In the Shadow of Antitrust Enforcement: Price Effects of Hospital Mergers from 2009-2016*

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Abstract

We examine 558 hospital mergers during a period of increased antitrust enforcement. Using US data on commercially insured patients from 2009-2016, we estimate an average price effect of roughly 5% with a smaller effect for mergers later in the sample. Mergers between hospitals that were substitutes for patients, in unconcentrated insurance markets, and less likely to lead to efficiencies had higher price increases. Using administrative data on merger investigations, we estimate higher than average price increases for mergers selected for more detailed investigation and find no evidence of higher than average price increases for non-reportable mergers.

1 Introduction

Recently, many observers have called for increased study and scrutiny of antitrust policy surrounding mergers in the United States. Some researchers have used merger retrospective analyses to argue that merger policy has been too permissive (Kwoka (2014)). Other researchers show that there are a significant number of mergers that are escaping the notice of antitrust authorities and leading to competitive harm (Wollmann (2019, 2021)).

These calls for reform have been particularly prominent in health care. The prices that private health insurers pay hospitals increased rapidly during the past twenty years, relative to prices for other health care services and relative to government benchmarks (for example Medicare prices) (Cooper et al. (2019b), Whaley et al. (2020)). At the same time, hospital merger activity also increased, particularly over the last ten years, despite the increased merger enforcement in the

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healthcare sector by the Federal Trade Commission (FTC) since 2008 (Cooper and Gaynor (2021)). This confluence of events has led some health policy experts to call for additional resources for antitrust enforcement (Cooper and Gaynor (2021)) or direct regulation of hospital prices (Chernew and Pany (2021)). However, to our knowledge, there has not been significant research on the relationship between antitrust policy, including the investigative process itself, and the price effects of hospital mergers. The increase in merger enforcement in the healthcare sector by the FTC since 2008 presents an opportunity to examine this relationship.

Using a large national database of commercial healthcare claims and administrative data on FTC and Department of Justice (DoJ) hospital merger investigations, we assess the price effects of hospital mergers from 2009-2016. We address two main questions in this paper. First, what was the average price effect of hospital mergers over this period and which mergers led to higher price increases? Second, how did the price effects of hospital mergers vary by the level of antitrust scrutiny prior to the merger?

To answer the first question, we estimate average price effects of hospital mergers at the hospital level and examine which mergers are most likely to lead to price increases based upon observable characteristics (for example diversion ratios between the merging hospitals, state-level concentration in the health insurance market). Overall, hospitals that merged with at least one other hospital no more than 400 miles away during this period experienced price increases of 5% on average, relative to controls. The average price effects were higher for hospitals that were closer substitutes for patients and for hospitals in states with lower insurer concentration. We also find some evidence that mergers between big and small systems, which could be more likely to lead to efficiencies, led to price decreases, while mergers between big systems, which could be more likely to lead to increased bargaining ability or leverage, led to price increases. We find that the price effects of hospital mergers, on average, are not fully manifested until four or five years following the merger, which is consistent with staggered post-merger renegotiation of reimbursement contracts.

Second, we find a nuanced picture regarding the role of antitrust enforcement in mitigating price increases. We test whether mergers that were required to notify the government under the Hart-Scott-Rodino (HSR) Act have different price effects from those that were not. The positive price effects of hospital mergers despite increased enforcement may be due to a lack of pre-merger reporting to the antitrust agencies for many hospital mergers (Wollmann (2019)). However, we find no evidence of a difference in average price effects between mergers that were reported under the HSR Act and mergers that were not reported. Therefore, we find no evidence that the lack of merger reporting led to post-merger price increases. This is consistent with the possibility that insurers or other entities informed the antitrust agencies of potentially anticompetitive hospital mergers regardless of filing status.

Among mergers that were reported under HSR, we find larger average price increases for mergers where in-depth subpoenas (that is "Second Requests") were issued. This suggests that while the antitrust enforcers were able to identify potentially problematic mergers, for a range of reasons, these mergers were allowed to consummate, though perhaps with some remedy. For mergers without an in-depth subpoena, we find larger price increases for mergers in which the investigation was terminated before the end of an initial waiting period ("Early Termination") than for mergers where the investigation continued through the end of the waiting period. We find suggestive evidence that this result is driven by the greater likelihood of early termination for bigger hospital systems.[1]

As noted above, 2008-2016 was a period of increasing antitrust enforcement of hospital mergers, and our long panel permits a preliminary examination of how price effects evolved over this period. We find suggestive evidence that the price effects of mergers declined over this period. While hospitals that merged in 2009 had average price effects of approximately 9% three years following the merger, for hospitals that merged in 2013, the three-year average price effect was only 1%. Mergers in the latter period were more likely to involve close competitors and more likely to involve independent hospitals or smaller systems joining larger systems. The decline in price effects with this change in composition may indicate a shift toward more cost efficient mergers, even as diversion ratios between merging parties increased. While these findings are consistent with increased antitrust enforcement leading to the lower price effects, we regard them as only suggestive and leave further examination of this effect for future work when additional data will permit a more complete examination.

In this paper, we bring together administrative data from hospital merger investigations conducted by the FTC and DoJ and insurance claims data from the Health Care Cost Institute (HCCI). The HCCI data allow us to obtain prices and estimate demand based upon the actual filed insurance claims. The administrative data on hospital merger investigations allow us to evaluate the price effects based upon accurate information on merger filing status and the status of the investigation. While other papers use merger reporting thresholds to determine filing status in other industries

[1] In February 2021, the FTC and DoJ announced the temporary suspension of discretionary Early Termination.(FTC, 2021)

(Wollmann (2019)), using administrative data on actual merger filings is superior, particularly in the hospital context due to the regulatory complexities associated with HSR filing for not-for-profit entities - including many hospitals. Also, while other papers have used administrative data on investigation status in a descriptive manner (Coate, 2018), we are unaware of studies that relate investigation status to average merger outcomes such as price effects.

To assess the price effects of hospital mergers, we use the difference-in-differences approach of Sun and Abraham (2021), which estimates the average treatment effect in the presence of heterogeneous cohort-level treatment effects and staggered adoption. To avoid compounding merger effects, we focus our results on hospitals that were only likely to be impacted by one merger during this period. We also consider three potential control groups: (i) hospitals that were not involved in a merger during this period; (ii) hospitals that were involved in a merger in 2016 (eliminating them from the treatment group); and (iii) a set of synthetic control hospitals.

We caution that our estimated price effects are due to mergers that were actually consummated – which is conditional on the enforcement policies that were in effect. We do not model how the composition of mergers may change in response to different enforcement policies. A full analysis of policy reforms would need to take those considerations into account.

In this paper, we contribute to the literature that studies the price effects of hospital mergers. Garmon (2017) documents nine mergers of competing hospitals that led to statistically significant price increases relative to controls, including seven that occurred between 2008 and 2012. Cooper et al. (2019a) study 366 hospital mergers that occurred between 2007 and 2011 (a sample that partially overlaps ours) and find that prices increased over 6% on average for mergers of geographically proximate hospitals, price effects decline as the distance between merging hospitals increases, and price effects increase over time after the merger. Dafny et al. (2019) study 332 mergers between 1996 and 2012–focusing on cross-market combinations of hospitals that are not direct competitors for patients–and estimate average price increases of 7%-9% for within-state mergers. Arnold and Whaley (2020) conduct a MSA-level analysis of hospital mergers over the period 2010-2016 and find that, on average, mergers caused a 2.6% increase in MSA-level prices.

We also contribute to the literature studying the role of antitrust institutions on economic outcomes. Wollmann (2019) argues that following the 2000 reform that raised the filing threshold for mergers, there was an increase in potentially anticompetitve mergers. Wollmann (2021) argues that the large number of dialysis mergers below the HSR threshold led to substantially worse patient outcomes due to the large number of quality reducing anticompetitive mergers that occurred "under the radar".

The paper proceeds as follows. In Section 2, we discuss the history of antitrust enforcement for hospital mergers. In Section 3, we describe our data. In Section 4, we detail our estimation strategy for the hospital-level merger analysis. In Section 5, we describe the relationship between post-merger price effects and the characteristics of mergers and merging hospitals, and in Section 6, we describe the relationship between price effects and the merger review process. Section 7 concludes with a discussion of the implications and limitations of our analysis.

2 Background

In the late 1980s and 1990s, private health insurers developed managed care health plans with selective contracting. These health plans negotiate discounts with select health care providers in exchange for a larger volume of patients steered toward the providers with favorable patient cost-sharing. The ability of managed care health insurers to negotiate discounts with hospitals depends on the presence of competitive alternatives. Economic theory predicts that mergers of closely competing hospitals increase the bargaining leverage of the merging hospitals in the negotiations with managed care insurers and lead to higher prices. (Town and Vistnes (2001), Capps et al. (2003), Gaynor and Vogt (2003))[2]

Recognizing this dynamic, the antitrust agencies began to challenge anticompetitive hospital mergers in the late 1980s. Initially, the FTC and DoJ were successful in preventing most of the hospital mergers they challenged (Greaney (1997), Scheffman et al. (2003)). However, this initial success was short-lived. By the end of the 1990s, the FTC and DoJ had lost eight straight hospital merger challenges. In 2000, the California Attorney General lost an additional hospital merger challenge (Ashenfelter et al. (2011)). By the turn of the century, effectively there was no antitrust enforcement for hospital mergers due to the leniency of established precedence. Two issues led courts to view the government's arguments with skepticism during this period. First, the use of inflow and outflow thresholds to establish antitrust geographic markets (that is the Elzinga-Hogarty Test) led courts to accept broad geographic markets, resulting in the conclusion that sufficient post-merger competition existed to prevent the exercise of increased market power. Second, most of the hospital merger challenges of the 1990s involved mergers of competing non-profit hospitals. The courts accepted

[2] We note, however, that contracts between hospitals and insurers that actually include terms such as steering mechanisms or network exclusions are not necessary for a merger between competing hospitals to increase the merging hospitals' bargaining leverage.

the defendants' argument that, even if the merger lessened competition substantially, the merged hospitals would not exercise their market power through higher prices because they are non-profit organizations focused on community welfare (Capps (2014)).

In the early 2000s, the FTC changed course by initiating in-depth retrospective studies of certain consummated hospital mergers and challenging the consummated acquisition of Highland Park Hospital by Evanston Northwestern Healthcare (ENH) based in part on post-merger evidence of price increases (Abelson (2002), Capps (2014)). The retrospective studies established that, in certain cases, merging non-profit hospitals exercised their enhanced market power by negotiating higher prices with managed care insurers. In addition, the studies established that mergers of closely competing hospitals in large metropolitan areas can lead to higher prices, even though inflow/outflow methods imply large antitrust geographic markets in these cases, casting doubt on the usefulness of such methods for hospital geographic market definition (Vita and Sacher (2001), Ashenfelter et al. (2011), Haas-Wilson and Garmon (2011), Tenn (2011), Thompson (2011)). The successful challenge of the ENH/Highland Park acquisition–involving non-profit hospitals located in a large metro area–effectively reversed the hospital merger legal precedence established in the 1990s.

While the FTC was conducting its retrospective studies and early enforcement, health economists developed new models and tools to better predict the price effects of hospital mergers by framing the analysis in the context of bilateral negotiations between hospitals and managed care insurers. These new tools included the change in consumer surplus associated with a hospital system's inclusion in an insurer's network (commonly referred to as Willingness-to-Pay (WTP)) [3], diversion ratios between merging hospital systems, [4] and merger simulations based on hospital/insurer negotiations (Town and Vistnes (2001), Capps et al. (2003), Gaynor and Vogt (2003)). Subsequent research found that these tools are more accurate in predicting the price effects of hospital mergers than the established concentration measures used in merger challenges (for example the HHI) (Dranove and Ody (2016), Garmon (2017), Balan and Brand (2022)).

Following the ultimate resolution of the ENH/Highland Park litigation in 2007, the FTC embarked on a renewed hospital merger enforcement program using the new economic tools of hospital merger analysis. Starting with the FTC's challenge of the proposed acquisition of Prince William Hospital by Inova Health in 2008, the FTC challenged 15 general acute care hospital mergers over the next 15

[3] Appendix A describes how WTP and WTP per patient are calculated.

[4] In the context of hospital mergers, the diversion ratio is based on the hypothetical network exclusion of a given hospital system, that is its removal from the patient's choice set.

Table 1: FTC Hospital Merger Challenges Since 2008

Year of Complaint	A Side	B Side	State
2008	Inova Health	Prince William	VA
2011	Phoebe Putny Health	Palmyra Park Hospital	GA
2011	ProMedica Health	St. Luke's Hospital	OH
2011	OSF Healthcare	Rockford Health	IL
2012	Reading Health	Surgical Institute of Reading	PA
2013	Capella Healthcare	Mercy Hot Springs	AR
2015	Advocate Health	Northshore University	IL
2015	Cabell Huntington Hospital	St. Mary's Medical Center	WV
2016	Penn State Hershey	Pinnacle Health	PA
2020	Hackensack Meridian Health	Englewood Healthcare	NJ
2020	Jefferson Health	Albert Einstein Healthcare	PA
2020	Methodist Le Bonheur	Saint Francis	TN
2022	RWJ Baranbas Health	Saint Peter's Healthcare	NJ
2022	HCA	Steward Healthcare	UT
2022	Lifespan	Care New England	RI

Source: Authors' analysis

years. Nine of these challenges occurred between 2008 and 2016, with the remaining six challenges occurring during 2020-2022. The FTC was successful in preventing merger consummation in twelve of these challenges, including seven of the nine challenges in 2008-2016. The two challenged mergers that consummated in 2008-2016 were aided by state action antitrust immunity (Phoebe Putny Health-Palmyra Park Hospital in 2011 and Cabell Huntington Hospital-St. Mary's Medical Center in 2015). Thus, over the past 15 years, the FTC lost only one hospital merger challenge based on the competitive merits (Jefferson Health-Albert Einstein Healthcare in 2020) and lost none during 2008-2016. Table 1 contains a list of the 15 FTC hospital merger challenges since 2008.[5]

In the context of this renewed and largely successful hospital merger enforcement program by the FTC, our results show the association between merger characteristics and price effects. This type of analysis is important to understand the role of past enforcement in reducing post-merger price increases and in targeting future enforcement resources. When interpreting our results, it is important to note that while our data cover nine FTC challenges during 2008-2016, two of the seven successful challenges were not finally resolved until 2016 (Advocate-North Shore and Hershey-Pinnacle). Moreover, two of the remaining five successful FTC challenges during 2008-2016

[5]During this time, the FTC's overall enforcement budget per Hart-Scott-Rodino merger filing was decreasing – see Appendix Figure OA1.

were not fully resolved until 2012 (OSF-Rockford) and 2014 (ProMedica).[6] Hence, our analysis provides a snapshot of the effect of the FTC's hospital merger enforcement program, but any effects likely continued and were amplified following our study period.

3 Data

3.1 Sources

To analyze the price effects of hospital mergers over time, we use data on hospital characteristics, insurance market structure, merger filing and antitrust investigation status, hospital prices, and patient characteristics. We outline each of these below.

Hospital characteristics We use the American Hospital Association's annual survey for data on hospital characteristics including the number of beds, the number of technologies, the number of nursing FTEs, and the 5-digit ZIP code of the hospital. To construct the set of all hospital mergers during 2009-2016, we use data from Cooper et al. (2019a) to generate a list of mergers based on changes in hospitals' system affiliation.[7] We then manually inspected the resulting list of mergers using internal FTC data on merger filings as well as internet searches. Finally, we match our hospital data to the HCCI inpatient claims database using an encrypted data merge facilitated by the HCCI.

Insurance Market Structure We obtain data on state-level health insurance market structure for large group plans from the Kaiser Family Foundation.[8] We focus on the data from just one year, 2013, since we find that there is little variation in state-level insurance market structure over time.

Merger Filing and Investigation Status We use administrative data from the FTC on mergers that were filed with the federal government under the HSR Act. This act mandates that all mergers involving transfers of assets above $200M (which is adjusted annually) and some mergers involving transfers of assets between $50M-$200M must be filed with the FTC and DoJ (FTC, 2009). However, there are some exceptions and these rules are complex, especially if not-for-profit entities with complex governance arrangements are involved (FTC, 2018). During the time period of our merger

[6] The District Court ruling in OSF-Rockford was issued in April 2012, and the Sixth Circuit ruling in ProMedica was issued in April 2014. In ProMedica, the parties appealed to the Supreme Court, which declined to hear the case in 2015. The proposed Capella/Mercy Hot Springs merger was abandoned in anticipation of a FTC challenge.

[7] We received an updated version of the Cooper et al. (2019a) hospital data in April 2022.

