Economics at the FTC: Retrospective Merger Analysis with a Focus on Hospitals

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Abstract   Economists at the Federal Trade Commission (FTC) support the agency's competition and consumer protection missions. In this year's essay we discuss efforts at the FTC and elsewhere to examine empirically the competitive effects of mergers. This work has ranged from subjective interview-based reports on post-merger behavior to more objective analyses of post-merger performance based on rigorous empirical analysis of prices. In this essay we discuss the merger retrospective literature generally, and focus on the FTC staff's recent empirical analyses of consummated hospital mergers.

Keywords   Antitrust · Consumer protection · FTC · Mergers · Merger retrospectives

1 Introduction

The Federal Trade Commission's Bureau of Economics (BE) is composed of about 70 Ph.D.-level economists, a small group of accountants, and 25 other staff (including research analysts). Its work supports the FTC's competition (antitrust) and consumer protection missions. Most of the Bureau's work is related directly to the Commission's law enforcement activities (i.e., investigations and litigation), but FTC economists also help promote competition-oriented policies domestically at the state and federal levels, and contribute to the global adoption of modern, economically oriented competition policies. Finally, and most relevant to this essay, the Bureau's staff engage in policy-oriented economic research.

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Last year's contribution to the Antitrust and Regulatory Update issue of this Review discussed a variety of topics, including the Google-DoubleClick merger, resale price maintenance, mortgage disclosures, and the effects of credit scoring on the pricing of automobile insurance policies to minorities. This year, too, FTC economists have been active in many areas. For example, the Bureau worked with international organizations to help refine and beneficially coordinate cross-border competition and consumer policies. FTC economists also participated in training programs for economists from overseas antitrust agencies.

In non-merger antitrust, we continued our efforts to understand better the positive and normative aspects of vertical restraints, focusing particularly on minimum resale price maintenance (RPM). In 2007, the US Supreme Court eliminated the long-standing policy of per se illegality of minimum RPM.[1] In response, the FTC held workshops to inquire further into the theoretical analysis of RPM, and to review relevant empirical evidence from both the United States and other nations.[2] Because RPM had been illegal per se in the US since 1975, there is little recent empirical evidence on the actual effects of private RPM programs.[3] The closest parallels tend to come from non-US government-mandated and enforced programs of price control that differ substantially from the privately adopted and enforced distribution controls that will be the subject of antitrust review in the US.[4]

In November 2008 we hosted our first annual academic-style Industrial Organization conference, conducted jointly with Northwestern University's Center for the Study of Industrial Organization and the Searle Center. Thanks in large part to the

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[1] Leegin Creative Leather Products, Inc. v. PSKS, Inc., 127 S. Ct. 2705 (2007). Dr. Miles Med. Co. v. John D. Park & Sons, 220 US 373 (1911) established initial illegality, but intervening laws (the Miller-Tydings Act and the McGuire Act) in 1937 and 1952, respectively, authorized the states to allow RPM via "Fair Trade Laws". Those laws were repealed in 1975 by the Consumer Goods Pricing Act of 1975, Public Law 94–145, 89 Stat. 801 (1975).

[2] For information on the FTC's recent RPM workshops and the presentations by economists and attorneys, see http://www2.ftc.gov/opp/workshops/rpm/.

[3] See, e.g., Ippolito (1991), and Ippolito and Overstreet (1996) for earlier analyses of minimum RPM. Also see Cooper et al. (2005) for a review of existing empirical evidence on minimum RPM.

[4] In 1997, France enacted a law known as the *Loi Galland*. The *Loi Galland* prevents *any* retailer from selling any product below its wholesale "invoice price." Furthermore, the invoice prices established by manufacturers are non-negotiable, and cannot differ across retailers, which means that for a given product, the same price floor applies to *all* retailers. Compliance with this law is enforced by the imposition of heavy fines. Unsurprisingly, Biscourp et al. (2008) found that this law sharply increased retail prices.

In the UK, book publishers were permitted to use the "Net Book Agreement" (NBA). According to a recent Office of Fair Trading publication (2008, p. 5), "approximately 1900 publishers used the NBA to restrict the retail price of books in the UK, thus preventing retailers from selling a book under the publisher's chosen (net) price. Any retailer that deviated from the agreement would be refused the supply of future books by *all* publishers [emphasis added]." As the OFT notes (2008, p. 20), "[t]his collective nature of the enforcement of the NBA constituted an unusual example of 'collective resale price maintenance'."

The openly collective (and in the French case, mandatory and government-enforced) nature of these two pricing arrangements is very different from the types of privately negotiated, privately enforced minimum resale price agreements that likely will be the subject of antitrust review in the US (see, e.g., In the Matter of Nine West Group Inc., FTC Docket No. C-3937 http://www.ftc.gov/os/caselist/c3937.shtm, in which the FTC allowed footwear manufacturer Nine West to engage in minimum RPM agreements with its dealers). We doubt therefore whether the British and French evidence provides much useful information about the competitive effects of this latter set of agreements.

