HAVE MERGERS RAISED PRICES? EVIDENCE FROM U.S. RETAIL

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ABSTRACT. We document the price and quantity effects of all US retail mergers from 2006–2017 associated with deals larger than $340 million. Prices increase by 0.49% on average for merging parties, with an interquartile range of almost 5%. Non-merging parties exhibit slightly smaller price changes on average. Total quantities decline on average by 3–5%, but there is even larger variation across mergers. We investigate the role of synergies and market power by analyzing the timing of price changes, the relationship with locations of production facilities, and measures of market structure. We collect data on merger enforcement (remedy proposals in our case), and through the lens of a simple model, we estimate that agency preferences are such that they aim to challenge mergers where prices are expected to increase by more than 3.7–5.6% overall, or about 8.2–8.8% for merging parties.

KEYWORDS. Antitrust, Merger Retrospectives, Market Power.

JEL CODES. D43, L12, L13, L41.

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I. Introduction

The Department of Justice and the Federal Trade Commission reviewed over two thousand prospective mergers and acquisitions in fiscal year 2019 (Simons and Delrahim, 2019). The antitrust enforcement agencies are tasked with identifying deals that lessen competition and convincing a court to either block them or force the parties to adopt remedies. This is a difficult task, as the price and consumer welfare effects of mergers are in general ambiguous: the standard academic treatment of horizontal mergers (Williamson, 1968) recognizes that increases in market power can be compensated by cost savings due to marginal cost synergies. Furthermore, mergers can induce changes in product quality or lead to repositioning (Sweeting, 2010; Fan, 2013). Accordingly, whether approved mergers typically increase or decrease prices, quantities, and product offerings is an empirical question that is important for evaluating antitrust policy in the US. Researchers have constantly stressed the importance of empirical work on merger retrospectives to understand what mergers have actually done (Whinston, 2007; Carlton, 2009; Ashenfelter et al., 2014).

While a fairly large body of prior work, reviewed in Section II, has conducted such retrospectives, mergers that have been studied are selected on particular dimensions. For a merger to be analyzed in a retrospective, it must satisfy three conditions: (1) the merging parties must have proposed it, (2) the enforcement agencies must have allowed it to go through (or unsuccessfully challenged it), and (3) researchers must have chosen to study it. Each step of this funnel leads to some selection in the set of mergers analyzed. The final step—the decision to even study a merger—is based on dimensions whose effects are unclear: interest in the popular press, data availability, and the potential for publication. Such selection has been shown to lead to significant bias in other economic contexts (Shapiro et al., 2021). Accordingly, even aggregating results over many published studies can lead to an unrepresentative distribution of merger effects.

This paper provides a systematic analysis of the price and quantity effects of mergers in US consumer packaged goods from 2006–2017. We analyze 108 product markets (e.g., canned soup or soluble coffee) in 40 transactions (e.g., a merger between large food conglomerates), consisting of essentially all transactions with a deal size larger than $340 million with consumer packaged goods

products likely to be sold through retail outlets. By analyzing the universe of mergers satisfying a particular deal size cutoff, we address the final step of the selection channel: our set of mergers is necessarily representative of (large) mergers that are proposed and approved in this industry. Our second contribution is to shed light on the second selection channel mentioned above—selection on enforcement. By connecting mergers to enforcement actions by the DOJ and FTC, we explicitly study the selection of mergers into approval (without remedies) and in the process quantify agency preferences.

Our baseline estimates of the effects of mergers rely on comparisons within geographies and within products, before and after the merger is completed, controlling for geography-specific time trends and seasonality. We supplement this analysis by controlling for changes in demographics and input costs, to account for demand- and supply-side characteristics that may have price effects. For over 90% of our mergers, we can also use the prices of products in geographic markets where the merging parties have negligible presence as a control.

Our first set of results, presented in Section IV, is that merging parties increase their prices by 0.5% on average in the two years following the merger. However, there is substantial heterogeneity in this distribution: the first quartile of price effects corresponds to a price decrease of 2.5%, and the third quartile corresponds to a price increase of 2.8%. Non-merging parties have a small price decrease of 0.2% on average with a slightly narrower distribution of price changes—with an interquartile range of 3.4 pp. Overall, the average effect of mergers on price changes is near zero, averaging to about a decrease of 0.2%. These estimates are fairly robust across specifications.

We repeat the analysis for quantities and measures of product changes. We document a drop in total quantities sold overall: merging parties see a reduction of about 5.5% in quantities sold and non-merging parties see a reduction of about 3.8%. The effects on quantities, however, are far more heterogeneous than the ones on price—with an interquartile range of almost 17 pp for merging parties and 10 pp for non-merging parties. On average, we find no effect of mergers on product introductions or removals—for either merging or non-merging parties.

What contributes to the heterogeneity in these price effects? In the basic Williamson (1968)

framework, price changes are due to synergies and changes in market power. Beyond this framework, mergers could induce changes in quality, product assortment, or pricing strategies (such as discrimination). A full welfare analysis of the trade-offs requires a structural model of demand and cost which is beyond the scope of this paper. Instead, we examine patterns in these price changes further to determine the extent to which these channels affect prices.

We first analyze the timing of the price changes. We find that for mergers that led to price increases (in the top quartile of price changes), these price increases materialized very soon after completion: by about 4–6 months after completion, prices stabilized to the new level. This finding is consistent with price increases being due to exercising increased market power rather than quality improvements, which likely take more time. On the other hand, price decreases happen more gradually. Moreover, mergers generally seem to exhibit price decreases between 12–24 months after completion. These latter observations are consistent with the presence of synergies, which take time to materialize (Focarelli and Panetta, 2003; Whinston, 2007).

We continue this analysis by next looking for direct evidence of synergies. We collect data on the production facilities of all firms and use the changes in distance to the nearest production facility as one measure of the synergies generated by the merger. We find that markets with greater decreases in distance typically have larger price decreases. This is consistent with mergers engendering distribution synergies. Although this is merely one source of these synergies, systematic evidence of synergies has remained limited in the literature.

We also study the correlation between price changes and market structure. Agencies often use measures of market concentration—such as the Hirschman-Herfindahl Index (HHI) and the change in this index (ΔHHI)—as measures of the competitive impact of mergers: mergers with especially high HHI or ΔHHI are "presumed likely to enhance market power."[1] Using comparisons both across geographies within a merger and across mergers, we find that prices changes tend to be higher when overall ΔHHI is larger (especially for mergers with low HHI), consistent with standard theories (e.g., Nocke and Whinston (2021)). However, we see no similar impact of the post-merger

[1]The 2010 revision of the Horizontal Merger Guidelines does advocate from a move away from such "structural presumptions" towards broader analysis of the merger.

HHI itself. We do not see any significant patterns in terms of HHI and ΔHHI when comparing price changes within-merger across geographic markets.

These results provide a systematic analysis of the effects of completed mergers in one sector, which is a quantity that has been discussed by many papers (Carlton, 2009) but rarely documented. However, as mentioned earlier, even though there is no selection from completion to being considered in our dataset, there is still selection into completion: mergers that would be especially anti-competitive do not get proposed, are successfully challenged and blocked, or go through with divestitures. This leads to two caveats about our results. First, we do not claim that these distributions are representative of the effects of all possible mergers, or all profitable mergers. Rather, these distributions are representative of the effects of observed mergers. Second, as Carlton (2009) illustrates, due to both this selection concern and the fact that the agency has at best a noisy signal of the future price change at the time the merger is proposed, the distribution of realized price changes does not inform whether an agency is too strict (blocking mergers with negative price effects) or too lax (allowing mergers with positive price effects).

Our second contribution, therefore, is to evaluate the stringency of current antitrust policy. To do so, we collect data on enforcement actions for each of the mergers in our dataset. In Section VI, we estimate a deliberately simple model of the agencies' decision to propose a remedy for a merger. In this model, the agency receives a noisy signal of the price change of the merger and proposes a remedy if this signal exceeds a threshold. Using data on enforcement decisions together with the estimates of the realized price changes, we estimate that the US antitrust agencies aim to propose remedies for mergers with an average price increase larger than 3.7–5.6%. When considering price changes of merging parties, the threshold is 8.2–8.8%. These thresholds aim to tackle Carlton (2009)'s critique of merger retrospectives head on—and in doing so, inform the current debate about the laxness of antitrust standards in the US (Kwoka, 2014; Scott Morton, 2019; Shapiro, 2021; Nocke and Whinston, 2021).

II. Prior Research on Merger Retrospectives

Merger analysis is one of the primary policy applications of industrial organization, and studying the outcomes of mergers that have actually occurred is a key input into our understanding of them. In the late 2000s, Whinston (2007) noted that looking at price effects of actual mergers is "clearly an area that could use more research" (p. 2425), and Carlton (2009) highlighted the need for more data to guide antitrust reform. Since then, there have been a growing number of merger retrospectives in the literature, culminating in Kwoka (2014), which provides an especially valuable survey and meta-analysis as well as a framework for organizing this literature. Farrell et al. (2009) and Hunter et al. (2008) provide other surveys, and Asker and Nocke (2021) discusses retrospectives in the broader context of the theory of mergers and collusion.

One class of merger retrospectives involve in-depth studies of a small handful of mergers, usually still focusing on prices and quantities. Papers have studied airlines (Peters, 2006; Kwoka and Shumilkina, 2010; Luo, 2014; Das, 2019), assorted consumer products (Ashenfelter and Hosken, 2010; Weinberg and Hosken, 2013), appliances (Ashenfelter et al., 2013), beer (Ashenfelter et al., 2015; Miller and Weinberg, 2017), hospitals (Haas-Wilson and Garmon, 2011; Garmon, 2017) and gasoline (Simpson and Taylor, 2008; Lagos, 2018). Relative to these papers (and many more that we do not have space to cite in this section), we consider a much larger set of mergers, which helps search for systematic patterns across a variety of mergers.

Another class of studies analyzes groups of mergers in an industry in one go. Examples include Kim and Singal (1993) on airlines and Focarelli and Panetta (2003) on Italian banks, and a larger set are summarized in Kwoka (2014). Such studies provide a more comprehensive view on mergers in an industry, as they are less subject to the critique that the set of mergers under consideration are selected. (For instance, Kim and Singal (1993) study the price effects of all US airline mergers in 1985–88.) However, as Kwoka (2014) notes, these studies often still only report the average price change of all mergers instead of the distribution, and they do not tie price changes to particulars of the merger (and in particular enforcement actions). Our analysis considers distributions and their relationship to enforcement actions, and we interpret the distribution to quantify the agencies'

objectives.

Kwoka (2014) puts together estimates with the goal of developing a broad understanding of the effects of consummated mergers.[2] From the studies of single mergers, he concludes that while there is substantial variation across mergers in their price effects, on average prices increase by 4–5%. While this provides an important benchmark, these estimates must be interpreted with some caution. Kwoka (2014) is careful to analyze the universe of reputable papers on merger retrospectives, but the mergers selected by the authors of the underlying papers are guided by data availability, the importance of the merger, and publication potential. This may lead to mergers being unrepresentative of any particular sector of the economy: as Vita and Osinski (2018) point out, for instance, almost one-quarter of the 42 data points studied in the analysis of individual mergers come from mergers of academic journals. More importantly, we cannot say that this distribution is representative of all completed mergers, as the process of forming this meta-analysis does not deal with the selection into publication, whose effect is difficult to interpret. One main contribution of our paper is that by developing a database ourselves and selecting mergers independently of any of the aforementioned considerations, we can avoid such selection.

We conclude by mentioning two recent papers that are especially related to this study. Atalay et al. (2020) have an interesting analysis of the effect of mergers on product availability—an outcome not typically considered in merger retrospectives—using a representative sample of retail mergers. They find that mergers lead to a reduction in product availability on average, with the acquiror dropping distant products of the target. Majerovitz and Yu (2021) also estimates price and revenue effects of mergers in retail, documenting large asymmetries in the sizes of targets and acquirors. We view these studies as complementary: using different control groups and different decision on how to process data, we arrive at similar conclusions for the average price effect of mergers in retail. Our study has a more explicit focus on other effects of mergers (such as quantities and product assortment), investigating the presence of synergies, analyzing the structural presumptions, and

[2]The Federal Trade Commission manages a bibliography of merger retrospectives at https://www.ftc.gov/policy/studies/merger-retrospective-program/bibliography. However, we are not aware of a meta-analysis of this larger set of studies, and such a meta-analysis would still suffer from some of the unavoidable sample selection issues in Kwoka (2014).

understanding the preferences of the enforcement agencies.

III. Data and Sample Selection

In this section, we discuss the data sources (Section III.A) and process to select the sample for analysis (Section III.B). We then describe a number of properties of the sample (Section III.C) and provide observations about mergers that are approved by antitrust agencies.

III.A. Data Sources

The first steps of any merger retrospective are to identify the merger of interest and find detailed data on prices and quantities (and possibly more). Given our goal of analyzing a representative set of mergers in an industry, we must limit ourselves to industries where mergers can be easily identified and price data is systematically available. We begin with the set of mergers tracked by SDC Platinum from Thompson Reuters, which provides comprehensive information on mergers, acquisitions, and joint ventures. We then restrict to mergers involving manufacturers of products that are sold in groceries and mass merchandisers, for which detailed price and quantity data are available in the NielsenIQ Retail Scanner Dataset.

NielsenIQ describes this dataset as providing "scanner data from 35,000 to 50,000 grocery, drug, mass merchandise, and other stores, covering more than half the total sales volume of US grocery and drug stores and more than 30 percent of all US mass merchandiser sales volume. Data cover the entire United States, divided into 52 major markets." The data cover 2.6–4.5 million UPCs, depending on the year, and include food, nonfood grocery items, health and beauty aids, and select general merchandise. For each UPC, Nielsen provides sales at the store-week level, along with the average price at which the product was sold. Nielsen also provides some information about the product, including a number of product characteristics and a classification of the product into a "module." As discussed in Section III.B, we use these modules to guide market definitions. We have access to this dataset from 2006 to 2018.

After identifying mergers and obtaining prices and quantities at the product level, we must map

each product to an owner, which is important for allocating products to merging and non-merging parties as well as for computing measures of market structure. Unfortunately, Nielsen does not provide ownership of each product. We therefore augment the Nielsen dataset with information from Euromonitor Passport, which tracks ownership over time. When needed, we supplement Euromonitor with further internet searches to manually match UPCs to owners. This is a departure from prior research working with NielsenIQ data, as ownership of products is usually obtained by looking at the first six to nine digits of a UPC, which correspond to a product's "company prefix"—a unique identifier of the company that owns the UPC. This approach is problematic when dealing with mergers and acquisitions, as the transfer of company prefixes during an acquisition can take up to a year, and there is no hard and fast rule determining whether company prefixes are transferred from acquirer to target after a partial divestiture.[3] Instead, working with Euromonitor Passport data allows us to build the entire product portfolio of the main players in each product market for the relevant time horizon, including immediately before and after the merger.

To account for demand- and supply-side characteristics that could influence prices, we supplement Nielsen with data from a number of other sources. First, for each merger, we collect a set of variables—in general, measures of input costs—that could shift production costs. We list inputs for products in each merger (e.g., wheat for cereal) and obtain commodity price indices, typically available from FRED.[4] Second, we collect data on demographics, to help control for changes in factors that could affect demand. We use demographic data county-level data from the American Community Survey and aggregate to the DMA level.

Transportation costs are another source of supply-side heterogeneity for which we seek to control. To proxy for these costs, we collect production and/or distribution facility locations for each product and compute the distance between the nearest facility and the DMA in which a product is sold. Unfortunately, there is no data source that systematically lists these facilities. In practice, there are three main ways to obtain this information: (1) Company 10K reports filed with the SEC

[3]See Section 1.6 of the GS1 General Specifications, Release 22.0, for complete details.

[4]There are cases in which multiple mergers involve the same product module. We ensure that the same set of cost shifters is used for mergers that share product modules.

often list production and distribution facilities, (2) the Food and Drug Administration maintains an inspection registry of food production facilities that are mapped to the companies that own them,[5] and (3) company websites sometimes list production facilities. We conduct a comprehensive search of these three souces to obtain facility locations, when available. If possible, we assign specific products to production facilities and otherwise assume that products can be produced at all facilities.

Finally, for our analysis of enforcement stringency in Section VI, we require data on enforcement actions pursued by the DOJ and FTC. In particular, we recover whether the agencies required divestitures for a given deal to be approved and which product markets within that deal were the subject of scrutiny. We obtain this information from publicly-available case filings, including Complaints and publicly-recorded Decision and Order documents, available on the websites of the DOJ and FTC.[6]

III.B. Merger Selection and Market Definition

To form our sample, we aim to identify all mergers for which the two parties competed in at least one market during the at-issue period (i.e., the period spanning 24 months before the merger's announcement to 24 months past the merger's completion date). To do so, we begin with SDC Platinum dataset and restrict to completed deals that took place on or after 2007, where (1) either the target or acquirer is in the United States, (2) the acquirer is not classified as "Investment and Commodity Firms, Dealers, Exchanges," (3) the deal involves SIC codes that satisfy a broad interpretation of retail products, and (4) the deal size is above $340 million. Table A.1 in Appendix A tabulates the SIC codes of both the deals that pass these initial filters and of the deals that comprise our final dataset.

Most of the deals that survive this initial filtering process either involve firms that do not sell retail products, or involve firms that only sell products that are not tracked in the NielsenIQ Scanner Dataset. To identify the deals that are relevant, we analyze each merger's press release, as well as

[5]This registry can be accessed at https://www.accessdata.fda.gov/scripts/inspsearch/.

[6]For more information on available documents, see https://www.justice.gov/atr/antitrust-case-filings-alpha (DOJ) and https://www.ftc.gov/enforcement/cases-proceedings (FTC).

the merging parties' SEC filings for the year prior to the merger, and identify their retail brands, if they have any. We then search for those brands in the Product files of the NielsenIQ Scanner Dataset.

