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 nonmerging entities in unobserved ways. To obtain an accurate estimate of the ex-post effect of consummated mergers, the author proposes 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 a merger on market outcomes can be obtained. Using this methodology, she evaluates the impact of all independent hospital mergers between 1989 and 1996 on rivals' prices. She finds sharp increases in rival prices following merger, with the greatest effect on the closest rivals. Results for the hospital industry are more consistent with predictions from structural models than with prior observational estimates.

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, 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 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 changes in the outcome measure as

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[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.*

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well as with the probability of merger will generate biased estimates of the impact of merger.

Some studies extend the 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.[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 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 during the 1990s.

The instrument I propose for merger in the hospital industry is co-location. Using the exact latitude and longitude coordinates for each hospital's main address in 1988, I

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[2] Rival analysis has also been used to infer the competitive effects of other decisions, such as changes in capital structure (Chevalier 1995).

identify co-located or adjacent hospitals, defined as hospitals within 0.3 miles of each other "as the crow files" and no more than 5 blocks apart. Using this criterion, 191 (3.6 percent) of the 5,373 general, non-federal 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: 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 shows that co-located hospitals are nearly three times as likely to merge as non-co-located hospitals, a factor that is scarcely diminished after controlling for a large set of hospital and market characteristics. 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. 7 miles.

The estimates indicate that a rival's merger between 1989 and 1996 resulted in a 40-percentage-point increase in price by 1997 for neighboring hospitals within 7 miles. Prices appear to stabilize thereafter. The price increase is greater for 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 results from observational studies of hospital mergers (e.g. Connor et al 1998), which generally find no effect or a negative effect of merger on price, with forecasts from 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 predictions of 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 first-stage and reduced-form results from the two-stage least squares rival analysis, respectively. Section 6 explores the sensitivity of the results to alternative specifications. Section 7 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 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 administered 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 1983-1988 (Bazzoli et al, 2002).[3] As a result, recent studies of hospital mergers have focused on this time period (e.g. Bazzoli et al. 2002, Dranove et al 2003).

Hospital mergers 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.[4] The localized nature of competition is also a source of concern for antitrust enforcement agencies, as monopoly and oligopoly providers in a given area can negotiate supracompetitive prices with private insurance companies as well as some public insurance programs.

The not-for-profit status of most hospitals, however, presents the possibility that 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.[5] 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.[6]

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[3] These merger counts refer to legal consolidations of two or more hospitals under single ownership, and were verified by the American Hospital Association for 1983-1988, and Bazzoli et al. for 1989-1996.

[4] *U.S. Statistical Abstract* (2003), Tables 158, 170, and 667.

[5] There are at least two distinct arguments espoused in these court rulings. In *Long Island Jewish Medical Center*, the court cited the "genuine commitment" of the merging hospitals "to help their communities." In *Butterworth Health Corporation,* 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." (*Improving Health Care: A Dose of Competition*, *A Report by the Federal Trade Commission and the Department of Justice,* Ch. 4 p. 30)

[6] *FTC Antitrust Actions in Health Care Services and Products*, Washington, DC, October 2003. After a respite of several years, the FTC recently filed a complaint against the not-for-profit Evanston

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. 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 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 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, Dranove and Lindrooth find post-merger cost decreases of 14 percent. 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). 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.

Northwestern Healthcare Corporation (ENH), alleging that ENH raised prices after acquiring nearby Highland Park Hospital in 2000 (*Evanston Northwestern Healthcare Corporation and ENH Medical Group, Inc.,* File No. 011 0234, Docket No. 9315, February 2004). The case is currently before an FTC administrative law judge.

Krishnan (2001) overcomes the selection problem by comparing price growth for diagnoses in which merging hospitals gained substantial market power (>20%) with price growth for 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 many hospital features (e.g. location) are constant across services. In examining hospital responses to diagnosis-specific price changes imposed by Medicare, Dafny (2005) finds little evidence that hospitals compete in quality at the diagnosis level; rather, the data are consistent with competition in overall hospital quality. These results suggest 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 1969-85 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 (Vita and Schumann 1991). 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. 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.

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 steps to examine and address the selection problem that persists in rival analyses of mergers. First, I restrict the sample to non-merging hospitals with 2 or more rivals within a 7-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. These sample restrictions substantially reduce the differences in observable characteristics of NMW and NMWO hospitals. 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 for 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.[7]

3   Data

Merger data constructed for Dranove and Lindrooth (2003) was generously provided by the authors. Using data from the Annual Survey of Hospitals by the American Hospital

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[7] An alternative approach would be to use own co-location 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), increasing the sample size substantially.

