Estimation and Identification of Merger Effects: An Application to Hospital Mergers

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Abstract

Existing empirical estimates of merger effects are compromised by the fact that merging and nonmerging entities differ in unobserved ways that independently affect outcomes of interest. To obtain an unbiased estimate of the effect of consummated mergers, I propose an approach that focuses on the response of rivals to mergers and accounts for the endogeneity of exposure to these mergers. I apply this approach to evaluate the impact of independent hospital mergers in the United States between 1989 and 1996. Using the physical colocation of rivals as an instrument for whether they merge, I find a sizeable, one-time increase in price following a rival's merger, with the greatest increase occurring among hospitals nearest the merging hospitals. These results are more consistent with predictions from structural models of the hospital industry than with prior observational estimates of the effects of hospital mergers.

1. Introduction

In recent years, economists have taken advantage of methodological advances in the estimation of structural demand models to simulate the impact of horizontal mergers. The strengths of this approach are many, not least the ability to predict the impact of future mergers rather than extrapolate from the experience of mergers that have already occurred. However, these models require extensive assumptions about consumer demand and firm objectives, do not fully incorporate rivals' reactions to actions taken by merging parties, and are computationally intensive and challenging to implement. Moreover, the predictions generated by such models can be validated only by analyzing the effects of consummated mergers. To date, the courts have also been more receptive to

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observational methods that provide what they consider to be "hard evidence" of the likely impact of a merger, as in the Staples–Office Depot case (*Federal Trade Commission v. Staples, Inc., and Office Depot, Inc.*, 970 F. Supp. 1066 [1997]).[1]

Unfortunately, most observational or reduced-form analyses of the impact of mergers fail to address fundamental selection problems arising from the fact that mergers are not randomly assigned. These studies typically compare outcomes of merging firms with those of nonmerging firms. The resulting estimates suffer from a classical selection problem, as merging firms are likely different from nonmerging firms in unobserved ways that affect the outcomes of interest. For example, suppose that financially distressed firms are more likely to be party to a merger and post merger the new entities reduce costs and decrease prices. Conditional on survival, these firms might have reduced costs and decreased prices even more absent a merger. More generally, any omitted factor that is correlated with the outcome measure as well as with the probability of a merger will generate biased estimates of the impact of a merger.

Some studies enhance the basic differences-in-differences approach by using matching algorithms to identify a superior control group (for example, Dranove and Lindrooth 2003). Yet another approach, introduced by Eckbo (1983), is to eliminate the merging entities from the analysis entirely and to focus on the responses of rivals to mergers. If merging parties exercise their newly acquired market power by raising price, ceteris paribus their rivals will be able to raise price as well.[2] Thus, rival analysis compares the outcomes of firms with merging rivals to the outcomes of firms without merging rivals. These results are also likely to be biased by selection, however, as firms with merging rivals are likely different from firms without merging rivals.

This paper improves on prior observational studies by combining rival analysis with instrumental variables (IV). I estimate the effect of a rival's merger on a firm's own price, instrumenting for whether a firm is exposed to a rival's merger. Provided that the instrument is correlated with the probability of rival merger and uncorrelated with other unobserved factors affecting a firm's own price, this methodology will generate unbiased estimates of the causal effect of merger on market-level outcomes. I test this approach using data on the general acute-care hospital industry in the United States, a sector that experienced a wave of merger activity during the 1990s.

The instrument I propose for merger in the hospital industry is colocation. Using the exact latitude and longitude coordinates for each hospital's main address in 1988, I identify colocated or adjacent hospitals, defined as hospitals

[1] In its successful attempt to block this merger, the Federal Trade Commission (FTC) presented evidence that office supply prices were lowest in markets where all three office supply superstores (Staples, Office Depot, and Office Max) competed. Prices were higher in markets with two competitors, and higher still in markets with a single office supply superstore.

[2] This argument assumes that prices are strategic complements. Hospitals are typically modeled as differentiated Bertrand competitors, hence the assumption. See Gaynor and Vogt (2003) for an excellent discussion of prior theoretical and empirical work. Rival analysis has also been used to infer the competitive effects of other decisions, such as changes in capital structure (Chevalier 1995).

within .3 miles of each other "as the crow flies" and no more than 5 blocks apart. Using this criterion, 191 (3.6 percent) of the 5,373 general nonfederal hospitals in the nonterritorial United States in 1988 were colocated with at least one other hospital. There are two reasons such hospitals should be more likely to merge: the potential to cut costs through the elimination of duplicate departments is greater, and the ability to increase price is greater because location is a primary differentiating factor for inpatient care (Dranove and White 1994; Tay 2003). This prediction is borne out in the data, which show that colocated hospitals are nearly three times as likely to merge as are noncolocated hospitals, a factor that is scarcely diminished after controlling for a large set of hospital and market characteristics. Thus, rival colocation is an excellent instrument for rival merger. In this study, a rival is defined as another hospital located within a certain distance from the hospital in question, for example, 7 miles.

Using this instrument together with data on hospital mergers occurring between 1989 and 1996, I find evidence of substantial postmerger price increases by rivals of merging hospitals. These increases were realized by 1997; prices appear to stabilize thereafter. Price increases were greater among hospitals that were geographically closer to the merging parties. Failing to instrument for rivals' mergers produces a statistically insignificant estimate of less than 2 percent. These results suggest that at least some hospital mergers have resulted in large price increases, a finding that stands in stark contrast to most of the empirical literature on this subject. Caution must be exercised, however, when extrapolating these conclusions to hospital mergers in general. The estimates I obtain rely on responses to mergers of colocated hospitals, which likely enjoy especially strong postmerger increases in market power.

The findings highlight the shortcomings of analyses that compare merging and nonmerging firms, particularly in the same market, where reactions to rivals' prices are likely. The estimates I obtain are far more consistent with predictions from structural models of demand in similar settings (Capps, Dranove, and Satterthwaite 2003; Gaynor and Vogt 2003) than with estimates from prior observational studies (for example, Connor, Feldman, and Dowd 1998), which generally find no effect or a negative effect of merger on price. This supports the use of structural models for prospective merger analysis. Finally, the results suggest that hospital markets are far smaller than those typically considered by researchers, practitioners, and courts.

The paper proceeds as follows. Section 2 lays the theoretical foundation for the empirical analysis. Section 3 describes the hospital industry and summarizes prior related research. Section 4 defines the study samples and provides descriptive statistics. Section 5 presents the empirical specifications and results. Section 6 explores the sensitivity of the results to alternative specifications. Section 7 concludes.

2. Theoretical Framework

The theoretical foundation for my empirical strategy is a simple model of spatial differentiation, Salop's (1979) "circular city." Firms independently maximize their profits taking others' actions as given; differentiation of the firms produces equilibrium prices that exceed costs. In Salop's model, the location of N firms along a circle of unit circumference is exogenously determined. Consumers of mass 1 are uniformly distributed along the circle and have unitary demand and value v for the product, regardless of the firm supplying it. When purchasing this product from firm j, consumer i incurs transport costs of td_{ij}, where d_{ij} denotes the distance (along the circle) between i and j. Transport costs can be viewed more generally as the costs associated with consuming a product that differs from the consumer's optimal product (that is, a product that coincides with her location in product space). To illustrate the effect of a merger in this setting, and particularly a merger of colocated firms, I solve Salop models that reflect the market structures I use to identify merger effects.

Gal-Or (1999) also uses the Salop circle to model competition in this sector, with separate circles for hospitals and insurers in a given geographic market. Gal-Or's treatment focuses on when it is optimal to merge without closing a facility post merger and on how this decision is influenced by the interaction between the market structures of the hospital and insurance sectors. The closure decision is not pertinent in the present setting, where the mergers of interest occur between colocated hospitals.[3]

Consider two circular cities with three firms each, denoted H, R1, and R2 (representing the hospital of interest, rival 1, and rival 2). In market 1, the rivals are located in exactly the same spot and H is as far away as possible. In market 2, the three firms are distributed evenly around the circle. These configurations are illustrated in Figure 1.