Whenever both the target and the acquirer own brands that are present in these files, the next step is to determine whether there is product market overlap between them. NielsenIQ categorizes products into product groups, broad categories such as "Prepared Foods - Frozen" or "Condiments, Gravies and Sauces", and product modules, finer subcategories such as "Soup - Frozen - Refrigerated", "Entrees - Meat - 1 Food - Frozen", "Barbeque Sauces" or "Sauce Mix - Taco". Although these categories divide products in a reasonable fashion, they are not designed to represent product markets, and the degree of granularity varies significantly across product groups. For example, the Nuts product group includes as modules "Nuts - Cans", "Nuts - Jars", "Nuts - Bags", and "Nuts - Unshelled", while the Snacks product group has product modules covering meat snacks, pork rinds, potato chips, puffed cheese snacks, pretzels, and popcorn, among others. Rather than defining product markets as either product groups or product modules, we define markets as groups of product modules based on our industry knowledge. These categorizations represent our best attempt to define product market overlap. For example, we group all nuts into a single market, but separate the aforementioned snacks into separate markets. We believe that this is a better approach than uniformly following Nielsen product modules or groups. Table A.2 in Appendix A presents a list of product markets for the deals that are considered in our final sample. We find 87 deals, covering 570 product markets, where both target and acquirer sell at least one product in the same product market. In what follows, we will refer to a product market - transaction pair as a merger, so that if companies X and Y merge and they both sell products in product markets 1 and 2, that deal will generate two mergers for our dataset.

The fact that the merging parties sell at least one product in the same product market does not necessarily imply that the deal involves competitors. It could be the case that they sell products in different geographic markets, or at different moments in time, or one of the two parties could own a UPC with a negligible market share. For a merger to qualify for our final dataset, we require that

both target and acquirer own at least one brand in a particular product market, and that they sell the brand in the same geographic market at the same time in a two year window around the completion date of the deal. To check this condition, we look at all UPCs that belong in the product market and that are sold within a two year window of the deal, select those that have a non-negligible market share, and assign each of them to their owners using Euromonitor Passport data.

To determine which UPCs have a non-negligible market share, we compute revenue shares at the DMA-month level and begin by considering all UPCs that have a share of at least 1% in any DMA-month in a two year window after the merger. If this is more than 100 UPCs, we keep the 100 best-selling UPCs and any UPCs that have more than a 5% share in any region-month. With this sample of products, we check market coverage: the fraction of sales volume in the product market that is captured by our selected group of UPCs. If the 10th percentile of the distribution of market coverage across DMA-months is smaller than 60%, we repeat this exercise with 200 UPCs. If continues to be the case, we expand the universe to 300 UPCs. Finally, if coverage continues to be too low, we drop the initial share cutoff from 1% to 0.5% and finally to 0.1%. This procedure allows us to work with a tractable number of products but also to expand the set of UPCs that are included in our analysis whenever the product market is particularly varied. Averaging over all mergers in our sample, the average value of the 10th percentile coverage is 73.2%, and the average value of the median coverage is 81.8%. This coverage reassures us that we are capturing the relevant products in each product market.

Having established ownership for each major UPC in the product market, we only keep mergers where both the target and the acquirer sell at least one major UPC in the 24 months prior to completion of the deal. This yields our final dataset of 108 mergers.

III.C. Properties of Approved Mergers

Table 1 presents summary statistics for our final sample. Each row in this table corresponds to a Nielsen Product Group, which is a coarser categorization that our product market definitions (in Table A.2), but serves to illustrate in which broad product categories the mergers are taking place.

Product Group Name	N	Product Market Sales (Million USD / yr)	Merging Parties' Revenue Share	HHI	DHHI
All	107	796.4	21.8%	2,689.1	211.9
Candy	3	2901.5	13.2%	2,763.3	111.3
Gum	2	1000.1	43.3%	3,270.9	69.9
Jams, Jellies, Spreads	1	2.7	56.3%	2,455.2	100.6
Juice, Drinks - Canned, Bottled	1	2295.8	16.6%	1,695.2	17.8
Pet Food	3	1461.1	15.7%	3,641.3	67.5
Prepared Food - Ready-To-Serve	3	140.9	8.5%	4,357.7	16.5
Vegetables - Canned	3	59.0	11.9%	3,688.0	9.0
Cereal	2	1258.5	8.6%	2,271.4	26.3
Coffee	2	1201.4	19.3%	2,248.3	8.2
Condiments, Gravies, And Sauces	10	68.5	46.3%	3,116.2	820.7
Pickles, Olives, And Relish	3	57.0	19.5%	2,435.6	27.0
Spices, Seasoning, Extracts	2	64.3	49.8%	3,459.4	141.5
Bread And Baked Goods	14	797.9	23.3%	2,929.1	182.8
Cookies	1	1670.8	0.6%	2,734.5	0.1
Crackers	1	2716.5	14.6%	2,309.7	88.0
Snacks	9	743.7	10.6%	2,494.9	10.2
Baked Goods - Frozen	2	878.3	27.5%	1,701.7	128.6
Pizza / Snacks / Hors D'Oeuvres-Frozen	1	2687.3	35.7%	973.1	570.5
Prepared Foods - Frozen	2	1437.0	2.9%	1,379.8	4.3
Unprepared Meat / Poultry / Seafood-Frozen	1	366.3	10.6%	2,979.8	27.2
Packaged Meats-Deli	7	1434.6	14.8%	1,705.6	30.2
Detergents	2	1253.5	12.2%	2,455.3	154.8
Beer	2	4762.1	30.3%	3,186.9	538.6
Liquor	2	1189.5	8.1%	1,239.7	40.7
Stationery, School Supplies	2	83.4	11.9%	2,642.4	6.3
Cosmetics	10	205.3	23.8%	2,446.9	221.1
Fragrances - Women	1	88.7	7.6%	2,444.2	8.2
Grooming Aids	2	126.8	6.0%	5,705.3	1.5
Hair Care	7	418.4	25.9%	2,447.3	705.5
Men's Toiletries	2	63.2	33.3%	3,070.4	161.7
Skin Care Preparations	4	330.0	18.0%	2,014.6	96.1

Table 1: Summary Statistics for the Final Sample of Mergers

Panels (a) and (b) of Figure 1 present histograms of average pre-merger HHI and naive ΔHHI. Most mergers have average (across DMA-month) pre-merger HHIs around 2500, with some mergers reaching values over 5000. Most values of ΔHHI are low, close to zero, but several mergers have values over 200. Panel (c) shows a scatter plot of average pre-merger HHI and naive ΔHHI. The mergers with the largest values of ΔHHI tend to have baseline HHI levels around 3000, and mergers

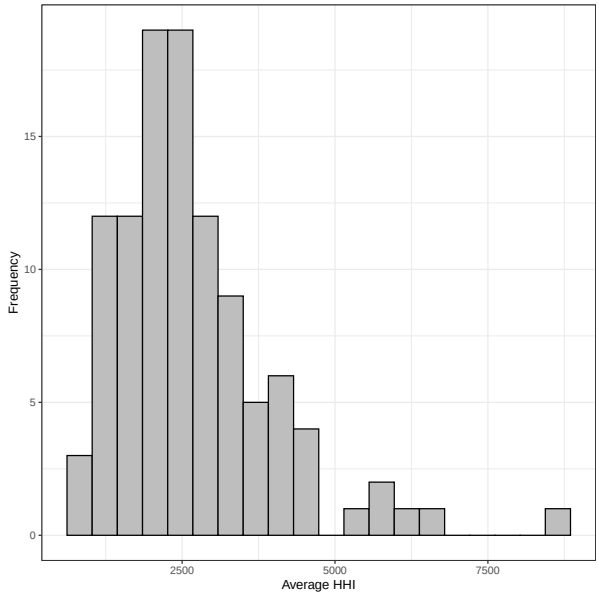
in markets with pre-merger HHI above 5000 seem to only be approved when ΔHHI is equal to zero. Panel (d) presents a scatter plot of average yearly sales of the merging parties (in millions of dollars) and naive ΔHHI. Around half of the mergers with ΔHHI over 500 are small, with average yearly sales for the merging parties below $100 million, but several of them feature ΔHHI near 500 and yearly sales around $1 billion. Overall, these patterns are consistent with the selection process that determines whether we observe a merger being consummated. That is, we expect greater antitrust scrutiny on mergers involving large product markets and high values of ΔHHI and pre-merger HHI. Nevertheless, several mergers involving substantial increases in naive ΔHHI have been approved, even in large product markets.

We also find that small mergers with large values of ΔHHI are able to survive antitrust scrutiny when they are embedded in large transactions. Figure 2 makes this point through a scatter plot of size of the transaction (in millions of dollars) and naive ΔHHI, where the size of the dots in the plot are proportional to the merging parties' sales in the merger. Recall that a particular deal often features many mergers, as there is overlap in multiple product markets. We find that extremely high values of ΔHHI—for example, above 1000—are only observed in markets with low sales, when they are a part of a considerably larger deal. For example, the point at the top-right corner of the plot is a market with sales around $200 million a year in a deal worth over $30 billion: a deal where no markets with sales above that amount had product market overlap. At the same time, we also observe ΔHHI values around 500 in mergers that seem to be the main market in a deal.

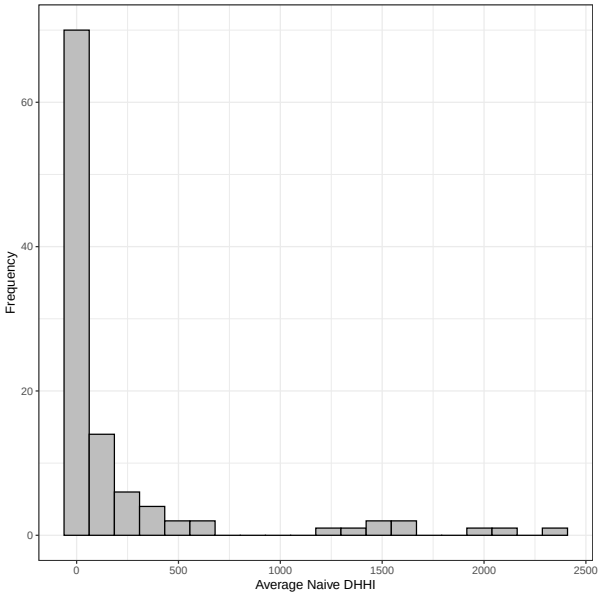
IV. Overall Effects on Prices and Quantities

IV.A. Empirical Strategy

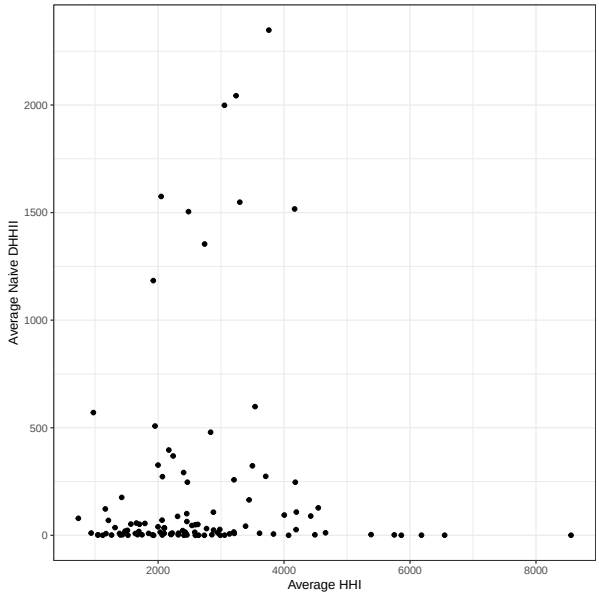
Retrospective analysis of mergers requires specifying a counterfactual for how prices (or quantities) would have evolved in the absence of the merger. Researchers have typically used one of three classes of controls. The first is the price changes of products of non-merging firms in the same market. For instance, Ashenfelter and Hosken (2010) use private label prices and those of rival products in their study of five consumer packaged goods mergers, and Haas-Wilson and Garmon



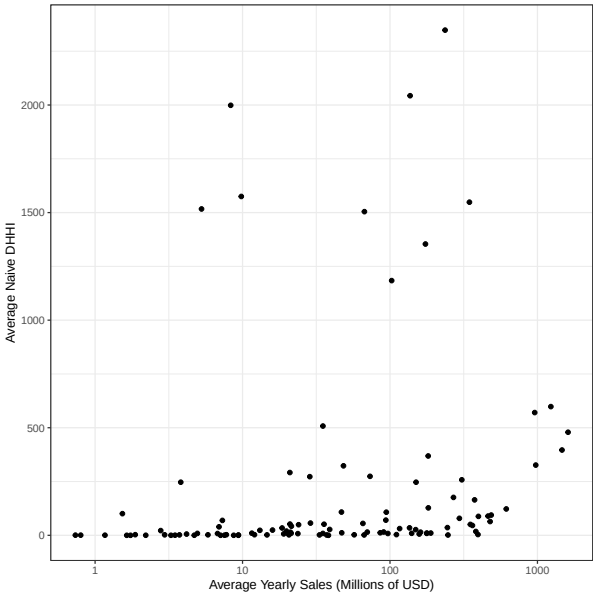
(a) Average Pre-Merger HHI



(b) Average Naive ΔHHI



(c) Average Pre-Merger HHI and ΔHHI



(d) Sales and ΔHHI

Figure 1: Distribution of pre-merger HHI, naive ΔHHI, and merging parties' yearly sales

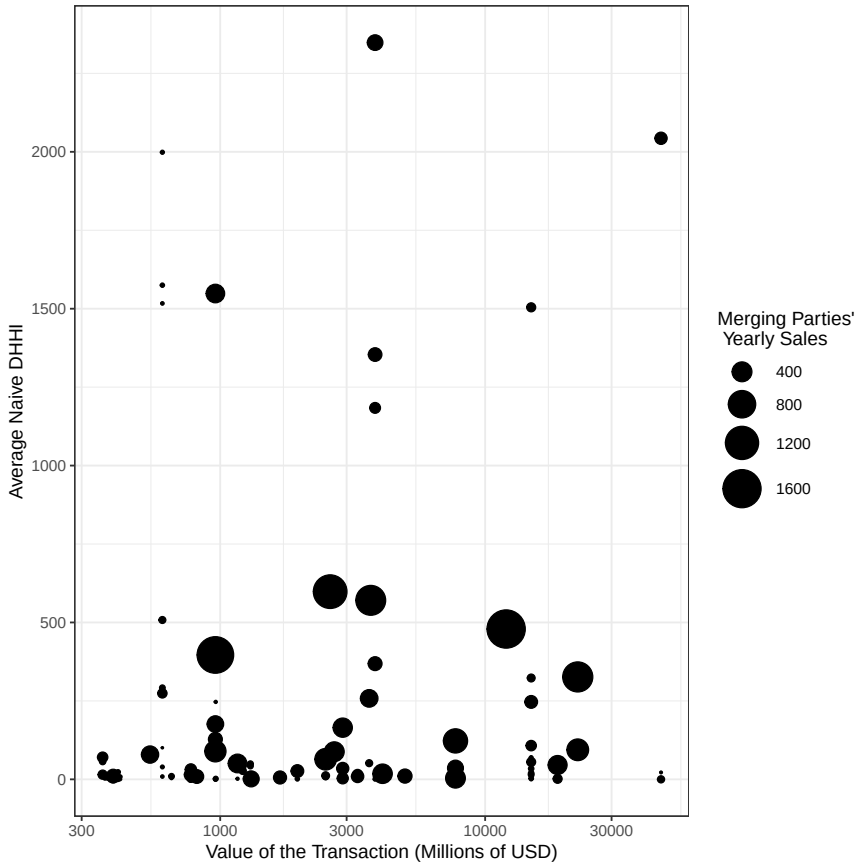


Figure 2: Scatter of the value of the transaction and naive ΔHHI, weighted by merging party sales

(2011) use prices of non-merging hospitals in their analysis of hospital mergers on Chicago's North Shore. The rationale is that these products are likely subject to the same cost and demand shocks as products of merging parties. However, an important drawback of this strategy is that non-merging firms are also competitors, and they may adjust their price due to strategic interaction. In this analysis, therefore, we avoid using the prices of non-merging firms entirely.

A second strategy is to use price changes of goods in other markets that are plausibly subject to similar cost and demand shocks. Ashenfelter et al. (2013) study the price effects of the Maytag-Whirlpool merger, which led to consolidation in five appliance markets, by using other appliances not affected by the merger as a control. Kim and Singal (1993) use airline prices in routes that were not impacted by the merger. The advantage of this empirical strategy is that it not subject to the same critique as above: we would likely not expect and price changes in products that are not substitutes for the markets under analysis, and thus any change in prices in such markets are likely

due to cost or demand changes. Of course, the difficulty is that since the industry is chosen to be sufficiently distinct, it may not be subject to the same cost and demand shocks as the industries in question. This makes it difficult to find control groups that fit the bill, and it is especially difficult at the scale at which this paper conducts the analysis.

The third strategy is to use price changes in geographic markets where there is no significant change in concentration because one or both parties are small. For instance, Dafny et al. (2012) study the price effects of insurance mergers by effectively using the price changes with low predicted changes in concentration as a control. The rationale for this control group is that in these markets may not have an incentive to adjust price in response to the merger itself. However, these firms may still be subject to national cost and demand shocks that could affect prices. This choice of control group does have two drawbacks. First, not all mergers have geographic markets that are "untreated" by the merger; a priori, this may have been especially problematic in our sample—which contains many large mergers where the merging parties are national staples—although we only lose about 10% of our mergers when defining untreated markets fairly narrowly. Second, this control group does require pricing to be local. If firms price at a national or regional level, as has been documented in some situations in recent work (Adams and Williams, 2019; DellaVigna and Gentzkow, 2019), then a merger could be price "spillovers" onto untreated markets: merging parties may adjust prices in treated markets, causing their competitors to also adjust prices in these markets, which could in turn cause change changes in untreated markets due to national pricing policies.

