrasts the effect on commercial insurer revenue against that on total operating expenses. The correlation is economically and statistically insignificant – for every \$100 in realized cost saving, the target hospital accepts about \$8 lower commercial revenue.

Our analysis on operating costs shows that the savings are primarily obtained on personnel expenses, which are about half of the total cost base of the average target hospital. These savings reflect lower staff headcount at the target hospital in the period after acquisition, likely due to direct actions taken by the acquirer following the deal. The acquirers may therefore be able to predict these savings with more certainty and plan to pass a portion of these savings to insurers. Accordingly, in Figure 5 Panel (b), we present an alternate scatter plot where the X-axis displays the decile mean values of the estimated effects on personnel expenses instead of on total operating costs. The correlation here is striking, and the fitted line follows the decile means rather closely, suggesting a linear relationship. The correlation is economically and statistically significant – for every \$100 in realized saving on personnel, the target hospital accepts \$30 lower commercial revenue.

Taken together, this evidence provides some support for the pass-through of cost savings obtained on personnel expenses following system ownership. However, the evidence on the pass-through of other sources of cost savings is inconclusive.

6 System ownership and quality

We do not find changes in patient volume at target hospitals following the transition to system ownership. This finding suggests patient perceptions of hospital quality did not change materially and is consistent with evidence from other recent studies (Schmitt 2017; Roos et al. 2019). In this section, we formally test for changes in hospital quality, studying the measures most commonly used in the literature, as well as by Medicare and other insurers in hospital quality incentive programs (Chandra et al. 2016; Hirji et al. 2020; Gupta 2021). We study changes in three patient outcomes: short-term readmission and mortality rates following inpatient stays for acute conditions, and subjective patient assessments of their inpatient hospital experience, as recorded in surveys.

40. The effect on commercial revenue for each target hospital is calculated by multiplying the effect on mean inpatient price estimated for that specific target with the baseline number of commercial admissions at that hospital (we use total inpatient admissions reported in the AHA survey in 2012 and the commercial share to arrive at an estimate for commercial inpatient stays). The effect on operating expenses for each target hospital is calculated in similar fashion by multiplying the effect on operating expense for that hospital with its number of beds. This approach allows us to compare the effects on price and operating expenses in the same unit, dollars.

6.1 Average effects

We begin our analysis by examining readmission outcomes for the commercial patients following an inpatient stay for an acute cardiac condition. We focus on cardiac care patients to mitigate concerns about unobserved patient selection, since patients typically cannot avoid or delay hospital care for these conditions. Table 6 Panel A column 1 presents the estimated coefficients obtained from our preferred specification, discussed in Section 4. The outcome variable is an indicator for a 90-day all-cause readmission from the date the patient was discharged from the index stay.

Readmission rates increase among the commercially insured cardiac care patients by nearly 3 percentage points. This is an economically and statistically significant effect, reflecting an increase of 17.5% relative to the mean. Figure 6 Panel (a) presents the corresponding event study figure with the dynamic effects. The figure suggests no consistent trends prior to system ownership but a gradual increase in readmissions following the acquisition. The dynamic effects increase over time following the change in ownership, suggesting this effect is not transitory.

Two pieces of evidence suggest it is unlikely that the increase in readmission rates is driven by changes in the composition of patients. First, as reported in Appendix Table A.3 Panel B, we find no change in patient mix among cardiac care patients. For example, we find no change in the mean Elixhauser score for cardiac patients at acquired hospitals, which is highly predictive of readmissions. The 90-day probability of readmission for cardiac patients with an Elixhauser score of zero is 10.9%, but it is nearly double, at 18.6%, for patients with a score greater than zero. Second, we detect no change in patient volume within this group (see Table A.4 cols. 3 and 4).

Since the commercial insurer sample contains patients covered by a single firm, we examine changes in quality for a wider sample of patients using the other datasets discussed in Section 3. The New York discharge data allow us to observe changes in outcomes for patients across all payer types (Medicaid, Medicare, privately insured, and uninsured), ages, and disease groups in one large state, while the Medicare sample allows us to examine effects for a large proportion of elderly patients across all 20 states. The commercial claims do not systematically record member mortality, precluding the study of this outcome for their patients. However, the New York and Medicare samples allow us to overcome this limitation as well. We study in-hospital mortality for New York patients and 90-day mortality for Medicare patients.[41]

Table 6 Panel A column 2 presents the effect on 90-day readmissions following a cardiac care stay for New York patients, which implies an increase of 1.3 pp, about 9% of the mean.[42] While this estimate is smaller than what we obtained for commercial patients, it is well within the confidence intervals of that estimate. Figure 6 Panel (b) presents the corresponding dynamic effects, which follow a similar pattern to those in Panel (a), except for the third post-acquisition

41. The New York sample records in-hospital deaths, but not deaths following discharge from the hospital. Nevertheless, in-hospital mortality has been used as a performance measure in several studies of hospital quality since it is highly predictive of the more frequently used 30- or 90-day mortality rates (Cooper et al. 2022).

42. Patients were identified using identical diagnoses and procedure codes as in the commercial insurer sample.

year. Table 6 Panel A column 3 presents the effect on in-hospital mortality for the New York cardiac care patients. We obtain a statistically insignificant estimate close to zero in magnitude. Figure 6 Panel (c) presents the corresponding event study, which suggests the presence of a mild pre-trend, but not significant dynamic effects post-acquisition. We do not emphasize this pattern since the DD coefficient is imprecisely estimated, and we cannot rule out moderate effects in either direction.

Next, we test the hypothesis of a change in hospital quality using the Medicare sample. A benefit of these data are that they are large enough to limit the sample to index stays for "non-deferrable" conditions that originated in the emergency department (ED). Previous studies have identified and used non-deferrable conditions to examine changes in hospital quality (Card, Dobkin, and Maestas 2009). It is assumed that patients with these conditions must rush to the hospital for care, which further mitigates the potential for bias due to unobserved changes in patient mix.[43] Another advantage in our particular application is that the Medicare sample allows us to observe acquired hospitals for 4 years prior to acquisition, instead of 3 as in the commercial and NY samples. Hence, we can observe pre-trends over a longer period to help validate the identifying assumption.

Table 6 Panel B column 1 presents the effect on readmissions following admissions for non-deferrable stays by Medicare patients. We estimate an increase in readmissions of 0.57 pp, which is about 2.3% of the mean value. Figure 6 Panel (d) presents the corresponding event study, which confirms no pre-trends and the increase in readmissions following system ownership. However, contrary to commercial and New York samples, the dynamic effects do not consistently increase over time following the change in ownership. While the magnitudes differ across samples, we find it compelling that we consistently detect an increase in readmission rates across three different patient samples and multiple disease cohorts.[44]

Table 6 Panel B column 2 presents the estimated effects on the probability of 90-day all-cause mortality for Medicare patients admitted through the ED with non-deferrable conditions. In contrast to the readmission results discussed above, we do not find any evidence that mortality rates changed after a transition in hospital ownership. The coefficient is statistically insignificant and smaller in magnitude than the corresponding effect for readmissions. Figure 6 Panel (e) displays the corresponding event study, which suggests no clear pattern in the period before or after the change in ownership. The estimated effect on mortality has the opposite sign of the readmission effect for Medicare patients, raising the question of whether system-owned hospitals may improve quality by keeping additional patients alive. In turn, these patients may then be readmitted at higher rates because they are frail. While it is true that mortality and

43. This cohort pools patients across three well-defined disease groups: circulatory, respiratory, and injuries. The most prominent conditions include heart attack (AMI), pneumonia, hip fracture, and stroke.

44. These results are not sensitive to the choice of examining readmissions at 90 days following discharge. Figure A.6 plots the point estimates obtained on readmissions over durations spanning 30 through 90 days following discharge from the index case. Panel (a) presents results for the two cardiac care samples, and Panel (b) plots the corresponding point estimates from the non-deferrable conditions Medicare sample. Since the baseline readmission rate differs by duration, we present the estimates in percent terms relative to the corresponding mean. The effects are stable in percent terms and statistically significant regardless of duration.

readmission are competing risks following a hospital stay, we carefully assess this possibility in Section 8 and do not find the evidence to be persuasive.

Our final hospital quality measure goes beyond administrative data and draws on self-reported patient satisfaction responses to the HCAHPS survey, an annual survey conducted on a random sample of inpatients at Medicare certified hospitals. The survey has been widely utilized by previous studies to understand hospital quality, and patient satisfaction scores have been shown to be positively associated with adherence to treatment guidelines, specifically for cardiac care, and lower risk-adjusted inpatient mortality (Glickman et al. 2010; Tsai, Orav, and Jha 2015; Beaulieu et al. 2020). We use a composite measure of these scores as our outcome of interest.

Table 6 Panel C presents results for the composite experience measure, which is the average z-score of five continuous measures of hospital quality. The five measures quantify the share of survey respondents that would recommend the hospital, rated it a 9 or 10 out of 10, reported their nurses or doctors always communicated well, and reported always receiving help as soon as they needed it. We do not find an economically or statistically significant change in the composite measure.

To summarize, our results indicate that system ownership leads to an increase in short-term readmission rates on average, but no detectable changes in short-term mortality or patient satisfaction.

6.2 Potential mechanisms

We examine heterogeneity in treatment effects on readmission rates, the quality measure where we detect an increase following system ownership. However, the analysis does not reveal any noticeable differences in the effect on readmissions by target, acquirer, or deal characteristics. The corresponding triple difference results are presented in Appendix Table A.5 columns 5 and 6.

In contrast to our results for formerly independent hospitals, we estimate small and statistically insignificant effects on readmission rates following acquisitions of system-owned hospitals (Table A.9). These results suggests that a change in hospital ownership alone does not elevate readmissions, but the transition from being independent to system-owned is an important component of the causal chain. Further, if we consider the evidence on corporatization and non-corporatization deals collectively, it is worth noting that we find an increase in readmissions in the deals where we find reductions in personnel, and vice versa. Note also that, although about 60% of the reduction in personnel at formerly independent hospitals is in support functions, we also detect declines among physicians and licensed practical nurses (LPNs).

To test the importance of the cuts in labor inputs as a mechanism, we examine whether larger personnel reductions across target hospitals are associated with greater increases in readmissions. Figure A.6 Panel (c) presents a binned scatter plot of the coefficients on 90-day cardiac care readmissions for commercial patients on the Y-axis and on hospital FTE employees on the X-axis. The coefficients are from separate DD regressions for each treated hospital against the full set of comparison hospitals. The figure presents mean values in each decile bin, and we overlay a linear fit from an OLS regression using the underlying estimates from each deal. The figure shows that experiencing a greater reduction in headcount is correlated with a larger increase in the readmission rate. The association appears to follow a remarkably linear pattern and implies that a reduction in labor intensity of 0.29 FTE per bed is associated with an increase in readmission probability for cardiac care patients of approximately 1.1 pp. We emphasize that this is a correlation and does not imply causality, but the magnitude is economically significant and explains about 40% of the increase in readmission rate we estimate for cardiac care patients.

There is well-established evidence linking readmission reductions to interventions such as better discharge planning, better drug reconciliation checks, improving coordination with primary care physicians, and the use of best-practice checklists (Naylor et al. 1999; Hannan et al. 2003; Coleman et al. 2005; Goldstein et al. 2016). These initiatives typically rely on a combination of clinical (e.g., nurses) and non-clinical staff (e.g., case managers or social workers) (Holliman, Dziegielewski, and Teare 2003). Hence, cuts to relevant non-clinical staff such as social workers could also impact readmissions. Recent studies of hospital care transition programs provide evidence consistent with this hypothesis (Boutwell, Johnson, and Watkins 2016; Jenq et al. 2016; Evans et al. 2021). In particular, Evans et al. (2021) argue that transition programs using social workers alone have been more effective in reducing readmissions. Social workers are more accessible to patients (they make lower wages than nurses and may be more willing to visit homes) and may be more effective at resolving unmet non-medical needs that otherwise lead to readmissions (e.g., help patients understand post-discharge instructions, schedule follow-up care, and navigate public assistance resources).

7 Robustness

This section reports results assessing the sensitivity of our baseline estimates to a battery of different robustness checks, organized into four categories. The tests demonstrate that the estimates remain qualitatively similar despite changes in covariates, specification, comparison group, or the inference approach used. Table 7 presents the corresponding results. Columns 1, 2, and 3 present the coefficients for our key outcomes: mean hospital inpatient price, operating expense per bed, and 90-day readmission probability for commercially insured patients following cardiac care stays, respectively. We limit our presentation of robustness checks to these key outcomes in the interest of brevity. The first row of the table presents the baseline estimates for ease of comparison. Across all tests, the estimates typically remain within one standard error of the baseline estimates.

Panel 1 presents sensitivity checks to varying the covariates in the baseline model. Reassuringly, the estimates remain stable across a range of different covariate choices, mitigating omitted variable bias concerns. The baseline model includes multiple patient- and hospital-level characteristics as covariates, as discussed in Section 4. Row 1A presents estimates from a “bare” model with only hospital and year fixed effects but no other covariates. Row 1B offers a minor variation from the baseline model by excluding DRG weights. Row 1C implements a more flexible model by including DRG fixed effects instead of the continuously varying DRG weights used in the baseline model. Row 1D shows results using a modified HHI measure computed using a narrower definition of hospital markets (Connor, Feldman, and Dowd 1998; Ho and Hamilton 2000; Petek 2022).[45]

Panel 2 presents results assessing sensitivity to using alternate specifications, but holds other aspects such as covariates and sample constant. Row 2A evaluates robustness to the functional form used. Recognizing the potential for bias due to a right skew in both prices and operating costs, we apply the log transformation to these outcomes. In the case of readmission rates, we present the marginal effect from a logit model, while the baseline implemented a linear probability model. Across all three outcomes, the corresponding point estimates are consistent with the baseline coefficients. In row 2B, we weight hospitals by their patient volume when studying prices and operating expenses. The effect on price declines slightly in magnitude and becomes significant at the 10% level, implying that larger target hospitals experienced smaller increases in price. This pattern is consistent with the results on effect heterogeneity across deals presented in Table 4. In row 2C, we estimate the price regressions directly on the patient-level data and find a similar estimate as in the baseline. Row 2D presents results from the baseline model after dropping the year of ownership change for the treated hospitals. Intuitively, the estimates increase in magnitude. Recent studies have shown that point estimates from conventional staggered DD models may be biased for the ATT in the presence of treatment effect heterogeneity (De Chaisemartin and d'Haultfoeuille 2020). To assess whether this concern is important here, we report the estimator proposed by Callaway and Sant'Anna (2020), which overcomes these limitations and is consistent for the ATT (row 2E).[46] We find a similar price estimate to our preferred one, but somewhat smaller and larger effects for expenses and readmission, respectively.

Panel 3 investigates sensitivity to using different analysis samples that help mitigate various identification concerns. Hospitals located near an acquired hospital may themselves be partially treated due to spillover effects. This is most obvious in the case of prices, but could also manifest on costs and readmissions if the patient mix at neighboring hospitals is affected. To mitigate this concern, row 3A presents results from models excluding hospitals within five miles of all target hospitals from the comparison group.[47] Using this comparison group does not meaningfully affect the estimates, except in the case of prices where the magnitude increases by about \$100, consistent with predictions by prior studies (Dafny 2009). Since our baseline sample is an unbalanced panel, row 3B estimates the model on a sample limited to hospitals that are observed for at least 5 years and finds slightly attenuated estimates.

45. In the baseline model, we use HHI values computed using hospital market shares at the HRR level. A concern with HRRs is that they are geographically large and contain about 15 hospitals on average. Therefore, they may not accurately reflect local competition. To mitigate this concern, we define alternate markets using Health Service Areas (HSAs) created by the US Census and updated by the National Cancer Institute. There are about 900 HSAs (versus about 300 HRRs), which contain about 5 hospitals on average. These are more granularly defined and should reflect local competition more accurately.

46. We use the Stata command "csdid" to obtain these estimates and set the comparison group to never-treated hospitals only.