[8] kff.org/other/state-indicator/large-group-insurance-market-competition (accessed on 6/14/2022).

sample (2009-2016), some non-profit hospital mergers above the transaction size thresholds were not HSR-reportable because of the structure of the transactions. For non-profit hospital mergers, one should not equate HSR reporting status with the HSR size of transaction thresholds. In our analysis, since we apply administrative data, we do not need to know the asset value or predict the filing status of each merger.

Additionally, we use administrative data from the FTC to determine the formal level of scrutiny each merger received in the FTC or DoJ's merger review process.[9] For example, when merging firms file under the HSR Act, there are a few major checkpoints early in the merger review process. Under the HSR Act, the antitrust agencies have thirty days to review a merger filing before deciding whether to issue a detailed document and data subpoena often referred to as a Second Request. Alternatively, the reviewing agency may determine that it does not require the full 30 days to review the transaction and will therefore grant Early Termination (ET), which permits the merging parties to close their transaction prior to the end of the initial thirty day waiting period.[10] Based on this, we define three categories of consummated mergers based on the extent of antitrust review: (i) mergers that received a Second Request, and hence were reviewed most closely and for the longest duration of time by agency staff; (ii) mergers that did not receive a Second Request but also did not receive ET; and (iii) mergers that received ET.

Prices To generate hospital-level prices, we use the Health Care Cost Institute's version 1 inpatient claims database. This database contains hospital claims from 36,879,419 inpatient events from 2008-2016. We limit our analysis to inpatient events for which the patient resided in the lower 48 states plus DC, was enrolled in a commercial POS or PPO insurance plan, was not transferred from another hospital, did not arrive via ambulance, and was not a newborn.[11] This yields a set of 8,202,908 admissions. To generate hospital prices, we impose additional restrictions on the data, which we outline in Appendix B.

To compute prices, we follow Barrette et al. (2020) and Cooper et al. (2019a) by computing hospital-level prices adjusting for the complexity of the procedure for each year. We provide details on our casemix adjustment procedure in Appendix B.

[9]Nearly all of the hospital mergers during this period that were reviewed, were reviewed by the FTC.

[10]When we refer to "Early Termination", we specifically intend an Early Termination prior to the issuance of a Second Request. Merging parties may also receive an Early Termination after a Second Request has issued.

[11]We exclude ambulance patients, transfers, and newborns since we restrict our set of patients to those who are initially choosing a hospital. This allows us to use a consistent set of patients for our demand estimation and our price calculations.

Table 2: Hospital Merger Summary Statistics (2008-2016)

	N	Mean	SD	P25	P50	P75
Number of Hospitals (Count)	611	21.05	32.25	4.00	8.00	23.00
Minimum Distance (mi)	611	61.02	154.93	11.97	22.79	42.96
Maximum Diversion Ratio (Percent)	581	0.15	0.16	0.02	0.10	0.23
Within Same State (Binary)	611	0.90	0.30	1.00	1.00	1.00
Filed HSR (Binary)	611	0.33	0.47	0.00	0.00	1.00
Second Request (Binary)	611	0.02	0.13	0.00	0.00	0.00

Sources: HCCI inpatient claims data, FTC PNO data, Cooper, et al. and authors' hospital merger data

Patient Characteristics The HCCI inpatient claims database includes a number of important patient characteristics, such as diagnosis codes, age, gender, and ZIP code of residence. The HCCI data also include an encrypted identifier of the hospital that treated the patient. Using these data, we compute diversion ratios between merging systems. This requires estimating demand for all hospitals. Here, we apply the approach of Raval et al. (2017). Using this approach, we group patients based upon observable patient characteristics. Then, in the estimation procedure, we obtain estimated choice probabilities for each hospital-patient group pair. The estimated choice probabilities are analogous to the output of a conditional logit model in which the explanatory variables are a set of hospital by patient group fixed effects for all hospital-patient group combinations in the data. Using those estimated choice probabilities, we calculate diversion ratios between merging systems. These diversion ratios give the share of patients from hospital A that would switch to system B in the event that hospital A was no longer available. (See Raval et al. (2022) for additional discussion.) More details on this analysis are in Appendix C.

3.2 Summary Statistics

In Table 2, we show summary statistics for the 611 mergers in our data.[12] The table contains information on the number of hospitals involved in the merger, the shortest great circle distance between all pairwise combinations of merging partner hospitals, the maximum diversion ratio from any single hospital on one side of the merger to all hospitals on the other side of the merger, an indicator for whether the merger involved at least one pair of merging hospitals located in the same state, an indicator for whether the merger was filed under the HSR Act, and an indicator for whether the FTC or DoJ issued a Second Request. Table 2 shows that the median merger involved

[12]Due to sample size limitations, we were unable to compute diversion ratios between all hospitals for 30 of the mergers in our sample.

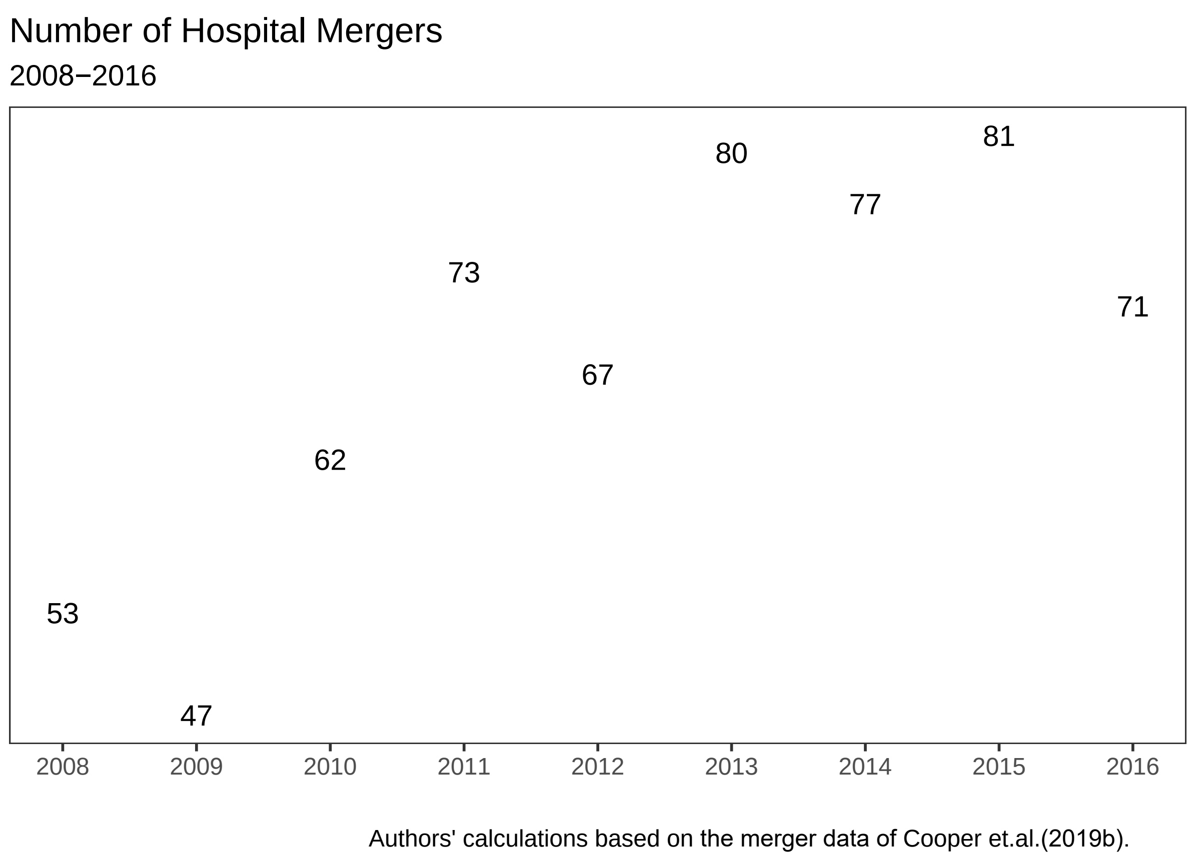


Figure 1: Mergers Over Time

Sources: Cooper, et al. and authors' hospital merger data

Table 3: Comparison of Never-Merged, Ever-Merged, and Once-Merged Hospitals

	Never-Merged (N=683)		Ever-Merged (N=967)		Once-Merged (N=401)	
	Mean	SD	Mean	SD	Mean	SD
Beds	290	216	285	216	296	205
WTP Per Patient	1.23	0.19	1.19	0.16	1.19	0.16
For Profit (Binary)	0.03	0.16	0.24	0.43	0.20	0.40
Not for Profit (Binary)	0.76	0.43	0.72	0.45	0.73	0.44
Teaching Hospital (Binary)	0.44	0.50	0.45	0.50	0.43	0.49

Note: N is the number of unique hospitals, but the statistics are computed at the hospital/year level.
Sources: HCCI inpatient claims data, FTC PNO data, Cooper, et al. and authors' hospital merger data

eight hospitals and had a maximum diversion ratio from one of the hospitals to all merging partner hospitals of 10%. 90% of the mergers involved at least one merging pair of hospitals located in the same state. 33% of the mergers were noticed to the federal government under the HSR Act, and the FTC or DoJ issued a Second Request in 2% of these consummated mergers.

In Figure 1, we show the number of consummated mergers in each year from 2008-2016. This illustrates an overall increase in merger activity during this period despite the increased prospective hospital merger enforcement by the FTC since 2008.

In our system data, we have information on 4,641 hospitals. We limit the set of treated hospitals to those who were involved in a merger during the period 2009-2016 and the merger involved at least one merging partner hospital less than 400 miles (using great circle distances) from the treated hospital. We drop hospitals that were first treated in 2008 since we have no pre-treatment data for these hospitals. We also drop hospitals that were never involved in a merger that included at least one merging partner hospital less than 400 miles away but were involved in at least one merger that included at least one merging partner hospital more than 400 miles away. This is to ensure that treatment effects even at great distances do not contaminate our control population. After imposing these restrictions, our sample includes 558 mergers, 1,650 hospitals involved in a merger, and 2,960 hospitals overall.[13] In Appendix Table OA5, we include a comparison of characteristics between the hospitals we include and those that we do not. While on many characteristics the hospitals look similar, the included hospitals are larger on average. Many hospitals were involved in more than one merger during our sample period and we discuss our treatment of these hospitals below.

In Table 3, we give descriptive statistics of the never-merged, ever-merged, and once-merged hospitals.

[13]See Appendix B for additional detail on our data processing and sample selection criteria.

The table illustrates that hospitals that were involved in a merger are much more likely to be for-profit and that hospitals that never-merged were much more likely to be public hospitals (which is the omitted profit status category in the table). Never-merged, ever-merged, and once-merged hospitals are similar in terms of bed counts, WTP per patient, and teaching status.

4 Estimation

In this section, we describe our analysis of the price effects of hospital mergers at the hospital level. In these analyses, we estimate the average price effects of hospital mergers at the level of an individual hospital, not at the level of a specific merger.

We begin by describing our econometric methods. The unit of observation is a hospital-year. The dependent variable, denoted Y_{jt}, is the natural log of the casemix adjusted price of hospital j in year t. (See Appendix B for a description of our case mix adjustment methodology.) We treat a merger as an absorbing state. That is, if hospital j is involved in a merger in year T, we assume that the hospital is treated in year T and in each subsequent year. However, we focus our analysis on hospitals that are only involved in one merger during our sample period.

Throughout our analyses, we apply a pooled linear regression model that is fully dynamic. That is, we allow for a full set of yearly lead and lag treatment effects and do not bin or trim the treatment effects at any point either before or after the treatment year T. We let T_j denote the treatment year of hospital j.

To begin, consider the two-way fixed effect linear regression model

$$Y_{jt} = \alpha_j + \delta_t + \sum_{l \in L} \mu_l D_{jt}^l + \epsilon_{jt}, \tag{1}$$

where α_j denotes a time-invariant hospital fixed-effect, δ_t denotes a year fixed-effect, and ϵ_{jt} denotes an error term that we assume is independent across hospitals. D_{jt}^l denotes an indicator variable for $t = T_j + l$. That is, $D_{jt}^l = 1$ indicates that, in year t, treated hospital j is l years away from its treatment year T_j. μ_l denotes the average treatment effect l years away from the treatment year, and L denotes the set of included lead and lag periods, which consists of $\{T-8, ..., T-2, T, T+1, ..., T+7\}$.

Following much of the literature, we use $T-1$ as the reference year.[14] We refer to this as the Event Study approach.

To account for the possibility of heterogeneous treatment effects by cohort and staggered adoption, we also apply the methodology of Sun and Abraham (2021). They develop a three-step approach that accounts for such heterogeneity. In the first step, Sun and Abraham (2021) divide the treated sample into cohorts based on the year of treatment. Here, let c index cohorts and C denote the set of cohorts. Sun and Abraham (2021) then estimate the average treatment effect coefficients on D^l_{jt} interacted with an indicator variable defined on whether hospital j is a member of cohort c. Hence, in the first step of the Sun and Abraham (2021) method, we estimate the linear regression model

$$Y_{jt} = \alpha_j + \delta_t + \sum_{c \in C} \sum_{l \in L} \mu_{lc} D_{jc} D^l_{jt} + \epsilon_{jt}, \tag{2}$$

where D_{jc} denotes an indicator variable for whether hospital j is a member of cohort c and μ_{lc} denotes the average treatment effect l years away from the treatment year for cohort c.

In the second step, Sun and Abraham (2021) estimate weights based on cohort-specific shares within a given year from treatment, l. In the third step, Sun and Abraham (2021) estimate the average treatment effect in period l that is analogous to an estimate of μ_l in equation (1). They do so by taking a weighted average of the estimated μ_{lc} parameters in equation (2) where the weights are the estimated cohort weights from the second step that are relevant for period l. We refer to this as the Sun and Abraham approach. While we conduct some analyses using both the Event Study and Sun and Abraham approaches, we don't find major differences in results between the two approaches and focus most of our analyses on the Sun and Abraham approach.[15]

We are interested in the heterogeneous treatment effects by observable hospital characteristics. Examples of such characteristics include the diversion ratio from the treated hospital to the merging partner hospitals (Section 5.1) and the insurance market HHI of the state in which the treated hospital is located (Section 5.2). Let G denote the set of realizations of such a merger characteristic

[14] See Sun and Abraham (2021). Six of the eight studies they cite use $T-1$ as the reference year.

[15] We use the implementation of the Sun and Abraham (2021) method found in the *fixest* R package. In this package, the weights are given by the empirical distribution of the observations in the data.

and g denote a given element of G. To estimate heterogeneous treatment effects, we estimate the specification

$$Y_{jt} = \alpha_j + \delta_t + \sum_{g \in G} \sum_{c \in C} \sum_{l \in L} \mu_{glc} D_{jg} D_{jc} D^l_{jt} + \epsilon_{jt}, \tag{3}$$

where μ_{glc} is the treatment effect for group g, cohort c, in period l and D_{jg} is a dummy variable for whether hospital j is in group g.

We estimate average treatment effects using various combinations of treatment and control hospitals. We discuss each of these in turn.

Regarding treated hospitals, we estimate average treatment effects applying two definitions of treated hospitals: (i) all ever-treated hospitals; and (ii) all once-treated hospitals. We define once-treated hospitals as the set of hospitals that were involved in exactly one merger during the period 2009-2016 and this merger involved at least one merging partner hospital less than 400 miles from the treated hospital. Some hospitals that meet this definition were also involved in at least one merger during 2009-2016 in which all of the merging partner hospitals were more than 400 miles away. We include these hospitals in our set of once-treated hospitals. We define ever-treated hospitals as the set of hospitals that merged with at least one merging partner hospital less than 400 miles away at least once during 2009-2016.

For the ever-treated hospitals, we follow Cooper et al. (2019a) by using the year of the first merger as T_j. We use the characteristics of this merger, for example, distance to the nearest merging partner hospital and the diversion ratio to all merging partner hospitals, in our analyses. Some ever-treated hospitals were involved in more than one merger during the year of their first merger. For example, a given hospital system may acquire three independent hospitals in the same year. Each of these acquisitions would count as a separate merger. For such hospitals, we again use the year of these mergers as T_j, and we use the specific merger with the highest diversion ratio to the merging partner hospitals as the relevant merger for defining the characteristics of the merger (e.g, distance, diversion ratios, filing status). In cases of ties, we use the merger with lowest distance to the nearest merging partner hospital to define the characteristics of the merger.[16] To avoid compounding merger effects, we focus most of our analyses on the once-treated hospitals.

[16] Ties occur because the diversion ratio in each merger that occurred in the year of the first merger is zero.

Regarding control hospitals, we apply three approaches. In our first approach, we simply use all hospitals that are never-treated during the period 2008-2016 as the set of control hospitals. We refer to this as the Baseline Control population. In our second approach, we use the set of hospitals that merged in 2016 as the controls, excluding never-treated hospitals and 2016 data. The rationale behind this specification is that treated and never-treated hospitals may be different in unobservable ways that make never-treated hospitals poor controls. We refer to this as the Merged Control population.

