Do Hospital Mergers Reduce Costs?

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Abstract

Proponents of hospital consolidation claim that mergers lead to significant cost savings, but there is little systematic evidence backing these claims. For a large sample of hospital mergers between 2000 and 2010, I estimate difference-in-differences models that compare cost trends at acquired hospitals to cost trends at hospitals whose ownership did not change. I find evidence of economically and statistically significant cost reductions at acquired hospitals. On average, acquired hospitals realize cost savings between 4 and 7 percent in the years following the acquisition. These results are robust to a variety of different control strategies, and do not appear to be easily explained by post-merger changes in service and/or patient mix. I then explore several extensions of the results to examine (a) whether the acquiring hospital/system realizes cost savings post-merger and (b) if cost savings depend on the size of the acquirer and/or the geographic overlap of the merging hospitals.

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1 Introduction

According to the healthcare market intelligence firm Irving Levin, there were nearly 900 announced hospital mergers and acquisitions[1] (M&A) between 2000 and 2012. As shown in Figure 1, after a decline in activity at the beginning of the millennium, hospital mergers remained relatively flat between 2002 and 2009 with around 50 to 60 transactions per year.[2] Concurrent with the Affordable Care Act, which was signed into law in early 2010, there has been a sharp uptick in hospital mergers, with the number of deals essentially doubling within three years. The merger wave continued in 2013, including the "mega-mergers" of Community Health Systems with Health Management Associates and Tenet Healthcare with Vanguard Health Systems.

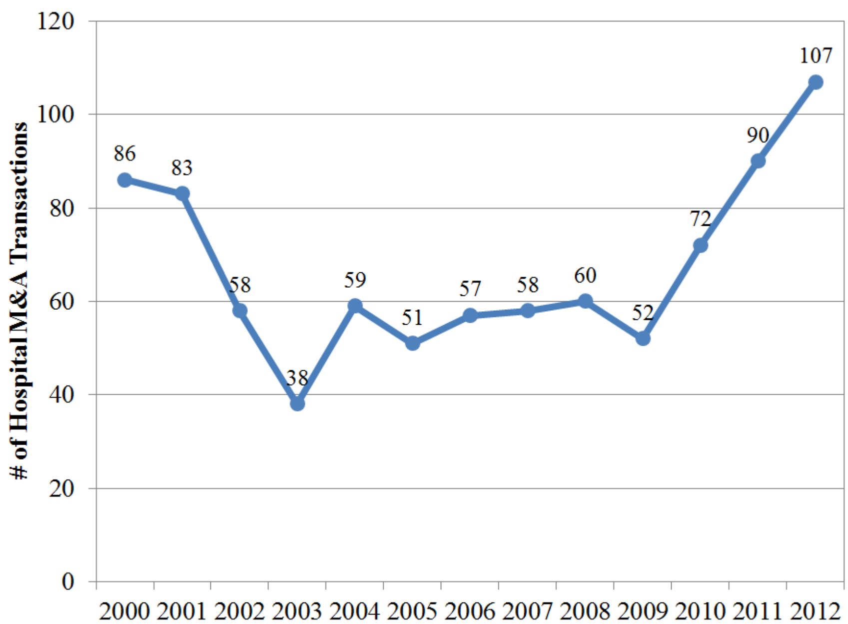

Figure 1: Hospital M&A Trends, 2000-2012

For antitrust authorities such as the Federal Trade Commission (FTC), consolidation of competing hospitals is often a major concern. Prior to the merger of two hospitals A and B, one factor that disciplines the pricing of each hospital is that higher prices will cause insurers and their enrollees to substitute to the competing hospital. After the merger, substitution between A and B is ineffective in restraining prices since the owner of the combined entity

[1] Throughout the paper, the terms "merger" and "acquisition" are used interchangeably.

[2] Source: AHA Trendwatch Chartbook 2014, Organizational Trends, Chart 2.9. Irving Levin Associates, Inc., *The Health Care Acquisition Report*, Twentieth Edition, 2014.

receives the profits from both hospitals.[3] Merger simulation models often predict substantial price increases from mergers (e.g., Capps et al. (2003) and Gowrisankaran et al. (2015)), and empirical studies of consummated mergers have documented sizable price increases as well (e.g., Dafny (2009) and Haas-Wilson and Garmon (2009)). Drawing on this work, antitrust authorities actively investigate and challenge hospital mergers that they believe will meaningfully reduce competition – "Hospitals that face less competition charge substantially higher prices" (Martin Gaynor, former Director of the FTC's Bureau of Economics).[4]

Merging hospitals, on the other hand, typically claim that the merger will yield reductions in cost and improve the quality of care. Common arguments are that administrative functions can be consolidated, duplicative services can be eliminated, and that there are economies of scale in things like purchasing hospital supplies and in effectively utilizing electronic medical record systems. Whether these claims of cost savings are legitimate (and to what degree) is of crucial importance for antitrust authorities, since cost reductions can offset or overturn the incentives for hospitals to increase prices after the merger. Beyond antitrust questions, there is also a general policy interest in promoting the delivery of cost-effective care. Given the clear policy relevance of the question, several past academic studies have examined whether hospital mergers have indeed triggered cost reductions; for example: Alexander et al. (1996), Connor et al. (1998), Spang et al. (2001), Dranove and Lindrooth (2003), Spang et al. (2009), and Harrison (2011). On balance, the evidence thus far fails to support strong claims of systematic cost savings from mergers; while these articles typically find cost savings for at least some subset of studied mergers, overall the evidence is mixed.[5]

The results from prior research are difficult to generalize to today for at least two reasons. First, the latest year of mergers in the studies cited above is 1997; technology in healthcare is continually evolving, and the ways in which hospitals can realize cost savings may be very different from decade to decade or even year to year. Second, the hospital consolidation examined in prior studies was mostly (a) between two hospitals and (b) local. Harrison (2011) reports that over 80 percent of mergers in her data are between two hospitals, and many of the papers cited above restrict their analysis to exclusively these types of mergers.

[3]There are also theories under which mergers between hospitals that are not directly substitutable with one another can increase prices. See recent work by Vistnes and Sarafidis (2013), Dafny et al. (2016), and Lewis and Pflum (2016).

[4]Pear, R. (2014, September 17). F.T.C. Wary of Mergers by Hospitals. *The New York Times*.

[5]In an Amicus Brief to the Supreme Court in *FTC v. Phoebe Putney Health System* (concerning Phoebe Putney's acquisition of rival Palmyra Medical Center in Albany, Georgia), a group of academic economists write: "the empirical evidence on whether hospital consolidation leads to cost savings is mixed at best." Brief of *Amici Curiae* Economics Professors in Support of Petitioner in *FTC v. Phoebe Putney Health System*, No. 11-1160.

Several of the cited papers also restrict their analysis to mergers within the same market area (e.g. MSA), and a comparison of merger counts across the studies indicates that these mergers characterize the majority of the available sample. Most hospital acquisitions today and throughout the 2000s are by multihospital systems, and many of these acquisitions occur across geographic markets (see section 2.2 for more descriptive details).[6] There are several ways in which acquisitions by multihospital systems could be meaningfully different. Economies of scale in purchasing and/or managing inputs, for example, may be much more salient for hospitals joining large systems than for those merely joining a single neighbor. Alternatively, multihospital systems may be more skilled in identifying poorly managed hospitals across the country, acquiring them, and then implementing efficiency-improving operational changes.

The goal of this paper is to evaluate whether mergers between general acute care hospitals that occurred between 2000 and 2010 have generated cost savings. Using cost data from the Centers for Medicare & Medicaid Services (CMS) linked to M&A data from the American Hospital Association (AHA) and the healthcare market intelligence firm Irving Levin, I estimate difference-in-differences models that compare cost trends at acquired hospitals (hereafter, "target" hospitals) to cost trends at several different groups of control hospitals whose ownership did not change during the period. Pre-merger, the two groups of hospitals appear to share common cost trends. Post-merger, target hospitals experience slower cost growth than control hospitals. The estimates indicate that target hospitals experience cost reductions of 4 to 7 percent in the years following the acquisition (on average).

One major question in interpreting these cost reductions is to what extent they might have been generated by simple changes in service and/or patient mix, as opposed to true efficiency improvements. While the main results control for the effect of contemporaneous changes in several observable measures like the percentage of inpatient discharges accounted for by Medicare, these measures may not fully control for the effect of possible changes in service and patient mix. To explore the likelihood of meaningful changes at target hospitals, I examine the effect of acquisition on several other measures besides costs. On balance, the results indicate that the observed cost reductions are not easily explained by simple changes in service and/or patient mix.

In addition to target hospitals, consolidation could also affect costs at the *acquiring* hos-

[6] It is not entirely clear whether multihospital and out-of-market acquisitions were uncommon in earlier time periods, or if they simply did not show up in available data (usually, American Hospital Association data). For instance, the Hospital Corporation of America (HCA) acquired many hospitals across the U.S. during the 1970s and 1980s, but these acquisitions may not have been tracked by the AHA data.

pital/system ("acquirer" hospitals). If mergers are capable of generating system-wide cost reductions, then consolidation has the potential for much larger aggregate cost effects. In contrast to the results for target hospitals, however, difference-in-differences models examining cost trends at acquirer hospitals fail to reject the null hypothesis of no effect. That said, for reasons related to the representativeness of the hospitals on which this result is based (see section 5.1 for details), I view this finding more tentatively than the main results for target hospitals.

To see if average cost savings differ by the size of the acquirer, I estimate separate effects for independent acquirers and multihospital systems of varying size (2-10, 11-50, and 51 or more hospitals). Target hospitals that were acquired by multihospital systems in all three size groups experience statistically significant reductions in cost, while hospitals acquired by independent hospitals do not. That said, it is unclear whether this difference is due to any inherent advantage of multihospital systems to achieve cost savings, or due to selection. In particular, acquisitions by independent hospitals are much more likely to occur in the same market than acquisitions by multihospital systems. These in-market acquisitions may be more likely to be driven by market power rather than cost saving motives, and I find evidence consistent with this hypothesis; after controlling for in-market vs. out-of-market acquisitions, both the magnitude and statistical significance of differences in cost savings by acquirer size shrink.

Moreover, the result that out-of-market acquisitions appear more likely to generate cost savings than in-market acquisitions implies caution when applying the main results of the paper – 4 to 7 percent cost reductions for target hospitals – to antitrust questions, which predominantly (though not exclusively) pertain to in-market acquisitions. Except when utilizing an extremely large market definition (state), I do not find statistically significant evidence of post-merger cost savings from in-market mergers. In addition, while the main results indicate potential for cost savings from mergers, they do not speak to the question of whether merger is necessary to achieve them. Since I am unable to precisely identify the source of the observed post-merger cost savings, it is possible that alternative actions – besides merger – would have been capable of generating the same cost savings.

Last, it is worth briefly connecting my findings for hospitals to the broader literature on mergers and acquisitions. Numerous prior studies examine plant-level data across a variety of industries in order to assess the effects of M&A on firm productivity. While many such studies find productivity gains post-acquisition (e.g., Lichtenberg et al. (1987); McGuckin and Nguyen (1995); Maksimovic and Phillips (2001)), recent work that attempts to more precisely separate productivity gains from market power (Blonigen and Pierce (2016)) finds little evidence of post-acquisition productivity improvements. That said, separating horizontal and non-horizontal M&A, Blonigen and Pierce (2016) do find productivity improvements for non-horizontal transactions, a finding broadly consistent with my results for in-market vs. out-of-market mergers. A related literature examines stock price data to study the impact of mergers, typically finding large increases for target firm stock prices after merger announcements and small and statistically insignificant effects for acquirer firms (Andrade et al. (2001)). The result here that target hospitals experience cost reductions but acquirer hospitals do not is therefore consistent with the broader evidence about the relative effect of mergers on targets vs. acquirers. Other research has explored the relationship between stock returns post-acquisition and firm size, finding that larger acquirers tend to do worse than smaller acquirers (Moeller et al. (2004)). The results here, on the other hand, indicate that cost savings are larger for multihospital system acquirers, though as noted above much of this difference may be attributable to selection.

The rest of the paper proceeds as follows. Section 2 provides an overview of the data on hospital costs and mergers. Section 3 outlines the empirical strategy for estimating the effect of mergers on costs. Section 4 presents the main results and examines whether the observed cost savings can likely be attributed to other factors, such as changes in service and/or patient mix. Section 5 explores costs at acquirer hospitals and the effect of acquirer size and geographic overlap. Section 6 conducts robustness checks. Section 7 concludes.

2 Data

2.1 Costs

Data on hospital costs comes from the Centers for Medicare & Medicaid Services' (CMS) Healthcare Cost Report Information System (HCRIS), 1998-2012.[7] Every Medicare-certified provider – including nearly every hospital in the U.S. – is required to submit an annual cost report containing information about facility characteristics, utilization, costs, and other financial data. From the HCRIS data, I construct a measure of hospitals' cost per patient: *total cost per adjusted discharge*, which is defined as:

[7]Data from 1998-1999 and 2011-2012 is collected for the purposes of having pre and post data for all mergers in the 2000-2010 sample.

$$\text{total cost per adjusted discharge} = \frac{\text{total cost}}{\text{inpatient discharges} \cdot (1 + \frac{\text{outpatient charges}}{\text{inpatient charges}})}. \quad (1)$$

Instead of patient count, the quantity in the denominator of (1) – often referred to as *adjusted discharges* – utilizes hospital charges to convert outpatient activity at the hospital into an estimate of the equivalent number of inpatient discharges. This adjustment is intended to account for the differences in resource intensity between inpatient and outpatient care, e.g. so that a shift toward outpatient services (which tend to be less costly) does not generate a mechanical decrease in the cost measure. That said, the measure remains an average over all patients that a hospital treats, and therefore is sensitive to changes in things like service and patient mix.[8] I return to this issue in section 3.1 when discussing the control variables included in the regression specifications, and again in section 4.2 in which I directly look for evidence that the observed cost savings are mechanical in nature rather than true cost reductions.