Association (AHA), 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 a single set of financial and utilization statistics and surrender one of their facility licenses. Figure 1 graphs the distribution of the mergers over time.[8] Because my instrument only predicts the incidence and not the timing of merger (i.e. 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, non-federal hospitals present in the 1988 AHA Survey and located in metropolitan statistical areas or counties with more than 100,000 residents.[9] (Dranove and Lindrooth did not consider mergers outside these areas.) The AHA data include hospital characteristics such as ownership type (government, not-for-profit, and for-profit), number of beds, and occupancy rate.

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

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[8] Merger figures reported by Cuellar and Gertler (2003) for 1994-2000 reveal a steep dropoff in merger activity in 1997, and a steady decline thereafter.
[9] Of the 5,373 general, non-federal hospitals located in the mainland U.S. in 1988, 466 are dropped due to these restrictions.

As in several prior studies, average hospital price in a given year is calculated as inpatient revenue per case-mix adjusted discharge. In calculating price, I exclude Medicare revenues and discharges 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 as a proxy for the non-Medicare CMI. The Medicare CMIs are reported in the annual Prospective Payment Impact Files, which can be downloaded from the CMS website.[10] The variables needed to calculate price are available for FY1985-2000, which spans the period 3 years before the first recorded merger to three years after the last recorded merger.[11]

Because the Cost Reports are not edited for quality, observations in the 5-percent tails of price in a given year are assigned a missing value for that year.[12] 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. I also construct two indicators of financial distress using the 1988 Cost Reports: the share of patients covered

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[10] CMS uses the distribution of each hospital's Medicare admissions across roughly 500 Diagnosis-Related Groups, or DRGs, to construct its annual CMI. 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. The data is available at http://www.cms.hhs.gov/providers/hipps/hist_impact_94-04.asp.

[11] 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/SNF – total Medicare discharges excluding swing/SNF)*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 above formula was constructed with the guidance of Cost Report experts at CMS. Records with discount factors outside of [0,1] or negative values for any measure in the price formula are excluded.

[12] 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 $8334. Price data is available in at least one year for 99% of the hospitals in the 1988 AHA data.

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. Estimates of county-level HMO penetration in 1994 were provided by Laurence Baker.[13]

Latitude and longitude coordinates for the main address reported by each hospital in the 1988 AHA survey were purchased from geocode.com. Using these coordinates, which contain 6 decimal places and are accurate up to the street segment, I calculate the straight-line distance between hospitals ("as the crow flies"). After identifying 213 hospitals located within 0.3 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 co-located hospitals to 191. In section 6, I illustrate the robustness of the first stage to alternative distance cutoffs.[14]

The first column in Table 1 presents descriptive statistics for the sample of hospitals for which all of the independent variables are available (4,487 out of 4,907 total hospitals, accounting for 91 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 co-located with at least one hospital.[15] Column 2 contains statistics for the

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[13] These estimates were constructed using data from the Group Health Association of America.
[14] For the purposes of identifying co-located hospitals and counting rivals, all hospitals with valid addresses are included; sample restrictions are applied after this step is complete.
[15] The sample includes at least one of the merging hospitals for 94 of the 97 independent mergers. Note that all 194 merging hospitals are present when rival merger counts are constructed, as missing data for merging hospitals is irrelevant for the rival analysis.

sample used in the rival analysis. Only *non-merging* hospitals that satisfy the following criteria are included in this sample: (1) two or more rivals within 7 miles in 1988; (2) price data during the pre-period and the treatment period. Hospitals in the rivals sample are 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. 26 percent), and more likely to offer teaching programs (14 vs. 6 percent).

The rivals sample is subdivided into hospitals with merging rivals (NMW hospitals, N=118, column 3), and hospitals without merging rivals (NMWO hospitals, N=759, column 4). NMW and NMWO hospitals share similar observable characteristics, although there are some statistically significant differences. NMW hospitals have a greater share of Medicaid patients and a larger number of rivals, and they operate in markets with slightly higher HMO penetration rates (24 vs. 21 percent, on average). Price growth in the three years prior to the merger wave is significantly *lower* for NMW than for NMWO hospitals (-2.9 vs. 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.

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 for co-located hospitals is 11.0 percent, as compared to 3.7 percent for non-co-located 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. Note that in this model, location is taken as exogenous. As 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 6 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 as one-to-one consolidations of hospitals that *did not* surrender a facility license and report joint data following the consolidation. The coefficient estimates from these regressions are small and statistically insignificant.[16] As expected, co-location is a good predictor of fully-integrated mergers but not of all merger and acquisition-related activity. Hence, the point estimates pertain only to these particular types of mergers.