I conjecture that R1 and R2 in market 1 are likelier to merge than are R1 and R2 in market 2. If true, and if firm location is exogenous, H is exogenously more likely to be exposed to a rival merger. Under these assumptions (explored in subsequent sections), the number of colocated rival pairs a hospital has can serve as an instrument for rival merger.

The outcome I consider in this study is price. To illustrate the effect on H's price of a merger between colocated rivals, I derive the pre- and postmerger equilibrium prices in market 1. For the sake of comparison, I derive the same for market 2, assuming that one of the R facilities closes post merger and the other remains in the same location.[4] Without loss of generality, I set the marginal cost of each firm equal to zero. Appendix A gives the objective functions and

[3] Colocated hospitals are by definition undifferentiated, so under common ownership their prices would be the same regardless of the number of hospitals that remain open at the site. I do not model the interaction between hospitals and insurers; this interaction will, however, influence final realized prices.

[4] This seems the most plausible scenario given the high costs of construction and chronic overcapacity in the industry during the merger wave.

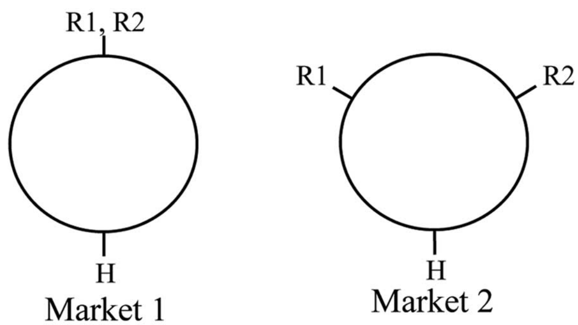

Figure 1. Illustration of two hypothetical markets

equilibrium prices for each firm type (R and H) in each market (1 and 2) and scenario (before and after the merger of R1 and R2). Here I describe the general results.

So long as t is not so high that hospital H enjoys a local monopoly and is unaffected by the actions of its rivals, a merger of colocated rivals will result in a very sizeable price increase (up to 100 percent).[5] The price increase following a merger of noncolocated rivals (as in market 2) is much smaller. The reason is that R1 and R2 compete more intensely before the merger in market 1. They are undifferentiated and therefore set their prices equal to cost. When they merge, market prices increase dramatically. In market 2, R1 and R2 are differentiated competitors pricing above cost prior to the merger, so the merger has a smaller impact on optimal postmerger pricing.

In Section 6, I return to these models to investigate the theoretical validity of the identifying assumption for my empirical analysis: that during the merger wave, price growth in the two market types (conditional on observables) would have been the same but for the increased frequency of mergers in markets with colocated rivals. (Note that I need not assume that premerger price levels of the two market types are the same; indeed, the solutions presented in Appendix A illustrate that they are not.) Both the theoretical results and the empirical tests presented below suggest that colocation is not associated with faster price growth except through its effect on the propensity to merge.

The following section describes the U.S. hospital industry during the 1980s and 1990s and discusses prior estimates of the effects of mergers in this sector.

3. Background

Until 1984, U.S. hospitals were generally reimbursed on a cost-plus basis by public and private insurers. In an effort to control escalating costs, the Medicare

[5] The markets I consider are sufficiently small and contain a large enough number of providers to rule out monopolistic behavior.

program instituted the Prospective Payment System (PPS) in 1984. Under PPS, hospitals receive a fixed payment for each Medicare patient in a given diagnosis-related group (DRG), making hospitals the residual claimants of any profits or losses. Payments were generous during the first few years of PPS, but by 1989 the majority of hospitals were earning negative margins on Medicare admissions (Coulam and Gaumer 1991). These financial pressures were exacerbated by the rise of managed care in the private sector. Managed care penetration increased from under 30 percent of private insurance in 1988 to nearly 95 percent by 1999 (Kaiser Family Foundation 2004), bringing about a shift from fee-for-service to negotiated prices. Thus, the motives to consolidate intensified substantially during the 1990s, triggering an unprecedented wave of mergers, acquisitions, and closures. Between 1989 and 1996, there were 190 hospital mergers, as compared to 74 during 1983–88 (Bazzoli et al. 2002).[6] As a result, recent studies of hospital mergers have focused on this time period (for example, Bazzoli et al. 2002; Dranove and Lindrooth 2003).

Hospital mergers have received a great deal of attention from health care economists and antitrust enforcement agencies, in part because of the volume of patients and revenues involved. In 2001, the 5,801 hospitals in the United States treated 1.68 million outpatients and 658,000 inpatients each day, collecting $451 billion in revenues. By comparison, expenditures on new passenger vehicles in 2001 totaled $106 billion (U.S. Census Bureau 2003, tables 158, 170, and 667). The localized nature of competition is also a source of concern for antitrust enforcement agencies, as monopoly and oligopoly providers in a given geographic area can sustain supracompetitive prices.

The not-for-profit status of most hospitals, however, presents the possibility that hospitals will not exploit postmerger increases in market power. This is an argument the courts have often cited in rejecting attempts to block proposed hospital mergers.[7] Since 1991, the Department of Justice and Federal Trade Commission (FTC) have tried to enjoin seven hospital mergers and failed to prevail a single time (Federal Trade Commission 2003; Town and Vogt 2005). After a long hiatus, the FTC changed course and began performing retrospective analyses of consummated mergers to identify possible anticompetitive conduct. This initiative was dealt a severe blow in 2007 when the full commission failed

[6] These merger counts refer to legal consolidations of two or more hospitals under single ownership.

[7] There are at least two distinct arguments espoused in these court rulings. In *Long Island Jewish Medical Center* (*United States v. Long Island Jewish Medical Center,* 983 F. Supp. 121, 149 [October 23, 1997]), the court cited the "genuine commitment" of the merging hospitals "to help their communities." In *Butterworth Health Corporation* (*Federal Trade Commission v. Butterworth Health Corp,* 1997-2 Trade Cas. (CCH) 71,863, 71,867–68 [6th Cir. 1997]), the court was convinced that the merging hospitals would not raise prices "[b]ecause the boards . . . are comprised of community and business leaders whose companies pay the health care costs of their local employees."

to uphold an order to divest issued by an administrative judge against not-for-profit Evanston Northwestern Healthcare Corporation.[8]

Despite the sustained interest in hospital mergers, including private lawsuits challenging postmerger price increases, economists have failed to reach a consensus on the price effects of mergers in this sector. Gaynor and Vogt (2000), Connor and Feldman (1998), and Dranove and Lindrooth (2003) provide excellent summaries of the extensive literature on hospital competition and mergers. Most relevant for the present work are longitudinal studies that compare pre- and postmerger outcomes. The majority of these studies focus on the cost reductions achieved by merging institutions because hospitals typically cite economies of scale and increased purchasing power as the main motives for merger. These studies have generally found very modest impacts of merger on costs, with two notable exceptions: Alexander, Halpern, and Lee (1996) and Dranove and Lindrooth (2003). Using data on mergers of previously independent hospitals that operate under a single license post merger, Dranove and Lindrooth find postmerger cost decreases of 14 percent. These are precisely the mergers I use for my analysis. The combination of large postmerger price increases (implied by my results) and cost decreases suggests sizeable profit gains for merging hospitals.

The pre- versus postmerger pricing studies are fewer in number and generally find price reductions following a merger (for example, Connor, Feldman, and Dowd 1998; Spang, Bazzoli, and Arnould 2001). These estimates are plagued by the selection problems described earlier and are biased downward by the use of nonmerging hospitals as control groups. If nonmerging rivals raise their prices in response to price increases by merging parties, mergers could be associated with no relative price increase for merging parties in a given market area but a large absolute price increase for the market area as a whole.