In our third approach, we construct a set of control hospitals using the method of synthetic controls.[17] Specifically, for each treated hospital j, we construct a synthetic control hospital, denoted j', from the set of never-treated hospitals. In constructing the synthetic control hospital, we match on specific hospital characteristics such as bed count, nursing FTEs and the number of technologies, as well as on pre-merger prices. We refer to this as the Synthetic Control population. See Appendix D for a detailed description of our synthetic control analysis.

Our estimation of the regression models (1), (2), and (3) is somewhat different in our Synthetic Control specifications compared to our Baseline and Merged Control specifications. In our Synthetic Control specification, we take advantage of the fact that the synthetic control hospital for each treated hospital is constructed specifically to match the treated hospital in terms of characteristics and pre-treatment prices. This allows us to estimate a time-invariant fixed effect for each treatment-synthetic control pair, as opposed to a time-invariant fixed effect for each individual hospital.

Using the price series for the synthetic control hospital, $\{Y_{j't}\}_{t\in[2008,2016]}$, we stack equation (1) for the treated hospital j,

$$Y_{jt} = \alpha_j + \delta_t + \sum_{l\in L} \mu_l D^l_{jt} + \epsilon_{jt}, \tag{4}$$

with the analogous equation for the synthetic control j',

$$Y_{j't} = \alpha_j + \delta_t + \epsilon_{j't}. \tag{5}$$

Note that we assume one fixed effect α_j for the treated-synthetic control pair (j, j'). Thus, this

[17] See Abadie (2021) for a general discussion of synthetic controls.

approach constructs merged-hospital specific controls. To the extent that the prices of the synthetic control are a good proxy for the price of the merged hospital but for the merger, this allows us to account for time and hospital specific factors that may be correlated with the time of merger indicator variables. Combining these pairs of equations across all treated hospitals (i.e, each treated hospital j and its synthetic control j') into a pooled linear regression model, we apply the Event Study and Sun and Abraham approaches described above. When we estimate heterogeneous treatment effects by cohort and group (that is equation (3)), we estimate a specification that is analogous to that outlined in equations (4) and (5), but that includes interactions with the cohort and group of the treated hospital.

Finally, in contrast to the Baseline and Merged Control specifications, we drop the 2009 cohort of treated hospitals in the Synthetic Control specification. This is to ensure that we have at least two years of price data prior to treatment in constructing the synthetic control.

5 Which Hospital Mergers Led to Price Increases

Figure 2 contains the regression coefficient estimates of equation (1) using the Event Study approach (top panel) and the price effect estimates based on equation (2) using the Sun and Abraham approach (bottom panel). The figure contains results using four combinations of treatment and control populations.[18] The first column, "All Treated", contains the results for the ever-treated hospitals using the Baseline Control population described in Section 4 . The second through fourth columns give the results for once-treated hospitals using the three control populations described in Section 4: Baseline Control, Merged Control, and Synthetic Control, respectively.

In addition to varying the control populations, we use the Gardner (2021), Roth and Sant'Anna (2022), and Callaway and Sant'Anna (2021) difference-in-differences methods and a hospital bed weighted estimator.[19] These results are in Appendix Figures OA3 and OA4. The results from all of these approaches paint a largely similar picture.

The results in Figure 2 are all similar to each other. We find relatively (but not perfectly) flat prices (relative to controls) leading up to the year of the merger and then steadily increasing prices following the merger, perhaps roughly plateauing in year four or five after the merger.[20] Focusing on

[18]The numbers associated with all regressions in the rest of the paper are located in Appendix F.

[19]To compute the different difference-in-differences approaches, we use the did2S R package.

[20]The pre-trend is least flat for the set of "all-treated" hospitals. This could be because those hospitals are engaged in multiple mergers both before and during our sample period and so the pre-period is picking up the effects of

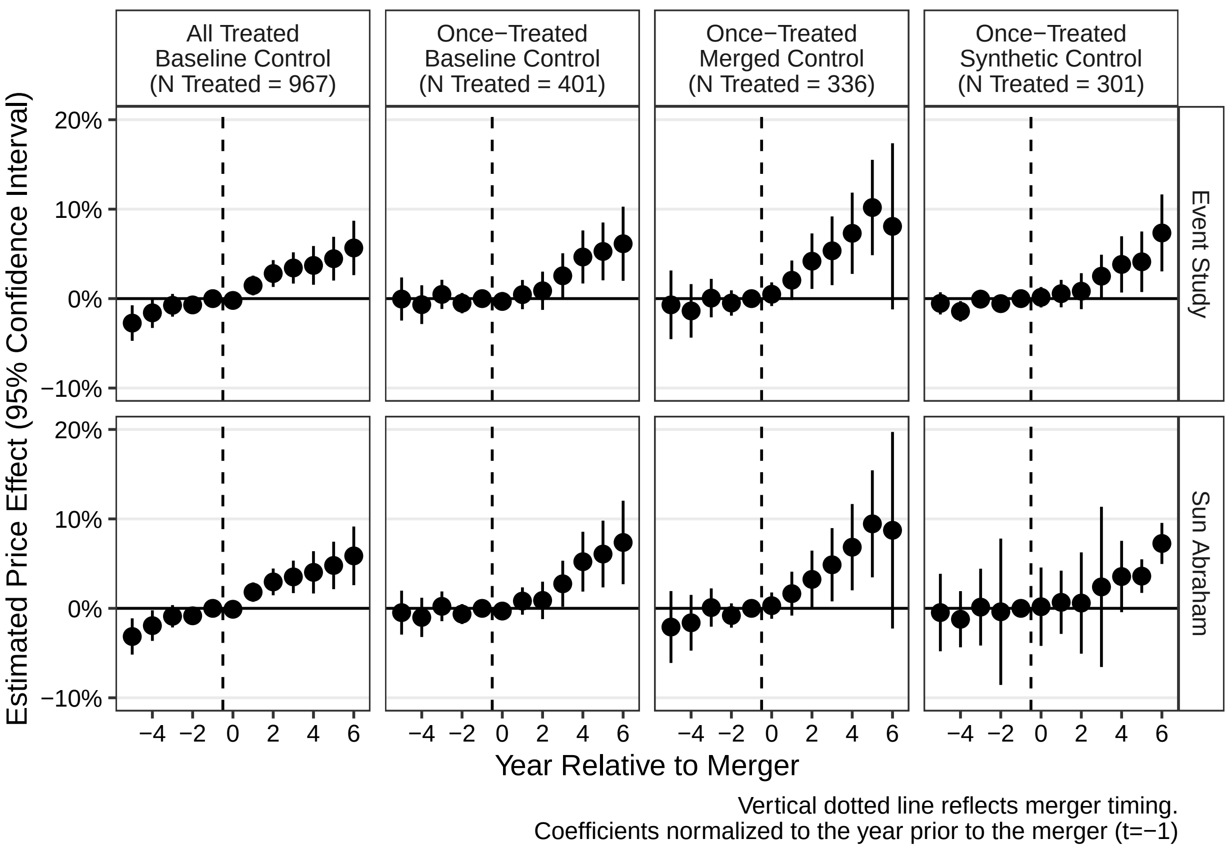


Figure 2: Unconditional Price Effects

Results from regressions described in text. Tables of coefficients in Appendix Tables OA6 and OA7.

the results using the Sun and Abraham specification, we estimate an average price effect of roughly 5%, ranging from 4% (based on the Synthetic Control specification) to 9% (based on the Merged Control specification). These estimates are from year five of the merger and reflect the increase relative to the control group of hospitals. Note that these estimated price effects are unconditional of all merger-specific attributes, including the proximity of the merging hospitals and whether the merger created any variable cost or quality efficiencies.

Our finding that the price effects of hospital mergers, on average, increase through roughly four or five years (and perhaps longer) does not necessarily suggest that the price effect of a given hospital merger typically takes this long to be fully manifested. Our findings may be driven by institutional features of this market in that contracts between insurers and hospitals may only come up for renegotiation every few years. Even if the price effects of hospital mergers are typically fully manifested in the terms of the first contract renewal following the merger, variation in the length of time between the year of merger and the year of the first contract renewal across treated hospitals would likely generate the upward sloping price effect estimates shown in Figure 2.[21] This underlines the importance of understanding insurer/hospital negotiations in analyzing the price effects of hospital mergers. This is a key feature in the relevant economic literature (see, for example, Capps et al. (2003), Gowrisankaran et al. (2015), and Dafny et al. (2019)) and a theme we return to below.

Our findings that the price effects of hospital mergers may increase for four or five years may also raise a concern for this analysis. Specifically, it may be the case that some hospitals in the never-treated population in our analysis were involved in a merger just prior to 2008. If the price effects of those mergers also increase over time, then the price effects of those mergers will likely contaminate the prices in our Baseline and Synthetic Control populations. Of course, it is also possible that some hospitals in our ever- and once-treated populations were also involved in another merger just prior to 2008. While this is potentially an important concern, because it is relevant for both our treated and control populations, it not clear that this biases our results one way or the other.

transactions prior to the beginning of our sample period. Regardless of the reason, for the rest of the paper, we focus on the results for once-treated hospitals, where there is no pre-trend.

[21] It is useful to compare our results with those in Vita and Sacher (2001), Haas-Wilson and Garmon (2011), Tenn (2011), and Thompson (2011). Each of these studies examines the price effects of specific hospital mergers, as opposed to the large set of mergers in our analysis. This enables those researchers to better control for the time difference between the year of the merger and the first contract renewal following the merger than we can in our analysis. Each of these studies generally find large in magnitude price effects shortly after the merger.

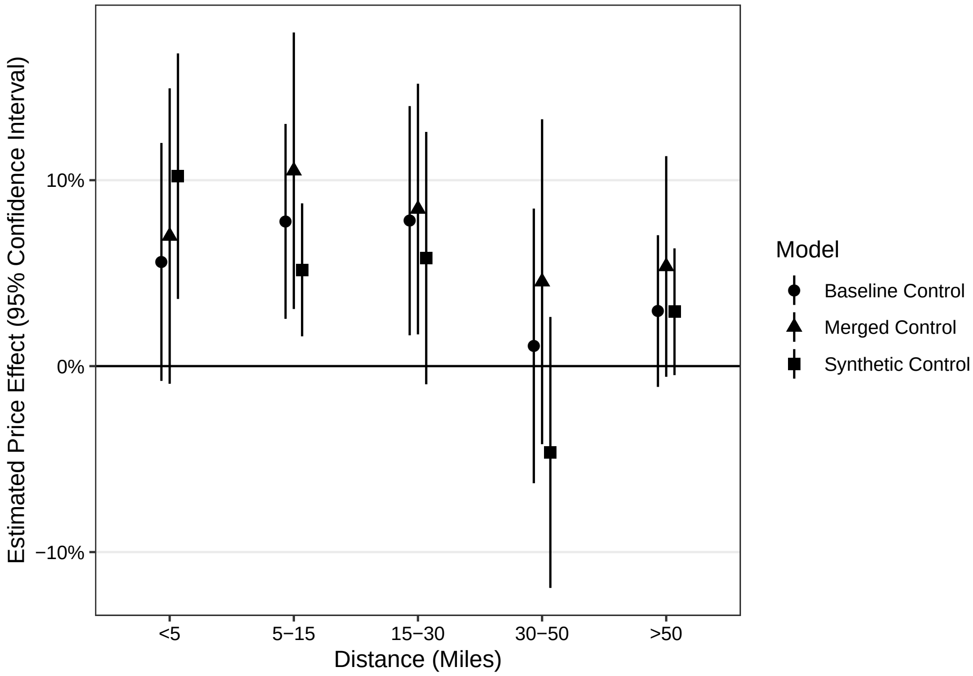
It is also possible that the results in Figure 2 are driven by a composition effect generated by variation in the price effects of hospital mergers across merger year cohorts. For example, suppose the price effects in the early part of our sample period are large and typically fully manifested shortly after the merger, while price effects in the late part of our sample period are, on average, very small in magnitude. Such a fact pattern would likely generate results similar to those in Figure 2. To test this, we replicate these results while limiting the analysis to the merger year cohorts 2009-2011. The results are given in Appendix Figure OA2. The figure exhibits an estimated price effect pattern similar to the results in Figure 2. This suggests that these results are unlikely to be driven by the composition effect described above. However, as discussed in Section 6.3, we do find that the estimated price effects are lower for later year merger cohorts.

For the rest of this paper, we focus on the results using the Sun and Abraham (2021) estimation approach, once-treated hospitals (using the three different control populations), and the average treatment effects from the third through seventh years following a merger. This is constructed by estimating cohort/calendar-year/year-of-merger average treatment effects from equation (3) or equation (4). We then aggregate those treatment effects using the number of mergers in each bin as weights.[22]

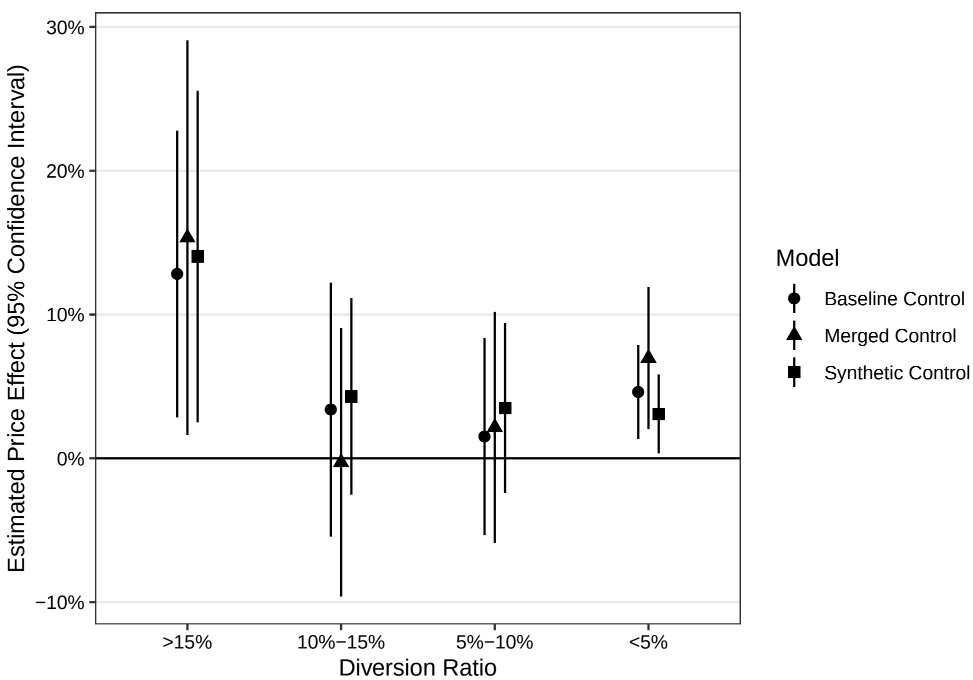
5.1 Changes in Market Power

Next, we test whether hospital mergers that create greater changes in the merging hospitals' market power lead to larger price increases. To do so, we focus on two metrics that are associated with the extent to which the merging hospitals competed prior to the merger. Each metric reflects the extent to which patients likely view the merging hospitals as substitutes at the point of service. The first metric is the distance from the treated hospital to the nearest merging partner hospital. This distance measure simply captures the proximity of the treated hospital to its merging partners and therefore provides some measure of the extent to which patients would view them as reasonable alternatives, at least in terms of geographic location. The second metric is the diversion ratio from the treated hospital to all of its merging partner hospitals. The distance measure has the advantage of simplicity, but it may be misleading for several reasons. First, some merging hospital pairs that are proximate may face many third-party competitors, while other merging hospital pairs that are far apart may face limited third-party competition. Second, distance does not account for

[22]We note that this implicitly puts more weight on mergers earlier in our sample. We compare average treatment effects of mergers over time in Section 6.3.



(a) Min Distance



(b) Diversion Ratio

Figure 3: Changes in Market Power and Price Effects
Results from regressions described in text. Tables of coefficients in Appendix Tables OA10 and OA11.

differences in the extent to which the merging hospitals overlap in terms of service lines. Third, distance accounts for the closest merging partner only and, therefore, ignores other merging partner hospitals that also may be nearby. The diversion ratio makes up for these shortcomings. However, the estimated diversion ratios may be sensitive to model specification and may understate the true diversion ratios in some cases (Raval et al. (2022)).