Scientific Committee for the conference (Susan Athey, Patrick Bajari, John List, Carl Shapiro, and Scott Stern), we attracted a stellar set of participants. Topics included the economics of privacy and Internet behavior, experiments and behavioral economics, and demand estimation and network economics. A second IO conference is scheduled for November 19–20, 2009. Our call for papers solicits contributions on a number of applied microeconomic topics that are relevant to the FTC's enforcement missions, including dynamic demand estimation, mergers, distribution practices, bundling, loyalty discounts, intellectual property, online advertising, information disclosure, consumer credit, and behavioral and experimental economics.

While FTC economists are interested in all of these topics, most of our work centers on issues related to merger enforcement. The dollar volume of general merger and acquisition (M&A) activity fell substantially as the credit crunch that began in mid-2007 continued to affect the macroeconomy adversely. Mergerstat reported that US M&A activity was about $0.7 TR in 2008, compared with $1.4 TR in the peak year of 1999.[5] Still, we reviewed 21 mergers in great depth in fiscal year 2008, and the agency challenged all or some aspect of 15 of those transactions. We also continued to make our enforcement efforts more transparent by releasing additional aggregated merger enforcement data for the past decade, and by producing a report examining how efficiency claims were handled in recent merger investigations.[6]

Thus, this year's essay stresses our continuing work on retrospective merger analysis, especially the Bureau's recent studies of consummated hospital mergers. Section 2 briefly discusses merger retrospectives in general, while Sect. 3 focuses on those in the US hospital industry.

2 Merger Retrospectives

A retrospective merger analysis attempts to determine *ex post* how, if at all, a particular merger affected equilibrium behavior in one or more markets. This is a challenging task. In principle, a thorough retrospective analysis of a merger might have to examine outcomes in all of the (perhaps many) markets affected by the transaction. For example, in banking mergers, prices of many deposit and loan products might be affected; in airline mergers, network effects might imply that a merger could affect even those antitrust markets where the merging entities did not compete directly. Multiple dimensions of competition might have to be examined (including product output, product quality, product variety, innovation, etc., for both the affected firms and their rivals). And the study might have to extend several years before and after the deal to allow sufficient time for any merger effects to appear, but not so long that the merger effects become confounded with other market shocks.

Analyzing a merger retrospectively necessarily involves modeling and estimating a counterfactual. If (as is usual) one studies a consummated merger, the counterfactual

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[5] MergerStat Review 2009.

[6] The aggregate merger data covering the years 1996 through 2007 are available at http://www.ftc.gov/os/2008/12/081201hsrmergerdata.pdf. The examination of merger efficiencies by Coate and Heimert (2009) is available at http://www.ftc.gov/os/2009/02/0902mergerefficiencies.pdf.

is "what would have happened if the merger had not taken place?" While there are alternative methods for estimating this counterfactual, the customary approach uses a comparison/control group—a set of firms, products, or markets that, ideally, would be unaffected by the merger, but in other respects behave just like those affected by the merger.

The choice of mergers to study also raises important questions. Usually a merger retrospective will consider one (or at most a small set of) consummated merger(s). To address whether merger enforcement policy should be tightened or loosened, many scholars have sought to identify mergers "at the margin," i.e., mergers that plausibly were anticompetitive or nearly so, but that nonetheless were not successfully challenged for a variety of idiosyncratic reasons. Carlton (2009) observes that in some respects, this research strategy is not well-matched to this policy question. He has recommended an alternate strategy [already used in some papers, such as Prager and Hannan (1998)] of studying all (or a representative set of) consummated mergers in a particular industry.

Having picked a set of mergers to study, what does one do? The simplest, and sometimes least costly, empirical approach is to ask knowledgeable industry participants (especially customers) after the fact whether the merger affected prices or outputs. This historical survey-intensive approach to retrospective assessment now has become common, and often it is supplemented with an extensive review of the original case files and the collection of objective aggregated data covering the pre-merger and post-merger periods. Possibly the first attempt at such an interview-based retrospective assessment is the work discussed in March 1990 by then-FTC Chairman Janet Steiger.[7] She described the analysis of four horizontal mergers from the late 1980s that were unchallenged "close calls" for the agency. Those cases were in oil refining, hospitals, pedal harps, and engine bearings. None of the reviews detected an anticompetitive price effect, and in some cases evidence of possible efficiencies was found. Several recent studies sponsored by other competition agencies have taken a similar approach, although the specific method used varies.[8]

FTC economists have produced recent studies of this sort. For example, Breen (2004) examined the efficiency claims made by the merging parties in connection with the Union Pacific/Southern Pacific railroad merger. Drawing on post-merger information provided by the firms, and an extensive post-merger review of the transaction by the Surface Transportation Board, he concluded that many of the efficiencies promised by the firms at the time of the investigation ultimately were realized. Breen did not, however, examine all aspects of the merger, so he did not reach a conclusion regarding the overall effects of the transaction.

Recently, Chen (2009) examined developments in the baby food industry after the FTC blocked the attempted merger of Heinz and Beech-Nut in 2000, an enforcement

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[7] See Steiger (1990).

