The Corporatization of Independent Hospitals*

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December 2022

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 over $1 trillion in annual spend. However, little is known about the effects of system ownership on hospital operations and quality. We combine novel, patient-level data with transaction prices from a large commercial insurer, Medicare claims, and New York hospital discharges between 2012 and 2018 to study changes at over 120 independent hospitals that transition to system ownership. These hospitals obtain differentially higher prices, but the gains from operating cost reductions, primarily by reducing employees in support functions, are far greater and imply economies of scale. However, readmission rates meaningfully worsen, with suggestive evidence of a trade-off with decreasing labor inputs.

*We are grateful to Jenn Kowalski and Andy Ellis of Elevance Health for providing industry insights; Yaa Akosa Antwi, Zack Cooper, Josh Gottlieb, Ryan McDevitt, Adam Sacarny, Matt Schmitt, Chris Stewart, and seminar participants at Lehigh University, Southern Economic Association, the Becker Friedman Institute, ASHEcon (Austin), and the Southeastern Health Economics Study Group offered helpful comments and suggestions. Funding from the Wharton Dean's Research Fund (Gupta) is greatly appreciated. Roshie Xing and Bill Zhang provided outstanding research assistance. We gratefully acknowledge support through National Institute of Aging pilot grant P01AG005842-31. The findings, interpretations, and conclusions expressed in the paper are entirely those of the authors and do not necessarily represent the views of Elevance health.

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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 rapidly growing influence of large firms throughout the US healthcare sector (Dranove and Burns, 2021). Evidence from hotels, restaurants, and retail sectors has shown that standalone units gain several benefits from affiliating with multi-unit firms, such as a reputable brand name, access to capital and knowledge, 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 even greater scrutiny in this sector. Surprisingly, there is very limited evidence on the causal effects of chain ownership in healthcare. This paper provides novel evidence by studying this question in the context of the US hospital industry.

The hospital industry is uniquely well suited to study this question. First, multi-unit firms, known as hospital systems, have rapidly expanded their presence and now dominate the industry – the share of national bed capacity under system control increased from 58% in 2000 to 81% by 2020 (Figure 1a). Hospital systems have similarly increased their share of total employment. Formerly independent hospitals experience a large shock to operational sophistication when they enter a system. In the median such transition, an independent hospital joins a firm that already owns five hospitals and serves over eight times more patients. We refer to this phenomenon as the corporatization of hospital care.[1] If large firms operate hospital establishments more efficiently, corporatization may lead to aggregate improvements in efficiency. Second, the effects of system ownership or size on hospital quality are not well known, since prior studies have typically examined the effects of hospital size (Gaynor, Rebitzer and Taylor, 2004; Gowrisankaran, Ho and Town, 2006). Finally, the hospital industry is the largest segment of the US healthcare sector ($1.3 trillion in annual spend) and, according to the Bureau of Labor Statistics (BLS), experienced the fastest price growth over 2000–19 across *all* sectors of the economy. Rapid corporatization may explain some of this price growth.

A lack of access to transaction prices between hospitals and insurers has stymied researchers, without which it is not possible to examine how systems change pricing of their member units. We make progress on this dimension by using patient-level claims data from the second largest private health insurer in the US, Elevance health, which allows us to observe negotiated transaction prices for hospital services across 20 states between 2012–18. Further, we combine data from five other complementary sources to study the effects on costs and quality. To examine changes in quality of care, we also use the universe of claims for Medicare fee-for-service beneficiaries, all-payor hospital discharge data from New York state, and Hospital Consumer

[1] 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 involved a change in profit status.

Assessment of Healthcare Providers and Systems (HCAHPS) survey data over the same period. Finally, we use annual surveys from the American Hospital Association (AHA) and hospital information reported in the Healthcare Cost Report Information System (HCRIS) to observe capital and labor inputs, operating expenses, and service portfolio breadth.[2]

We first show that, at a point in time, hospitals owned by larger systems obtain higher prices without delivering superior quality of care. Operating costs per bed decline as the parent firm becomes larger, with steep declines for the largest systems. These patterns suggest that hospitals in larger systems are more profitable but do not necessarily deliver higher quality. We then study 119 hospital deals that occurred from 2013 to 2017, in which over 120 independent hospitals transitioned to system control. These independent hospitals experienced large and varying shocks to firm size and operations. We use a staggered differences in differences research design and compare trends for independent hospitals that joined systems to those for hospitals that remained independent.

Systems may negotiate higher prices for their member units due to better negotiating skills or by using their market power (Lewis and Pflum, 2015). We find that mean prices for commercially insured patients do increase by 5%, holding procedure intensity constant. Across the top 7 specialities by volume, prices increase 5–8%, with digestive, infectious diseases, delivery, and cardiac care experiencing the largest price increases.[3] We find little evidence to support the claim that larger hospital systems are able to negotiate higher prices. All else equal, targets of acquirers above and below the median size experience similar price increases. Overall, corporatization does not confer unusual pricing benefits to the hospital since we find similar price increases in deals where a system owned hospital is bought by another system.

Operating expenses decline by 5.6% at the acquired hospital following system ownership, without any offsetting increase in costs at the acquirer. We find declines across all major operating cost components, but our results are primarily driven by reduced labor intensity. We detect a reduction in personnel spending (the labor share), which accounts for about 60% of the total decline in cost. Reduced personnel spending is in turn explained by a decrease in the number of employees concentrated in support functions. We also estimate a decline in compensation per employee, but it is not statistically significant. Beyond the decline in the labor share, we detect a disproportionate reduction in capital and financing costs, measured through changes in depreciation and interest. Despite the changes in prices and staff availability, patient volume remains unchanged, suggesting demand is inelastic. It also suggests that larger firms are able to reduce inputs while maintaining the same output levels.

Considering the effects on revenue and expenses together, we estimate an increase in hospital operating profits of about $64,700 per bed per year, or 6.4% of mean operating expenses. Price increases contribute only about $8,180 of this amount.[4] The lack of a demand response

[2] To our knowledge there is no single national, patient-level data source that records information for patients across all payors. Medicare data are national, but only cover fee-for-service Medicare beneficiaries (about a third of all hospital admissions). Hospital discharge and claims files cover all payors, but are limited to individual states.

[3] We find no evidence of changes in patient mix on multiple measures in all three patient files – Elevance Health, Medicare, and New York.

[4] This assumes the increase in price estimated for the commercial insurer in our sample is representative for all

implies that higher prices lead to a transfer from consumers — assuming full pass-through from payors in the form of higher premiums or lower wages — to the hospitals (Arnold and Whaley, 2020). The reduction in labor inputs may represent an improvement in hospital productivity if system ownership enables the acquired hospitals to maintain service quantity *and* quality with fewer inputs.

Hospital quality is multi-dimensional and difficult to measure. We examine the following standard measures common in 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 fail to reject null effects on mortality rates and patient satisfaction scores. Reassuringly, these patterns are consistent across the different patient samples, implying the results reflect hospital-wide changes in quality. Our estimates are precise enough to rule out modest changes in mortality and patient satisfaction. In contrast, the increase in readmission rates is economically meaningful. Our most conservative estimate is a 2-3% increase in readmissions for Medicare patients admitted with acute, non-deferrable conditions, regardless of whether we measure readmissions within 30 or 90 days of discharge. However, certain patient groups are more adversely affected. For example, the corresponding increase for Elevance cardiac care patients is 10-12%. Event studies show that the readmissions effect increases in magnitude each year following corporatization, suggesting it is not transitory. Acquired hospitals that experience greater staff reductions experience greater readmission rate increases, suggesting the reduction in labor inputs may be an important channel.

Computing the monetary cost to consumers of the increase in readmissions is necessarily a subjective exercise. To avoid making additional assumptions, we use the penalty imposed by the federal government for elevated Medicare readmissions as an estimate of their harm to consumers (MedPAC, 2018).[5] We consider a range of scenarios, progressively expanding the scope of the penalty. Our preferred estimates of the decrease in consumer welfare range from about \$10,700 to \$14,300 per bed per year. These estimates imply that the harm to consumers caused by elevated readmission rates is comparable to that caused by higher inpatient prices.[6] The quality effects of system ownership in healthcare therefore deserve greater scrutiny.

To probe potential mechanisms, we test for heterogeneity in the effects of corporatization based on the characteristics of the system and the target hospital. This exercise yields three insights. First, we do not detect significant heterogeneity in price effects across different types

private insurers. We scale the increase in revenue from private insurers to the hospital-level by assuming no changes in the share or mean reimbursement from other payors. This calculation implies a 1% increase in total hospital revenue, relative to the baseline operating expenses. This is likely an underestimate since we do not consider potential changes in outpatient prices or payor shares.

[5]We apply the same penalty per readmission as levied under the Hospital Readmissions Reduction Program (HRRP) – approximately 5x the reimbursement of the corresponding index case. The rationale behind the formula is unclear, but it provides a useful benchmark of what the government considers an acceptable measure of the cost to taxpayers and consumers of a hospital readmission.

[6]Our estimates are aggregated to the hospital-level across all patient types. Price increases affect only privately insured patients, while readmissions increase for all types. Hence, privately insured patients are worse off than other patient groups.

of transitions. For example, price increases are indistinguishable regardless of whether the target hospital is located in the same market as the acquirer or not. Second, creating a new system delivers little benefit to independent hospitals on the key outcomes we study and over our time horizon of about three years. Hospitals that form a new system receive about half the price benefits as when they are acquired by an existing system. Operating costs increase rather than decrease, and readmissions increase by greater amounts. Third, there are large scale economies on operating costs with respect to the acquirer's size. Acquisition by a system greater than the median size more than triples the reduction in operating cost per bed for the target hospital.

We take multiple steps to assess the validity of our estimates. First, we formally test and are unable to reject the null hypothesis of no differences in trends at acquired hospitals prior to corporatization compared to never treated hospitals, implying the former were not on track to experience the observed changes absent treatment. Second, we confirm the robustness of our results to a battery of design, specification, and sensitivity checks, including the use of a matched comparison group and an alternate estimator that corrects for the staggered nature of treatment (Callaway and Sant'Anna, 2020).

This article contributes to two distinct literatures. We present, to our knowledge, the first evidence on the causal effects of multi-unit firm ownership in the hospital industry. Previously, studies have empirically quantified the benefits of large, multi-unit firms in other industries or sectors such as hotels, retail, pharmaceuticals and fast-food restaurants (Kalnins and Mayer, 2004; Foster et al., 2006; Kosová and Lafontaine, 2012; Arora et al., 2023). This literature has often focused on the relative strengths and weaknesses of the franchising model or on outcomes such as physical productivity and R&D output, aspects typically not relevant to hospitals.

Within healthcare, our paper is closely related to Eliason et al. (2020), who study the effects of ownership by large chains in the dialysis industry and find similar patterns, such as large economies of scale in operating costs, a reduction in labor inputs, and an ensuing decline in quality. Although systems dominate hospital bed capacity and employment both nationally and across the vast majority of geographic markets in the US, few prior studies have empirically shown how they change operations and quality at individual establishments. We find that the main contribution of system ownership is to reduce operating costs for member hospitals by lowering the capital spending and labor intensity. In addition to estimating the average effects, we document scale benefits to being a member of a larger system. In contrast, there appear to be modest to no scale benefits on prices and quality. By examining multiple dimensions of performance, we are able to show that cost reduction is by far the most important benefit. We view our paper as complementary to Hausman and Lavetti (2021), who study the effects of concentration in the physician practice market. They find that an increase in establishment concentration (holding firm concentration fixed) leads to lower prices, which they attribute to firms passing through the benefits of lower costs to consumers. In contrast, our results indicate large economies of scale at the firm-level, likely due to the much greater fixed costs involved in hospital operations. Since individual hospitals tend to be stable in size, changes in establishment concentration are minimal in our setting.