Cost per adjusted discharge is a commonly used measure in studies of hospital costs. For example, of the studies cited in the introduction, Alexander et al. (1996), Connor et al. (1998), Spang et al. (2001), and Spang et al. (2009) all utilize cost per adjusted discharge or patient day as the primary dependent variable. An alternative approach (utilized in Dranove and Lindrooth (2003) and Harrison (2011)) is to use total cost as the dependent variable and include a function of patient volume on the right hand side – I examine the robustness of the results to following this alternative approach in section 6.3.

In frontier merger simulation models (e.g., Gowrisankaran et al. (2015) and Ho and Lee (2016)), the relevant hospital cost in determining market outcomes is the marginal cost of treating a patient. Ideally, costs in the data could readily be split between fixed and marginal, but (a) it is difficult to cleanly classify various cost line items as purely fixed or purely marginal,[9] and (b) the allocation of costs to line items may be somewhat arbitrary and vary across hospitals. I therefore rely on total cost per adjusted discharge as the main dependent variable in the analysis, though the measure should be viewed only as a rough approximation of marginal cost.[10]

[8]This aggregation is by necessity; costs and discharges by diagnosis/procedure are not available in the HCRIS data.

[9]More information about the line items that make up total costs are given in Table A1 in the appendix.

[10]In their empirical analysis, Ho and Lee (2016) also use an average cost measure as an approximation of marginal cost.

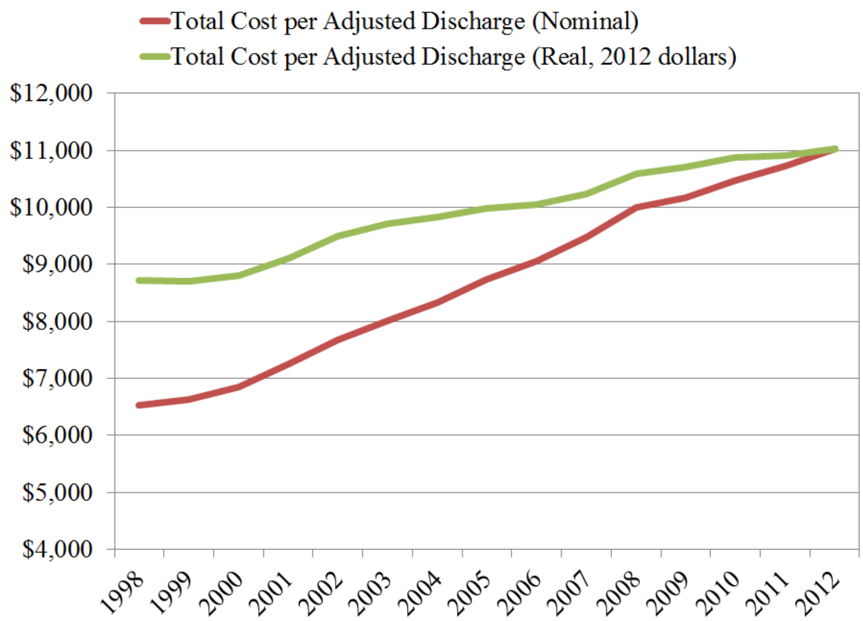

Figure 2: HCRIS Cost Trends, 1998-2012 The real series adjusts for economy-wide inflation using the BEA's GDP deflator.

Figure 2 plots the average total cost per adjusted discharge (hereafter often referred to only as "cost") from 1998 to 2012 for all hospitals in the HCRIS data. Between 1998 and 2012, cost grew from $6,500 to $11,000 in nominal terms (3.8 percent growth annually), and $8,700 to $11,000 in real terms (1.7 percent growth annually). These numbers are similar to other estimates using nationwide samples. Using nationwide inpatient discharge data and cost-to-charge ratios from CMS, the Healthcare Cost and Utilization Project (HCUP) reports average cost per inpatient discharge of $7,500 in 2003 and $10,500 in 2011.[11] The equivalent numbers from the HCRIS data are similar: $8,010 and $10,721, respectively. Therefore, it appears that the adjustment for outpatient activity in equation (1) does a reasonable job of converting outpatient activity into an equivalent number of inpatient discharges.

2.2 Mergers

The starting point in identifying hospital mergers is the American Hospital Association (AHA) *Annual Survey of Hospitals* (1998-2012), which contains a variable tracking system ownership: sysid. A tentative way to identify mergers is thus to scan the data for changes in sysid. While it is uncommon for the data to report what appear to be wrong sysids, it

[11]HCUP, Cost of Inpatient Discharges 2003 to 2013, Report #2013-01, 2013.

happens somewhat frequently that hospitals which belong to systems do not utilize a sysid until the later years of the data. If all changes in sysid are considered mergers, this approach will result in many "false positives" in which hospitals with no change in ownership are treated as having been acquired.

To address these kinds of errors, I supplement the AHA data with the 1998-2012 versions of the *Hospital Acquisition Report*, a yearly report tracking hospital M&A published by the healthcare market intelligence firm Irving Levin. Each yearly report lists the deals that Irving Levin identified as occurring during the year, along with information such as the purchase price and some characteristics of both the target and the acquirer (e.g., bed sizes, geographic locations, etc.). By matching transactions in the Irving Levin data to the AHA data (using hospital names and locations), I use the Irving Levin data to construct a new system identification variable that changes for a given hospital only if that hospital was acquired in a transaction identified by Irving Levin. This new system identification variable differs from sysid in at least one year for around 30 percent of hospitals in the data, and for these hospitals I then manually searched the internet (looking at news stories, archived versions of hospital websites, etc.) in an attempt to resolve all discrepancies. The resulting system identification variable – a combination of AHA data, Irving Levin reports, and a heavy dose of internet research – is what is used to identify mergers and calculate statistics such as acquirer size. The final variable matches the original AHA sysid for about 90 percent of observations.[12]

The data indicates that hospital M&A is currently driven by multihospital systems and that much of this activity occurs across geographic markets. Excluding acquisitions of Critical Access Hospitals (CAHs),[13] Table 1 provides summary statistics for mergers between 2000 and 2010: the number of transactions, the number of target hospitals, the median number of hospitals owned by the acquirer at the time of the transaction (Median Acquirer Size), and the median number of hospitals acquired in each transaction (Median Target Size). Transactions in which acquirer size is zero, e.g. when a system terminates a long-term lease with a local hospital authority, are excluded from the table and in all future analyses;

[12]See sections 8.3 and 8.4 in the appendix for (a) further information about challenges in measuring hospital ownership and (b) a comparison of mergers in my final dataset to the mergers contained in the Irving Levin reports.

[13]Critical Access Hospitals are rural hospitals that must meet various criteria (e.g., having no more than 25 beds and offering round-the-clock emergency care) and are reimbursed by Medicare on a cost basis. Due to their small size and the eligibility requirements of the CAH designation, CAHs tend to operate quite differently from non-CAH hospitals. While CAHs are excluded from the subsequent analysis for this reason, the results remain largely unchanged when including them.

Table 1: Merger Descriptive Statistics, 2000-2010

Market Definition	Type	# of Transactions	# of Target Hospitals	Median Acquirer Size	Median Target Size
–	Total	459	641	7	1
HSA	In-Market	91	105	4	1
	Out-of-Market	368	536	8	1
HRR	In-Market	281	421	6	1
	Out-of-Market	178	220	11.5	1
State	In-Market	395	557	7	1
	Out-of-Market	64	84	12	1

these transactions represent changes in ownership but not consolidation in the industry. The statistics are further broken out by whether the acquirer owned at least one hospital in the same market as at least one of the target hospitals (in-market), or not (out-of-market). The table reports statistics for three market definitions of increasing size: hospital service area (HSA), hospital referral region (HRR), and state. Recall that prior work on the effect of hospital mergers on costs primarily examined mergers between two hospitals in the same local area. While the median transaction throughout the period involves only a single hospital being bought, the acquirer tends to be a multihospital system, and many acquisitions occur out-of-market. For instance, HRRs are typically somewhat large,[14] containing an average of around 13 hospitals; yet, 39 percent of mergers in the data did not have any HRR overlap between target and acquirer. In short, recent consolidation in the hospital industry is very different from the consolidation examined by prior research. In the main results in section 4, I pool together transactions irrespective of acquirer size and geographic overlap; section 5.2 explores how merger effects vary according to these factors.

3 Empirical Strategy

After linking the HCRIS data to the AHA data (using hospital Medicare provider numbers), I proceed with the econometric analysis. Each observation in the final dataset is a unique combination of hospital and year, with variables containing hospital costs, ownership, and a handful of hospital characteristics. The primary focus is determining whether target hospitals (i.e., hospitals that were acquired at some point between the years 2000-2010) realized

[14] HRRs are defined by determining where Medicare patients receive major cardiovascular surgery and neurosurgery. Each HRR contains at least one city where patients can receive both types of surgery.

reductions in cost as a result of the acquisition. To examine this question, I compare cost trends at target hospitals to cost trends at hospitals that were never directly involved in merger activity during the period (either as a target or acquirer); I refer to these hospitals as "control hospitals" or "controls." Hospitals of the *acquirer* (i.e., those gaining a new system member) – which are excluded from the pool of control hospitals – are examined in section 5.1.

3.1 Primary Specification

The primary estimating equation is a fixed-effects model of the form:

$$\ln(cost_{ht}) = \alpha_h + \gamma_t + X_{ht}\beta + \sum_{k=-4}^{4} \lambda_k \cdot \mathbb{1}[h \in \mathcal{A}, t = \tau_h + k] + \varepsilon_{ht} \,, \tag{2}$$

where h is hospital (α_h are hospital fixed effects), t is year (γ_t are year fixed effects), X_{ht} is a vector of time-varying hospital characteristics, $\mathbb{1}[\ ]$ is the indicator function, $\mathcal{A}$ is the set of target hospitals, and τ_h is the year in which hospital h was acquired. For example, $\mathbb{1}[h \in \mathcal{A}, t = \tau_h - 2]$ is a dummy variable that is equal to one if hospital h was acquired and the year is two years prior to that acquisition.[15] Observations four or more years prior to the acquisition are combined into a single category, as are observations four or more years after the acquisition (e.g., $k = -4$ refers to years at least four years prior to the acquisition).[16] $cost_{ht}$ is total cost per adjusted discharge (for hospital h in year t), as described in section 2.1.

The λ_k parameters ("leads and lags") measure how much cost trends for target hospitals depart from the cost trends for control hospitals ($h \notin \mathcal{A}$), both before and after the acquisition. The key identifying assumption in estimating equation (2) is that the control hospitals have the right counterfactual cost trends for target hospitals – formally, that the acquisition dummy variables $\mathbb{1}[h \in \mathcal{A}, t = \tau_h + k]$ are uncorrelated with the error term ε_{ht}. The λ's with $k < 0$ (the leads) can be interpreted as a suggestive test of this assumption. If these coefficients are close to zero, this means that target hospitals have similar cost trends to control hospitals pre-merger. The λ's with $k > 0$ (the lags) can then more safely be interpreted as the effect of the merger as opposed to reflecting unobserved differences in cost trends between target and control hospitals.

[15] The inclusion of hospital fixed effects requires omitting one of these dummies from the regression; I omit the year before the merger ($t = \tau_h - 1$).

[16] Restricting the data for target hospitals to the 4 years before and after the acquisition (rather than combining with observations more than 4 years out) yields extremely similar results.

I also estimate a simpler version of equation (2) that reduces the number of λ parameters to two: one for the year of the merger (λ_0) and one after (λ_{post}). This model has the benefit of combining the year-specific post-merger effect estimates into one, which can be interpreted as the overall effect of the merger:

$$\ln(cost_{ht}) = \alpha_h + \gamma_t + X_{ht}\beta + \lambda_0 \cdot \mathbb{1}[h \in \mathcal{A}, t = \tau_h] + \lambda_{post} \cdot \mathbb{1}[h \in \mathcal{A}, t > \tau_h] + \varepsilon_{ht}\,. \qquad (3)$$

Returns to Scale

The regression equations (2) and (3) implicitly assume constant returns to scale. While the specifications do allow different hospitals to have different cost structures and for those differences to vary flexibly by hospital size (via the hospital fixed effect α_h), within hospital the specifications assume that cost per adjusted discharge does not depend on patient volume. I discuss how this implicit assumption might affect the interpretation of the estimates in section 4.2. In section 6.3, I report results from an alternative specification – in which the dependent variable is total cost – that allows for increasing or decreasing returns to scale.

Hospital Characteristics

It is also worthwhile to highlight the role of hospital characteristics X_{ht} in the estimating equations (2) and (3). Total cost per adjusted discharge aggregates over all patients seen by a hospital. If a hospital's service mix changes following the merger – e.g., by shifting from high-tech to low-tech services – cost per discharge will change mechanically in turn. But this is not what we have in mind when referring to cost efficiencies, which are reductions in cost *for the same service*. If service mix changes post-merger, costs would need to be below what would be predicted given the change in service mix to judge the merger as having generated cost efficiencies. With this kind of concern in mind, I include the following variables in X_{ht}:[17]

- For-Profit status (0/1): available from HCRIS
- Transfer-Adjusted Case Mix Index (CMI, logged): Case Mix Index measures the clinical complexity of a hospital's Medicare patients; available from CMS
- Total Beds (logged): available from HCRIS
- % of inpatient discharges accounted for by Medicare: available from HCRIS

[17] There are many other possible control variables that can be included, such as measures of technological capabilities, teaching intensity, area demographics, quality, etc. For results from specifications including an even richer set of controls, see section 8.2 in the appendix. The impact of these additional controls on the results is negligible.