In analyzing how the price effects of hospital mergers vary by distance and the diversion ratio, we focus on the average price effects from the third year to the seventh year following the merger. The results are given in Figure 3. The top component of Figure 3 contains the results on how the estimated price effects vary with distance between the treated hospital and the nearest merging partner hospital. We divide the sample of treated hospitals into the following distance bins: 0-5 miles, 5-15 miles, 15-30 miles, 30-50 miles, and 50-400 miles. While the estimated price effects are not monotonically decreasing in distance, as intuition would suggest, we find that the price effects of hospital mergers generally do decline in the distance between the merging hospitals. Note that we find economically significant (though not quite statistically significant) price effects even in the highest distance bin, 50-400 miles. These results indicate that mergers even between hospitals that are quite distant lead to meaningful price increases, on average. Our findings are consistent with the results in a recent literature examining the price effects of cross-market hospital mergers (Lewis and Pflum (2017) and Dafny et al. (2019)).[23]

The bottom component of Figure 3 contains the results on how the estimated price effects vary with the estimated diversion ratio from the treated hospital to all of the merging partner hospitals. We divide the sample of treated hospitals into the following diversion ratio bins: 0%-5%, 5%-10%, 10%-15%, and greater than 15%. Similar to our results on the distance to the nearest merging

[23]We note that while our results are consistent with the findings in the recent literature on cross-market price effects, other explanations are also consistent with our results. For example, consider a hospital, denoted A, that is involved in a merger and is not close to any hospital in the merging partner system. However, other hospitals in the same system as A are close to hospitals in the merging partner system. Such a merger may lead to a price increase, and, if so and if the system to which A belongs implements a single price strategy, the price increase due to the merger would be manifested in the prices of all hospitals in the system to which A belongs, including A itself. Hence, the price of hospital A may increase because of a merger even though hospital A in not close to any merging partner hospital. However, this price effect is not a cross-market effect, but rather a within-market price effect that is just spread out over the prices of all merging hospitals, not just the proximate hospitals. We note that in this scenario, the single price strategy is not the source of the anticompetitive harm nor does the single price strategy imply that the total harm from the price increase is necessarily higher than it would be absent the single price strategy. On a separate point, while our results are consistent with Lewis and Pflum (2017) in that we find evidence of cross-market price effects, our results are also inconsistent with Lewis and Pflum (2017) in that we find that mergers between hospitals that are close substitutes, that is within-market mergers, exhibit higher average price effects than cross-market mergers. In contrast, Lewis and Pflum (2017) find that cross-market mergers actually exhibit higher average price effects than within-market mergers.

partner hospital, the estimated average price effects are not monotonically increasing in the diversion ratio, as theory and intuition would suggest. However, we do find that the price effects of hospital mergers are by far the highest when the diversion ratio from the treated hospital is greater than 15%. Note that we again find economically significant, and here statistically significant, price effects even for treated hospitals in the lowest diversion ratio bin: 0%-5%. These results are also consistent with the findings in the existing literature on cross-market hospital mergers.

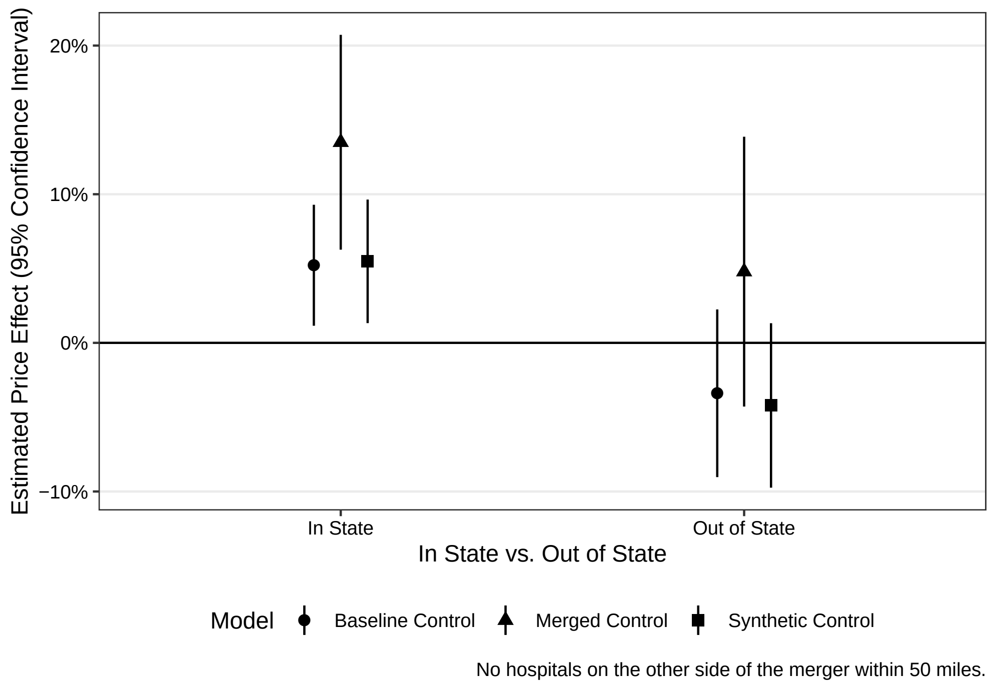
In sum, our findings are generally consistent with predictions of theory that mergers between hospitals that are more likely to be closer substitutes from the perspective of consumers and insurers, and therefore more likely to increase the merging hospitals' market power, generally lead to higher price increases. However, our findings also indicate that the price effects of hospital mergers do not go to zero, on average, as the merging hospitals become very distant substitutes. These findings are consistent with the results of other recent studies showing significant price increases resulting from cross-market hospital mergers.

5.2 Role of Insurers

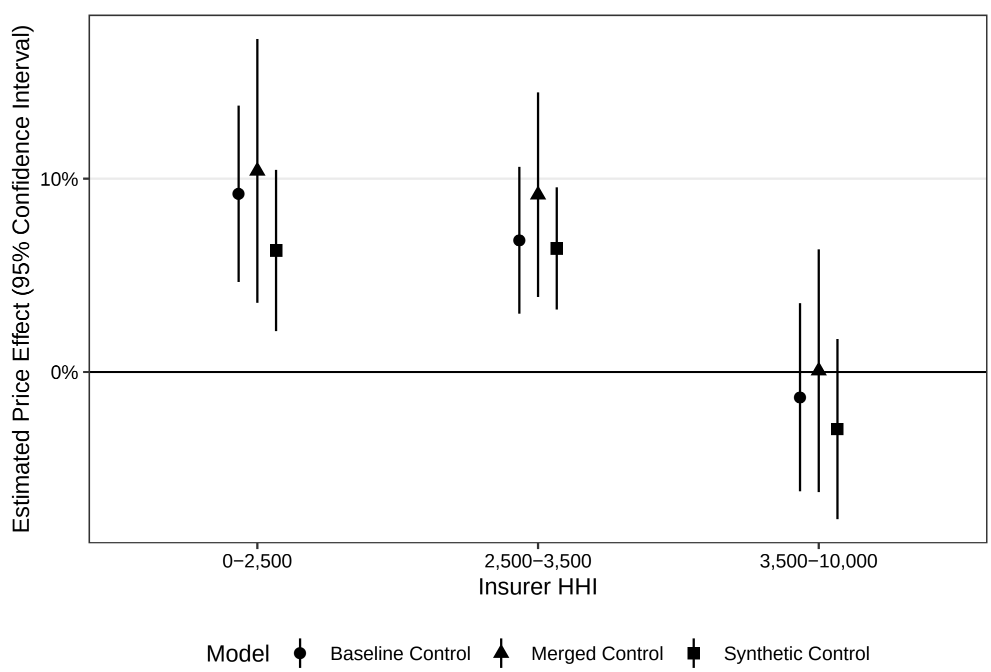
In this section, we consider the relationship between the price effects of hospital mergers and the level of competition in the insurance market. Since hospital prices are determined through bilateral negotiations between hospitals and insurers, some recent papers have highlighted the role of insurers in determining hospital prices. Two distinct theoretical mechanisms have been explored. First, in an examination of cross-market price effects, Dafny et al. (2019) show that mergers between hospitals that are unlikely to be substitutes for patients at the point of service nonetheless lead to price increases and find significant price effects only if the merging hospitals are located in the same state.[24] Vistnes and Sarafidis (2013), Dafny et al. (2019), and Brand and Rosenbaum (2019) suggest that this effect can arise if hospitals are substitutes for insurers even if they are not for patients. Second, in an examination of countervailing market power, Barrette et al. (2020) find that hospital prices are lower in concentrated insurance markets.

We explore both of these possibilities. First, following Dafny et al. (2019), we estimate price effects of mergers between hospitals that are unlikely to be substitutes, but are within the same state. Second, we examine the relationship between the price effects of mergers and level of concentration

[24]However, Lewis and Pflum (2017) find that the price effects of cross-market hospital mergers are 50% higher when the merging hospitals are not located in the same state compared to cross-market mergers between hospitals that are located in the same state.



(a) In State



(b) Insurer HHI (2013)

Figure 4: Insurers and Price Effects

Results from regressions described in text. Tables of coefficients in Appendix Tables OA12 and OA13.

in the insurance market.

The results are given in Figure 4. In the upper panel of the figure, we show the price effects for once-treated hospitals that had no merging partner hospitals within 50 miles. The figure gives the estimated average price effect for two types of mergers: where the treated hospital and at least one merging partner hospital were located in the same state and those in which no merging partner hospital was located in the same state as the treated hospital. Dafny et al. (2019) suggest that the existence of common employer customers for insurers within the same state could lead to price increases when hospitals are within the same state, but not across states. Brand and Rosenbaum (2019) discuss more broadly how hospitals could be substitutes for insurers even if they are not substitutes for patients. Since insurers are regulated at the state level and frequently organized corporately at the state level, price effects may be more likely when the merger involves at least one merging hospital pair in the state rather than when the merger does not.

Across all three control population specifications, we find economically significant differences in the average price effects of within-state (but still distant) mergers and across-state mergers. The estimated differences are economically significant - 10% in the Synthetic Control specification, 9% in the Baseline Control specification, and 9% in the Merged Control specification. Such findings are consistent with the findings in Dafny et al. (2019). However, we caution that we cannot rule out zero difference in price effects within and across states (at the 95% confidence level) in any specification.

In the lower panel of Figure 4, we show the price effects of mergers on once-treated hospitals when the hospitals are in states with different levels of insurance concentration. We bin states into three groups based on insurer HHI: (i) 0-2,500 (markets defined as unconcentrated or somewhat concentrated under the 2010 FTC-DoJ Horizontal Merger Guidelines (HMG)); (ii) 2,500-3,500 (markets at the lower end of the highly concentrated markets as defined by the HMG); and (iii) 3,500-10,000 (markets that are well above the threshold defining highly concentrated markets in the HMG).

The results show that treated hospitals in states where the insurance HHI is above 3,500 do not show appreciable price effects, with point estimates near zero. In contrast, the average price effects for treated hospitals in states where the insurance HHI is below 3,500 are above 5%. These results are consistent with the findings of Barrette et al. (2020) who find that prices of hospitals in concentrated insurance markets are less affected by the extent to which a hospital is valued by patients (as measured by WTP per patient). Our finding corroborates their results in a merger context and is consistent with the intuition that the price effects of hospital mergers will depend on, among other things, the relative bargaining leverage of insurers.

5.3 System Size

Next, we consider whether the price effects of hospital mergers differ by the size of the systems involved in the merger. We measure size by the number of hospitals in the system. There are a number of ways in which system size can influence the price effects of hospital mergers. First, if a hospital in a small system merges with a large system, it may give that hospital increased skill in negotiating with insurers, thereby allowing it to negotiate higher prices (Lewis and Pflum (2017) and Lewis and Pflum (2015)).[25] Second, if a hospital is going from a small system to a large system, the small system may experience efficiency benefits with respect to back-office operations, training, and billing (Schmitt (2017)). Finally, the merger may provide the larger system with additional degrees of freedom to extract preexisting market power. For example, if a hospital joins a large system which has significant market power in some markets, but the large system faces regulatory or public relations constraints that prevent it from fully exercising its market power in those markets, the merger may give the large system the ability to do so in the relatively unregulated market of the newly acquired hospital (Dafny et al. (2019)).

To analyze this question, we limit our sample of treated hospitals to once-treated hospitals that did not have a merging partner hospital less than 50 miles away. This is to minimize the effect of the elimination of direct competition on the estimated price effects. We define a "big" hospital system as one that has at least five hospitals. Using this system size threshold pre- and post-merger, we categorize each treated hospital in terms of the following size transitions: "Big to Big", "Small to Big", and "Small to Small". Figure 5 gives our results. In these results, we again focus on the average estimated price effects three through seven years following the merger. For "Big to Big" transitions, we find evidence of significant price effects, ranging from 4.2% to 12.9%. For "Small to Big" transitions, we find evidence of significant price reductions using the Merged Control specification, -12.4%, but no evidence of significant price effects using the Baseline Control or Synthetic Control specifications. These results are at least consistent with the intuition that a merger that moves a hospital from a small to a large system may present meaningful efficiency

[25] For example, in the context of bargaining theory, an acquisition of a small system by a large system may enable the small system to capture a larger share of the joint surplus in negotiating with insurers.

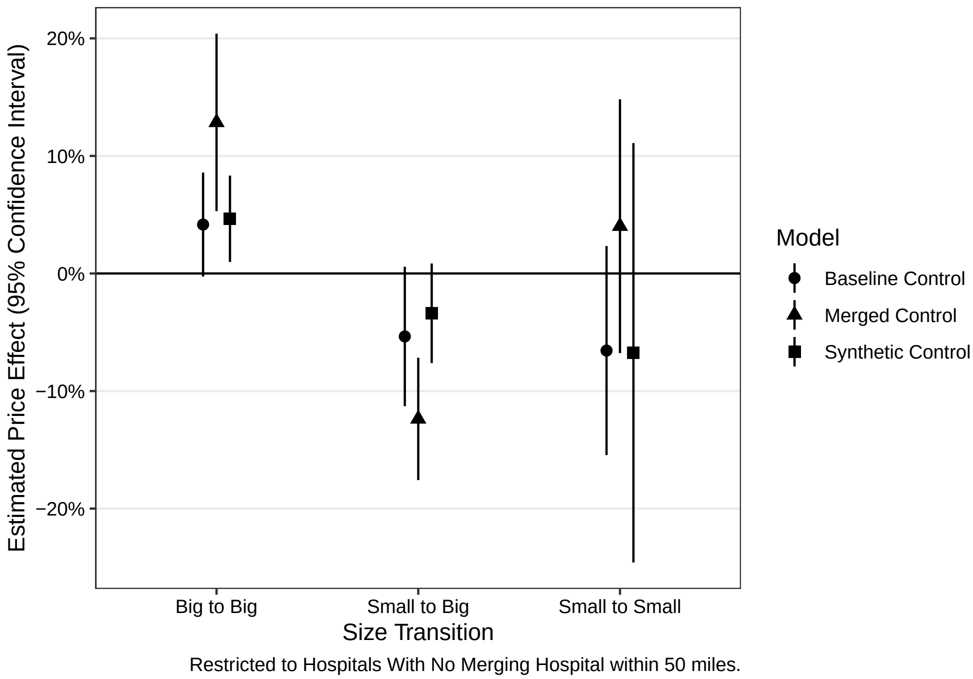


Figure 5: Price Effects By Size

Results from regressions described in text. Table of coefficients in Appendix Table OA14.

benefits.[26] For "Small to Small" transitions, our estimates are very imprecise and so provide little evidence of meaningful price effects. We return to these results in the following section.

6 Antitrust Review Process

As discussed in Section 2, the 2008-2016 period was a period of heightened antitrust scrutiny in the healthcare sector. In this section, we study how the price effects of those mergers differ depending on the level of antitrust review that a merger received. We study this by analyzing if price effects differ depending upon:

1. Whether a merger was noticed to the FTC and DoJ under the HSR Act;
2. The level of review that a filing received within the antitrust agencies; and
3. Whether the merger occurred early or late in our sample period (since there were pro-enforcement court decisions during our sample period).

We use the same estimation approach as in Section 5 and, as above, we focus our results on the Sun

[26] Note that our results are again not consistent with Lewis and Pflum (2017), who find that, on average, the effects of mergers (involving hospitals that were at least 90 miles apart) on the prices of independent hospitals were larger when the acquiring system was large (five or more hospitals) than when the acquiring system was small (four or fewer hospitals).