Our results are consistent with the hypothesis developed in Autor et al. (2020) that industries gradually come to be led by large, "superstar" firms with low labor shares and high markups. This process appears to have played out in the hospital industry, which is increasingly dominated by systems. Our finding of a reduction in labor intensity implies a reduction in labor share of expenses at the member units. Since we find similar reductions in employment regardless of whether the new member hospital is in the same market or not, monopsony power does not appear to be the driver of the reduction in labor share.

Our second contribution is to the literature studying the effects of horizontal consolidation in hospitals and other industries (Gaynor, Ho and Town, 2015). Specifically in the case of hospitals, most prior studies did not have access to negotiated transaction prices and therefore had to rely on average prices inferred from accounting data reported to the federal government (Sloan et al. 2001; Dafny 2009; Lewis and Pflum 2015, 2017, Dafny, Ho and Lee 2019). However, recent work has shown that these imputed prices are only weakly correlated with true prices (Darden, McCarthy and Barrette, forthcoming). We overcome this limitation by using novel micro-data on transaction prices negotiated with the commercial insurer. We document limited heterogeneity in price increases across different service lines, suggesting the price negotiation process between hospitals and insurers may not greatly distinguish between specialties. Our work is distinct from Cooper et al. (2019), who also used negotiated hospital prices to study the price effects of mergers, but did not focus on the effects of system ownership specifically. A similar comment applies to the effects of consolidation on costs, where most prior studies have not focused on the effects for standalone hospitals (Schmitt, 2017; Craig, Grennan and Swanson, 2021).

Beyond the effects on prices and costs, we also quantify the effects on quality of care, an aspect that has been understudied in the consolidation literature more generally. The prior evidence on the effects of acquisitions on hospital quality is limited and mixed (Ho and Hamilton, 2000; Cuellar and Gertler, 2005; Beaulieu et al., 2020). Our results indicate that the harm to patients from the increase in readmission rates is comparable to the increase in prices. This finding echos the result in Fan (2013), who examined a newspaper merger and showed that the loss in consumer welfare due to quality reduction was substantial. Our results also appear similar to those of Braguinsky et al. (2015), who examined changes in profitability and productivity for cotton mills following acquisitions and found large increases in the former due to cost reductions, but not in the latter.

The paper is organized as follows. Section 2 provides a brief background on hospital market structure. Section 3 describes the data. Section 4 describes the empirical strategy and presents descriptive evidence. Sections 5 and 6 present the results on profitability and quality, respectively. Section 7 tests robustness. Section 8 discusses the policy implications of our results, and Section 9 concludes.

2 Background

2.1 Hospital market structure and spending

Hospital care is the largest segment in the $3.6 trillion US healthcare sector, with $1.1 trillion in annual spending. Despite a decline in inpatient volume over the last decade, hospital spending *increased* as a share of the sector from 30 to 31%.[7] BLS data 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)). Hospital consumer prices 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.[8]

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. 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 brought under the control of large corporate firms; we refer to this as the "corporatization" of hospital care. Figure 1 panel (b) captures this phenomenon through a different lens, highlighting its pervasiveness across geography. 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 (Figure A.1a).[9]

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.[10] Mean

[7]Source: National health expenditures data 2020 (Table 2) and authors' calculations using MedPAC databooks 2013 (chart 6-6), 2019 (chart 6-6), and 2021 (chart 6-4).

[8]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.

[9]This analysis arguably understates the level of corporatization in local markets, since HRRs are very large and were defined primarily for complex services such as cardiac care. On average, HRRs span 10 counties and contain about 20 hospitals. Patients likely do not consider hospitals over such a large area for acute care or routine procedures like deliveries.

[10]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 would decrease.

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 doesn't 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 as a source of variation in our empirical analyses.

2.2 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 the standard 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 mergers. 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, 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, predicting price effects in deals without any patient or employer overlap.

While there is substantial empirical evidence on the price effects of hospital mergers, very few studies have focused specifically on the effects of system ownership. To our knowledge, Lewis and Pflum (2017) is the only study to examine the effects of acquisitions of independent hospitals. They report large price effects of 10% or more following acquisitions over 2000–10. However, 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.

The evidence on the effects of system ownership on hospital quality is similarly limited, with greater focus on the effects of mergers. Ho and Hamilton (2000) find an increase in readmission rates following acquisitions, but others have found inconsistent effects (Cuellar and Gertler, 2005; Gaynor et al., 2021). We argue that 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.

Notably, the wider literature on the effects of multi-unit firms or chains in other sectors has seemingly not focused on product quality either. This literature 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 1a.

We also considered 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. They argue that these mechanisms will collectively improve both their finances and the quality of care for patients.

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.[11] 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

[11] 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.

described the merger 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, 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.[12] Financial filings suggest that system ownership quickly improved reimbursement rates at NWH, but the effects on quality and efficiency are not obvious. This type of ambiguity 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 six 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.[13]

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) associa-

[12] 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.

[13] 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.

tion.[14] 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 represent all 9 Census divisions and span the spectrum of regulatory preferences, as reflected in the cost of doing business.[15]

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 therefore 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-payor 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 payor of hospital admissions in the US, accounting for about 30% of all hospital admissions.[16] 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 payors 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.

We complement the claims based quality performance measures with subjective patient as-

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

[15] 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.

[16] 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.

sessments captured in the HCAHPS data, an annual survey conducted on a random sample of inpatients at Medicare certified hospitals. The data were obtained from the federal Hospital Compare portal. HCAHPS measures are used in federal performance incentive programs and by prior studies to examine changes in hospital quality (Beaulieu et al., 2020).

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 of these data is that, while we can observe price and quality measures by specialty (eg., cardiac care) in the claims data, 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 — prices, quality, and inputs — pertain to the same hospital sample.

We use data on system membership (a unique system ID) to infer changes in system ownership over time, including when a hospital transitions from being independent to system owned. We augment this 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 hospital deal through Internet searches of public (hospital websites, press releases, and news articles) and proprietary sources (American Hospital Directory). Appendix A.1 provides more details on the data sources and sample construction.

3.2 Descriptive evidence

We begin by describing the differences between independent and system owned hospitals in 2012, the first year we observe most hospitals. Table 1 describes key aspects of the markets (panel A), hospitals (panel B), prices (panel C), inputs (panel D), and quality of care (panel E) observed in our sample. Column 1 describes the full analysis sample of 1,653 hospitals. Of these, 981 hospitals were already system owned in 2012 and did not experience any change in ownership during the sample period (column 2). The remaining hospitals either remained independent throughout the period (column 3), were independent at the start but were acquired by a system (column 4), or were system owned at the start and acquired by another system (column 5). For brevity, we focus on four key outcomes of interest: mean price, operating expenses, and two measures of quality: 90-day readmission and mortality rates.

The raw data suggests that system owned hospitals (cols. 2 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. 3 and 4). 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.[17] Among independent hospitals, those that were subsequently acquired enjoyed lower operating costs than the "never acquired" hospitals and were less likely to be in rural markets. Both types of independent hospitals had similar average profit margins at the start of the sample.

The unadjusted comparisons may be difficult to interpret because of differences in market characteristics and patient mix across hospitals. To better account for such differences, we estimate the following predictive models for each outcome:

$$Y_h = \alpha_m + X_h'\delta + \xi_h, \tag{1}$$

where h and m denote hospital and market, 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 price or cost markets (eg., urban markets). Similarly, the vector X helps eliminate differences in mean procedure complexity and patient risk. We then use the residuals, ξ_h, to investigate the relationship between adjusted hospital performance and system size. To ease 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. If a hospital is independent, we assign it to a system of size 1. 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 has 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 on the scatter plot from a linear regression estimated on the underlying hospital-level data to formally illustrate the underlying correlation. The corresponding slope coefficient is also presented. We exclude the two largest systems from the sample for this exercise since they are outliers in firm size.[18]

Panel (d) presents the association between mean hospital prices 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 9% 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

[17] The profit margin measure was obtained from the HCRIS reporting system. It only accounts for revenue and costs pertaining to patient care services. For example, it excludes income from investments and concessions.

[18] 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.

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. Note that system owned establishments are similar in size to the independent establishments (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 24% s.d. lower than for a standalone hospital (34 x -0.71 = -24.1).

Panel (f) presents the association between cardiac care 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 8% 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. We present our research design to obtain causal effects in the next section.

4 Empirical strategy

To estimate causal effects of system ownership on hospital performance, we zoom in on the changes in patterns at independent hospitals that were acquired by systems and compare them to the patterns for similarly independent hospitals that did not experience a change in ownership. This approach sacrifices a large fraction of the data, since about 60% of hospitals were already system owned at the start of the sample and did not experience any further changes in ownership. These hospitals will not play any role in the analysis (Table 1 col. 1). However, this approach helps mitigate the role of persistent unobserved differences between hospitals, an important source of selection bias.

Our baseline models implement a staggered difference-in-differences (DD) research design, following the recent literature on hospital ownership (Lewis and Pflum, 2017; Dafny, Ho and Lee, 2019; Craig, Grennan and Swanson, 2021). Hospitals that remain independent throughout the sample period constitute the comparison group (Table 1 col. 3), and they offer an intuitive counterfactual for the independent hospitals that were acquired (Table 1 col. 4).[19] Their price trends are not contaminated by previous acquisitions (treatments), a complication with using already system owned (treated) hospitals as controls. Potential bias from using already-treated units as controls has also been recently highlighted in the treatment effects literature (De Chaisemartin and d'Haultfoeuille, 2020).

Equation 2 below presents our baseline model. Y_{ht} denotes the outcome of interest for hos-

[19] Independent hospitals that exit the sample are retained in the comparison group. Conceptually, exit is also a valid counterfactual to acquisition.

pital 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.[20] 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 of outcomes across patients or over time for the same hospital, which is the unit of treatment.

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

We note that, while our approach follows the prior literature, 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. Under the assumption that the acquired and comparison hospitals would have evolved on parallel trends in the absence of the transaction, β recovers the average treatment effect on the treated hospitals (ATT). We assess 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 a matched subset of hospitals),

[20] 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.

the specification (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 adjusts for staggered treatment and is consistent for the ATT.

4.1 Sample construction

We focus on hospital deals that closed between 2013 and 2017 in order to ensure that we observe the acquired hospital for at least one year before and after treatment.[21] We identify 119 corporatization deals in which an independent target hospital in the 20 commercial insurer states is acquired by a system (102 deals and targets), or joins (an)other independent hospital(s) to form a new system (17 deals and 21 targets). Our primary analysis sample includes these 123 target hospitals and a set of 377 comparison hospitals that remain independent through 2018 or until they exit the sample. In supplementary analyses, we also study the effects on 172 system owned hospitals acquired by other systems in 75 deals during this period.

Figure 2 panel (a) presents the number of mergers and acquisitions separately in each year over 2013 through 2017. No particular year dominates in terms of number of deals. Panel (b) illustrates the geographic coverage of the hospital deals by plotting the location of the treated (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.

Table 1 columns 3 and 4 describe our comparison and primary treated groups of hospitals, respectively. The acquired hospitals are more likely to be located in urban markets than those not acquired, and are slightly larger in terms of bed capacity. Treated hospitals had slightly lower mean price levels and operating costs per bed in 2012, but were not systematically better or worse on quality measures. The differences in baseline values, though generally small, highlight the need to examine within-hospital changes.

