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

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Abstract

Advances in structural demand estimation have substantially improved economists' ability to forecast the impact of mergers. However, these models rely on extensive assumptions about consumer choice and firm objectives, and ultimately observational methods are needed to test their validity. Observational studies, in turn, suffer from selection problems arising from the fact that merging entities differ from non-merging entities in unobserved ways. To obtain an accurate estimate of the ex-post effect of consummated mergers, I propose a combination of rival analysis and instrumental variables. By focusing on the effect of merger on the behavior of rival firms, *and* instrumenting for these mergers, unbiased estimates of the effect of merger on market outcomes can be obtained. Using this methodology, I evaluate the impact of all independent hospital mergers between 1989 and 1996 on rivals' prices. I find sharp increases in rival prices following merger, with the greatest effect on the closest rivals. The results for this industry are more consistent with predictions from structural models than with prior observational estimates.

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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 been consummated. However, these models require extensive assumptions about consumer demand and firm objectives, and they do not fully incorporate rivals' reactions to actions taken by the merged institution. Moreover, the predictions generated by such models can only be validated by analyzing the effects of consummated mergers. To date, the courts have also been more receptive to observational methods that provide "hard evidence" of the likely impact of merger, as in the Staples-Office Depot case.[1]

Most observational or "reduced-form" analyses of the impact of mergers compare the outcomes of merging firms with those of non-merging firms. These estimates suffer from a classical selection problem, as merging firms are likely different from non-merging parties 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 increase average price. Conditional on survival, these firms would likely have increased price even absent a merger. A similar bias could result if a promising product both increases the optimal future price of the product and makes its

[1] In its successful attempt to block this merger, the FTC presented evidence that office supply prices were lowest in markets where all three office supply superstores competed (Staples, Office Depot, and Office Max). Prices were higher in markets with two competitors, and higher still in markets with a single office supply superstore. *Federal Trade Commission v. Staples, Inc. and Office Depot, Inc., 1997.*

manufacturer a more attractive acquisition target. More generally, any omitted factor that is correlated with changes in the outcome measure as well as the probability of merger will generate biased estimates of the impact of merger.

Several studies improve upon the basic differences-in-differences approach by using matching algorithms to identify a superior control group, e.g. Dranove and Lindrooth (2003). Yet another approach, introduced by Eckbo (1983), is to eliminate the merging entities from the analysis entirely and focus on the responses of rivals to the merger "event." If, for example, merging parties exercise their newly-acquired market power by raising price, *ceteris paribus* their rivals will be able to raise price as well. Thus, *rival analysis* compares the outcomes of firms with merging rivals to the outcomes of firms without merging rivals. This comparison also suffers from a selection problem, as firms with merging rivals are different from firms without merging rivals.

This paper improves upon 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 this 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 U.S., a sector that experienced a wave of merger activity between the mid-1980s and late 1990s.

The instrument I propose for merger is co-location. Using the exact latitude and longitude coordinates for each hospital's main address in 1988, I identify co-located or adjacent hospitals, defined as hospitals within .3 miles of each other. Using this criterion, 4.5 percent of general acute-care hospitals in the non-territorial U.S. in 1988 were co-located with at least one other hospital. There are two reasons such hospitals should be more likely to merge: (1) the potential to cut costs through the elimination of duplicate departments is greater, and (2) the ability to increase price is greater because location is a primary differentiating factor for acute care. This prediction is borne out in the data, which shows that co-located hospitals are nearly three times as likely to merge as non-co-located hospitals, a factor that is scarcely diminished by the inclusion of hospital and market-level controls. Thus, *rival* co-location is an excellent instrument for *rival* merger. A rival is defined as another hospital located within a certain distance from the hospital in question, e.g. 5 miles.

The estimates indicate that a rival's merger between 1989 and 1996 resulted in a 65-percentage-point increase in price by 1997 for neighboring hospitals within 5 miles. Prices appear to stabilize thereafter. The magnitude of the increase varies by market and hospital characteristics, with larger effects for markets that are less concentrated initially and hospitals that are geographically closer to merging parties. Failing to instrument for rivals' mergers produces a statistically insignificant estimate of less than 2 percent.