[8] This work includes reviews of several mergers in diverse industries and more extensive examination of one merger in the power cable industry. See the UK Office of Fair Trading and Competition Commission (2005, 2009) and the work on the power cable systems market performed for the European Commission et al. (2006). As with the FTC's merger retrospectives, these studies were in part intended to help the agencies determine if their initial analyses were correct.

decision that has been described as "controversial."[9] Unfortunately, estimating the impact of a counterfactual merger is harder than estimating the counterfactual of no merger. Still, it is interesting to explore the possible effects of blocking a proposed merger.[10] Chen found that the ownership of all three major players changed over the past eight years. The Gerber brand increased its market share from 72 to 80% in traditional jarred baby food; one rival (Beech-Nut) retained its 12% share; another rival brand (Nature's Goodness, previously owned by Heinz) declined from a 13% share to a 2% share; and a small niche firm featuring organic products (Earth's Best) grew to a 6% share. Real prices per-unit remained unchanged since 2000. A new, publicly funded Beech-Nut plant is in construction in upstate New York. In terms of product development, the industry has moved toward greater use of plastic jars, organic product lines have expanded, and yogurt has become a bigger part of the baby feeding business.

These survey-intensive studies have two intrinsic weaknesses: (1) the subjective nature of the evidence and analysis concerning what *did* happen post-merger; and (2) the non-rigorous method for predicting what *would have* happened absent the merger. Consequently, these studies often are unconvincing.

Accordingly, many researchers have attempted to analyze transactions where they can find objective, detailed data on variables (typically prices) of interest to merger enforcers, and where the counterfactual outcome can be more rigorously estimated or characterized. These requirements often have led researchers to analyze mergers in industries where the firms compete in multiple markets (which facilitates the creation of "control" markets), and for which data are readily available (e.g., collected by a government agency or by a private vendor such as Nielsen or OPIS). In particular, many retrospectives have examined mergers in airlines, banking, oil, consumer goods, and, as stressed below, in the hospital industry.

Pautler (2003), Hunter et al. (2008), Weinberg (2008), and Ashenfelter et al. (2009) review this literature in great detail. As noted, most retrospective studies examine transactions on the "margin" of antitrust enforcement: acquisitions that were perceived to raise potential antitrust issues, but did not trigger a successful enforcement action for a variety of idiosyncratic reasons. These studies typically do not examine the price effects of all attempted or completed mergers, or even of all attempted or completed horizontal mergers (although a few airline and banking merger studies cover a wide range of mergers in those industries). Nor does most of this work inform us about the non-price effects of mergers.[11]

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[9] According to Baker (2009, p. 162), the decision to challenge the transaction was controversial within the FTC, in part because the firms mounted a vigorous efficiency defense. Baker reports that both the economics and legal staffs recommended that the transaction not be challenged. The Commission decided to seek an injunction only by a vote of 3 to 2.

[10] For example, several merger retrospective studies have found that prices rose in anticipation of a horizontal merger; if such a merger is unexpectedly blocked, one might expect to find that prices then fall.

[11] An exception is Vita and Sacher (2001), who conducted several tests of the hypothesis that the Dominican Santa Cruz-AMI Community hospital merger may have increased the quality of care at the merged entity. Certain other retrospective studies in hospitals or railroads examine industry costs, and some studies of drug markets examine various non-price dimensions of competition.

The FTC's Bureau of Economics has produced many of these analyses. Among the first was the Barton and Sherman (1984) study of two mergers in microfilm production, where data from competitive bidding for microfilm was used as a cost control.[12] Later, Schumann et al. (1992) conducted an ex-post evaluation of three mergers, including one in the titanium dioxide industry. Because the industry was not characterized by local markets, they could not define a control group by looking at the market in an otherwise similar area with no merger. Instead, they used a reduced form time series model of industry pricing to simulate the "but-for" pricing in the no-merger counterfactual.

More recently, FTC economists have completed a number of retrospective analyses of horizontal and vertical transactions in oil-related markets. Simpson and Taylor (2008) analyzed Marathon Ashland Petroleum's (MAP) 1999 acquisition of the Michigan assets of Ultramar Diamond Shamrock (UDS), while Taylor and Hosken (2007) studied the refining and marketing joint venture of Marathon and Ashland that originally formed MAP. Taylor et al. (2007) analyzed the Thrifty/ARCO transaction involving retail gasoline stations. The first two studies found no adverse price effects for consumers, and the third found a small and statistically insignificant adverse price effect.[13]

FTC economists have analyzed mergers in numerous other industries. For example, Ashenfelter and Hosken (2010) studied the competitive effects of five mergers involving various consumer products and found price increases associated with four of the transactions.[14]

The FTC is not alone in studying merger outcomes. Economists at the Department of Justice and several academics also have conducted retrospective merger analyses.[15] So far, the literature has produced a range of results. Merger retrospectives that use case studies, or samples based on "close call" mergers that were not blocked, have repeatedly found post-merger price increases. Although no recent published census of the literature exists, it is almost surely true that price increases are found over half the time. Substantial post-merger price increases have been found in mergers between publishers of academic and legal journals. Price increases also have been frequently detected following mergers in hospital and airline markets. In addition, small adverse effects on consumers have been routinely observed following mergers in the banking industry. Finally, modest price increases have been detected following mergers in various branded consumer goods markets.

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[12] The paper originally appeared as Bureau of Economics Working Paper #98 in August 1983.

[13] The General Accounting Office (2004) also examined various oil industry mergers, but did not provide convincing evidence of post-merger price increases. See the FTC conference website and a 2004 critique at http://www.ftc.gov/ftc/workshops/oilmergers/index.shtm.