Krishnan (2001) addresses the selection problem by comparing price growth for diagnoses in which merging hospitals gained substantial market power (>20 percent) with price growth for diagnoses in which they gained insignificant share (<5 percent). Using data on 11 hospital mergers in Ohio in 1994 and 1995, Krishnan finds that merging hospitals increased price 8.8 percent more in diagnoses where they gained substantial market share. By design, this estimate is biased downward: it eliminates hospitalwide price increases, which are likely because many hospital features (for example, location) are constant across services. In examining hospital responses to diagnosis-specific changes in price imposed by Medicare, Dafny (2005) finds little evidence that hospitals compete

[8] The complaint against Evanston Northwestern Healthcare Corporation (ENH) alleged that ENH increased price "far above price increases of other comparable hospitals" after acquiring nearby Highland Park Hospital in 2000 (Federal Trade Commission 2007). In October 2005, chief administrative law judge Stephen J. McGuire ruled in favor of the FTC and ordered ENH to divest Highland Park Hospital. This order was stricken upon appeal to the full commission in August 2007. The opinion concurred with the finding of anticompetitive conduct but called the divestiture order "unwarranted" (Opinion of the Commission, Docket No. 9315 [August 6, 2007]).

in quality at the diagnosis level; rather, the data are consistent with competition in overall hospital quality. These results suggest that the downward bias in Krishnan's estimates may be substantial.

Two prior studies use rival analysis to estimate the impact of merger on average market price. Woolley (1989) is a classic event study that traces the effect of 29 merger-related events from 1969 to 1985 on the stock prices of rival hospital chains. The study finds a positive relationship between pro-merger events and stock price but has been criticized on methodological grounds because of the events selected, the definition of rival chains, and the fact that only a small fraction of hospitals are owned by publicly traded firms (Vita and Schumann 1991). Connor and Feldman (1998) compare price and cost growth between 1986 and 1994 for nonmerging hospitals with merging rivals (hereafter NMW hospitals) and nonmerging hospitals without merging rivals (hereafter NMWO hospitals). They find no effect of rival mergers on price, with the exception of mergers with an intermediate level of postmerger market share, where a small effect (3 percent over 8 years) is found. The lack of an effect for larger mergers is attributed to the ability of the newly formed hospitals to dominate the market and suppress rivals' prices through merger-related quality improvements.

My analysis also focuses on price changes of nonmerging hospitals over a long period of time (1988–97) and across all states. However, I take steps to examine and address the selection problem that persists in rival analyses of mergers. First, I apply sample restrictions that substantially reduce the differences in observable characteristics of NMW and NMWO hospitals. Second, I introduce rival colocation as an instrument for rival merger. These steps are discussed in turn in Sections 4 and 5.

4. Data

Merger data constructed for Dranove and Lindrooth (2003) were generously provided by the authors. Using the *Annual Survey of Hospitals* and the *Annual Guide to Hospitals,* both produced by the American Hospital Association (AHA), Dranove and Lindrooth identified 97 independent hospital mergers between 1989 and 1996. They define an independent merger as a combination of two hospitals that are not affiliated with any hospital system into a single entity. To qualify as a merger in this data set, the newly created hospital must report a single set of financial and utilization statistics and surrender one of its facility licenses. Figure 2 graphs the distribution of these mergers over time.[9] Because my instrument predicts only the incidence and not the timing of merger (that is, the instrument is not time varying), I cannot exploit merger dates in my analysis. I therefore create an indicator variable for merger between 1989 and 1996 using the sample of general nonfederal hospitals present in the 1988 AHA survey and located in

[9] Although Dranove and Lindrooth's data end in 1996, merger figures reported by Cuellar and Gertler (2003) for 1994–2000 reveal a steep drop-off in merger activity in 1997 and a steady decline thereafter.

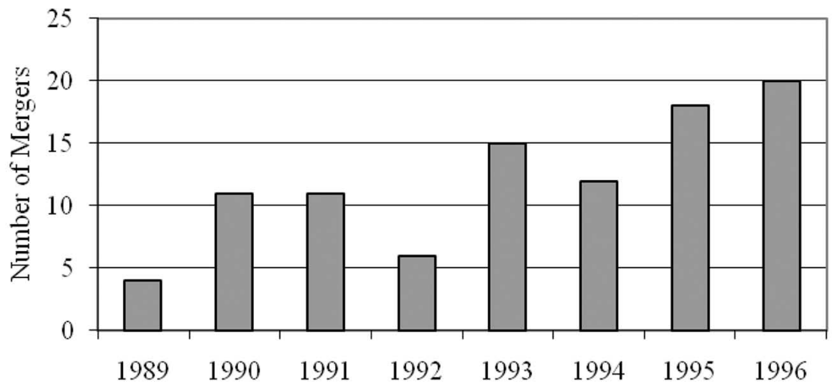

Figure 2. Timing of independent hospital mergers 1989–96 (Dranove and Lindrooth 2003)

metropolitan statistical areas (MSAs) or counties with more than 100,000 residents.[10] (I apply these restrictions because Dranove and Lindrooth did not consider mergers in rural areas.) The AHA survey provides descriptive data for each hospital, including location, ownership status, and number of beds. Note that while independent mergers were more common during the latter part of the study period, those involving colocated hospitals (the subset that provides the variation exploited in the IV analysis) are fairly evenly distributed over time.[11]

For each hospital in the sample, I obtain panel data on financial measures from the Healthcare Cost Report Information System (HCRIS), a database maintained by the Centers for Medicare and Medicaid Services (CMS). The HCRIS contains annual financial and utilization data for all providers receiving reimbursement from either program under CMS's purview. Over 99 percent of the hospitals in my sample appear in HCRIS, which can be downloaded from the CMS Web site.[12]

Average hospital price in a given year is calculated as inpatient revenue per case-mix-adjusted discharge.[13] Other researchers have used similar measures, for example, inpatient revenue per discharge (Connor and Feldman 1998) or in-

[10] Of the 5,373 general nonfederal hospitals located in the mainland United States in 1988, 466 are dropped because of these restrictions (American Hospital Association 1988).

[11] There are 10 mergers involving colocated hospitals during the study period; nine of these are cases in which two colocated hospitals merged.

[12] U.S. Department of Health and Human Services, Centers for Medicare and Medicaid Services, Cost Reports (http://www.cms.hhs.gov/CostReports).

[13] More precisely, price = [(hospital inpatient routine service charges + hospital intensive care charges + hospital inpatient ancillary charges) × discount factor − Medicare primary payor amounts − Medicare total amount payable]/[(total discharges excluding swing/skilled nursing facility − total Medicare discharges excluding swing/skilled nursing facility) × case mix index]. The discount factor is defined as 1 − (contractual discounts/total patient charges) and reflects the common practice of discounts for private insurers. The formula was constructed with the guidance of HCRIS experts at the Centers for Medicare and Medicaid Services (CMS). Records with discount factors outside of [0, 1] or negative values for any measure in the price formula are excluded.

patient revenue per diem, controlling for patient diagnosis (Keeler, Melnick, and Zwanziger 1999).[14] In calculating price, I exclude Medicare revenues and discharges because the federal government sets prices for these patients. However, I use each hospital's case-mix index (CMI) for Medicare patients as a proxy for the non-Medicare CMI, which is not reported. Medicare CMIs are obtained from the Prospective Payment Impact Files produced annually by CMS.[15] The variables needed to calculate price are available for fiscal years 1985–2000, which spans the period 3 years before the first merger in the data to 3 years after the last merger in the data.