4.2 Transition to system ownership

Table 2 characterizes the transitions 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 two types of transitions. Columns 1 and 2 present the mean and median values, respectively, for the 119 deals in which independent hospitals are acquired by an existing system or join to form a new system. These are the transitions of interest since independent hospitals enter a large corporate firm or create one. As a contrast, columns 3 and 4 present the mean and median values, respectively, for the 78 deals in which previously system owned hospitals are acquired by another system.

[21] 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.

The average corporatization deal involves a large increase in firm scale for the new unit. The average target has approximately 270 beds and serves 12,000 admissions per year. In contrast, the average acquiring system has nearly 3,400 beds across 15 hospitals and serves about 160,000 patients annually — nearly a 13x 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 22 hospitals sells 2 of its member units to an acquirer with about 20% more hospitals and 33% more beds. Hence, these transitions involve a relatively smaller increase in firm size for targets. If a greater increase in firm size produces a greater increase in the target's bargaining power, these patterns imply a greater price effect following corporatization deals, all else equal. We test this hypothesis in our empirical analysis.

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.[22] 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 5% overall, and the effect ranges between 5 and 8% across the different service lines.[23] These estimates are similar, though slightly smaller in magnitude, to those reported by Dafny, Ho and Lee (2019) and Lewis and Pflum (2017). However, these studies relied on hospital-level mean reimbursements per stay that could not granularly adjust for changes in procedure intensity. Controlling for procedure intensity matters in the case of cardiac care patients, where the estimated price increase drops from 9% to 6% if the DRG weight is included as a covariate.

The increase in prices cannot be explained by an increase in the patient risk profile. Specifically, we do not detect changes in observed patient risk factors such as age, length of stay, and Elixhauser score. These results are presented for the pooled commercial insurer sample in Appendix Table A.2 panel A. Our 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 0.42 years (0.065 + 0.175x2) and in the Elixhauser score of more than 0.04 in this sample. In panel B we present the corresponding results for the subset of commercial patients admitted with cardiac

[22] Estimating the models at the patient level generated similar results, without gains in precision.

[23] 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.

conditions. Overall, we find similarly unchanging risk levels, but we do detect a 6% increase in mean DRG weight, also alluded to above. Taken together, these results imply that system owned hospitals perform more intense procedures for cardiac patients with similar observed histories and risk profiles. This pattern is consistent with upcoding behavior under system ownership, but is not replicated in the other specialties.

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.

5.1.2 Potential mechanisms

In this section, we examine heterogeneity in the price effect to assess the importance of different potential contributing mechanisms. The two main candidate explanations we test are whether larger hospital firms provide a greater boost to the target's price by increasing its bargaining weight, or alternatively, whether the price increase is largely driven by an increase in market power. As discussed in Section 2, large firms in other sectors have been shown to have higher margins and productivity than small firms, which would be consistent with an increase in the bargaining weight (Autor et al., 2020). The market power hypothesis is supported by economic theory (Nocke and Whinston, 2021). Both factors can operate concurrently, along with other nuisance factors (eg., differences in market size or competitiveness) that may act as confounders if not controlled for. To isolate the effect of different factors, we estimate a rich model including a number of triple difference interactions to simultaneously test for the importance of these two mechanisms. Table 4 presents the corresponding results.

Table 4 column 1, row 1 presents the main result from the baseline specification as a reference, and column 2 displays the results from the richer model where we include interactions of the main effect with relevant acquirer and market characteristics. Row 2 presents the coefficient on the interaction of treatment with a binary variable indicating whether the acquisition led to an above median change in system size, which was 4 hospitals in our sample. This coefficient tests whether targets acquired by larger firms enjoy a greater increase in price, all else equal. Row 3 presents the coefficient on the differential effect in deals where the targets form a new entity as opposed to being acquired by an existing firm. This allows us to isolate the effects of system formation from being acquired by a small system, which would otherwise be conflated. Row 4 presents the differential effect for deals involving a target located in the same market. Within-market deals involve a greater increase in market concentration by construction, and hence this coefficient tests for the incremental price increase in circumstances with a greater increase in market power. Row 5 presents the differential effect for deals where the target is located in an above-median size market (more than 9 hospitals). This interaction serves to control for differences in baseline market attributes (such as greater consumer willingness to pay in more urban markets) and competitiveness, which we view as confounders. The main coefficient in row 1 then estimates the average effect for units located in markets with fewer than 9 hospitals, acquired by firms with fewer than 4 hospitals and located in a different market than the target.

We experimented with even more flexible specifications, but including other interactions or estimating stratified models did not qualitatively change the results.[24] That being said, we note the potential for other uncontrolled confounders to bias the coefficients of interest in rows 2 and 4, so these must be interpreted with caution.

We anticipated that the triple difference coefficients in rows 2 and 4 would be positive; however, the point estimates are negative, and *none* of the interaction terms in column 2 are statistically significant. The results do not support the hypothesis that being part of a larger firm confers a greater bargaining weight to the target hospital. The negative coefficient on system formation does suggest an advantage to being acquired by an existing small system rather than creating a new system. Here it is important to note that our results reflect medium-term outcomes since we can follow the targets for about 3 years on average. Finally, the lack of a greater price effect for within-market deals does not by itself invalidate the role of market power but does imply constraints on the ability of systems to increase prices even in settings where they enjoy a greater increase in leverage. Our result is consistent with those of Lewis and Pflum (2017), who report a differentially *greater* increase in prices due to acquisitions in cross-market deals. In our case the effects are statistically indistinguishable.

As another test of the gradient between firm size and bargaining ability, we compare the price effect for independent hospitals to that for system owned hospitals when they join another system. Table 2 highlighted that independent hospitals experience a much greater increase in firm size when they are acquired, relative to system owned hospitals. Hence, we may expect a greater price increase for independent hospitals. Appendix Table A.3 column 1 presents the corresponding results for system owned hospitals and shows that prices increase by approximately 4% for the target hospital(s) following acquisition by another system. This coefficient is similar in magnitude to the main result reported above for independent hospitals. This evidence further supports the viewpoint that a transition involving a greater increase in firm size does not differentially increase bargaining ability.

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.[25] We describe the results on total expenses as well as on different, mutually exclusive buckets of

[24] For example, we estimated regressions stratifying the deals by the predicted change in HHI (less or more than 200 HHI points). The results do not indicate greater price increases in deals with a larger increase in predicted HHI.

[25] 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.

spending. Panel A column 1 presents the effect on total operating costs per bed, which implies a 5.6% reduction relative to the mean operating costs of about a million dollars per bed. Column 2 presents the effect on combined depreciation and interest rate costs.[26] 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 12% reduction in this bucket, implying a larger reduction in capital costs due to system ownership relative to other components. This result is consistent with Gaynor et al. (2021), who find a reduction in capital spending after a merger between two large hospital systems.

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 6.4% 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 2%, strikingly similar to the 1.9% reduction in purchasing costs reported by Craig, Grennan and Swanson (2021) using granular purchasing transaction data. Overall, the patterns suggest that systems drive cost reductions primarily by reducing labor intensity at the hospital. This is consistent with the main hypothesis in Autor et al. (2020) that large, successful firms reduce labor inputs.

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 larger long-run effects.

The reduction in spending on labor may reflect a reduction in employment, salaries and benefits, hours, or a combination of all three channels. To quantify the role of each channel, we present 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 a marginally significant 0.253 FTE per bed, or 4% 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 $21,000 in spending per bed

[26] A decrease in depreciation may reflect changes to the acquired hospital's accounting practices post-merger, 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 2.1 years (11%) on average following the transition in ownership, relative to the pre-deal mean of 20.5 years. A decrease in useful life would mechanically increase depreciation and therefore cannot explain the effect we observe.

(0.253 FTE per bed x 82,200 payroll spend per FTE = $20,800 spend per bed), which alone is about 40% of total cost reduction. The remainder, therefore, is due to a reduction in compensation per employee. In unreported results, we confirm that spending per employee declines by 2%, but this estimate is statistically insignificant. The coefficient is $1,679 (3,477). However, our data cannot distinguish between a reduction on the intensive margin in the number of hours worked versus wages.

The reductions in staffing appear to be concentrated among specific employee types. Panel B column 2 presents the result on employment among all 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.[27] The coefficient from other employees accounts for 80% of the total reduction in employment. 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 subcategories 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 present 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 8%. Although this group only accounts for a third of total employment, it accounts for two-thirds of the reduction, implying new owners disproportionately cut headcount in 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 0.005 FTE per bed (-0.013 + 2 x 0.009), negligible relative to the estimated reduction in employment. This helps rule out the possibility that systems converted former employees 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. Overall, these results point to a picture of significant employee reductions at the target hospital, focused in support functions.

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

[27]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 0.05 FTE per bed, which implies a 2% reduction among physicians and nurses. This is entirely driven by physicians.

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 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. Appendix Figure A.2 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.[28] 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

There are several possible channels through which ownership by a larger firm may reduce the need for staff at the acquired hospital. Our finding that staff reductions are concentrated among overhead functions is consistent with the strategy of consolidating back office functions such as marketing, IT, accounting, and HR into the corresponding teams at the parent firm.

Unlike in the case of prices, we find robust evidence of returns to scale in operating costs in our heterogeneity analysis, reported in Table 4 column 4. Row 2 shows that operating costs decline at the target hospital by $75,000 per bed more when it enters a system with more than 4 hospitals, relative to a system with 4 or fewer hospitals. Hence larger systems generate more than thrice the reduction in costs. This difference is economically meaningful and statistically significant.[29] In row 3, we find an increase in operating costs when independent hospitals join together to form a new system. The differential increase is marginally statistically significant (p-value of 0.07), but large enough to imply that operating costs *increase* at hospitals following the formation of a new system. This result is consistent with economies of scale, but also with other theories of managerial costs. For example, it suggests that the creation of a new organization may distract hospital administrators and disrupt routine operational protocols. Finally, the coefficient in row 4 implies a smaller reduction in costs when the target hospital is located in the same market as the acquirer. This coefficient is not statistically significant, so we do not emphasize it. Overall, these findings corroborate the pattern first observed in the cross-sectional analysis in Section 3.2: larger firms are able to confer scale benefits to the newly acquired hospital establishments.

We estimate a comparable reduction in operating costs at system owned hospitals when they transition to a different system (Appendix Table A.3 col. 2). However, we interpret this

[28] The point estimate for rural targets is -0.168 (0.062), while the corresponding coefficient for urban targets is -0.006 (0.03). Hence, there may be greater consolidation of services following acquisitions in rural markets versus their urban counterparts.

[29] 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.

result with caution since the dynamic effect coefficients indicate declining pre-trends, i.e., these hospitals were already experiencing a decline in costs prior to the transition of interest, likely the benefits of their previous system ownership. Hence, for this particular outcome, it is difficult to disentangle the effects of the second treatment from ongoing effects of the first.

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 (see Table A.4) or composition (discussed earlier), suggesting changes in revenue are driven entirely by price increases. Our baseline estimate implies mean prices grow 4.7% after acquisitions, or $724 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 $8,181 per bed ($724 x 11.3 admissions per bed). This may understate the real increase in prices if the commercial insurer 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 conservative for other reasons.[30] On the expenses side, we apply the result from Table 5 panel A column 1, which indicates that operating expenses decrease by $56,474 per bed per year. Taken together, these estimates imply an increase in operating profits of about $64,650 per bed per year, or about 6.4% 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 price increases. To put this in perspective, system ownership increases revenue for the average acquired hospital in our sample by about $2.0mm per year (244 beds x $8,180 per bed), but it decreases expenses by $13.8mm per year. Overall, system ownership increases hospital surplus by an average of $15.8mm per year.