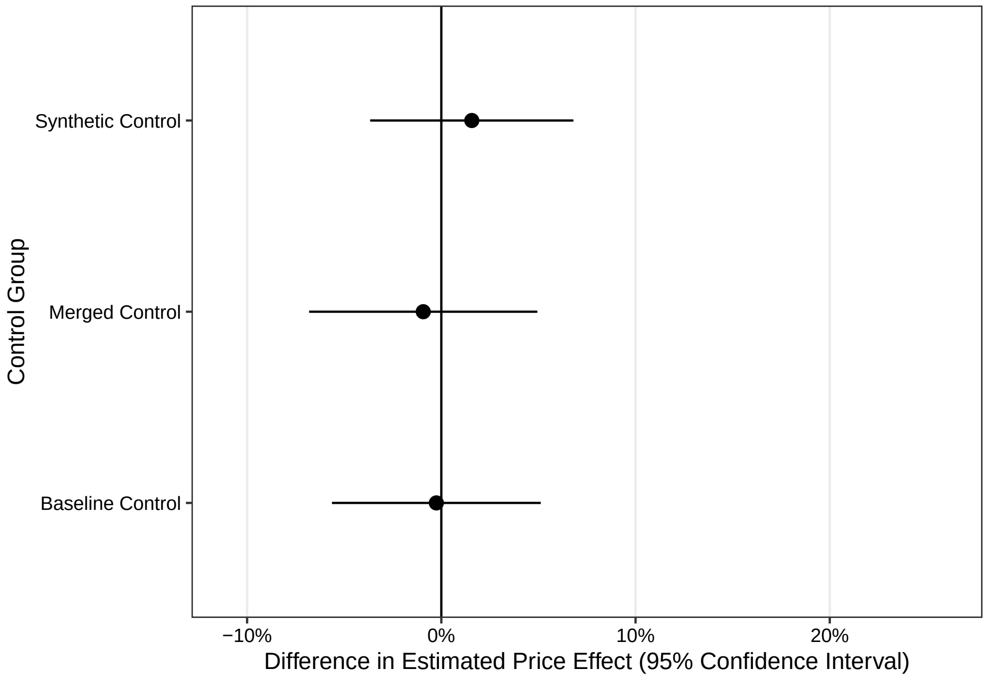
and Abraham estimation approach, once-treated hospitals with different control groups, and the average treatment effects from the third through seventh years following a merger.

6.1 HSR Filed Mergers

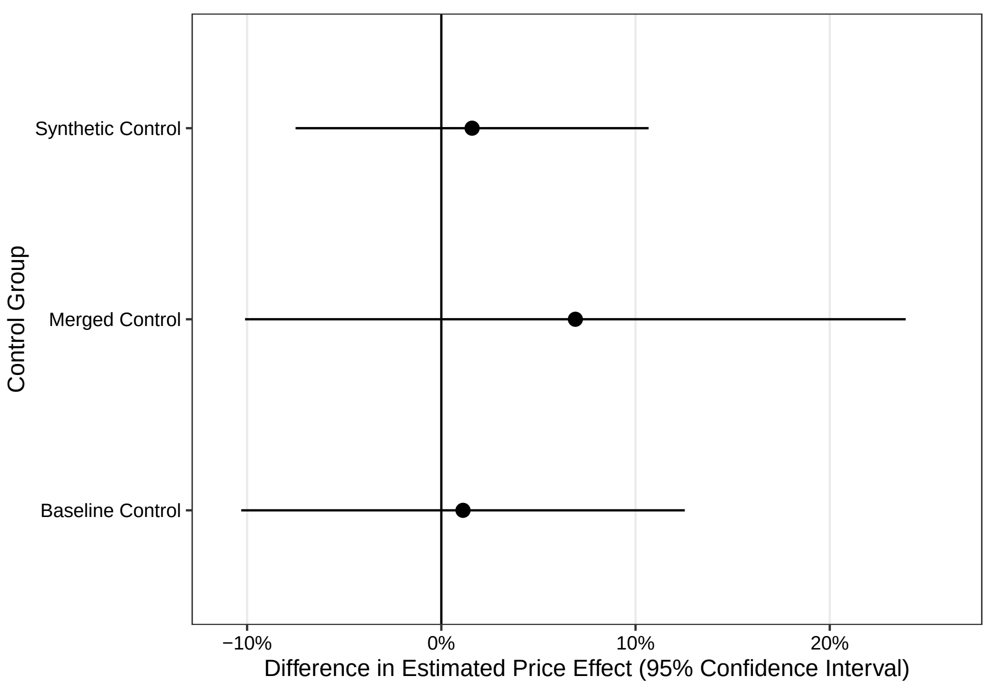
Recently, research has suggested that in the pharmaceutical and dialysis industries, there is "stealth consolidation", that is mergers that go undetected by the antitrust enforcers (Wollmann (2021); Cunningham et al. (2021)). Moreover, Wollmann (2021) shows that non-HSR dialysis mergers led to worse patient outcomes than HSR mergers. In Figure 6, we give the estimated difference between the price effects of hospital mergers that were not filed under the HSR Act and the price effects of hospital mergers that were filed under the HSR Act. The top panel of Figure 6 gives the results for all once-treated hospitals. We do not find evidence that hospital mergers that did not file under the HSR Act led to higher price increases than mergers that did file under the HSR Act. This is true across all of our control group specifications.

It is possible that this result arises due to different distributions of characteristics between filed and non-filed mergers such that non-filed mergers have a higher variance of price effects than filed ones. In other words, non-filed mergers have more mergers that are unlikely to lead to price increases and more mergers that are. Therefore, we estimate the same model as above, but only look at once-treated hospitals involved in mergers that are more likely to lead to a price increase. We define this set of hospitals as those with a diversion ratio to the merging party that is above 10%. We show these results in the bottom panel of Figure 6. As with the unconditional results in the top panel of Figure 6, we do not find any evidence that non-filed mergers have greater price effects than filed mergers. Though the point estimates of the difference in price effects are somewhat higher in the results conditional on a diversion ratio greater than 10%, the estimates are very imprecise.

There are a number of possible reasons why pre-merger notification may have different effects for hospitals than other industries. First, as noted above, hospital prices are determined in bilateral negotiations between hospitals and insurers. Moreover, hospitals (potentially in contrast with dialysis providers), likely account for a significant share of health insurers' costs. Therefore, insurers may inform the FTC and DoJ of mergers of concern regardless of whether those mergers are required to file with the agencies under HSR. Second, State Attorneys General may become aware of hospital mergers in their state and then inform the agencies that they are occurring – again, regardless of filing status.



(a) Unconditional



(b) Diversion Ratio Above 10%

Figure 6: HSR Filing Status: Not Filed - Filed

Results from regressions described in text. Tables of coefficients in Appendix Tables OA15 and OA16.

6.2 Agency Merger Review Process

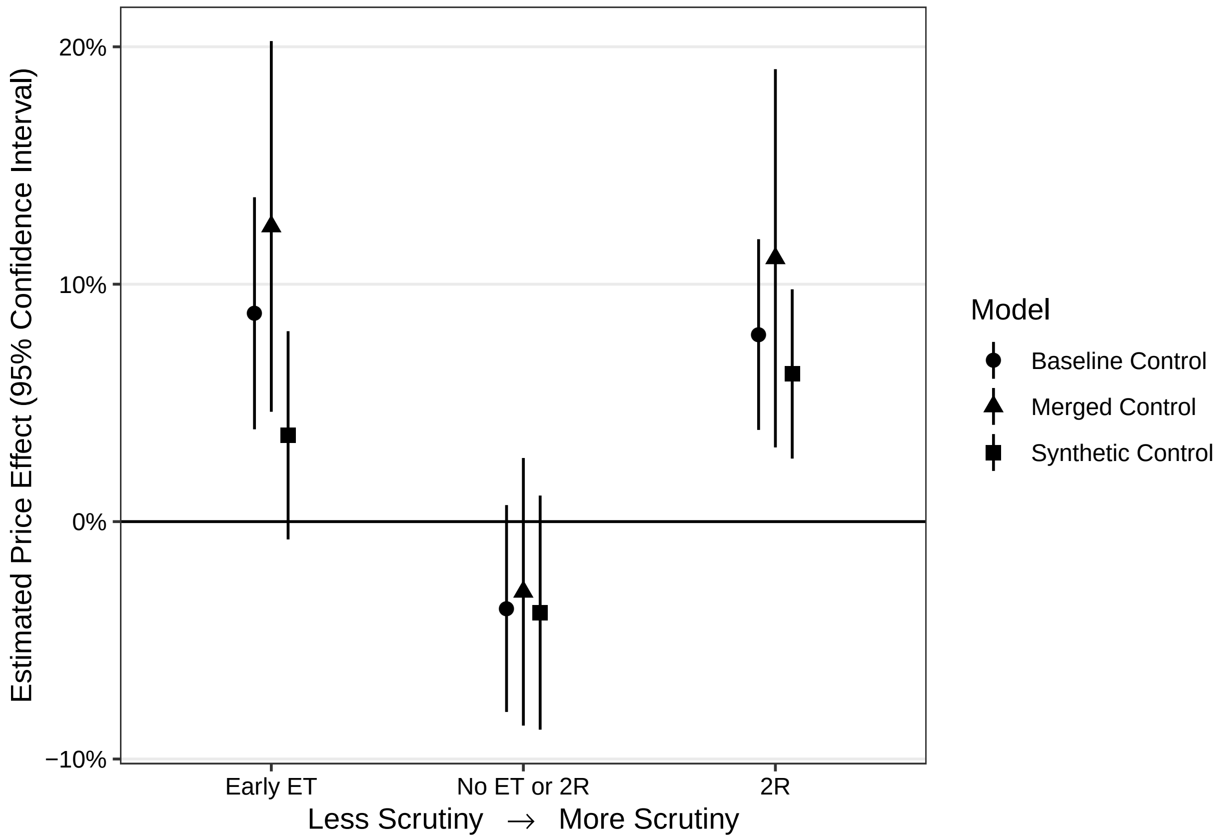


Figure 7: Agency Merger Review

Results from regressions described in text. Table of coefficients in Appendix Table OA17.
Early ET: FTC granted Early Termination. *No ET or 2R*: FTC did not grant Early Termination and did not issue Second Request. *2R*: FTC issued Second Request

In this section, we examine how the price effects of mergers vary with the level of scrutiny the merger received from the antitrust agencies conditional on filing under the HSR Act. In Figure 7, we show the post-merger price effects for hospitals involved in mergers that received three different levels of scrutiny (listed in increasing order of scrutiny): Early Termination (ET), no Early Termination and no Second Request (No ET or 2R), and Second Request (2R).[27] The graph shows that the price effects were highest for mergers that received either Early Termination or a Second Request. In our baseline control specification, mergers that received a Second Request had average price effects of roughly 8%, while mergers that were granted Early Termination had average price effects of approximately 9%. In contrast, mergers that received neither a Second Request nor Early Termination did not show statistically significant price effects.

[27]We discuss more about the agency review process in Section 3.

Table 4: Characteristics of Hospital Mergers in Different Investigation Phases (2008-2016)

	Diversion <10%		Diversion >10%	
Investigation Status	N Merger	Percent	N Merger	Percent
ET	58	54.21	31	38.27
No ET or 2R	49	45.79	50	61.73
All	107	100.00	81	100.00

Sources: HCCI inpatient claims data, FTC PNO data, Cooper, et al. and authors' hospital merger data

Table 5: Probability of ET By Post-Merger System Size, 2008-2016

Conditional on No Second Request and Diversion Ratios Below 10%

	Big System		Small System	
Investigation Status	N Merger	Percent	N Merger	Percent
ET	47	59.49	11	39.29
No ET or 2R	32	40.51	17	60.71
All	79	100.00	28	100.00

Sources: HCCI inpatient claims data, FTC PNO data, Cooper, et al. and authors' hospital merger data

Based on these results, it seems that the agencies were successful (on average) in identifying in the preliminary phase of the investigation which mergers were most likely to be anticompetitive – and issued Second Requests in those cases. However, since those mergers were ultimately consummated and, on average, led to price increases, the agencies appear to have not obtained remedies sufficient to mitigate anticompetitive effects. This could be for several reasons. First, the agencies did not have sufficient resources to challenge these mergers even though they concluded the mergers were anticompetitive. Second, the agencies concluded that these mergers were not anticompetitive or that the evidence supporting a challenge was too weak. Third, the agencies concluded that there were quality improvements that would offset any price increases that would occur. We cannot distinguish between these three explanations.

These results also show that mergers that received Early Termination had higher price effects, on average, than mergers that received further investigation, but not a Second Request. To investigate why, in Table 4, we compare the probability of receiving Early Termination by different diversion ratio buckets. We find that among mergers where all pairs of hospitals had diversion ratios of below 10%, Early Termination was more likely. In contrast, we find that among mergers where at least one hospital had a high diversion ratio, there is a lower probability of Early Termination. This is intuitive, since it suggests that Early Termination was more likely among the mergers without hospitals that were closer substitutes for patients. Moreover, a greater share of the mergers receiving Early Termination had low diversion ratios than high diversion ratios, which is also intuitive. However, this finding further raises the question of why price increases were higher, on average, among the mergers that received Early Termination.

In Table 5, we show the probability of an Early Termination by the size of the merged entity. In this table, we focus on the 107 mergers where there are no hospitals with diversion ratios of 10% or higher. We find that mergers that involve large hospital systems (those with more than five hospitals post-merger) have a roughly 60% probability of receiving Early Termination, while mergers that involve smaller systems have a roughly 40% probability of receiving Early Termination. These results suggest that large hospitals systems may be more likely to receive Early Termination than small systems even though their mergers look similar from the perspective of patient substitution. We think these results should encourage further study of the Early Termination process to better illuminate why those mergers that received this truncated review had higher price effects on average than those that received a preliminary review (i.e, no Early Termination) but not a Second Request.

6.3 Changes over Time

As discussed in Section 2, the FTC was successful in challenging several hospital mergers during 2008-2016. Included in these challenges were court findings for the FTC in OSF-Rockford (2012), ProMedica (2014), Advocate-North Shore (2016), and Hershey-Pinnacle (2016). These court decisions may have important implications for hospital merger enforcement. For example, if these court decisions had a meaningful impact in deterring the most problematic mergers, either because they were not proposed or because they were abandoned in the face of antitrust scrutiny, then the average price increase from mergers should decline over time.

In Figure 8, we give results that offer some support for this possibility. Here, we plot the average price effects of hospital mergers by merger year cohort. The figure contains the estimated price effects two years following the merger in the top panel, three years following the merger in the middle panel, and four years following the merger in the bottom panel.[28] Generally, we do find a downward trend in the average price effects. For example, in the third year following a merger and in our Baseline Control specification, we find average price effects of roughly 9% for mergers that occurred in 2009 and average price effects of roughly 1% for mergers that occurred in 2013.

[28]Recall that we drop treated hospitals that merged in 2009 in our Synthetic Control analysis.

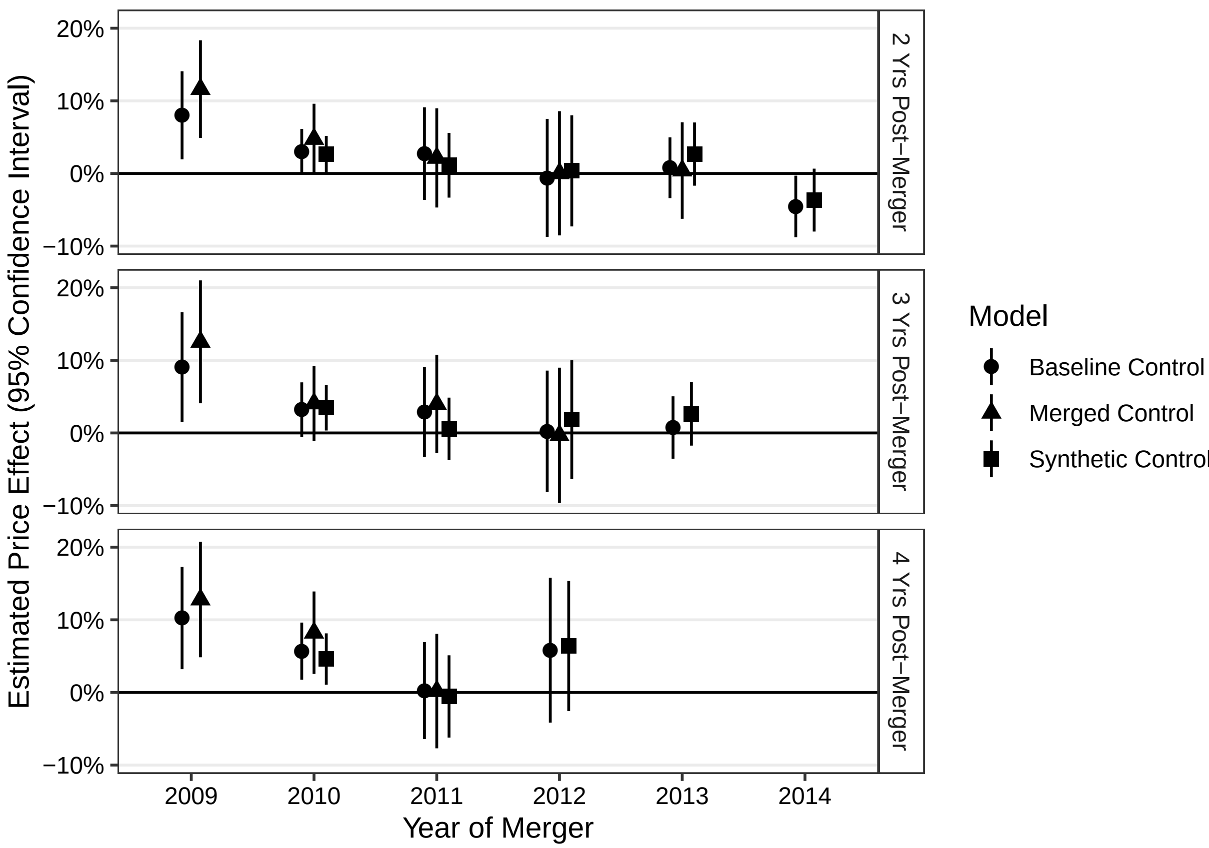


Figure 8: Price Effects by Cohort

Results from regressions described in text. Tables of coefficients in Appendix Tables OA18, OA19, and OA20.