- % of inpatient discharges accounted for by Medicaid: available from HCRIS
- Wage Index (logged): Medicare's payments to hospitals are adjusted to reflect geographic differences in wage levels; available from CMS

3.2 Sample Restrictions and Descriptive Statistics

Before estimating equations (2) and (3), I make several additional sample restrictions. First, I eliminate hospitals outside of the 50 U.S. states and the District of Columbia (e.g., Puerto Rico). Second, I drop military, Veterans Affairs, and Indian hospitals. Third, I keep only hospitals that are always identified as general acute care hospitals, excluding long-term care hospitals, children's hospitals, and psychiatric hospitals. Fourth, I require that hospitals do not have gaps in their data and that they appear in the data for at least five years. Fifth, I drop hospitals that are acquired more than once in the 2000-2010 period (about 15 percent of all target hospitals). It is unclear how to properly construct the acquisition timing variables for hospitals that are acquired more than once (e.g., a year will be in the post period for one acquisition and in the pre period for another). Sixth, I require that target hospitals have at least two years of data prior to being acquired and at least two years of data after being acquired, so that all in-sample target hospitals contribute identifying variation to both the pre-merger and post-merger effect coefficients (leads and lags) in equation (2).

Table 2 compares transactions in the final sample (i.e., after performing the drops listed above) to the full set of (non-CAH) transactions. Metro is a dummy variable marking whether a hospital is located in a Metropolitan Statistical Area (MSA); all other variables have been defined above. While the table reveals some small differences in the samples, the only difference that is statistically significant is that target hospitals in the final sample are less often located in the West South Central Census division (e.g., Arkansas and Texas).[18] Given the small differences between the samples, I believe that the transactions and hospitals included in the final sample are likely largely representative of the full universe of hospital mergers over the 2000-2010 period.[19]

Table 3 reports summary statistics for target and control hospitals; statistics for acquirer hospitals are also included for completeness. The statistics are based on the initial year that a hospital appears in the data (1998 for 94 percent of the sample), and are thus calculated prior to the acquisitions under study. Many of the differences between target and control hospitals

[18] The full listing of states in each Census division is available at `https://www2.census.gov/geo/pdfs/maps-data/maps/reference/us_regdiv.pdf`, accessed 1/3/2017.

[19] Further details about the distribution of acquirer and target sizes are provided in Figure A2 in the appendix; the full and final samples remain similar when examining the full distributions.

Table 2: Comparing the Full and Final Samples

	Full Sample	Final Sample
Transaction Characteristics:		
Total Transactions, 2000-2010	459	337
Total Target Hospitals	641	436
Median Acquirer Size	7	8
Median Target Size	1	1
Target Characteristics (Mean):		
CMI	1.311	1.312
Beds	148.56	153.46
% Medicare	0.412	0.407
% Medicaid	0.146	0.142
OP Charges / IP Charges	0.948	0.954
Wage Index	0.951	0.959
Metro	0.710	0.706
Census division		
East North Central	0.125	0.135
East South Central	0.134	0.122
Middle Atlantic	0.112	0.126
Mountain	0.058	0.073
New England	0.020	0.021
Pacific	0.095	0.094
South Atlantic	0.195	0.211
West North Central	0.059	0.062
West South Central	0.201	0.156*

Notes: Target characteristics are in the year of the merger. (***,**,*) indicates that the difference in means is significant at the (1%, 5%, 10%) level.

are statistically significant; e.g., target hospitals are somewhat smaller than control hospitals on average, are more likely to be for-profit, and have a different geographic distribution. Another standard measure of (dis)similarity between groups is the absolute standardized difference, which is defined as the absolute difference in means divided by the standard deviation (where the standard deviation is computed over all observations).[20,21] My review of the literature indicates that absolute standardized differences less than 0.1 are typically viewed as small differences between groups (Austin (2011)). Target and control hospitals are more similar according to this standard, but many differences still exceed the 0.1 threshold

[20]This measure may be preferred to statistical significance testing because of the confounding effects of sample size on significance levels. Large differences between groups may be statistically insignificant if samples are small, and tiny differences between groups may be highly significant if samples are large.

[21]The formula is slightly different for categorical variables like For-Profit, but the idea is the same.

Table 3: Comparing Acquirer, Target, and Control Hospitals

	Acquirer	Target	Control	Target vs. Control: p-value	Target vs. Control: Standardized Difference
Hospitals	882	436	1,674	–	–
Inpatient Discharges	10,736	6,777	8,134	0.001***	0.180
Cost per Adjusted Discharge	$6,027	$5,301	$5,806	0.000***	0.193
For-Profit	0.346	0.268	0.062	0.000***	0.673
CMI	1.447	1.321	1.320	0.916	0.006
Beds	241.4	163.7	186.9	0.004***	0.154
% Medicare	0.381	0.407	0.404	0.731	0.019
% Medicaid	0.130	0.141	0.151	0.092*	0.091
OP Charges / IP Charges	0.656	0.736	0.801	0.009***	0.139
Wage Index	0.963	0.940	0.964	0.006***	0.149
Metro	0.823	0.706	0.665	0.104	0.087
Census Division					
East North Central	0.149	0.135	0.162	0.174	0.073
East South Central	0.091	0.122	0.090	0.048**	0.106
Middle Atlantic	0.080	0.126	0.155	0.128	0.082
Mountain	0.080	0.073	0.055	0.145	0.078
New England	0.016	0.021	0.062	0.001***	0.182
Pacific	0.126	0.094	0.119	0.146	0.078
South Atlantic	0.238	0.211	0.144	0.001***	0.184
West North Central	0.065	0.062	0.079	0.218	0.066
West South Central	0.155	0.156	0.134	0.233	0.064

Notes: The table reports means across hospitals. All statistics are calculated in the initial year of the data (1998 for 94 percent of the sample). (***,**,*) indicates that the difference between target and control means is significant at the (1%,5%,10%) level. The standardized difference is the absolute value of the difference in means divided by the overall standard deviation.

or come close to doing so.

While it can be checked whether the two groups of hospitals share common cost trends pre-merger (in spite of any covariate imbalance), even if that is the case, there may be concerns that what happened in the pre-merger period is not fully representative of the post-merger period. For instance, if hospitals in the South Atlantic Census division (which are overrepresented in the target hospital group relative to the control group) experienced slower cost growth than the rest of the country in the post-merger period for reasons unrelated to hospital consolidation and uncontrolled for in the estimating equation, this slow cost growth may be spuriously attributed to mergers. The fact that the "post-merger period" is scattered across time (since hospitals are acquired at different points in time) greatly lessens these concerns, but one may still be uncomfortable that the control hospitals are observably different from target hospitals, even if they appear to share the same cost trends prior to acquisition. I turn to this issue in the next section, finding a subset of the control hospitals that is as similar as possible to target hospitals on observable dimensions.

3.3 Matching

In addition to specifications using the full set of control hospitals, I also estimate models in which I match target hospitals to a subset of the full control sample that is more similar along observable dimensions. More specifically, I perform 1-to-1 optimal Mahalanobis matching,[22] where the "distance" between hospitals is defined using the eight continuous variables in Table 3: inpatient discharges, cost per adjusted discharge, CMI, beds, % Medicare, % Medicaid, the ratio of outpatient to inpatient charges, and wage index (all measured in the initial year that a hospital appears in the data). I also require that the control hospital shares the same Metro status as the target hospital, is in the same Census division, and is present in the data for all years that the target hospital is present. Section 8.5 in the appendix provides more details about the matching procedure.

The results of the match are summarized in Figure A1, which shows the absolute standardized differences between target and control hospitals, both for the full control sample and the matched control sample. After matching, the differences for Metro and Census division are reduced to zero (by construction), and the differences for the other covariates all fall below the 0.1 threshold. That said, in the main matching specification I do not impose any conditions on for-profit status, and target and control hospitals remain extremely dissimilar on this dimension; target hospitals are substantially more likely to be for-profit. Matching exactly on for-profit status (in addition to the other matching criteria) requires dropping more than 50 target hospitals from the analysis – there is no match that meets all the constraints for the full sample – and also worsens the match for the other matching criteria. Results from a specification requiring exact matching on for-profit status are reported in section 6.2.

3.4 Additional Specification Using the Matched Control Sample

In addition to estimating equations (2) and (3) using the matched control sample, I also explore an additional specification that exploits the explicit pairing between target and

[22]See Stuart (2010) for a nice review of the matching literature.

control hospitals. In the alternate specification, the cost trend for each target hospital's matched control serves as the counterfactual cost trend, rather than the common year effects γ_t in equations (2) and (3).[23] This approach allows for more flexibility in cost trends. Defining $c(h)$ as the control hospital for target hospital h, I estimate:

$$\ln\left(\frac{cost_{ht}}{cost_{c(h)t}}\right) = \alpha_h + (X_{ht} - X_{c(h)t})\beta + \sum_{k=-4}^{4} \lambda_k \cdot \mathbb{1}[h \in \mathcal{A}, t = \tau_h + k] + \varepsilon_{ht}\,. \qquad (4)$$

I retain the full set of λ's to check that the ratio of costs for a target hospital and its matched control is stable in the pre-merger period, and also control for the effects of differential changes in hospital characteristics by subtracting the data for the matched control, $X_{c(h)t}$. As with the primary specification, I also estimate a specification that reduces the number of λ coefficients to one in the year of the merger and one after:

$$\ln\left(\frac{cost_{ht}}{cost_{c(h)t}}\right) = \alpha_h + (X_{ht} - X_{c(h)t})\beta + \lambda_0 \cdot \mathbb{1}[h \in \mathcal{A}, t = \tau_h] + \lambda_{post} \cdot \mathbb{1}[h \in \mathcal{A}, t > \tau_h] + \varepsilon_{ht}\,. \quad (5)$$

4 Main Results

For all reported results, standard errors are clustered by hospital and each hospital-year observation is weighted by the corresponding number of adjusted discharges. I weight so that the estimated post-merger effect can be interpreted as the cost savings for an average patient; unweighted specifications yield similar results. Table 4 presents estimates of equations (3) and (5) (i.e., without the full set of leads and lags). The three columns of the table correspond to three different control strategies: (1) equation (3) with all controls ("All"), (2) equation (3) with the matched controls ("Matched"), and (3) equation (5) with the matched controls ("Differenced").

In the year of the merger ($t = \tau_h$), cost growth at target hospitals is indistinguishable from cost growth at control hospitals; for all specifications, the estimated coefficient is close to zero and insignificant. The lack of an immediate effect is consistent both with (a) the coded year of the merger corresponding to a partial year of ownership at most, and (b) cost savings taking some time to be realized. Pooling together all years after the merger ($t > \tau_h$), the results indicate cost savings between 4 and 7 percent. The estimated effects of the hospital characteristics are also intuitive. For example, Medicare patients tend to require costlier procedures, and thus increases in Medicare patients are associated with increases in

[23]See Dafny et al. (2016) for another implementation of this idea.

Table 4: Main Results

	Control Group		
	All	Matched	Differenced
Post Merger	-0.039***	-0.050***	-0.074***
	(0.010)	(0.010)	(0.011)
Year of Merger	0.001	-0.006	-0.007
	(0.009)	(0.009)	(0.012)
For-Profit	-0.041***	-0.042***	-0.030*
	(0.014)	(0.015)	(0.018)
ln(CMI)	0.386***	0.441***	0.282***
	(0.052)	(0.067)	(0.081)
ln(Beds)	0.002	0.023	0.002
	(0.015)	(0.020)	(0.026)
% Medicare	0.949***	0.988***	1.131***
	(0.058)	(0.065)	(0.078)
% Medicaid	0.050	-0.007	0.019
	(0.042)	(0.040)	(0.062)
ln(Wage Index)	0.361***	0.254**	0.155
	(0.063)	(0.099)	(0.121)
R-squared	0.931	0.904	0.747
Observations	29,972	12,708	6,216
Hospitals	2,110	872	872
Target	436	436	436
Control	1,674	436	436

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors are clustered by hospital and observations are weighted by adjusted discharges. All specifications include hospital fixed effects. All specifications except the differenced specification include year fixed effects. For the differenced specification, all covariates are measured as the difference between each target and its matched control.

costs. Treating sicker Medicare patients (a higher CMI) is also associated with increased costs, as is having higher labor costs.

Results with the full set of leads and lags (equations (2) and (4)) are plotted in Figure 3 and presented in Table A2 in the appendix. In the figure, 95% confidence intervals are shown for the specification estimated using the all control group. For all specifications, target hospitals experience similar cost growth to control hospitals prior to being acquired; all lead coefficients are small in magnitude and statistically insignificant. In the year of the merger, cost growth remains similar at the two groups of hospitals. For all years following the merger, costs grow more slowly at target hospitals than controls, with the magnitude

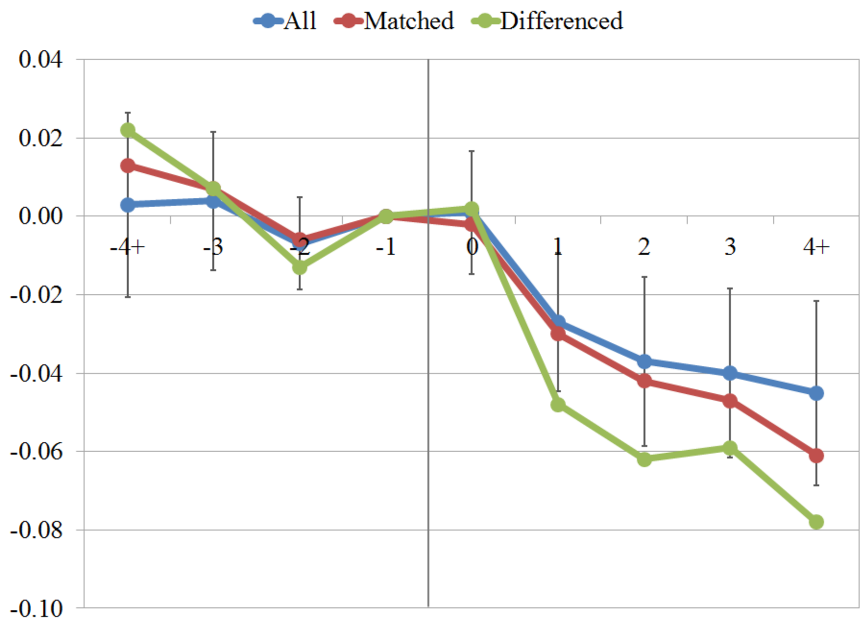

Figure 3: Leads and Lags Coefficient Estimates Zero corresponds to $t = \tau_h$, the year of the merger. The year before the merger, $t = \tau_h - 1$, is the omitted category. 95% confidence intervals are shown for the "All" specification.

of the savings increasing as time passes. For all specifications, the post-merger coefficients ($t > \tau_h$) are significant at the 1% level. Four years after being acquired, target hospitals are estimated to have cost per adjusted discharge at least 4 percent lower than they would have absent the merger.