To reduce the influence of coding errors, observations in the 5 percent tails of price in a given year are assigned a missing value for that year.[16] The dependent variables are the change in log price for a given hospital during 1985–88 (the premerger period), 1988–97 (the treatment period), and 1997–2000 (the postmerger period). All dependent variables are also censored at the 5th and 95th percentiles. I construct two indicators of financial distress using the 1988 HCRIS data: the share of patients covered by Medicaid and the aggregate debt/asset ratio. Prior research suggests that financially distressed hospitals are more likely to be party to a merger or acquisition. I obtain market-level control variables such as county per capita income in 1990 from the Area Resource File, a database compiled by the U.S. Department of Health and Human Services. Estimates of county-level HMO penetration in 1994 were provided by Laurence Baker.[17]

Latitude and longitude coordinates for the main address reported by each hospital in the 1988 AHA survey were purchased from Tele Atlas's Geocode.com. Using these coordinates, which contain six decimal places and are accurate up to the street segment, I calculate the straight-line distance between hospitals. After identifying 213 hospitals located within .3 miles of another, I performed a secondary check by examining individual maps of these pairs from Mapquest.com. Restricting the definition to exclude hospitals located more than 5 blocks apart reduces the final number of colocated hospitals to 191. In Section 6, I illustrate the robustness of the results to alternative definitions of colocation.[18]

The all-hospitals sample in Table 1 includes hospitals with nonmissing data for all independent variables (4,487 out of 4,907 hospitals, accounting for 91

[14] Keeler, Melnick, and Zwanziger (1999) use a two-step process to adjust for each hospital's case mix. First, they use California discharge data for 10 common diagnoses to run 10 separate regressions of net inpatient revenue per diem on patient characteristics and hospital dummies. Next, they use the 10 coefficients for each hospital to construct a weighted average price index.

[15] The CMS uses the distribution of a hospital's Medicare admissions across roughly 500 diagnosis-related groups, or DRGs, to construct that hospital's annual case-mix index (CMI). Each DRG is associated with a weight. The CMI is the admissions-weighted average DRG weight for the hospital. The weights were originally constructed (in 1984) so that the average CMI across all hospitals would equal 1; this average has since crept higher.

[16] Between 1985 and 2000, the 5th percentile of the annual price distribution ranges from $1,374 to $1,664 (in year 2000 dollars), and the 95th percentile from $6,256 to $8,334.

[17] These estimates were constructed using data from the Group Health Association of America.

[18] For the purposes of identifying colocated hospitals and counting rivals, all general nonfederal hospitals with valid addresses in the nonterritorial United States are included; sample restrictions are applied after this step is complete.

Table 1
Descriptive Statistics: Sample Means

	All Hospitals	Rivals Sample		
		All	NMW	NMWO
Dependent variables:				
1985 Price ($)	3,223	3,951	3,935	3,953
1988 Price ($)	3,404	4,057	3,737	4,107
1997 Price ($)	3,851	4,091	3,823	4,133
2000 Price ($)	3,908	4,067	4,014	4,075
ln(1988 Price) − ln(1985 price)	.064	.032	−.029	.042
ln(1997 Price) − ln(1988 price)	.132	.010	.020	.009
ln(2000 Price) − ln(1997 price)	.013	.001	.039	−.005
Merger indicators and instruments:				
Merger (%)	4.0			
Colocated (%)	3.6			
Number of rival mergers		.156	1.161	0
Number of colocated rival pairs		.332	.712	.273
Hospital characteristics:				
For profit (%)	15.2	15.2	16.1	15.0
Government (%)	25.5	10.0	8.5	10.3
Teaching hospital (%)	6.4	16.3	18.6	15.9
Medicaid share of discharges (%)	11.4	11.2	15.1	10.6
Debt/asset ratio (%)	55.1	55.7	58.9	55.2
Occupancy rate (%)	56.5	66.3	67.7	66.1
Beds:				
0–99 (%)	41.0	5.4	10.2	4.6
100–199 (%)	26.1	18.8	16.1	19.2
200–299 (%)	14.7	26.1	25.4	26.2
300–399 (%)	8.3	20.9	22.9	20.6
400+ (%)	9.9	28.8	25.4	29.4
Market characteristics:				
Rivals within 7 miles	3.16	7.37	12.03	6.64
MSA population:				
Not in MSA (%)	44.0	3.0	0.8	3.3
<250,000 (%)	10.7	9.9	6.8	10.4
250,000–499,999 (%)	9.4	17.6	16.9	17.7
500,000–1,000,000 (%)	10.7	19.2	20.3	19.0
1,000,000–2,500,000 (%)	13.8	27.4	25.4	27.7
>2,500,000 (%)	11.4	23.0	29.7	22.0
County HMO penetration (%)	14.5	21.3	23.7	20.9
County per capita income ($)	17,154	19,923	20,036	19,905
N	4,487	877	118	759

Note. Prices are inflated to year 2000 dollars using the Consumer Price Index—All Urban Consumers. Price change variables are censored at the 95th and 5th percentiles. Hospital and market characteristics are measured as of 1988, with the exception of county health maintenance organization (HMO) penetration, which is for 1994. Rivals are defined as hospitals located within a 7-mile radius. In column 1, *N* values for the price data are 3,802 (1985), 4,026 (1988), 3,462 (1997), and 3,240 (2000). All hospitals in the rivals sample have price data for 1985, 1988, and 1997. Year 2000 data are available for 99 of the nonmerging hospitals with merging rivals (NMW) and 672 of the nonmerging hospitals without merging rivals (NMWO) hospitals. MSA = metropolitan statistical area.

percent of 1988 discharges). Within this sample, 178 (4 percent) were party to an independent merger between 1989 and 1996, and 163 (3.6 percent) were colocated with at least one hospital.[19] Eighteen of the 163 subsequently merged, yielding a merger rate of 11 percent in the colocated subset.[20]

The rivals sample in the second column is limited to nonmerging hospitals in the all-hospitals sample that have two or more rivals within 7 miles in 1988 and nonmissing price data during the premerger and treatment periods. The rationale for requiring two or more rivals is straightforward: if a nonmerging hospital has fewer than two rivals, it cannot experience a rival merger and thus should not be included in the sample. The rationale for the 7-mile cutoff is that the merger of adjacent hospitals can reasonably be expected to affect the prices of rivals located within fairly tight geographic bounds. In Section 6, I examine the sensitivity of the results to alternative market definitions. Given the sample restrictions, hospitals in the rivals sample are generally located in densely populated urban areas. As compared to those in the all-hospitals sample, they are less likely to be government owned (10 versus 26 percent) and more likely to offer teaching programs (16 versus 6 percent).

The rivals sample can be subdivided into hospitals with merging rivals (NMW hospitals) and hospitals without merging rivals (NMWO hospitals). Both NMW and NMWO hospitals share similar observable characteristics, although there are some statistically significant differences. The NMW hospitals have a greater share of Medicaid patients, a larger number of rivals, and operate in markets with slightly higher HMO penetration rates. They are also more likely to be very small (<100 beds). Price growth in the 3 years prior to the merger wave is significantly lower for NMW than for NMWO hospitals (−2.9 versus 4.2 percent). This suggests that NMWO hospitals are inappropriate controls for NMW hospitals; that is, treating rival mergers as exogenous will produce underestimates of the impact of rival merger on price. This conjecture is corroborated in the ordinary least squares (OLS) results presented below.

5. Empirical Analysis

To estimate the price effects of merger, I focus on the prices charged by rivals of merging hospitals. If merging hospitals raise prices, and prices are strategic complements, rivals of these hospitals should be able to raise prices as well. This approach allows me to eliminate merging hospitals from the primary analysis, which is ideal as they differ from nonmerging hospitals in unobservable ways

[19] A total of 194 hospitals were involved in the 97 independent mergers between 1989 and 1996. Of these hospitals, 192 are included in the 1988 American Hospital Association data, which is the starting point for the analysis. All 192 are present when rival merger counts and colocation variables are constructed. However, the all-hospitals sample excludes 13 of the 192 because of missing covariates and one because it is located in a rural area. (As noted earlier, rural hospitals are excluded because Dranove and Lindrooth did not seek to identify mergers in rural areas. The sole exception is due to a merger of a nonrural and a rural hospital.)

[20] All 10 mergers involving colocated hospitals are represented among these 18 hospitals (two of the 20 hospitals involved in the mergers are missing, as mentioned above).

that are likely to be correlated with price changes. However, selection issues persist even in the sample of nonmerging hospitals, as nonmerging hospitals in markets with mergers (NMW hospitals) are likely to be different in relevant, unobserved ways from nonmerging hospitals in markets without mergers (NMWO hospitals). Thus, I introduce an instrument for exposure to a rival merger, namely, the number of colocated rival hospital pairs. If this measure is correlated with the propensity for rivals to merge and otherwise uncorrelated with the price growth of area hospitals, it is a valid instrument for rival merger and can be used to produce unbiased estimates of the price effect of rival merger.