Table 6: Characteristics of Merging Hospitals Over Time

Years	Diversion > 10%	In State	Insurance HHI < 3500	Small to Big
2009-2011	7%	90%	68%	6%
2012-2014	18%	85%	62%	17%

Sources: HCCI inpatient claims data, FTC PNO data, Cooper, et al. and authors' hospital merger data

These results may suggest a shift in the composition of mergers over this period. To explore this possibility, we compare the characteristics of merging hospitals in the early and later period of our sample. The results are given in Table 6. We use our baseline set of once-treated hospitals in these results. Even though we find a decrease in average price effects over time, we find an increase in the share of hospitals involved in mergers with high diversion ratios and an decrease in the share of mergers occurring in locations with insurer HHIs under 3,500 – both of which were associated with higher prices as we showed in Section 5. Conversely, we find a more than doubling in the share of hospitals in smaller systems moving into larger systems, which was associated with lower prices following the merger. This suggests that a contributor to the decline in price changes over time may have been an increase in the number of efficient mergers taking place over this period. However, it is also possible that hospitals increasingly exercised increases in market power due to mergers in non-price terms, but we do not study this question.

We also consider the possibility that this decline over time is the result of our sample selection. While in this analysis, we restrict to hospitals that had only one merger between 2009-2016, it is possible that some of the hospitals in the early years of our sample period were still experiencing effects from a previous merger in the early years of our sample. Therefore, this result of declining merger effects over time could reflect the difference between the effects of multiple mergers and the effect of a single merger. However, since our results in Section 4 are very similar for hospitals that experience one merger and hospitals that experience multiple mergers, we think that this explanation is unlikely.

7 Conclusion

In this paper, we illustrate the price effects of hospital mergers during a period of more intensive and increasing antitrust scrutiny. Overall, we estimate price effects of roughly 5%, with the average declining over our sample period. We reinforce previous findings in the literature by finding evidence that price effects of mergers are higher when the merging hospitals are substitutes for patients, insurance markets are unconcentrated, and merging hospitals are in the same state. These results suggest that even under the more intensive antitrust scrutiny of the period, there were still many types of mergers that were on average leading to price increases.

We find that the mergers identified by agency staff for further review typically led to price increases of roughly 8%, but that there was no evidence of a difference in price effects for mergers that filed under the HSR Act compared to those that did not. Further, we find that mergers receiving a truncated review by the agencies (Early Termination) led to price increases of roughly 9%, but those that received a full preliminary review but no Second Request, on average, did not lead to significant price increases. In the aggregate, these results suggest that agency staff identified problematic hospital mergers if those mergers were not granted Early Termination and that the HSR filing status does not seem to have a major effect on the identification of problematic hospital mergers.

Generally, our findings are consistent with the predictions of the analytical framework that the FTC has applied in its hospital merger investigations since 2008 (Capps et al. (2018)). As discussed in Section 2, the empirical tools derived from this framework include Willingness-to-Pay (WTP), diversion ratios, and merger simulations. These tools predict that, absent efficiencies, mergers between hospitals that are closer substitutes for patients will lead to higher price increases (Garmon (2017) and Balan and Brand (2022)). Hence, the consistency of our findings with these predictions suggests that the underlying analytical framework likely has played a useful role in the FTC's hospital merger enforcement agenda since 2008. The careful application of this or a similar framework in other settings may also lead to improved merger analysis.

However, our findings also illustrate a potential limitation of the empirical tools that the FTC has applied recently. Specifically, our finding that the price effects of hospital mergers, on average, remain significant even when the merging hospitals are not close substitutes for patients (that is cross-market mergers) suggests that the empirical tools that the FTC has applied recently are not capturing all of the important factors driving the price effects of hospital mergers. As discussed in Brand and Rosenbaum (2019), additional research on this topic seems worthwhile.

Our work has some limitations. First, the HCCI data we use only include data from some commercial insurers and exclude the Blue Cross/Blue Shield insurers. To the extent that Blue Cross/Blue Shield insurers are dominant insurers in states with concentrated insurance markets, this exclusion may bias our estimates by insurer HHI category. Second, we only have access to state-level insurer concentration measures, which may reduce the precision of our price effects estimates. Third, we focus our main empirical results on once-treated hospitals. While that provides much better internal validity, hospitals that are involved in multiple mergers may be affected differently by a merger than those involved in only one. Fourth, our results are all conditional on a given set of antitrust policy enforcement choices at the time, and we cannot use our analysis to evaluate counterfactual policy changes. Finally, our results are based on price changes for inpatient services. Hospital mergers may also affect outpatient prices and reimbursements for hospital-employed physicians, but we did not study these changes.

We think that this work should prompt additional research into understanding the role of antitrust policy on the effects of mergers – both in the hospital sector and beyond. There is much to be done to understand the deterrent effect of the HSR filing and agency review processes on which mergers are consummated. Contrasting our results to some of the previous literature suggests that these effects are likely to differ according to the institutional details of the industry, so we hope that this research will take place in a range of different settings.

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Online Appendix for
In the Shadow of Antitrust Enforcement: Price Effects of Hospital Mergers from 2009-2016

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A Willingness-to-Pay

In this appendix, we describe the Willingness to Pay (WTP) per patient calculation for hospitals using the framework of Capps et al. (2003).[1]

Using the choice probabilities described in Section 3 and Appendix Section C, we can compute the WTP per patient for each hospital or hospital system. For a hospital system denoted h, this is given by the formula

$$\overline{WTP_h} = -\sum_i \log(1 - \sum_{j \in J_h} s_{ij}) / \sum_i \sum_{j \in J_h} s_{ij},$$

where i denotes a patient; J_h denotes the set of all hospitals j in system h; and s_{ij} is the estimated choice probability for hospital j by patient i given by the demand model.

B Data Description and Case Mix Adjustment

In this appendix, we provide a description of our data. We begin with 8,202,908 inpatient admission events generated from the HCCI inpatient claims database covering 2008-2016. These inpatient events exclude psychiatric and behavioral inpatient events (i.e., events with MDC 19 or 20). These

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[1]See Capps et al. (2003) and Balan and Brand (2022) for a discussion of the relevance of WTP in the context of bilateral bargaining between hospitals and insurers.

data contain basic patient characteristics such as age category, gender, the MS-DRG associated withe the inpatient event, the admission date, the length of stay, and an encrypted identifier of the hospital that treated the patient. We merge these data with the MS-DRG weight provided by the CMS based on the DRG and admission date of the inpatient event. We drop 9,794 events for which the MS-DRG weight is missing. We drop an additional 1,518 events for the the observed patient gender is inconsistent with the observed MS-DRG.[2] This leaves a sample of 8,190,828 inpatient events.

Following Cooper et al. (2019), we drop inpatient events if the patient was less than 18 years of age. We also drop inpatient events for all Critical Access Hospitals and if the observed hospital treated no Medicare patients.[3] To control for outlier events, we drop events for which the observed payment to the hospital was greater than the 99^{th} percentile or less than the first percentile conditional on the MS-DRG. We also drop events for which the observed length-of-stay is greater than the 99^{th} percentile again conditional on the MS-DRG. After dropping these events, there are 459 events for which the observed payment is zero. We drop these events. Finally, we drop each hospital-year combination for which there are fewer than 50 events. This leaves 7,196,728 inpatient events and 2,624 unique hospitals.

We also exclude specialty hospitals. Broadly speaking, we define a specialty hospital as a hospital for which a small number of service lines account for a large share of its patients. We use the Major Diagnostic Category (MDC) code associated with the MS-DRG as our definition of a service line. To identify specialty hospitals, we construct a concentration index based on within-hospital shares of the MDC codes for the patients treated by the hospital. This concentration index is similar to an HHI in that we sum the squared shares of events accounted for by specific MDC code values. We define a specialty hospital as a hospital for which this concentration index exceeds 0.9. After dropping hospitals that meet this criterion, we have a final sample of 7,157,244 inpatient events and 2,596 unique hospitals.

In Table OA1, we give the number of inpatient events and the number of unique hospitals by fiscal year. Of the 2,596 unique hospitals in our data, 1,555 appear in each of the nine years. 1,650 unique hospitals appear in at least five years. This is the set of hospitals that we use in our baseline analyses in Section 5.

[2]For example, we drop an inpatient event if the observed MS-DRG corresponds to MDC 12 (diseases of the male reproductive system) and the observed patient gender is female.

[3]We determine if the hospital treated no Medicare patients using AHA data.

Table OA1: Summary Statistics

Fiscal Year	Inpatient Events	Unique Hospitals
2008	608,835	1,908
2009	827,286	2,111
2010	904,956	2,191
2011	904,527	2,164
2012	876,984	2,131
2013	849,681	2,088
2014	755,117	2,069
2015	713,714	1,974
2016	716,144	1,956

Source: HCCI inpatient claims data

We now turn to our casemix adjustment procedure. Broadly speaking, we apply a linear regression model using the natural log of the payment to the hospital for each inpatient event as the dependent variable and explanatory variables that include fixed effects for hospitals and patient characteristics. The primary patient characteristic that captures variation in casemix across hospitals is the MS-DRG, though we include patient gender and age category as well. We estimate this linear regression model separately for each fiscal year. The output of this analysis is an estimate of a price for each hospital under the hypothetical scenario that each hospital treated exactly the same set of patients within fiscal years.

Let i index inpatient events, j denote the hospital associated with inpatient event i, and R_{ij} denote the raw payment to hospital j for inpatient event i as generated from the HCCI inpatient claims data. Let I_t denote the set of inpatient events in year t. The linear regression model we estimate is

$$\ln(R_{ij}) = drg_i + age\_cat_i + gender_i + \alpha_j + \epsilon_{ij}, \forall i \in I_t, \tag{OA1}$$

where drg_i denotes a fixed effect for the MS-DRG associated with inpatient event i, age_cat_i denotes a fixed effect for the age category of the patient, $gender_i$ denotes a fixed effect for the gender of the patient, α_j denotes a hospital fixed effect, and ϵ_{ij} is the residual.

Using the fitted model of equation (OA1), the variation in the casemix adjusted prices across hospitals is based on differences the fitted values $\exp\{\widehat{\alpha_j}\}$. We scale these fitted values by the mean value of the exponential of the fitted components of $\ln(R_{ij})$ based patients characteristics (DRG,

Table OA2: Volume-Weighted Mean Casemix Adjusted Prices

Fiscal Year	Mean Price	Growth Rate
2008	11,812	NA
2009	12,727	0.077
2010	13,685	0.075
2011	14,397	0.052
2012	15,341	0.066
2013	16,305	0.063
2014	17,112	0.049
2015	17,676	0.033
2016	18,890	0.069

Source: HCCI inpatient claims data

age category, and gender) as well as the fitted residuals, $\widehat{\epsilon_{ij}}$.[4] That is, we calculate the casemix adjusted price for each hospital k in year t, denoted $price_{kt}$, as

$$price_{kt} = \exp\{\widehat{\alpha_k}\} \frac{\sum_{i \in I_t} \exp\{\widehat{drg_i} + \widehat{age\_cat_i} + \widehat{gender_i} + \widehat{\epsilon_{ij}}\}}{\# I_t}. \tag{OA2}$$

Table OA2 gives the volume-weighted mean casemix adjusted price by fiscal year and the year-over-year growth rate in this mean price. During the period 2008-2016, the volume-weighted mean casemix adjusted price grew by an average of 6% per year. We also find considerable variation in hospitals' casemix adjusted prices even after accounting for average increases over time. After regressing out hospital and year fixed effects, we find that, on average, the standard deviation in a given hospital's residual casemix adjusted price is 8.9% of its mean casemix adjusted price.[5]

C Demand Estimation

We estimate hospital demand using the approach outlined in Raval et al. (2017). For that estimation approach, we need to select covariates, order them, and select a minimum group size. In this paper, we use gender, age (in bands), diagnosis code (MS-DRG), diagnosis category (MDC), patient zip

[4]Even though the fitted residuals are mean zero by construction, they nonetheless affect the scaling of the casemix adjusted prices because of the log transformation of the dependent variable.

[5]We calculate this as follows. First, we regress the full set of casemix adjusted prices on a set of year fixed effects and hospital effects. Second, we evaluate the fitted residuals from this regression and calculate the standard deviation of the residuals for each hospital across time. Third, we calculate the volume-weighted mean (across hospitals) of the ratio of each hospital's residual standard deviation to its mean casemix adjusted price. This volume-weighted mean is the reported 8.9%.

code (5 digit), patient zip code (3 digit) , and patient hospital referral region. We eliminate these in reverse order in our estimation strategy. We use a minimum group size of 25.

In practice, this means that we first group all patients by all 7 of these covariates. For any groups that do not have 25 people in them, we drop gender and then regroup all of the patients keeping those groups with above 25 people. After that grouping, we drop age and then regroup. We continue to do this until we have gone through all of the covariates. Within each of these groups, we estimate the choice probability for hospitals – and treat that as the estimated choice probability for all individuals within each group.

We eliminate the 16,390 patients whom we could not include in a group of 25 after doing this procedure. This leaves 8,186,133 patients in our demand estimation.

D Synthetic Control Analysis

In this appendix, we described our synthetic control analysis and present additional results. Throughout, we base our analysis on the synthetic control group method described in Abadie and Gardeazabal (2003), Abadie et al. (2010), Abadie et al. (2011), and Abadie et al. (2015). For each treated hospital, we construct a synthetic control hospital from a subset of never-treated by matching on hospital characteristics and pre-merger prices. Using the weights that define the optimal linear combination of control hospital characteristics, we construct the price vector for the synthetic control hospital.

The specifics of our analysis are as follows. We begin with 645 ever-treated hospitals and 505 never-treated hospitals for which we have a casemix adjusted price for each year in 2008-2016. For each hospital, we limit the set of possible control hospitals to those that are not in the same Hospital Referral Region as the focal hospital and to hospitals that have a bed count within a given range of the bed count of the focal hospital. For hospitals with a bed count in the [50,500] range, which account for 84% of the hospitals in our analysis, we apply a relatively narrow bed count range of 25. For smaller and larger hospitals, we apply a larger range in order to bring more hospitals into the possible control hospital set. Table OA3 contains the bed count thresholds that we apply. For example, if the focal hospital has 400 beds, we limit the set of control hospitals to those that have a bed count in (375,425). If the focal hospital has 800 beds, we limit the set of control hospitals that to those have a bed count in (600,1000). Across all treated hospitals in our analysis, the mean number of hospitals in the set of possible controls is 49.5 and the standard deviation is 13.9.

Table OA3: Bed Count Limits in Control Group Definition

Bed Count of Hospital	Bed Count Range
(0,50)	50
[50,500]	25
(500-750]	100
(750-1,000]	200
(1,000-1,500]	500
>1,500	1000

Sources: HCCI inpatient claims data and Cooper, et al. and authors' merger data

Given the set of control hospitals for each focal hospital, we solve for the optimal control by matching on the following characteristics: bed count, nursing FTEs, the number of technologies, the mean length-of-stay, the mean MS-DRG weight, and WTP per patient. We evaluate WTP per patient at the hospital level, i.e., we ignore system affiliation. Our purpose in including WTP per patient is to match based on being similarly situated in terms of the presence or absence of nearby alternative hospitals. In all of the above characteristics, we match based on their mean values across all years in our data.

We also match on two price related terms. First, we match on the price in the year prior to the merger. Second, we match on the mean price across all years prior to the year before the merger.

The search algorithm in the synthetic control analysis failed for 8 treated hospitals. This leaves us with a sample of 637 treated hospitals and 505 never-treated control hospitals.

Table OA4 gives the mean weight from the synthetic control analysis for each of characteristics on which we match treated hospitals to control hospitals. These mean weights give some information on the relative importance of these characteristics in constructing the optimal synthetic control. The table indicates that the price during the year prior to the merger is by far the most important characteristic, with a mean weight of about 50%. The mean price across all years prior to the year before the merger is the second most important characteristic with a mean weight of 26%. Each of the non-price characteristics has a mean weight in the 3%-4% range.