47. We adopted the threshold of 5 miles from Beaulieu et al. (2020), who used it for the same purpose.

A concern with our baseline comparison group is that they may differ from the target hospitals on unobservables correlated with the outcomes of interest. We do two checks to address this concern. We implement an alternate design using a set of independent hospitals acquired by systems over 2019–2021 as the comparison group. Thus, the comparison hospitals are also treated, but not during the sample period.[48] A key limitation of this design is that the sample reduces in size since there are only 67 such comparison hospitals in our sample. Row 3C presents the corresponding results. All three estimates are substantially attenuated but remain statistically significant at the 5% or 10% level. Row 3D presents coefficients from the Callaway-Sant'anna estimator where the comparison group is also limited to "not yet treated" hospitals. Hence, this estimate is obtained by comparing acquired hospitals to those acquired later in the sample. These estimates are closer in magnitude to the baseline coefficients.

In row 3E, we return to our matching design with a slight modification such that we also match on hospital profit margin in the period prior to acquisition. This approach ensures that the comparison hospitals were similarly financially positioned in the pre-treatment period, and the estimates remain very similar. The next row, 3F, presents estimates obtained without using matching to identify the comparison hospitals, and instead using all hospitals that were independent through the sample period. The results remain similar, with the main change being that the effect on operating cost increases in magnitude and the effect on readmissions declines substantially.

Finally, Panel 4 tests whether statistical significance is affected by changing the level of clustering of the standard errors. The baseline approach clusters by hospital, which is the level of treatment. We consider two alternative approaches. Rows 4A and 4B present results obtained by clustering instead by acquirer and by market, respectively. This addresses potential concerns that clustering at the hospital level may not adequately capture correlations in the error terms across patients in the same market or associated with the same acquirer. Changing the clustering level does not meaningfully affect the standard errors.

Appendix Table A.10 presents the corresponding set of robustness checks for treatment effect estimates when the target hospitals were already system-owned (as opposed to being standalone, as in the main analysis). These checks further corroborate the lack of a change in operating costs for the target hospitals in these deals.

8 Discussion and Conclusion

Hospital systems have rapidly expanded their share of total hospital capacity over the last two decades primarily by acquiring independent hospitals, a phenomenon we refer to as corporatization. Yet, to our knowledge, there has been little systematic study of the corporatization of hospitals, particularly in recent years. This paper begins to shed light on this phenomenon in an important sector of the US economy.

Using data on transaction prices from a large commercial insurer, we show that the corpo-

48. We thank an anonymous referee for suggesting this alternate design.

ratization of independent hospitals increases negotiated inpatient prices differentially by about 6% within 2–3 years. Consistent with theory, we detect larger price increases when the target and acquirer share the same market or when the deal is predicted to lead to a larger increase in concentration. We detect a similar differential increase in price in deals where the target hospital was already system-owned prior to the deal and therefore did not experience the benefits of system ownership for the first time. We interpret these similar price effects, despite dramatic differences in the nature of the transition, as evidence against system ownership providing unusual advantages in price negotiations with insurers. Hence, price increases following system acquisitions may be driven more by traditional antitrust concerns of market power rather than increases in the target's bargaining power.

The corporatization of hospitals does confer large operating cost benefits to the newly acquired target hospitals, which are more valuable than the increase in revenue through higher prices. The efficiencies are driven primarily by lower capital costs and eliminating headcount in support functions. Both mechanisms are consistent with economies of scale benefits due to greater firm size and sophistication. Importantly, we do not detect changes in labor costs for system-owned hospitals when they transition to another system. We therefore conclude that corporatization, and not just greater firm size, produces the improved efficiency. We find robust evidence suggesting that larger acquirers obtain significantly greater reductions in costs when they acquire independent hospitals. This pattern also potentially explains why we do not find greater price effects for larger acquirers — they may pass on some of the cost benefits to insurers to limit price increases. We formally test and confirm a strong negative correlation between personnel cost reductions and price increases. This analysis implies that for each dollar of savings on personnel expenses, expected commercial revenue is reduced 30 cents through lower prices. The relationship appears remarkably linear and broad-based. Medicare and Medicaid (and therefore taxpayers) may not share these gains immediately since they set reimbursement rates based on market-level average costs. However, in the long-run, greater corporatization should reduce market-level costs and growth in reimbursement rates for public payers as well.

Finally, system ownership does not improve hospital quality, and may reduce it. We find a robust increase in short-term readmission rates across three different patient samples spanning multiple payers and disease groups. A concern is whether the increase in readmissions reflects improved mortality rates (Laudicella, Donni, and Smith 2013), although more recent studies have cast doubt on this hypothesis (Doyle, Graves, and Gruber 2018). The estimated effects on mortality in both the Medicare and New York samples are not large enough to explain the corresponding estimated increase in readmission rates, even if every marginally alive patient were to be readmitted.[49] The increase in readmissions is uncorrelated with attributes of the

49. While 100% readmissions is possible in theory, in practice we do not observe such high readmission rates for any patient group. For example, consider our estimates for Medicare patients. Very frail Medicare patients with Elixhauser scores of 6 or more have a 90-day readmission rate of about 32%. We estimate a 0.36 pp decline in 90-day mortality. Even if all incrementally alive patients were to be readmitted (increasingly unrealistic as we consider deaths avoided close to the 90-day timestamp), this would increase readmission rates by 0.36 pp. If we instead assume 35% of marginal patients would be readmitted, the mortality decline could explain an increase in readmissions of only 0.13 pp (0.36 x 0.35), far lower than the estimated effect of 0.57 pp.

targets and acquirers alike, suggesting the mechanism is not specific to a certain type of hospital or acquirer. Instead, we find evidence consistent with the decline in staff as a key driver of elevated readmissions.

We caution the reader about two external validity concerns. First, our analysis is limited to 20 states. The specific point estimates obtained here may therefore not generalize to the average effects of corporatization across all states. However, the data include the majority of large states and span all major geographic regions. Second, the price data are sourced from a single commercial insurer that may not be representative of all commercial insurers. It is reassuring that our price effects are quantitatively similar to those reported by previous studies using data from multiple insurers (Cooper et al. 2019).

There are a number of directions for future research. Our results on price effects are consistent with increases in market power as a more important driver than increases in bargaining ability due to system ownership. However, carefully quantifying the relative importance of these two mechanisms is a task for future work. Using more detailed data, perhaps from a case study, researchers can make more progress on quantifying the role of better managerial performance in reducing operating costs. We did not perform a formal welfare analysis for consumers, although our results suggest consumers are likely worse off due to system acquisitions since prices increase and quality potentially declines. Relatedly, a greater body of evidence needs to be compiled on the mechanisms that exacerbate readmission rates following system ownership. Finally, future studies should quantify the precise pass-through of cost reductions due to system ownership to insurers and what factors determine the magnitude of the pass-through. These parameters are necessary inputs for regulators assessing the effects of hospital systems in the future.

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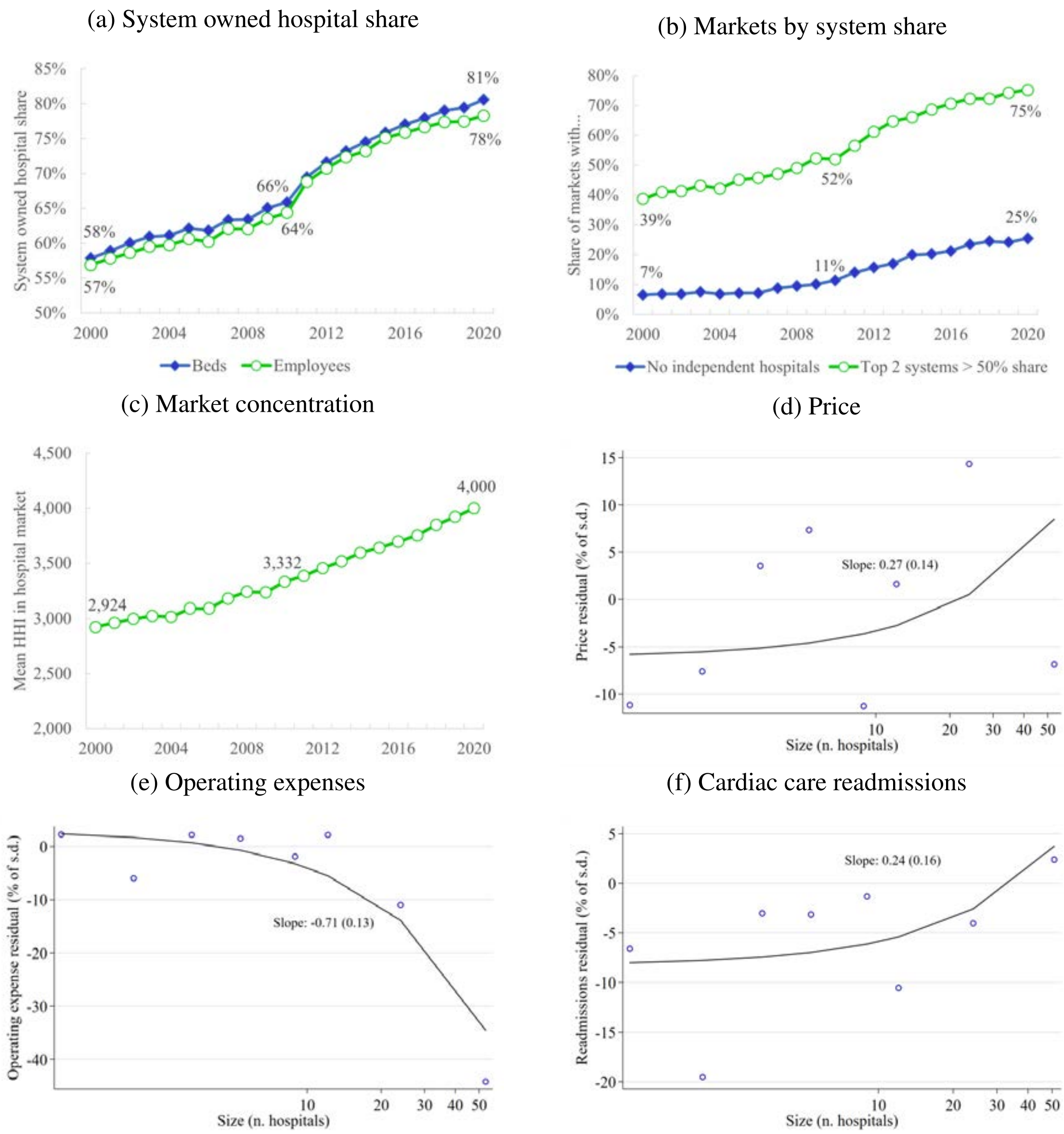
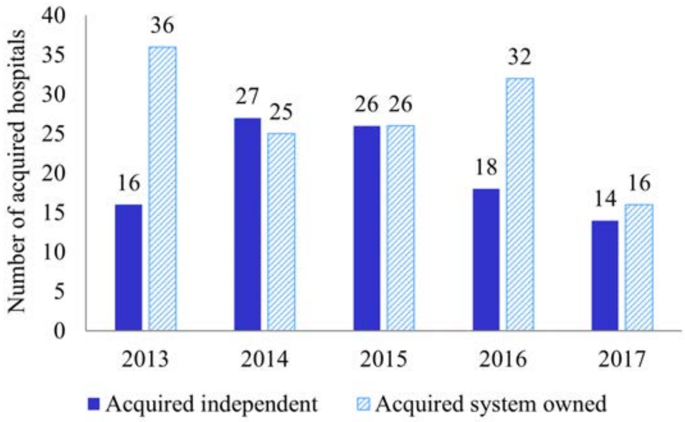


Figure 1: Hospital markets, firm size, and performance

Note: Panels (a)–(c) present unadjusted trends over 2000–20 using national data. Panel (a) presents the trend in national share of bed capacity and full time equivalent hospital employees held by system owned hospitals. Panel (b) presents the trend in the fraction of markets in which the top two hospital systems account for more than 50% of beds (green circles) and of markets without a single independent hospital (blue diamonds). Panel (c) presents the average Herfindahl–Hirschman index (HHI) across markets. We use hospital referral regions (HRRs) to define hospital markets and bed counts to compute market shares. Panels (d)–(f) present cross-sectional relationships between system size (in number of hospitals) and the key outcomes of interest: prices, total operating expenses per bed, and cardiac care readmission rates, respectively. The Y-axis plots binned means of residuals (expressed in standard deviation units) obtained from regressions of the outcomes on patient controls and market fixed effects, where the bins are defined by deciles of system size. The models were estimated on unmatched, hospital-level data derived from the commercial claims and the American Hospital Association (AHA) survey in the 20 states as discussed in Section 3. The binned scatter plots display means of residual values, ξ_h, obtained by estimating Equation 1 in each bin using data from 2012 and 2013. They also plot lines of best fit obtained using OLS regressions on the hospital-level residuals; the lines appear curved since the X-axes are log scaled. The slopes are reported with bootstrapped standard errors; we bootstrap standard errors over both steps to account for estimation error in the first step.

(a) Acquired hospitals



(b) Acquired independent and never acquired hospitals

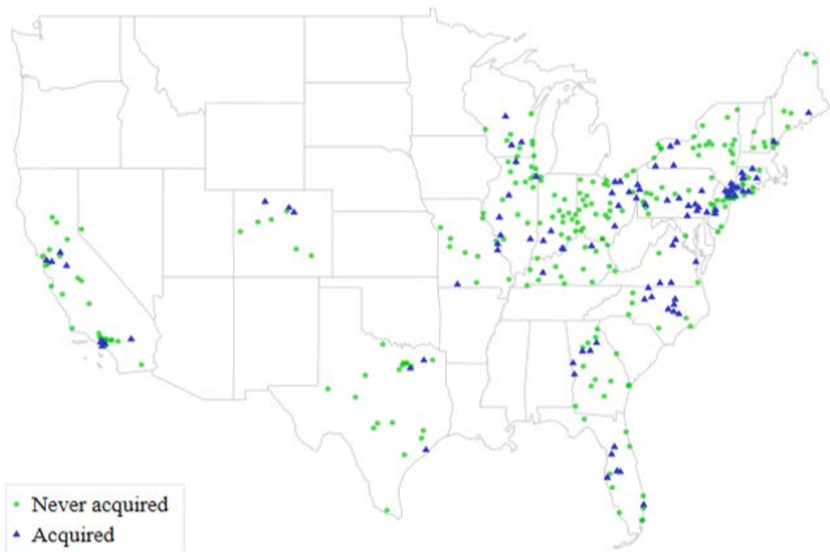


Figure 2: Hospital acquisitions

Note: Panel (a) presents the counts of independent hospitals acquired by systems ("acquired independent") and of already system-owned hospitals acquired by other systems ("acquired system owned") in our sample over 2013 to 2017. The map in Panel (b) displays the locations of all acquired independent and never acquired hospitals in the sample of corporatization deals. Note that this is not a national sample, and only contains hospitals in the 20 states discussed in Section 3. Appendix Figure A.2 presents the equivalent map for hospitals involved in non-corporatization deals.