I proceed in two steps. First, I validate the conjecture that colocated hospitals are more likely to merge. For this analysis, I use the all-hospitals sample. Second, I use the rivals sample to obtain an IV estimate of the effect of rival merger on price.[21] I compare this estimate with the estimate from an OLS regression that takes rival merger to be exogenous.

5.1. Colocation and the Probability of Merger

The raw data from the all-hospitals sample suggests that colocation is a good predictor of merger: the merger rate for colocated hospitals is 11.0 percent, as compared to 3.7 percent for noncolocated hospitals. Table 2 presents the results of a linear probability model that includes all of the hospital characteristics reported in Table 1 as well as market characteristics such as the county-level HMO penetration rate, per capita income, and total population. To control for the possibility that state regulatory boards affect the merger rate, results are also presented with state fixed effects.

The relationship between the probability of merger and colocation is robust to all of the controls: colocation is associated with an increase of 6–7 percentage points in the probability of merger. As a falsification exercise, I reestimate these models using an indicator for system merger as the dependent variable. System mergers are defined by Dranove and Lindrooth (2003) as one-to-one consolidations of hospitals that did not surrender a facility license and/or report joint data following the consolidation. The coefficient estimates from these regressions are small and statistically insignificant.[22] As expected, colocation is a good predictor of fully integrated mergers but not of all activity related to mergers and acquisitions. Hence, the point estimates pertain only to these particular types of mergers.

Given the strong relationship between colocation and merger, the relationship between rival colocation and rival merger in the rivals sample should also be strong. Table 2, column 3, reports the results of a linear regression of the number of rival mergers on the number of colocated rival pairs, again controlling for hospital and market characteristics. Column 4 adds state fixed effects. These

[21] An alternative approach would be to use own colocation as an instrument for own merger. The advantage of rival analysis is that it potentially exploits each merger several times (when multiple hospitals are exposed to the same merger), which increases the sample size substantially.

[22] The point estimates are −.020 (.011) with or without state fixed effects.

Table 2
Relationship between Merger/Rival Merger and Colocation/Rival Colocation: First Stage

	Own Merger		Number of Rival Mergers	
	(1)	(2)	(3)	(4)
Colocated	.066**	.062**		
	(.016)	(.016)		
Colocated rival pairs			.119**	.112**
			(.018)	(.019)
Hospital characteristics:				
For profit	−.005	.003	.071	.090*
	(.009)	(.009)	(.044)	(.046)
Government	−.045**	−.037**	−.067	−.045
	(.007)	(.008)	(.047)	(.047)
Teaching hospital	.027*	.022	−.008	−.006
	(.015)	(.015)	(.045)	(.044)
Medicaid share	.040	.037	.399**	.321*
	(.031)	(.032)	(.130)	(.130)
Debt/asset ratio	−.009	−.008	−.006	−.059
	(.008)	(.008)	(.049)	(.048)
Occupancy rate	.012	−.004	.189	−.125
	(.020)	(.021)	(.120)	(.126)
Beds:				
100–199	.009	.013	−.156*	−.118+
	(.008)	(.008)	(.067)	(.064)
200–299	.019+	.023*	−.153*	−.129*
	(.010)	(.010)	(.067)	(.064)
300–399	−.018	−.010	−.153*	−.089
	(.013)	(.013)	(.070)	(.067)
400+	−.019	−.009	−.184*	−.127+
	(.014)	(.014)	(.072)	(.069)
Market characteristics:				
MSA population:				
<250,000	.047**	.053**	.047	.110
	(.011)	(.011)	(.092)	(.092)
250,000–499,999	−.001	.004	.055	.101
	(.012)	(.012)	(.087)	(.087)
500,000–1,000,000	−.003	.001	.056	.059
	(.012)	(.012)	(.089)	(.091)
1,000,000–2,500,000	−.038**	−.019	−.030	.021
	(.012)	(.013)	(.090)	(.092)
>2,500,000	−.050**	−.035*	.056	.141
	(.014)	(.015)	(.093)	(.096)
HMO penetration	.037	−.012	.464**	.379+
	(.032)	.043	(.141)	(.222)
ln(Per capita income)	.071**	.046*	.002	−.277**
	(.018)	(.021)	(.087)	(.097)
State fixed effects	No	Yes	No	Yes
N	4,487	4,487	877	877

Note. All specifications are estimated by ordinary least squares methods. Models using own merger as the dependent variable are estimated on the all-hospitals sample, while models using number of rival mergers as the dependent variable are estimated on the rivals sample. MSA = metropolitan statistical area; HMO = health maintenance organization.

+ Significant at $p < .10$.
* Significant at $p < .05$.
** Significant at $p < .01$.

specifications reveal that having one additional pair of colocated rivals is associated with an increase of roughly .11 in the number of rival mergers, as compared to a mean of .16. This regression constitutes the first stage in the two-stage least squares (2SLS) rival analysis.

5.2. The Impact of Merger on Rivals' Prices

The reduced-form results are depicted graphically in Figure 3, which charts the ratio of average prices for hospitals with and hospitals without colocated rivals (that is, stratifying the sample by the instrument rather than the endogenous treatment).[23] This ratio declines during the premerger period, increases during the treatment period, then declines again during the postmerger period (which suggests a return to the pre-merger-period trend). Although this simple graphical analysis does not account for control variables, the general trends persist in the regression results.

The reduced-form analysis regresses price growth during the treatment period on the number of colocated rival pairs and all of the control variables. Price growth is measured as the change in logged price between 1988, the year before the first merger in the data set, and 1997, the year following the last merger in the data set. Results are reported in columns 3 and 4 of Table 3. Each additional pair of colocated rivals is associated with a statistically significant increase of .045 in price growth, as compared to a mean of .010 during this period. The estimate falls slightly, to .034, and remains statistically significant upon inclusion of state fixed effects.

Columns 1–2 and 5–6 of Table 3 report results from analogous regressions using price growth in the pre- and postmerger periods as the dependent variable, respectively.[24] I find no evidence that price growth during the premerger period is higher for hospitals with colocated rivals; if anything, there is weak evidence for the converse. Assuming this trend would have remained the same during the treatment period but for the increased propensity of these hospitals to be exposed to rival mergers, the estimated merger effects should be viewed as conservative.[25] These effects are reported in Table 4, which gives the IV estimate

[23] Prices are inflated to year 2000 dollars using the Consumer Price Index—All Urban Consumers. Mean price in 1985 (2000) is $3,975 ($4,303) for hospitals with colocated rivals and $3,944 ($4,250) for hospitals without colocated rivals.

[24] Regressions for each period use hospital covariates as of the start of the period, that is, 1985 for the premerger period, 1988 for the treatment period, and 1997 for the postmerger period, except as noted in Table 3.

[25] This assumption would be violated if, for example, competition in areas with colocated rivals was excessive or unsustainably fierce during the premerger period, so that price growth during the treatment period would have been steeper than that during the premerger period even absent the greater incidence of mergers. Similarly, this assumption would fail if entry in markets with colocated hospitals (or entry of colocating hospitals) was correlated with expected price growth during the treatment period. The latter scenario, however, is rather unlikely, as there has been very little entry in the acute-care hospital industry since the Hospital Survey and Construction Act (known as Hill-Burton; ch. 958, 60 Stat. 1040 [August 13, 1946]). Exit was not uncommon; however, less than 1 percent of the hospitals in the study sample have rivals that exited during the study period. All results are robust to including a control for the number of exiting rivals.