6 System ownership and quality

We do not find changes in patient volume at target hospitals following changes in ownership. Patient demand for hospitals tends to be price inelastic (Gowrisankaran, Nevo and Town, 2015), but if patients respond to changes in quality, this result suggests that the change in ownership did not alter perceptions of the targets' quality. 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.

[30] We estimate the average treated hospital had 11.3 private insurer admissions per bed per year using total volume from the AHA and private insurer share from the New York discharge data. This estimate could be conservative since it does not account for potential growth in the share of privately insured patients or for increased prices for outpatient care services.

6.1 Average effects

We begin our analysis by examining readmission outcomes for the commercial patients following an inpatient stay for an acute 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 2 percentage points. This is an economically and statistically significant effect, reflecting an increase of 12% relative to the mean.[31] Figure 5 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.2 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 9.5%, but it is nearly double, at 16.5%, 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 payor 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.[32]

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 0.9 pp or about 6% of the mean.[33] While this estimate is smaller than what we obtained for commercial patients, it is well within the confidence intervals of that estimate. Figure 5 panel (b) presents the corresponding dynamic

[31] We do not detect an effect on readmissions following deliveries, which have a low baseline readmissions rate.

[32] 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).

[33] Patients were identified using the same set of diagnoses and procedure codes as in the commercial insurer sample.

effects, which follow a strikingly similar pattern to those in panel (a). 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 5 panel (c) presents the corresponding event study, which is inconclusive but suggestive of an increase in mortality as well. We do not emphasize this pattern since the DD coefficient is very 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.[34]

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 0.54 pp, which is about 2% of the mean value. Figure 5 panel (d) presents the corresponding event study, which confirms no pre-trends and the increase in readmissions following system ownership. While the magnitudes differ across samples, it is reassuring that we consistently detect an increase in readmission rates across three different patient samples and two sets of disease categories.[35]

Table 6 panel B column 2 presents the estimated effects on the probability of 90-day all-cause mortality for Medicare patients admitted to 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 5 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 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 is based on self-reported patient satisfaction responses from the HCAHPS survey, discussed in Section 3. The survey has been widely utilized by

[34]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.

[35]These results are not sensitive to the choice of examining readmissions at 90 days following discharge. Figure A.3 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.

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 et al., 2015; Beaulieu et al., 2020). We study changes in a composite measure of these scores for the same set of hospitals used to study prices and the other outcomes.

Table 6 column 3 in 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 economically or statistically significant changes in this measure.[36]

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

Table 4 columns 5 and 6 investigate heterogeneity in the readmissions effects by target and acquirer characteristics. We focus on cardiac care readmissions in the commercial insurer sample in this exercise for two reasons. First, this sample allows us to take advantage of all 119 deals; second, it allows us to relate effects on readmissions to those on prices for the commercial insurer. As the results show, however, we do not find statistically significant coefficients on any interaction terms. Thus, there is no robust evidence of differential benefit or harm for readmissions on any of these dimensions. These results contrast to the heterogeneity in effects on operating costs (Table 4 col. 4), suggesting that any economies of scale generated by system ownership do not yield improvements in quality of care. We do obtain a large, positive coefficient corresponding to system formation, which may imply that when hospitals form new systems, disrupted operations also hurt clinical quality.

Economic theory predicts that softening competition for patients may lead to reductions in quality of care. Hence, deals involving greater increases in local market concentration may be more harmful to patient outcomes. In addition, the acquiring system may be more likely to encourage physicians and other staff to rotate across its hospitals as the distance to the target decreases. While this is likely driven by good intentions (e.g., to provide access to new experts), it may unintentionally disturb existing protocols and communication within medical teams. Table 4 column 6 row 4 shows that the effect on readmissions for within-market deals is not in fact significantly different from that for cross-market deals, suggesting that proximity and softening of competition are unlikely to explain the changes in quality here.

In contrast to our results for formerly independent hospitals, we estimate insignificant ef-

[36] If we expand the sample to consider transitions across all states, the coefficients are more precise and marginally significant, but they remain economically small. The composite quality index decreases by 0.052 standard deviations, driven by declines in the share of patients who would recommend the hospital and who would rate it a 9 or 10.

fects on cardiac care readmissions following acquisitions of system owned hospitals, though the coefficient is similar in magnitude (Table A.3 col. 3). This result suggests that a change in ownership by itself is not likely to elevate readmissions, but the transition from being independent to system owned is important.

Overall, our heterogeneity analysis did not identify target or acquirer characteristics that might elevate readmissions following system ownership. Another possibility is that the reduction in staff inputs (eg., fewer nurse assistants, social workers, and case managers) reduces the hospital's capacity to avoid rehospitalizations. As this is more speculative, we discuss it in Section 8.

7 Robustness

We assess the sensitivity of our results on prices, operating expenses, and readmission rates against a battery of robustness tests. We vary controls and weighting, the comparison group of hospitals (including the use of a matched group), functional form, the level of clustering, and account for potential bias due to the staggered treatment design. Table A.5 presents the results of these checks. Columns 1, 2, and 3 present the coefficients for mean prices, operating expenses per bed, and cardiac care readmissions for commercially insured patients, respectively. For brevity, we present results only for these key outcomes, but the results are qualitatively similar for other measures.

Row 1 presents our preferred estimates, while rows 2–4 assess the sensitivity of the estimates to toggling covariates in the estimating equation. Row 2 presents estimates from a "bare" model with hospital and year fixed effects but no covariates. Row 3 offers a minor variation by including patient weights in the hospital-level models for prices and operating expenses. Row 4 includes DRG fixed effects instead of the continuously varying DRG weights used in the baseline model, which allows us to estimate changes in prices and readmissions within the same DRG. Across all three rows, the estimates for each outcome remain within two standard errors of the corresponding preferred estimates in row 1, and are typically statistically significant at the 5% level.

Rows 5–7 address potential concerns related to the composition of the comparison group. Our main approach uses all hospitals that remain independent throughout the sample period as the comparison group. Hospitals located near an acquired hospital may experience spillover price effects due to their exposure to the nearby hospital acquisition (Dafny, 2009). To mitigate the potential for bias due to such spillover effects, row 5 presents results from models excluding hospitals within five miles of a target hospital from the comparison group.[37] Using this comparison group does not meaningfully affect the estimates. Rows 6 and 7 present results obtained by implementing two different matching methods (propensity score and coarsened exact matching) to identify an appropriate subset of independent hospitals as the comparison group.[38]

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

[38] We use the "psmatch2" and "cem" Stata commands to implement propensity score (PSM) and coarsened exact matching (CEM), respectively. Each target hospital is matched to never acquired hospitals in the year before it

Reassuringly, the coefficients are similar across the two matching models and statistically indistinguishable from the baseline estimates.

Next, we consider sensitivity to the functional form used. Recognizing the potential for bias due to a right skew in price and operating costs, we apply the log transformation to these outcomes. Row 8 presents the corresponding point estimates, which are consistent with the estimates in levels. In the case of readmission rates, we present the marginal effect from a logit model, which is remarkably similar to the baseline.

Recent studies have shown that point estimates from conventional staggered DD models may be biased for the ATT. This is due to negative weights on 2x2 DD estimates that use already-treated units as controls (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 9).[39] We find a similar price estimate to our preferred one, but somewhat smaller and larger expenses and readmission effects, respectively.

Rows 10 and 11 test robustness of the statistical significance to clustering standard errors at higher levels. This addresses potential concerns that clustering at the hospital level may not adequately capture correlations in the error terms across patients or hospitals located in the same market or involved in the same deal (especially in the case where independent hospitals join to form a new system). Changing the clustering level does not affect the standard errors meaningfully.

8 Discussion

Hospital systems have rapidly expanded their share of total hospital capacity over the last two decades. Yet, to our knowledge, there have been few systematic studies of their effects on hospital operations. This paper begins to shed light on this phenomenon in an important sector of the US economy. We briefly summarize our main results here and note key implications.

We find that the corporatization of independent hospitals increases inpatient prices by about 5%. However, three pieces of evidence suggest that system ownership and being part of a larger firm do not confer an unusual price advantage. First, we detect a similar magnitude price increase when the target hospital is already system owned and experiences an increase in firm size, but not the same magnitude a standalone hospital does. Relatedly, when hospitals join together to create a new system, they obtain very little price benefit. Third, larger systems do not obtain higher prices for their targets than smaller systems do. At least in the medium term, these results argue against the claim that systems, and larger systems in particular, increase the bargaining weight of the target hospital in negotiations with insurers. It is therefore not clear

was merged. We match on the following variables: rural status; number of hospitals in the market (1, 2, 3, 4+); teaching hospital status; non-profit, for-profit, or public ownership; beds; hospital Medicare and Medicaid share; county poverty; and county unemployment. For PSM, we use 10 nearest neighbors and a caliper of 0.05.

[39] We use the Stata command "csdid" to obtain these estimates. We do not use hospitals in the "not-yet-treated" group as controls, ensuring the estimation relies only on never-treated hospitals.

that the phenomenon of corporatization has played an important role in the rapid increase in hospital prices over this period.

System ownership does appear to confer large operating cost benefits to target hospitals. These derive primarily from improvements in labor intensity — target establishments serve the same patient volume with about 4% fewer employees, mostly by cutting back on overhead functions. We do not detect a reduction in wages. Systems also reduce the cost of capital inputs for the targets. Importantly, the magnitude of the cost benefit does not depend on whether the target is located in the same market as the acquirer. Hence, the cost reduction does not appear to reflect monopsony power. We find clear evidence of economies of scale — hospitals that join a system with more than 4 hospitals experience a reduction in operating costs more than three times larger than those acquired by a system with fewer than 4 hospitals. The pattern of costs declining with scale is very robust and evident both in the cross-section and panel data analysis. This pattern 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.

Finally, system ownership does not improve hospital quality, and may reduce it. Two results support this: first, we do not find an increase in patient volume at the acquired hospital following system ownership. Under a revealed preference style argument, this implies patients do not perceive the hospital to be of greater quality. Second, we find a robust increase in readmission rates across all three patient samples. We tested whether greater cuts in labor inputs are associated with a greater increase in readmissions. Figure A.3 panel (c) presents binned scatter plots of the estimated change in 90-day cardiac care readmissions for commercial patients on the Y-axis, and on hospital FTE employees on the X-axis. We also overlay a linear fit based on a predictive regression using the underlying estimates from each transition.[40] The figure shows that experiencing a greater reduction in staff availability 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 0.25 FTE per bed is associated with an increase in readmission probability for cardiac care patients of approximately 1.1 percentage points. Hence, the estimated decline in labor intensity explains about 55% of the increase in readmission rate we estimate for cardiac care patients. We emphasize that this is a correlation and does not imply causality. However, it is consistent with reduced staff availability being an important channel for the increase in readmissions.