4.1 Geographic Differences in Cost Trends

The main regression specifications include year fixed effects to capture cost trends over time, but these trends are not allowed to vary by geography. While the matched specifications match each target hospital to a control hospital in the same Census division, it is also possible to control for differential geographic cost trends more finely. Figure 4 plots coefficient estimates from specifications including area by year fixed effects for increasingly smaller areas: Census divisions (9 sets of year fixed effects), states (51 sets), and hospital referral regions (306 sets). 95% confidence intervals are shown for the specification including HRR by year fixed effects. As shown in the figure, including the finer time trend controls has only a small impact on the point estimates. For every specification, the estimated post-merger effect remains statistically significant at the 1% level. In addition to the finding in the main results that cost trends prior to the merger are similar at target and control hospitals, these

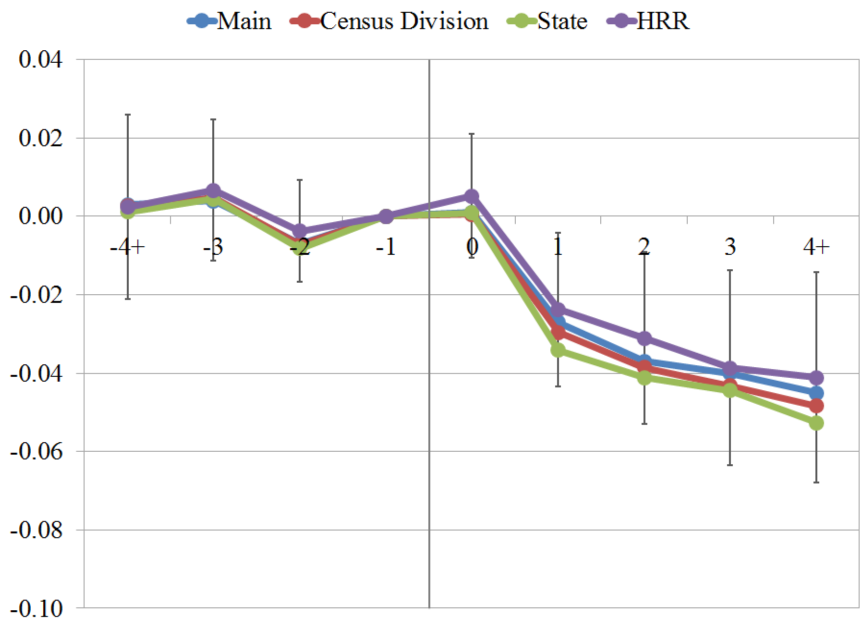

Figure 4: Leads and Lags Coefficient Estimates: Area by Year Fixed Effects Zero corresponds to $t = \tau_h$, the year of the merger. The year before the merger, $t = \tau_h - 1$, is the omitted category. All specifications use the all control group. "Main" refers to the results from estimating equation (2), while the others add the corresponding area by year fixed effects. 95% confidence intervals are shown for the specification including HRR by year fixed effects.

results further provide evidence against the hypothesis that acquisitions are systematically clustered in areas with slower than average cost growth.

That said, when including HSA by year fixed effects (more than 1,500 sets of year fixed effects), the estimated post-merger cost savings shrinks toward zero and becomes statistically insignificant. While these results indicate that the data cannot reject HSA-specific cost trends as an alternative explanation for the observed cost reductions, this is an extremely demanding test – around two-thirds of target hospitals are the only hospital in their HSA, and only 11 percent of target hospitals have at least four other hospitals in their HSA. HSA by year fixed effects therefore greatly reduce the variation available to estimate the post-merger effect.

4.2 Service and Patient Mix

The results to this point indicate that target hospitals experience reductions in cost per adjusted discharge of 4 to 7 percent in the years following the acquisition, and that these reductions do not appear to be explained by pre-existing systematic differences in cost trends between target and control hospitals. A natural next question is to what extent the observed reduction in costs reflects true efficiency gains as opposed to changes in service and/or patient mix. If cost reductions come via a shift away from costly procedures and/or patients, that would have very different implications (e.g., for antitrust questions) than if the cost reductions come from improved operational efficiency. While the regression models do control for the effects of any contemporaneous changes in things like the percentage of inpatient discharges coming from Medicare, there is a reasonable concern that these controls may only imperfectly account for the effects of potential changes at target hospitals. For instance, a shift away from Medicare patients may also correspond to a broader shift away from costly procedures, and the coefficient on % Medicare may not fully capture the cost implications of these other changes.

While I cannot fully address the possibility of contemporaneous changes in service and/or patient mix mechanically generating the observed cost savings, I can examine whether the measures I do observe appear to be changing post-merger. This analysis sheds light not only on observable changes at target hospitals, but also on the plausibility of unobserved changes (to the extent that changes in observed measures are correlated with changes in unobserved factors). For example, if there are no systematic changes in % Medicare or % Medicaid post-merger, it may be relatively less likely that other unobserved measures of patient mix *did* change.

To examine post-merger changes in measures other than costs, I estimate equations similar to equation (3) but with a variety of different outcome variables on the left hand side. I examine the following outcomes: (log) Case Mix Index (CMI), % Medicare, % Medicaid, (log) beds, (log) adjusted discharges, (log) inpatient discharges, the ratio of outpatient charges to inpatient charges, and (log) length of stay. For all specifications, I retain the hospital and year fixed effects (excluding the other covariates) and estimate the regressions using the all control group.[24] Table 5 gives the results.

For CMI, % Medicare, and % Medicaid, there are no discernible changes at target hospitals relative to controls. In other words, for several particularly salient measures of service and patient mix, there is no evidence of changes at target hospitals. That said, one important limitation with CMI is that it only captures a hospital's Medicare population, and therefore may not be completely representative of the hospital's broader case mix. Specifications that split hospitals into three groups based on the percentage of inpatient discharges accounted for by Medicare over the period – below the 25th percentile (32 percent), between the 25th

[24]The results are very similar when including area (e.g., HRR) by year fixed effects.

Table 5: Other Outcomes

Dependent Variable	Post Merger	Year of Merger	R-squared	+p-value of pre-trend test
ln(CMI)	-0.001	0.001	0.921	0.957
	(0.004)	(0.003)		
% Medicare	-0.001	-0.003	0.884	0.311
	(0.004)	(0.004)		
% Medicaid	0.004	-0.005	0.796	0.650
	(0.004)	(0.004)		
ln(Beds)	-0.028**	-0.017	0.969	0.008***
	(0.013)	(0.011)		
ln(Adjusted Discharges)	-0.035**	-0.083***	0.953	0.024**
	(0.015)	(0.021)		
ln(Inpatient Discharges)	-0.014	-0.071***	0.965	0.059*
	(0.015)	(0.021)		
OP Charges / IP Charges	-0.046***	-0.019	0.855	0.615
	(0.016)	(0.014)		
ln(Length of Stay)	-0.013*	-0.010	0.787	0.732
	(0.007)	(0.007)		

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors are clustered by hospital and observations are weighted by adjusted discharges. Each row is a separate regression. All specifications use the all control group and include hospital and year fixed effects (no covariates). +p-value of a test of the null hypothesis that the $t \leq \tau_h - 4$, $t = \tau_h - 3$, and $t = \tau_h - 2$ coefficients from specifications with the full set of leads and lags are all equal to zero.

and 75th percentiles, and above the 75th percentile (48 percent) – indicate statistically and economically insignificant changes in CMI post-merger for all three groups. For the "high" Medicare group in particular, for which about half or more of the hospital's inpatient discharges come from Medicare, CMI is likely to reflect the hospital's overall case mix. Another way to evaluate the reliability of CMI as a proxy for overall case mix is to interact ln(CMI) with indicators for the three groups in the main results – is CMI a stronger predictor of costs for hospitals that treat more Medicare patients? Compared to the pooled estimate of 0.386 in Table 4 (using the all control group), the estimated coefficients for low, medium, and high % Medicare hospitals are 0.426, 0.349, 0.370. These estimates are all statistically significant at the 1% level and cannot statistically be distinguished from one another. In short, while CMI only captures a hospital's Medicare population, it appears that it likely serves as a good proxy for overall case mix.[25]

[25]Besides looking directly at Case Mix Index, % Medicare, and % Medicaid on the left hand side, another way to examine the likelihood of changes in service and/or patient mix generating the results is to estimate

While the most direct measures of service and patient mix – CMI, % Medicare, and % Medicaid – do not indicate any contemporaneous changes in service and/or patient mix that would threaten the interpretation of the observed cost savings as efficiencies, the other measures in Table 5 do exhibit statistically significant changes. Of course, it is unsurprising that operations are changing at target hospitals; the key question for the purposes of the present analysis is how any such changes affect the interpretation of the observed post-merger cost reductions. The first result is a decrease in beds and adjusted discharges, though notably these decreases predate the merger. Recall that the main regression equation implicitly assumes constant returns to scale; if hospitals actually operate with increasing returns to scale, then the main results will likely understate the level of cost savings relative to what would be expected given the observed contraction in size/patient volume.[26] On the other hand, it is also possible that the decrease in beds and adjusted discharges signals a shift toward lower-complexity services, though any such shift would need to occur only for the hospital's non-Medicare business (given the lack of observed changes in CMI).

Inpatient discharges fall sharply in the year of the merger before rebounding to close to their pre-merger level. One interpretation of this pattern is that a substantial reorganization of hospital activity occurs at target hospitals in the year of the merger. This reorganization could in principle reflect a change in service or patient mix, but if so it is surprising that any such change would not also be reflected in CMI, % Medicare, or % Medicaid. Another interpretation is that target hospitals are contracting in the years leading up to the acquisition, and that the decrease in the year of the merger is simply the culmination of that pre-existing trend. For instance, target hospitals (on average) may be suffering from financial distress as a result of falling patient volume, which may make them likely candidates for acquisition.[27]

specifications that do not include these controls and compare the estimated post-merger coefficients to the main results that do. Oster (2016) formally examines this idea and proposes procedures to bound the true effect based on assumptions about the relationship of observables to unobservables (among other criteria). Excluding the three listed controls slightly increases the magnitude of the estimated post-merger effect – by 0.2 to 0.6 percentage points – which suggests that these controls do play some role. That said, applying the bounding procedure developed by Oster (2016), the lower bound on the magnitude of the post-merger cost savings remains above zero (with cost savings of around 2 percent), providing further evidence that the observed cost savings are unlikely to be explained by changes in service and/or patient mix alone. Full results for these specifications are available upon request.

[26]Results from specifications that match target and control hospitals on the basis of bed and discharge trends (in addition to the other matching criteria) are consistent with this possibility. After matching on bed and discharge trends, the estimated post-merger cost savings increases by around 0.5 percentage points compared to the original matched controls estimate (i.e., the second column of results in Table 4).

[27]This possibility is consistent with outside evidence that hospitals in financial distress seek out mergers. See, e.g., Ellison, A. (2015, May 11). Moody's: More distressed hospitals seek mergers to escape financial troubles. *Becker's Hospital Review*.

The key requirement for the cost analysis is that any such factors that are more prevalent for target hospitals compared to control hospitals are not correlated with cost trends. The examination of cost trends prior to merger is informative about many such possibilities – any factors on which target hospitals are selected do not appear to be associated with differences in cost trends between target and control hospitals prior to merger.

The ratio of outpatient charges to inpatient charges also falls post-merger at target hospitals. One explanation for this result is that target hospitals are on average shifting away from outpatient services to inpatient services. Another possible explanation is that post-merger pricing is more aggressive for inpatient services than outpatient services, which may result in a lower ratio of outpatient to inpatient charges even holding volume and service/patient mix constant. In either case, it is unclear whether the change would be capable of meaningfully contaminating the estimated post-merger cost savings (i.e., resulting in an estimate of significant cost reductions when the true effect is negligible). For instance, if the observed reduction in adjusted discharges is largely due to price changes rather than an actual reduction in patient volume, the main results will tend to understate the true cost savings (if the denominator in the cost per adjusted discharge calculation is artificially low post-merger, cost per adjusted discharge will be artificially high).

Last, the results indicate a small reduction in length of stay (LOS) post-merger. This result is consistent with target hospitals more efficiently utilizing hospital resources post-merger. That said, it is also consistent with a change in service and/or patient mix, though again it would be surprising if any such change did not show up in the directly observable measures. The literature on the effect of LOS on hospital costs demonstrates clear savings from LOS reductions (e.g., Carey (2000); Fine et al. (2000); Taheri et al. (2000); Carey (2002)), though the magnitude of estimated effects – for example, Carey (2002) finds an elasticity of cost with respect to LOS of around 0.3 – indicates that the observed 1.3 percent decline in LOS may only account for a small proportion of the total observed cost savings.

On balance, I do not find clear evidence that the observed cost savings at target hospitals can easily be traced to changes in service and/or patient mix. The most direct measures of service and patient mix (CMI, % Medicare, and % Medicaid) do not appear to change post-merger, and it is unclear that the measures that do change (beds, discharges, OP/IP charges, and length of stay) threaten the interpretation of the observed cost savings as efficiency improvements. Nonetheless, the results should be interpreted with this caveat in mind; especially with aggregate data, it is difficult to completely dismiss the possibility that the observed cost reductions are generated by alternative explanations besides cost efficiencies.

5 Extensions

5.1 Hospitals of the Acquirer

When considering the aggregate potential gains from hospital consolidation, it is important to know whether cost savings from mergers extend beyond target hospitals to the hospitals already owned by acquiring hospitals/systems. To examine the effect of acquisition for acquirer hospitals, I construct a sample of hospitals that either acquired another hospital or belong to a system that acquired another hospital. As with target hospitals, I restrict the sample to hospitals that were involved in a single (non-CAH) M&A transaction during the 2000-2010 period so that I can cleanly construct the timing variables. Since hospital systems often make multiple acquisitions during the period, this subsample contains only 278 hospitals even though many more hospitals than that belong to systems that made acquisitions. I also retain the 115 target hospitals that were acquired in these transactions (a subset of the 436 target hospitals on which the main results are based). I then estimate specifications similar to the main results, allowing for different post-merger effects for acquirers and targets.