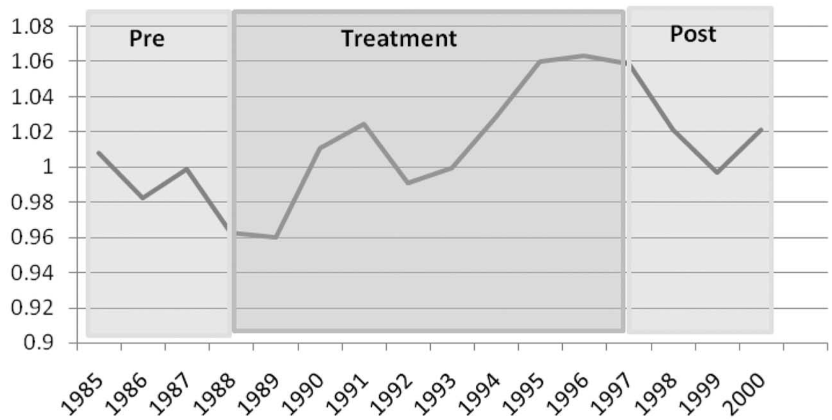

Figure 3. Ratio of mean prices for hospitals with and without colocated rivals

of the effect of a rival's merger between 1989 and 1996 on price growth between 1988 and 1997. The IV estimate is simply the ratio of the reduced-form and first-stage coefficient estimates, $.045/.119 \approx .376$, with a standard error of .132.[26] This figure translates into a cumulative price increase of approximately 46 percent (35 percent using the model with state fixed effects; $e^{.376} \approx 1.46$ and $e^{.301} \approx 1.35$). This is equivalent to moving a hospital from the 25th to the 65th percentile of price growth during this period, or the 75th to the 95th percentile. (Real price growth in the rivals sample averaged 1 percent between 1988 and 1997, with a standard deviation of 33 percent.)[27] Given that there is no relationship between colocated rival pairs and price growth during the postmerger period, these mergers appear to have induced a large one-time price increase or short-term boost in the pace of price growth rather than a transition to a permanently steeper price trajectory.

Table 4 also reports OLS estimates of the effect of rival merger on price growth. As in Connor and Feldman (1998), I too find no statistically significant impact

[26] When using the exact (unrounded) coefficients, this ratio is identical to the instrumental variables estimate because there is a single endogenous regressor and the model is exactly identified.

[27] These estimates are similar in magnitude to the price increases implemented by Evanston Northwestern Healthcare (ENH) following its 1999 acquisition of nearby (but not colocated) Highland Park Hospital. Evanston Northwestern Healthcare did not dispute the price increases alleged in the FTC complaint. These include increases of 52 percent at the Evanston facility for UnitedHealthcare's health maintenance organization (HMO), 190 percent for UnitedHealthcare's preferred provider organization (PPO), 60 percent for Humana, 40 percent for Private Healthcare Systems, and 15–20 percent for Aetna and Cigna's HMOs (Taylor 2006). Note that these figures represent increases by merging hospitals rather than their rivals. Increases by merging hospitals may be higher or lower than increases by rivals. As demonstrated by Gal-Or (1999), Capps, Dranove, and Satterthwaite (2003), and Ho (2007), the average negotiated postmerger price for any given hospital depends on its relative bargaining power vis-à-vis insurers in the postmerger marketplace.

of a rival's merger on price using OLS. Hausman specification tests easily reject equality of the two estimates for models with and without state fixed effects.

6. Extensions and Robustness

6.1. Alternative Explanations

The identification strategy assumes that, controlling for observable characteristics, any systematic difference in the price growth of nonmerging hospitals in markets with colocated rivals and nonmerging hospitals in markets without colocated rivals is due to the greater frequency of rival mergers to which the former group is exposed. The finding that price growth is similar for both groups prior to the start of the merger wave provides some support for this assumption; however, the study period is long, and it is possible that omitted factors affect the evolution of prices in these markets differently during this particular period.

The most important such factor is the strength of managed care. Although I control for the percentage of the population enrolled in managed care in each market, the negotiating power of these organizations varies widely. As a result, it is helpful to use the model from Section 2 to generate predictions regarding how changes in managed care would affect prices in the two market types. In the context of this model, the growth of managed care is similar to a reduction in v, buyers' maximum willingness to pay for hospital services. The impact of a decline in v on equilibrium price (that is, dP^*/dv) depends on the value of the transport cost t, but for the relevant range of t, price is as sensitive or more sensitive to v in markets with colocated rivals. The intuition for this result is that prices in more competitive markets are more sensitive to changes in costs or demand because firms are less able to absorb such shocks by cutting into their profit margins. Thus, if anything, we might expect lower price growth in markets with colocated rivals during the treatment period, strengthening the argument that the higher observed growth is due not to the structural propensity for hospitals in such markets to raise prices particularly rapidly but rather to the increased frequency of mergers induced by colocation.

Another empirical test of the identifying assumption is to see if the results are robust to the exclusion of all control variables. Assuming that the correlation between observable factors and the instrument is similar to that between unobservable factors and the instrument, a robust result suggests that the estimates are not biased by unobserved factors. Indeed, the estimated merger effect, presented in column 2 of Table B1, is virtually unchanged when all controls are omitted.

6.2. Specification Checks

The first key specification check confirms that the results are not overly sensitive to the assumption of independent error terms. While each hospital has its own market area, and hence the independent variable of interest (namely, the number of rival mergers) varies by hospital, it is possible that the error terms

Table 3

Relationship between Price Growth and Rival Colocation: Reduced Form

	ln(1988 Price) − ln(1985 Price)		ln (1997 Price) − ln(1988 Price)		ln (2000 Price) − ln(1997 Price)	
	(1)	(2)	(3)	(4)	(5)	(6)
Colocated rival pairs	−.016	−.013	.045**	.034*	−.008	−.001
	(.010)	(.011)	(.014)	(.015)	(.013)	(.014)
Hospital characteristics:						
For profit	.001	−.009	−.087*	−.052	−.026	−.018
	(.024)	(.025)	(.035)	(.036)	(.027)	(.029)
Government	.062*	.056*	.021	.042	.023	.034
	(.025)	(.026)	(.037)	(.037)	(.034)	(.035)
Teaching hospital	−.052*	−.048*	.014	.018	−.013	−.007
	(.024)	(.024)	(.035)	(.035)	(.030)	(.031)
Medicaid share	−.501**	−.441**	.315**	.224*	.066	.059
	(.079)	(.082)	(.102)	(.103)	(.073)	(.077)
Debt/asset ratio	−.155**	−.040	.046	.004	.021	.012
	(.033)	−(.035)	(.038)	(.038)	(.032)	(.034)
Occupancy rate	−.255**	−.024	.107	−.079	.025	.033
	(.071)	(.078)	(.093)	(.100)	(.073)	(.078)
Beds:						
100–199	−.048	−.060+	.028	.048	.021	.027
	(.037)	(.036)	(.052)	(.050)	(.048)	(.048)
200–299	−.017	−.025	.013	.040	−.033	−.017
	(.037)	(.036)	(.053)	(.051)	(.048)	(.049)
300–399	−.011	−.032	.003	.024	.005	.028
	(.039)	(.038)	(.055)	(.053)	(.049)	(.050)
400+	.007	−.005	.022	.045	−.005	.016
	(.040)	(.040)	(.056)	(.055)	(.050)	(.051)

Market characteristics:						
MSA population:						
<250,000	.064	−.011	−.020	.104	.049	.059
	(.050)	(.052)	(.072)	(.073)	(.062)	(.067)
250,000–499,999	−.003	−.055	−.060	.030	.032	.047
	(.048)	(.049)	(.068)	(.069)	(.058)	(.062)
500,000–1,000,000	.007	−.072	−.045	.047	−.018	−.011
	(.049)	(.051)	(.070)	(.072)	(.060)	(.064)
1,000,000–2,500,000	.026	−.049	−.105	−.029	.000	.017
	(.050)	(.051)	(.071)	(.073)	(.060)	(.064)
>2,500,000	.047	.012	−.124+	−.025	−.072	−.057
	(.051)	(.054)	(.073)	(.076)	(.062)	(.068)
HMO penetration	−.166*	−.124	−.529**	−.332+	.284**	.089
	(.078)	(.124)	(.110)	(.176)	(.097)	(.162)
ln(Per capita income)	−.111*	−.059	.269**	.14+	.017	.049
	(.048)	(.055)	(.068)	(.077)	(.058)	(.067)
State fixed effects	No	Yes	No	Yes	No	Yes
N	877	877	877	877	703	703

Note. All specifications are estimated by ordinary least squares methods using the rivals sample. Regressions for each period use hospital covariates as of the start of the period, that is, 1985 for the premerger period, 1988 for the treatment period, and 1997 for the postmerger period, with the following exceptions: health maintenance organization (HMO) penetration rate (measured as of 1994) and ln(per capita income) (measured in 1990). MSA = metropolitan statistical area.