[14] The transactions were: Pennzoil's purchase of Quaker State motor oil; Proctor and Gamble's purchase of Tambrands; General Mills' purchase of Chex cereal brands; the combination of liquor giants Guiness and Grand Metropolitan; and Aurora's (Mrs. Butterworth) purchase of Log Cabin breakfast syrup. Most of the merger retrospective studies mentioned above used unaffected local markets to obtain control groups. Ashenfelter and Hosken made use of product market variation, in the form of private label products, to provide the experimental control.

[15] Mergers in several other industries areas have also recently been examined by academic authors, including mergers in pharmaceuticals, scholarly journals, telecommunications, newspapers, and railroads.

On the other hand, the nascent literature on drug industry mergers indicates that these mergers have not produced consistent effects (for good or for ill) on a range of measures of performance and R&D outcomes. Similarly, studies of mergers in the US oil industry have produced no evidence of significant adverse price effects in the antitrust markets that were the focus of the studies.

Indeed, beneficial effects have been uncovered in several retrospective merger studies. For example, in the small number of studies that examined post-merger cost effects, cost decreases following mergers have been found in hospital campus consolidations, in railroad consolidations, and in backroom operations of banks. Studies of bank mergers also have found evidence of better risk-matching for bank customers. In addition, consumer price decreases have been seen in Italian banking, although the effects take three years to be visible. Furthermore, efficiencies have been uncovered following certain airline consolidations, including alliances that fall short of outright mergers. Most of the mergers examined in this literature were not challenged by the antitrust authorities, so one might not have expected to see consistent or large price increases.

3 The FTC's Hospital Merger Retrospectives

In 2001, Vita and Sacher published a study of a 1990 merger between two Santa Cruz (California) hospitals using a set of similar hospitals as the control. They found a significant post-merger price increase at both the acquiring hospital and its principal rival. This study served as a model for three more recent studies that we discuss here.

In 2009, the FTC's Bureau of Economics released three working papers that analyzed the competitive effects of four consummated hospital mergers. These transactions were: (1) Evanston Northwestern Healthcare's (ENH) purchase of Highland Park Hospital (HPH) in Highland Park, Illinois, in 2000; (2) the merger (also in 2000) of St. Therese Medical Center (STMC) and Victory Memorial Hospital (VMH), in Waukegan, Illinois; (3) Sutter's 1998 acquisition of Summit, a nonprofit hospital located in Oakland, California, which combined Summit with Sutter's Alta Bates hospital in Berkeley, California; and (4) the 1998 acquisition by New Hanover Regional Medical Center ("New Hanover") of Columbia Cape Fear Memorial Hospital ("Cape Fear") in Wilmington, North Carolina. These studies were part of the FTC's Hospital Merger Retrospectives Project. This project studied the competitive effects of consummated hospital mergers with two interrelated goals: to identify potential targets of FTC enforcement actions; and better to inform the FTC "about the consequences of particular transactions and the nature of competitive forces in health care."[16]

Although hospital mergers, like any merger between competitors, potentially can affect both price and nonprice aspects of competition, these four studies focused on whether the mergers caused inpatient prices to private payers to increase anticompetitively. All four studies used essentially the same empirical method: compare the change in inpatient prices (from pre-merger to post-merger) at the merged hospitals, to the corresponding change in prices over the same period at a group of "control" hospitals, chosen to have similar characteristics to the merging hospitals, but to be

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[16] See Muris (2002, especially pp. 9–10).

relatively unaffected by the transaction. The studies did not examine the impact of the transactions on outpatient prices, nor did they analyze the effects of the transactions on non-price competition (e.g., quality of care).[17]

Beyond shedding light on merger outcomes in general, these studies can help address one long-standing question in antitrust policy: a substantial number of US hospitals are organized as "not-for-profit" (NFP) entities. As Phillipson and Posner (2009) recently observed, "[t]he fact that NFP firms cannot distribute profits to their 'owners' has persuaded some judges and scholars that such firms are not as interested in exploiting market power as [for-profit] firms are." This belief has contributed to the unsuccessful efforts by the FTC and the Department of Justice to challenge proposed mergers between NFP hospital competitors.[18] By examining the impact of such mergers retrospectively, scholars can test directly the validity of this conjecture.

3.1 Empirical Methodology

3.1.1 Difference-in-Differences

Although scholars have used differing techniques to estimate the effects of consummated mergers, the most popular technique—and the technique used in most of the recent FTC studies—is the "difference-in-differences" ("D-I-D") method. This research approach attempts to mimic, to the greatest possible extent, the design of controlled experiments (Meyer 1995). This technique long has enjoyed widespread use in other areas of economics (most importantly, labor economics),[19] but it is also sometimes well-suited to the analysis of other events, such as changes in market structure.