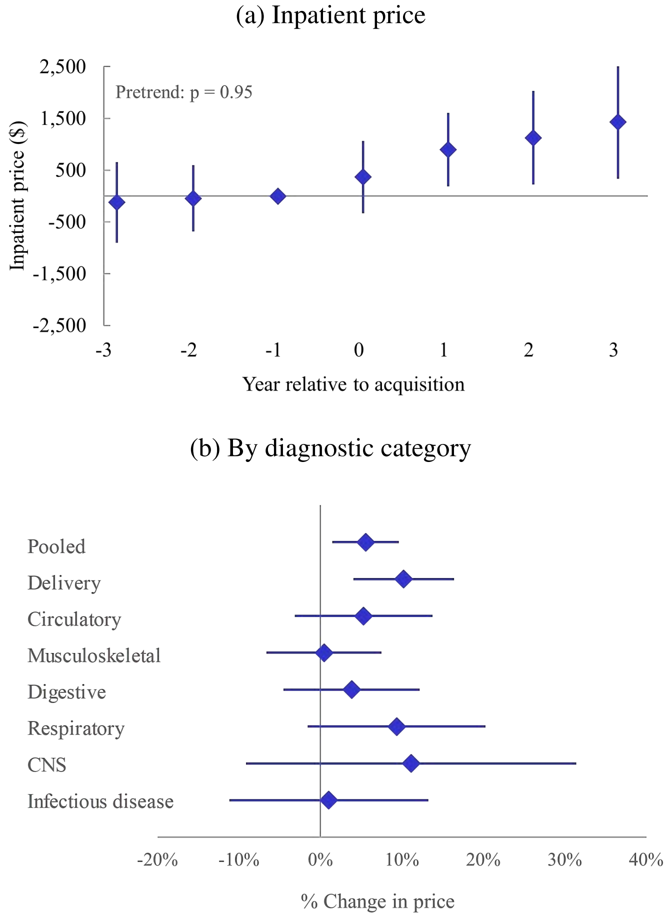


Figure 3: Effects on hospital inpatient prices

Notes: The figure presents the main results for the estimated effects of system ownership on hospital inpatient prices following corporatization deals. Panel (a) presents dynamic effects for mean inpatient prices. The coefficients were obtained by estimating Equation 3 on hospital-year level data constructed from the commercial claims. The year prior to the deal is the omitted reference year. Panel (b) presents difference-in-differences (DD) coefficients obtained by estimating Equation 2 with average inpatient price as the dependent variable on different patient samples, including the entire pooled sample and the seven largest patient cohorts. The cohorts are defined by major diagnostic categories (MDCs) and ranked in descending order by patient volume. “CNS” denotes central nervous system. Table 3 presents the corresponding coefficients, noting the mean prices. All regressions include hospital and year fixed effects and controls as described in Section 4. The figures present 95% confidence intervals with standard errors clustered by hospital; panel (a) also reports the p-value from an F-test of the joint significance of the pre-treatment coefficients. Appendix Figure A.4 presents the corresponding plots for acquisitions of system-owned hospitals.

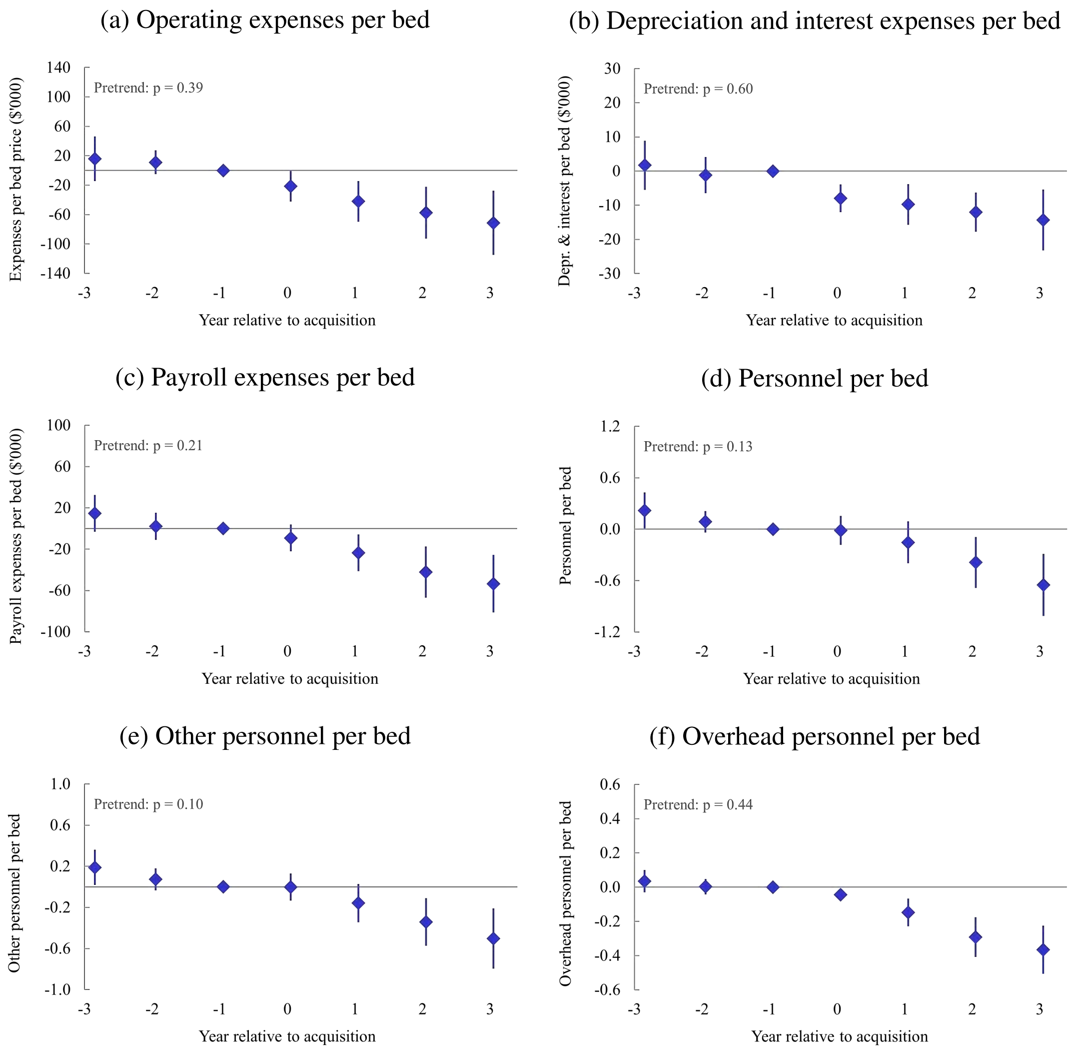
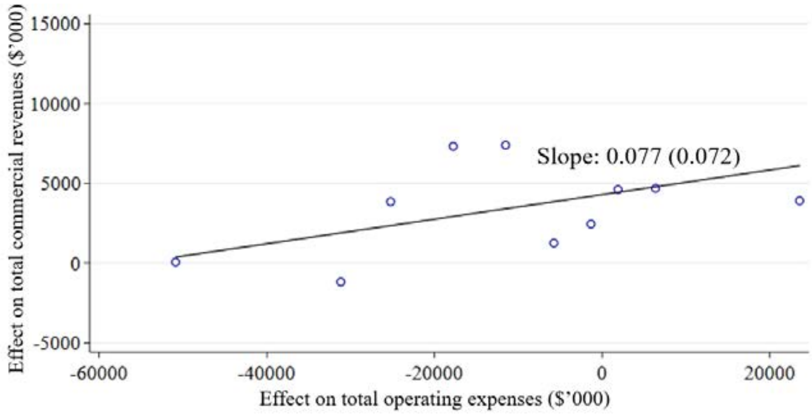


Figure 4: Effects on hospital inputs

Notes: The figure presents dynamic effects following corporatization deals obtained by estimating Equation 3 on various measures of hospital inputs: (a) total operating expenses per bed, (b) depreciation and interest expense per bed, (c) payroll and benefits spending per bed, (d) total full time equivalent (FTE) personnel per bed, (e) "other" FTE, defined as total FTEs less physicians, dentists, and nurses per bed, and (f) overhead FTEs per bed. Overhead FTEs are described in Section 5.2. All regressions include hospital and year fixed effects and hospital and market controls as described in Section 4. The year prior to the deal is the omitted reference year. The figures present 95% confidence intervals with standard errors clustered by hospital and p-values from F-tests of the joint significance of pre-treatment coefficients. Appendix Figure A.7 presents the corresponding plots for acquisitions of system-owned hospitals.

(a) Effect on revenue vs. on operating expenses ($)



(b) Effect on revenue vs. on personnel expenses ($)

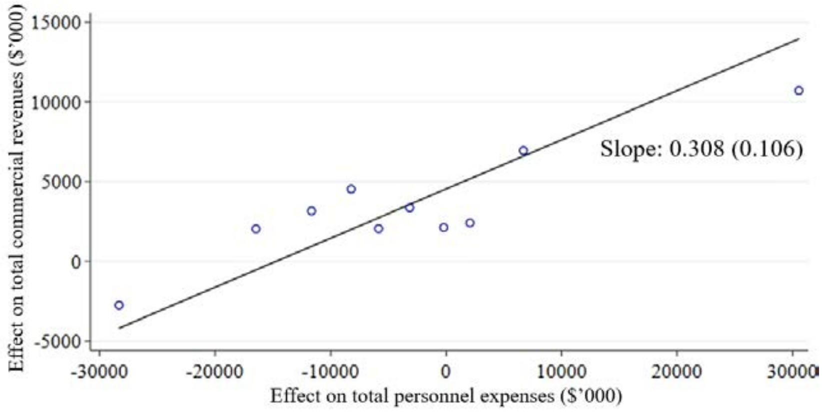
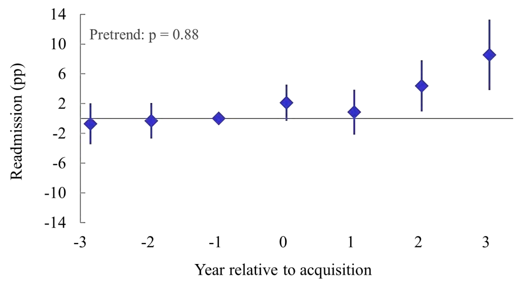


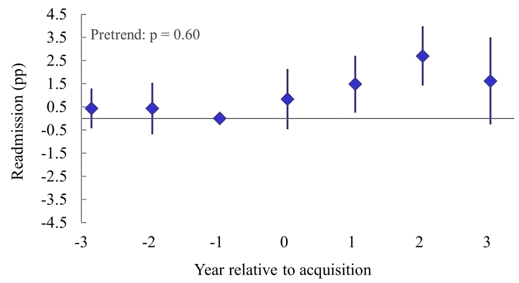
Figure 5: Effect on prices vs. on operating costs

Notes: Panel (a) presents a binned scatter plot of the effect of system ownership following corporatization deals on commercial revenue (Y-axis) against its effect on total operating expenses (X-axis) in decile bins. Panel (b) presents the corresponding scatter plot of the effects on commercial revenue versus on personnel expenses. The effect on commercial revenue is calculated for each target hospital as the product of the estimated effect on inpatient price in $ per stay, 2012 commercial share of patients, and inpatient admissions. The effect on total personnel expenses for each target hospital is the product of its estimated effect on personnel (in FTE per bed), 2012 mean wage ($ per FTE), and number of beds. The effect on total operating expense for each hospital is the product of its estimated effect on operating expenses ($ per bed) and the 2012 number of beds. For each of these outcomes, we first estimate each target's deal-specific DD coefficients by comparing it to the full set of comparison hospitals. The plots include lines of best fit and slope coefficients from a linear regression using the underlying deal-level estimates. Standard errors for the slope coefficients are in parentheses; they are bootstrapped over both steps to account for estimation error in the first step of obtaining deal-specific DD effects.

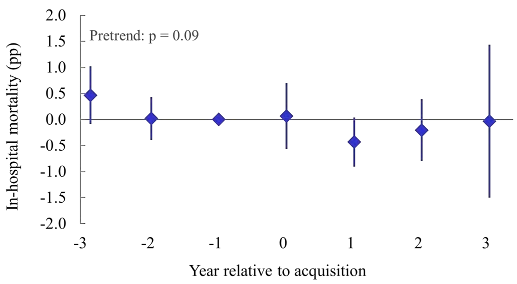
(a) Cardiac care reads. (commercial insurer)



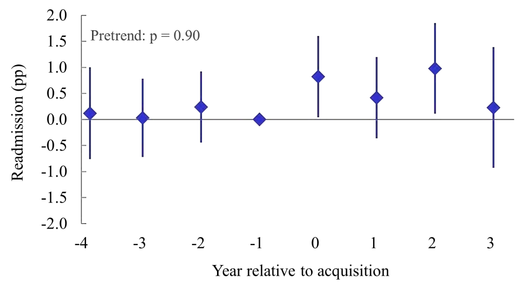
(b) Cardiac care reads. (New York)



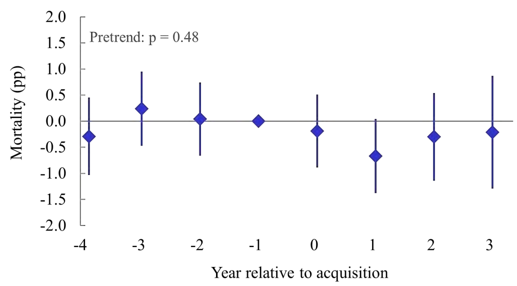
(c) Cardiac care mortality (New York)



(d) Non-deferrable reads. (Medicare)



(e) Non-deferrable mortality (Medicare)



(f) Patient experience composite

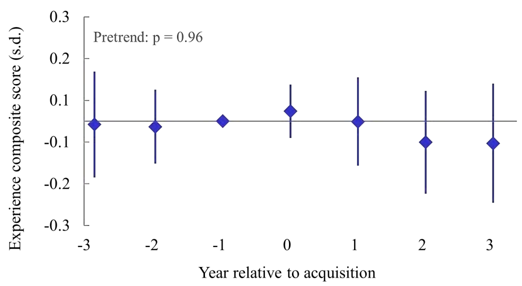


Figure 6: Effects on hospital quality

Notes: The figure presents dynamic effects obtained by estimating Equation 3 on various measures of hospital quality following corporatization deals. The year prior to the deal is the omitted reference year. Panel (a) presents results from a model with 90-day readmission rates following cardiac care admissions for the commercial patients as the dependent variable. Panel (b) reports results for the same outcome and equivalent patients in the New York all-payer discharge data; panel (c) examines in-hospital mortality rates for the same patients. Panel (d) presents 90-day readmission results for patients admitted with non-deferrable conditions through the ED in the fee-for-service Medicare sample. Panel (e) examines 90-day mortality for the same patients. The Medicare sample starts in 2009, so we can include the fourth year prior to all deal instances. Panel (f) reports results for patient satisfaction scores obtained using hospital-year level patient experience data from the HCAHPS survey. The composite measure is the average z-score of five survey outcomes: the percent of patients that would recommend a hospital, that rated it $\geq$9 out of 10, that reported their nurses (doctors) communicated well, and that reported always receiving help quickly. All samples are limited to hospitals in the the commercial sample to maintain consistency. All regressions include hospital and year fixed effects and controls as described in Section 4. The figures present 95% confidence intervals based on standard errors clustered by hospital and display p-values from F-tests of the joint significance of pre-treatment coefficients. Appendix Figure A.8 presents the corresponding plots for acquisitions of system-owned hospitals.

Table 1: Descriptive statistics

	(1) All hospitals	(2) Never acquired	(3) Acquired inde-pendent	(4) Already system owned	(5) Acquired system owned
Panel A: Market characteristics					
Rural	0.28	0.30	0.29	0.18	0.13
Poverty rate	0.16	0.16	0.16	0.17	0.15
Panel B: Hospital characteristics					
Beds	269.6	273.0	230.8	275.2	274.2
Medicare or Medicaid share	0.68	0.68	0.69	0.67	0.67
Non-profit	0.83	0.84	0.86	0.79	0.71
Patient care operating margin	-0.02	-0.03	-0.04	0.01	0.02
Panel C: Inpatient price and inputs per bed					
Commercial inpatient price ($)	15,248	15,213	14,431	16,411	16,111
	(5,327)	(5,308)	(5,053)	(5,175)	(5,590)
Operating expenses ($'000)	984	984	977	1,015	986
	(459)	(460)	(418)	(471)	(485)
Personnel spend ($'000)	507	510	505	485	493
	(255)	(254)	(234)	(253)	(279)
Personnel (FTE)	5.97	6.01	6.20	5.57	5.52
	(2.38)	(2.41)	(2.31)	(2.29)	(2.15)
Panel D: Quality of care					
Readmissions (90-day):					
Commercial insurer	0.181	0.182	0.167	0.168	0.183
	(0.079)	(0.077)	(0.078)	(0.070)	(0.087)
Medicare fee-for-service	0.244	0.243	0.248	0.241	0.246
	(0.035)	(0.036)	(0.032)	(0.031)	(0.032)
Mortality (90-day):					
Medicare fee-for-service	0.174	0.173	0.178	0.174	0.177
	(0.028)	(0.028)	(0.031)	(0.027)	(0.025)
Number of hospitals	615	251	101	128	135

Notes: The table presents descriptive statistics for the hospitals in our analysis sample. It describes all hospitals in the final analysis sample (col. 1), independent hospitals not acquired during the sample period of 2012–18 (col. 2), independent hospitals acquired by systems (col. 3), hospitals already owned by systems in 2012 with no further change in ownership (col. 4), and system-owned hospitals acquired by other systems (col. 5). The hospitals in cols. 2 and 3 are used to study corporatization deals, while those in cols. 4 and 5 are used to study non-corporatization deals, as described in Section 4. Each value represents data from 2012 or the first year the hospital is observed. Panel A presents market characteristics from the American Community Survey. Panel B presents hospital characteristics from the AHA survey and hospital cost reports. Patient profit margin is defined as the ratio of net income from patient services and net patient revenues. Panel C summarizes inpatient prices for commercially-insured members observed in the commercial claims data, computed using the total allowed amounts for each stay. It also summarizes key measures of hospital inputs obtained from the AHA, normalized by the number of beds in the first year each hospital appears in our data, typically 2012. Panel D describes measures of hospital quality computed using patient-level data: 90-day readmissions following cardiac care events in the commercial claims data, plus 90-day readmissions and mortality rates for patients admitted through the ED with non-deferrable conditions in the Medicare fee-for-service claims data. Corresponding standard deviations are reported in parentheses for the outcome variables in Panels C and D. The datasets are described in Section 3, with additional details in Appendix Section A.1. All dollar figures in Panel C are deflated to 2018 values.