+ Significant at $p < .10$.

* Significant at $p < .05$.

** Significant at $p < .01$.

Table 4

Effect of Rival Mergers on Price Growth: ln(1997 Price) − ln(1988 Price)

	Instrumental Variables		Ordinary Least Squares	
	(1)	(2)	(3)	(4)
Number of rival mergers	.376**	.301*	.016	−.003
	(.132)	(.147)	(.026)	(.027)
State fixed effects	No	Yes	No	Yes

Note. Hospital and market characteristics are included for all specifications. $N = 877$.
* Significant at $p < .05$.
** Significant at $p < .01$.

for hospitals from the same general market area are correlated because of local economic shocks. For this reason, I also estimate standard errors clustered by hospital service area (HSA). Defined by the Dartmouth Atlas Working Group (1996), HSAs represent local hospital markets where the majority of residents obtain their hospital care.[28] This adjustment yields larger confidence intervals for most coefficients, but the key results remain statistically significant, if not at $p < .05$ then at $p < .10$.

Table 5 explores the sensitivity of the results to alternative definitions for colocation and changes in market boundaries. Instrumental variables estimates without state fixed effects are reported for all combinations of these definitions and boundaries.[29] The results are fairly insensitive to the colocation definition, with statistically significant point estimates ranging between .326 and .511. The Mapquest corrections eliminate a small amount of noise in the colocation measure, but this noise does not appear to be systematic. In the (unreported) first-stage regression using .3 miles as the colocation definition (that is, eliminating the 5-block Mapquest restriction), the coefficient on colocated rival pairs is .117 (.017), as compared to .119 (.018) for the Mapquest-corrected version (reported in Table 2).

The alternative definitions for colocation can also be used to perform an overidentification test of the colocation instrument. The model can be estimated by 2SLS using two instruments for rival merger: the number of rival pairs less than .2 miles apart and the number of rival pairs .2–.3 miles apart. Regressing the residuals from this model on the instruments and exogenous regressors and multiplying the resulting R^2-value by the number of observations produces a test statistic that is distributed as a χ^2-statistic with 1 degree of freedom (Hausman 1983). The test statistic of .36 (p-value $= .55$) supports the null hypothesis of exogeneity of the instruments. I obtain similar results using 0–.2 and .2–.4 as the colocation ranges.

To expand the instrument set, I also considered a variant of the colocation instrument: the number of colocated rival pairs of the same ownership type

[28] Details are available at Dartmouth Atlas Working Group, The Dartmouth Atlas of Health Care (http://www.dartmouthatlas.org/faq/data.shtm).

[29] Results with state fixed effects are similar and available on request.

Table 5
Sensitivity of Results to Distance Cutoffs Defining Colocation

Market Radius	.2 Miles	.3 Miles	.3 Miles and <5 Blocks	.4 Miles
5 Miles ($N = 722$)	.992	.851*	.962**	1.116$^+$
	(.687)	(.340)	(.377)	(.603)
7 Miles ($N = 877$)	.511$^+$	.326**	.376**[,a]	.431$^+$
	(.298)	(.125)	(.132)	(.221)
10 Miles ($N = 1{,}041$)	.046	.038	.051	.021
	(.082)	(.050)	(.054)	(.072)

Note. Values are instrumental variables estimates from separate rival analyses.
[a] Corresponds to column 1 of Table 4.
$^+$ Significant at $p < .10$.
* Significant at $p < .05$.
** Significant at $p < .01$.

(that is, both not-for-profit hospitals, both for-profit hospitals, and both government hospitals). Ceteris paribus, hospitals of the same ownership type may be more likely to merge because of common objectives and financial arrangements. Including these additional instruments had virtually no effect on the results, as there were too few nonzero values. Other variants that may be correlated with the propensity for colocated hospitals to merge, such as the overlap of particular service offerings, are not time invariant and may not be exogenous to contemporaneous market conditions.[30]

In the main analysis, the market for a given hospital is defined to include all rivals within 7 miles. The number of rival mergers and colocated rival pairs within this circular boundary is then counted. Theoretically, the effect of rival merger should be stronger for closer rivals and weaker for rivals located farther away. Indeed, the point estimates more than double when the market radius is set at 5 miles, while the price effect is small and statistically insignificant when all rivals within 10 miles are included.[31]

Appendix B presents results from a series of alternative specifications, including a model with a negative binomial regression in the first stage and a model with changes in price levels (rather than logs) as the dependent variable. The uniformity of the estimates across the various specifications confirms the initial results: mergers between independent, close rivals lead to dramatic increases in market prices for inpatient care.

7. Conclusions

Observational studies of merger effects are plagued by severe selection bias. To overcome this bias, I propose a combination of rival analysis with instrumental variables. This approach uses the responses of rivals to gauge the competitive

[30] A small number of hospitals did undergo ownership conversions during the study period, but for the vast majority, ownership status is time invariant (as is location).
[31] Note that reducing the market size also reduces the number of observations, as there are fewer hospitals with two or more rivals within a shorter distance.

effects of mergers, instrumenting for whether a rival is exposed to a merger in the first place. Using data on mergers in the hospital industry between 1989 and 1996, I find that hospitals increase price by roughly 40 percent following the merger of nearby rivals.

For these mergers to have increased consumer welfare, they would have had to generate enormous quality improvements. Only one prior study has explored the effect of hospital mergers on quality, and this study finds evidence of slight reductions in quality (Hamilton and Ho 2000). On the other hand, producer welfare appears to have increased substantially, as a result of both price gains (paired with inelastic demand) and potentially large cost reductions (Dranove and Lindrooth 2003).

As with all merger analyses, it is important to recognize that the estimates I obtain reflect the competitive milieux of the mergers in question. The merger effects in this study are identified by responses of nearby competitors to fully integrated consolidations of independent, physically adjacent hospitals. The point estimate should not, therefore, be construed as a measure of the average impact of all hospital consolidations during the study period. Rather, the results offer four key insights. First, comparing price growth of merging firms with price growth of nonmerging rivals is likely to yield substantial underestimates of merger effects in differentiated oligopoly settings. Second, when selection bias is addressed, there is conclusive evidence that mergers of independent hospitals can lead to large increases in area prices, a result that has not emerged from most prior longitudinal studies. Third, the magnitudes of these increases are consistent with predictions generated from structural models of similar settings (Capps, Dranove, and Satterthwaite 2003; Gaynor and Vogt 2003).[32] Although these models have mainly been used to predict price increases of merging firms, the large estimated effects (in concentrated markets) suggest that rivals of these firms could sustain significant price increases. Therefore, the results in this paper validate the use of careful structural modeling to estimate the impact of proposed mergers. Fourth, the analysis reveals that most geographic definitions of hospital markets are too large for urban areas. Fixed-radius definitions of 5–7 miles appear to be more appropriate than the commonly used 15–20 miles (and, by extension, counties or MSAs).