Table OA4: Mean Synthetic Control Weight by Hospital Characteristic

Characteristic	Mean Synthetic Control Weight
ln(Price), T-1	0.505
ln(Price), T-2 and before	0.262
Beds	0.036
FTEs	0.040
Number of Technologies	0.037
Mean LOS	0.041
Mean MS-DRG Weight	0.038
WTP per patient	0.041

Sources: HCCI inpatient claims data, FTC PNO data, Cooper, et al. and authors' hospital merger data

E Supplemental Figures

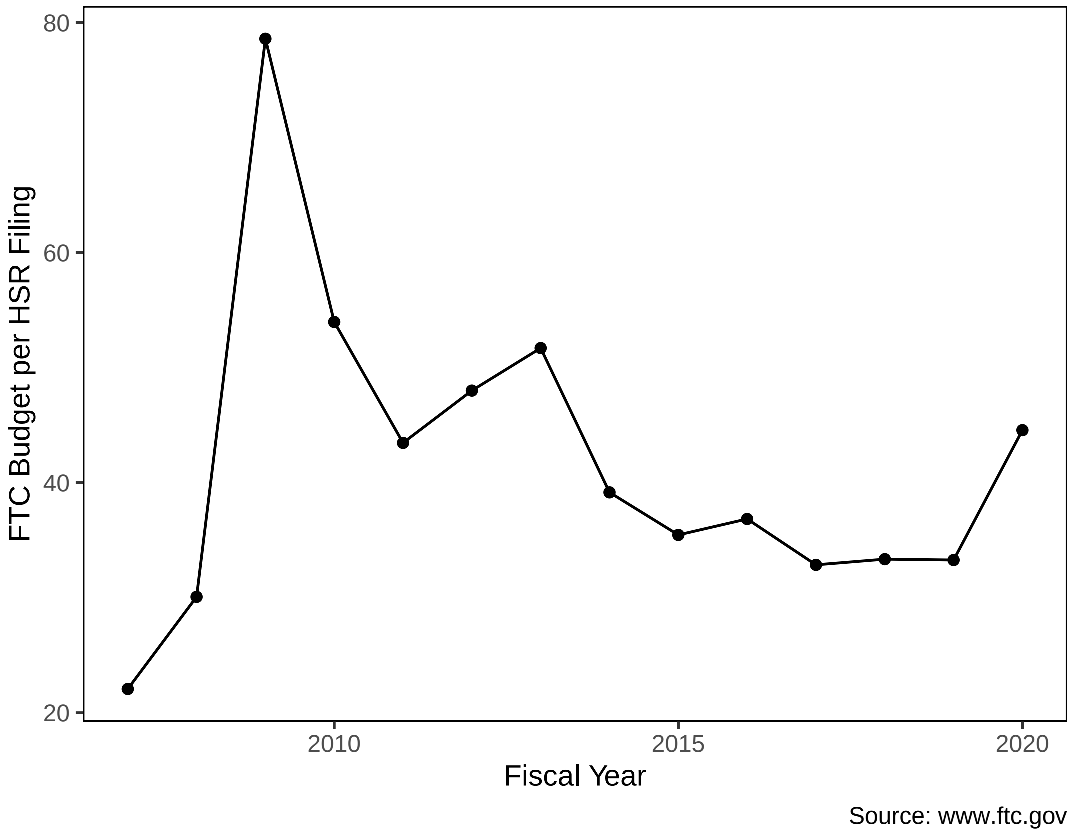

Figure OA1: FTC Maintain Competition Budget per HSR Filing, 2007-2020
(in thousands of dollars)

Table OA5: Comparison of Hospitals Included and Excluded from Regression Analysis

	All			In Analysis		
	N	mean	sd	N	mean	sd
Beds	7666	184.11	176.88	2960	258.00	199.27
WTP Per Patient	7017	1.14	0.14	2948	1.18	0.14
For Profit (Binary)	7666	0.40	0.49	2960	0.41	0.49
Not for Profit (Binary)	7666	0.55	0.50	2960	0.57	0.50
Teaching Hospital (Binary)	7666	0.26	0.44	2960	0.41	0.49

Sources: HCCI inpatient claims data, FTC PNO data, Cooper, et al. and authors' hospital merger data

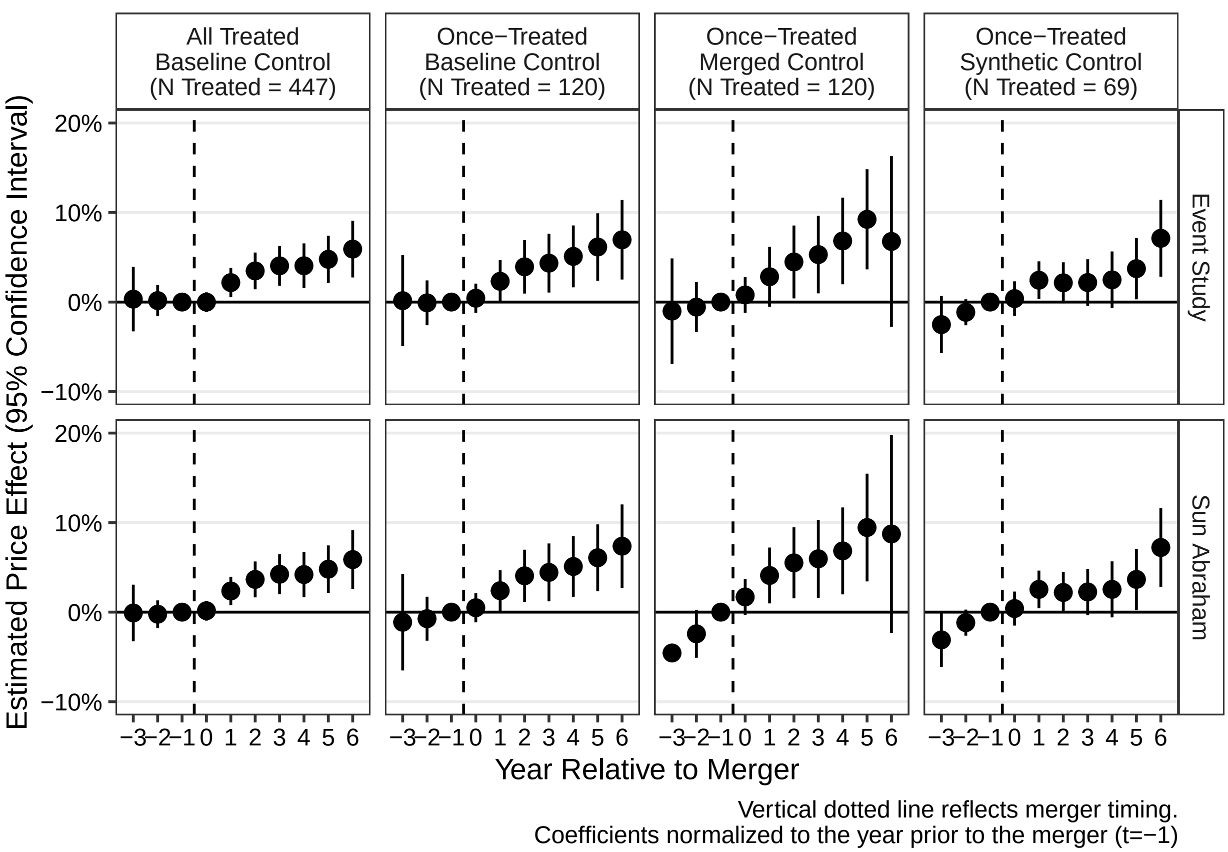

Figure OA2: Unconditional Price Effects, Mergers Pre-2012

Results from regressions described in text. Tables of coefficients in Appendix Tables OA8 and OA9.

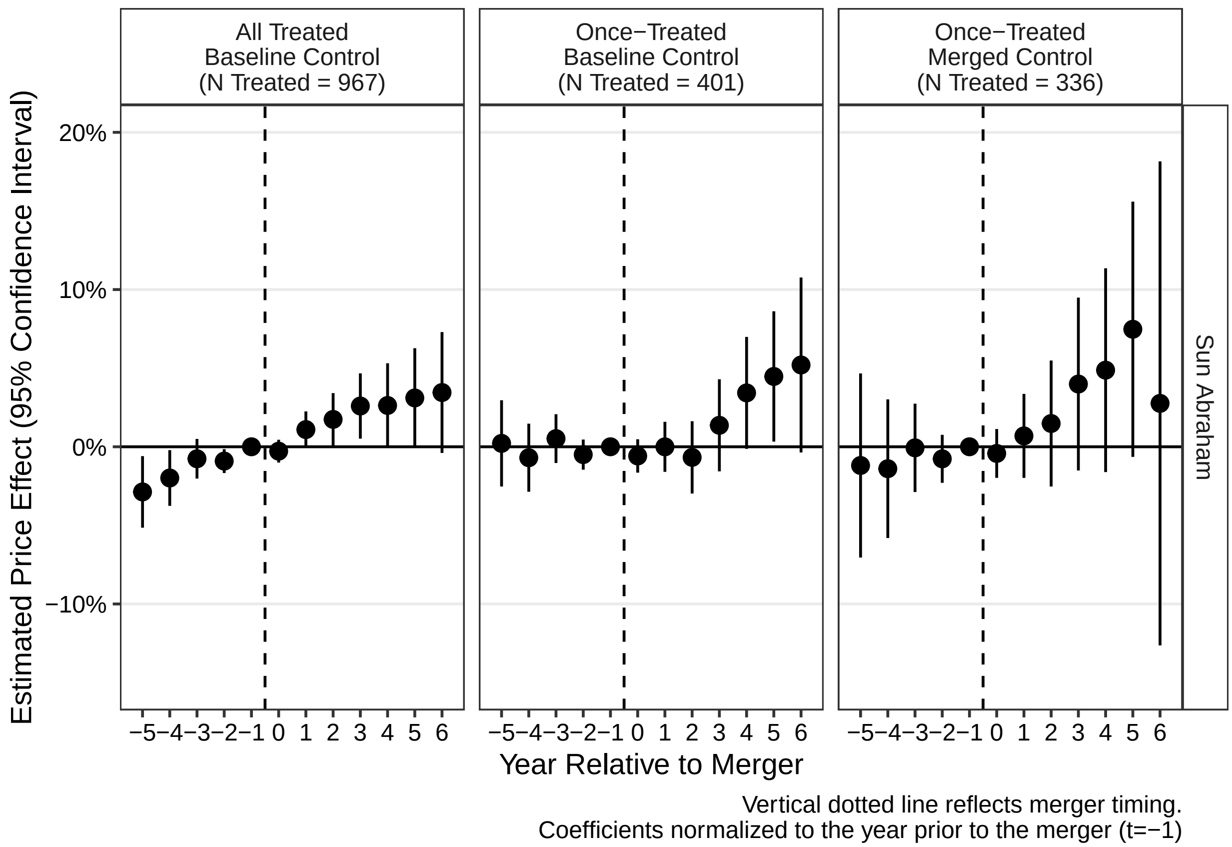

Figure OA3: Unconditional Price Effects Weighted by Number of Beds

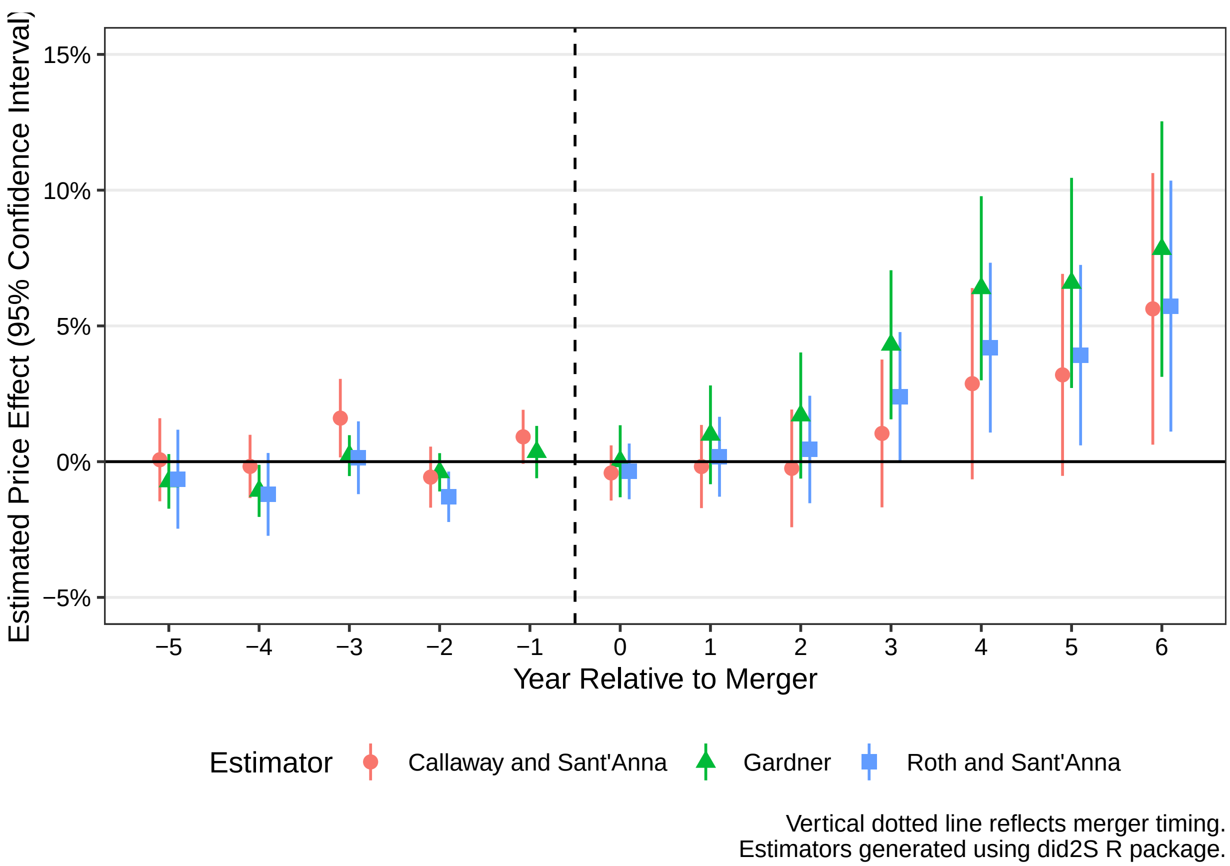

Figure OA4: Unconditional Price Effects Using Different DiD Methods

F Supplemental Tables

Table OA6: Price Effects: By Year Relative to Merger - Event Study Results

	All Treated	Baseline Control	Merged Control	Synthetic Control
Year::-8	0.004	0.043		0.038
	(0.021)	(0.021)		(0.015)
Year::-7	-0.007	0.020	0.033	0.024
	(0.016)	(0.018)	(0.031)	(0.011)
Year::-6	-0.009	0.024	0.030	0.013
	(0.013)	(0.015)	(0.025)	(0.009)
Year::-5	-0.027	0.000	-0.007	-0.005
	(0.010)	(0.012)	(0.020)	(0.006)
Year::-4	-0.016	-0.007	-0.014	-0.014
	(0.009)	(0.011)	(0.015)	(0.006)
Year::-3	-0.007	0.005	0.001	-0.001
	(0.006)	(0.008)	(0.011)	(0.005)
Year::-2	-0.007	-0.005	-0.005	-0.006
	(0.004)	(0.006)	(0.007)	(0.004)
Year::0	-0.002	-0.003	0.005	0.001
	(0.004)	(0.005)	(0.007)	(0.006)
Year::1	0.015	0.004	0.021	0.006
	(0.006)	(0.008)	(0.011)	(0.008)
Year::2	0.028	0.009	0.042	0.008
	(0.008)	(0.011)	(0.016)	(0.010)
Year::3	0.034	0.026	0.053	0.025
	(0.009)	(0.013)	(0.020)	(0.012)
Year::4	0.037	0.047	0.073	0.038
	(0.011)	(0.015)	(0.023)	(0.016)
Year::5	0.045	0.053	0.102	0.041
	(0.012)	(0.016)	(0.027)	(0.017)
Year::6	0.057	0.061	0.081	0.073
	(0.016)	(0.021)	(0.047)	(0.022)

	All Treated	Baseline Control	Merged Control	Synthetic Control
Year::7	0.069	0.019		
	(0.022)	(0.038)		
Num.Obs.	13946	9110	3082	5418
R2	0.907	0.912	0.908	0.925
AIC	-21766.0	-14704.9	-5562.1	-11062.8
BIC	-21645.3	-14591.0	-5477.6	-10963.9
RMSE	0.11	0.11	0.10	0.09
Std.Errors	by: id_e	by: id_e	by: id_e	by: sc_id
FE: year	X	X	X	X
FE: id_e	X	X	X	
FE: sc_id				X