These findings help to reconcile the results from observational studies of hospital mergers, which generally find no effect or a negative effect of merger on price, and structural models of hospital demand, which imply large increases in price as a result of mergers in concentrated markets. The estimates presented here are consistent with the structural predictions in recent papers such as Capps, Dranove and Satterthwaite (2003) and Gaynor and Vogt (2003).

The paper proceeds as follows. Section 2 describes the hospital industry and summarizes prior related research. Section 3 defines the study samples and provides descriptive statistics. Sections 4 and 5 present estimates from the first and second stages of the two-stage least squares rival analysis, respectively. Section 6 explores the sensitivity of the results to alternative specifications, and Section 7 extends the analysis to allow the impact of rival merger to vary by initial market share and market concentration. Section 8 concludes with a discussion of the implications of these findings and suggestions for additional applications.

2 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 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, exacerbated by the rise of managed care in the private sector, triggered an unprecedented wave of mergers, acquisitions, and closures.

Hospital mergers and acquisitions have received a great deal of attention from healthcare economists and antitrust enforcement agencies, in part because of the volume of patients and revenues involved. In 2001, the 5,801 hospitals in the U.S. 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.[2] The localized nature of competition is also a source of concern for antitrust enforcement agencies, as monopoly and oligopoly providers in a given area are able to negotiate higher prices with private insurance companies as well as some public insurance programs. The not-for-profit status of most hospitals, however, presents the possibility hospitals will not choose to exploit post-merger increases in market power. This is an argument that courts have often cited in rejecting attempts to block proposed hospital mergers.

Since 1991, the Department of Justice and Federal Trade Commission have brought 7 hospital merger cases to trial and failed to prevail a single time.[3] After a respite of several years, the FTC recently filed a complaint against the not-for-profit Evanston Northwestern Healthcare Corporation (ENH), alleging that ENH raised prices after acquiring nearby Highland Park Hospital in 2000.[4] *The Economist* reported on the case in July, stating that "so far the courts have been sympathetic to the hospitals' argument that as not-for-profit organizations they would not exploit any market power

[2] *U.S. Statistical Abstract* (2003), Tables 158, 170, and 667.
[3] *FTC Antitrust Actions in Health Care Services and Products*, Washington, DC, October 2003.
[4] The complaint also alleges price-fixing in negotiating managed-care contracts for affiliated physicians. *Evanston Northwestern Healthcare Corporation and ENH Medical Group, Inc.,* File No. 011 0234, Docket No. 9315, February 2004.

gained by mergers. But the courts may become less indulgent as it becomes clear that this argument is bogus," (17 July 2004).

Despite the sustained interest in these mergers – including private lawsuits challenging post-merger price increases -- economists have failed to reach a consensus on the price effects of mergers in this sector. Reduced-form studies find little evidence of price increases, while structural models predict potentially large impacts of simulated mergers. Gaynor and Vogt (2000), Connor and Feldman (1998), and Dranove and Lindrooth (2001) 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 post-merger outcomes. The majority of these studies focus on the cost reductions achieved by merging institutions, as 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 cost, with two recent exceptions, Alexander (1996) and Dranove and Lindrooth (2003). Using data on mergers of previously independent hospitals that operate under a single license post-merger, these studies find cost decreases of 14 and 33 percent, respectively. These are precisely the mergers studied in the analysis below, suggesting that profits may have increased even more than prices.

The pre vs. post pricing studies are fewer in number and generally find price reductions following merger (e.g. Connor, Feldman, and Dowd 1998; Spang, Bazzoli, and Arnould 2001). However, these estimates are plagued by the selection problems described earlier, and 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.