Applied to hospital mergers, the D-I-D method compares the change in prices (pre-merger to post-merger)[20] at the merged hospitals to the change in price over the same period at a group of "control" hospitals similar to the merging hospitals, but not affected by the transaction. In regression terms, the analyst estimates some version of the following equation:

$$\ln p_i = \alpha + \beta \times M_i + \gamma \times \text{POST}_i + \delta \times M_i \times \text{POST}_i + \lambda \times X_i + \varepsilon_i$$

where $\ln p_i$ = the log of the price charged for an admission i; $M_i = 1$ if admission i is at a merging hospital, 0 otherwise; $\text{POST}_i = 1$ if admission i occurs in post-merger period, 0 otherwise; X_i = a vector of characteristics for admission i, such as age and sex of patient; type of insurance plan (e.g., PPO, HMO); diagnosis code for admission i; and hospital type (e.g., teaching, for-profit, public);

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[17] The investigations of these transactions (and any litigation) would have addressed those issues.

[18] See Richman (2007).

[19] Many examples are provided by Imbens and Wooldridge (2009, especially pp. 67–71).

[20] In merger policy, the terms "pre-merger" and "post-merger" are often used to mean "without the merger" and "with the merger." It is sometimes clarified that this usage does not mean chronologically prior to and after the merger. Here we *do* mean the latter. Because chronologically pre- and post-merger data play an important role in most merger retrospectives that seek to estimate actual-versus-counterfactual pricing, there is scope for confusion in this usage.

$\varepsilon_i =$ error term.

In this specification, the parameter δ is the D-I-D estimate of the merger effect. If the estimated coefficient δ is to provide a valid measure of the price effect of the merger, a number of conditions must hold (see Meyer 1995, pp. 152–153, for a thorough discussion). Probably most important among these is the suitability of the "control" group. Ideally, the control group should consist of firms that closely resemble (in terms of cost, demand, and competitive environment) the merged entities, but which were unlikely to have been affected by the merger.[21] As discussed below, selecting good control groups was a major challenge in the recent FTC studies.

3.1.2 Data

All of the hospital studies used data on actual amounts paid for private inpatient admissions. These data were obtained via subpoena from both the merged entities and from the private payers with which the merged entities had contracts during the pre- and post-merger periods. The payers also supplied data on admissions at hospitals in the control groups. As an additional check on data validity, the authors also employed Medicare Cost Reports and, where available, data from state Public Health Departments, which often collect information on hospital inpatient admissions.

Constructing "price" and other basic variables for empirical analysis is a much more formidable task in hospital markets than in other markets (e.g., airlines, banks, oil) where retrospective studies have been conducted, as Haas-Wilson and Garmon 2009, (pp. 16–18), discuss. The starting point is the private payer claims data. In those data, the unit of observation is a "claim," which corresponds to a particular procedure or service. A single hospital admission—the unit of observation that is the ultimate focus of the empirical analysis—generally will consist of many "claims," so the analyst must aggregate (using patient ID numbers and dates of admission/discharge) these multiple claims to determine the amount paid for an entire "admission." Typically, the payer data contain information on: (1) the amount paid by the insurer; (2) the amount paid by the patient; (3) information about the patient (e.g., age, sex); and (4) information about the admission (e.g., admission length, diagnosis codes, and procedure codes). The patient- and admission-specific information enables the analyst to control for the extraordinary degree of heterogeneity in hospital admissions that doubtless accounts for much of the observed variation in "prices" across hospitals and over time. If this heterogeneity changes over time, it will be correlated with the merger; failing to control for it could bias the estimated merger effect. In addition, failure to control for these other factors could cause the estimates to be so imprecise that no effect is found even if one exists.

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[21] If the equation correctly captured all of the effects of hospital characteristics this would be unnecessary; and if it came close to doing so, then the inclusion of a larger control group would be worthwhile, even encompassing non-comparable hospitals. The prevailing judgment is that the equation and estimation techniques cannot be expected to do such a good job; and thus, although it is prudent to include hospital characteristics in the equation, one should seek to limit the control group to closely comparable (other than involvement in the merger) hospitals.

The analysis also takes account of hospital-specific and payer-specific characteristics that could affect prices. Typically, this involves incorporating either categorical or continuous variable measures of attributes such as: Medicare and Medicaid patient share, teaching status, for-profit status, total bed size, overall casemix, and type of insurance plan (e.g., PPO, HMO, or indemnity). Controlling for these factors reduces the error variance and may (if they are correlated with the merger) reduce bias in the estimate of the merger effect.

3.2 Evanston Northwestern/Highland Park and St. Therese/Victory

On January 1, 2000, Evanston Northwestern Healthcare (ENH)—a 500-bed two-hospital system consisting of a teaching hospital in Evanston, Illinois, and a community hospital in Glenview, Illinois—purchased the 160-bed Highland Park Hospital (HPH), its nearest rival to the north. On February 1, 2000, Provena St. Therese Medical Center (STMC) and Victory Memorial Hospital (VMH), both community hospitals located in Waukegan, Illinois, combined to form Vista Health. All four hospitals were operated as not-for-profit entities.