In this paper, we take two broad approaches to estimating the price effect of mergers. First, we compare prices before and after the merger, controlling for trends and factors that can shift price. To estimate the effect of the merger on outcomes of interest such as average prices and quantities, we regress

$$\begin{aligned}\log p_{idt} = \beta_0 + \beta_1 \cdot \mathbb{1}[\text{Merging Party}]_i + \beta_2 \cdot \mathbb{1}[\text{Post-Merger}]_t \cdot \mathbb{1}[\text{Merging Party}]_i \\ + \beta_3 \cdot \mathbb{1}[\text{Post-Merger}]_t \cdot \mathbb{1}[\text{Non-Merging Party}]_i + \xi_{id} + \xi_{m(t)} + \text{Controls}_{idt} + \epsilon_{idt}, \quad (1)\end{aligned}$$

where i is a UPC, d is a DMA, and t is a month. We discuss the fixed effects and controls below. The coefficients of interest are both β_2, which gives the average difference in prices before and after the merger for the merging parties' products, and β_3, which is the same quantity for non-merging parties. We run variations of (1) for other outcomes, and we explain these modifications in the relevant sections below.

In the baseline specification, we add fixed effects ξ_{id} at the UPC-DMA level to allow for persistent price differences in products across geographies. We also include month-of-year fixed effects $\xi_{m(t)}$ to control for seasonality. Finally, we include a DMA-specific linear time trend as a further control to account for secular price changes, such as increases in stocking costs in an area or a persistent decrease in the demand for products. With these controls, we effectively estimate the merger effect as any departure from the trend for pre-merger prices for the same product, in the same geography, at the same time of year: the pre-merger period serves as the control group, and (1) can be interpreted as an event study. As a robustness check, we add time-varying controls for demographics at the DMA level (log income per household) and time-varying cost shifters, such as prices of inputs. We do not include any cost shifters that could be directly impacted by the merger: for instance, we do not include proxies for transport costs to the nearest production facility. This check helps us evaluate whether the estimated effects are due to demand or cost structures changing at the time of the merger.

The baseline identification strategy, especially without further cost controls, is broadly based on the idea that any secular trends in demand or cost are gradual; moreover, price data at the monthly level lets us estimate these trends well. Is a linear time trend sufficient to capture any changes in the environment that are leads to changes in prices and quantities but not directly tied to the merger? In Section V.A, we show detrended prices for merging and non-merging parties as part of an analysis of the timing of price effects (Figure 5): here, we find no further pattern in prices before the merger, after partialling out a DMA-specific linear time trend. Even given this evidence, one may be concerned that the estimated effects are still due to some sort of trend. Two pieces of evidence help us address these concerns. First, we find in Section V.A that price changes start

very soon after a merger: we find it unlikely that the merger completion date would be timed to coincide exactly with when the secular trend in demand or costs would have also led to changes in price. Second, the likely version of this concern is that for mergers in which we document price increases, prices were increasing anyway before the merger—and vice versa for price decreases. However, we find no evidence of pretrends even when splitting the sample by mergers where prices increases significantly, decreased significantly, or did not change much (Figure 5 again). Figure A.4 in Appendix A scatters the mean time trend against the estimated price effect, and do not see any evidence of a positive relationship between these quantities.

Nevertheless, we investigate another identification strategy for robustness. Our second approach using price changes in geographic markets where the merging parties comprise a small share of total sales—markets that are "untreated"—as a control group. The regression associated with this specification is

$$\begin{aligned}\log p_{idt} = \beta_0 &+ \beta_1 \cdot \mathbb{1}[\text{Merging Party}]_i + \beta_2 \cdot \mathbb{1}[\text{Post-Merger}]_t \\ &+ \beta_3 \cdot \mathbb{1}[\text{Post-Merger}]_t \cdot \mathbb{1}[\text{Merging Party}]_i \cdot \mathbb{1}[\text{Treated}]_d \\ &+ \beta_4 \cdot \mathbb{1}[\text{Post-Merger}]_t \cdot \mathbb{1}[\text{Non-Merging Party}]_i \cdot \mathbb{1}[\text{Treated}]_d \\ &+ \xi_{id} + \xi_{m(t)} + \text{Controls}_{idt} + \epsilon_{idt}, \quad (2)\end{aligned}$$

where the "Untreated" dummy corresponds to a market with less than 2% market share of both merging parties in the baseline specification. Here, the coefficients of interest are β_3 and β_4. Our definition of untreated markets as having low total presence of merging parties requires these spillovers to happen through non-merging parties. A definition that uses low naive ΔHHI to define untreated markets would also allow for spillovers to happen through merging parties (if only one party is present).

We weigh all regressions by the pre-merger volume at the UPC-DMA level to downweight the price effect of small UPCs when estimating the average price effect of a merger. When aggregating results across mergers, we weigh estimates by their inverse variance. This practice is recommended

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	108	-0.15	3.43	-2.06	-0.74	1.21
		(0.46)		(0.46)	(0.48)	(1.10)
Merging Parties	108	0.48	4.54	-2.54	0.35	2.76
		(0.67)		(0.86)	(0.67)	(1.32)
Non-Merging Parties	108	-0.19	3.37	-2.09	-0.79	1.34
		(0.45)		(0.47)	(0.35)	(0.98)
B. Cost and Demographic Controls						
Overall	106	0.16	2.65	-1.46	-0.18	1.34
		(0.36)		(0.52)	(0.33)	(0.69)
Merging Parties	106	0.82	3.82	-0.65	0.58	2.62
		(0.43)		(1.05)	(0.48)	(1.10)
Non-Merging Parties	106	0.12	2.70	-2.16	-0.19	1.44
		(0.37)		(0.44)	(0.38)	(0.67)
C. Treated/Untreated						
Overall	101	-0.74	4.34	-2.39	-0.35	0.58
		(0.72)		(0.82)	(0.51)	(0.76)
Merging Parties	101	-0.22	5.48	-1.51	-0.18	1.04
		(0.64)		(1.34)	(0.51)	(1.09)
Non-Merging Parties	101	-1.37	3.34	-2.17	-1.62	0.21
		(0.52)		(0.69)	(0.57)	(0.66)

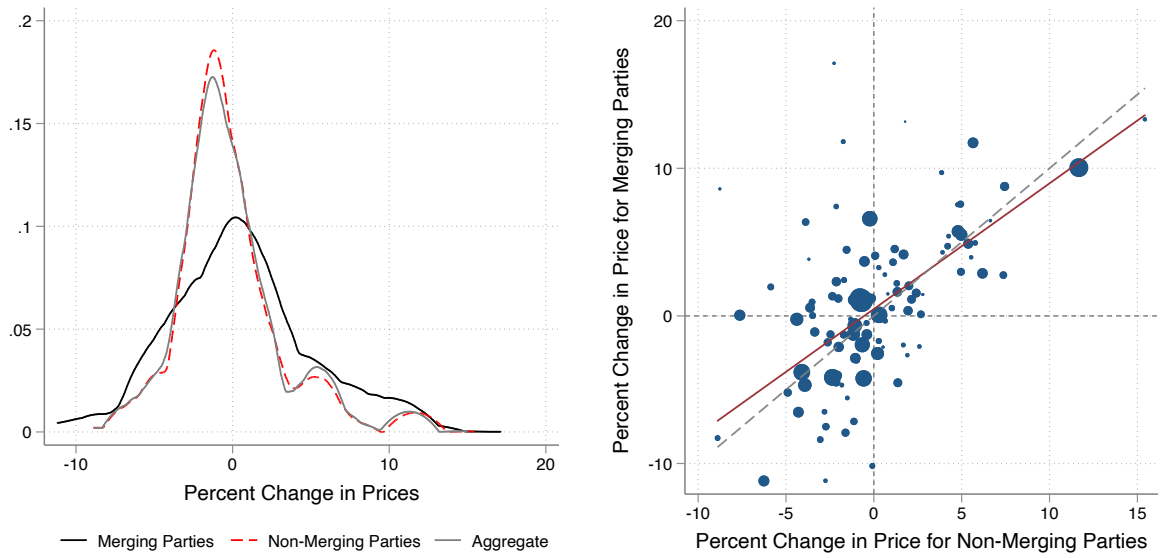
Table 2: Overall Price Effects. We aggregate across mergers using the inverse variance of the relevant parameter estimate.

by Vita and Osinski (2018) and corresponds to the "fixed effects" aggregation methodology proposed by DerSimonian and Laird (1986) for meta-analysis.

IV.B. Prices

Table 2 presents the distribution of price effects across mergers, both for all products as well as separately for products associated with merging and non-merging parties. The top panel displays results for the baseline specification, the middle adds time-varying cost shifters and demographic controls, while the bottom uses markets without merging party presence as a control.

The results from the baseline specification (Panel A) show that mergers on average have no effect on prices: we estimate a decline of about 0.15% for all products. We estimate that merging parties increase prices by 0.48% on average. Non-merging parties have a smaller price change,



(a) Distribution of price changes

(b) Scatter of price changes

Figure 3: Price changes for merging and non-merging parties, as estimated by (1). Distributions in Panel (a) are weighed by the inverse variance of each parameter estimate. The size of each point in Panel (b) corresponds to the inverse variance of the parameter estimate for the merging parties.

which we estimate to be a decrease of 0.19% on average.[7] Importantly, in all cases, the standard errors rule out the possibility of large price changes (in either direction) on average. However, these aggregate responses mask substantial heterogeneity across mergers. The standard deviation of price effects is 3.4%. That for non-merging parties in 3.4%, and merging parties have a slightly wider distribution of effects, with a standard deviation of 4.5%. The estimated interquartile ranges are comparable to the standard deviation. We find that about 25% of mergers have a price effect for merging parties that exceeds 2.8%.

These findings are evident in Figure 3(a), which plots the distributions of merger effects using the baseline specification. As shown in the summary statistics, we document substantial heterogeneity in price effects both across and within firm types (i.e., merging and non-merging). Panel (b) scatters the estimated effects for merging and non-merging parties against each other; the size of each point corresponds to the inverse variance of the estimate of the merging parties' price change for a

[7]Note that the estimates in each row are weighted by the inverse variance of the specific coefficient. Thus, each row in Table 2 may have a weighting scheme.

given merger. The solid, red line is the best fit of a regression of merging parties' price changes on non-merging parties' price changes, with weights again given by the inverse variance of the estimate of the merging parties' price coefficient. The dashed, gray line is the 45-degree line. As would be predicted by standard models of strategic interactions (i.e., those in which prices are strategic complements), the price changes of non-merging parties are typically of the same sign as those of merging parties—and of a similar but slightly smaller magnitude, as the best fit line is slightly flatter than the 45° line. To our knowledge, there are no general conditions comparing the price changes of merging to those of non-merging parties. A relevant benchmark is provided by Deneckere and Davidson (1985), who show that under fairly general demand systems but ex-ante symmetric firms, merging parties increase prices more than non-merging parties do. While we find this is true on average, it does not hold merger-by-merger in our sample.

Are these estimated price changes due to changes in costs or demand not induced by the merger? Panel B repeats the analysis with controlling for cost and demographic effects. Overall, we find that the point estimates for average price effects are slightly larger (by about 0.4 pp), and the estimated width of the distribution is slightly smaller; an inspection of the quantiles suggests that this is due mostly to a shorter left tail of the distribution of price changes. Nevertheless, the main observations still hold with these estimates: average price changes are small, and there is substantial heterogeneity in the estimated price effects. Moreover, Figure A.1 in Appendix A shows that not only are the estimated distributions of price effects similar but the estimated effects are correlated merger-by-merger.

Panel C uses "untreated" markets as a control, dropping seven mergers in which we have no such markets. At a broad level, the takeaways are similar to the other panels—with a small average treatment effect and a wide distribution overall. The estimated average effects are more negative, with the average overall effects and that for merging parties about 0.6–0.7 pp smaller than in the baseline. The estimates for the nonmerging party is a drop of 1.4%. Interestingly, we find that this is mostly due to a reduction in upper tail of the distribution: the first quartile of the distribution is similar to the one in the baseline, but the third quartile is estimated to the systematically smaller.

One could devise explanations for these results. If price increases are due to industry-wide cost increases or demand increases (reduction in elasticity) not captured in Panel B, then we may expect to see increases in prices in the untreated markets that would call into question whether the price increases are really due to the merger. In Section V.A, we show that price increases materialize quickly after a merger is completed, which seems at odds with (likely more gradual) price and cost changes. Alternatively, the control group itself could be partially contaminated by the merger effect: national or regional pricing policies could lead to price increases in untreated DMAs as well. However, we choose not to read too much into the differences in point estimates between Panels A and C and instead simply view it as a robustness check on the main observations. This is for two reasons. First, the standard error on the point estimates is large, and for both the overall effects and the merging parties' effect, the average from Panel A lies within one standard error of the mean from Panel C. Second, the reduction in the point estimates is sensitive to the choice of weighting scheme to aggregate results: Table A.3 in Appendix A shows results uses the random effects weighting scheme of DerSimonian and Laird (1986), a standard in meta-analysis in biostatistics, and using untreated markets as a control leads to larger point estimates. However, the main observations—that price effects are small on average, they are smaller for non-merging parties, and the distribution of price effects is wide—are robust to all specifications and all weighting schemes.

Taken together, the results in this section contrast with the findings of Kwoka (2014), which documents an average price change of 7.2% (Table 7.2). The difference between our findings and Kwoka's highlights the importance of collecting a comprehensive set of consummated mergers. Our analysis suggests that, on average, the typical approved merger of consumer goods firms leads to modest price increases. However, we document substantial heterogeneity in merger effects. Furthermore, as noted by Carlton (2009) and examined in more detail in Section VI of this paper, average price changes are not indicative of enforcement stringency, which should instead be judged by the expected price effect of the marginal merger.

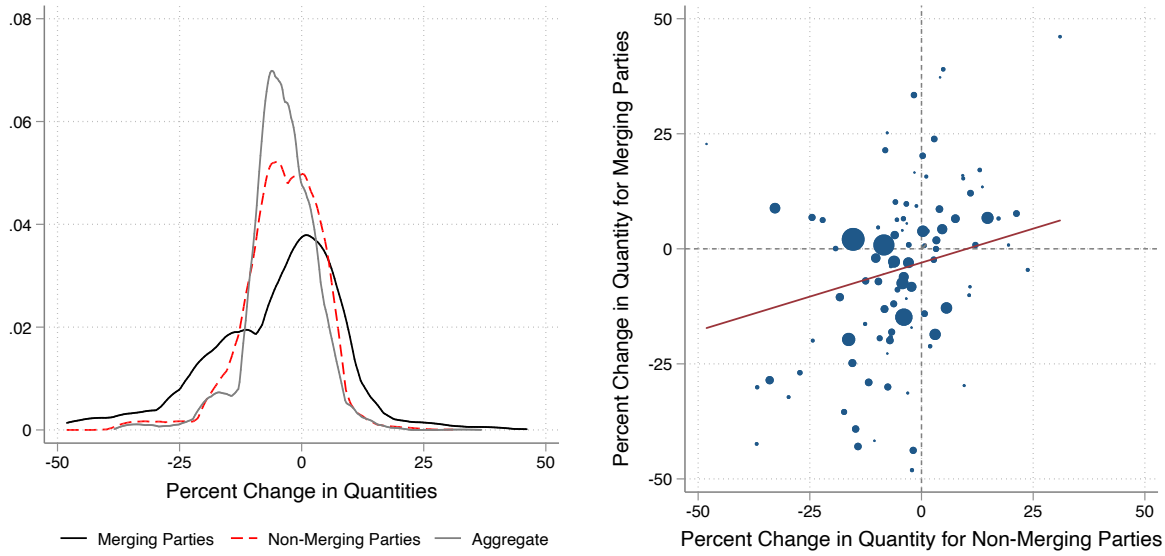
	N	Mean	S.D.	25th Pct.	Median	75th Pct.
Overall	108	-4.00	6.78	-7.91	-4.60	-0.02
		(0.82)		(1.15)	(1.31)	(1.83)
Merging Parties	108	-5.47	14.82	-14.85	0.06	2.06
		(1.93)		(4.12)	(3.86)	(1.91)
Non-Merging Parties	108	-3.75	7.81	-7.07	-3.97	3.06
		(1.19)		(1.67)	(2.01)	(1.59)

Table 3: Quantity Effects

IV.C. Quantities

While most merger retrospectives have focuses on prices, a natural question is whether mergers have reduced quantities on average. Conventional intuition would suggest that even if a mergers have small price effects on average, a significant drop in quantity may be indicative of mergers having adverse welfare effects. Lazarev et al. (2021) formalize this intuition: this argue that under certain conditions—including an assumption that the welfare effect of a merger has the same sign for all customers—then the sign of the effect on total quantity coincides with that of the welfare effect of the merger. While we do not claim that the results reported this section should be interpreted as welfare effects, theoretical results like in Lazarev et al. (2021) would indicate that documenting quantity effects is of direct interest.

To compute quantity effects, we aggregate to either the DMA-month-firm type level, where a firm type is whether that firm is a merging party or not. That is, we sum all quantities of all products sold by all non-merging parties in each DMA-month, and we sum all quantities of all products sold by merging parties in a DMA-month. We regress the log of this quantity on the same right-hand side as (1), with DMA-level time trends and DMA fixed effects. We also run a specification where we aggregate to the DMA-month level and simply include a post-merger dummy (again with DMA-specific time trends). This is for two reasons. First, running the regressions at the UPC-DMA level as with price would return an average of how product-level quantities have changed in percentage terms. Given the wide dispersion in quantities for different UPCs (unlike for prices), this average is difficult to interpret. Second, results like the one in Lazarev et al. (2021) rely on tests of total quantity.