The results are presented in Table 6. The results indicate small and statistically insignificant changes in costs at acquirer hospitals. The estimates for target hospitals are several times larger in magnitude, but are statistically significant only for the differenced control strategy. The null hypothesis that the acquirer and target effects are equal can also only be rejected for the differenced control strategy (at the 5% level). While these results fail to support the idea that acquirer hospitals experience cost reductions, this interpretation is complicated by the somewhat weaker evidence – as compared to the main results – for cost reductions at target hospitals.

Turning to the leads and lags results, which are presented in Table A3 in the appendix, target hospitals are estimated to experience cost savings of 6 to 8 percent four or more years after the acquisition, and this effect is statistically distinguishable from the effect for acquirer hospitals at the 10% level for all three control strategies (and at the 5% level for two of them). This difference is further illustrated in Figure 5, which also plots the results for the full set of target hospitals (i.e., the first column of results in Table A2). While cost savings in the full sample of target hospitals occur sooner than in the more limited subsample, the effects are quite similar four or more years post-merger. The estimated effects for acquirer

Table 6: Acquirer Hospitals

	Control Group		
	All	Matched	Differenced
Post Merger (Acquirer)	0.002	-0.007	-0.014
	(0.010)	(0.011)	(0.014)
Post Merger (Target)	-0.022	-0.029	-0.073***
	(0.018)	(0.018)	(0.022)
Year of Merger (Acquirer)	0.007	0.001	-0.003
	(0.010)	(0.010)	(0.011)
Year of Merger (Target)	0.012	0.007	-0.019
	(0.018)	(0.018)	(0.026)
For-Profit	-0.036	-0.035	-0.046
	(0.023)	(0.026)	(0.033)
ln(CMI)	0.356***	0.343***	0.160*
	(0.051)	(0.069)	(0.083)
ln(Beds)	-0.004	0.003	-0.018
	(0.016)	(0.026)	(0.030)
% Medicare	0.984***	1.154***	1.068***
	(0.060)	(0.070)	(0.115)
% Medicaid	0.085**	0.095*	-0.104
	(0.042)	(0.053)	(0.068)
ln(Wage Index)	0.391***	0.431***	0.285**
	(0.059)	(0.091)	(0.117)
R-squared	0.935	0.932	0.799
Observations	29,374	11,459	5,618
Hospitals	2,067	786	786
Acquirer	278	278	278
Target	115	115	115
Control	1,674	393	393

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors are clustered by hospital and observations are weighted by adjusted discharges. All specifications include hospital fixed effects. All specifications except the differenced specification include year fixed effects. For the differenced specification, all covariates are measured as the difference between each target/acquirer and its matched control.

hospitals, on the other hand, remain close to zero and are statistically insignificant.

On balance, these results are suggestive that cost savings are limited to target hospitals, a result that is consistent with broader studies of merger effects across industries (Andrade et al. (2001)). That said, the subsample of hospitals analyzed in this section is not entirely representative of the full universe of hospital M&A, so I view this finding more tentatively

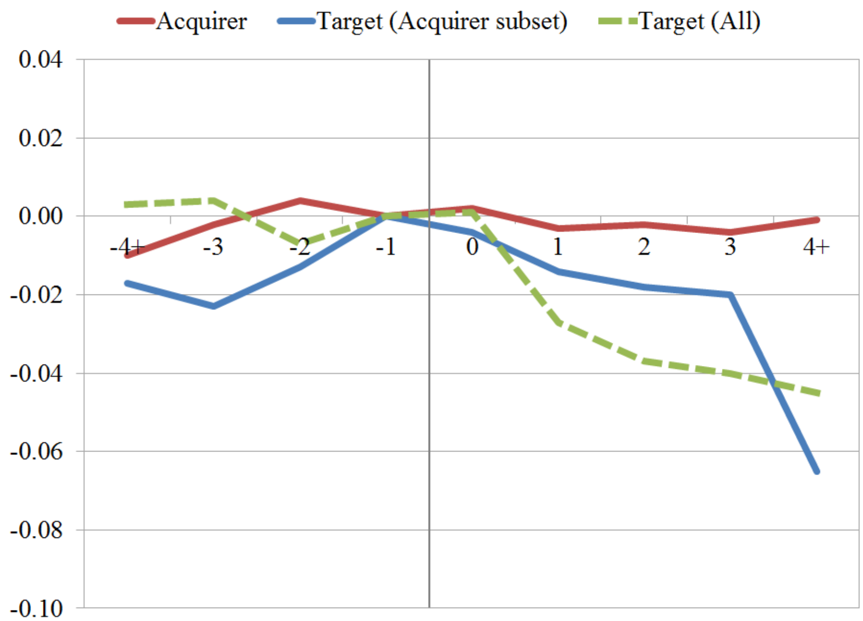

Figure 5: Merger Effects for Acquirer Hospitals Zero corresponds to $t = \tau_h$, the year of the merger. The year before the merger, $t = \tau_h - 1$, is the omitted category. All effects are estimated using the all control group. The solid lines are from Table A3 and the dashed line is from Table A2.

than the main results for target hospitals. One major observable difference between the two samples is that the subsample analyzed in this section tends to exclude acquisitions by large hospital systems, since these systems are more frequently involved in hospital M&A (which complicates construction of the timing variables for acquirer hospitals). In the next section, I directly examine whether acquisitions by multihospital systems appear to be associated with differential cost savings.

5.2 Acquirer Size and Geographic Overlap

One of the primary motivations for re-examining whether hospital mergers reduce costs is that most current M&A involves multihospital systems, whereas the consolidation examined by prior academic research was typically between two independent hospitals. Rather than estimating a single post-merger effect for target hospitals (as with the main results), here I estimate different effects based on the number of hospitals owned by the acquirer in the year of the acquisition: 1 hospital (independent acquirers), 2-10 hospitals, 11-50 hospitals, and 51 or more hospitals.

Table 7 gives the results. For acquisitions by multihospital systems of all three size categories, the estimated post-merger effect is negative and statistically significant. The

Table 7: Results by Acquirer Size

	Independent (1 hospital)	System (2-10)	System (11-50)	System (51+)
Post Merger	-0.018	-0.034**	-0.035**	-0.075***
	(0.024)	(0.015)	(0.015)	(0.022)
Year of Merger	-0.017	0.010	-0.001	-0.005
	(0.024)	(0.013)	(0.013)	(0.026)
$^{+}$p-value of test of H_0	–	0.566	0.535	0.075*
Target Hospitals	56	158	138	84

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors are clustered by hospital and observations are weighted by adjusted discharges. The table reports results from a single regression estimated using the all control group, and including hospital fixed effects, year fixed effects, and all covariates (i.e., For-Profit, ln(CMI), ln(Beds), % Medicare, % Medicaid, and ln(Wage Index)). $^{+}$p-value of a test of the null hypothesis that the post-merger effect is the same as the effect for independent acquirers.

estimated effect for independent acquirers is also negative, but smaller in magnitude and statistically insignificant. A difference in the ability of systems to achieve cost savings relative to independent acquirers is consistent with the discrepancy between my findings of meaningful cost savings from mergers and prior work like Dranove and Lindrooth (2003) that primarily examines mergers between independent hospitals (and found little systematic evidence of cost reductions). That said, while the effect for acquirers owning 51 or more hospitals can be statistically distinguished from the effect for independent acquirers (at the 10% level), the effects for the smaller systems cannot.

Selection: In-Market vs. Out-of-Market Mergers

The result that hospitals acquired by multihospital systems experience statistically significant cost reductions while hospitals acquired by independent hospitals do not is consistent with several possible explanations. One possibility is that multihospital systems are not inherently better at achieving cost savings, but that acquisitions by multihospital systems are less likely to be driven by alternative rationales for acquisition, such as increasing market power. A descriptive statistic that suggests this possibility is that, for independent acquirers, 55 percent of in-sample target hospitals are located in the same county as the acquirer. For multihospital systems, on the other hand, only 23 percent of in-sample target hospitals are located in the same county as even one of the acquirer's hospitals.

To examine this possibility, I estimate separate post-merger effects for in-market and out-of-market acquisitions, along with allowing the post-merger effects to vary by the size

Table 8: In-Market vs. Out-of-Market Acquisitions and Acquirer Size

	Market Definition				
Acquirer Size	HSA	County	Commuting Zone	HRR	State
In-Market					
Independent (1 hospital)	0.016	0.019	-0.014	0.004	-0.011
	(0.043)	(0.032)	(0.028)	(0.029)	(0.025)
System (2-10)	-0.047	-0.012	-0.038**	-0.015	-0.028*
	(0.033)	(0.026)	(0.017)	(0.017)	(0.015)
System (11-50)	0.067	0.025	0.021	0.014	-0.009
	(0.070)	(0.037)	(0.025)	(0.024)	(0.019)
System (51+)	-0.127	0.058	-0.048	-0.095***	-0.083***
	(0.081)	(0.062)	(0.042)	(0.035)	(0.026)
Out-of-Market					
Independent (1 hospital)	-0.033	-0.066**	-0.035	-0.062	-0.157***
	(0.029)	(0.034)	(0.043)	(0.040)	(0.025)
System (2-10)	-0.033**	-0.043**	-0.027	-0.059**	-0.073
	(0.016)	(0.018)	(0.027)	(0.025)	(0.048)
System (11-50)	-0.047***	-0.051***	-0.071***	-0.067***	-0.080***
	(0.014)	(0.016)	(0.016)	(0.017)	(0.020)
System (51+)	-0.072***	-0.095***	-0.090***	-0.066***	-0.057**
	(0.022)	(0.021)	(0.024)	(0.025)	(0.029)
R-squared	0.932	0.932	0.932	0.932	0.932
Observations	29,972	29,972	29,972	29,972	29,972
Hospitals	2,110	2,110	2,110	2,110	2,110
Target (In-Market)	60	118	231	231	352
Target (Out-of-Market)	376	318	205	205	84
Control	1,674	1,674	1,674	1,674	1,674

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors are clustered by hospital and observations are weighted by adjusted discharges. All specifications use the all control group and include hospital fixed effects, year fixed effects, and all covariates (i.e., For-Profit, ln(CMI), ln(Beds), % Medicare, % Medicaid, and ln(Wage Index)).

of the acquiring system. The hypothesis is that in-market acquisitions are more likely to be driven by market power motives, and thus less likely to generate cost savings. Table 8 reports the estimated post-merger effects, using five different market definitions: hospital service area (HSA), county, commuting zone,[28] hospital referral region (HRR), and state.

The results are largely consistent with the stated hypothesis. Every point estimate for out-of-market acquisitions is negative, and most are statistically significant. The point es-

[28]Developed by Tolbert and Sizer (1996), commuting zones are groups of counties constructed to reflect an area's local economy.

timates for in-market acquisitions, on the other hand, are often positive and most are statistically insignificant. That said, testing for equality of the in-market and out-of-market effects, I am only able to reject equality in 7 of 20 cases (4 acquirer sizes times 5 market definitions). Failure to detect statistically significant differences is consistent with a lack of power due to estimating separate effects for each acquirer size; combining acquirer sizes and estimating pooled in-market and out-of-market effects, I am able to reject equality at the 5% level for 4 of the 5 market definitions. These pooled results are reported in Table A4 in the appendix. For the pooled results, the only market definition for which the estimated in-market post-merger effect is statistically significant is the *state*.

After controlling for in-market vs. out-of-market acquisitions, the difference in effects by acquirer size also shrinks, and in some cases the estimated cost savings for independent acquirers from out-of-market acquisitions is larger than for system acquirers. For in-market acquisitions, there is some evidence that systems have been able to achieve cost savings that independent acquirers have not; for instance, systems with at least 51 hospitals achieve large and statistically significant cost savings for acquisitions with HRR and/or state overlap, and these effects are statistically distinguishable from the effects for independent acquirers.

The primary takeaway from these results is that caution must be taken when applying the main results from section 4 to antitrust questions. While those results indicate average cost savings of 4 to 7 percent post-merger, this average is over a broad sample of acquisitions that includes both in-market and out-of-market acquisitions. From an antitrust perspective, out-of-market acquisitions – for which cost savings appear to be more prominent – are likely not as relevant as in-market acquisitions.[29]

6 Robustness

In this section, I report results from a variety of additional analyses that seek to evaluate the robustness of the main results.

6.1 Acquisitions of Multihospital Systems

While cost trends at target hospitals do not diverge from cost trends at control hospitals prior to the merger, it could be that acquirers are able to select acquisition targets based on expected future cost trends, strategically acquiring hospitals that may have been likely to

[29] That said, out-of-market mergers have recently come under increased antitrust scrutiny (see footnote 3).

Table 9: Acquisitions of Multihospital Systems

Target Restriction	Post Merger	Target Hospitals	Transactions
None (Main result: Table 4)	-0.039*** (0.010)	436	337
System (2+ hospitals)	-0.054*** (0.015)	139	40
System (4+)	-0.054*** (0.020)	75	12
System (4+, excluding largest)	-0.050** (0.021)	63	12

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors are clustered by hospital and observations are weighted by adjusted discharges. Each row is a separate regression. All specifications use the all control group and include hospital fixed effects, year fixed effects, and all covariates (i.e., For-Profit, ln(CMI), ln(Beds), % Medicare, % Medicaid, and ln(Wage Index)).

experience cost reductions even absent the merger. One idea to limit the potential influence of this type of strategic acquisition is to restrict the sample to acquisitions of multihospital systems. The basic idea is that such acquisitions may be generated by system-wide considerations rather than expected cost trends at any individual hospital.