Increased readmissions could partly reflect a quality gain if the hospital keeps more patients alive, who are then more likely to be readmitted since they are more frail. There is some evidence in favor of this competing risks hypothesis (Laudicella, Donni and Smith, 2013). On the other hand, in their study evaluating hospital quality measures, Doyle, Graves and Gruber (2018) find no evidence for a causal link between a hospital's performance on readmissions and on mortality. They suggest potential correlations could arise due to differential focus on technologies that mitigate one type of outcome but not the other. In our results, we do not detect a significant

[40] We use our preferred specification to estimate the DD coefficients on commercial cardiac care readmissions and FTE per bed corresponding to each transition. We then assess the association between these effects across the 123 hospitals.

change in short-term mortality rates in the New York discharges or the Medicare data. Setting statistical significance aside, the estimated effect on mortality for Medicare patients is also not large enough to explain the corresponding estimated increase in readmission rates, even if every marginally alive patient were to be readmitted.[41] Hence, our evidence implies an increase in hospital readmission rates, even if we consider potential changes in mortality.

A comprehensive welfare analysis of corporatization is out of the scope of this paper, but we combine our estimates on prices, costs, and quality to consider the monetary impact for producers (hospitals) and consumers.[42] We quantify the magnitudes of these channels in dollars per bed per year for a standalone hospital that enters system ownership. Our estimates imply that higher private insurance prices increase hospital revenue by about $8,180 per bed. Since patient volume is unaffected, we interpret this as a transfer from consumers to hospitals, assuming insurers and employers pass these price increases through to consumers in the form of higher premiums and lower wages, respectively.

To quantify the loss in value to consumers due to elevated readmission rates (and not simply the additional payment for the readmission), we assume that the penalty set by the federal government for an incremental readmission under the Hospital Readmission Reduction Program (HRRP) approximates the welfare cost to a patient of experiencing a rehospitalization.[43] If we apply the penalty only to additional 30-day readmissions for traditional Medicare patients, as CMS does, then we estimate a cost to consumers of about $2,500 per bed. However, we prefer to consider the cost to all patients, and therefore apply this penalty across all insurers. This results in a predicted cost of $10,700 per bed. If we instead penalize readmissions up to 90 days following discharge, as Medicare's flagship bundled payment programs do, the penalty would be $14,300 per bed.

Taken together, our estimates imply that the corporatization of an independent hospital increases costs for consumers by about $19,000–22,000 per bed per year. Interestingly, these calculations suggest that the adverse cost effect on consumers due to quality is comparable or may exceed the adverse cost effect due to higher inpatient prices. This is because the price increases affect only privately insured patients, while readmissions rise across all patient groups.

[41] While 100% readmissions is possible in theory, in practice we do not observe such high readmission rates for any patient group. For example, very frail patients with Elixhauser scores of 6 or more have a 90-day readmission rate of about 32%. We estimate a 0.27 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 0.27 pp, which is only half of the effect we estimate. If we instead assume 35% of marginal patients would be readmitted, the mortality decline could explain an increase in readmissions of 0.1 pp.

[42] A possible concern is whether corporatization helps standalone hospitals survive, therefore improving access to care. We formally investigated this possibility by estimating the effects on survival against a matched set of comparison hospitals, but could not reject the null hypothesis of no effect.

[43] HRRP's penalty is a function of Medicare Part A's "base payment" amount multiplied by the inverse of the national readmission rate. The current national readmission rate of about 20% implies a multiplier of 5. Table A.1 in Gupta (2021) reports that base payments are 72% of mean Medicare reimbursements. Hence, we set the base payment amounts for commercial, Medicare, and Medicaid inpatient stays at 72% of the observed reimbursements. We set Medicaid at 72% of Medicare base payment rates based on Kaiser Family Foundation estimates of the ratio of Medicaid to Medicare rates. Based on the New York payor mix, this leads to an average payor mix weighted base payment price of $15,931, and a penalty per readmission of $79,653. We then estimate the total number of 30-day readmissions in a given year attributed to hospital acquisitions in New York, the total cost of readmissions based on the base payment penalty, and the standardized total cost per hospital bed per year.

Policy responses should accordingly weight both types of harms.

The average acquired hospital experiences an increase in surplus of about $64,650 per bed, most of it due to a reduction in its operating cost (as described in Section 5). Private insurers may capture some of the cost gains during price negotiations. However, traditional Medicare and Medicaid (and therefore taxpayers) do not currently share in these gains, since their reimbursement rates are largely determined by average market-level costs.

9 Conclusion

We find that the corporatization of hospital care improves profitability through both higher prices and lower operating costs, increasing operating profits by about 6.4% of baseline expenses. The cost reduction is large, increases with the size of the acquiring firm, and is substantially driven by decreases in capital costs and labor inputs. In contrast, we find no evidence of quality improvements. For example, patient demand is unchanged, and no quality measure improves. Instead, we find suggestive evidence that certain services are terminated and short-term readmission rates increase following the change in ownership. These results are robust to a large number of sensitivity checks. Consumers are unambiguously worse off since they face higher prices and readmission rates on average. However, greater system ownership may lead to meaningful reductions in aggregate operating costs for the hospital industry.

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 national data (Cooper et al. 2019).

There are a number of directions for future research. We document an increase in price due to system ownership, but do not examine specific price setting strategies. We detect an increase in prices following cross-market deals, but it is unclear whether this reflects an increase in the target's bargaining power due to system ownership, or due to employer overlap across markets. Using more detailed data, perhaps on a case study, researchers can make more progress on understanding whether the reduction in labor inputs represents better managerial processes or economies of scale. While we have made a foray, a greater body of evidence needs to be compiled on the mechanisms behind the quality effects of system ownership for hospitals and other healthcare providers. More importantly, our evidence on the channels that may exacerbate readmissions — such as staff reductions — is only suggestive. Finally, a comprehensive welfare analysis should study whether the cost reductions partially pass through to consumers in the form of lower prices as well as quantify the general equilibrium effects on the local labor market. All of these parameters are necessary inputs for policymakers to understand the value of hospital systems.

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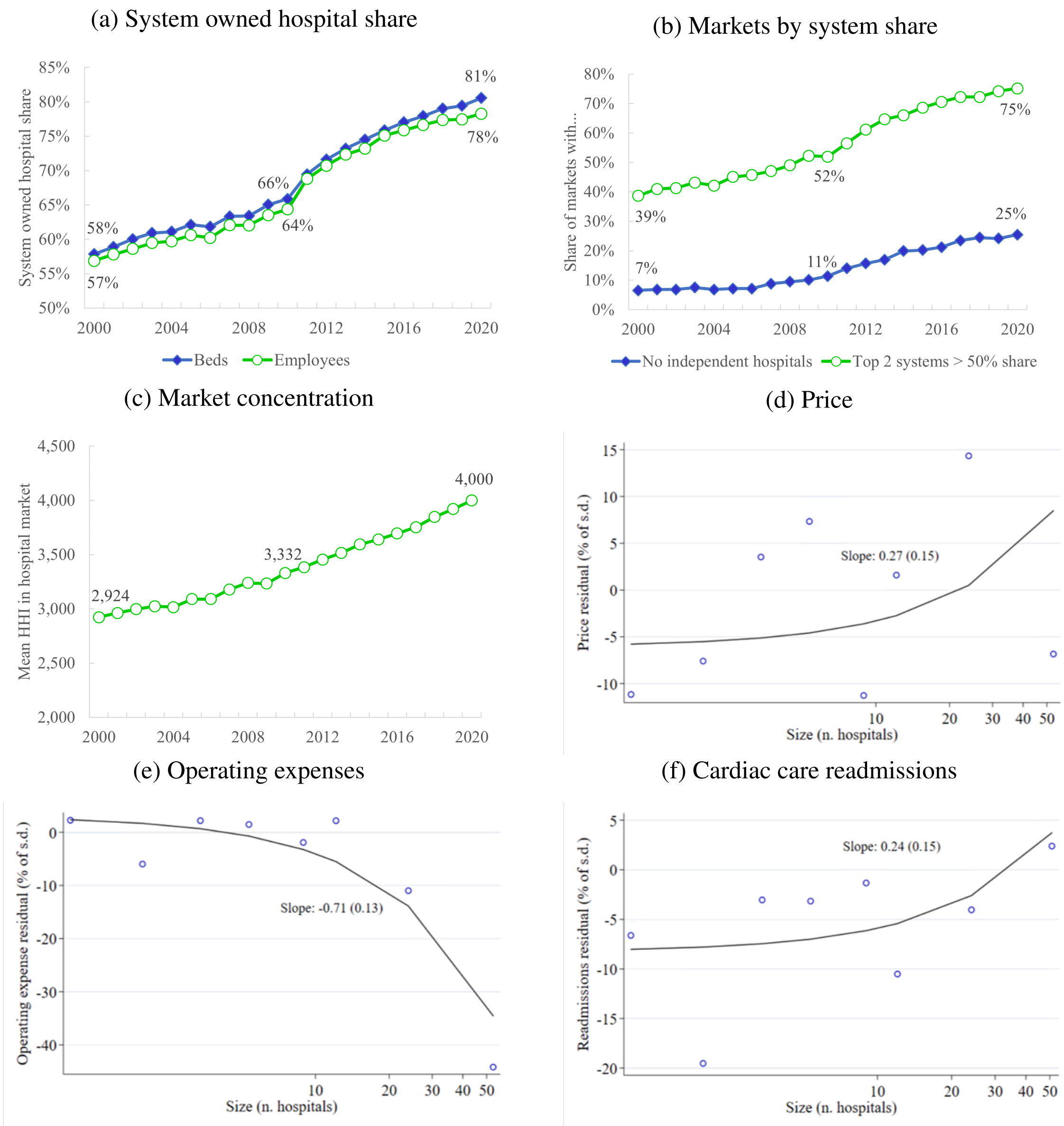


Figure 1: Hospital markets, firm size, and performance

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. Panels (d)–(f) present cross-sectional relationships between system size (in number of hospitals) and the key outcomes of interest: prices, 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 year fixed effects, where the bins are defined by deciles of system size. The models were estimated on hospital-level data derived from the commercial claims and the American Hospital Association (AHA) survey in the 20 states discussed in Section 3. The binned scatter plots display mean values in each bin as observed in 2012 and 2013. They also plot lines of best fit, which appear curved since the X-axes are log scaled. We use hospital referral regions (HRRs) to define hospital markets and bed counts to compute market shares.