(a) Distribution of quantity changes

(b) Scatter of quantity changes

Figure 4: Quantity changes for merging and non-merging parties, as estimated by (1). We restrict to changes that fall in the interval [-50,50]. Distributions in Panel (a) are weighed by the inverse variance of each parameter estimate. The size of each point in Panel (b) corresponds to the inverse variance of the parameter estimate for the merging parties.

Table 3 and Figure 4 show results. On average, we find sizable decreases in quantities: about 4% overall, 5.5% for merging parties (although this is driven by a long left tail), and 3.8% for non-merging parties. There is substantial heterogeneity in this estimate, but the magnitude of the drops in quantity at the first quartile is typically substantially larger than the magnitude of the increases in quantity at the third quartile. In fact, we estimate that the third quartile of overall quantity effects is close to 0. Figure 4(b) scatters the quantity effects for merging parties against those for non-merging parties, and we generally see a positive association between these estimates.

IV.D. Product Assortment and Availability [Preliminary]

While merger retrospectives have typically focused on price effects, horizontal mergers can have important effects on product assortment—whether new products are introduced or existing ones are removed. The impact of a merger on these incentives is ambiguous as well. The desire to avoid cannibalization may push merging parties to remove similar products. The cost structure of the new

firm also changes, which could lead to effects in either direction: it may make certain product lines unprofitable or make it economical to increase the product line. Finally, the incentive to "innovate" by designing new products changes as well.

Berry and Waldfogel (2001) discuss these competing effects and conclude that the effect of mergers on variety is necessarily an empirical question. While researchers have argued that ignoring product changes after a merger can bias welfare effects (Draganska et al., 2009; Fan, 2013), retrospectives remain rare, with some exceptions in radio (Berry and Waldfogel, 2001; Sweeting, 2010). To our knowledge, Atalay et al. (2020) provide the most comprehensive study on this question, studying product availability in a large set of mergers between retail products. They document a reduction in the number of UPCs offered by merging parties on average. Using a careful analysis of product descriptions and characteristics, they trace this back to firms dropping "fringe" products and focusing on their core competencies. Our first contribution is mostly a replication of their results: we conduct their analysis on a broader sample, albeit with specifications that focus on changes in the product mix rather than the count of products. Second, we also study the geographic distribution of products.

For each merger, we create an original list of products by taking all UPCs sold nationwide in the first six months of our sample (i.e., 18 to 24 months before the merger completion date) which survive our sample selection criteria. For each subsequent month, we compute the proportion of UPCs sold by a firm that are "new"—i.e., that are not included on this original list—computed as the ratio of the number of new products to the number of old products. These results are under "Introduction of Existing Products." We also compute the share of UPCs from this original list that have survived, computed as the number of old products still sold in the month divided by the number of products on the original list, and we list results under "Continuation of Existing Products." To understand regional product introductions and removals, we compute an analogous list at the DMA level as well. We mark a product as new in subsequent months if it is new to the DMA (and mark it as removed if it is removed from the DMA). We use a specification similar to the one in Section IV.C.

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. National Level						
Introduction of New Products						
Merging Parties	96	0.58	2.91	-0.63	-0.05	0.16
		(0.43)		(0.33)	(0.15)	(0.93)
Non-Merging Parties	106	0.25	0.75	-0.04	0.11	0.26
		(0.06)		(0.03)	(0.06)	(0.19)
Continuation of Existing Products						
Merging Parties	98	0.44	1.32	-0.04	0.11	0.47
		(0.19)		(0.03)	(0.06)	(0.51)
Non-Merging Parties	108	-0.04	0.70	-0.13	0.00	0.03
		(0.05)		(0.11)	(0.04)	(0.04)
B. DMA Level						
Introduction of New Products						
Merging Parties	96	0.50	4.12	-0.66	0.25	1.90
		(0.63)		(0.66)	(0.37)	(0.90)
Non-Merging Parties	106	0.33	1.31	-0.18	0.07	0.48
		(0.22)		(0.29)	(0.06)	(0.38)
Continuation of Existing Products						
Merging Parties	98	3.48	17.13	1.23	1.96	7.25
		(2.20)		(1.47)	(1.49)	(2.37)
Non-Merging Parties	108	-0.81	3.17	-0.86	0.04	0.80
		(0.58)		(1.13)	(0.33)	(0.19)

Table 4: Assortment Effects. Effects are in percentage points.

Table 4 shows results. For context, the mean value of introductions of new products across all mergers is 3.45 pp: about 3.5% of the UPCs carried in a particular DMA-month were not in the list at the start of the sample. The distribution of treatment effects for both merging parties and non-merging parties is roughly centered at zero. Thus, like with all other quantities we have studies in this section, we do not see any systematic effect of merging on product introductions or product removals. This is true both for the national-level regressions and the DMA-level ones: not only do we not see evidence that merger systematically lead to legitimately new products, but we do not find systematic evidence that they lead to existing products being sold in a wider range of geographies.[8]

An important observation, however, is that the interquartile range is somewhat large compared to the mean of the variable for merging parties. At the national level, the difference between the third and first quantiles is about one-third the sample mean. Future drafts of this paper will investigate

[8]This observation is a contrast to the ones in Atalay et al. (2020), who find that merger on average seem to reduce the number of products carried by merging parties. We are investigating whether sample selection decisions affect the difference between these two studies.

the sources of this heterogeneity. However, a somewhat robust result is that the distribution of effects on non-merging parties is rather tight for any specification: the standard deviation of effects for non-merging parties is at most half that for merging parties—and usually much less. This distribution is also centered around zero. Overall, we conclude that mergers have little effect on the product assortment decisions of non-merging parties.

V. A Closer Inspection of Price Changes

The results in Section IV show a wide range of price changes across mergers. What contributes to the heterogeneity in the price effects of mergers? Broadly, the Williamson (1968) trade-off is between increased market power generated by the merger (due to unilateral or coordinated effects) that could lead to price increases, and marginal cost synergies that are passed through to the customer, which lead to price decreases. Beyond the Williamson (1968) trade-off, mergers could also lead to changes in product assortments, changes in quality for the existing set of products, or changes in price discrimination strategies. In this section, we aim to understand the extent to which these channels impact observed price changes.

A full unpacking of these channels requires estimates of demand and cost functions before and after the merger, which is outside the scope of this analysis. Instead, we rely on providing suggestive evidence that price changes are correlated with measures of these channels. First, we study the timing of price changes, which prior research (Kim and Singal, 1993; Focarelli and Panetta, 2003; Whinston, 2007) has argued could be informative of whether price changes are due to firms exercising market power or realizing synergies. Second, we look for evidence of anticipatory price effects: price changes ocurring after the announcement but prior to completion of the merger. Third, to look for direct measures of synergies, we study whether measures of cost reductions—in our case, changes in distance from the nearest production facility—are related to price reductions. Finally, as a way of evaluating whether firms are exercising the market power generated by the merger, we study the relationship between price changes and measures of concentration.

Why should we be concerned about understanding what drives these price changes? First,

we believe that such an analysis is of direct interest. While the Horizontal Merger Guidelines have moved away from structural presumptions, they are still an important summary measure for the agencies: understanding whether realized price changes for completed mergers is correlated with HHI and ΔHHI can inform these presumptions. Moreover, credible possibilities of synergies are a common defense of a merger; however, the literature lacks systematic evidence on whether synergies have actually materialized in many completed mergers. Thus, these analyses can also inform enforcement agencies' decisions by identifying merger characteristics that suggest an increased ability to exercise market power or the potential for cost synergies. Second, while the majority of merger retrospectives have focused on price effects—partly since they are typically the first screen used by agencies—whether mergers have harmed consumer surplus is also a fundamental question of interest. A full analysis of consumer surplus would require a structural demand model that is outside the scope of this paper. However, ruling out the degradation of product quality or decreased product availability would suggest that small price changes are not coupled with significant changes in consumer welfare.

V.A. Timing

How quickly after a merger's completion are price changes realized? Conventional wisdom suggests that any strategic effects of the merger due to increased market power would materialize quickly, as it becomes legal to discuss pricing strategy between companies at the date of completion. Thus, if price increases are due primarily to market power, we would expect such increases to materialize soon after the merger. On the other hand, price changes due to adjustments in product quality or synergies would take longer to realize, as the firms integrate their operations and adjust their production and distribution facilities.[9] To evaluate whether changes in market power, product quality, or synergies contribute to the observed price effects, we document systematic evidence on

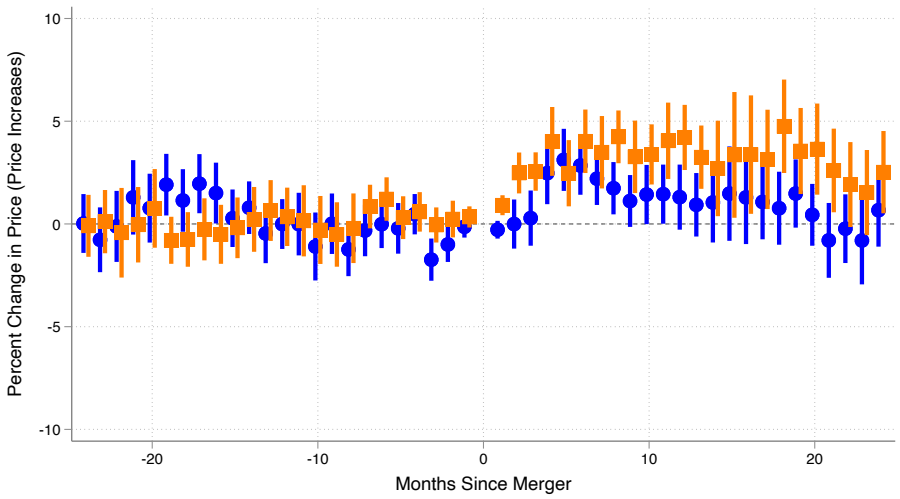
[9]Some limited empirical evidence supports this view: Focarelli and Panetta (2003) document that short-run price increases in banking mergers dissipate over the period of 3–6 years, attributing these to synergies materializing. However, researchers have also identified cases where synergies are realized especially quickly: Eliason et al. (2020) show that acquired dialysis firms adapt the parent chain's policies (many of which are harmful for patients) within one or two quarters of acquisition. An exception may be synergies that are due to increased buyer power after the merger, which may materialize more quickly.

the timing of price changes.

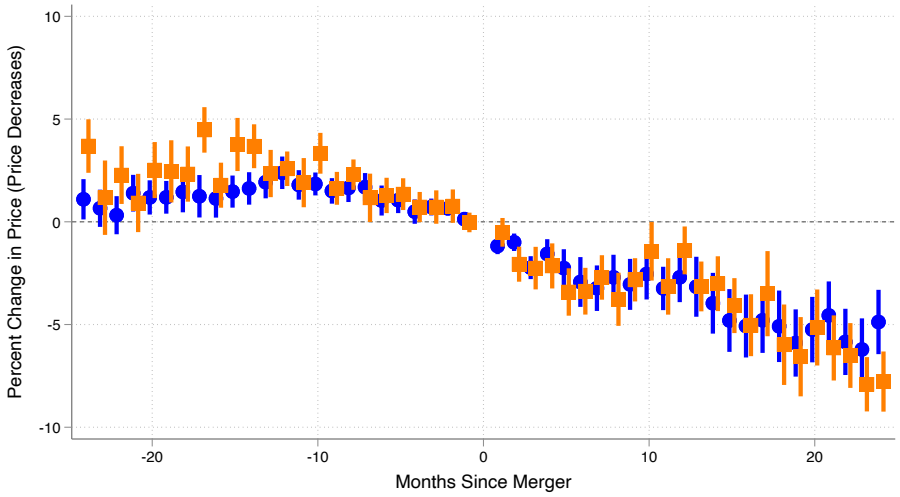
First, we estimate a version of (1) with finer bins of time than simply post-merger: we replace the post-merger $\times$ merging and post-merger $\times$ non-merging dummies with months-since-merger fixed effects, interacted with merging and non-merging dummies. We depart from (1) in one simple way. We still wish to control for a time trend to control for secular changes in costs and demand, and these fixed effects would be collinear with the time trend. Thus, we first detrend the data with a DMA-specific linear time trend estimated off the pre-period. We also normalize the fixed effects so that the dummies are equal to 0 for the month immediately preceding the merger. The specifications also control for UPC-DMA fixed effects (but drop seasonality fixed effects, as those are collinear with the months-since-merger effect for each merger). Figure 5 displays the mean price change for merging parties, displayed as orange squares, and non-merging parties, displayed as blue circles, across time. We display these plots separately for price changes in the top quartile of the distribution across mergers (high price changes), those in the bottom quartile (low price changes), and those in the second and third quartiles (stable prices).

Our first observation is that, after controlling for a linear time trend, prices tend to be generally flat before the merger is completed. We can interpret this as a response to the natural concern that price effects that we attribute to the merger are instead due to secular time trends. If mergers that we estimate to have large price increases exhibited a trend of price increases before the completion date as well, we would be skeptical of whether the estimated positive price effect is due to the merger itself. Panel (a) shows evidence that this is not the case: price increases happen after the merger, and there is no visible trend on average before the completion date. Panel (b) shows a similar result for mergers that exhibit low price changes (i.e., large price decreases), although there is arguably a small price decrease over approximately 8 months before the completion date itself on average. Panel (c) shows that mergers in the middle 50% of the price change distribution also tend to have a relatively flat price trend.

Of course, the main goal of Figure 5 is to illustrate the pattern of price changes after the merger. Focusing on Panel (a), we find that mergers with a substantially large price increase tend to realize



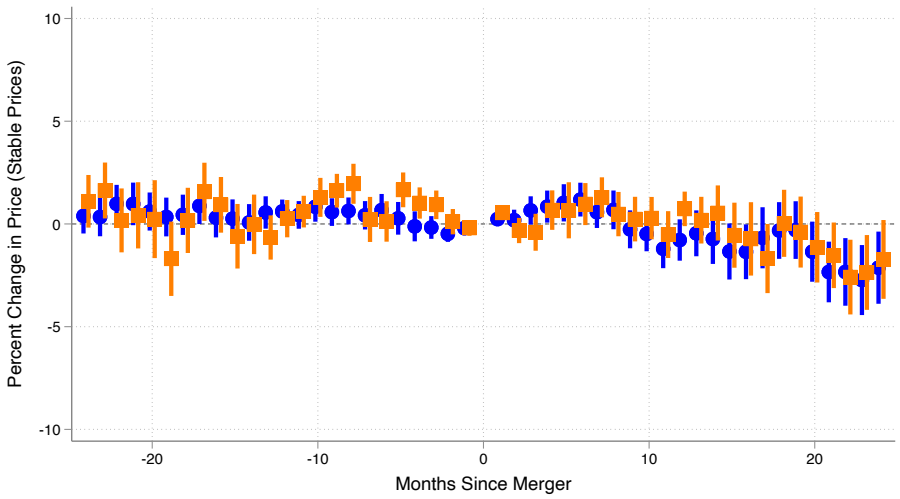
(a) High price changes



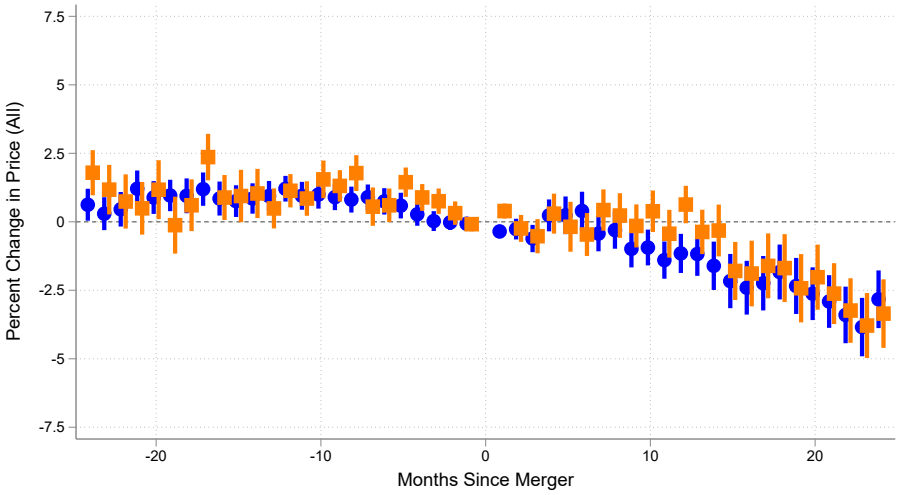
(b) Low price changes

Figure 5: Timing of price changes, for merging parties (orange square) and non-merging parties (blue circle). The marker indicates the mean price change, relative to a pre-merger DMA-specific, linear price trend, the given number of months after the merger becomes effective, and the thick line is the 90% confidence interval of that mean. Panel (a)–(c) shows subsamples: Panel (a) restricts to mergers with price changes in the top quartile, Panel (b) restricts to mergers with changes in the bottom quartile, while Panel (c) displays the remaining mergers. Panel (d) shows all mergers. (Continued on next page.)

that increase fairly quickly. On average, the price increase for merging parties stabilizes within 4–6 months from the date the merger is completed, suggesting that market power effects are realized fairly quickly. We find that non-merging parties also realize a price increase equal to that of merging parties for the first 4–6 months after the merger, but their price increases drop to about half of the



(c) Stable prices



(d) All price changes

Figure 5: (Continued) Timing of price changes, for merging parties (orange square) and non-merging parties (blue circle).

maximum level by about 9 months post-merger. We see some evidence that prices of both merging and non-merging parties drop further approximately 18 months after the merger, by about 1–2 pp.

The time pattern for mergers with large price decreases, documented in Panel (b), is qualitatively different. Merging parties in such mergers exhibit a decrease until about six months after the merger, after which the price stabilizes on average. We then see a further decrease by another 3–5 pp on average in the subsequent year. The price changes for non-merging parties largely mirror the prices

changes for merging parties in this case. Finally, for mergers where prices are relatively stable, we do not see much movement overall. However, there is some evidence that prices decrease by 1–2 pp starting about 18 months after the merger.