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. Table 2, column 3 reports the results of a linear regression of the number of rival mergers on the number of co-located rival pairs, again controlling for hospital and market characteristics. Column 4 adds state fixed effects. These specifications reveal that having one additional pair of co-located rivals is associated with an increase of 0.11 in the number of rival mergers, as compared to a mean of 0.16. This regression constitutes the first stage in the two-stage least squares rival analysis.

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

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 rival pairs and the controls listed above.[17] The results, reported in columns 1 and 2 of Table 3, reveal a negative and statistically insignificant relationship between the number of co-located rival pairs and price growth. Thus, there is no evidence suggesting that price growth before the merger wave was greater for hospitals with co-located rivals.

5   The Impact of Merger on Price

The reduced-form of the rival analysis is a regression of price growth during the treatment period on the number of co-located 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 recorded merger, and 1997, the year following the last recorded merger. Results from the reduced-form are reported in columns 3 and 4 of Table 3. Each additional pair of co-located rivals is associated with a statistically-significant increase of 0.045 in price growth, as compared to a mean of 0.010 during this period. The estimate falls slightly to 0.034 (.015) with the inclusion of state fixed effects. Columns 5 and 6 report 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.

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[17] Regressions for pre-period price growth use hospital covariates from 1985. Regressions for the treatment period use covariates from 1988, and regressions for the post-period use covariates from 1997.

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 reduced-form and first-stage coefficient estimates, $0.045/0.119 \approx 0.380$, with a standard error of 0.132. This figure translates into a cumulative price increase of approximately 46 percent (37 percent using the model with state fixed effects).[18] 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 (the distribution of price growth is skewed right). Given there is no relationship between co-located rival pairs 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.

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 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

Table 5 explores the sensitivity of the results to alternative definitions for co-location and changes in market boundaries. IV estimates without state fixed effects are reported for all combinations of these definitions and boundaries.[19] The results are fairly insensitive to the co-location definition, with statistically-significant point estimates ranging between

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[18] $e^{0.0380} \approx 1.46$

[19] Results with state fixed effects are similar and available upon request.

0.33 and 0.53. The Mapquest corrections eliminate a small amount of noise in the co-location measure, but this noise does not appear to be systematic. In the (unreported) first-stage regression using 0.3 miles as the co-location definition (i.e. eliminating the 5-block restriction), the coefficient on co-located rival pairs is 0.117 (.017), as compared to 0.119 (.018) for the Mapquest-corrected version (reported in Table 2).

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 co-located rival pairs within this circular boundary are then counted. Theoretically, the effect of rival merger should be stronger for closer rivals, and weaker for rivals located further 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.[20]

The Appendix presents results from a series of alternative specifications, including a model without any controls, and a model using a negative binomial regression in the first stage. 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.

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[20] Note that reducing the market size also reduces the number of observations, as there are fewer hospitals with 2+ rivals within a shorter distance.

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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 anticompetitive effects of mergers, 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 substantially following the merger of rivals within 7 miles. The point estimate of approximately 40 percentage points is consistent with predictions from structural models of hospital choice.[21]

Caution must be exercised when extrapolating these estimates to hospital mergers in general. The estimates I obtain are based on mergers of co-located hospitals, which enjoy especially strong post-merger increases in market power. 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 potentially large cost reductions (Dranove and Lindrooth 2003).

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[21] 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 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.

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, assuming they meet the requirement of exogeneity.

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

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Appendix

The following table 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 if all controls are excluded, suggesting that the coefficient estimates are not biased by omitted variables. 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 co-located rivals as well as merging rivals, it is possible that the number of co-located rival pairs 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,…9,10-15, and 15+ rivals. The result indicates a small downward bias, if any. Column 5 excludes hospitals that are co-located with other hospitals from the estimation sample (note the number of co-located rival pairs always excludes the pair to which a hospital belongs, if any). Finally, Column 6 uses the fitted values from a negative binomial first-stage regression as the instrument for the number of rival mergers.

Table A1. Specification Checks

	ln (1997 price) -ln(1988 price)					
	(1)	(2)	(3)	(4)	(5)	(6)
Number of rival mergers	.380***	.352***	.407**	.418**	.331**	.230**
	(.132)	(.132)	(.143)	(.199)	(.122)	(.099)
Hospital characteristics	Y	N	Y	Y	Y	Y
Market characteristics	Y	N	Y	Y	Y	Y
Censored dependent variable	Y	Y	N	Y	Y	Y
Number of rival dummies	N	N	N	Y	N	N
Drop co-located hospitals	N	N	N	N	Y	N
Use neg. binomial in first stage	N	N	N	N	N	Y
N	877	877	877	877	877	877

Notes: All models are estimated by 2SLS.
*** signifies p<.01, ** signifies p<.05, * signifies p<.10