A creative approach by Krishnan (2001) overcomes these problems by comparing price growth for diagnoses in which merging hospitals gained substantial market power (>20%) with diagnoses in which they gained insignificant share (<5%). Using data on 11 independent 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 downward-biased: it eliminates hospital-wide price increases, which are likely because location is a critical component of quality in this market

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 1969-85 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 due to the events selected, the definition of rival chains, and the fact that only a small fraction of hospitals are owned by publicly-traded firms. Connor and Feldman (1998) compare price and cost growth between 1986 and 1994 for non-merging hospitals with merging rivals (hereafter *NMW* hospitals) and non-merging 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 post-merger market share, where a small effect (3 percent over 8 years) is found. This is attributed to the ability of large mergers to dominate the market and suppress rivals' prices through merger-related quality improvements.

The analysis below also explores price changes of non-merging hospitals over a long period of time (1988-1997) and across all states. However, I take several steps to examine and address the main concern that Connor and Feldman acknowledge: markets with and without mergers are different. First, I restrict the sample to non-merging hospitals with 2 or more rivals within a 5-mile radius. The rationale for the 2+ rival requirement is intuitive: if a nonmerging hospital has fewer than 2 rivals, it cannot experience a rival merger. The rationale for the second requirement is that the merger of adjacent hospitals can reasonably be expected to affect the prices of rivals located within fairly tight geographic bounds. Second, I show that even in this restricted sample, price growth for NMW hospitals is significantly less than price growth for NMWO hospitals during the pre-merger period, which suggests that simple comparisons of price growth in these two groups during the merger period will underestimate the true effect of merger. Finally, I introduce rival co-location as an instrument for rival merger.

The results are consistent with recently published studies that use structural demand models to simulate the effects of merger. Using hospital discharge data from California, Capps et al. (2003) and Gaynor and Vogt (2003) predict price increases of 10 to 58 percent for hypothetical mergers in markets with few competitors. These estimates may also 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 newlymerged entity will raise prices more because it anticipates the reaction of its rivals. This response may explain the large average price increases documented below.

3 Data

The primary data sources are the American Hospital Association's (AHA) Annual Survey of Hospitals (1988-2000), Medicare's Prospective Payment System Impact Files (1985-2000), the Medicare Cost Reports (1985-2000), and the Area Resource File (2000). Merger data constructed for Dranove and Lindrooth (2003) was generously provided by the authors. Using the AHA surveys, Dranove and Lindrooth identified 97 hospital mergers between 1989 and 1996, where a merger is defined as a combination of two independent hospitals within the same metropolitan area into a single entity. To qualify as a merger in this dataset, the newly-created hospital must report only one set of financial and utilization statistics and surrender one of their facility licenses.

The main sample consists of hospitals present in the 1988 AHA Survey and located in metropolitan statistical areas or counties with more than 100,000 residents.[5] (Dranove and Lindrooth did not consider mergers outside these areas.) I match financial data and discharge statistics from the 1985-2000 Medicare Cost Reports to the hospitals in this sample. As in several prior studies, average hospital price is calculated as inpatient revenue per case-mix adjusted discharge.[6] Revenues and discharges, obtained

[5] Of the 5,204 hospitals located in the mainland U.S. in 1988, 461 are dropped due to these restrictions.
[6] More precisely, price = [(hospital inpatient routine service revenue + hospital intensive care revenue + hospital inpatient ancillary revenue)*(1-contractual discounts/total patient revenue) – Medicare primary payor amounts – Medicare total amount payable]/[(total discharges excluding swing/SNF – total Medicare discharges excluding swing/SNF)*case-mix index]. All variables are included in the Medicare Cost Reports, with the exception of case-mix index, which is in the Annual Impact Files. Records with negative values for any measure in the price formula or discount factors greater than 1 were dropped, where discount

from the Medicare Cost Reports, exclude Medicare patients because the federal government sets prices for these patients. Hospital-level case-mix indices (CMIs) are only available for Medicare patients, however, so this study follows earlier work in using the Medicare CMI for each hospital (reported in the annual Prospective Payment Impact Files) as a proxy for the non-Medicare CMI.[7] Price data is available for FY1985-2000, which spans the period 3 years before the first recorded merger to three years after the last recorded merger.[8] Because the Cost Reports are not edited for quality, observations in the 5% tails of price in a given year are assigned a missing value for that year.[9] The dependent variables are the change in log price for a given hospital between 1985-1988 (the "pre period"), 1988-1997 (the "treatment period"), and 1997-2000 (the "post period"). All dependent variables are censored at the 5th and 95th percentiles.