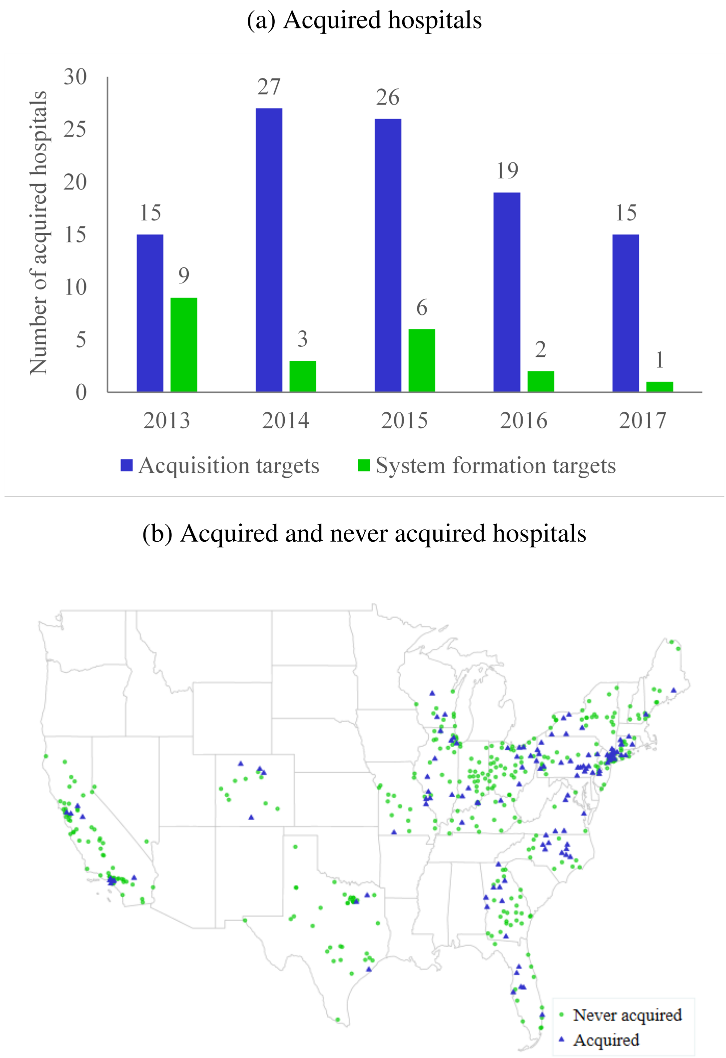
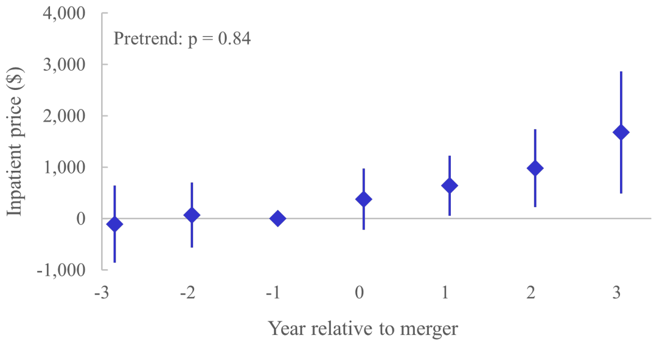


Figure 2: Acquisition of independent hospitals (corporatization)

Note: Panel (a) presents the count of independent hospitals acquired by existing systems ("acquisition targets") and those that merged to form new systems ("system formation targets") over 2013 to 2017. The map in panel (b) displays the locations of all acquired and never acquired, independent hospitals in the commercial sample. Note that this is not a national sample, and only contains hospitals in the 20 states discussed in Section 3.

(a) Price



(b) Diagnostic categories

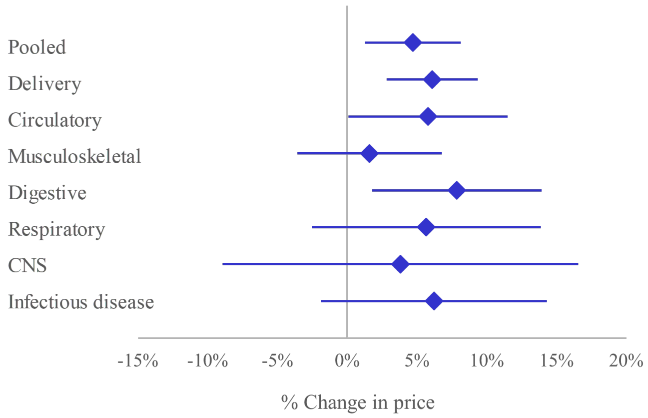


Figure 3: Effects on hospital prices

Notes: The figure presents the main results for the estimated effects of system ownership on hospital prices. 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 commercial cardiac care and deliveries samples and five major diagnostic categories (MDCs). 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 pre-treatment coefficients.

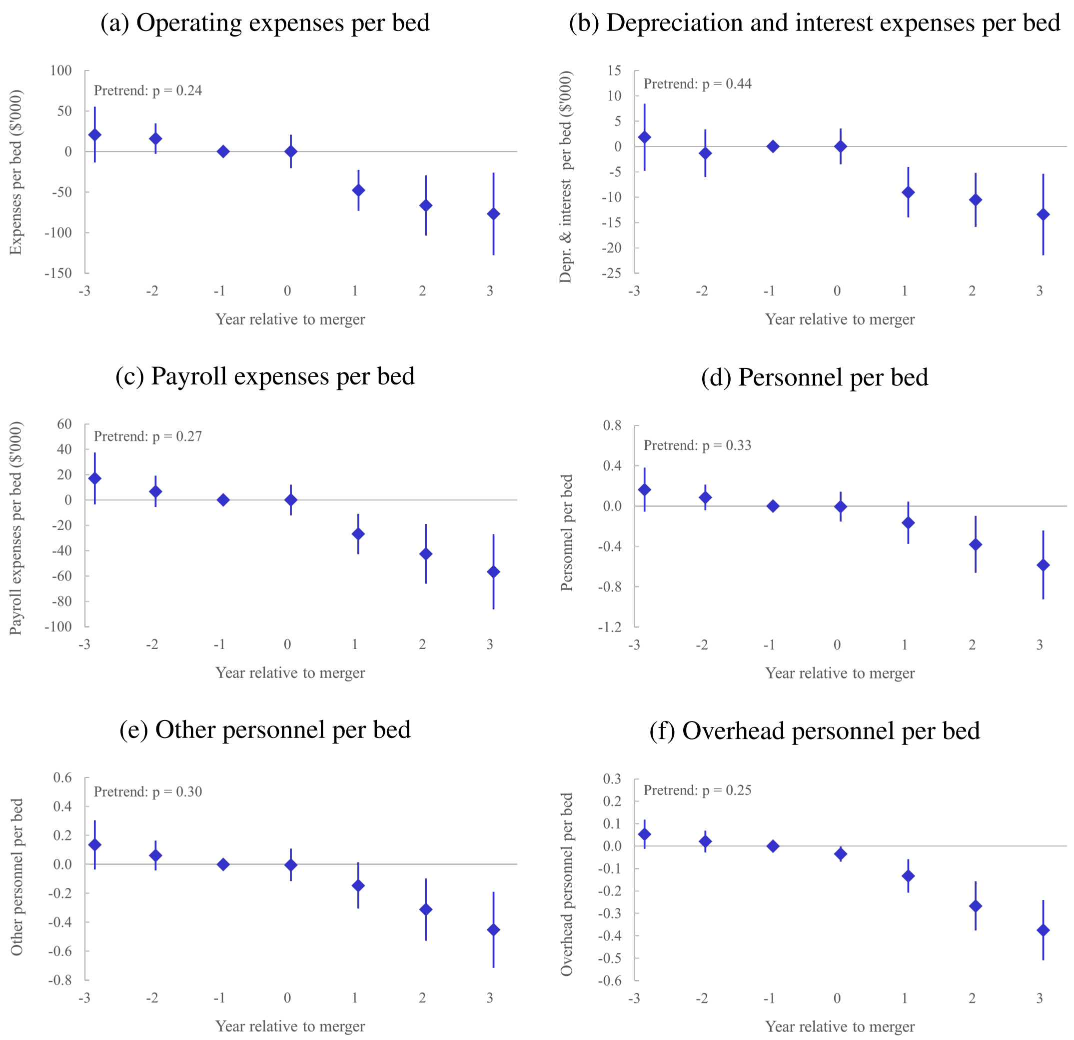


Figure 4: Effects on hospital inputs

Notes: The figure presents dynamic effects obtained by estimating Equation 3 on various measures of hospital inputs: total operating expenses per bed (a), depreciation as a measure of capital stock and interest expense per bed (b), payroll and benefits spending per bed (c), total full time equivalent (FTE) personnel per bed (d), "Other" FTE, defined as total FTEs less physicians, dentists, and nurses, per bed, and overhead FTEs per bed (e), overhead categories are discussed 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.

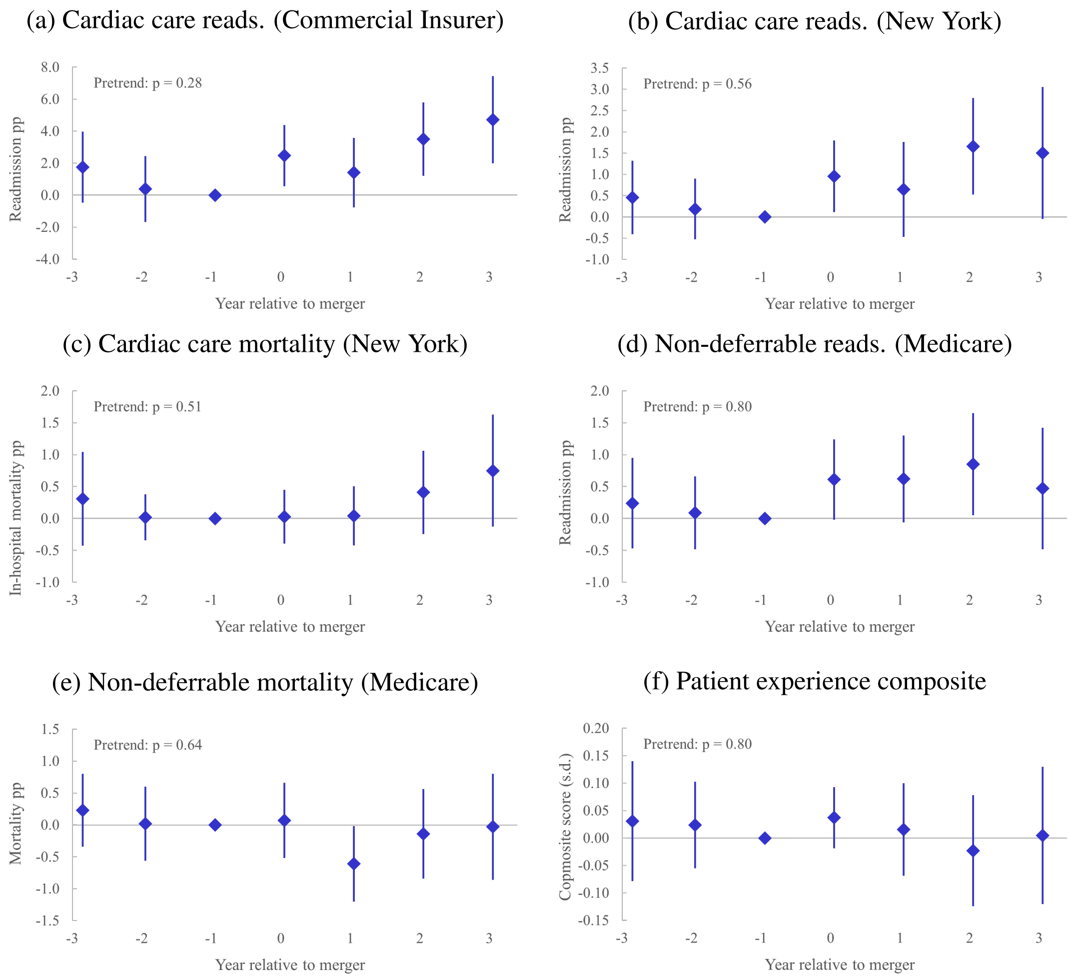


Figure 5: Effects on hospital quality

Notes: The figure presents dynamic effects obtained by estimating equation 3 on various measures of hospital quality following system ownership. 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-payor 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. 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 and the 20 states available with commercial claims data. 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.