The FTC opened formal investigations of both transactions in 2002. Finding no evidence of actual anticompetitive effects, the FTC closed its investigation of the Waukegan transaction in 2004.[22] However, that same year the FTC issued an administrative complaint against ENH, alleging that its acquisition of Highland Park reduced competition, resulting in higher inpatient prices.[23] The case was tried before an administrative law judge. The judge held that the transaction indeed had allowed the merged hospitals to raise price anticompetitively. As a remedy, he ordered ENH to divest Highland Park. On appeal, the FTC Commissioners (acting in their appellate role) affirmed the administrative law judge's finding that the merger violated the Clayton Act; however, the FTC eschewed divestiture in favor of a remedy that required separate contracting for the Evanston Northwestern and Highland Park hospitals, subject to binding arbitration.[24]

Haas-Wilson and Garmon (2009) analyzed the competitive effects of both transactions using four years of claims data obtained from the five largest private payers in the Chicago area. Haas-Wilson and Garmon found it somewhat difficult to create an "ideal" control group (i.e., hospitals similar to the merged entity, but unaffected by the transaction). They addressed this problem by carrying out the analysis with multiple control groups, arguing that "if the results are robust across multiple control

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[22] See http://www.ftc.gov/os/caselist/0110225/040630ftcstatement0110225.shtm.

[23] See http://www.ftc.gov/opa/2004/02/enh.shtm.

[24] On October 16, 2007, a group of eight health economists led by David Dranove filed a brief comment questioning the wisdom and viability of the behavioral remedy. The FTC noted that conduct remedies are not often preferred in merger situations, but in this case, the agency accepted the remedy as final on April 28, 2008. See Brief *Amicus Curiae* of Economics Professors and Opinion of the Commission on Remedy, each in the matter of Evanston Northwestern Health Care Corporation, Docket No. 9135. Undoubtedly, it is often difficult to craft an effective and efficient remedy for an anticompetitive merger after-the-fact, which was a major reason for the enactment of the Hart-Scott-Rodino Act of 1976, which required pre-merger notification to the FTC and the DOJ.

groups, we can be confident that the measured net price change at the merged hospitals adequately controls for demand and cost shocks in the area."

Their first, and broadest, control group (Control Group 1) consisted of all non-federal general acute-care hospitals in the Chicago Primary Metropolitan Statistical Area (PMSA). As they note, using this control group poses two potential sources of bias. First, some of these hospitals were themselves involved in mergers during the relevant time period; this will tend to mask anticompetitive effects of the merger under study if the other mergers were also anticompetitive, but to exaggerate such effects if the other mergers were pro-competitive (led to lower prices among the control group). Second, some of these hospitals may have changed their prices in response to any merger-induced price changes by the merged entity; this will tend to bias towards zero the estimates of any effect of the merger.

To deal with the first source of possible bias, Haas-Wilson and Garmon also created a second control group, consisting of all non-federal general acute-care hospitals in the Chicago PMSA that *were not* involved in mergers between 1996 and 2002 (Control Group 2).[25]

Because ENH is a teaching hospital, Haas-Wilson and Garmon also created two additional control groups of teaching hospitals specifically for the ENH/HPH analysis to account for the cost shocks that teaching hospitals faced in this time period. The first consisted of non-federal general acute-care hospitals in the Chicago PMSA that had residency programs at the time of the merger (Control Group 3). The second consisted of all of the non-federal general acute-care hospitals in the Chicago PMSA that had more than 0.25 residents and interns per staffed bed between 1998 and 2002 (Control Group 4). There were thus four control groups for the ENH/HPH analysis.

Last, because STMC and VMH are nonteaching community hospitals, Haas-Wilson and Garmon also created a control group consisting of all the non-teaching nonfederal general acute-care hospitals (i.e., "community hospitals") in the Chicago PMSA as a control group specifically for the STMC/VMH price change estimates (Control Group 5). There were thus three control groups for the STMC/VMH analysis.

Haas-Wilson and Garmon estimated Equation 1 above on a payer-by-payer basis for the five major payers in the Chicago area. Their key results are presented in Tables 1 and 2 below. They found that in the case of the ENH-Highland Park merger, four of the five payers experienced large and statistically significant price increases. This finding was robust both to the choice of control group, as well as to the choice of casemix adjustment.[26] For the STMC/VMH merger, the results were very different. There, only Payer D experienced consistent post-merger price increases relative to the control groups; Payer A experienced significant increases relative to two of the

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[25] Haas-Wilson and Garmon considered creating a control group of non-merging hospitals from outside the Chicago area to avoid this bias from rival effects. They decided against this option because they concluded that there are no healthcare markets in downstate Illinois that are sufficiently similar to Chicago, and also because several of the insurers doing business in Chicago had few contracts elsewhere in the state, thus limiting the amount of data available on these downstate hospitals.

[26] Haas-Wilson and Garmon used six different methods to adjust for casemix complexity. Here, we have presented their results, which correspond to adjustment via "All Patient Refined Diagnosis Related Groups" (APDRG)/"Severity of Illness" (SOI) dummy variables. Their results are robust in both magnitudes and significance with respect to variations in the method of casemix adjustment.