Latitude and longitude coordinates for the main address reported by each hospital in the 1988 AHA survey were purchased from geocode.com. I use these coordinates, which contain 6 decimal places and are accurate up to the street segment, to calculate the straight-line distance between hospitals ("as the crow flies"). County-level data on per-capita income in 1990 and the HMO penetration rate (only available for 1998) were extracted from the Area Resource File.

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factor=(1-contractual discounts/total patient revenue). Where multiple records were available for a given hospital and fiscal year (<1% of the sample), the following decision rules were used: 1) drop observations with lower "filing status," 2) drop observations with earlier processing dates; 3) retain the observation with the maximum reporting period.

[7] All Medicare discharge records are assigned to a Diagnosis-Related Group, or DRG. Each DRG has a "weight" that is multiplied by a base amount to determine the reimbursement provided by the Medicare program. The original 1984 weights were constructed so that the average DRG weight for hospitals, called the *case-mix index*, would equal 1.

[8] Fiscal Years vary by hospital and do not necessarily coincide with calendar years.

[9] Between 1985 and 2000, the 5th percentile of the annual price distribution ranges from $1374 to $1664 (in $2000), and the 95th percentile from $6256 to $8333. Price data is available in at least one year for 99% of the hospitals in the 1988 AHA data.

The first column in Table 1 presents descriptive statistics for the sample of hospitals for which all of the independent variables are available (4495 out of 4743 total hospitals, accounting for 99 percent of 1988 discharges). Column 2 contains statistics for the rivals sample. Only *non-merging* hospitals that satisfy the following requirements are included in the rivals sample: (1) two or more rivals within 5 miles in 1988; (2) price data during the pre-period and the treatment period. The rivals sample is subdivided into hospitals with merging rivals (NMW, column 3), and hospitals without merging rivals (NMWO, column 4).

The descriptive data indicate that just under 4 percent of hospitals were party to an independent merger between 1989 and 1996. Nearly 5 percent of hospitals were co-located with at least one other hospital in 1988, using .3 miles as the cutoff distance for co-location. Hospitals in the rivals sample were much more likely to be located in an MSA than hospitals in the overall sample (97 vs. 56 percent), less likely to be government-owned (10 vs. 25 percent), and more likely to offer teaching programs (19 vs. 6 percent).

Within the rivals sample, NMW and NMWO hospitals are more similar on observables, although there are some statistically significant differences. NMW hospitals have a greater share of Medicaid patients (14.6 vs. 11.6 percent) and a greater number of rivals (7.65 vs. 5.06 on average), and they operate in markets that are more heavily penetrated by HMOs (17 vs. 12 percent). Perhaps relatedly, price growth in the three years prior to the merger wave is significantly *lower* for NMW than for NMWO hospitals (-2.9 vs. 3.6 percent). This suggests that NMW hospitals are inappropriate controls for

NMWO hospitals; that is, treating rival mergers as exogenous will produce underestimates of the impact of rival mergers on price.

4 Co-location and the Probability of Merger

Within the raw data, co-location performs quite well as a predictor of merger: the merger rate among the 222 hospitals that are co-located with at least one additional hospital is 9.9 percent, as compared to 3.7 percent among the 4,273 non-co-located hospitals. Table 2 presents the results of a linear probability model that includes the hospital and market-level variables reported in Table 1, as well as individual state dummies to control for differences in state regulatory environments. In this model, location is taken as exogenous. Since there has been virtually no entry in the acute care hospital industry since the Hospital Survey and Construction Act of 1946 (known as Hill-Burton), this seems a reasonable assumption.

The relationship between the probability of merger and co-location is robust to all of the controls: co-location is associated with an increase of 5.8 percent in the probability of merger. As a falsification exercise, I reestimate this model using an indicator for *system merger* as the dependent variable. System mergers are defined by Dranove and Lindrooth as one-to-one consolidations of hospitals that *did not* surrender a facility license and report joint data following the consolidation. The coefficient estimate from this regression is -.002 (.010). As expected, co-location is a good predictor of fully-integrated mergers but not of all merger and acquisition-related activity.