Table 1: Descriptive statistics

	(1) All hospitals	(2) Already system owned	(3) Never acquired	(4) Acquired inde-pendent	(5) Acquired system owned
Panel A: Market characteristics					
Rural	0.20	0.14	0.34	0.24	0.18
Poverty rate	0.16	0.16	0.17	0.16	0.16
Panel B: Hospital characteristics					
Beds	253.5	264.5	223.4	244.1	270.7
Teaching	0.09	0.10	0.09	0.06	0.09
Percent Medicare or Medicaid	0.65	0.64	0.66	0.67	0.67
For profit	0.21	0.26	0.09	0.07	0.32
Patient profit margin	0.01	0.03	-0.03	-0.03	0.02
Panel C: Prices					
Price ($)	15,974	16,676	14,869	14,194	15,286
	(6,089)	(5,990)	(6,446)	(4,962)	(5,613)
Panel D: Inputs per bed					
Operating expenses ($'000)	1,052	1,057	1,083	990	961
	(550)	(533)	(622)	(452)	(492)
Personnel spend ($'000)	512	501	548	507	485
	(270)	(256)	(290)	(244)	(319)
Personnel (FTE)	5.92	5.68	6.55	6.27	5.48
	(2.49)	(2.44)	(2.60)	(2.38)	(2.22)
Panel E: Quality of care					
Cardiac care readmissions:					
Commercial insurer (90-day)	0.179	0.179	0.183	0.172	0.180
	(0.082)	(0.079)	(0.091)	(0.081)	(0.087)
Non-deferrable readmissions:					
Medicare (90-day)	0.245	0.244	0.244	0.247	0.246
	(0.036)	(0.035)	(0.041)	(0.034)	(0.034)
Non-deferrable mortality:					
Medicare (90-day)	0.176	0.175	0.177	0.174	0.177
	(0.031)	(0.031)	(0.033)	(0.032)	(0.026)
Number of hospitals	1,653	981	377	123	172

Notes: The table presents descriptive statistics for the hospitals in our analysis sample and specific subsets. It describes all hospitals in the 20 commercial claims states (col. 1), hospitals already owned by systems in 2012 with no change in ownership during the sample period of 2012–18 (col. 2), independent hospitals not acquired during this period (col. 3), independent hospitals acquired by systems (col. 4), and system owned hospitals acquired by other systems (col. 5). Our main empirical analyses focus on the hospitals in cols. (3)–(4). 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. Panel D 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 E 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. Each value represents data from the first year the hospital is observed. Corresponding standard deviations are reported in parentheses for outcome variables. The datasets are described in Section 3, with additional details in Appendix Section A.1. All dollar figures in Panels C and D are deflated to 2017 values.

Table 2: Hospital deals

	Corporatization		Non-corporatization	
	(1)	(2)	(3)	(4)
Variable	Mean	Median	Mean	Median
Panel A: Acquirer characteristics				
System hospitals	15.2	5.0	27.3	11.0
System beds	3,435	1,746	6,317	3,209
System admissions ('1000)	158.2	83.7	295.1	148.0
Panel B: Target characteristics				
Hospitals	1.0	1.0	2.3	1.0
Prior system hospitals	1.0	1.0	21.9	5.5
Beds	265	206	603	317
Prior system beds	265	206	4,394	1,387
Admissions ('1000)	11.8	8.2	28.0	13.8
% Non profit	0.87	1.00	0.71	1.00
% For profit	0.07	0.00	0.28	0.00
Panel C: Target market characteristics				
P(acquirer is in target HRR)	0.50	1.00	0.54	1.00
Mean distance to closest acquirer (mi.)	64.0	25.6	168.6	33.4
Mean HHI	2,249	1,951	2,467	2,039
Post-merger ΔHHI	277	159	124	66
Post-merger Δpredicted HHI	138	0	50	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 119 deals in the 20 states in our sample in which independent hospitals are acquired by existing systems or come together to form new systems ('corporatization' deals). Cols. (3) and (4) describe the 78 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 its prior system owner (number of hospitals and beds) in the year before the deal. The non-profit category excludes publicly owned hospitals, and thus non-profit and for-profit may not sum to 1. Panel C describes specific aspects of the 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. 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 that bed shares reflect the change in hospital ownership.

Table 3: Effects on prices

Dependent variable price:	Pooled	Delivery	Cardiac care	Musculo-skeletal
	(1)	(2)	(3)	(4)
Acquired * post	724.2	785.1	2,294	443.2
	(269.7)	(214.8)	(1,157)	(732.1)
Observations	2,918	2,417	2,317	1,466
Dep. var. mean ($)	15,407	12,894	39,659	27,772
	Digestive	Respir-atory	CNS	Infectious disease
	(5)	(6)	(7)	(8)
Acquired * post	1,406	1,065	722.2	2,033
	(554.8)	(788.0)	(1,231)	(1,349)
Observations	702	583	378	348
Dep. var. mean ($)	17,915	18,820	18,937	32,695

Notes: The table provides details on the price effects of system ownership 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 diagnostic categories by volume in the commercial claims. Col. (1) is weighted to match the average hospital wide DRG distribution in the New York discharge data during our sample period. Cols. (2) and (3) use the commercial insurer delivery and cardiac care samples, respectively. Cols. (4)–(8) are major diagnostic categories defined using DRGs. 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.

Table 4: Heterogeneity in effects

Dependent variable:	Price		Operating expenses		Cardiac care reads.	
	(1)	(2)	(3)	(4)	(5)	(6)
Acquired * post	724.2	1,019	-56,474	-30,476	0.0198	0.0166
	(269.7)	(510.5)	(15,709)	(21,849)	(0.0075)	(0.0123)
Above med. Δsystem size		-550.6		-75,549		-0.0097
		(565.4)		(22,848)		(0.0121)
System formation		-541.6		87,564		0.0133
		(866.6)		(48,841)		(0.0158)
In-HRR		-31.8		41,471		-0.0025
		(566.9)		(28,154)		(0.0134)
Above med. market size		154.4		-51,903		0.0079
		(559.7)		(27,747)		(0.0144)
Dependent var. mean	15,407	15,407	1,016,550	1,016,550	0.164	0.164
Observations	2,918	2,918	2,918	2,918	44,705	44,705

Notes: The table presents coefficients obtained by estimating Equation 2 with additional interactions for different types of transitions to system ownership. Cols. (1), (3) and (5) repeat the estimates from the baseline models. Mean inpatient price, operating expenses per bed, and 90-day cardiac care readmissions are the respective dependent variables. Price and readmissions are calculated from the commercial claims data, while operating expenses are sourced from the AHA survey. Cols. (2), (4) and (6) add several interactions of deal type with an indicator for post-acquisition. The first additional control is an indicator for a deal involving an above-median change in system size, in terms of the number of hospitals in the target's system pre- vs. post-merger. System formation is an indicator for when the deal forms a new system, as opposed to enlarging an existing system. In-HRR indicates whether the target hospital and at least one acquiring hospital are located in the same HRR. Finally, above median market size indicates whether the target is located in an HRR with more hospitals than the median target's market. 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 5: Effects on inputs and service portfolio

Panel A: Expenses				
Dependent variable:	Operating expenses	Deprec. and interest	Payroll expenses	Misc. expenses
	(1)	(2)	(3)	(4)
Acquired * post	-56,474	-9,130	-33,365	-9,428
	(15,709)	(2,714)	(9,110)	(10,673)
Observations	2,918	2,293	2,918	2,293
Dependent var. mean ($)	1,016,550	79,656	518,228	427,620

Panel B: Personnel categories				
Dependent variable:	Total personnel	Other personnel	Overhead	Contract
Acquired * post	-0.253	-0.205	-0.166	-0.013
	(0.109)	(0.080)	(0.035)	(0.009)
Observations	2,918	2,918	2,494	2,318
Dependent var. mean	6.3	4.4	2.1	0.2

Panel C: Service portfolio				
Dependent variable:	Number of services	Number of tech services	Cardiac extensive margin	Deliveries extensive margin
Acquired * post	-0.917	0.106	-0.013	-0.050
	(1.872)	(0.403)	(0.028)	(0.028)
Observations	2,918	2,918	2,866	2,863
Dependent var. mean	61.4	14.0	0.8	0.8

Notes: The table 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 2017 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. 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. Appendix Figure A.3 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.

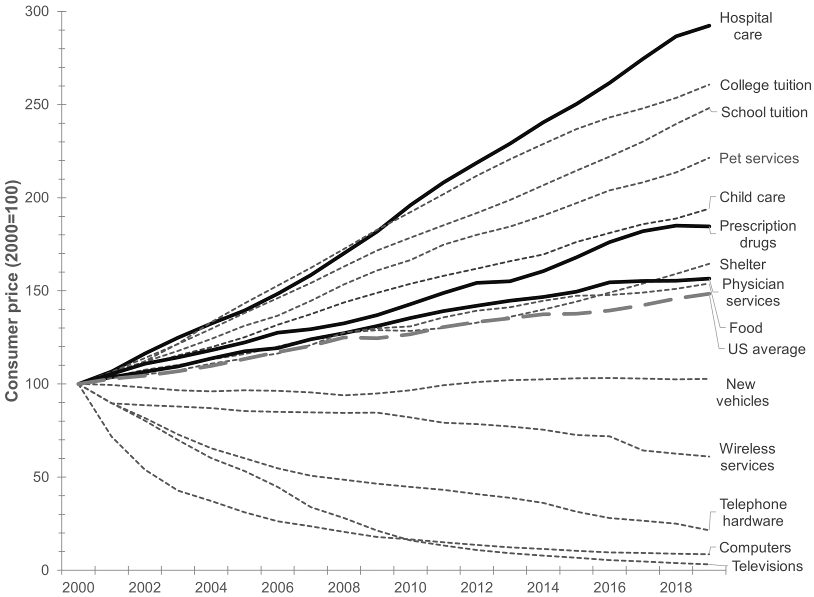
Table 6: Effects on quality

	(1)	(2)	(3)
	Panel A: Cardiac care		
	Commercial Claims	New York	
	Readmissions	Readmissions	Mortality (in-hospital)
Acquired * post	0.0198	0.0088	0.0009
	(0.0075)	(0.0031)	(0.0035)
Observations	44,705	166,834	166,834
DV mean	0.164	0.146	0.010
	Panel B: Non-deferrable conditions		Panel C: Patient experience
	Medicare		
	Readmissions	Mortality	Composite score
Acquired * post	0.0054	-0.0027	0.0012
	(0.0023)	(0.0022)	(0.0391)
Observations	867,897	867,897	2,894
DV mean	0.237	0.182	-0.143

Notes: The table 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 and 20 commercial states. 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 figures

(a) Consumer price growth



(b) Trend in hospital labor share

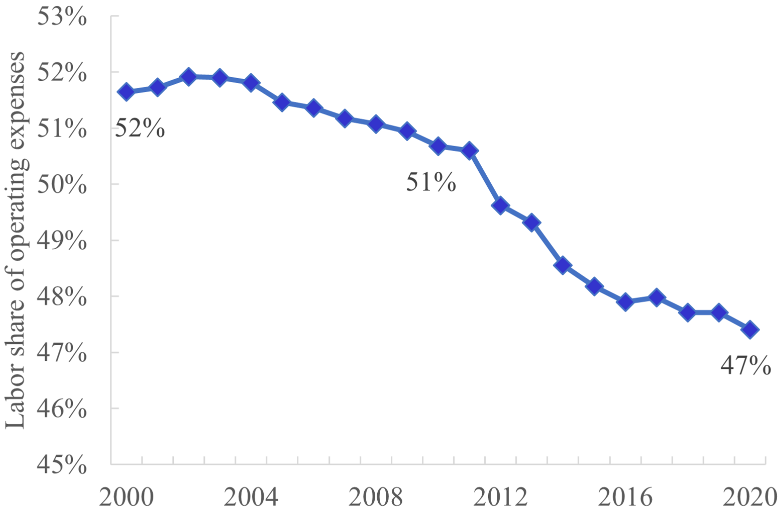


Figure A.1: Trends in consumer prices and hospital labor share

<u>Note:</u> 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.