Table 2: Hospital deals

	Corporatization		Non-corporatization	
Variable	(1) Mean	(2) Median	(3) Mean	(4) Median
Panel A: Acquirer characteristics				
System hospitals	17.3	6.0	25.8	9.0
System beds	3,851	2,027	6,110	2,036
System admissions ('1000)	176.6	96.9	288.1	102.0
Panel B: Target characteristics				
Hospitals	1.0	1.0	2.0	1.0
Prior system hospitals	1.0	1.0	17.7	5.0
Beds	228	190	543	330
Prior system beds	228	190	3,683	1,071
Admissions ('1000)	9.8	7.3	25.2	13.2
% Non profit	0.85	1.00	0.79	1.00
% For profit	0.07	0.00	0.21	0.00
Panel C: Target market characteristics				
P(acquirer is in target HRR)	0.51	1.00	0.49	0.00
Distance to closest acquirer (mi.)	71.5	28.9	163.9	20.1
Mean HHI (pre-deal)	2,654	2,278	2,724	2,299
Change in HHI	396	293	425	285
Predicted change in HHI	150	0	87	0

Notes: The table presents mean and median values for characteristics of the hospital deals studied in the paper. Cols. (1) and (2) describe the 101 deals in which independent hospitals are acquired by existing systems ("corporatization" deals). Cols. (3) and (4) describe the 67 deals in which system-owned hospitals are bought by other systems ("non-corporatization" deals). Panel A describes the number of hospitals, beds, and annual patient volumes at the acquiring system in the year prior to the deal. Panel B provides a corresponding description of the target hospital and, in the case of system-owned targets, its prior system owner in the year before the deal (specifically, its number of hospitals and beds). The non-profit category excludes publicly owned hospitals, and thus non-profit and for-profit may not sum to 1. Note that values for targets may differ slightly from those mentioned in Table 1 since that table presents values from 2012, while here we present values from the year prior to the deal. Panel C describes aspects of the hospital referral region (HRR) in which the target hospital is located. The distance between the target and acquirer is defined using the acquiring hospital closest to the target. Herfindahl-Hirschman Index (HHI) is calculated using shares of beds for hospitals located in the target's HRR. The predicted change in HHI assumes that the number of hospitals and beds in an HRR is fixed following the deal, and bed shares only reflect the change in hospital ownership. By construction, predicted change in HHI>0 only when at least one hospital of the acquirer is present in the same market as the target.

Table 3: Effects on inpatient prices

	Pooled	Delivery	Cardiac care	Musculo-skeletal
	(1)	(2)	(3)	(4)
Acquired * post	855.9	876.8	1,114	117.0
	(323.1)	(271.0)	(905.9)	(979.8)
Observations	2,189	2,035	2,084	2,113
Dep. var. mean ($)	15,459	8,581	21,051	27,212
	Digestive	Respir-atory	CNS	Infectious disease
	(5)	(6)	(7)	(8)
Acquired * post	557.8	1,334	1,728	290.6
	(623.0)	(794.9)	(1,605)	(1,773)
Observations	1,930	1,866	1,626	1,693
Dep. var. mean ($)	14,611	14,277	15,523	28,470

Notes: The table provides details on the price effects of system ownership following corporatization deals using the commercial claims data. It presents coefficients obtained by estimating Equation 2 with mean inpatient price as the dependent variable on hospital-year level data. Col. (1) presents results for the pooled sample and cols. (2)–(8) for the top seven major diagnostic categories by volume in the commercial claims, defined using DRGs. "CNS" denotes Central Nervous System. Each category is weighted to match the average DRG patient shares observed in the New York discharge data during our sample period. All regressions include hospital and year fixed effects and patient (female, age, Elixhauser co-morbidity scores, an indicator for prior year hospital stay, DRG weights, and plan attributes such as product type [HMO, PPO, CDHP, POS, EPO, or other], relationship to subscriber [self, spouse, child, or parent], individual exchange, individual non-exchange, fully insured), hospital (number of beds, teaching status, Medicare and Medicaid shares of patients), and market controls (rural, white, college educated, unemployed, poverty, elderly, Medicaid expansion, and lagged HHI). The control variables are described in Section 4. Standard errors are clustered by hospital and presented in parentheses. The dependent variable mean is computed over treated hospitals in the year prior to the deal. Appendix Table A.6 presents the corresponding estimates for acquisitions of system-owned hospitals.

Table 4: Heterogeneity in effects

Dependent variable:	Price per inpatient stay (1)	(2)	Operating expenses per bed (3)	(4)	N. acquired (5)
Baseline estimate	855.9	–	-48,281	–	101
	(323.1)	–	(13,729)	–	
1. Target attribute					
A. Below med. pre-deal price	1,137	548.0	-70,100	-38,423	52
	(434.7)	(578.1)	(15,249)	(21,055)	
B. Below med. target size	988.6	264.5	-47,229	5,344	52
	(447.9)	(575.1)	(20,946)	(21,914)	
2. Acquirer attribute					
A. Above med. system size	787.1	-143.9	-87,576	-79,574	53
	(381.1)	(578.6)	(17,037)	(20,452)	
B. For-profit system	570.2	-324.2	-126,439	-93,489	19
	(624.3)	(690.2)	(30,818)	(30,938)	
3. Deal attribute					
A. In-HRR	1,062.0	438.8	-35,063	33,361	51
	(396.3)	(580.5)	(18,107)	(22,048)	
B. Above 100 ΔHHI	1,038.0	285.5	-35,970	24,231	32
	(432.3)	(550.2)	(19,168)	(21,893)	

Notes: The table presents results on heterogeneity in treatment effects following corporatization deals across targets (Panel 1), acquirers (Panel 2), and deals (Panel 3). Columns 1 and 3 present coefficients obtained by estimating Equation 2 using sub-samples of acquired hospitals highlighted by the attribute of interest. Columns 2 and 4 present the triple difference coefficient (Post x Acquired x Attribute of interest) obtained by estimating a triple difference model on the full sample. Column 5 presents the number of acquired hospitals in each sub-sample. The dependent variables are the mean price per inpatient stay (cols. (1)–(2)) and operating expenses per bed (cols. (3)–(4)), respectively. Each cell in columns (1)–(4) therefore presents a coefficient from a different regression. The first attribute of interest (in Panel 1 Row A) is an indicator equal to one if the target's pre-deal price was less than the median across all targets. Panel 1 Row B indicates if the target is smaller (in beds) than the median target. Panel 2 Row A presents results for deals where the acquiring system is larger (owns more hospital members) than the median acquirer. Panel 2 Row B presents results for deals where a majority of members in the acquiring system are for-profit. Panel 3 highlights heterogeneity across deals based on changes in market concentration. In-HRR indicates that the target and at least one hospital of the acquiring system share the same HRR (Row A). In Panel 3 Row B we focus on deals where we predict an increase in HHI of at least 100. All models include hospital and year fixed effects and controls as described in Section 4. Standard errors are clustered by hospital and presented in parentheses. Since columns 1 and 3 present coefficients using different samples, we do not present mean values or number of observations. Appendix Table A.5 presents the full set of coefficients estimated in the triple difference models, including for 90-day readmissions following cardiac care in the commercial patient sample.

Table 5: Effects on inputs and service portfolio

Panel A: Expenses ($ per bed)				
Dependent variable:	Operating expenses	Deprec. and interest	Payroll expenses	Misc. expenses
	(1)	(2)	(3)	(4)
Acquired * post	-48,281	-10,114	-29,495	-5,663
	(13,729)	(3,149)	(8,735)	(8,940)
Observations	2,189	1,749	2,189	1,749
Dependent var. mean ($)	1,003,081	77,057	510,033	415,407

Panel B: Personnel (FTE per bed)				
Dependent variable:	Total personnel	Other personnel	Overhead	Contract
Acquired * post	-0.287	-0.245	-0.176	-0.018
	(0.117)	(0.090)	(0.041)	(0.010)
Observations	2,189	2,189	1,941	2,189
Dependent var. mean	6.2	4.3	2.1	0.2

Panel C: Service portfolio				
Dependent variable:	Number of services	Number of tech services	Any Cardiac care	Any Labor & Delivery
Acquired * post	-1.312	0.148	-0.012	-0.050
	(2.178)	(0.448)	(0.031)	(0.032)
Observations	2,189	2,189	2,763	2,760
Dependent var. mean	57.4	12.6	0.8	0.8

Notes: The table presents effects on operating costs and various inputs at targets following corporatization deals, obtained by estimating Equation 2. Panel A presents results for total operating expenses (col. 1), depreciation and interest costs (col. 2), payroll and benefits payments (col. 3), and miscellaneous costs (col. 4, defined as all remaining expenses). The outcomes are expressed in 2018 dollars and normalized by the number of beds in the hospital's first year in the sample, typically 2012. Panel B examines total FTE employees per bed and specific components: total less physicians, dentists, and nurses ("other"), overhead, and contract employees. Total FTE captures all personnel on the hospital payroll except for trainees and interns. Overhead and contract labor are sourced from hospital cost reports. Overhead includes administrative, benefits, maintenance, and other general services. Contract labor includes contracted personnel engaged in direct patient care, management, and overhead activities. Panel C examines various measures of service portfolio. Number of services (col. 1) counts the services offered by the hospital as recorded in the AHA survey. Number of technology services (col. 2) counts the subset of services that rely on technology, identified in consultation with physicians. Cardiac and deliveries extensive margins are coded based on whether we observe these claims in the commercial patient sample. Appendix Figure A.3 Panel (b) further decomposes this outcome into service closures and additions. With the exception of cols. (3) and (4) in Panels B and C, all outcomes are sourced from the AHA survey. All regressions include hospital and year fixed effects, plus hospital and area covariates as described in Section 4. Standard errors are clustered by hospital and presented in parentheses. The dependent variable mean is computed over treated hospitals in the year prior to the deal. Appendix Table A.8 presents the corresponding estimates for acquisitions of system-owned hospitals.

Table 6: Effects on quality

	(1)	(2)	(3)
	Panel A: Cardiac care		
	Commercial Claims	New York	
	Readmissions	Readmissions	Mortality (in-hospital)
Acquired * post	0.0291	0.0135	-0.0031
	(0.0099)	(0.0043)	(0.0026)
Observations	32,440	107,188	107,188
DV mean	0.166	0.151	0.011
	Panel B: Non-deferrable conditions		Panel C: Patient experience
	Medicare		
	Readmissions	Mortality	Composite score
Acquired * post	0.0057	-0.0036	-0.0028
	(0.0028)	(0.0024)	(0.0497)
Observations	795,887	795,887	2,172
DV mean	0.251	0.183	-0.170

Notes: The table presents effects on quality of care at targets following corporatization deals, obtained by estimating Equation 2. Panel A presents results for cardiac care patients. The dependent variable in col. (1) is 90-day readmissions from the commercial claims data. Cols. (2) and (3) use 90-day readmissions and in-hospital mortality, respectively, from the New York discharge data. In the fee-for-service Medicare claims data, we are able to refine the dependent variable by limiting index stays to those originating in the ED for various acute, non-deferrable conditions. Section 6 describes how we identified non-deferrable cases. Results from this sample are presented in Panel B. Cols. (1) and (2) report results for 90-day readmissions and 90-day mortality, respectively. Panel C reports results for hospital-year level patient satisfaction scores from the HCAHPS survey. The composite measure is the average z-score of five survey outcomes: the percent of patients that would recommend a hospital, that rated it $\geq$9 out of 10, that reported their nurses (doctors) always communicated well, and that reported always receiving help quickly. All samples are limited to hospitals observed in the commercial claims to maintain consistency. All regressions include hospital and year fixed effects and various covariates described in Section 4. Standard errors are clustered by hospital and presented in parentheses. The dependent variable mean is computed over treated hospitals in the year prior to the deal. Appendix Table A.9 presents the corresponding estimates for acquisitions of system-owned hospitals.

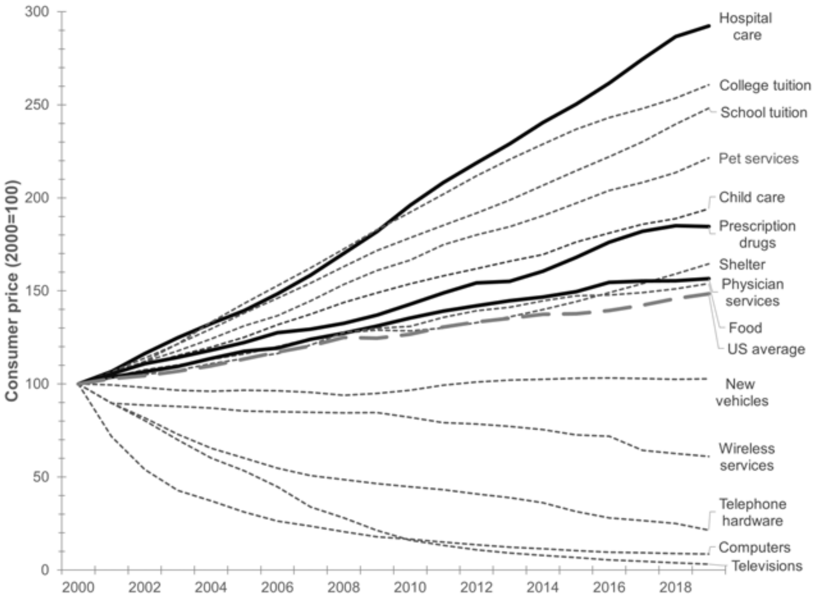
Table 7: Robustness

	(1) Price ($ / stay)	(2) Operating cost ($ / bed)	(3) Readmission rate (probability)
Baseline estimate	855.9	-48,281	0.0291
	(323.1)	(13,729)	(0.0099)
1. Alternate covariates			
A. No controls	905.5	-54,342	0.0226
	(354.9)	(13,955)	(0.0093)
B. No DRG weights	912.5	-48,281	0.0296
	(342.2)	(13,729)	(0.0098)
C. DRG fixed effects	1,003	-56,259	0.0259
	(359.0)	(15,658)	(0.0103)
D. Narrower market definition	890.8	-53,933	0.0261
	(321.5)	(13,346)	(0.0094)
2. Alternate specifications			
A. Log Y / logit model	0.0542	-0.0478	0.0304
	(0.0199)	(0.0119)	(0.0099)
B. Patient weighted	776.9	-47,946	–
	(424.2)	(14,048)	–
C. Patient-level, DRG fixed effects	845.7	–	–
	(408.0)	–	–
D. Exclude deal year	1,103	-58,995	0.0309
	(382.1)	(16,906)	(0.0118)
E. Callaway Sant'Anna	917.2	-40,661	0.0360
	(393.2)	(12,887)	(0.0229)
3. Alternate comparison groups			
A. Non-neighbor control group	960.2	-50,403	0.0316
	(320.5)	(14,515)	(0.0102)
B. Balanced treatment panel	768.3	-39,668	0.0288
	(398.2)	(14,105)	(0.0118)
C. Treated soon control group	644.9	-28,682	0.0168
	(303.1)	(15,491)	(0.0092)
D. Callaway Sant'Anna with	906.2	-40,478	0.0352
not yet treated controls	(392.8)	(12,832)	(0.0227)
E. Matching on profits	793.9	-44,401	0.0244
	(332.7)	(13,117)	(0.0109)
F. No matching	784.6	-58,858	0.0159
	(284.3)	(14,536)	(0.0082)
4. Alternate clustering level			
A. By deal	855.9	-48,281	0.0291
	(323.1)	(13,729)	(0.0099)
B. By HRR (market)	855.9	-48,281	0.0291
	(303.6)	(14,729)	(0.0093)
Observations	2,189	2,189	32,440
Dependent var. mean	15,459	1,003,081	0.166