Given the strong relationship between co-location and merger, the relationship between *rival* co-location and *rival* merger in the rivals sample should also be strong. Column 2 of Table 2 reports the results of a linear probability regression of the number of rival mergers on the number of co-located rivals, again controlling for hospital characteristics, market characteristics, and state fixed effects. *Controlling for the total number of rivals* (one of the market characteristics)*,* having one additional co-located rival increases the number of merging rivals by .053, as compared to a mean of .196. This regression constitutes the first stage in the two-stage least squares rival analysis.

For rival co-location to be a good instrument for rival merger, it must also be uncorrelated with unobserved factors related to price growth. To examine whether this condition is satisfied, I regress price growth during the *pre* period on the number of co-located rivals and the controls listed above. The results, reported in column 1 of Table 3, reveal a negative and statistically insignificant relationship between the number of co-located rivals and price growth. Thus, there is no evidence suggesting that price growth before the merger wave was larger for hospitals with co-located rivals.

5 The Impact of Merger on Price

The second stage of the rival analysis is a regression of price growth during the treatment period on the number of co-located rivals and all of the control variables. Price growth is measured as the change in logged price between 1988, the year before the first recorded merger, and 1997, the year following the last recorded merger. Column 2 of Table 3 reports the results of this regression. Each additional co-located rival is associated with a statistically-significant increase of .027 in price growth, as compared to a mean of .006 during this period. Column 3 reports results using price growth in the post period, 1997-2000, as the dependent variable. As in the pre period, there is no relationship between price growth and the number of co-located rivals.

Table 4 presents the IV estimate of the effect of a rival's merger between 1989 and 1996 on price growth between 1988 and 1997. The point estimate is simply the ratio of the second to the first stage coefficient estimates, $.027/.053 \approx .501$, with a standard error of .253. This figure translates into a price increase of approximately 65 percent. Given that the second-stage estimates indicate no relationship between co-located rivals and price growth during the post-merger 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.

Column 2 of Table 4 reports the OLS estimate of the effect of rival merger on price growth. As in Connor and Feldman (1998), I too find no statistically significant impact of a rival's merger on price using OLS. A Hausman specification test easily rejects equality of the two estimates.

6 Robustness

Table 5 explores the sensitivity of the results to alternative definitions for co-location and market size. The first panel demonstrates the effect on the first stage results of an increase or decrease in the co-location cutoff distance. The initial cutoff was arbitrarily set at .3 miles so as to include the merger that inspired the instrument: Beth Israel and Deaconness Hospitals in Boston, MA, whose main addresses are .27 miles apart. The coefficient estimate in the first stage is still sizeable for a distance of .2 miles, but smaller and imprecisely estimated when the cutoff is increased to .4 miles.

The second panel in Table 5 examines the sensitivity of the second-stage results to the market definition. The second stage effectively draws a circle of a given radius around each hospital, and estimates the effect on price of an additional co-located rival within that circle (controlling for the total number of rivals in the circle). Because location is a crucial differentiating factor for hospitals, this effect should be greater the smaller the circle. Indeed, the second-stage estimates are much larger when the radius is restricted to 3 miles, and smaller when the radius is extended to 7 miles.

7 Extensions

The impact of rival merger on price will vary depending upon the structure of the market as well as the characteristics of the individual firms. Although there are no clear theoretical predictions in this differentiated-oligopoly setting with asymmetric firms, it is interesting to explore how the estimated effect of rival merger varies with a hospital's initial market share as well as the degree of concentration in its local market, as measured by the Herfindahl-Hirschman Index (HHI). Among hospitals with merging rivals, the interquartile range for initial market share is .05 –.29, and .15 – .37 for the HHI. [10] Columns 1 and 2 of Table 6 present the results of the IV specification with the addition of interaction terms for the number of merging rivals and initial market share, and the number of merging rivals and initial HHI, respectively. To create instruments for these terms, each is multiplied by the number of co-located rivals.