The methodology in this study could be applied to a number of industries that have also experienced merger waves, ranging from independent video stores to retail banks. Various permutations of distance between firms or outlets—whether in product or physical space—could serve as instruments for mergers, assuming they meet the requirement of exogeneity. More generally, research that carefully addresses the endogeneity of merger events or models the appropriate

[32] Using hospital discharge data from California, Capps, Dranove, and Satterthwaite (2003) and Gaynor and Vogt (2003) predict price increases of 10–58 percent for hypothetical mergers in markets with few competitors. These estimates are likely to be downward biased, as the models assume that rivals do not react to the price increases of the merged institution. If prices are strategic complements, the newly merged entity will raise prices more because it anticipates the reaction of its rivals.

counterfactual outcome in the absence of mergers is needed in order to achieve a greater understanding of the effects of consolidations in various settings.

Appendix A

Comparative Statics in Salop Circle Models

In this appendix, I derive the equilibrium price sets for the Salop circle models corresponding to market structures 1 and 2 in the text. I compare these prices to the prices that would arise following a merger between R1 and R2 (in each market structure).

Market 1

Recall that colocated firms are undifferentiated and hence price at cost, which is zero. If t is sufficiently low, H will set price to maximize

$$\Pi_{\mathrm{H}} = p_{\mathrm{H}} \times D(p_{\mathrm{H}},\ p_{\mathrm{R1}} = 0,\ p_{\mathrm{R2}} = 0) = p_{\mathrm{H}} \times 2\left(\frac{1}{4} - \frac{p_{\mathrm{H}}}{2t}\right).$$

H's demand (denoted D) consists of the individuals on either side (hence the multiple 2) for whom transport costs to firms R1 (and R2) exceed p_{H}. The equilibrium price is $p^{*}_{\mathrm{H}} = \frac{t}{4}$.

If t exceeds a threshold level, H will set price so that the marginal consumer earns zero surplus. If t is extremely high, H is totally unconstrained by competition from R1 (and R2) and prices as a local monopolist. In this case, the market will not be fully covered; that is, not all consumers will purchase the product. (This range is unrealistic in this setting because hospital services generate a sufficiently high v relative to t to ensure fully covered markets.) The full solution set is described by[33]

$$p^{*}_{\mathrm{H}} = \begin{cases} \dfrac{t}{4}, & t < \dfrac{8v}{3} \\ 2v - \dfrac{t}{2}, & \dfrac{8v}{3} < t < 3v \\ \dfrac{v}{2}, & t > 3v. \end{cases}$$

If R1 and R2 merge (into R), competition between them ceases, which relaxes

[33] This solution set may not appear intuitive at first blush. When t is small, H's price increases in t as the higher transport cost reduces competition with its rivals. Once t is sufficiently large, H begins to compete with another option available to consumers: no purchase. The marginal consumer's reservation value is a binding constraint on H's price, yielding $p^{*}_{\mathrm{H}} = v - t \times (\frac{1}{2} - \frac{v}{t}) = \frac{t}{2}$. (Note that the marginal consumer is located at $\frac{v}{t}$, as this is the point at which surplus from purchasing from R1 [and R2] equals zero.) Within this intermediate range of t, price decreases in t in order to continue serving the marginal consumer. Finally, when the value of t is extremely high, the firm acts as a local monopolist and is no longer constrained by R1 (and R2). In this range, t does not affect the trade-offs at the margin and therefore does not enter into the monopolist's price.

the constraint on H substantially. For the lowest range of t, H now maximizes

$$\Pi_{\mathrm{H}} = p_{\mathrm{H}} \times D(p_{\mathrm{H}}, p_{\mathrm{R}}) = p_{\mathrm{H}} \times 2\left(\frac{1}{4} - \frac{p_{\mathrm{H}}}{2t} + \frac{p_{\mathrm{R}}}{2t}\right).$$

By symmetry, $p_{\mathrm{H}}^{*} = p_{\mathrm{R}}^{*}$ and takes the following form:

$$p_{\mathrm{H}}^{*} = \begin{cases} \dfrac{t}{2}, & t < \dfrac{4v}{3} \\ v - \dfrac{t}{4}, & \dfrac{4v}{3} < t < 2v \\ \dfrac{v}{2}, & t > 2v. \end{cases}$$

These solutions suggest that a merger of colocated rivals will result in a very sizeable price increase (up to 100 percent) so long as H actively competes with R1 (and R2) (that is, the first two ranges for t).

Market 2

The premerger p_{H}^{*} (and p_{R1}^{*} and p_{R2}^{*}, by symmetry) in market 2 is described by

$$p_{\mathrm{H}}^{*} = \begin{cases} \dfrac{t}{3}, & t < 2v \\ v - \dfrac{t}{6}, & 2v < t < 3v \\ \dfrac{v}{2}, & t > 3v. \end{cases}$$

Assuming that one of the R facilities closes post merger and the other remains in the same location, post merger p_{H}^{*} is

$$p_{\mathrm{H}}^{*} = \begin{cases} \dfrac{t}{2}, & t < \dfrac{6v}{5} \\ v - \dfrac{t}{3}, & \dfrac{6v}{5} < t < \dfrac{3v}{2} \\ \dfrac{v}{2}, & t > \dfrac{3v}{2}. \end{cases}$$

These solutions illustrate why mergers among colocated hospitals are likely to be associated with particularly large price increases. In the realistic range of t for each market (the range in which there is pre- and postmerger competition among hospitals), the price increases in market 2 are much smaller on average than in market 1.[34]

[34] When $v = 1$, the average price increase for market 1 is 66.7 percent, and for market 2 it is 40 percent (the percentage increase varies with t).

Appendix B

Specification Checks

Table B1 presents the coefficients of interest from several specification checks. All models are based on the main specification without state fixed effects. Column 1 repeats the main results as a reference point. Column 2 demonstrates that the results are similar when all hospital and market controls are excluded. Column 3 reveals that censoring of the dependent variable has only a slight effect on the point estimates. Column 4 adds controls for the number of rivals within a hospital's market. Because hospitals with more rivals are more likely to have colocated rivals as well as merging rivals, it is possible that the instrument is also capturing the effect of having more rivals. Theoretically, this could bias the estimate downward, as it would cause a larger first-stage coefficient and a smaller reduced-form coefficient. Column 4 includes individual dummies for markets with 2, 3, . . . , 9, 10–14, and 15+ rivals. The result indicates a small downward bias, if any. Column 5 excludes hospitals that are colocated with other hospitals from the estimation sample (note that the number of colocated rival pairs always excludes the pair to which a hospital belongs, if any). Column 6 uses the fitted values from a negative binomial first-stage regression as the instrument for the number of rival mergers (per Wooldridge 2002). Finally, column 7 uses the change in price levels in place of the change in log prices as the dependent variable. The point estimate of $1,566 (in year 2000 dollars) is equivalent to 1.1 standard deviations of the distribution of price changes during this period and corresponds to a movement from the 25th to the 65th, or the 75th to the 95th, percentiles in price (the same magnitude obtained using the original dependent variable). Yet another specification check (excluded here for brevity) confirms that controlling for the number of rival hospital closures during 1988–97 does not affect the results.

Table B1
Specification Checks

Dependent Variable	ln (1997 Price) − ln(1988 Price) (1)	(2)	(3)	(4)	(5)	(6)	1997 Price − 1988 Price (7)
Number of rival mergers	.376**	.352**	.402*	.408*	.326*	.222*	1,566**
	(.132)	(.132)	(.143)	(.198)	(.122)	(.099)	(563)
Hospital characteristics	Yes	No	Yes	Yes	Yes	Yes	Yes
Market characteristics	Yes	No	Yes	Yes	Yes	Yes	Yes
Censored dependent variable	Yes	Yes	No	Yes	Yes	Yes	Yes
Number of rival dummies	No	No	No	Yes	No	No	No
Excluding colocated hospitals	No	No	No	No	Yes	No	No
Negative binomial in first stage	No	No	No	No	No	Yes	No
N	877	877	877	877	816	877	877

Note. Instrumental variables estimation is used for all models. Prices are inflated to year 2000 dollars using the Consumer Price Index—All Urban Consumers.
* Significant at $p < .05$.
** Significant at $p < .01$.

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