Table 1 Estimated percentage post-merger price change at ENH/HP hospital

	Control Group 1	Control Group 2	Control Group 3	Control Group 4
Payer A	23.1	35.1	24.9	30.4
Payer B	17.2	26.5	16.3	17.8
Payer C	−0.8	3.8	−0.8	0.2
Payer D	55.7	64.9	50.1	48.7
Payer E	11.0	20.1	12.2	15.2

Source: Haas-Wilson and Garmon (2009) (Table 2, column 1). The estimated changes are different from zero at $p = 0.01$ level of significance, *except* for payer C, for which none of the estimated changes are different from zero at conventional levels of significance, and for Control Group 4 for Payer E, which is significant at the 0.10 level. Control Group 1 consists of Chicago PMSA hospitals; Control Group 2 consists of non-merging Chicago PMSA hospitals; Control Group 3 consists of Chicago PMSA teaching hospitals; and Control Group 4 consists of major teaching hospitals in the Chicago PMSA

Table 2 Estimated percentage post-merger price change at STMC/VMH hospital

	Control Group 1	Control Group 2	Control Group 5
Payer A	6.1[b]	10.7[a]	4.3
Payer B	−15.6[a]	−11.6[a]	−16.0[a]
Payer C	−6.7[a]	−5.4[a]	−8.1[a]
Payer D	18.9[a]	28.4[a]	26.9[a]
Payer E	−21.7[a]	−19.7[a]	−20.8[a]

Source: Haas-Wilson and Garmon (2009) (Table 3, column 1). Superscripts on coefficients: "a" = significantly different from zero at $p = 0.01$ level; "b" = significantly different from zero at $p = 0.05$ level. Control Group 1 consists of Chicago PMSA hospitals and Control Group 2 consists of non-merging Chicago PMSA hospitals. Control Group 5 consists of Chicago PMSA community hospitals

three control groups; and the other three payers (Payers B, C, and E) experienced post-merger price *decreases* relative to the control groups.

3.3 Sutter-Summit

In 1998, the Sutter hospital network acquired Summit, a non-profit hospital in Oakland, California. Sutter owned Alta Bates Medical Center, a 551 bed general tertiary care hospital located in neighboring Berkeley. Summit operated Summit Medical Center, a 534 bed general tertiary care hospital in Oakland, less than 3 miles away. Because Alta Bates and Summit Medical Center were the only two tertiary care hospitals serving the general population in the Berkeley-Oakland area,[27] this proposed acquisition raised competitive concerns, even though there were other hospitals in the area and both merging hospitals were non-profits. The California Attorney General filed suit

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[27] Tenn (2008) provides a more detailed discussion of the differences and similarities between the two merging institutions. He concludes that the two hospitals were very closely positioned to each other not just geographically, but in product space as well. One other general hospital existed in the area, but it served only Kaiser insurance patients.

Table 3 Estimated percentage post-merger price change at Summit Medical Center/Alta Bates

	Summit Medical Center	Alta Bates Medical Center
Payer 1	23.2[a]	7.1
Payer 2	24.8[b]	−8.7
Payer 3	50.4[c]	−2.4

Source: Tenn (2008, Table 2). Superscripts on coefficients: "a" = significantly different from zero at $p = 0.01$ level; "b" = significantly different from zero at $p = 0.05$ level; "c" = significantly different from zero at the 0.06 level

to block the transaction, but the motion for a preliminary injunction was denied on December 27, 1999, on the grounds that (1) the relevant geographic market was much broader than the "Inner East Bay" alleged by the Attorney General, and (2) that Summit Medical Center was a failing hospital, with no other potential purchasers.

Tenn (2008) analyzed the competitive effects of this transaction by applying the D-I-D method to pricing and admissions data obtained from the merged entity and from three large private payers. He constructed control groups by starting with urban, non-government, general service hospitals with at least 200 beds. He removed hospitals that recently had been involved in a merger, and any hospitals in the same metropolitan statistical area as these merged hospitals. This yielded, depending on the payer, a large (between 40 and 71) set of potential control hospitals.

Table 3 summarizes Tenn's empirical findings. He found that Summit's price increase relative to the control group ranged from 23 to 50%, depending on the insurer. All of these estimated price changes were different from zero at the 0.06 level or better. By contrast, Tenn did not find a statistically significant price increase at Alta Bates for any of the payers. Indeed, the estimated price change to Payer 2 was −8.7%, albeit statistically insignificant.

Why did the Summit price increase so dramatically post-merger, compared to the control group, while the Alta Bates price may not have increased at all? Tenn speculates that this asymmetry might reflect the fact that Alta Bates was a large provider of hospital services to commercial insurance patients, while Summit was not. Accordingly, competition from Alta Bates was important pre-merger in constraining Summit's prices to private insurance payers. However, the converse was less important: Summit attracted relatively few commercial patients, suggesting that Alta Bates' pre-merger pricing was principally constrained by non-Summit hospitals. In other words, the diversion ratio (for commercial patients) from Summit to Alta Bates would have been large, whereas the diversion ratio from Alta Bates to Summit would have been small. Other things equal, this situation would lead one to expect a large post-merger price increase at Summit, and a smaller post-merger price increase at Alta Bates, consistent with Tenn's results.

3.4 New Hanover/Cape Fear

In 1998, New Hanover Regional Medical Center ("New Hanover") acquired Columbia Cape Fear Memorial Hospital ("Cape Fear"). The two hospitals were located about

Table 4 Estimated percentage post-merger price change at New Hanover/Cape Fear Hospital

	New Hanover/Cape Fear
Payer 1	56.5[b]
Payer 2	65.3[a]
Payer 3	7.2
Payer 4	−30.0[a]

Source: Thompson (2009, Table 4). Superscripts on coefficients: "a" = significantly different from zero at $p = 0.01$ level; "b" = significantly different from zero at $p = 0.05$ level

6 miles apart in Wilmington, North Carolina; the next closest hospital was about 20 miles away. New Hanover was a large (546 bed) public non-profit hospital that offered a wide range of primary, secondary, and tertiary services. By contrast, Cape Fear was a small (109 bed) community hospital that offered only general acute care services.