The results indicate that the largest post-merger price increases occur in markets that are initially less concentrated. In addition, smaller hospitals gain more when their

[10] HHI is defined as the sum of the squared market shares for all rivals within 5 miles.

rivals merge. This result is consistent with a model in which small hospitals are fringe competitors whose prices are more dependent on the behavior of rivals. Overall, Table 6 suggests that antitrust authorities should consider the particulars of a market when evaluating a proposed merger.

8 Conclusions

This study shows that research that treats merger or rival merger as exogenous will likely lead to biased estimates of the impacts of merger. To overcome these biases, I propose a methodology that combines rival analysis with instrumental variables. This methodology uses the response of rivals to mergers to gauge anticompetitive effects, instrumenting for whether a rival is exposed to a merger in the first place. Using data on one-to-one mergers in the hospital industry between 1989 and 1996, I find that hospitals increase price by 65 percent on average when a rival within 5 miles experiences a merger. This effect is diminished when merging rivals are geographically further, the market is concentrated to begin with, or the hospital has a high initial market share.

Caution must be exercised before extrapolating these large estimates to all hospital mergers. The estimates I obtain are based on mergers of co-located hospitals, which enjoy particularly strong post-merger increases in market power because location is a primary differentiating factor in this industry. For these particular 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, both as a result of the price gains (paired with inelastic demand) and large estimated cost reductions (Dranove and Lindrooth 2003).

The methodology employed here 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 rival merger.

It is notable that the estimates presented here are far more consistent with predictions from structural models of demand than estimates from prior observational studies. This finding suggests that structural models may yield superior estimates of the impact of mergers when instruments are unavailable.

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Table 1. Sample Means

	All Hospitals	Rivals Sample: All	NMW	NMWO
Dependent Variables				
1985 price	$3,222	$3,988	$3,932	$3,996
1988 price	$3,405	$4,083	$3,721	$4,132
1997 price	$3,853	$4,103	$3,792	$4,146
2000 price	$3,909	$4,073	$3,972	$4,086
ln(1988 price)-ln(1985 price)	.064	.028	-.029	.036
ln(1997 price)-ln(1988 price)	.132	.006	.016	.004
ln(2000 price)-ln(1997 price)	.012	.001	.039	-.004
Merger Indicators and Instruments				
Merger	3.96%			
Co-located	4.94%			
Rival merger		12.1%	100%	0%
# merging rivals		.196	1.63	0
# Co-located rivals		.718	1.45	.617
Hospital Characteristics				
For-profit	15%	15%	18%	15%
Government	25%	10%	9%	10%
Church-operated	13%	24%	18%	24%
Teaching hospital	6%	19%	17%	19%
Medicare share of discharges	38%	32%	31%	32%
Medicaid share of discharges	11%	12%	15%	12%
Beds	183	347	312	352
Market Characteristics				
# Rivals	2.01	5.37	7.65	5.06
Located in MSA	56%	97%	99%	97%
Ln(1990 Per capita income)	9.72	9.88	9.89	9.88
County HMO penetration (1998)	12%	13%	17%	12%
*N**	4495	730	88	642

Notes: Prices are inflated to year 2000 dollars using the CPI-U. Observations in the 5% tails for price are dropped, and the price change variables are censored at the 95th and 5th percentiles. Rivals are defined as hospitals located within a 5-mile radius.

*In column 1, N for the price data is: 3,806 (1985), 4,034 (1988), 3,468 (1997), and 3,246 (2000). All hospitals in the rivals sample have price data for 1985, 1988, and 1997. 2000 data is available for 74 of the NMW hospitals and 562 of the NMWO hospitals.