Notes: The table presents results from four types of robustness checks. Cols. (1) and (3) report results for prices and readmissions from the commercial claims data, respectively, while col. (2) reports results for operating expenses per bed from the AHA survey. Estimates in cols. (1)–(2) are obtained using hospital-year level data, while those in col. (3) use patient-level data. Prices and costs are expressed in 2018 dollars. Panel 1 tests sensitivity to varying covariates in the baseline model. Row 1D reports results when an alternate HHI, computed using more granular "Health Service Areas," described in Section 7, replaces the HRR-based measure. Panel 2 presents results from alternate specifications. In row 2A, the outcome is log-transformed in cols. (1) and (2), while col. (3) uses a logit model. Row 2B presents results from the preferred specification when weighted by inpatient volume in the first year we observe the hospital. We use commercial patient stays and AHA all-payer admissions as weights in cols. (1) and (2), respectively. Row 2C estimates the baseline model on patient-level data. Row 2E reports the estimator proposed by Callaway and Sant'Anna (2020), which is robust to bias due to the staggered treatment design. We explicitly exclude not-yet-treated hospitals as potential controls in this exercise. Panel 3 presents results from the baseline model using alternate comparison group hospitals. In row 3A, we exclude never-acquired hospitals located within 5 miles of any acquired hospital ('neighbors'). Row 3B reports results from a sample retaining only those acquired hospitals observed for at least 2 years pre and post the deal year, which varies across different units. Comparison hospitals observed for less than 5 years are also dropped. Row 3C reports results using an alternate control group, 67 hospitals that were acquired in 2019—2021. Row 3D reports the Callaway and Sant'Anna estimator using not yet treated hospitals as controls. Row 3E presents results using a modified matching algorithm where we also match on hospital profit margin prior to the deal. Row 3F reports results using the full hospital sample without matching. Panel 4 presents results obtained by clustering standard errors at higher levels. The dependent variable mean and observations pertain to the baseline sample; the dependent variable mean is computed across independent hospitals in the year prior to the deal. Appendix Table A.10 presents the corresponding estimates for acquisitions of system-owned hospitals.

Appendix figures

(a) Consumer price growth



(b) Trend in hospital labor share

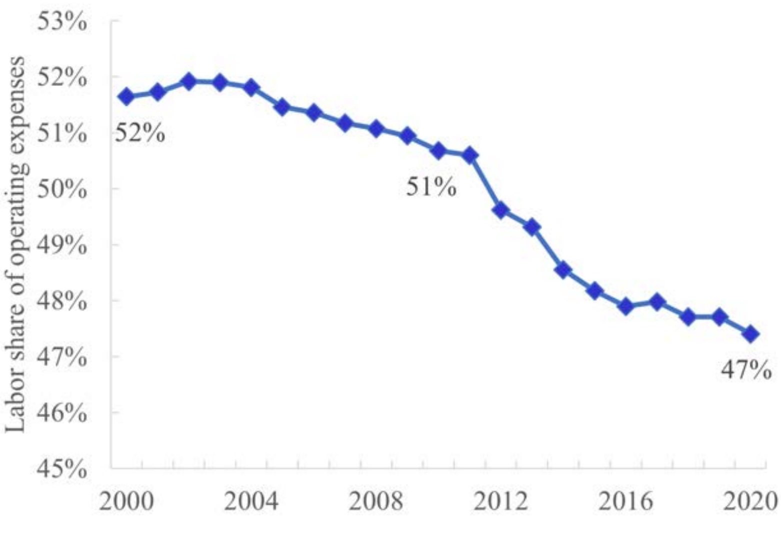


Figure A.1: Trends in consumer prices and hospital labor share

Note: Panel (a) presents the relative growth in consumer prices across different sectors of the US economy, relative to prices in 2000. Data was sourced from the Bureau of Labor Statistics (BLS) CPI-urban series and is not seasonally adjusted. For brevity, a selected subset of sectors (2 digit codes) is presented. Hospital care and televisions experienced the highest and lowest growth across all sectors, respectively. US mean prices across all sectors grew from 100 to 149 over this period. Panel (b) presents the unadjusted trend in hospital labor share over 2000–20 using national data. Labor share is defined as the ratio of payroll and benefits to operating expenses as reported in the AHA survey.

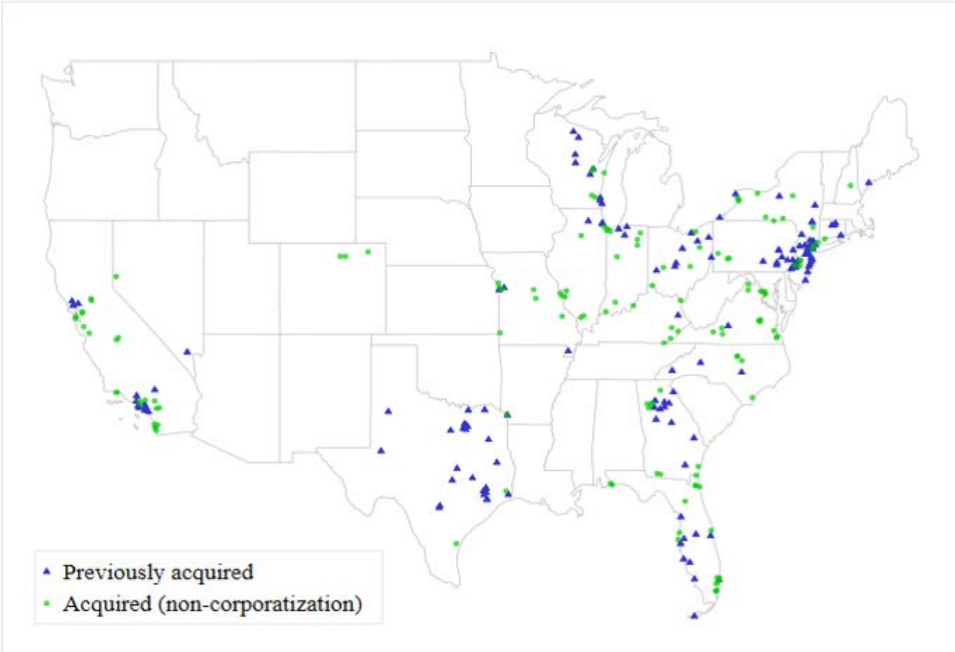


Figure A.2: Hospital acquisitions (non-corporatization)

Note: The map presents the locations of all acquired system-owned and comparison hospitals in the sample used to study the effects of non-corporatization deals. Note that this is not a national sample, and only contains hospitals in the 20 states discussed in Section 3. Figure 2 Panel (b) presents the equivalent map pertaining to hospitals in the corporatization deals.

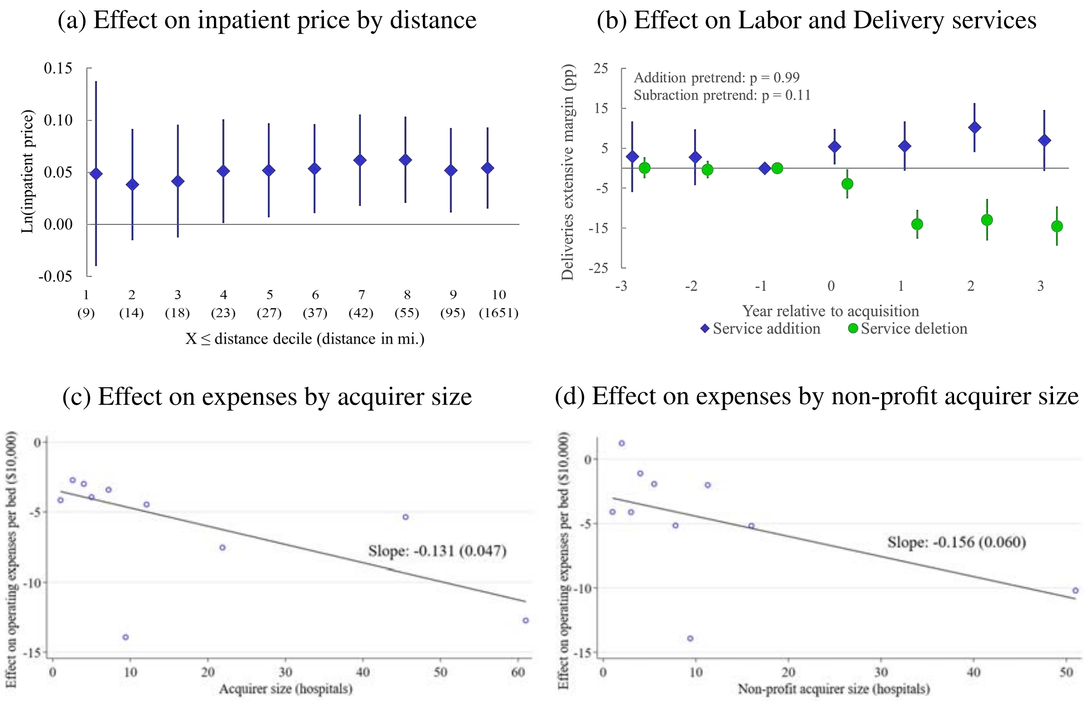


Figure A.3: Additional evidence on prices and operations

Notes: Panel (a) presents effects obtained by estimating Equation 2 on the natural log of pooled prices. It plots the coefficients estimated using increasing subsamples of the data defined by the lowest distance between the acquired and acquiring hospitals (in miles). The panel reports distance deciles and the maximum distance within each subsample in parentheses on the X-axis. Panel (b) presents dynamic effects obtained by estimating Equation 3 on a dummy variable equal to one when a hospital adds (blue diamonds) or closes (green circles) delivery services, as observed in the commercially insured patient claims. Service closure is defined for the subset of treated hospitals that provided the respective service in the pre-acquisition period. Conversely, service addition is defined for the subset of treated hospitals that did not provide the service in the pre-acquisition period. Theses two measures combine to give the net effects on service provision, which are presented in Table 5, Panel C, col. (4). The year prior to the deal is the omitted reference year. Panels (a) and (b) present 95% confidence intervals with standard errors clustered by hospital. Panel (b) also reports p-values from F-tests of the joint significance of pre-treatment coefficients. Panels (c) and (d) present binned scatter plots of the mean effects of system ownership on operating expenses per bed on the Y-axis against decile means of acquiring system size (in number of hospitals) on the X-axis. Panel (d) repeats the exercise but restricts the sample to non-profit acquirers. The panels also plot lines of best fit predicted using coefficients obtained from OLS regressions on the underlying deal-specific estimates. The slope coefficients for the linear fit are presented along with standard errors obtained by bootstrapping over both steps to account for estimation error in the first step. All regressions include hospital and year fixed effects and hospital and market controls as described in Section 4.

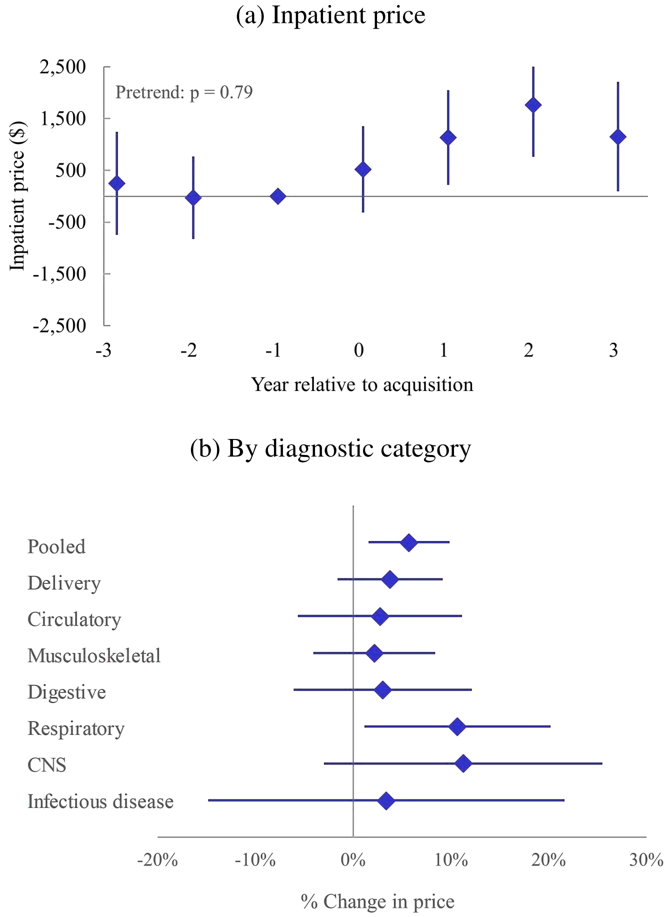
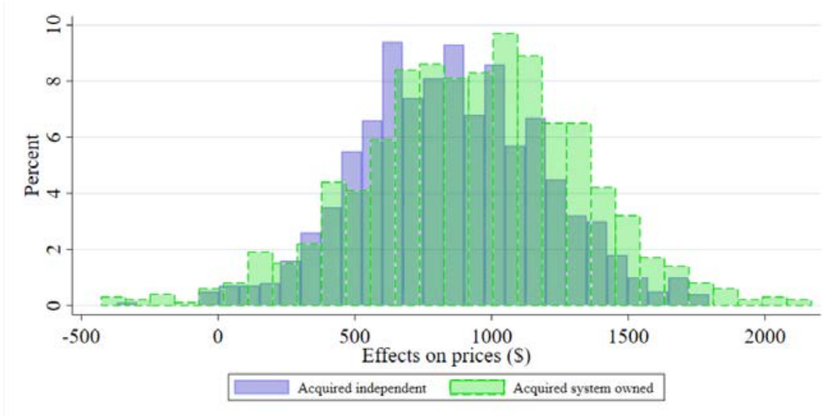


Figure A.4: Effects on hospital prices (non-corporatization)

Notes: The figure presents the estimated effects of system ownership on hospital inpatient prices in the case of non-corporatization deals. It is equivalent to Figure 3 and follows an identical format. Panel (a) presents dynamic effects for mean inpatient prices. The coefficients were obtained by estimating Equation 3 on hospital-year level data derived from the commercial claims. The year prior to the deal is the omitted reference year. Panel (b) presents difference-in-differences (DD) coefficients obtained by estimating Equation 2 with average inpatient price as the dependent variable for the pooled sample and the seven largest patient cohorts, ranked in descending order by patient volume. The cohorts include the seven largest major diagnostic categories (MDCs) by inpatient volume. "CNS" denotes central nervous system. All regressions include hospital and year fixed effects and controls as described in Section 4. The figures present 95% confidence intervals with standard errors clustered by hospital; panel (a) also reports the p-value from an F-test of the joint significance of pre-treatment coefficients. Appendix Table A.6 presents the corresponding coefficients, noting the mean prices.