Table OA7: Price Effects: By Year Relative to Merger - Sun and Abraham Results

	All Treated	Baseline Control	Merged Control	Synthetic Control
Year::-8	-0.007	0.018		0.025
	(0.036)	(0.041)		(0.014)
Year::-7	-0.003	0.023	0.009	0.021
	(0.023)	(0.027)	(0.050)	(0.039)
Year::-6	-0.007	0.028	0.027	0.010
	(0.016)	(0.018)	(0.039)	(0.022)
Year::-5	-0.029	0.002	-0.012	-0.005
	(0.012)	(0.014)	(0.030)	(0.022)
Year::-4	-0.020	-0.007	-0.014	-0.012
	(0.009)	(0.011)	(0.022)	(0.016)
Year::-3	-0.008	0.005	-0.001	0.001
	(0.006)	(0.008)	(0.014)	(0.022)
Year::-2	-0.009	-0.005	-0.008	-0.004
	(0.004)	(0.005)	(0.008)	(0.042)
Year::0	-0.003	-0.006	-0.004	0.002

	All Treated	Baseline Control	Merged Control	Synthetic Control
	(0.004)	(0.005)	(0.008)	(0.022)
Year::1	0.011	0.000	0.007	0.007
	(0.006)	(0.008)	(0.014)	(0.018)
Year::2	0.017	-0.007	0.015	0.006
	(0.009)	(0.012)	(0.020)	(0.029)
Year::3	0.026	0.014	0.040	0.024
	(0.011)	(0.015)	(0.028)	(0.046)
Year::4	0.026	0.034	0.049	0.036
	(0.014)	(0.018)	(0.033)	(0.020)
Year::5	0.031	0.045	0.075	0.036
	(0.016)	(0.021)	(0.041)	(0.010)
Year::6	0.035	0.052	0.028	0.073
	(0.020)	(0.028)	(0.078)	(0.012)
Year::7	0.029	0.002		
	(0.027)	(0.067)		
Num.Obs.	13946	9110	3082	5418
R2	0.909	0.909	0.919	0.927
AIC	-21731.1	-14734.2	-5600.6	-11186.0
BIC	-21610.5	-14620.4	-5516.1	-11087.1
RMSE	0.11	0.11	0.10	0.09
Std.Errors	by: id_e	by: id_e	by: id_e	by: sc_id
FE: year	X	X	X	X
FE: id_e	X	X	X	
FE: sc_id				X

Table OA8: Price Effects: By Year Relative to Merger - Event Study Results (2009-2011)

	All Treated	Baseline Control	Merged Control	Synthetic Control
Year::-3	0.003	0.002	-0.010	-0.025
	(0.018)	(0.026)	(0.030)	(0.016)

	All Treated	Baseline Control	Merged Control	Synthetic Control
Year::-2	0.002	-0.001	-0.006	-0.011
	(0.009)	(0.013)	(0.014)	(0.007)
Year::0	0.000	0.004	0.008	0.004
	(0.006)	(0.008)	(0.010)	(0.010)
Year::1	0.022	0.023	0.028	0.024
	(0.008)	(0.012)	(0.017)	(0.011)
Year::2	0.035	0.039	0.045	0.022
	(0.010)	(0.015)	(0.021)	(0.012)
Year::3	0.040	0.043	0.053	0.022
	(0.011)	(0.017)	(0.022)	(0.013)
Year::4	0.041	0.051	0.068	0.025
	(0.013)	(0.018)	(0.025)	(0.016)
Year::5	0.048	0.061	0.092	0.037
	(0.013)	(0.019)	(0.029)	(0.017)
Year::6	0.059	0.070	0.068	0.071
	(0.016)	(0.023)	(0.049)	(0.022)
Year::7	0.070	0.027		
	(0.022)	(0.039)		
Num.Obs.	9377	6609	1398	3330
R2	0.907	0.914	0.905	0.946
AIC	-14480.2	-10638.0	-2745.3	-8298.4
BIC	-14401.6	-10563.3	-2692.8	-8237.3
RMSE	0.11	0.11	0.09	0.07
Std.Errors	by: id_e	by: id_e	by: id_e	by: sc_id
FE: year	X	X	X	X
FE: id_e	X	X	X	
FE: sc_id				X

Table OA9: Price Effects: By Year Relative to Merger - Sun and Abraham Results (2009-2011)

	All Treated	Baseline Control	Merged Control	Synthetic Control
Year::-3	-0.001	-0.011	-0.046	-0.031
	(0.016)	(0.027)	(0.029)	(0.015)
Year::-2	-0.002	-0.007	-0.024	-0.012
	(0.008)	(0.013)	(0.014)	(0.007)
Year::0	0.002	0.005	0.017	0.004
	(0.006)	(0.008)	(0.010)	(0.010)
Year::1	0.024	0.024	0.041	0.025
	(0.008)	(0.012)	(0.016)	(0.011)
Year::2	0.037	0.041	0.055	0.022
	(0.010)	(0.015)	(0.020)	(0.012)
Year::3	0.042	0.044	0.060	0.023
	(0.011)	(0.016)	(0.022)	(0.013)
Year::4	0.042	0.051	0.068	0.025
	(0.013)	(0.017)	(0.025)	(0.016)
Year::5	0.048	0.061	0.094	0.037
	(0.014)	(0.019)	(0.031)	(0.017)
Year::6	0.059	0.074	0.087	0.072
	(0.017)	(0.024)	(0.056)	(0.022)
Year::7	0.075	0.047		
	(0.025)	(0.050)		
Num.Obs.	9377	6609	1398	3330
R2	0.907	0.914	0.907	0.946
AIC	-14490.5	-10659.4	-2773.6	-8316.5
BIC	-14411.9	-10584.6	-2721.2	-8255.4
RMSE	0.11	0.11	0.09	0.07
Std.Errors	by: id_e	by: id_e	by: id_e	by: sc_id
FE: year	X	X	X	X
FE: id_e	X	X	X	
FE: sc_id				X

	All Treated	Baseline Control	Merged Control	Synthetic Control

Table OA10: Price Effects by Distance Band

Model	Distance Band	N Treated	Estimate	Std. Error	t value	Pr(>|t|)
Baseline Control	<5	26	0.0560454	0.0326451	1.7168064	0.0863026
Merged Control	<5	24	0.0699642	0.0405321	1.7261415	0.0850941
Synthetic Control	<5	18	0.1021551	0.0336888	3.0323208	0.0026387
Baseline Control	5-15	87	0.0778427	0.0267400	2.9110969	0.0036759
Merged Control	5-15	76	0.1050746	0.0379400	2.7694903	0.0058758
Synthetic Control	5-15	71	0.0517740	0.0182438	2.8378949	0.0048508
Baseline Control	15-30	79	0.0782068	0.0314483	2.4868379	0.0130393
Merged Control	15-30	72	0.0844739	0.0343852	2.4566960	0.0144460
Synthetic Control	15-30	60	0.0581055	0.0346241	1.6781784	0.0943536
Baseline Control	30-50	71	0.0108769	0.0376793	0.2886703	0.7728893
Merged Control	30-50	48	0.0453950	0.0445836	1.0182006	0.3091981
Synthetic Control	30-50	49	-0.0464156	0.0371722	-1.2486625	0.2127621
Baseline Control	>50	138	0.0296197	0.0208132	1.4231166	0.1549925
Merged Control	>50	116	0.0535966	0.0302739	1.7703932	0.0774233
Synthetic Control	>50	103	0.0292449	0.0173874	1.6819602	0.0936171

Table OA11: Price Effects by Diversion Ratio Band

Model	Diversion Band	N Treated	Estimate	Std. Error	t value	Pr(>|t|)
Baseline Control	>15%	31	0.1281530	0.0508873	2.5183680	0.0119343
Merged Control	>15%	24	0.1534693	0.0700073	2.1921900	0.0289414
Synthetic Control	>15%	22	0.1402886	0.0588390	2.3842794	0.0177340
Baseline Control	10%-15%	24	0.0338905	0.0450578	0.7521567	0.4521215
Merged Control	10%-15%	24	-0.0027107	0.0476510	-0.0568861	0.9546643
Synthetic Control	10%-15%	20	0.0430219	0.0348432	1.2347304	0.2178971
Baseline Control	5%-10%	33	0.0151014	0.0349174	0.4324905	0.6654717
Merged Control	5%-10%	29	0.0216091	0.0410005	0.5270443	0.5984550
Synthetic Control	5%-10%	24	0.0350352	0.0301249	1.1629992	0.2457541
Baseline Control	<5%	313	0.0461503	0.0167201	2.7601632	0.0058755
Merged Control	<5%	259	0.0697386	0.0252174	2.7654919	0.0059470
Synthetic Control	<5%	235	0.0308890	0.0140094	2.2048871	0.0282207

Table OA12: Price Effects for Distant Mergers Within and Out of State

Model	HHI Band	N Treated	Estimate	Std. Error	t value	Pr(>|t|)
Baseline Control	In State	89	0.0522049	0.0207620	2.514447	0.0121163
Merged Control	In State	73	0.1349540	0.0368710	3.660166	0.0003588
Synthetic Control	In State	68	0.0548371	0.0212078	2.585706	0.0111304
Baseline Control	Out of State	49	-0.0339193	0.0287765	-1.178715	0.2388610
Merged Control	Out of State	43	0.0479019	0.0463105	1.034362	0.3027894
Synthetic Control	Out of State	35	-0.0421132	0.0282293	-1.491829	0.1388304

Table OA13: Price Effects by Insurer HHI

Model	HHI Band	N Treated	Estimate	Std. Error	t value	Pr(>|t|)
Baseline Control	0-2,500	74	0.0921610	0.0232779	3.9591569	0.0000802
Merged Control	0-2,500	61	0.1040272	0.0347780	2.9911777	0.0029510
Synthetic Control	0-2,500	60	0.0627846	0.0212904	2.9489685	0.0034391
Baseline Control	2,500-3,500	172	0.0681558	0.0193554	3.5212802	0.0004475
Merged Control	2,500-3,500	149	0.0916689	0.0270071	3.3942496	0.0007569
Synthetic Control	2,500-3,500	124	0.0639067	0.0161112	3.9666118	0.0000912
Baseline Control	3,500-10,000	155	-0.0131041	0.0248108	-0.5281618	0.5974961
Merged Control	3,500-10,000	126	0.0006611	0.0320120	0.0206512	0.9835342
Synthetic Control	3,500-10,000	117	-0.0295444	0.0237505	-1.2439496	0.2144892

Table OA14: Price Effects from Merger Size

Model	Merger Type	N Treated	Estimate	Std. Error	t value	Pr(>|t|)
Baseline Control	Big to Big	103	0.0416122	0.0225650	1.8441049	0.0655361
Merged Control	Big to Big	95	0.1285248	0.0385188	3.3366732	0.0010916
Synthetic Control	Big to Big	78	0.0465623	0.0187224	2.4869767	0.0145055
Baseline Control	Small to Big	14	-0.0535639	0.0302495	-1.7707382	0.0769836
Merged Control	Small to Big	12	-0.1237151	0.0265363	-4.6620992	0.0000073
Synthetic Control	Small to Big	10	-0.0338664	0.0216063	-1.5674299	0.1201114
Baseline Control	Small to Small	20	-0.0655857	0.0453613	-1.4458490	0.1486097
Merged Control	Small to Small	9	0.0402291	0.0550848	0.7303127	0.4664461
Synthetic Control	Small to Small	14	-0.0674602	0.0910088	-0.7412489	0.4602457

Table OA15: Difference in Price Effects for Non-HSR Mergers

Model	Estimate	Std. Error	t value	Pr(>|t|)
Baseline Control	-0.0025546	0.0274278	-0.0931382	0.9258111
Merged Control	-0.0093051	0.0299745	-0.3104329	0.7563935
Synthetic Control	0.0156891	0.0267028	0.5875443	0.5572802

Table OA16: Difference in Price Effects for Non-HSR Mergers: Diversion Ratio Above 10%

Model	Estimate	Std. Error	t value	Pr(>|t|)
Baseline Control	0.0111681	0.0582645	0.1916796	0.8480472
Merged Control	0.0690231	0.0867791	0.7953887	0.4298699
Synthetic Control	0.0158456	0.0463726	0.3417016	0.7343207

Table OA17: Price Effects by Agency Investigation Status

Model	Investigation Status	N Treated	Estimate	Std. Error	t value	Pr(>|t|)
Baseline Control	Early ET	78	0.0877536	0.0249365	3.519080	0.0004557
Merged Control	Early ET	65	0.1243363	0.0398352	3.121269	0.0020744
Synthetic Control	Early ET	51	0.0363503	0.0223726	1.624765	0.1063844
Baseline Control	No ET or 2R	73	-0.0366021	0.0222270	-1.646735	0.0999774
Merged Control	No ET or 2R	56	-0.0295589	0.0287547	-1.027971	0.3052371
Synthetic Control	No ET or 2R	60	-0.0383241	0.0251582	-1.523323	0.1298558
Baseline Control	2R	45	0.0787925	0.0204940	3.844666	0.0001296
Merged Control	2R	45	0.1109159	0.0406422	2.729083	0.0069319
Synthetic Control	2R	35	0.0622005	0.0181887	3.419738	0.0008143

Table OA18: Price Effects By Merger Cohort: Year 2 Post-Merger

Model	Year of Merger	N Treated	Estimate	Std. Error	t value	Pr(>|t|)
Baseline Control	2009	27	0.0801270	0.0309448	2.5893471	0.0097457
Merged Control	2009	27	0.1161707	0.0343103	3.3858831	0.0007797
Baseline Control	2010	54	0.0298166	0.0160709	1.8553160	0.0638247
Merged Control	2010	54	0.0475445	0.0247132	1.9238509	0.0550826
Synthetic Control	2010	43	0.0263502	0.0128896	2.0443027	0.0417964
Baseline Control	2011	37	0.0273993	0.0325101	0.8427932	0.3995316
Merged Control	2011	37	0.0214498	0.0348542	0.6154163	0.5386298
Synthetic Control	2011	26	0.0112929	0.0227198	0.4970491	0.6195185
Baseline Control	2012	24	-0.0060696	0.0414662	-0.1463750	0.8836528
Merged Control	2012	24	0.0002465	0.0436307	0.0056493	0.9954953
Synthetic Control	2012	22	0.0035921	0.0390308	0.0920327	0.9267335
Baseline Control	2013	67	0.0079139	0.0213487	0.3706968	0.7109364
Merged Control	2013	67	0.0040427	0.0338936	0.1192755	0.9051170
Synthetic Control	2013	58	0.0267115	0.0222033	1.2030437	0.2299079
Baseline Control	2014	61	-0.0453856	0.0216217	-2.0990765	0.0360429
Synthetic Control	2014	51	-0.0366610	0.0220879	-1.6597739	0.0980048

Table OA19: Price Effects By Merger Cohort: Year 3 Post-Merger

Model	Year of Merger	N Treated	Estimate	Std. Error	t value	Pr(>|t|)
Baseline Control	2009	26	0.0907944	0.0385008	2.3582474	0.0185397
Merged Control	2009	26	0.1254533	0.0431664	2.9062724	0.0038610
Baseline Control	2010	54	0.0320089	0.0191996	1.6671630	0.0957733
Merged Control	2010	54	0.0406402	0.0263535	1.5421190	0.1238354
Synthetic Control	2010	43	0.0347905	0.0160147	2.1724125	0.0306073
Baseline Control	2011	35	0.0290046	0.0315633	0.9189327	0.3583369
Merged Control	2011	35	0.0398900	0.0345762	1.1536832	0.2493189
Synthetic Control	2011	26	0.0056925	0.0219085	0.2598294	0.7951736
Baseline Control	2012	24	0.0022800	0.0426255	0.0534899	0.9573515
Merged Control	2012	24	-0.0032930	0.0475350	-0.0692749	0.9448055
Synthetic Control	2012	22	0.0182485	0.0417310	0.4372883	0.6622169
Baseline Control	2013	67	0.0074986	0.0219126	0.3422025	0.7322654
Synthetic Control	2013	58	0.0263965	0.0223514	1.1809783	0.2385468

Table OA20: Price Effects By Merger Cohort: Year 4 Post-Merger

Model	Year of Merger	N Treated	Estimate	Std. Error	t value	Pr(>|t|)
Baseline Control	2009	27	0.1023885	0.0359018	2.8519023	0.0044290
Merged Control	2009	27	0.1279566	0.0405785	3.1533080	0.0017361
Baseline Control	2010	52	0.0568495	0.0200685	2.8327726	0.0047008
Merged Control	2010	52	0.0822499	0.0289604	2.8400756	0.0047406
Synthetic Control	2010	43	0.0459848	0.0180346	2.5498151	0.0112752
Baseline Control	2011	35	0.0025779	0.0340337	0.0757442	0.9396367
Merged Control	2011	35	0.0018457	0.0401937	0.0459204	0.9633966
Synthetic Control	2011	26	-0.0055103	0.0288992	-0.1906730	0.8489107
Baseline Control	2012	23	0.0581770	0.0508879	1.1432389	0.2531939
Synthetic Control	2012	22	0.0638729	0.0456919	1.3979027	0.1631750

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