Table 9 reports estimated post-merger coefficients after restricting the sample of target hospitals to be those that were acquired in a multihospital deal. The top row of the table reports the estimate from the full sample, the second row restricts the sample to multihospital system acquisitions, and the third row further restricts the sample to target systems with at least four hospitals. The fourth row excludes the largest target hospital (in terms of beds) in each transaction as well, with the argument that the acquisition may have been driven by the flagship hospital of the target system. For all three specifications, the estimated post-merger coefficient remains statistically significant and similar in magnitude to the main results.

6.2 Alternative Matching Specifications

As explained in section 3.3, for the main results I do not match target hospitals to controls on the basis of for-profit status, as doing so requires dropping more than 50 target hospitals from the analysis. The results from a specification requiring that the target hospital and its matched control have the same for-profit status (in addition to the other matching criteria described in section 3.3) are reported in Table 10. The table also gives the results from a specification in which I match target hospitals to controls solely on the basis of their

Table 10: Alternative Matching Specifications

	Matching Specification		
	Main result: Table 4	For-Profit Exact	Propensity Score
Post Merger	-0.050***	-0.044***	-0.056***
	(0.010)	(0.012)	(0.011)
R-squared	0.904	0.909	0.910
Observations	12,708	10,851	12,225
Hospitals	872	746	872
Target	436	373	436
Control	436	373	436

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors are clustered by hospital and observations are weighted by adjusted discharges. All specifications include hospital fixed effects, year fixed effects, and all covariates (i.e., For-Profit, ln(CMI), ln(Beds), % Medicare, % Medicaid, and ln(Wage Index)).

estimated propensity scores, which I construct by estimating a probit model of acquisition as a function of base year covariates: cost per adjusted discharge, CMI, beds, % Medicare, % Medicaid, wage index, inpatient discharges, the ratio of outpatient charges to inpatient charges, metro status, and for-profit status. For both specifications, the estimated impact of merger remains negative, statistically significant, and of a similar magnitude.

6.3 Total Cost Function

An alternative approach to evaluate the effect of mergers is to estimate a total cost function in which total cost is the dependent variable and hospital outputs – inpatient and outpatient discharges – appear on the right hand side of the regression equation.[30] Compared to the specifications estimated above, the primary benefit of such a specification is that it allows inpatient and outpatient discharges to more flexibly impact costs (e.g., allowing increasing or decreasing returns to scale), with the downside of needing to accurately estimate the relevant parameters (and with hospital fixed effects, estimating these parameters using within-hospital variation).

Table 11 reports results from specifications with (log) total cost as the dependent variable and with quadratic functions of (log) inpatient and outpatient discharges on the right

[30] A count of outpatient discharges is not reported in the HCRIS data. I therefore construct an estimate of (inpatient-equivalent) outpatient discharges as inpatient discharges times (outpatient charges/inpatient charges).

Table 11: Total Cost Function

	Control Group			
	All	All	Matched	Matched
Post Merger	-0.053***	-0.322	-0.049***	0.885
	(0.010)	(1.006)	(0.010)	(1.237)
Scale Economy	1.66***	1.68***	1.54***	1.58***
	(0.07)	(0.07)	(0.09)	(0.1)
Scale Economy Post		1.54***		1.48***
		(0.06)		(0.07)
Scope Economy	0.31***	0.32***	0.27***	0.27***
	(0.02)	(0.02)	(0.02)	(0.02)
Scope Economy Post		0.28***		0.25***
		(0.02)		(0.02)
R-squared	0.991	0.991	0.985	0.985
Observations	29,972	29,972	12,708	12,708
Hospitals	2,110	2,110	872	872
Target	436	436	436	436
Control	1,674	1,674	436	436

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variable is ln(Total Cost) and a quadratic function of (log) inpatient and outpatient discharges is included on the right hand side. Standard errors are clustered by hospital and observations are weighted by adjusted discharges. All specifications include hospital fixed effects, year fixed effects, and all other covariates (i.e., For-Profit, ln(CMI), ln(Beds), % Medicare, % Medicaid, and ln(Wage Index)). "Scale Economy" refers to the measure described in the text and in footnote 31; the reported significance levels are from a test of the null hypothesis that the measure is equal to 1 (constant returns to scale). "Scope Economy" refers to the measure described in the text and in footnote 32; the reported significance levels are from a test of the null hypothesis that the measure is equal to 0 (no economies of scope). Both scale and scope economies are evaluated at the mean levels of inpatient and outpatient discharges across hospitals (8,678 and 6,590, respectively). Standard errors for the scale and scope economy measures are calculated using the delta method.

hand side, using both the all and matched control groups. In the first and third columns, mergers are permitted to only affect the intercept of the cost function; in the second and fourth columns, I fully interact the post-merger indicator with the quadratic function of (log) inpatient and outpatient discharges. To evaluate the presence of economies of scale, I calculate the implied ray scale economy from the regression results; this measure – for which a value of 1 indicates constant returns to scale, with higher values indicating economies of scale and lower values indicating diseconomies of scale – captures the change in total cost that results from a proportional change in outputs.[31] To evaluate the presence of economies

[31]This measure is given by one divided by the sum of the cost elasticities: Scale Economy $= 1/(\frac{\partial \ln \mathrm{C}}{\partial \ln \mathrm{IP}} +$

of scope, I calculate the percentage change in costs that would result from producing the same output at two semi-specialized hospitals (one specializing in inpatient services and the other specializing in outpatient services) as compared to a single hospital in which the services are combined.[32] These measures are adopted from prior work estimating hospital cost functions (e.g., Vita (1990); Carey (1997); Preyra and Pink (2006); Gaynor et al. (2015)).

Beginning with scale economies, the results indicate the presence of increasing returns to scale. In the first column of results, for instance, the estimate indicates that a 1 percent increase in inpatient and outpatient discharges would increase costs by 1/1.66=0.6 percent. Comparing this result to the literature, it is most similar to Carey (1997), who estimates ray scale economies in the 1.4 to 1.5 range. The results also indicate the presence of economies of scope. Again considering the first column of results, the estimate indicates that production in two semi-specialized hospitals would increase costs by 31 percent compared to the same total output being produced at one hospital. While differences in the measured outputs make direct comparisons with the literature difficult (e.g., Gaynor et al. (2015) examine a much richer set of outputs than inpatient and outpatient discharges), the results in Preyra and Pink (2006) also support the presence of economies of scope. Gaynor et al. (2015) also find some evidence of across-output economies of scope, though the estimates are not always statistically significant.

Turning to the merger effect estimates, the results in the first and third columns indicate cost reductions of around 5 percent at target hospitals post-merger. This estimate is statistically significant and similar in magnitude to the main results. When also allowing scale and scope economies to change post-merger, the estimate of the effect of merger on the intercept of the cost function becomes extremely unstable, indicating implausibly large effects of varying sign (and with a large standard error). The results also indicate a reduction in scale and scope economies post-merger, though it is difficult to interpret these results strongly given the clear instability of the specification. Since the specification includes hospital fixed effects, this instability is arguably somewhat unsurprising; the fully interacted specification requires estimating an intercept shift and five parameters governing the change in the curvature of the cost function, all based on post-merger, within-hospital variation in output. Overall, I view these results as indicating that (a) the main result of 4 to 7 percent cost savings is robust to a natural alternative modeling assumption, but that (b) the data

$\frac{\partial \ln \text{C}}{\partial \ln \text{OP}}$), where C is total cost and IP and OP are inpatient and outpatient discharges.

[32]This measure is given by: $\frac{\text{C(IP-}\mu_\text{I},\mu_\text{O}) + \text{C}(\mu_\text{I},\text{OP-}\mu_\text{O})}{\text{C(IP,OP)}} - 1$, where C is total cost, IP and OP are inpatient and outpatient discharges, and μ_I and μ_O are parameters. I set μ_I and μ_O equal to the 25th percentile of inpatient and outpatient discharges across hospitals (2,713 and 3,045, respectively).

is not rich enough to precisely measure the impact of mergers on more detailed features of hospital costs like economies of scale and scope.

7 Conclusion

Analyzing a large sample of hospital mergers between 2000 and 2010, I find that hospitals that were acquired ("target hospitals") realized cost savings of 4 to 7 percent in the years following the merger (on average). A key challenge in interpreting these findings is understanding to what extent alternative explanations besides cost efficiencies could be driving the results. While on balance the evidence seems to suggest that the observed cost reductions are not easily explained by other mechanisms – e.g., cost trends for target and control hospitals move in parallel pre-merger, the results are robust to various matching specifications and the inclusion of granular geographic controls, and prominent measures of service and patient mix do not change post-merger – mergers are of course not randomly assigned, and the results should be interpreted with that caveat in mind. In addition, the costs examined here refer to costs incurred by the hospital. It is also important to understand to what extent these cost reductions have been passed through to the buyers of hospital services. That question is beyond the scope of this paper, though the results here are a meaningful first step; if there are no cost savings in the first place, the question of pass through is far less relevant.

Besides target hospitals, I also examine whether acquiring hospitals/systems realize cost savings in the years following acquisition. I find that they do not, though the sample on which this result is based is somewhat unrepresentative of the full universe of hospitals involved in hospital M&A (as discussed in section 5.1). Last, while the hospital consolidation studied in prior research was primarily between independent hospitals, current hospital M&A is driven by multihospital systems. I find that mergers between independent hospitals yield small and statistically insignificant effects on costs, while acquisitions by multihospital systems generate larger and statistically significant reductions. However, much of this difference appears to be attributable to selection. In particular, mergers between independent hospitals are more likely to be in-market, and hence arguably more likely to be driven by market power (rather than cost saving) motives. Estimating separate effects for in-market and out-of-market acquisitions, I find evidence consistent with this hypothesis: out-of-market acquisitions generate larger cost savings (on average) and the difference in effects between independent hospitals and multihospital systems shrinks. In fact, the only market definition I examine for which the estimate of post-merger cost savings from in-market M&A is statistically significant is the *state*. Estimated post-merger cost savings from out-of-market M&A, on the other hand, are statistically significant for all five examined market definitions (HSA, county, commuting zone, HRR, and state).

Therefore, while the main result of 4 to 7 percent cost savings suggests that recent hospital consolidation may truly be delivering on claims of systematic cost savings, it appears that those cost savings may not be as prevalent for mergers in which the acquiring system is nearby – which are exactly the transactions for which antitrust concerns are likely to be the strongest. The main results should thus not be interpreted as direct evidence for systematic cost efficiencies in hospital merger cases; on balance, there is not much evidence that in-market mergers – on average – have delivered substantial cost savings. In addition, the results are silent with respect to the question of merger specificity; i.e., while target hospitals on average experience cost reductions post-merger, this result does not imply that a merger was necessary to achieve those cost reductions. For instance, improved management practices may be feasible even absent a change in ownership.

Beyond antitrust, given 2014 aggregate U.S. hospital operating margins of 6.4 percent,[33] the observed 4 to 7 percent cost reductions at target hospitals may be generating substantial economic surplus. That said, precisely identifying the sources of these cost savings, if and when mergers are necessary to achieve them, and the circumstances under which cost savings are likely to be passed through remain important (and largely open) policy questions.

[33] AHA Trendwatch Chartbook 2016, Trends in Hospital Financing, Table 4.1.

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8 Appendix

8.1 Additional Tables and Figures

Table A1: HCRIS Cost Centers

Cost Center	% of Total	Cost Center	% of Total
General Service Cost Centers	***44.5%***	***Ancillary Service Cost Centers***	***29.5%***
Capital Related Costs, Buildings and Fixtures	3.7%	Operating Room	5.2%
Capital Related Costs, Movable Equipment	3.4%	Recovery Room	0.5%
Employee Benefits	8.2%	Delivery Room and Labor Room	1.2%
Administrative and General	14.2%	Anesthesiology	0.3%
Maintenance and Repairs	1.0%	Radiology - Diagnostic	3.3%
Operation of Plant	2.5%	Radiology - Therapeutic	0.4%
Laundry and Linen Service	0.4%	Radioisotope	0.3%
Housekeeping	1.3%	Laboratory	3.8%
Dietary	1.1%	PBP Clinical Laboratory Services	0.0%
Cafeteria	0.2%	Whole Blood & Packed Red Blood Cells	0.2%
Maintenance of Personnel	0.0%	Blood Storing & Processing	0.4%
Nursing Administration	1.2%	Intravenous Therapy	0.1%
Central Services & Supply	1.0%	Respiratory Therapy	1.3%
Pharmacy	2.0%	Physical Therapy	1.0%
Medical Records & Medical Records Library	1.2%	Occupational Therapy	0.2%
Social Service	0.3%	Speech Pathology	0.1%
Nursing School	0.1%	Electrocardiology	0.7%
Intern & Resident Costs (Approved)	1.8%	Electroencephalography	0.1%
Other General Service Costs	0.9%	Medical Supplies Charged to Patients	5.6%
		Drugs Charged to Patients	3.9%
Inpatient Routine Service Cost Centers	***15.0%***	Renal Dialysis	0.3%
Adults and Pediatrics (General Routine Care)	8.6%	ASC	0.3%
Intensive Care Unit	2.1%	Other Ancillary Service Costs	0.3%
Coronary Care Unit	0.4%		
Burn Intensive Care Unit	0.1%	***Other Reimbursable Cost Centers***	***2.6%***
Surgical Intensive Care Unit	0.2%	Home Program Dialysis	0.0%
Nursery	0.6%	Ambulance Services	0.3%
Other Inpatient Service Costs	3.0%	Durable Medical Equipment	0.0%
		Intern & Resident Costs (Not Approved)	0.0%
Outpatient Service Cost Centers	***4.8%***	Other Reimbursable Costs	2.3%
Clinic	1.8%		
Emergency	2.7%	***Non-Reimbursable Cost Centers***	***3.1%***
Observation Beds	0.0%	Gift, Flower, Coffee Shop, & Canteen	0.3%
Other Outpatient Service Costs	0.3%	Research	0.7%
		Physicians' Private Offices	1.3%
Special Purpose Cost Centers	***0.4%***	Nonpaid Workers	0.3%
Organ Acquisition	0.2%	Other Non-Reimbursable Costs	0.5%
Hospice	0.2%		
Other Special Purpose Costs	0.0%		

Notes: Cost centers are sorted within category according to how they appear on the cost reports. Percentages are calculated after summing costs over all general acute care hospitals and years (1998-2012).