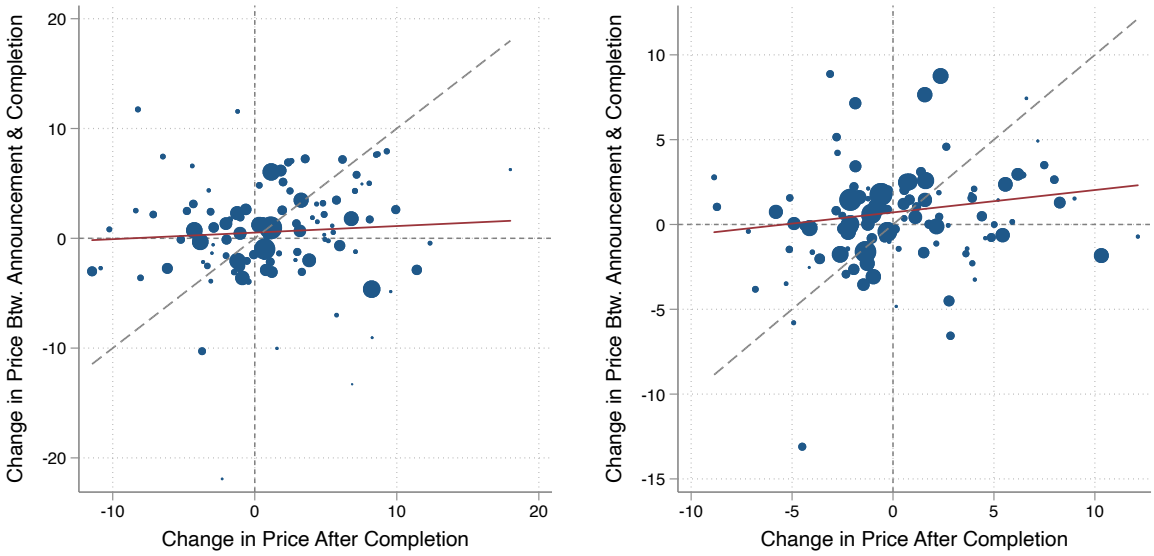
One takeaway from these results is that firms do not seem to wait to exercise new market power: price increases, when they happen, happen quickly after a merger. This observation also would call into question an alternate explanation for price increases—that they reflect quality improvements rather than exercising market power—as it is unlikely that quality can be improved so quickly after a merger. On the other hand, price decreases materialize over a longer time horizon, and we find evidence that all classes of mergers exhibit an average price decrease of around 2.5% about 18–24 months after the merger. This result is consistent with synergies taking a year or more to come into play.

Panel (d) of Figure 5 shows the timing plot for all mergers aggregated. The salient pattern in this figure is that overall, we see a drop in prices about 12–24 months after merger completion. In fact, the average price change two years after merger completion is around -2.5%. A natural explanation in our minds is the realization of synergies on this time frame. In Section V.C, we investigate more direct evidence for synergies.

V.B. Anticipatory Price Changes

A small set of empirical studies of airline mergers (Borenstein, 1990; Kim and Singal, 1993) and banking (Prager and Hannan, 1998) have documented evidence of anticipatory price increases, i.e., changes in behavior prior to the completion date of a proposed merger. Weinberg (2008) provides a review of these findings and notes that it is difficult to rationalize these finding with standard models of price competition, without appealing to managerial incentives.[10] Whinston (2007, p. 2427) offers a more direct explanation: that managing teams spend time together before a merger and can discuss strategy. Are such anticipatory price changes common among a broader set of mergers? To

[10] Weinberg (2008) provides one rationalization through the lens of managerial incentives and switching costs. If managers expect to get fired after a merger during restructuring, they may not have an incentive to increase market share by maintaining low prices. While an interesting rationalization for a difficult-to-explain phenomenon, it might not be broadly applicable, nor is it the only role managerial incentives may play in mergers.



(a) Merging parties

(b) Non-merging parties

Figure 6: Price changes for merging and non-merging parties between announcement and completion and after completion, as estimated by an adjusted version of (1). The size of each point corresponds to the inverse variance of the parameter estimate for the price change after completion.

answer this question, we estimate an adjustment to our main empirical specification in (1): we add indicators, separately for merging and non-merging parties, for the period between the merger's announcement and its completion.

Figure 6 displays a scatter plot of the change in price after completion versus the change in price between the merger's announcement and its completion. We estimate both changes relative to the pre-announcement period using an adjustment to (1). In particular, we add a dummy for the period between the announcement date and effective date and interact it with merging and non-merging party indicators. The size of each point corresponds to the inverse variance of the post-merger period estimate.

The plots presented in Figure 6 do not provide evidence of uniform, anticipatory behavior across mergers. The average price change realized between the merger's announcement and execution is positive and small in magnitude for a large range of post-completion price effects. The width of the estimated distribution of anticipatory price changes is also approximately half the size of that of price changes after the merger. Moreover, we do not see any systematic patterns: it is not the

case that mergers with large estimated price changes typically have large anticipatory price changes. Overall, we do not find any systematic evidence for anticipatory price changes—or for the stories of changing managerial incentives proposed by Weinberg (2008) or to the pre-merger strategy discussions referenced by Whinston (2007)—but some of the large effects estimated warrant further analysis.

V.C. Synergies

The decrease in prices more than a year after the merger, presented in Section V.A, suggests the presence of cost synergies. However, this analysis alone does not shed light on the form they take. Empirical evidence on the magnitude and form of synergies in horizontal mergers is limited and focuses on a small number of industries.[11] Identifying the sources of synergies has important implications for the agencies' review of potential mergers, as synergies are known to be difficult to evaluate ex ante (Whinston, 2007) but frequently factor into agencies' decisions. In this section, we analyze one natural source of synergies in our setting: merger-induced changes in the geography of the merged firm's production facilities. We explore whether products sold in markets that experienced a decline in the distance to the nearest production facility faced muted price increases or larger price declines than others.

Formally, we estimate an extension of (1) that includes an interaction of the indicator of the post-merger period for merging firms with an indicator of the merger-induced distance change falling below a given threshold. The regression takes the form

$$\log p_{idt} = \beta_0 + \beta_1 \cdot \mathbb{1}[\text{Merging Party}]_i + \beta_2 \cdot \mathbb{1}[\text{Post-Merger}]_t \cdot \mathbb{1}[\text{Merging Party}]_i + \beta_3 \cdot \mathbb{1}[\text{Post-Merger}]_t \cdot \mathbb{1}[\text{Non-Merging Party}]_i$$

[11] For a summary, see Whinston (2007), who notes that some studies have examined "price or efficiency effects in actual mergers (none look at both). This is clearly an area that could use more research." Berger and Humphrey (1992) and Peristiani (1997) study the banking industry and do not find strong evidence of efficiencies realized in the post-merger period. Pesendorfer (2003) finds an increase in firms' efficiency in the paper industry following a set of mergers in the 1980s. In recent years, such studies have remained sparse: Ashenfelter et al. (2015) provide some evidence of efficiencies in the Miller-Coors merger while Craig et al. (2021) find no such evidence in the context of hospital mergers.

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
Distance Change Definition						
$<$ 0 miles	74	0.07	10.67	-0.97	0.38	4.89
		(2.28)		(1.37)	(2.30)	(1.55)
$<$ -50 miles	65	-3.04	12.53	-3.57	-0.80	0.58
		(2.53)		(10.30)	(1.17)	(2.97)
$<$ -100 miles	65	-3.05	12.65	-5.82	-0.73	0.54
		(2.58)		(10.13)	(1.22)	(2.97)
$<$ -128 miles	63	-2.24	13.51	-1.34	-0.68	1.01
		(2.93)		(10.13)	(0.58)	(4.84)
$<$ -200 miles	61	-2.23	14.52	-0.99	-0.61	0.95
		(3.33)		(10.74)	(0.59)	(6.51)

Table 5: Incremental price effect in markets with declines in distance to the nearest production facility. For each distance-change classification, we display the distribution of β_4 in (3), weighted using the inverse variance of the estimate.

$$+ \beta_4 \cdot \mathbb{1}[\text{Post-Merger}]_t \cdot \mathbb{1}[\text{Merging Party}]_i \cdot \mathbb{1}[\Delta\text{Distance} < \bar{D}]_{id} + \xi_{id} + \xi_{m(t)} + \text{Controls}_{idt} + \epsilon_{idt}, \quad (3)$$

where $\bar{D}$ is the specified distance threshold (in miles), and $\bar{D} \in \{0, -50, -100, -128, -200\}$.[12] In this specification, β_4 is the coefficient of interest, which measures the additional price effect on products sold in markets that experienced sufficiently large declines in the distance to the nearest production facility.

Table 5 displays the distribution of the incremental price effect in markets that experience declines in distance to the nearest production facility, i.e., β_4 of (3). We compute the distributions separately for different definitions of distance changes and weight using the inverse variance of the estimate. We observe lower price changes, on average, in markets characterized by a decline in the distance to the nearest production facility of more than 50 miles. This result is suggestive of the realization of cost synergies through changes in the firms' distribution networks. It is important to note, however, that there is substantial heterogeneity across mergers in both the direction and magnitude of the effect. This variation could stem, for example, from differences in transportation costs across product types. Miller and Weinberg (2017) note the importance of transportation costs

[12]The first quartile of the distribution of distance changes across all mergers is -128 miles. We specify this as one value of $\bar{D}$ and examine robustness to a range of others.

in beer distribution, but freight costs vary substantially across products (Behrens et al., 2018).

The results in this section highlight one potential way in which synergies could manifest themselves in mergers of consumer packaged goods firms. Following the completion of a merger, firms have the option to reallocate production across their new set of facilities. We find evidence that the resulting reallocation is passed through to prices, on average. However, as explained above, this is not the case across all mergers. Together, these results point to future avenues of research to determine: (1) which subset of firms and products are most likely to benefit from distributional synergies, and (2) whether changes to the geography of production allow firms to better cater to region-specific tastes. Work is in progress to address these two points.

V.D. Correlation With Market Structure

Our final analysis studies the market-power channel more directly, as we examine the correlation between the observed price changes and standard measures of the level and change in market concentration (i.e., post-merger HHI and ΔHHI). Why focus on these metrics? When evaluating a merger, antitrust agencies typically weigh the potential to realize cost synergies like those described in Section V.C with changes in market structure. The latter piece is codified in the 2010 Horizontal Merger Guidelines, which detail presumptions by which the agencies identify mergers likely to lead to consumer harm.[13] Therefore, standard measures of market structure frequently enter into the agencies' decisions regarding whether to approve a merger or require an enforcement action.

The theoretical basis of these presumptions has been the focus of recent work in the academic literature. Theoretical results show a relationship between ΔHHI and the price and consumer surplus effects of mergers (Nocke and Schutz, 2018; Nocke and Whinston, 2021). On the other hand, Nocke and Whinston (2021) call into question the basis of basing merger screens on levels of HHI: they show that under both stylized theoretical demand systems and tractable empirical ones,

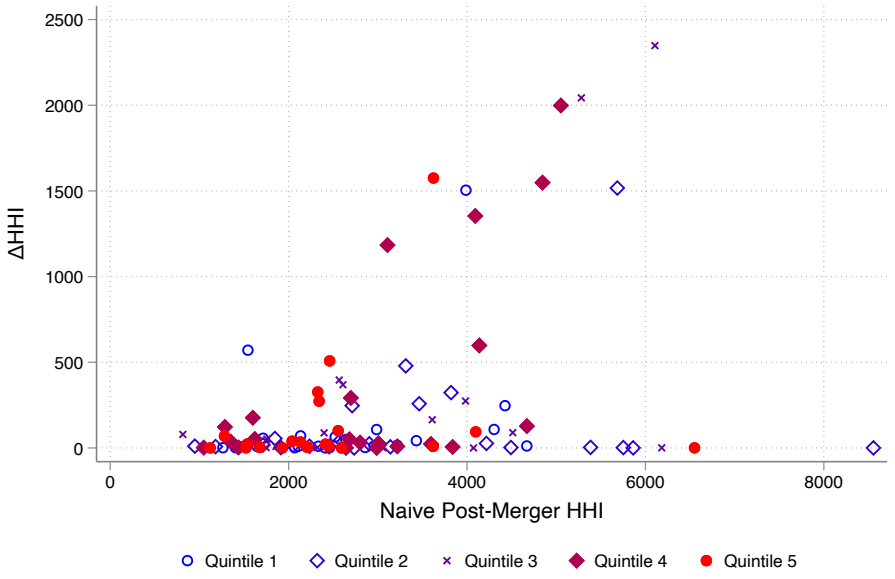
[13] Mergers that change HHI by more than 100 points and lead to a post-merger HHI between 1,500 and 2,500 or change HHI by between 100 and 200 points and lead to a post-merger HHI above 2,500 "raise significant competitive concerns and often warrant scrutiny." Mergers that change HHI by more than 200 points and lead to a post-merger HHI above 2,500 are "presumed to be likely to enhance market power." U.S. Department of Justice and the Federal Trade Commission (2010): "Horizontal Merger Guidelines," available at https://www.justice.gov/atr/horizontal-merger-guidelines-08192010#1.

the surplus effects of a merger are not related to HHI. Nevertheless, one may imagine alternate reasons that HHI would play a role in the effects of mergers: for instance, Loertscher and Marx (2021) note that competition authorities have historically used HHI as a measure of the potential for coordinated effects (but also call this into question). Ultimately, evaluating the screens is fundamentally an empirical question, and despite the recent attention paid to these presumptions, there is little evidence examining their efficacy.[14] In this section, we provide large-scale empirical evidence that the price effects of a merger correlate with the nationwide change in HHI but not with its post-merger level. These findings, however, do not hold across geographic markets within a merger.

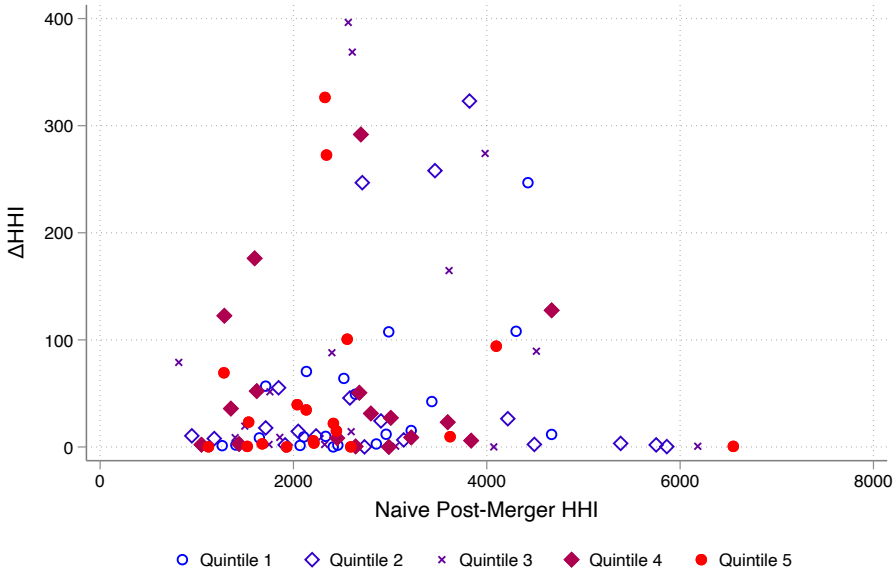
Figure 7 illustrates the relationship between nationwide concentration metrics and price changes. Specifically, it presents a scatter plot of merging and non-merging party price changes across HHI and ΔHHI. Recall that the structural presumptions rely on cutoffs in both HHI and ΔHHI: mergers raise competitive concerns if they are large on both dimensions. The different colors and shapes represent the quintile of the distribution of price changes for merging parties. Movement in the color from blue to red indicates higher average price changes for the given set of mergers. Panel (a) presents the plot for all mergers, while Panel (b) restricts to mergers with $\Delta\text{HHI} < 400$ and post-merger HHI < 7000 to remove the influence of outliers. We present the analogous plots for non-merging parties in Figure A.2, as well as heat-map versions in Figure A.3. Generally, price changes are larger for mergers with larger ΔHHI, especially for low HHI: the red dots are more concentrated above low values of ΔHHI. However, we do not see a similar pattern for HHI.

Figure 8 illustrates another cut of this relationship: the horizontal axis in each panel plots either HHI or ΔHHI, and the vertical axis plots the average price change of all mergers that have a value at least as large. Panel (a) shows the graphs as a function of ΔHHI, and we generally see price changes increase with ΔHHI. Panel (b) shows these price changes as a function of HHI, and we see that they are fairly flat. In aggregate, these results are consistent with the theoretical work of Nocke and

[14] Majerovitz and Yu (2021) find statistically insignificant differences in the merger effects on price and revenue across values of ΔHHI, but their sample contains mergers with small changes in HHI, on average (mean of 4.14). Our sample contains more variation in ΔHHI.



(a) Merging, Unrestricted



(b) Merging, Restricted

Figure 7: Scatter plots of nationwide ΔHHI against nationwide post-merger HHI for mergers in our sample. Panel (a) displays the results for all mergers, while Panel (b) restricts to mergers for which $\Delta\text{HHI} < 400$ and post-merger HHI $< 7,000$. Colors/shapes represent the quintile of the price change for merging parties, with solid (red) colors representing larger price changes. Note that $\Delta\text{HHI} < 0$ corresponds to divestitures being required in the merger.