(a) Inpatient price



(b) Operating expenses per bed

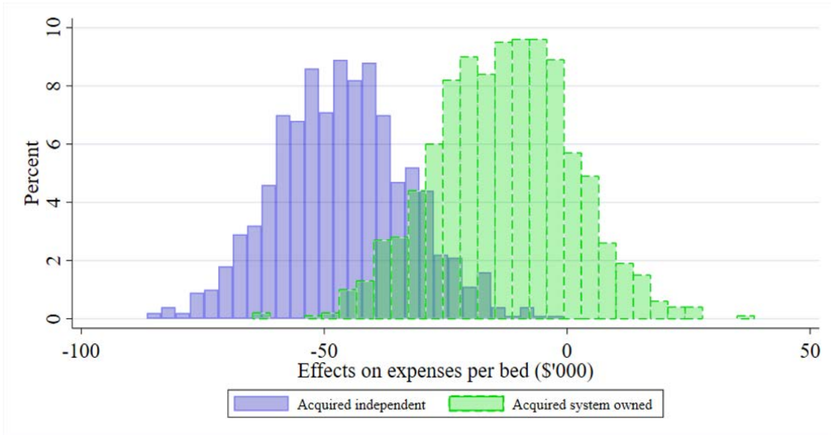


Figure A.5: Effect distributions for corporatization and non-corporatization deals

Notes: The figure presents the distributions of average DD effects obtained by estimating Equation 3 on 1,000 bootstrapped samples pertaining to corporatization and non-corporatization deals. The dependent variables are (a) the mean inpatient price and (b) total operating expenses per bed. All regressions include hospital and year fixed effects and hospital and market controls as described in Section 4. In the case of operating expenses per bed, a 2-sample t-test rejects the null hypothesis of equivalent mean values with $p < 0.001$.

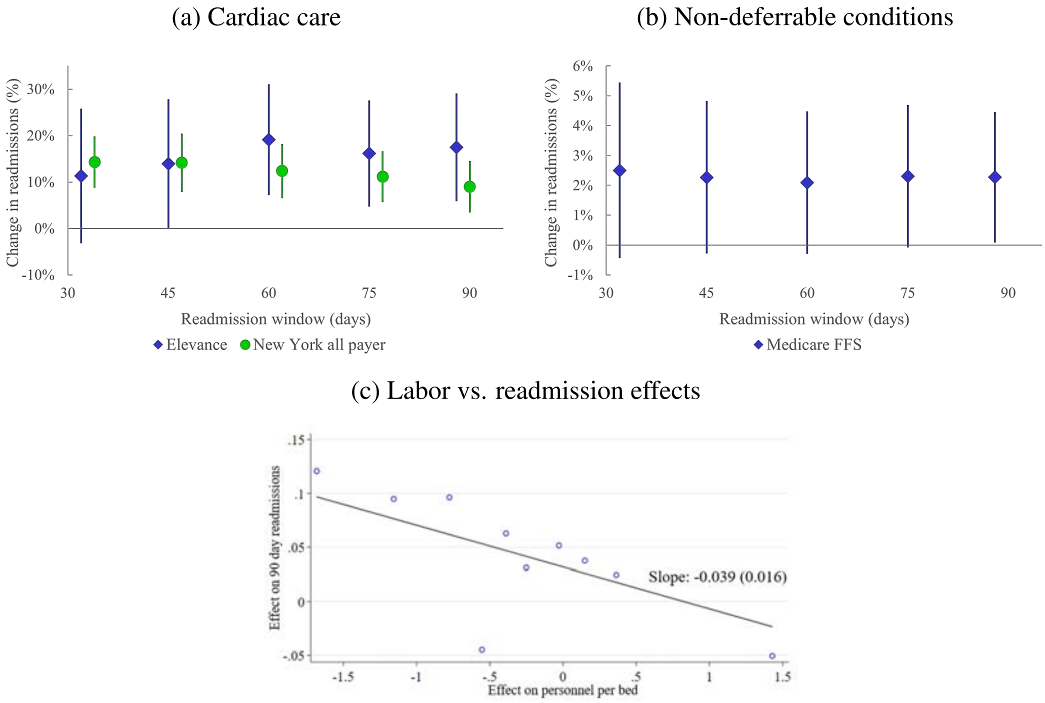


Figure A.6: Additional evidence on readmission rates

Notes: Panels (a) and (b) present estimated effects for readmission rates over different durations spanning 30–90 days following discharge in the cardiac care and non-deferrable conditions samples, respectively. The figures plot 95% confidence intervals, with standard errors clustered by hospital. Panel (c) presents a binned scatter plot of the effects of system ownership on employees in FTEs per bed (X-axis) against the effects on 90-day cardiac care readmissions in the commercial insurer sample (Y-axis). We first estimate our baseline DD specification for commercial cardiac care readmissions and FTE per bed for each target hospital against all comparison hospitals, thus obtaining deal-specific estimates. We then assess the correlation between these effects across the 101 deals by binning the individual effects into deciles. The figure also plots the linear fit and reports the slope coefficient from a linear regression using the underlying deal-level estimates. The standard error of the slope coefficient is obtained by bootstrapping over both steps to account for estimation error in the first step.

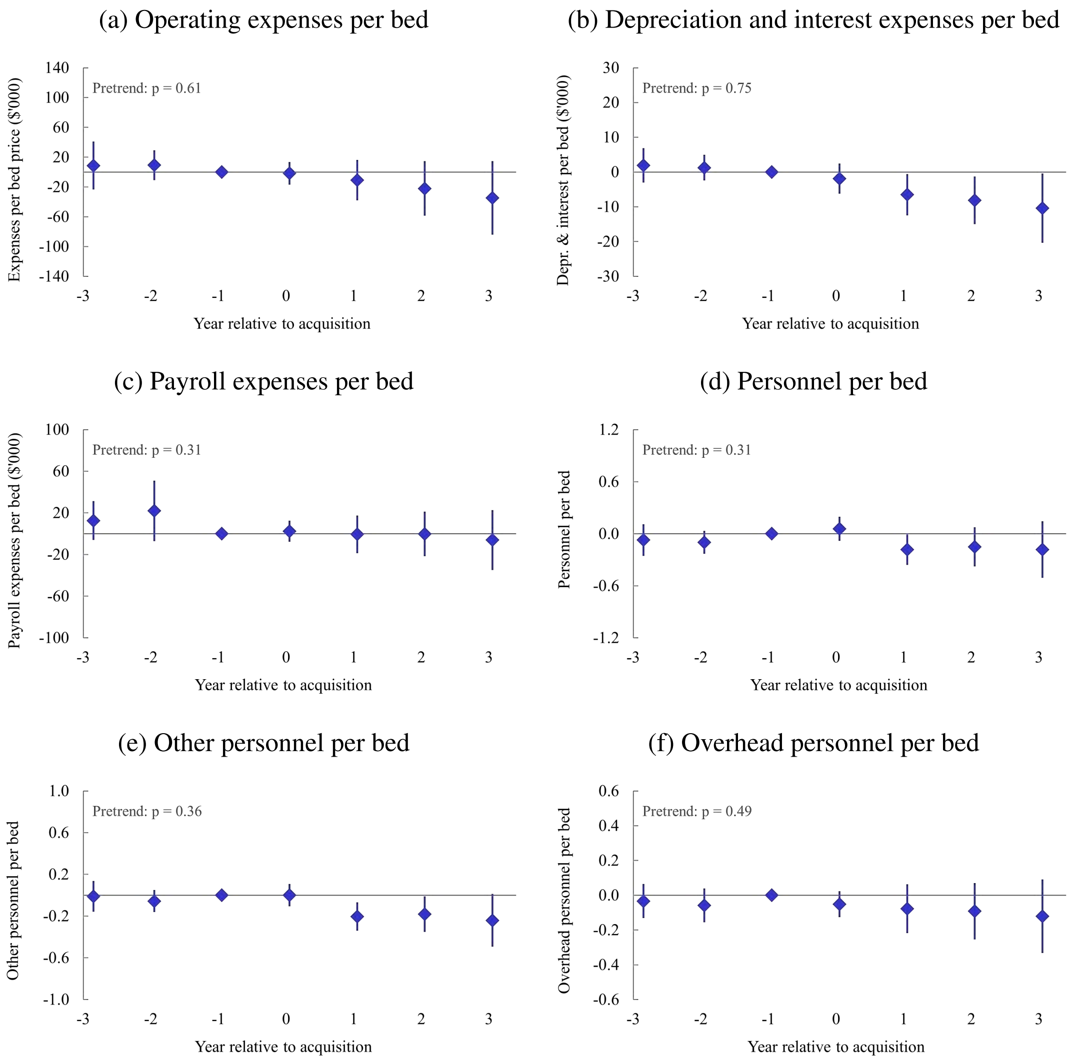


Figure A.7: Effects on hospital inputs (non-corporatization)

Notes: The figure presents event studies pertaining to the effect on inputs in the case of non-corporatization deals. It is equivalent to Figure 4 and follows an identical format. Dynamic effects obtained by estimating Equation 3 for various measures of hospital inputs are presented: (a) total operating expenses per bed, (b) depreciation as a measure of capital stock and interest expense per bed, (c) payroll and benefits spending per bed, (d) total full time equivalent (FTE) personnel per bed, (e) "other" FTE, defined as total FTEs less physicians, dentists, and nurses per bed, and (f) overhead FTEs per bed. Overhead FTEs are described in Section 5.2. All regressions include hospital and year fixed effects and hospital and market controls as described in Section 4. The year prior to the deal is the omitted reference year. The figures present 95% confidence intervals with standard errors clustered by hospital and p-values from F-tests of the joint significance of pre-treatment coefficients. Appendix Table A.8 presents the corresponding DD coefficients, noting mean values.

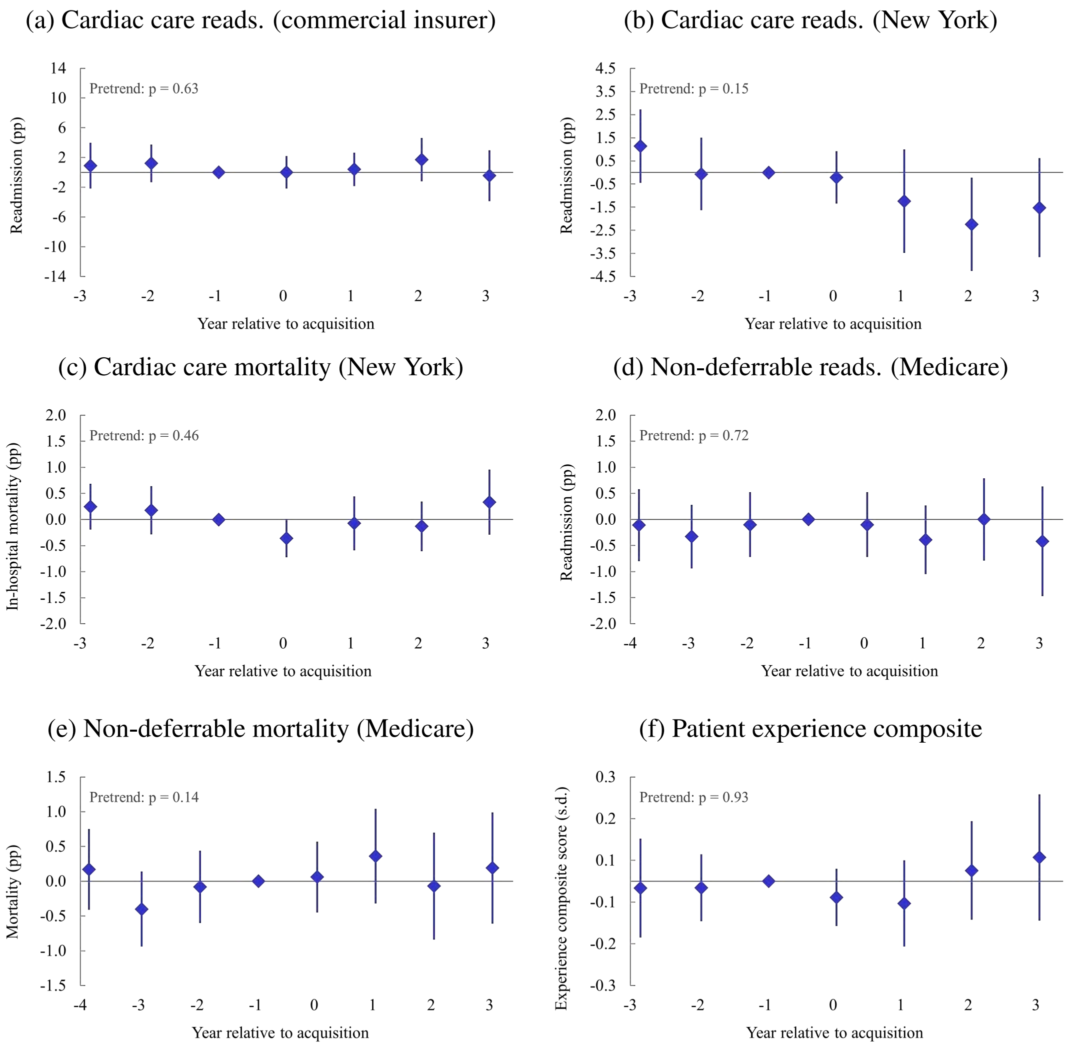


Figure A.8: Effects on hospital quality (non-corporatization)

Notes: The figure presents effects on target quality in the case of non-corporatization deals. It is equivalent to Figure 6 and follows an identical format. Dynamic effects obtained by estimating Equation 3 on various measures of hospital quality are presented. The year prior to the deal is the omitted reference year. Panel (a) presents results from a model with 90-day readmission rates following cardiac care admissions for the commercial patients as the dependent variable. Panel (b) reports results for the equivalent outcome and patients in the New York all-payer discharge data; panel (c) examines in-hospital mortality rates for the same patients. Panel (d) presents 90-day readmission results for patients admitted with non-deferrable conditions through the ED in the fee-for-service Medicare sample. Panel (e) examines 90-day mortality for the same patients. The Medicare sample starts in 2009, so we can include the fourth year prior to all deal instances. Panel (f) reports results for patient satisfaction scores obtained using hospital-year level patient experience data from the HCAHPS survey. The composite measure is the average z-score of five survey outcomes: the percent of patients that would recommend a hospital, that rated it $\geq$9 out of 10, that reported their nurses (doctors) communicated well, and that reported always receiving help quickly. All samples are limited to hospitals in the the commercial sample to maintain consistency. All regressions include hospital and year fixed effects and controls as described in Section 4. The figures present 95% confidence intervals based on standard errors clustered by hospital and display p-values from F-tests of the joint significance of pre-treatment coefficients. Appendix Table A.9 presents the corresponding DD coefficients, noting mean values.

Appendix Tables

Table A.1: Top Diagnosis Related Groups (Commercial Claims)

Diagnosis Related Group	Number of inpatient stays
Vaginal delivery	555,669
Major joint replacement of lower extremity	277,210
Cesarean section	193,429
Cesarean section with CC/MCC	113,366
Septicemia without MV > 96 hours with MCC	88,479
Spinal fusion	76,281
Vaginal delivery with CC/MCC	74,064
Percutaneous cardiovascular procedures with drug-eluting stent	66,149
Heart failure & shock with CC/MCC	49,379
Esophagitis & digestion disorders	43,037
Circulatory disorders with cardiac cath. excl. AMI	39,764
Simple pneumonia with CC/MCC	37,500
Major small & large bowel procedures with CC/MCC	32,295
Renal failure with CC/MCC	30,804
Cardiac arrhythmia with CC/MCC	30,114
Cellulitis	25,735
Psychoses	24,569
Cardiac arrhythmia	24,218
G.I. hemorrhage with CC	20,079
Other antepartum diagnoses with CC/MCC	20,003
Kidney & urinary tract infections	18,637
Cesarean section	17,854
Hip & femur procedures excl. major joint with CC	16,764
Miscellaneous disorders of nutrition	16,406
Chemotherapy with CC	16,405

Notes: The table lists the twenty-five DRGs with the largest patient volumes in the unmatched commercial sample. (M)CC refers to (major) complication or comorbidity. The number of inpatient stays corresponds to admissions in the commercial claims data over 2012–18. Appendix Section A.1 describes the restrictions used to arrive at this final sample in detail.