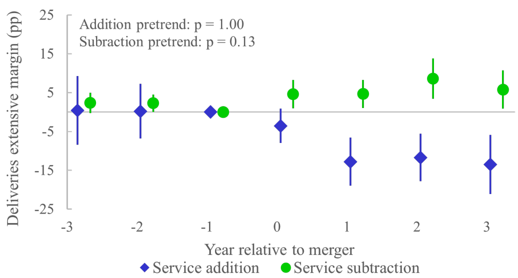
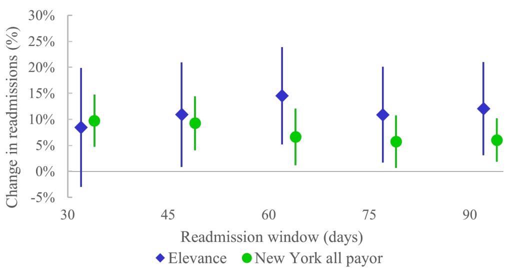


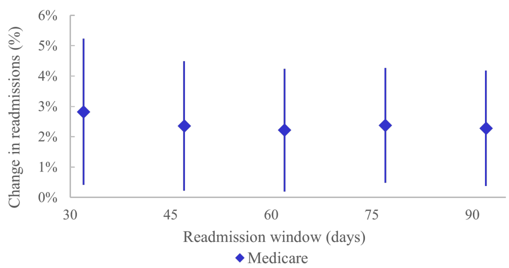
Figure A.2: Additional evidence on inputs and service portfolio

Notes: The figure presents dynamic effects obtained by estimating Equation 3 on a dummy variable equal to one when a hospital starts (blue diamonds) or stops (green circles) offering delivery services, as observed in the commercially insured patient claims. 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 figure presents 95% confidence intervals with standard errors clustered by hospital and p-values from F-tests of the joint significance of pre-treatment coefficients.

(a) Cardiac care



(b) Non-deferrable conditions



(c) Labor vs. readmission effects

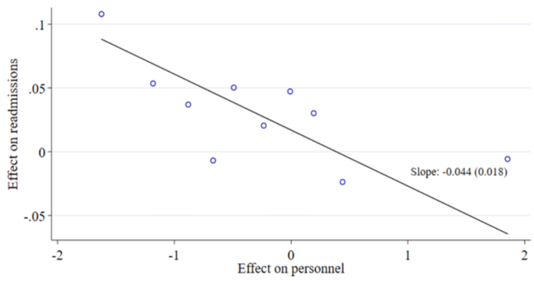


Figure A.3: Additional evidence on readmission rates

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) The figure also mentions the slope coefficient from a linear regression using the underlying hospital-level estimates.

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 diagnoses with the largest patient volumes in the commercial sample. (M)CC refers to (major) complication or comorbidity. The number of inpatient stays corresponds to admissions in the 20 commercial states over 2012–18. Appendix Section A.1 describes the restrictions used to arrive at this final sample in detail.

Table A.2: Changes in patient mix

	(1)	(2)	(3)	(4)
Dependent variable:	DRG weight	Age	Length of stay	Elixhauser score
Panel A: Pooled sample (Commercial claims)				
Acquired * post	0.004	0.065	-0.009	0.005
	(0.011)	(0.175)	(0.024)	(0.018)
Observations	630,290	630,290	630,290	630,290
Dependent var. mean	0.90	39.22	3.77	0.76
Panel B: Cardiac care (Commercial Claims)				
Acquired * post	0.175	-0.280	-0.069	-0.008
	(0.074)	(0.419)	(0.105)	(0.092)
Observations	44,705	44,705	44,705	44,705
Dependent var. mean	2.76	63.50	4.66	3.46
Panel C: Cardiac care (New York)				
Acquired * post	0.189	-0.901	-0.170	-0.069
	(0.151)	(0.472)	(0.298)	(0.081)
Observations	166,834	166,834	166,834	166,834
Dependent var. mean	2.89	68.16	4.53	1.11
Panel D: Non-deferrable (Medicare)				
Acquired * post	-0.006	-0.014	-0.028	0.007
	(0.009)	(0.043)	(0.045)	(0.017)
Observations	867,897	867,897	867,897	867,897
Dependent var. mean	1.60	81.55	6.00	3.05

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 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.3: Non-corporatization deals: average effects

Dependent variable:	Price	Operating expenses	Cardiac care reads.
	(1)	(2)	(3)
Acquired * post	689.1	-71,399	0.0095
	(274.7)	(12,317)	(0.0072)
Observations	3,151	3,151	46,498
Dependent var. mean	16,448	961,012	0.175

Notes: The table provides details on the price, cost, and readmission effects of non-corporatization deals (i.e., when a system acquires a hospital already owned by another system) estimated using Equation 2 on the commercial claims and AHA data. Col. (1) presents coefficients obtained using mean inpatient price as the dependent variable on hospital-year level commercial claims data. Col. (2) presents results for operating expenses per bed, as reported in the AHA survey. Col. (3) presents coefficients obtained with cardiac care 90-day readmissions as the dependent variable on commercial patient-stay level data. 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.

Table A.4: Changes in patient volume

	Comm. claims pooled		Comm. claims cardiac care		NY cardiac care	
Dependent variable:	Volume	Vol. per bed	Volume	Vol. per bed	Volume	Vol. per bed
	(1)	(2)	(3)	(4)	(5)	(6)
Acquired * post	-4.1	-0.093	-0.1	0.000	6.7	0.027
	(7.0)	(0.064)	(0.9)	(0.005)	(16.2)	(0.042)
Observations	2,918	2,918	2,325	2,325	324	324
Dependent var. mean	252	1.113	22	0.077	107	0.256

Notes: The table presents results for patient volumes, obtained by estimating Equation 2 on hospital-year level data. The first four columns report results for the number of patients and patients per bed using the commercial data. Cols. (5)–(6) report results for the same measures using the NY discharge data. Results in cols. (1)–(2) use pooled commercial claims volumes, while (3)–(4) restrict to the cardiac care subsample. Since our sample of pooled commercial data is incomplete, our price regressions weight inpatient stays to reflect the distribution of DRGs in the NY discharge data. The results in cols. (5)–(6) are based on the New York cardiac care sample. 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 treated hospitals in the year prior to the deal.

Table A.5: Robustness

	(1) Price	(2) Expenses per bed	(3) Cardiac reads.
1. Preferred estimate	724.2	-56,474	0.0198
	(269.7)	(15,709)	(0.0075)
2. No controls	699.7	-60,579	0.0172
	(300.8)	(16,308)	(0.0086)
3. Patient weighted	535.3	-37,922	–
	(363.1)	(26,234)	–
4. DRG fixed effects	681.8	–	0.0184
	(289.0)	–	(0.0079)
5. Non-neighbor untreated	706.7	-59,185	0.0191
	(269.2)	(15,851)	(0.0078)
6. Propensity score matching	738.1	-48,803	0.0198
	(263.5)	(14,774)	(0.0074)
7. Coarsened exact matching	758.0	-27,268	0.0232
	(307.8)	(16,482)	(0.0080)
8. Ln(dependent var.), Logit	0.0400	-0.0527	0.0205
	(0.0159)	(0.0150)	(0.0076)
9. Callaway Sant'Anna	771.3	-37,177	0.0340
	(327.1)	(13,808)	(0.0167)
10. Clustered by deal	724.2	-56,474	0.0198
	(263.8)	(16,660)	(0.0075)
11. Clustered by HRR	724.2	-56,474	0.0198
	(260.6)	(20,338)	(0.0066)
Observations	2,918	2,918	44,705
Dependent var. mean	15,407	1,016,550	0.164

Notes: The table displays ten different robustness checks (rows 2–11) that change the covariates, sample, functional form, or specification to test the sensitivity of the preferred estimates for key outcomes (row 1). Cols. (1) and (3) use the commercial claims data, while col. (2) uses data from the AHA survey on operating expenses per bed (2017\$). Row 3 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-payor admissions as weights in cols. (1) and (2), respectively. Row 5 presents results from the preferred specification when we exclude never acquired hospitals located within 5 miles of any acquired hospital ('neighbors'). This sample mitigates concerns regarding spillover contamination. Rows 6 and 7 present results on matched subsamples where the matched controls are identified using propensity score and coarsened exact matching, respectively. Section 7 details the covariates used in matching. In row 8, cols. (1)–(2) present results for log values, while col. (3) uses a logit model. Row 9 presents Callaway and Sant'Anna (2020) estimates, which are robust to bias due to the staggered treatment design. We explicitly exclude not-yet-treated hospitals as potential controls in this exercise. If not specified otherwise, results in cols. (1)–(2) are obtained using hospital-year level models (not weighted by patient volume), while those in col. (3) present results from patient-level models. Results in col. (1) are weighted to match the distribution of DRGs in the New York discharge data. Unless specified, all regressions include hospital and year fixed effects and controls as described in Section 4. Standard errors are in parentheses and typically clustered at the hospital level, except in rows 10 and 11. The dependent variable mean is for independent hospitals in the year prior to merger.

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 the 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 (coordination of benefits). 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 CMS IDs, 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 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.[44] These records are available for

[44] Our study does not use hospital ambulatory surgery visits, which are also available.

research purposes in the New York State Inpatient Database (SID) as part of the Healthcare Cost and Utilization Project (HCUP), sponsored by 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 a large hospital merger wave during the study period.

The SID contain clinical and resource-use information for visits for all expected payors, including Medicare, Medicaid, private insurance, self-pay, and "no charge." Available data elements include the patient's age, gender, ZIP code of residence, primary payor, 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 consists of trimming 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. We find similar results by averaging margins over two years and using the ratio of total net income to total revenue.

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 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 mergers 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 payors who are admitted to hospitals serving Medicare or Medicaid patients, including all hospitals in the Elevance sample.[45] 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 $geq9$ 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, delivery, orthopedic surgery, and oncology care. These inpatient stays were identified using combinations of CPT codes and ICD9/ICD10 procedure codes. This allows us to describe the patients' health care use before and after the care episode.

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 (CAH), 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 "Datasets and Sources.")

For our main price analyses, we exclude observations with prices below the 1st percentile or above the 99th percentile and limit the sample to commercial 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 also exclude individuals with fewer than 90 days of insurance eligibility following their discharge from

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

the index event from our analyses with 90-day readmissions. All pregnancy analyses exclude individuals with ages above the 99th percentile. 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 "Identifying Hospital Mergers." We further limit our sample of treated hospitals to four years prior to and including the merger and three years after it.

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 payors 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 admitted between Jan 1, 2009 and Sept 30, 2017; this ensures that we can observe 90-day mortality outcomes for all patients from the admission date. We exclude stays longer than 365 days, as well as the 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 ICD10 equivalent codes for the ones listed in that paper.

A.3 Identifying Hospital Mergers:

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 merger 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 merger 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 mergers and 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 QHR Health (N = 12 hospitals in our sample). For each hospital, we consider the first merger 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. We define a system creation as the first year in which two non-critical access hospital, general acute care hospitals are owned by the same system. Finally, we searched hospital press releases for hospital leaders' motivations for merging. The most frequently cited motives included easing capital/growth constraints, improving quality, expanding services, avoiding closure/conversion, reducing operating costs, and increasing prices.