Table A2: Main Results: Leads and Lags

	Control Group		
	All	Matched	Differenced
$t \leq \tau_h - 4$	0.003	0.013	0.022
	(0.012)	(0.012)	(0.015)
$t = \tau_h - 3$	0.004	0.007	0.007
	(0.009)	(0.009)	(0.012)
$t = \tau_h - 2$	-0.007	-0.006	-0.013
	(0.006)	(0.006)	(0.009)
$t = \tau_h - 1$	0	0	0
	–	–	–
$t = \tau_h$	0.001	-0.002	0.002
	(0.008)	(0.008)	(0.010)
$t = \tau_h + 1$	-0.027***	-0.030***	-0.048***
	(0.009)	(0.009)	(0.012)
$t = \tau_h + 2$	-0.037***	-0.042***	-0.062***
	(0.011)	(0.011)	(0.014)
$t = \tau_h + 3$	-0.040***	-0.047***	-0.059***
	(0.011)	(0.012)	(0.014)
$t \geq \tau_h + 4$	-0.045***	-0.061***	-0.078***
	(0.012)	(0.014)	(0.016)
For-Profit	-0.040***	-0.041***	-0.028
	(0.014)	(0.015)	(0.018)
ln(CMI)	0.386***	0.440***	0.282***
	(0.052)	(0.067)	(0.081)
ln(Beds)	0.002	0.022	0.002
	(0.015)	(0.021)	(0.026)
% Medicare	0.948***	0.988***	1.129***
	(0.058)	(0.064)	(0.077)
% Medicaid	0.050	-0.009	0.013
	(0.042)	(0.040)	(0.061)
ln(Wage Index)	0.361***	0.250**	0.135
	(0.063)	(0.099)	(0.121)
R-squared	0.931	0.904	0.749
Observations	29,972	12,708	6,216
Hospitals	2,110	872	872
Target	436	436	436
Control	1,674	436	436

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors are clustered by hospital and observations are weighted by adjusted discharges. All specifications include hospital fixed effects; the year before the merger ($t = \tau_h - 1$) is the omitted category. All specifications except the differenced specification include year fixed effects. For the differenced specification, all covariates are measured as the difference between each target and its matched control.

Table A3: Acquirer Hospitals: Leads and Lags

	Control Group					
	All		Matched		Differenced	
	Acquirer	Target	Acquirer	Target	Acquirer	Target
$t \leq \tau_h - 4$	-0.010	-0.017	-0.002	-0.012	0.005	0.038
	(0.012)	(0.030)	(0.012)	(0.030)	(0.015)	(0.039)
$t = \tau_h - 3$	-0.002	-0.023	-0.001	-0.024	0.001	0.000
	(0.009)	(0.018)	(0.009)	(0.019)	(0.013)	(0.032)
$t = \tau_h - 2$	0.004	-0.013	0.005	-0.014	0.002	0.001
	(0.005)	(0.016)	(0.005)	(0.016)	(0.009)	(0.022)
$t = \tau_h - 1$	0	0	0	0	0	0
	–	–	–	–	–	–
$t = \tau_h$	0.002	-0.004	0.000	-0.008	0.000	0.001
	(0.006)	(0.016)	(0.006)	(0.017)	(0.007)	(0.022)
$t = \tau_h + 1$	-0.003	-0.014	-0.006	-0.018	-0.007	-0.039
	(0.007)	(0.021)	(0.008)	(0.021)	(0.009)	(0.028)
$t = \tau_h + 2$	-0.002	-0.018	-0.006	-0.023	-0.014	-0.039
	(0.009)	(0.023)	(0.009)	(0.024)	(0.013)	(0.032)
$t = \tau_h + 3$	-0.004	-0.020	-0.009	-0.027	-0.027*	-0.041
	(0.011)	(0.025)	(0.011)	(0.026)	(0.016)	(0.030)
$t \geq \tau_h + 4$	-0.001	-0.065**	-0.010	-0.073***	-0.005	-0.076**
	(0.013)	(0.027)	(0.014)	(0.028)	(0.018)	(0.035)
$^+$p-value test of H_0	0.029**		0.032**		0.072*	
R-squared	0.935		0.932		0.800	
Observations	29,374		11,459		5,618	
Hospitals	2,067		786		786	
Acquirer	278		278		278	
Target	115		115		115	
Control	1,674		393		393	

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors are clustered by hospital and observations are weighted by adjusted discharges. Each group of two columns contains the results from a single regression. All specifications include hospital fixed effects; the year before the merger ($t = \tau_h - 1$) is the omitted category. All specifications except the differenced specification include year fixed effects. All specifications include all covariates (i.e., For-Profit, ln(CMI), ln(Beds), % Medicare, % Medicaid, and ln(Wage Index)). For the differenced specification, covariates are measured as the difference between each target/acquirer and its matched control. $^+$p-value of a test of the null hypothesis that the $t \geq \tau_h + 4$ effect is the same for acquirers and targets.

Table A4: In-Market vs. Out-of-Market Acquisitions

	Market Definition				
Acquirer Size	HSA	County	Commuting Zone	HRR	State
Post Merger (In-Market)	-0.008	0.011	-0.020	-0.016	-0.030***
	(0.028)	(0.017)	(0.013)	(0.013)	(0.011)
Post Merger (Out-of-Market)	-0.043***	-0.057***	-0.059***	-0.061***	-0.073***
	(0.010)	(0.011)	(0.013)	(0.013)	(0.017)
+p-value test of H_0	0.248	0.001***	0.026**	0.009***	0.023**
R-squared	0.931	0.932	0.931	0.931	0.931
Observations	29,972	29,972	29,972	29,972	29,972
Hospitals	2,110	2,110	2,110	2,110	2,110
Target (In-Market)	60	118	231	231	352
Target (Out-of-Market)	376	318	205	205	84
Control	1,674	1,674	1,674	1,674	1,674

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors are clustered by hospital and observations are weighted by adjusted discharges. All specifications use the all control group and include hospital fixed effects, year fixed effects, and all covariates (i.e., For-Profit, ln(CMI), ln(Beds), % Medicare, % Medicaid, and ln(Wage Index)). +p-value of a test of the null hypothesis that the post-merger effect is the same for in-market and out-of-market acquisitions.

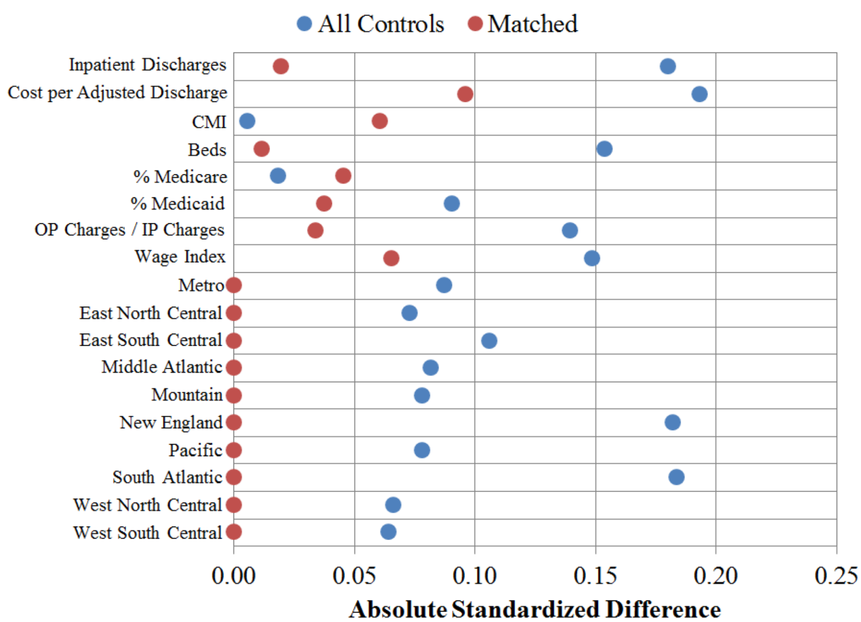

Figure A1: Covariate Balance Before and After Matching

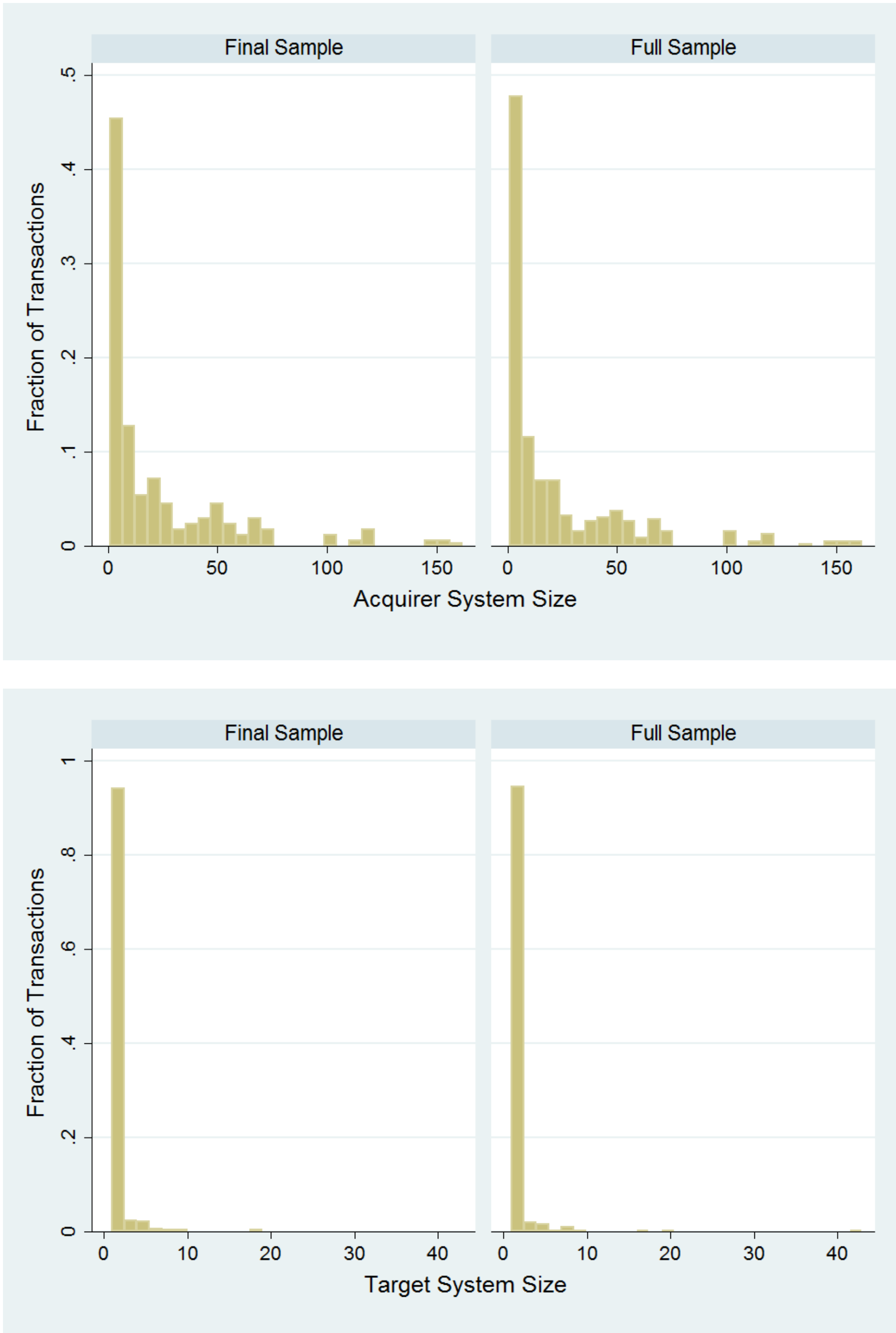

Figure A2: Acquirer and Target Sizes in the Full and Final Samples The distribution of acquirer sizes across transactions is plotted in the top panel. The distribution of target sizes is plotted in the bottom panel. For both acquirers and targets, the distributions are similar in the full and final samples: two-sample Kolmogorov-Smirnov tests fail to reject the null hypothesis that the full and final samples have the same acquirer and target size distributions.

8.2 Additional Control Variables

While the main results in section 4 control for several predominant hospital characteristics (e.g., CMI), it is also worth examining whether the results are sensitive to the inclusion of additional controls. Specifically, I constructed the following additional control variables:

- AHA Hospital Characteristics: an indicator for whether the hospital has an emergency department, an indicator for whether the hospital has a certified trauma center, the percentage of hospital beds that are classified as intensive care, the percentage of full-time equivalents that are medical residents (a measure of teaching intensity), a count of technology offerings,[34] and a count of accreditations.[35]
- Area Demographics: the percentage of the county population that is age 45 to 64, the percentage of the county population that is age 65 or older, the percentage of the county population that is hispanic, and the percentage of the county population that is black.[36]
- Area Income: the percentage of the county population in poverty and the county median household income.[37]
- Hospital Quality: a quality score constructed from CMS' Hospital Compare data, which is only available from 2005 and on. After standardizing each hospital's score on each available process of care measure (by year), I take the simple average across measures by hospital and year.

Table A5 reports post-merger effect estimates from specifications including these additional controls. Adding the AHA hospital characteristics and area demographic and income controls has a negligible effect on the estimated post-merger cost savings. In addition, the only variable that is a statistically significant predictor of costs is the percentage of the population age 65 or older, which unsurprisingly is estimated to increase hospital costs.

Turning to quality, any changes in quality post-merger that also affect costs will potentially confound the estimates. Since quality data from Hospital Compare is only available for years 2005 and later, however, I decided not to control for available quality measures

[34]CT, MRI, PET, SPECT, and diagnostic radioisotope capability.

[35]Accreditation by the Joint Commission, a cancer program approved by the American College of Surgeons, residency training approved by the Accreditation Council for Graduate Medical Education, and residency training approved by the American Osteopathic Association.