Table A.2: Standardized differences between acquired and comparison hospitals

	Corporatization		Non-corporatization	
	(1)	(2)	(3)	(4)
Variable	Full sample	Matched sample	Full sample	Matched sample
Price ($)	-0.01	0.00	-0.04	0.00
Operating expenses per bed ($'000)	-0.02	0.00	-0.10	-0.01
Personnel spend per bed ($'000)	-0.02	0.00	-0.09	0.00
Personnel per bed	-0.01	0.00	-0.08	0.00
Beds	0.00	0.00	-0.05	0.00
Teaching	-0.02	-0.01	-0.04	0.00
Rural	-0.01	0.00	0.01	-0.02
Percent Medicare of Medicaid	0.00	0.00	0.01	-0.01
Non profit	0.03	0.00	-0.05	-0.02
Patient profit margin	0.00	0.00	0.01	0.00

Notes: The table summarizes balance in hospital characteristics between treated and control hospitals before and after matching. The measure of balance is the standardized difference between the acquired and comparison groups of hospitals, computed as the difference in mean values divided by the standard deviation of the pooled sample. Cols. (1)–(2) report standardized differences between hospitals acquired in corporatization deals and never acquired controls, while cols. (3)–(4) present the corresponding standardized differences between non-corporatization targets and their comparison hospitals. Cols. (1) and (3) report standardized differences for the full samples, while cols. (2) and (4) report the corresponding values for the matched samples. We match treated and control hospitals using coarsened exact matching on covariates as described in Section 4. In each sample, we calculate standardized differences using data from the year prior to the deal for target hospitals, and years 2012–2017 for control hospitals.

Table A.3: Changes in patient mix

Dependent variable:	(1) DRG weight	(2) Age	(3) Length of stay	(4) Elixhauser score
Panel A: Pooled sample (Commercial claims)				
Acquired * post	0.007	0.094	-0.011	-0.003
	(0.016)	(0.252)	(0.036)	(0.024)
Observations	448,829	448,829	448,829	448,829
Dependent var. mean	0.91	39.35	3.76	0.82
Panel B: Cardiac care (Commercial claims)				
Acquired * post	0.078	-0.409	0.031	-0.047
	(0.050)	(0.520)	(0.107)	(0.125)
Observations	32,440	32,440	32,440	32,440
Dependent var. mean	2.59	63.95	4.66	3.41
Panel C: Cardiac care (New York)				
Acquired * post	0.117	-1.027	-0.384	-0.103
	(0.152)	(0.544)	(0.221)	(0.076)
Observations	129,037	129,037	129,037	129,037
Dependent var. mean	2.91	68.41	4.61	1.13
Panel D: Non-deferrable (Medicare)				
Acquired * post	-0.010	-0.031	-0.032	-0.006
	(0.010)	(0.049)	(0.049)	(0.021)
Observations	795,887	795,887	795,887	795,887
Dependent var. mean	1.59	81.71	6.03	3.02

Notes: The table presents results for various measures of patient mix, obtained by estimating Equation 2 on patient-level data. Panels A and B report results using the commercial insurer's pooled and cardiac care samples, respectively. Panel C reports results for the New York cardiac care cohort, and Panel D for the Medicare non-deferrable conditions cohort. Each of these samples is used to compute effects on prices and/or readmission rates. We do not observe the full claims history for the year prior to the index admission for all patients in the commercial sample, so we compute the Elixhauser score in Panel A based on diagnoses and procedures observed during the index stay. For patients in the cardiac care sample, we compute the Elixhauser score based on the full prior year history across inpatient and outpatient care (excluding ED visits). In the New York data, we do the same, except that we can only observe inpatient history. All regressions include hospital and year fixed effects and DRG weight controls (except for col. 1). Standard errors are clustered by hospital and are presented in parentheses. The dependent variable mean is computed over treated hospitals in the year prior to the deal.

Table A.4: Changes in patient volume

	All patients		Cardiac care (comm. claims)		Cardiac care (NY)	
Dependent variable:	Volume	Volume per bed	Volume	Volume per bed	Volume	Volume per bed
	(1)	(2)	(3)	(4)	(5)	(6)
Acquired * post	-89.38	-1.660	-2.320	-0.003	-30.10	-0.012
	(177.8)	(0.997)	(1.641)	(0.008)	(45.45)	(0.139)
Observations	2,189	2,189	1,816	1,816	294	294
Dependent var. mean	9,894	43.984	22.8	0.092	308	0.724

Notes: The table presents results for patient volumes, obtained by estimating Equation 2 on hospital-year level data. Columns (1)–(2) report results for all hospital inpatients (across payers and service lines) and inpatients per bed using the AHA survey data. Columns (3)–(6) report the same measures for the cardiac care subsample; cols. (3)–(4) use commercial claims, while columns (5)–(6) use the NY discharge data. All regressions include hospital and year fixed effects and hospital and market controls as described in Section 4. Standard errors are clustered by hospital and presented in parentheses. The dependent variable mean is computed over acquired hospitals in the year prior to the deal.

Table A.5: Heterogeneity in treatment effects: Triple difference models

Dependent variable:	Price per inpatient stay		Operating expenses per bed		Readmissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline estimate	855.9	–	-48,281	–	0.0291	–
	(323.1)	–	(13,729)	–	(0.0099)	–
1. Target attribute						
A. Below med. pre-deal price	590.0	548.0	-29,614	-38,423	0.0235	0.0147
	(433.3)	(578.1)	(18,809)	(21,055)	(0.0120)	(0.0152)
B. Below med. target size	721.8	264.5	-50,994	5,344	0.0275	0.0066
	(416.2)	(575.1)	(13,432)	(21,914)	(0.0106)	(0.0172)
2. Acquirer attribute						
A. Above med. system size	931.3	-143.9	-6,644	-79,574	0.0212	0.0152
	(485.2)	(578.6)	(16,343)	(20,452)	(0.0138)	(0.0157)
B. For-profit system	914.1	-324.2	-31,462	-93,489	0.0301	-0.0061
	(354.1)	(690.2)	(13,614)	(30,938)	(0.0102)	(0.0220)
3. Deal attribute						
A. In-HRR	634.8	438.8	-65,122	33,361	0.0309	-0.0035
	(470.8)	(580.5)	(17,204)	(22,048)	(0.0117)	(0.0148)
B. Above 100 ΔHHI	770.7	285.5	-55,518	24,231	0.0325	-0.0224
	(400.5)	(550.2)	(16,016)	(21,893)	(0.0090)	(0.0258)
Observations	2,189	2,189	2,189	2,189	32,440	32,440
Dependent var. mean	15,459	15,459	1,003,081	1,003,081	0.166	0.166

Notes: The table presents results on heterogeneity in treatment effects across targets (Panel 1), acquirers (Panel 2), and deals (Panel 3). It presents coefficients obtained by estimating a triple difference model on the full sample with an additional coefficient on (Post x Acquired x Attribute of interest). Odd columns report the difference-in-differences coefficient (Post x Acquired), and even ones the triple difference coefficient (Post x Acquired x Attribute). The dependent variables are the mean price per inpatient stay (cols. (1)–(2)), operating expenses per bed (cols. (3)–(4)), and 90-day readmissions in the cardiac care Elevance sample (cols. (5)–(6)). Each row presents results from a different regression model. The first attribute of interest (in Panel 1 Row A) is an indicator equal to one if the target's pre-deal price was less than the median across all targets. Panel 1 Row B indicates if the target is smaller (in beds) than the median target. Panel 2 Row A presents results for deals where the acquiring system is larger (owns more hospital members) than the median acquirer. Panel 2 Row B presents results for deals where a majority of members in the acquiring system are for-profit. Panel 3 highlights heterogeneity across deals based on the change in concentration. In-HRR indicates that the target and at least one hospital of the acquiring system share the same HRR (Row A). In Panel 3 Row B we focus on deals where we predict an increase in HHI of at least 100. All models include hospital and year fixed effects and controls as described in Section 4. Standard errors are clustered by hospital and presented in parentheses. The dependent variable mean is computed over treated hospitals in the year prior to the deal.

Table A.6: Effects on prices (non-corporatization)

Dependent variable price:	Pooled	Delivery	Cardiac care	Musculo-skeletal
	(1)	(2)	(3)	(4)
Acquired * post	957.0	350.5	614.7	654.9
	(354.6)	(254.8)	(961.5)	(963.3)
Observations	1,425	1,275	1,380	1,372
Dep. var. mean ($)	16,780	9,284	22,439	30,275
	Digestive	Respir-atory	CNS	Infectious disease
	(5)	(6)	(7)	(8)
Acquired * post	497.2	1,745	2,120	813.3
	(763.4)	(794.2)	(1,369)	(2,228)
Observations	1,254	1,231	1,095	1,133
Dep. var. mean ($)	16,402	16,361	18,839	23,988

Notes: The table presents the DD effects on inpatient price for target hospitals involved in non-corporatization deals. It is equivalent to Table 3 and has an identical format. It presents coefficients obtained by estimating Equation 2 with mean inpatient price as the dependent variable on hospital-year level data. Col. (1) presents results for the pooled sample and cols. (2)–(8) for the top seven diagnostic categories by volume in the commercial claims. Each category is weighted to match the average hospital wide DRG distribution in the New York discharge data during our sample period. Cols. (2)–(8) are major diagnostic categories defined using DRGs. All regressions include hospital and year fixed effects and patient, hospital, and market controls as described in Section 4. Standard errors are clustered by hospital and presented in parentheses. The dependent variable mean is computed over treated hospitals in the year prior to the deal.

Table A.7: Changes in patient mix (non-corporatization)

	(1)	(2)	(3)	(4)
Dependent variable:	DRG weight	Age	Length of stay	Elixhauser score
Panel A: Pooled sample (Commercial claims)				
Acquired * post	0.026	0.180	-0.018	0.001
	(0.0128)	(0.199)	(0.053)	(0.001)
Observations	393,684	393,684	393,684	393,684
Dependent var. mean	0.89	38.81	3.66	0.87
Panel B: Cardiac care (Commercial claims)				
Acquired * post	-0.043	0.306	-0.058	-0.061
	(0.053)	(0.406)	(0.114)	(0.074)
Observations	28,579	28,579	28,579	28,579
Dependent var. mean	2.77	64.08	4.90	3.32

Notes: The table presents results for various measures of patient mix at target hospitals involved in non-corporatization deals. The coefficients are obtained by estimating Equation 2 on patient-level data. Panels A and B report results using the commercial insurer's pooled and cardiac care samples, respectively. Each of these samples is used to compute effects on inpatient prices and readmission rates. We do not observe the full claims history for the year prior to the index admission for all patients in the commercial sample, so we compute the Elixhauser score in Panel A based on diagnoses and procedures observed during the index stay. For patients in the cardiac care sample, we compute the Elixhauser score based on the full prior year history across inpatient and outpatient care (excluding ED visits). All regressions include hospital and year fixed effects and DRG weight controls (except for col. 1). Standard errors are clustered by hospital and are presented in parentheses. The dependent variable mean is computed over treated hospitals in the year prior to the deal.

Table A.8: Effects on inputs and service portfolio (non-corporatization)

Panel A: Expenses				
Dependent variable:	Operating expenses	Deprec. and interest	Payroll expenses	Misc. expenses
	(1)	(2)	(3)	(4)
Acquired * post	-14,636	-5,721	-7,925	-655.3
	(13,521)	(2,574)	(6,619)	(11,945)
Observations	1,425	1,178	1,425	1,178
Dependent var. mean ($)	997,847	75,897	477,513	450,198

Panel B: Personnel categories				
Dependent variable:	Total personnel	Other personnel	Overhead	Contract
Acquired * post	-0.033	-0.094	-0.049	-0.005
	(0.081)	(0.060)	(0.048)	(0.017)
Observations	1,425	1,425	1,214	1,425
Dependent var. mean	5.4	3.6	1.8	0.2

Panel C: Service portfolio				
Dependent variable:	Number of services	Number of tech services	Cardiac extensive margin	Deliveries extensive margin
Acquired * post	0.962	0.196	-0.024	-0.040
	(2.044)	(0.521)	(0.027)	(0.028)
Observations	1,425	1,425	1,898	1,804
Dependent var. mean	63.3	15.7	0.8	0.9

Notes: The table presents the effects on inputs for target hospitals involved in non-corporatization deals. It is equivalent to Table 5 and has an identical format. It presents coefficients obtained by estimating Equation 2 on hospital-year level data with various measures of inputs and service portfolio as the dependent variables. Panel A presents results for total operating expenses (col. 1), depreciation and interest costs (col. 2), payroll and benefits payments (col. 3), and miscellaneous costs (col. 4, defined as all remaining expenses). The outcomes are expressed in 2018 dollars and normalized by the number of beds in the hospital's first year in the sample. Panel B examines total FTE employees per bed and specific components: overhead, contract, and other employees. Total FTE captures all personnel on the hospital payroll except for trainees and interns. Other FTE is defined as the total less physicians, dentists, and nurses. Overhead and contract labor are sourced from hospital cost reports. Overhead includes administrative, benefits, maintenance, and other general services. Contract labor includes contracted personnel engaged in direct patient care, management, and overhead activities. Panel C examines various measures of service portfolio. Number of services (col. 1) counts the services offered by the hospital as recorded in the AHA survey. Number of technology services (col. 2) counts the subset of services that rely on technology, identified by consulting with physicians. Cardiac and deliveries extensive margins are coded based on whether we observe these claims in the commercial patient sample. With the exception of cols. (3) and (4) in panels B and C, all outcomes are sourced from the AHA survey. All regressions include hospital and year fixed effects, plus hospital and area covariates as described in Section 4. Standard errors are clustered by hospital and presented in parentheses. The dependent variable mean is computed over treated hospitals in the year prior to the deal.

Table A.9: Effects on quality (non-corporatization)

	(1)	(2)	(3)
	Panel A: Cardiac care		
	Commercial claims	New York	
	Readmissions	Readmissions	Mortality (in-hospital)
Acquired * post	-0.0002	-0.0101	-0.0021
	(0.0099)	(0.0072)	(0.0014)
Observations	28,579	100,094	100,094
DV mean	0.174	0.156	0.014
	Panel B: Non-deferrable conditions		Panel C: Patient experience
	Medicare		
	Readmissions	Mortality	Composite score
Acquired * post	-0.0009	0.0021	-0.0168
	(0.0022)	(0.0024)	(0.0484)
Observations	650,290	650,290	1,423
DV mean	0.250	0.182	-0.153

Notes: The table presents results on the effect on quality at target hospitals involved in non-corporatization deals. It is equivalent to Table 6 and has an identical format. It presents coefficients obtained by estimating Equation 2 with various hospital quality measures as the dependent variables. Panel A presents results for cardiac care patients. The dependent variable in col. (1) is 90-day readmissions from the commercial claims data. Cols. (2) and (3) use 90-day readmissions and in-hospital mortality, respectively, from the New York discharge data. In the fee-for-service Medicare claims data, we are able to refine the dependent variable by limiting index stays to those originating in the ED for various acute, non-deferrable conditions. Section 6 describes how we identified non-deferrable cases. Results from this sample are presented in Panel B. Cols. (1) and (2) report results for 90-day readmissions and 90-day mortality, respectively. Panel C reports results for hospital-year level patient satisfaction scores from the HCAHPS survey. The composite measure is the average z-score of five survey outcomes: the percent of patients that would recommend a hospital, that rated it $\geq$9 out of 10, that reported their nurses (doctors) always communicated well, and that reported always receiving help quickly. All samples are limited to hospitals in the commercial claims sample to maintain consistency. All regressions include hospital and year fixed effects and various covariates described in Section 4. Standard errors are clustered by hospital and presented in parentheses. The dependent variable mean is computed over acquired hospitals in the year prior to the deal.