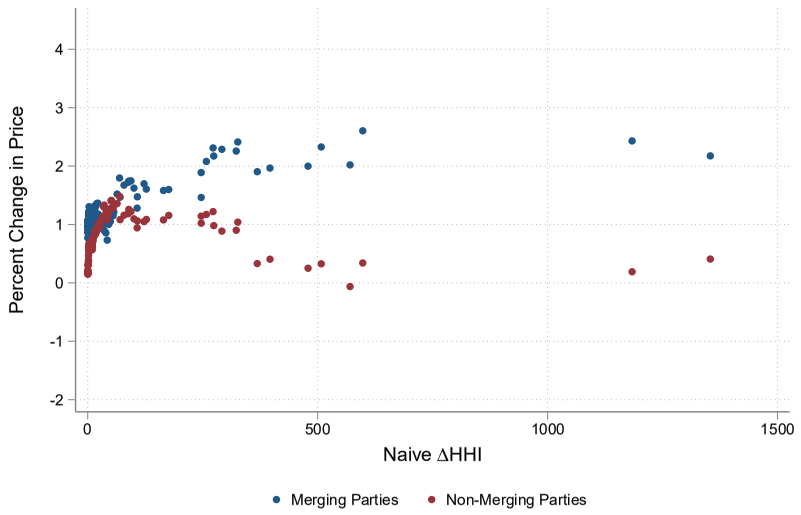
Whinston (2021) and suggest that ΔHHI (at the national level) correlates with the parties' ability to exploit market power afforded them by a merger.

Panel (c) cuts the relationship shown in Panel (a) by HHI (splitting on an HHI of 2,500). We see that the slope is larger for mergers with low HHI. One potential explanation for the difference in these relationships is a form of selection. Mergers with high post-merger HHI and ΔHHI likely face greater scrutiny from the antitrust agencies and therefore may require a higher burden of proof that such deals would produce cost synergies. On the other hand, we see no clear pattern for HHI, even when we split Panel (b) at ΔHHI equal to 200 (Panel (d)).

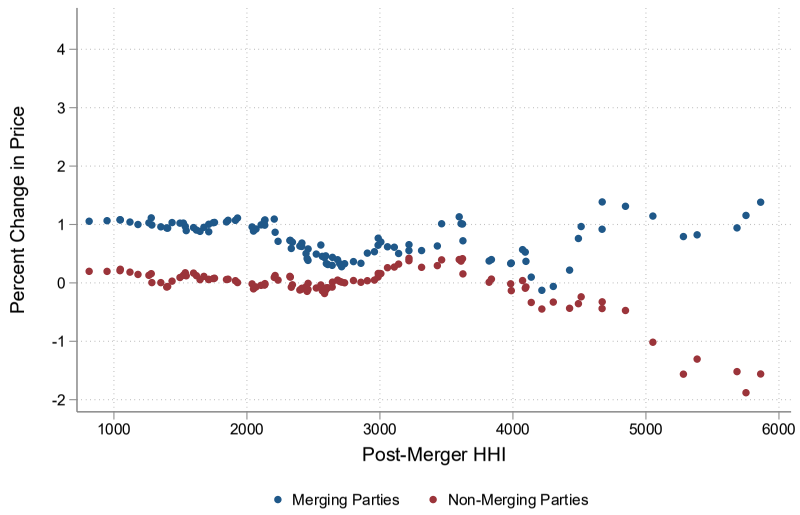
We have thus far focused on variation in price effects across mergers. How do price effects differ within-merger across geographic markets (i.e., DMAs), which also exhibit heterogeneity in HHI and ΔHHI due to the merger? We estimate an adjustment to (1), where we interact the post-merger dummy for merging and non-merging parties with indicators of whether a market falls in given ΔHHI or post-merger HHI category.[15] In Figure 9, we plot the mean price change, across mergers, in each category, weighed by the inverse variance of the parameter estimate. Orange squares correspond to merging parties, and blue circles correspond to non-merging parties. The solid lines indicate the 90% confidence interval of that mean.

While results are somewhat noisy, we see no clear evidence of a gradient in price effects by ΔHHI or by HHI of the market. We find some evidence of an inverted-U shape, at least for merging parties: we see no price effects for markets with especially low or especially high HHI or ΔHHI, but prices increase in markets with intermediate levels of these variables. One explanation for these effects may again be selection. Given that antitrust agencies have the ability to require divestitures only in a subset of geographies (although this power is exercised only once in our dataset), the fact that agencies allowed mergers to go through even though certain markets had large increases in concentration may be indicative of them anticipating large synergies. Second, the set of mergers that contribute to each estimate is different, as not all mergers have markets in each bin. This induces another source of selection: a low estimate of the price effect for high HHI could be explained

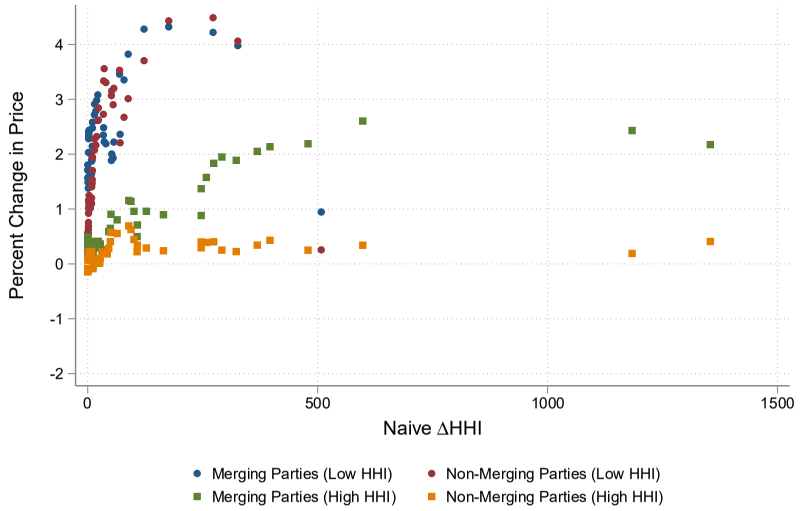
[15] We group ΔHHI in buckets of size 25 from 0 to 200. For post-merger HHI, we use the following cutoffs: (375, 750, 1,125, 1,500, 1,750, 2,000, 2,250, and 2,500).



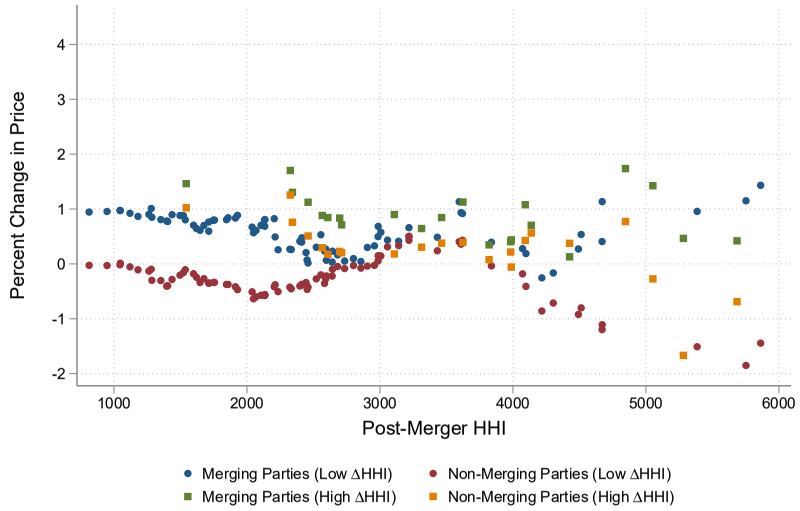
(a) Naive ΔHHI



(b) Post-merger HHI

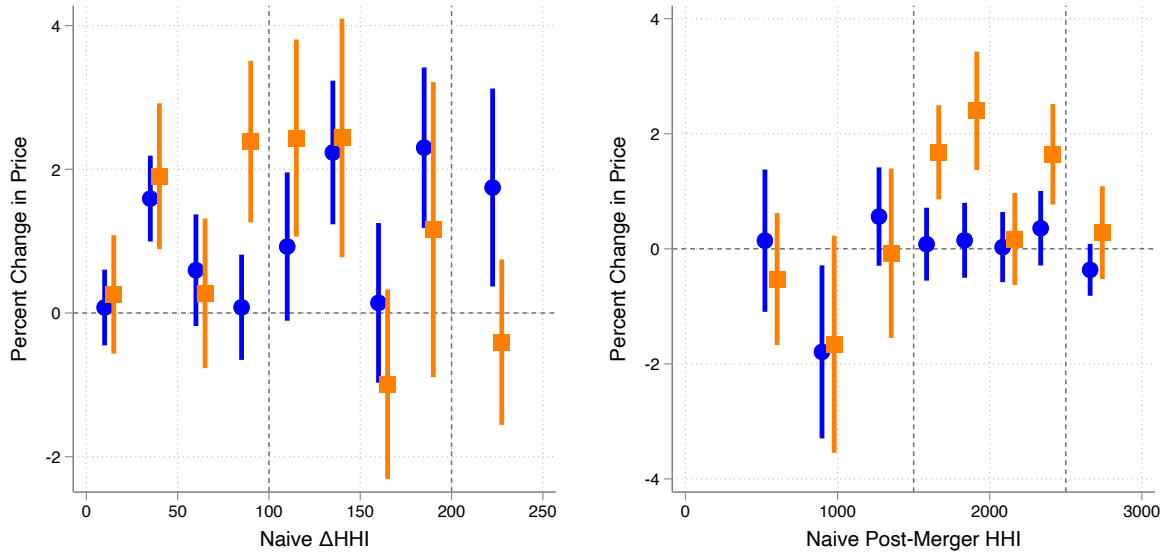


(c) Naive ΔHHI, by HHI $\gtrless$ 2500



(d) Post-merger HHI, by ΔHHI $\gtrless$ 200

Figure 8: Average price changes of a merger with at least a given level of ΔHHI ((a) and (c)) or post-merger HHI ((b) and (d)). Panels (c) and (d) split the sample by levels of HHI or ΔHHI.



(a) ΔHHI (b) Post-Merger HHI

Figure 9: Prices changes post-merger, by bins of naive ΔHHI and HHI, for merging parties (orange square) and non-merging parties (blue circle). The marker indicates the mean price change, weighed by the inverse variance of the parameter estimate, for geographic markets in that bin across all mergers, and the thick line is the 90% confidence interval of that mean.

by mergers with markets large HHI also having low overall ΔHHI.[16] In Section VI, we develop a model that accounts for selection into merger approval.

It is important to note that we do not assign a causal interpretation to these findings. The industrial organization literature has long cautioned against regressions of price or quantity on measures of market concentration (see, e.g., Demsetz (1973), Schmalensee (2012), and Berry et al. (2019)). Price, quantity, and HHI are all equilibrium outcomes, and it is difficult to define an ideal experiment that varies HHI. However, the agencies include HHI and ΔHHI in their presumptions of consumer harm, and it is plausible that they relate to the merged entity's ability to exercise market power. Therefore, it is still useful to provide large-scale evidence of whether these measures correlate with price and quantity changes.

[16] Additionally, while we do not find strong evidence of this in Section IV.B, some firms may still use coarse pricing rules. If they set prices regionally or nationally and thus do not respond to DMA-level changes in concentration due to the merger, the resulting spillovers across geographies could confound the results presented in Figure 9.

VI. How Stringent is US Antitrust Enforcement?

Analyzing the universe of large mergers avoids selection into publication: the effects we document are necessarily representative of mergers in US retail above the chosen deal size. This does not mean, however, that they are representative of the effects of all possible mergers—even between firms similar to the ones in our dataset. The effects that we have documented thus far are mediated through two important channels of selection. First, the merging parties have to propose the merger, meaning it must be profitable. Second, the enforcement agencies must approve it (or lose a challenge in court), meaning that there should be an expectation that it would not be too harmful to consumer surplus.

In this section, we delve into the second source of selection mentioned above: what types of mergers are the agencies willing to approve? As noted in Carlton (2009), noting that the mean price change is small—or even that the a large set of mergers have substantial price decreases—does not suggest that the enforcement agencies are strict, challenging mergers that have large price increases (or perhaps dissuading them from being proposed at all). Carlton (2009) notes that the stringency of an enforcement agency can be measured by the "marginal" merger: in a simple world in which agencies could unilaterally approve or reject a merger, and where merger effects could be perfectly predicted, this would be the largest price effect observed among consummated mergers. However, in a more realistic setup, the existence of consummated mergers with large price changes could also be indicative of uncertainty.

Conceptually, we can model the agencies as choosing to "challenge" mergers that they believe to be sufficiently anticompetitive—that they expect will lead to significant price increases. Generally, a challenge could be one of many actions, e.g., a motion to block the merger or a proposal for a remedy. In our setting, a "challenge" always involves proposing a remedy: the agencies did not (unsuccesfully) try to block any of the mergers in our dataset, but they did propose remedies in four of the deals in our dataset.[17] The agency has some information X_i about the merger i, which gives

[17] SDC Platinum also lists deals that were withdrawn, although they do not systematically list whether the reason was antitrust scrutiny. We are in the process of identifying whether the reason for withdrawal was antitrust scrutiny; we will include information for deals for which we believe this to be the case and treat them as deals the agencies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
True Price Change								
Constant	0.44	0.37	0.92	0.92	1.16	1.11	1.63	1.63
	[-0.31, 1.20]	[-0.37, 1.13]	[-0.49, 2.33]	[-0.49, 2.33]	[0.13, 2.21]	[0.09, 2.16]	[-0.31, 3.57]	[-0.30, 3.57]
HHI			-2.02				-2.14	
			[-6.48, 2.47]				[-8.29, 4.00]	
DHHI			3.25				4.09	
			[-3.66, 10.29]				[-5.24, 13.60]	
Mean HHI				-2.03				-2.16
				[-6.48, 2.46]				[-8.30, 3.98]
Mean DHHI				3.18				4.00
				[-3.73, 10.21]				[-5.35, 13.51]
σ_{p^*}	3.87	3.81	3.84	3.84	5.30	5.25	5.23	5.23
	[3.35, 4.48]	[3.32, 4.40]	[3.33, 4.45]	[3.33, 4.44]	[4.57, 6.15]	[4.53, 6.09]	[4.53, 6.06]	[4.52, 6.05]
Price Threshold								
Constant	12.75	20.02	10.48	10.06	12.16	12.76	11.18	10.81
	[7.76, 30.08]	[6.99, 66.10]	[7.30, 16.13]	[6.84, 15.54]	[8.80, 20.24]	[7.54, 32.05]	[8.15, 16.16]	[7.79, 15.68]
Log(Sales)	-1.63		-1.41	-1.36	-1.60		-1.49	-1.44
	[-4.76, 0.21]		[-3.29, 0.00]	[-3.28, 0.11]	[-3.75, -0.11]		[-3.21, -0.21]	[-3.19, -0.12]
Log(Merg Sales)		-5.53				-3.17		
		[-22.11, -0.69]				[-9.95, -0.82]		
HHI			1.69				2.39	
			[-5.69, 9.87]				[-4.33, 9.19]	
DHHI			-22.47				-19.10	
			[-40.51, -12.20]				[-35.69, -9.86]	
Mean HHI				2.75				3.37
				[-4.48, 11.24]				[-3.09, 10.37]
Mean DHHI				-22.59				-19.20
				[-40.73, -12.32]				[-35.91, -9.81]
σ_ϵ	6.01	13.22	4.01	4.03	4.28	6.60	3.42	3.43
	[2.50, 16.83]	[3.05, 46.70]	[2.18, 7.24]	[2.20, 7.26]	[1.94, 10.03]	[2.16, 21.34]	[1.77, 6.37]	[1.76, 6.41]
Eff. Threshold	4.56	3.69	5.61	5.63	8.20	8.18	8.75	8.83
	[1.74, 7.31]	[0.73, 7.72]	[3.25, 8.10]	[3.27, 8.14]	[4.85, 10.77]	[3.24, 12.16]	[6.16, 11.11]	[6.21, 11.21]
Price	Overall	Overall	Overall	Overall	Merging	Merging	Merging	Merging

Table 6: Posterior means and 95% credible intervals of the model of selection into enforcement. Total pre-merger sales ("Sales") and pre-merger sales of the merging party ("Merg Sales") are in billions of dollars. HHI and (naive) DHHI are calculated at the national level, and Mean HHI and Mean DHHI are averaged over markets, in the pre-merger period. Columns (1)–(4) use average price changes as measured in a regression of a post-merger dummy on UPC-DMA fixed effects, month fixed effects, and a DMA-specific time trend. Columns (5)–(8) use the price change of merging parties, as measured in the baseline.

it a prior for the true price change p_i^* of the merger. They also get a noisy signal of the price change of the merger based on further due diligence conducted and then decide whether to challenge the merger.

In a general model, the agency effectively has a probability $\lambda(p_i^*, X_i)$ of challenging a merger where the true price change is p_i^* and observable characteristics are X_i. The randomness in this decision (when viewed from the perspective of the econometrician) could come from two sources: (i) noise in due diligence or (ii) characteristics that are unobserved to the econometrician but used in the agencies' decision. In this section, we estimate an empirical version of the model by imposing the first interpretation. Appendix B discusses the connection to the second interpretation.

We provide a simple empirical implementation of this model of antitrust enforcement. The true price impact of merger i, with observed characteristics X_i, is p_i^*. Agencies have a prior that this price impact is drawn from $N(\mu_{p^*}(X_i), \sigma^2_{p^*})$, based on the price impacts of mergers that were similar on observables. From further investigation of the proposed merger at hand, they also observe a noisy signal $p_i = p_i^* + \epsilon_i$, with $\epsilon_i \sim N(0, \sigma^2_\epsilon)$. Agencies will ask for remedies if their signal of the price impact is larger than some threshold, i.e., if $p_i \geq \bar{p}(X_i)$.