[36]I extracted these variables from the CDC Wonder database, but the underlying data is from the U.S. Census Bureau.

[37]These estimates are taken from the U.S. Census Bureau's Small Area Income and Poverty Estimates (SAIPE) data.

Table A5: Adding Control Variables

	Post Merger	R-squared	+F-test p-value
Main result: Table 4	-0.039***	0.931	–
	(0.010)		
Extra hospital controls	-0.038***	0.931	0.560
	(0.010)		
Extra area controls	-0.039***	0.932	0.182
	(0.010)		
Extra hospital & area Controls	-0.039***	0.932	0.289
	(0.010)		
Main result (2005+)	-0.030**	0.951	–
	(0.013)		
Add quality score	-0.028**	0.951	0.323
	(0.013)		

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors are clustered by hospital and observations are weighted by adjusted discharges. Each row is a separate regression. All specifications use the all control group and include hospital fixed effects, year fixed effects, and all other covariates (i.e., For-Profit, ln(CMI), ln(Beds), % Medicare, % Medicaid, and ln(Wage Index)). "Hospital controls" refers to: an emergency department indicator, a trauma center indicator, the percentage of hospital beds that are intensive care, the percentage of full-time equivalents that are medical residents, a count of technology offerings, and a count of accreditations. "Area controls" refers to: the percentage of county residents aged 45 to 64, the percentage of county residents aged 65 or older, the percentage of county residents that are hispanic, the percentage of county residents that are black, the percentage of county residents in poverty, and the county median household income. $^{+}$p-value of an F-test for the joint significance of the added control variables.

in the main results. The bottom panel of Table A5 reports results from equation (3) but restricting the data to the years 2005-2012 (row "Main result (2005+)"), and then adding the quality score variable (row "Add quality score"). The results continue to indicate statistically significant cost savings post-merger, and adding the quality score variable has only a small impact on the estimated post-merger effect.[38]

[38] In fact, estimating a specification with the quality score on the left hand side, target hospitals are estimated to have *higher* quality post-merger. This result runs contrary to several other studies (Ho and Hamilton (2000), Capps (2005), and Cuellar and Gertler (2005)) that find no systematic evidence of quality improvement or deterioration as a result of mergers. While closer examination of the link between mergers and quality is beyond the scope of this paper, this result suggests that skimping on care is unlikely to be the cause of the observed cost reductions at target hospitals.

8.3 Obstacles in Measuring Hospital Ownership

There are challenges in identifying hospital ownership beyond those posed by hospital M&A. Consider Figure A3, which is a partial organizational chart of Marian Health System in 2010, a multihospital health system that was acquired by Ascension Health in 2013.[39] As shown in the chart, Marian was the parent of three regional health systems: Ministry Health Care, Via Christi Health, and St. John Health System. Facility names with asterisks are co-sponsored/joint ventures. Via Christi Health, for example, was a joint venture between Marian and another partner. Within each regional health system, there are further breakdowns between different hospitals and/or groups of hospitals. There are also joint ventures within the regional systems such as Affinity Health System and Mercy Regional Health Center (a joint venture of a joint venture!). In such cases, it is difficult to say who the owner of each hospital really is.

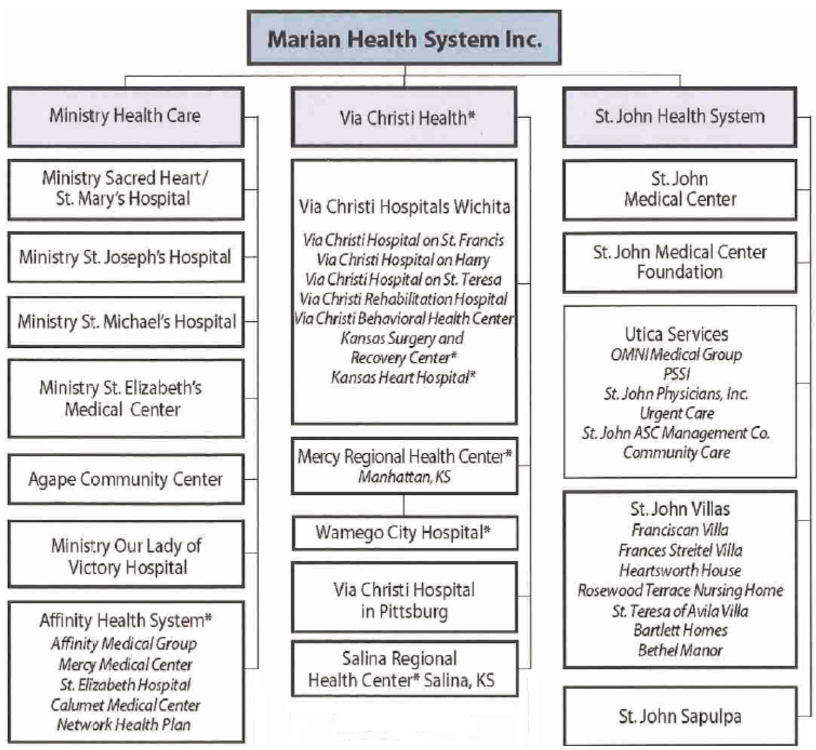

Figure A3: Marian Health System, 2010 (Partial) Organizational Chart

There are challenges beyond cases like Marian Health System as well. Many local government owned hospitals are operated by third parties. In some cases, the operating company is paid a management fee to operate the hospital. In others, the hospital is leased, sometimes

[39] `http://www.sistersofthesorrowfulmother.org/Marian_Org_Chart.pdf`, accessed 1/6/2015.

only for a handful of years and other times for decades. Whether or not the operator of the hospital should be treated as the "owner" of the hospital for the purposes of the analysis presumably depends on the precise nature of the contract, which is typically unobserved. In general, I treat holding a long-term lease as owning a hospital but having an agreement to manage a hospital for a monthly/yearly fee as not owning it (since profits are retained by the original owner, though again this will depend on the specifics of the unobserved contract). I also rely more heavily on the AHA sysid in ambiguous cases, being more willing to count a lease or management agreement as ownership if a hospital's sysid changes in turn. At any rate, the point is merely that hospital ownership is complex, and that the measure I use – while likely more accurate and complete than any individual source alone – is still assuredly imperfect.

8.4 Merger Trend Comparison with Irving Levin

Figure A4 plots acquisitions by year (including CAH acquisitions) according to the final system identification variable I use in the paper (solid line), compared to the number of transactions reported by Irving Levin (dashed line, as in Figure 1).

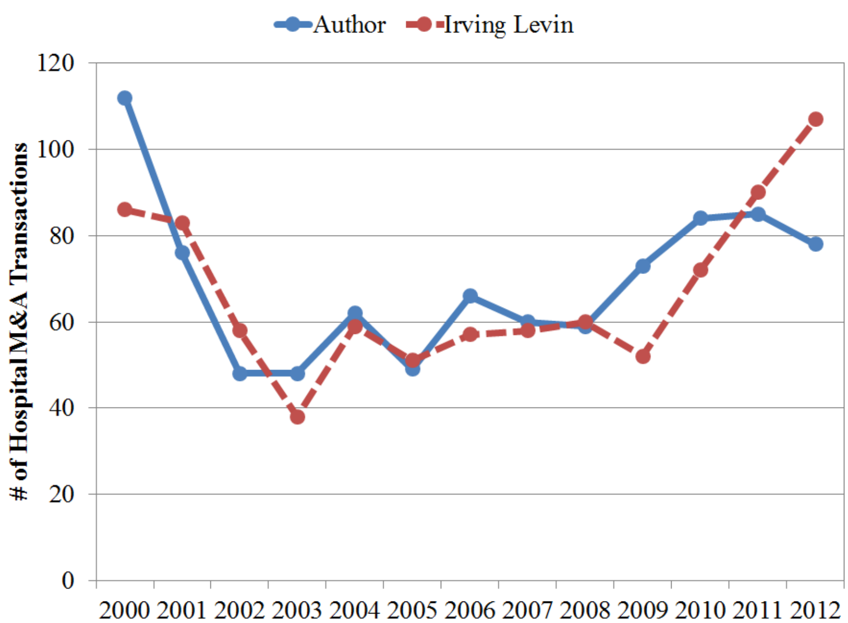

Figure A4: Hospital M&A Trend Comparison, 2000-2012

Overall, the patterns are very similar. In 2012, however, the number of transactions in my data is nearly 30 less than in the Irving Levin data. The primary cause of this discrepancy is that Irving Levin often reports not only deals that are closed, but also deals that are announced. Many of these deals do not close until later years, so they do not count as having occurred in 2012 in my data. Another cause of discrepancies between Irving Levin/AHA and my final data is the coding of acquisition timing. In some cases, a hospital's sysid in the AHA data may change in year X while the transaction was not actually completed until a year later (or vice-versa, with sysid changes in the AHA data lagging the actual transaction). In these cases, I code the acquisition as occurring in the earliest recorded year. The reason for this choice is to push all effects resulting from the transaction into the post period. Otherwise, if an early change in sysid signals real changes that occurred prior to the formal closing of the deal, and these changes are coded as occurring prior to the transaction, it could make interpreting the results of the statistical analysis more difficult (i.e., effects of the transaction may appear prior to the coded time of the transaction). Hospitals not appearing in the AHA data or exiting the data upon the merger (e.g., if the acquired hospital begins reporting under the acquirer's AHA ID) are further examples that can result in discrepancies.

8.5 Matching Details

Drawing on the literature on optimal matching, I frame the goal of finding controls that are as similar as possible to target hospitals as a binary integer programming problem. There are 436 target hospitals and 1,674 controls, so one can imagine a match as a vector with 729,864 (=436*1,674) elements: one element for each possible combination of target and control. The elements corresponding to combinations that are matched together take a value of 1, while the elements corresponding to combinations that are unmatched take a value of 0. The goal is to find the "best" vector that also meets any constraints imposed upon the match. Formally, the matching problem is given by:

$$\min_{\boldsymbol{a}} \sum_{t=1}^{T} \sum_{c=1}^{C} a_{t,c} \cdot d(t,c) \quad s.t. \tag{6}$$

$$a_{t,c} \in \{0,1\} \quad \forall\, t,c \tag{6.1}$$

$$\sum_{c=1}^{C} a_{t,c} = k_1 \quad \forall\, t \tag{6.2}$$

$$\sum_{t=1}^{T} a_{t,c} \leq k_2 \quad \forall\, c \tag{6.3}$$

$$g(\boldsymbol{a}) \in \Omega \tag{6.4}$$

t indexes target hospitals and c indexes control hospitals. $d(t, c)$ is a function that maps any possible target-control pairing to the distance between the two. The objective of (6) is thus to find the pairing of targets and controls that minimizes the sum of the distances between the paired target and control hospitals. The constraints require that matches meet various conditions. Constraint (6.1) imposes that each element of $\boldsymbol{a}$ is either 0 (unmatched) or 1 (matched) (binary integer constraints). Constraint (6.2) imposes that each target should be matched to k_1 controls. Constraint (6.3) imposes that each control should be matched to at most k_2 target hospitals. Constraint (6.4) – that the match meets some criteria beyond those captured by (6.1)-(6.3) – is purposefully vague. It is possible to put many conditions on the match, such as various measures of covariate balance and/or parallel cost trends pre-merger. Propensity scores can also be straightforwardly incorporated into (6), either by including the estimated propensity scores in the distance function or in the constraints.

For $d(t, c)$, I use the Mahalanobis distance in terms of the eight continuous variables in Table 3: inpatient discharges, cost per adjusted discharge, CMI, beds, % Medicare, % Medicaid, the ratio of OP charges to IP charges, and wage index. All values are measured in the initial year that a hospital appears in the data. I set $k_1 = k_2 = 1$, so that each target is matched to a single control and each control is not permitted to be matched to more than one target. For additional constraints, I require that the control hospital shares the same metro status as the target hospital, is in the same Census division, and that the control hospital is present in the data for all years that the target hospital is present.

8.6 Data Cleaning: Outliers and Missing Data

For each hospital-year observation, I winsorize length of stay at the 1% level and $\frac{\text{OP charges}}{\text{IP charges}}$ at the 2% level (by year). I trim (i.e., replace as missing) the top and bottom 2% of all cost per discharge variables by year. The cutoff and choice to winsorize vs. trim was made by manual examination of the data, e.g. whether I judged values in the tails to appear to be data errors, or to represent unlikely within-hospital changes (e.g., if cost per discharge values in the tails were uniformly large departures from average within-hospital costs, then trimming seems more appropriate than winsorizing).

To limit the potential effect of changes in sample composition due to missing data – in particular for the differenced specifications (equations (4) and (5)), which require matching data for target and control hospitals – I use linear interpolation to fill out missing data for all variables that enter the main regressions: costs, discharges, beds, etc. In total, the interpolation affects about 500 observations of the roughly 30,000 total observations in the

Table A6: Data Cleaning Robustness Checks

Count	Description	Post Merger	Count	Description	Post Merger
1	Main results (Table 4, All Controls)	-0.039*** (0.010)	4	Winsorizing/trimming, no interpolation	-0.037*** (0.010)
2	No winsorizing or trimming, no interpolation	-0.031*** (0.011)	5	5% winsorizing, no interpolation	-0.039*** (0.010)
3	No winsorizing or trimming, interpolation	-0.034*** (0.011)	6	5% trimming, no interpolation	-0.044*** (0.010)

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors are clustered by hospital and observations are weighted by adjusted discharges. All specifications use the all control group and include hospital fixed effects, year fixed effects, and all covariates (i.e., For-Profit, ln(CMI), ln(Beds), % Medicare, % Medicaid, and ln(Wage Index)).

full sample (less than 2% of all observations).

To examine the robustness of the results with respect to changes in the handling of outliers and missing data, I estimated a variety of specifications modifying the cutoffs, whether or not to winsorize or trim, eliminating the interpolation, etc. Table A6 reports estimated post-merger coefficients from equation (3) (using the all control group) under several different data cleaning options. For all specifications I have examined, the estimated cost savings remain statistically significant and of a similar magnitude to the main results reported in section 4.