Thompson (2009) analyzed this transaction using essentially the same empirical framework as Tenn (2008), and Haas-Wilson and Garmon (2009). She obtained admissions data from New Hanover hospital and from four large private payers. Her control group was drawn from the set of 12 urban hospitals in North Carolina with over 400 beds. One of these hospitals was eliminated because it had been involved in a merger with a geographically proximate rival during the sample period; another two hospitals were eliminated for some payers because those payers did not contract with them during the sample period, yielding a control group of eleven hospitals for three of the payers and nine hospitals for the other payer.

Thompson's results, shown in Table 4, were much more mixed than was the case in the other three studies discussed above. Her results suggest that payers 1 and 2 experienced very large post-merger price increases (57 and 65%, respectively, both significant at the 1% level), while payer 3 experienced an estimated increase of 7.2% that was not statistically significant at conventional levels, and payer 4 enjoyed a substantial (−30%) price *decrease* (significant at the 1 percent level). Like the other authors, Thompson subjected her data to a wide variety of sensitivity tests for model specification, event windows, control groups, and data sources (see Thompson 2009, pp. 13–15). Her results did not vary qualitatively in response to these modifications.

3.5 What Can We Learn From the FTC Hospital Retrospective Studies?

The FTC studies of consummated hospital mergers yield several insights for antitrust enforcers and policymakers. First, the studies corroborate the findings of Vita and Sacher (2001), that mergers between not-for-profit hospitals can result in substantial anticompetitive price increases. All of the four transactions discussed above involved not-for-profit entities, and in two of the four cases, the studies obtained powerful empirical evidence that the mergers were followed by substantial post-merger price increases that cannot reasonably be attributed to other causes.

Second, the studies indicate that hospital competition can be highly localized. The two mergers with the strongest evidence of anticompetitive price effects (Evanston-Northwestern/Highland Park and Sutter/Summit) occurred in large metro-

politan areas with numerous other hospitals. Both a generous verbal market definition and some conventional quantitative methods for delineating hospital geographic markets (e.g., the "Elzinga-Hogarty" test)[28] would likely suggest quite a large geographic market, and hence quite a small corresponding index of concentration (e.g., the HHI); such indicia would normally be taken to suggest that the merger would be unlikely to reduce competition. Indeed, the judge invoked precisely this reasoning in denying California's request for a preliminary injunction in the Sutter/Summit case.[29] The results reported in Tenn (2008) and Haas-Wilson and Garmon (2009) suggest that this analysis was flawed.

Third, these and similar studies may provide the foundation for evaluating various methods for prospective evaluation of proposed mergers. During the past decade, it has become common for economists to predict the effects of horizontal mergers through the explicit estimation of a structural model of industry competition (see, e.g., Werden and Froeb 1994; Capps et al. 2003; Gaynor and Vogt 2003). Economists are now attempting to assess empirically the predictive performance of these simulation models by comparing predicted to actual outcomes for consummated mergers. Peters (2006) conducted such an exercise for consummated airline mergers, while Weinberg and Hosken (2008) have examined consummated mergers in motor oil and maple syrup. Balan and Brand (2009) have begun the task of evaluating structural models of hospital competition through Monte Carlo exercises; the next logical step in that line of research would be to simulate an actual hospital merger (e.g., ENH/Highland Park) and compare the predicted and actual price changes. An open question is how the simpler "upward pricing pressure" index proposed by Farrell and Shapiro (2008) performs in these respects. Similarly, one can use retrospectives to gauge the predictive accuracy of other indicia used or advocated for use in prospective merger analysis, such as customers' views of the merger or experts' assessments of the ease of entry.

4 Conclusion

Merger retrospectives take many forms, ranging from relatively simple and inherently subjective interview-based reviews of a merger's outcome to highly complex, data-driven estimation of post-merger effects. The increasing availability of detailed data now allows for more rigorous empirical merger retrospectives. The Bureau of Economics at the FTC is taking advantage of this improvement to provide retrospective evidence when good candidates present themselves. The recent hospital work provides three new additions to the literature on merger outcomes. This literature indicates that price increases are not uncommon following mergers, notably in the banking, hospital, and airline industries. As Carlton (2009) has noted, given that prospective merger review is not perfect, one would expect to find that *some* mergers raise price even under a correctly calibrated standard; but the literature appears to find that such price increases are common. Unless those results are somehow biased, they may indicate that merger policy in recent decades has been too weak, at least within some industries.

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[28] Elzinga and Hogarty (1974).

[29] California v. Sutter Health System, 130 F. Supp. 2d at 1124 (N.D. California, 2001).

The diversity of results across industries, however, is substantial, as the oil industry retrospectives reveal, and this suggests a different lesson: rather than simply asking whether horizontal merger control should be tightened at the margin, we can use retrospective studies to improve our ability to evaluate proposed mergers by better understanding the factors that drive pricing.

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