In our setting, we observe two important pieces of information for each merger: (i) an estimate of the price change due to the merger (overall, and of merging parties in particular), and (ii) whether the agency proposed a remedy for it. We also observe some characteristics X_i about each merger i, such as the market size and market structure. We assume that our estimate of the price change is $\hat{p}_i \sim N(p_i^*, \sigma_i^2)$, where σ_i is the standard error of our estimate of the price effect. We do so only for mergers that were not challenged: otherwise we only observe an estimate of the price effect after the remedy, and we wish to avoid taking a stance on how this price change would compare with the one without a remedy.

To gain intuition for how the data informs the parameters of this model, suppose that true price changes are observed for consummated mergers. Suppose in addition that there is a merger-specific property Z_i that affects the agencies' threshold $\bar{p}(\cdot)$ but not the prior distribution of expected price

"blocked."

changes. Consider the case where $\sigma_\epsilon = 0$: the agency can also perfectly predict the price change. When Z_i is such that the agency is especially permissive, we observe the distribution of price changes unfiltered through the agencies' actions: this is a good estimate of the prior distribution itself and thus of $(\mu_{p^*}, \sigma_{p^*})$. As Z_i changes and the agency grows less permissive, the maximum observed price change among all consummated mergers informs $\bar{p}$. In the more realistic case with noise in the agencies' assessments ($\sigma_\epsilon > 0$), one can compare the observed price change distribution for a stringent Z_i with one for a permissive Z_i: if the distributions are similar, then there is still a fair amount of residual noise in the agencies' assessment.

We parameterize $\mu_{p^*}(\cdot)$ and $\bar{p}(\cdot)$ as linear combinations of merger characteristics. In the most general specification, we let both depend on HHI and ΔHHI, either at the merger level or an average over the values for geographic markets. We assume that the characteristic Z_i that enters the price threshold but is excluded from the price change distribution is total sales in the market. The rationale behind this choice is that antitrust agencies may be more likely to scrutinize mergers in larger markets even conditional on concentration. We estimate the model using a computational Bayesian approach of Gibbs sampling with data augmentation. Table 6 shows results.

The first set of columns use the average price change of all parties (merging and non-merging) as the price change of interest, estimated using a specification with UPC-DMA fixed effects, month fixed effects, and DMA-specific time trends. Column (1) shows a specification where μ_{p^*} is parameterized with a constant, and the price threshold $\bar{p}$ depends on the total pre-merger sales in the market. Consistent with our intuition, we find a negative point estimate: a 10% increase in market size reduces the threshold by 0.16 pp. Column (2) uses the total pre-merger sales by merging parties in the market, and we again find a negative relation, although the magnitude is about three times as large. To interpret the magnitude of the threshold, we compute the expected price change for a merger whose signal p_i equals the threshold $\bar{p}(X_i)$; this is the posterior mean $(\bar{p}(X_i) \cdot \sigma_\epsilon^{-2} + \mu_{p^*}(X_i) \cdot \sigma_{p^*}^{-2})/(\sigma_\epsilon^{-2} + \sigma_{p^*}^{-2})$. The effective threshold, reported in the final row of Table 6 is the average of this quantity over all mergers in the sample. In Columns (1) and (2), we find thresholds of 4.6% and 3.7%, respectively; overall, enforcement agencies propose remedies for

any merger whose expected price increase exceeds this threshold. The credible intervals for these estimates are somewhat large, but they do not contain 0.

Columns (3) and (4) add measures of pre-merger market structure to both the mean of the true price distribution and the threshold—either the nationwide pre-merger HHI and (naive) ΔHHI or the average of these measures at the market-level. We do not document any significant relationship between the measures of market structure and the prior of the price distribution, but we consistently find a significant negative relationship between ΔHHI and the price threshold. A ΔHHI that is 100 points larger corresponds to a reduction in the threshold of about 0.2 pp—consistent with the observation that the agencies are more likely to scrutinize mergers with larger changes in concentration. In these specifications, we estimate a slightly larger effective threshold of about 5.6%.

Finally, Columns (5)–(8) repeat the specifications in Columns (1)–(4) but use the price change of merging parties as the effect of interest, estimated from the baseline. Results are qualitatively similar, but we estimate a larger effective threshold of 8.2–8.8% depending on the specification.

An interpretation of this result is that the "marginal" merger would have a price effect in the range of 3.7–4.6% overall, or with a price increase of 8.2–8.8% for merging parties. Kwoka (2014, p. 86) argues that one interpretation of the selection bias in published studies is that these studies are more likely to be of such marginal mergers: these are the deals that garnered press attention partly because of agency scrutiny. It is thus noteworthy that he arrives at a quantitatively similar conclusion, with mean price changes of mergers around 7.2% (Table 7.2 in Kwoka (2014)).[18]

[18] Another interpretation places some caveats on the threshold interpretation. A general model is that the probability of challenging a merger is a function of the true price change. Appendix B shows that the probability function is identified from data, and this threshold is simply one way to interpret this function. However, the decision to approve or reject need not be solely due to a noisy estimate of the price change: the agencies may well have more information about the merger, on which it bases this decision. We imagine this is realistic, not just for institutional reasons (such as the fact that agencies conduct diligence on proposed mergers and receive more information from parties in the HSR filings) but also because we estimate σ_ϵ to be roughly the same magnitude as σ_{p^*} in most specifications. If other factors are part of this ϵ, we can roughly interpret the threshold reported in this section as an average of thresholds conditional on merger-specific information unobserved to us.

VII. Conclusion

In this paper, we conduct a large-scale retrospective of all sufficiently large mergers in US retail. Over a number of specifications, we find a relatively modest average effect of 0.5% on price and a decrease in quantity of around 3%. These means mask substantial heterogeneity in outcomes, with an interquartile range of almost 5% for price changes. These average effects are nevertheless considerably smaller than those reported in previous meta-analyses of merger retrospectives; the difference is that our sample consists of inframarginal mergers—all mergers that were proposed and allowed by the enforcement agencies—rather than consistent of a selected sample of mergers that may have garnered particular interest from the agencies, the press, or researchers. We show that this mean is by itself not informative of the agencies' stringency. We use a simple selection model of the agencies' enforcement decision to estimate that agencies tend to suggest remedies for mergers they expect will increase average prices by 3.7–5.6%.

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A. Additional Tables and Figures

In this appendix, we first provide additional tables containing information about the mergers we study. We document SIC codes of deals that were scrutinized and deals with overlap in Table A.1 and outline product market definitions in Table A.2.

We next provide some robustness analysis for the results presented in the body of the paper. Figure A.1 compares the estimated price and effects at the merger level across specifications. We show that the estimates are correlated at the merger level: not only is the distribution of price effects similar in the baseline to in the specifications with controls, but the estimates are similar at the merger level. This provides evidence that the subsequent analysis will not depend strongly on the fact that we use the baseline estimates. Table A.3 shows another dimension of robustness: we aggregate across mergers using the random effects weighting scheme proposed in DerSimonian and Laird (1986) instead of inverse variance weighting. Results are similar especially when taking into account standard errors, and the main difference is that the estimates for the specification comparing to untreated markets are more positive.

Figure A.2 replicates Figure 7 but using price changes for nonmerging parties. Here, the pattern is somewhat clearer that price changes are larger when ΔHHI is larger, while the correlation with HHI is not as large. Figure A.3 bins the scatters in Figures A.3 and A.2 to show average price effects by bin of HHI-ΔHHI.

Finally, Figure A.4 scatters the mean pre-merger time trend against the estimated price effects. We find no significant correlation, suggesting that price increases are not due to price trends.

SIC Code	Target Industry Sector	Deals Scrutinized	Deals with Overlap
212	Agriculture, Forestry, and Fishing	1	0
2011	Food and Kindred Products	1	1
2013	Food and Kindred Products	2	1
2015	Food and Kindred Products	2	1
2023	Food and Kindred Products	1	0
2026	Food and Kindred Products	1	0
2032	Food and Kindred Products	1	0
2033	Food and Kindred Products	3	2
2038	Food and Kindred Products	3	1
2041	Food and Kindred Products	1	0
2043	Food and Kindred Products	2	1
2047	Food and Kindred Products	1	1
2051	Food and Kindred Products	6	4
2052	Food and Kindred Products	1	0
2064	Food and Kindred Products	1	1
2068	Food and Kindred Products	1	1
2082	Food and Kindred Products	3	2
2084	Food and Kindred Products	4	1
2085	Food and Kindred Products	3	1
2086	Food and Kindred Products	3	1
2095	Food and Kindred Products	3	1
2096	Food and Kindred Products	3	2
2099	Food and Kindred Products	4	4
2111	Tobacco Products	1	0
2121	Tobacco Products	2	0
2621	Paper and Allied Products	1	1
2834	Drugs	4	0
2841	Soaps, Cosmetics, and Personal-Care Products	1	1
2844	Soaps, Cosmetics, and Personal-Care Products	9	6
2891	Chemicals and Allied Products	1	0
3841	Measuring, Medical, Photo Equipment; Clocks	1	0
5122	Wholesale Trade-Nondurable Goods	1	0
5143	Wholesale Trade-Nondurable Goods	1	1
5182	Wholesale Trade-Nondurable Goods	1	0
5431	Retail Trade-Food Stores	1	0
5461	Retail Trade-Food Stores	2	2
8049	Health Services	1	1

Table A.1: SIC Codes and Industry Sectors for Scrutinized Deals and for Deals in the Final Sample

Market	Nielsen Product Group	Nielsen Product Modules in Product Market
1	Baked Goods-Frozen	Bakery-Bagels-Frozen
2	Baked Goods-Frozen	Dough Products-Bread-Frozen, Bakery-Bagels-Frozen, Bakery - Doughnuts - Frozen, Bakery-Cheesecake-Frozen, Bakery - Biscuits/Rolls/Muffins - Frozen, Bakery-Breakfast Cakes & Sweet Rolls-Frozen, Bakery - Cobbler/Dumplings/Strudel - Frozen, Bakery-Bread-Frozen, Bakery-Cookies Rte/Cookie Dough-Frozen, Bakery - Dessert Cakes - Frozen, Bakery - Pies - Frozen, Bakery - Remaining - Frozen
3	Beer	Beer, Near Beer/Malt Beverage, Stout And Porter, Light Beer (Low Calorie/Alcohol), Ale, Malt Liquor
4	Beer	Beer, Stout And Porter, Light Beer (Low Calorie/Alcohol), Ale, Malt Liquor
5	Bread And Baked Goods	Bakery-Bread-Fresh
6	Bread And Baked Goods	Bakery-Bagels-Fresh
7	Bread And Baked Goods	Bakery-Breakfast Cakes/Sweet Rolls-Fresh
8	Bread And Baked Goods	Bakery-Buns-Fresh
9	Bread And Baked Goods	Bakery-Cheesecake-Fresh
10	Bread And Baked Goods	Bakery-Doughnuts-Fresh
11	Bread And Baked Goods	Bakery-Muffins-Fresh
12	Bread And Baked Goods	Bakery-Pies-Fresh
13	Bread And Baked Goods	Bakery-Rolls-Fresh
14	Candy	Candy-Chocolate-Miniatures, Candy-Chocolate, Candy-Chocolate-Special
15	Candy	Candy-Hard Rolled, Candy-Chocolate-Miniatures, Candy-Chocolate, Candy-Non-Chocolate-Miniatures, Candy-Non-Chocolate, Candy-Lollipops, Candy-Kits, Candy-Dietetic - Non-Chocolate, Candy-Dietetic - Chocolate, Gift Package With Candy Or Gum
16	Cereal	Cereal - Granola & Natural Types
17	Cereal	Cereal - Ready To Eat
18	Coffee	Coffee - Soluble Flavored, Coffee - Soluble
19	Coffee	Ground And Whole Bean Coffee, Coffee - Liquid
20	Condiments, Gravies, And Sauces	Barbecue Sauces
21	Condiments, Gravies, And Sauces	Cooking Sauce
22	Condiments, Gravies, And Sauces	Meat Sauce, Worcestershire Sauce
23	Condiments, Gravies, And Sauces	Sauce & Seasoning Mix-Remaining
24	Condiments, Gravies, And Sauces	Sauce & Seasoning Mix-Remaining Mexican
25	Condiments, Gravies, And Sauces	Sauce Mix - Spaghetti
26	Condiments, Gravies, And Sauces	Sauce Mix - Taco
27	Condiments, Gravies, And Sauces	Seasoning Mix - Chili
28	Condiments, Gravies, And Sauces	Seasoning Mix - Sloppy Joe
29	Cookies	Cookies
30	Cosmetics	Cosmetic Kits
31	Cosmetics	Cosmetics - Concealers
32	Cosmetics	Cosmetics-Blushers
33	Cosmetics	Cosmetics-Eye Shadows
34	Cosmetics	Cosmetics-Eyebrow & Eye Liner
35	Cosmetics	Cosmetics-Face Powder
36	Cosmetics	Cosmetics-Foundation-Liquid, Cosmetics-Foundation-Cream And Powder
37	Cosmetics	Cosmetics-Lipsticks
38	Cosmetics	Cosmetics-Mascara
39	Cosmetics	Cosmetics-Remaining
40	Crackers	Crackers - Flaked Soda, Crackers - Graham, Crackers - Sprayed Butter, Crackers - Cheese, Crackers - Remaining, Crackers - Flavored Snack, Snacks - Pork Rinds, Snacks - Puffed Cheese, Snacks - Potato Chips, Snacks - Potato Sticks, Snacks - Pretzel

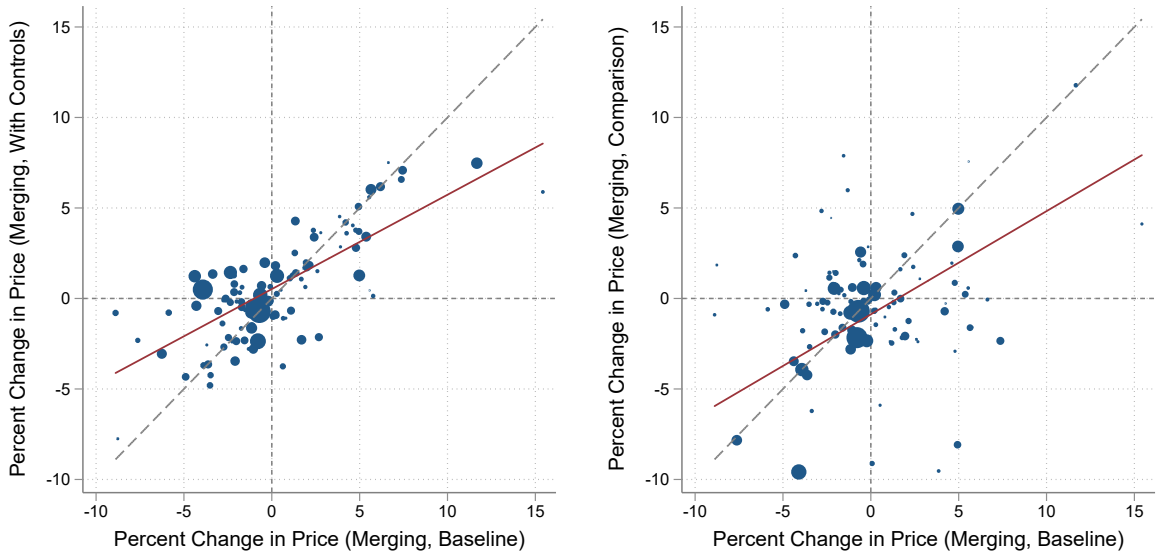
Market	Nielsen Product Group	Nielsen Product Modules in Product Market
41	Detergents	Detergents-Packaged, Detergents - Light Duty, Detergents - Heavy Duty - Liquid
42	Detergents	Packaged Soap, Laundry Treatment Aids, Detergent Boosters, Fabric Washes - Special
43	Fragrances - Women	Cologne & Perfume-Women's
44	Grooming Aids	Cosmetic And Nail Grooming Accessory
45	Grooming Aids	Cosmetics - Noncotton Aplctrs/Puffs/Etc.
46	Gum	Gum-Bubble, Gum-Chewing, Gum-Chewing-Sugarfree, Gum-Bubble-Sugarfree, Breath Sweeteners
47	Hair Care	Creme Rinses & Conditioners
48	Hair Care	Hair Preparations - Other Than Men's
49	Hair Care	Hair Spray - Women's
50	Hair Care	Shampoo-Aerosol/ Liquid/ Lotion/ Powder, Shampoo-Combinations
51	Hair Care	Wave Setting Products
52	Jams, Jellies, Spreads	Garlic Spreads
53	Juice, Drinks - Canned, Bottled	Fruit Drinks-Canned, Fruit Drinks-Other Container, Water-Bottled
54	Liquor	Bourbon-Straight/Bonded, Bourbon-Blended, Canadian Whiskey, Irish Whiskey, Remaining Whiskey, Scotch, Gin, Vodka, Rum, Tequila, Brandy/Cognac, Cordials & Proprietary Liqueurs
55	Liquor	Vodka
56	Medications / Remedies / Health Aids	Foot Preparations-Athlete's Foot
57	Men's Toiletries	Cologne/Lotion-Men's
58	Packaged Meats-Deli	Bacon-Refrigerated, Sausage-Dinner, Sausage-Breakfast, Bacon-Beef & Canned
59	Packaged Meats-Deli	Bacon-Refrigerated
60	Packaged Meats-Deli	Bratwurst & Knockwurst, Frankfurters-Refrigerated, Franks-Cocktail-Refrigerated
61	Packaged Meats-Deli	Bratwurst & Knockwurst, Sausage-Dinner, Frankfurters-Refrigerated
62	Packaged Meats-Deli	Lunchmeat-Sliced-Refrigerated, Lunchmeat-Nonsliced-Refrigerated, Lunchmeat-Deli Pouches-Refrigerated
63	Packaged Meats-Deli	Lunchmeat-Sliced-Refrigerated
64	Packaged Meats-Deli	Sausage-Breakfast
65	Pet Food	Cat Food - Wet Type, Cat Food - Moist Type, Cat Food - Dry Type
66	Pet Food	Dog & Cat Treats
67	Pet Food	Dog Food - Wet Type, Dog Food - Moist Type, Dog Food - Dry Type
68	Pickles, Olives, And Relish	Pickles - Sweet
69	Pickles, Olives, And Relish	Relishes
70	Pizza/Snacks/Hors Doeurves-Frzn	Frozen/Refrigerated Hors D' Oeuvres & Snacks, Pizza-Frozen, Pizza Crust-Frozen, Meal Starters, Entrees - Remaining - 2 Food - Frozen, Entrees - Seafood - 2 Food - Frozen, Entrees - Meat - 2 Food - Frozen, Entrees - Poultry - 2 Food - Frozen, Entrees - Multi Pack - Frozen, Entrees - Italian - 2 Food - Frozen, Dinners-Frozen, Entrees - Mexican - 2 Food - Frozen
71	Prepared Food-Ready-To-Serve	Chicken - Shelf Stable
72	Prepared Food-Ready-To-Serve	Chili-Shelf Stable
73	Prepared Food-Ready-To-Serve	Stew - Beef - Shelf Stable, Stew - Remaining - Shelf Stable, Stew - Chicken - Shelf Stable
74	Prepared Foods-Frozen	Entrees - Meat - 1 Food - Frozen
75	Prepared Foods-Frozen	Entrees - Remaining - 2 Food - Frozen, Entrees - Seafood - 2 Food - Frozen, Entrees - Meat - 2 Food - Frozen, Entrees - Poultry - 2 Food - Frozen, Entrees - Multi Pack - Frozen, Entrees - Italian - 2 Food - Frozen, Dinners-Frozen, Entrees - Mexican - 2 Food - Frozen, Entrees - Meat - 1 Food - Frozen, Entrees - Poultry - 1 Food - Frozen, Entrees - Oriental - 1 Food - Frozen, Entrees - Italian - 1 Food - Frozen
76	Skin Care Preparations	Hand & Body Lotions
77	Skin Care Preparations	Hand Cream
78	Skin Care Preparations	Skin Cream-All Purpose
79	Snacks	Dip - Mixes
80	Snacks	Popcorn - Popped, Snacks - Caramel Corn