Sources: Medicare Cost Reports, Prospective Payment Impact Files, Dranove and Lindrooth (merger data), geocode.com, American Hospital Association, Area Resource File, author's calculations

Table 2. Probability of Merger/Merging Rivals and Co-location (First Stage)

	Dependent Variable	
	Own Merger	# Merging Rivals
Sample	*All Hospitals*	*All Rivals*
Co-located	.058***	
	(.014)	
# Co-located rivals		.053***
		(.018)
Hospital Characteristics		
For-profit	.008	.020
	(.010)	(.067)
Government	-.031***	.007
	(.008)	(.074)
Church-operated	.029***	.004
	(.009)	(.051)
Teaching hospital	.005	-.054
	(.015)	(.064)
Medicare discharge share	-.040	.137
	(.027)	(.228)
Medicaid discharge share	.017	.028
	(.035)	(.217)
Number of beds (00s)	.000	-.017
	(.002)	(.013)
Market Characteristics		
# Rivals	-.002*	.009
	(.001)	(.006)
Located in MSA	.015*	.182
	(.008)	(.128)
Ln(1990 Per capita income)	.004	-.301**
	(.020)	(.135)
County HMO penetration (1998)	.010	.233
	(.020)	(.147)
State Dummies	Y	Y
N	4495	730

*** signifies p<.01, ** signifies p<.05, * signifies p<.10

Table 3. Price Growth and Co-located Rivals
(Second Stage)

	Dependent Variable		
	ln (1988 price) - ln(1985 price)	ln (1997 price) - ln(1988 price)	ln (2000 price) - ln(1997 price)
# Co-located rivals	-.010	.027***	.004
	(.007)	(.010)	(.009)
Hospital Characteristics			
For-profit	-.017	-.055	.067**
	(.027)	(.038)	(.034)
Government	.068**	.027	.018
	(.030)	(.042)	(.037)
Church-operated	-.020	-.106	-.029
	(.021)	(.029)	(.025)
Teaching hospital	-.053**	.007	.001
	(.026)	(.037)	(.032)
Medicare discharge share	.362***	-.467***	.137
	(.093)	(.130)	(.116)
Medicaid discharge share	-.134	.069	.203*
	(.088)	(.124)	(.108)
Number of beds (00s)	.015***	-.012	.015**
	(.005)	(.008)	(.007)
Market Characteristics			
# Rivals	.003	-.006*	-.009***
	(.003)	(.004)	(.003)
Located in MSA	-.109*	-.025	.065
	(.052)	(.073)	(.062)
Ln(1990 Per capita income)	-.007	.001	-.082
	(.054)	(.077)	(.066)
County HMO penetration (1998)	-.030	.120	-.220***
	(.060)	(.084)	(.074)
State Dummies	Y	Y	Y
N	730	730	636

*** signifies p<.01, ** signifies p<.05, * signifies p<.10

Table 4. Estimates of Effect of Merging Rivals on Price Growth

	Dependent Variable is ln(1997 price) -ln(1988 price)	
Estimation	*IV*	*OLS*
# merging rivals	.501**	.016
	(.253)	(.022)
Hospital Characteristics	Y	Y
Market Characteristics	Y	Y
State Dummies	Y	Y
N	730	730

*** signifies p<.01, ** signifies p<.05, * signifies p<.10

Table 5. Sensitivity of Results to Distance Cutoffs

First Stage	Dependent Variable is # of merging rivals		
Co-location definition	*.3 miles*	*.2 miles*	*.4 miles*
Rival definition	*5 miles*	*5 miles*	*5 miles*
# Co-located Rivals	.053***	.045*	.024
	(.018)	(.023)	(.017)
N	730	730	730
Second Stage	Dependent Variable is ln (1997 price) -ln(1988 price)		
Co-location definition	*.3 miles*	*.3 miles*	*.3 miles*
Rival definition	*5 miles*	*3 miles*	*7 miles*
# Co-located Rivals	.027***	.034**	.019**
	(.010)	(.016)	(.009)
	730	484	881

*** signifies p<.01, ** signifies p<.05, * signifies p<.10

Table 6: Extensions

	Dependent Variable is ln (1997 price) -ln(1988 price)	
# merging rivals	.643**	.790*
	(.313)	(.411)
# merging rivals *		
Initial HHI	-3.48**	
	(1.63)	
Initial Market Share		-4.81*
		(2.62)
Hospital Characteristics	Y	Y
Market Characteristics	Y	Y
State Dummies	Y	Y
N	730	730

*** signifies p<.01, ** signifies p<.05, * signifies p<.10