Table A.10: Robustness (non-corporatization)

	(1) Price ($/stay)	(2) Operating cost ($/bed)
Baseline estimate	957.0	-14,636
	(354.6)	(13,521)
1. **Alternate covariates**		
A. No controls	845.0	-12,604
	(436.0)	(12,883)
B. No DRG weights	837.0	-14,636
	(373.5)	(13,521)
C. DRG fixed effects	1,374	-14,092
	(392.6)	(13,636)
D. Alternate HHI control	973.6	-10,185
	(360.2)	(12,541)
2. **Alternate specifications**		
A. Logged outcome	0.061	-0.018
	(0.021)	(0.012)
B. Patient weighted	719.0	-20,876
	(295.3)	(14,189)
C. Patient-level, DRG fixed effects	264.1	–
	(228.9)	–
D. Exclude deal year	1,376	-21,158
	(404.5)	(17,558)
E. Callaway Sant'Anna	512.8	-4,230
	(477.1)	(15,561)
3. **Alternate comparison groups**		
A. Non-neighbor control group	937.1	-15,291
	(357.7)	(14,463)
B. Balanced treatment panel	1,273	-4,272
	(518.2)	(15,955)
C. Callaway Sant'Anna with	539.1	4,879
not yet treated controls	(466.1)	(15,166)
D. Matching on profits	900.0	-9,093
	(356.4)	(15,243)
E. Matching corp. deal characteristics	994.3	-13,975
	(367.9)	(13,173)
F. No matching	411.6	-39,035
	(321.5)	(13,010)
4. **Alternate clustering level**		
A. Clustered by deal	957.0	-14,636
	(380.8)	(14,259)
B. Clustered by HRR	957.0	-14,636
	(369.6)	(15,672)
Observations	1,425	1,425
Dependent var. mean	16,780	997,847

Notes: The table presents results from four types of robustness checks for the results pertaining to non-corporatization deals. Col. (1) reports results for inpatient prices from the commercial claims data, while col. (2) reports for operating expenses per bed from the AHA survey. All values are expressed in 2018 dollars. Estimates are obtained using hospital-year level data. Panel 1 tests sensitivity to varying covariates in the baseline model. Row 1D reports results when an alternate HHI, computed using more granular "Health Service Areas," replaces the HRR-based measure. Panel 2 presents results from alternate specifications. In row 2A, the outcome is log-transformed. Row 2B presents results from the preferred specification when weighted by inpatient volume in the first year we observe the hospital. We use commercial patient stays and AHA all-payer admissions as weights in cols. (1) and (2), respectively. Row 2C estimates the baseline model on patient-level data instead. Row 2E reports the estimator proposed by Callaway and Sant'Anna (2020), which is robust to bias due to the staggered treatment design. We explicitly exclude not-yet-treated hospitals as potential controls in this exercise. Panel 3 presents results from the baseline model using alternate comparison group hospitals. In row 3A, we exclude never-acquired hospitals located within 5 miles of any acquired hospital ('neighbors'). Row 3B reports results for a sample where we only retain acquired hospitals with data for 2 years pre and post the deal year, which varies across different hospitals. Comparison hospitals are retained if they have at least 5 years of data. Row 3C reports the Callaway and Sant'Anna estimator using not yet treated hospitals as controls. Row 3D presents results using a modified matching algorithm where we also match on hospital profit margin prior to the deal. Row 3E presents results obtained by re-weighting the acquired hospitals using matching weights generated by coarsened exact matching targets to those from corporatization deals on deal characteristics (acquirer size, target-acquirer distance, and predicted change in HHI). Row 3F reports results using the full hospital samples without matching. Panel 4 presents results obtained by clustering standard errors at higher levels. The dependent variable mean and observations pertain to the baseline estimates; the dependent variable mean is for acquired hospitals in the year prior to the deal.

A Online Appendix

A.1 Datasets and Sources:

Elevance claims: Elevance Blue Cross Blue Shield (BCBS) is one of the largest private health insurers in the US and the largest for-profit healthcare organization in the BCBS Association. Elevance is a licensee of BCBS plans in 14 states including CA, CO, CT, GA, IN, KY, ME, MO, NV, NH, NY, OH, VA, and WI. Elevance also has members in each US state through its national accounts. We selected six of these states with the largest commercial populations during the study period: FL, IL, NC, NJ, TX, and PA. In 2018, the last year of our study, Elevance had approximately 24 million individuals enrolled in its commercial products (excluding Medicare Advantage).

The data include medical claims for individuals with fully-insured and self-insured (employer-sponsored) coverage. Employer-sponsored plans include national, large, and small accounts. The data contain member-level information, as well as inpatient, outpatient, and professional / physician claims. We use these data to create our inpatient sample, identify index procedures and inpatient stays, and construct other individual-level characteristics. Importantly, our data also contain information on actual negotiated prices paid to providers during the study period. Specifically, we have data on plan-paid and patient-paid amounts, as well as any amount paid by a third party. We focus on the total negotiated price that includes all three components of payments for services rendered.

The claims data also include unique hospital identifiers that allow us to merge in hospital characteristics and analyze variation in outcomes across providers. While the claims data do not always contain the hospital Medicare IDs assigned by the Centers for Medicare and Medicaid Services (CMS), we also have access to a wide array of other hospital identifiers, such as National Provider Identifiers (NPIs) and Tax IDs. We used a separate Elevance-generated hospital-level table that maps hospital IDs from claims to CMS IDs. Specifically, we use hospital tax IDs to assign each provider from the claims to a CMS ID. We then use CMS IDs to combine the claims with hospital characteristics from the American Hospital Association (AHA) annual survey.

Traditional Medicare claims: We obtained access to a 100% sample of hospital inpatient claims for fee-for-service Medicare beneficiaries over 2009–17 through a data use agreement with CMS. We also observe beneficiary enrollment and demographic information during this period, which allows us to create indicators for mortality at different durations following discharge from the stay. Each row of the inpatient file pertains to a distinct hospital stay. We observe the dates of service, the hospital CMS ID, diagnosis and procedure codes (up to 10), the DRG associated with the stay, and the payment amount.

New York hospital discharge data: These data constitute the universe of hospital discharges in the state of New York from 2010-18. Each record summarizes one discharge abstract from an inpatient or emergency department hospital setting.[50] These records are available for research purposes in the New York State Inpatient Database (SID) as part of the Healthcare Cost and Utilization Project (HCUP), sponsored by the Agency for Healthcare Research and Quality (AHRQ). New York offers three key advantages for our study: it is one of the most populous states, it is an Elevance state, and it experienced many hospital acquisitions during the study period.

The SID contain clinical and resource-use information for visits for all expected payers, including Medicare, Medicaid, private insurance, self-pay, and "no charge." Available data elements include the patient's age, gender, ZIP code of residence, primary payer, and procedure and diagnosis codes. HCUP also provides a unique patient identifier and synthetic timing variable to track patients over time and across hospital settings within New York. The SID can be merged to hospital-level AHA data using HCUP's AHA Linkage Files.

American Hospital Association annual survey: We obtain data on hospital characteristics from the AHA annual survey over 2010–18. We use hospital location, non-profit status (public, for-profit, or non-profit), system ownership, size, service portfolio, finances (e.g., expenses), and personnel variables. AHA collects responses from over 6,200 hospitals each year and has administered the survey since 1946. Additional information on the survey data is available at https://www.ahadata.com/aha-annual-survey-database.

Healthcare Cost Report Information System hospital cost reports: We obtain additional data on hospital employment and finances from HCRIS annual hospital cost reports. All Medicare-certified hospitals are required to submit an annual cost report to a Medicare Administrative Contractor, which is publicly available for fiscal years after 1995 on the Center for Medicare & Medicaid Services' website (https://www.cms.gov/Research-Statistics-Data-and-Systems/Downloadable-Public-Use-Files/Cost-Reports/Hospital-2010-form).

Employment information is contained in Worksheet S-3, Parts I and II. We combine all contract labor rows (relating to direct patient care, top level management and other management and administrative services, physician-Part A administrative, general and administrative, housekeeping, and dietary). We aggregate all overhead labor rows (reported in Part II under "Overhead costs - Direct Salaries"). Following Prager and Schmitt (2021), we convert paid hours into full time equivalent employees assuming a 40 hour work week. We also adopt their cleaning methodology, which trims negative values, 5% outliers in each year, and outliers within each hospital. Missing values are then imputed by averaging values across adjacent years within hospitals.

Net revenue and net income from service to patients are found in Worksheet G-3. We trim negative revenues, then calculate the patient profit margin as the ratio of net income to net profits. Last, we exclude 5% outliers. The resulting margins are between -1 and 1 in our sample.

American Community Survey data: We use the following variables from the American Community Survey conducted by the US Census Bureau: the percentage of employed working age adults (16+ years of age), of adults with some college education or higher (25+), of individuals

50. Our study does not use hospital ambulatory surgery visits, which are also available.

below the poverty line, of elderly individuals (65+), and of white individuals.

Irving Levin Associates' HealthcareMandA.com deal database: These data include deal characteristics for mergers and acquisitions in 13 healthcare industries, including hospitals. We use deals from 2012–18 to cross-check hospital deals identified from the AHA survey files.

Patient Experience Measures: We use patient experience measures from the Hospital Consumer Assessment of Healthcare Providers and Systems (HCAHPS) survey (obtained from https://data.cms.gov/provider-data/). These are aggregated to the hospital level and publicly reported on the CMS Hospital Compare webpage (https://www.medicare.gov/care-compare/). The survey is intended to measure patients' perceptions of their hospital experience. It is administered annually to a random sample of adult patients across medical conditions and payers who are admitted to hospitals serving Medicare or Medicaid patients, including all hospitals in the Elevance sample.[51] We use five patient experience measures that are commonly acknowledged signals of care quality and are available for all years of the study period, 2012-18. Specifically, we use the fraction of patients that would definitely recommend the hospital, that rated it 9 or more out of 10, that reported their nurses and doctors always communicated well, and that reported always receiving help quickly.

A.2 Defining the Inpatient Sample:

Pooled analyses include all inpatient stays except for those with primary psychiatric diagnoses (as defined by the CMS Center for Outcomes Research Evaluation criteria) and abortion procedure codes. An inpatient stay is defined by calculating the length of stay based on inpatient claims that include the admission date and the discharge date. We retain all medical claims for individuals in the 12 months before and 24 months following inpatient stays for cardiac care stays. This allows us to describe the patients' health care use before and after the care episode (e.g., readmissions). Cardiac care stays were identified using a combination of CPT codes and ICD9/ICD10 codes in consultation with physicians.

Sample restrictions: For all inpatient stays, we limit our analysis to adults over the age of 18. We exclude cases with lengths of stay above the 99th percentile, which may be unusually complicated or reflect clerical billing errors. We also drop newborn (789-795), ungroupable (998-99), and rare DRGs from our sample (defined as having fewer than 25 observations per year, on average). All observations must match to non-federal, short-term, non-critical access hospital, general acute care hospitals in the AHA data. We further limit our sample to hospitals with at least 15 inpatient observations that are located in the 20 states where Elevance has a significant presence (detailed in Section A.1)

We exclude observations with prices below the 1st percentile or above the 99th percentile and limit the sample to commercial employer sponsored insurance claims. For our readmission analyses, we retain both commercial and Medicare Advantage claims, but exclude observations with a prior hospitalization in the last 90 days or that were transferred out of the hospital. We

51. Additional details are available at: https://www.cms.gov/Medicare/Quality-Initiatives-Patient-Assessment-Instruments/HospitalQualityInits/HospitalHCAHPS.

also exclude individuals with fewer than 90 days of insurance eligibility following their discharge from the index event from our analyses with 90-day readmissions. All pregnancy analyses exclude individuals with ages above the 99th percentile. For our hospital input analyses, we rely on data from the AHA survey, which are only reported at the more aggregated hospital-year level. We winsorize rather than trim observations with values below the 1st percentile or above the 99th. In accordance with our research design, we limit our sample to observations from independent and acquired hospitals. Our control group is comprised of hospitals that were not system owned during 2008–2018, and our treated group includes hospitals that were acquired or joined a system during 2013–2017. Additional details are in the next section, "Identifying hospital acquisitions." We further limit our sample of acquired hospitals to a maximum of four years prior to and three years after the deal.

After conditioning our data to inpatient cases delivered at hospitals that are registered with the AHA, we have 5,279,863 cases delivered at 5,706 facilities between 2012 and 2018. We limit our data to commercial and Medicare Advantage patients age 18 or older and retain 4,433,310 cases at 5,648 hospitals. After excluding critical access, specialty, and federal hospitals, we have 4,168,657 cases in 3,300 hospitals. Conditioning on hospitals in the 20 Elevance states, we retain 3,649,006 cases in 2,097 hospitals. Excluding length of stay outliers and hospitals with fewer than 50 cases, we have 3,523,208 cases and 1,453 hospitals. Finally, we limit our sample to acquired and never acquired hospitals. This substantially focuses our sample size to 630,290 cases and 500 hospitals.

We apply the same sample restrictions for the Elevance data to the New York discharge data, except we include records from all expected payers and cannot exclude individuals based on insurance eligibility. We limit New York hospitals to those observed in the Elevance data.

We use the following sample restrictions to examine the effects on mortality for fee-for-service Medicare patients: we start with all inpatient admissions for hospitals in the 20 Elevance states of interest. We retain all patients who are 65 or older and enrolled for at least 12 months in Medicare parts A and B before the admission. This ensures that we can compute risk indicators for each patient. We retain claims for patients discharged between Jan 1, 2009 and Sept 30, 2017; this ensures that we can observe 90-day mortality outcomes for all patients from the discharge date. We exclude stays longer than 365 days, as well as about one-third of patients with a prior hospital stay in the last 90 days before the admission of interest. Finally, we drop hospitals with fewer than 50 patients over the sample period after the above exclusions.

To identify patients admitted through the ED for non-deferrable conditions, we use the diagnosis codes listed in Appendix 1 of Doyle et al. (2015). Since our sample period spans the period of both ICD9 and ICD10 diagnosis codes, we used the equivalent ICD10 codes for the period after September 2015.

A.3 Identifying hospital acquisitions:

We use the AHA annual surveys to identify changes in hospital system ownership, which we then validate by hand and against a supplementary M&A database. The AHA data contain a unique system ID that allows one to determine if a hospital is independently owned and track changes in hospital system ownership over time. For example, if the system ID of a hospital changes between 2012 and 2013, we infer that the hospital experienced a change in system ownership in 2013.

We manually validated each hospital deal through Internet searches of public (hospital websites, press releases, and news articles) and proprietary sources (American Hospital Directory). Each deal was validated by two people, and any conflicting or ambiguous cases were resolved with a third person. We supplemented the AHA data with the Irving Levin Associates' HealthcareMandA.com Deal Database, which contains detailed information about the parties involved, announcement, and closing dates of acquisitions. We confirmed that each deal in the Irving Levin data matched to the details of one identified in the AHA data.

As previous researchers have noted, the AHA survey occasionally consolidates two merging hospital IDs into a single ID (Cooper et al. 2019). We excluded hospitals that were deleted from the AHA sample in conjunction with a merger, or that merged another hospital into them (N = 10 hospitals in the Elevance sample). We also recoded hospitals that the AHA reports as belonging to management and consulting company "systems" as independent; the largest of these companies is Quorum Health Resources (N = 12 hospitals in our sample). The AHA survey also occasionally introduces new systems, which may reflect system "creations," in which individual hospitals come together to form new systems. We identified 15 potential system creations from the AHA data. We excluded two hospitals involved in bona fide creations and recoded 13 others as previously acquired (in instances where the AHA added missing but pre-existing systems to the data). For each hospital, we consider the first deal within our study period, and we exclude deals that only involve one year of system ownership.

We separately categorize corporatization and non-corporatization deals as those in which a target transitions from independent to system-owned and from one system's ownership to another, respectively.

A.4 Matching treated and control hospitals:

We use coarsened exact matching to match each target hospital to control hospitals in the year before the acquisition. We match on the following hospital and area characteristics: above median bed size, rural status, non-profit ownership, above median hospital Medicare and Medicaid share, quartile of expenses per bed, and lagged quartile of expenses per bed. We selected the first four variables because they are strongly correlated with the probability of treatment, and the latter two because they exhibited differential pre-trends for hospitals acquired in non-corporatization deals in unmatched regressions. We summarize the balance of these characteristics and others before and after matching in Table A.2. We use weights supplied by the "cem" command in STATA to implement our matched regressions. Control hospitals may be matched to target hospitals in multiple years, in which case we use the sum of their weights.