Market	Nielsen Product Group	Nielsen Product Modules in Product Market
81	Snacks	Snacks - Health Bars & Sticks
82	Snacks	Snacks - Potato Chips, Snacks - Potato Sticks
83	Snacks	Snacks - Potato Chips
84	Snacks	Snacks - Pretzel
85	Snacks	Snacks - Remaining
86	Spices, Seasoning, Extracts	Pepper
87	Spices, Seasoning, Extracts	Vegetables - Onions - Instant
88	Stationery, School Supplies	Dry Erase Bulletin Board And Accesory
89	Stationery, School Supplies	Personal Planners Binders And Folders
90	Unprep Meat / Poultry / Seafood-Frzn	Frozen Poultry
91	Vegetables - Canned	Mushrooms - Shelf Stable
92	Vegetables - Canned	Vegetables-Mixed-Canned
93	Vegetables - Canned	Vegetables - Peas - Remaining - Canned, Vegetables - Peas - Canned, Vegetables - Peas & Carrots - Canned

Table A.2: Product Market Definitions

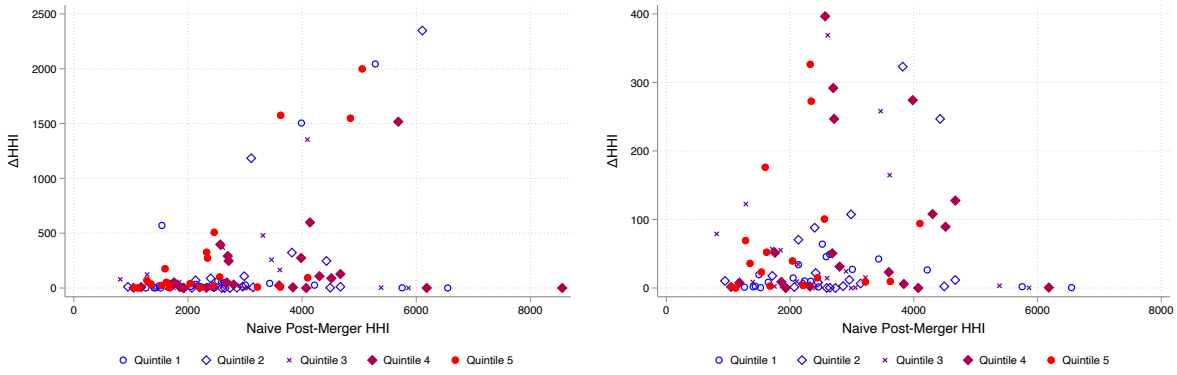
	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	108	0.29	3.75	-2.15	-0.16	1.98
		(0.36)		(0.36)	(0.39)	(0.64)
Merging Parties	108	0.88	5.19	-2.07	1.03	3.97
		(0.49)		(0.74)	(0.51)	(0.74)
Non-Merging Parties	108	0.17	3.82	-2.13	-0.41	1.99
		(0.37)		(0.38)	(0.41)	(0.66)
B. Cost and Demographic Controls						
Overall	106	0.52	2.86	-0.92	0.01	1.74
		(0.27)		(0.40)	(0.27)	(0.52)
Merging Parties	106	1.22	4.84	-1.72	0.74	3.99
		(0.47)		(0.70)	(0.54)	(0.71)
Non-Merging Parties	106	0.46	2.85	-1.10	0.14	1.81
		(0.28)		(0.33)	(0.27)	(0.50)
C. Treated/Untreated						
Overall	101	0.64	8.18	-1.65	-0.01	1.21
		(0.78)		(0.36)	(0.24)	(0.42)
Merging Parties	101	1.40	9.33	-2.30	0.04	2.92
		(0.91)		(0.67)	(0.37)	(0.95)
Non-Merging Parties	101	0.52	7.91	-1.78	-0.28	1.09
		(0.74)		(0.34)	(0.27)	(0.40)

Table A.3: Overall Price Effects. We aggregate across mergers using the random effects weighting scheme of DerSimonian and Laird (1986).



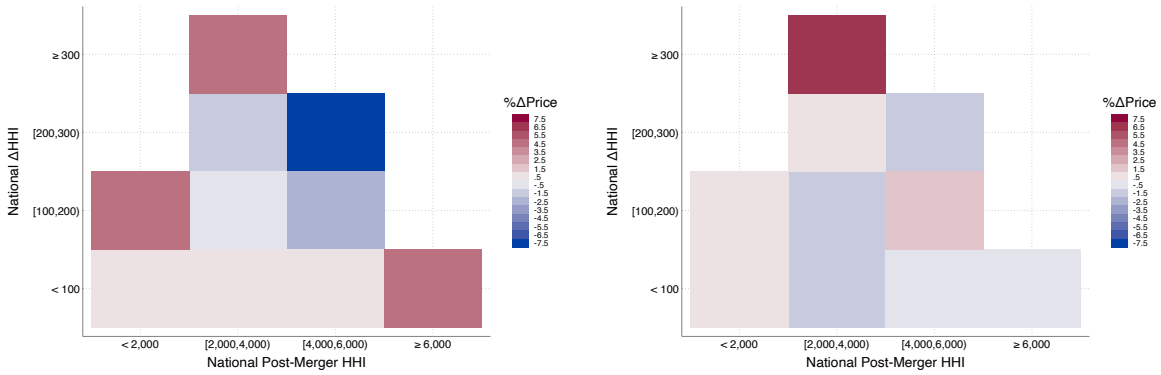
(a) Cost and Demographic Controls (b) Untreated Markets

Figure A.1: Price changes for merging parties, estimated (a) with and without controls and (b) comparing to untreated markets or not. The size of each point corresponds to the inverse variance of the parameter estimate in the baseline.



(a) Unrestricted (b) Restricted

Figure A.2: Scatter plots of nationwide ΔHHI against nationwide post-merger HHI for mergers in our sample. Panel (a) displays the results for all mergers, while Panel (b) restricts to mergers for which $\Delta\text{HHI} < 400$ and post-merger HHI $< 7,000$. Colors/shapes represent the quintile of the price change for non-merging parties, with solid (red) colors representing larger price changes. Note that $\Delta\text{HHI} < 0$ corresponds to divestitures being required in the merger.



(a) Merging Parties

(b) Non-Merging Parties

Figure A.3: Heat map of nationwide ΔHHI against nationwide post-merger HHI for mergers in our dataset. Colors represent different mean changes in price, as estimated by (1), for each bin of HHI and ΔHHI. Means are weighed by the inverse variance of each underlying parameter estimate.

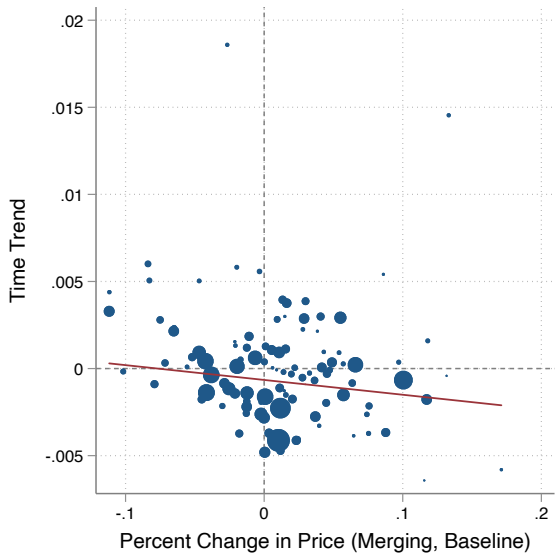


Figure A.4: Scatter plot of time trend estimated using only pre-merger data versus merging party price effects as measured by 1. The size of each point corresponds to the inverse variance of the estimate of the price change.

B. Selection Model

In this appendix, we provide further details about the selection model in Section VI. Section B.1 discusses a general version of the model, and Section B.2 provides more detail of the estimation method.

B.1. Mechanics of the Model

Suppose that a merger is categorized by a vector (X_i, Z_i) of characteristics, and the distribution of price changes is given by $F(p_i, X_i)$. The antitrust authority proposes a remedy for a merger with probability $\lambda(p_i, X_i, Z_i)$, where Z_i has sufficient variation that for sufficiently extreme Z_i $\lambda(p_i, X_i, Z_i)$ is arbitrarily close to 0. Importantly, Z_i enters into the probability of a remedy but not into the distribution of price changes.

Suppose that true price changes are observed if the merger is allowed, and if the merger is not allowed we know that it is not. For a sufficiently extreme Z_i, we will observe the true distribution $F(X_i)$ of price changes. This is illustrated by the bold dark distribution in the top panel of Figure B.1.

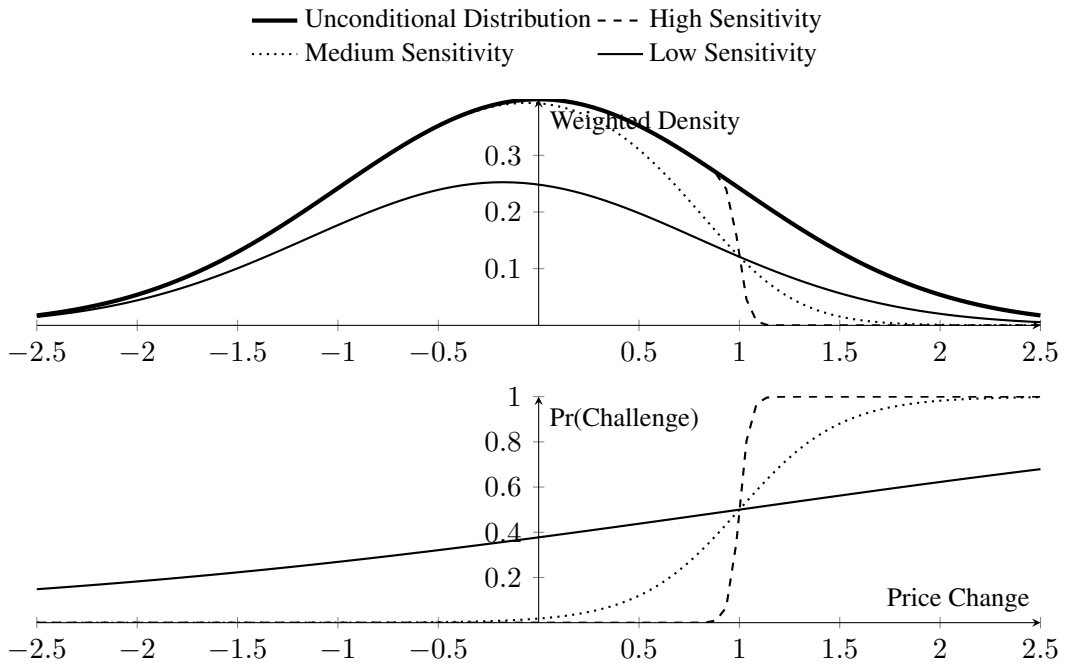


Figure B.1: Observed distribution of prices (top) and implied $\lambda(\cdot)$ (bottom)

For different values of Z_i, we observe the density of price changes of consummated mergers too—as well as the probability a merger was not challenged. This give rise to a set of "weighted densities" illustrated in the top panel—distributions that integrate to the probability of consummation (without remedies) given Z_i. The thinner lines illustrate three possibilities of such weighted densities, depending on the agencies' enforcement behavior. All these densities lie beneath the unconditional density.

Everything in the top panel is data. The ratio of the unconditional density and the weighted one gives $\lambda(p_i, X_i, Z_i)$, illustrated for the three possibilities. The dashed line (labeled "high sensitivity") is a situation where the agency challenges mergers with price changes above 1% and lets others through. The "medium" and "low" sensitivity lines are policies where the agency challenges some mergers with lower price increases as well and is not as aggressive at challenging mergers with larger price changes.

The function $\lambda(p_i, X_i, Z_i)$ is an estimable primitive. In Section VI, we interpret the agency's decision as a threshold: it challenges a merger if its perceived price increase is larger than some cutoff $\bar{p}(X_i, Z_i)$. This interpretation is consistent with the model here. Since $\lambda(\cdot)$ is increasing in p_i and bounded in $[0, 1]$, it is a CDF of some random variable $\tilde{\epsilon}(X_i, Z_i)$. Thus, we can write

$$\begin{aligned}\lambda(p_i, X_i, Z_i) &= \Pr(\tilde{\epsilon}(X_i, Z_i) \leq p_i) \\ &= \Pr(\tilde{\epsilon}(X_i, Z_i) - \mathbb{E}[\tilde{\epsilon}(X_i, Z_i)] \leq p_i - \mathbb{E}[\tilde{\epsilon}(X_i, Z_i)]) \\ &= \Pr(p_i - \epsilon_i \geq \mathbb{E}[\tilde{\epsilon}(X_i, Z_i)]), \end{aligned} \tag{4}$$

where ϵ_i is the demeaned version of $\epsilon(X_i, Z_i)$. The calculations in (4) show that $\bar{p}(X_i, Z_i)$ is simply the expectation of the variable whose cdf is given by the bottom panel of Figure B.1. When this function is close to an indicator for being large than a threshold (in true price changes), the associated expectation is of course this threshold. In general cases, it can be interpreted as an average of prices, weighted by how price-sensitive the probability of a challenge is at that true price.

The agencies likely have more information about mergers. Suppose the agencies make their

decisions based on X_i, Z_i and some merger-specific information $\tilde{X}_i$ not available to us. In this case, what we recover is simply the expected probability of challenging a merger with a price change of p_i and observable characteristics X_i and Z_i, i.e.,

$$\lambda(p_i, X_i, Z_i) = \int \Pr(\text{challenge}|p_i, X_i, \tilde{X}_i, Z_i)\, dF(\tilde{X}_i|X_i).$$

The expected value of the random variable with this cdf will still be interpreted as an average threshold. However, this observation highlights that the error term should not be interpreted literally as an uncertainty in the estimate of a price change; it could be masking other considerations not taken into account into the analysis without necessarily threatening the interpretation of $\bar{p}$ as a threshold.

B.2. Estimation Details

The model discussed in Section VI is a parametric version of the one in Appendix B.1, and it lends itself to a Bayesian estimation procedure involving Gibbs sampling with data augmentation; in particular, we augment with the true price change p_i^* (which is unobserved) and the belief errors ϵ_i. This involves the following steps.

1. *Initialize.* We first initialize variables that we do not observe. This involves picking $\beta_{\bar{p}} = 0$, setting $\epsilon_i = 0$, and then setting $p_i^* = -1$ for each merger that was approved without a remedy and $p^* = 1$ for all other mergers.

2. *Draw p_i^*.* For mergers that had a remedy, all we know is that $p_i^* \sim N(X_i'\beta_{p^*}, \sigma_{p^*})$, truncated to being above $\bar{p}(X_i) - \epsilon_i$. For mergers that were allowed through, we know that $p_i^* \sim N(\tilde{\mu}, \tilde{\sigma}^2)$, where $\tilde{\mu}$ and $\tilde{\sigma}$ are the appropriate mean and standard deviation of the conditional distribution given the observed price change (with noise) is p_i.

3. *Draw β_{p^*} and σ_{p^*}.* This is Bayesian OLS of p_i^* on X_i.

4. *Update ϵ_i.* This involves drawing from truncated normals, knowing that $p_i^* + \epsilon_i$ must be below

or above $\bar{p}(X_i)$, depending on whether the merger was let through without remedies.

5. *Draw* $\beta_{\bar{p}}$. This involves a Gibbs sampler through each dimension. The conditional distribution of each $\beta_{\bar{p},i}$ is truncated normal, where the truncation points are picked to restrict the constraints that $p_i^* + \epsilon_i \leq \bar{p}(X_i)$ for all mergers that were allowed without remedies, and the opposite inequality holds for all other mergers. We then loop back to Step 1.