Merger Effects and Antitrust Enforcement: Evidence from US Retail

VIVEK BHATTACHARYA[1], GASTÓN ILLANES[1], and DAVID STILLERMAN[2]

[1]Department of Economics, Northwestern University and NBER
[2]Kogod School of Business, American University

March 2, 2023

ABSTRACT. We document the effects of a comprehensive set of US retail mergers. On average, prices increase by 1.5% and quantities decrease by 2.3%, with significant heterogeneity in outcomes across mergers. Price changes correlate with the screens codified in the Horizontal Merger Guidelines. Through a model of enforcement, we find that agencies challenge mergers they expect would increase average prices more than 8–9%. Modest increases in stringency reduce prices and the prevalence of approved anti-competitive mergers, with minimal impacts on blocked pro-competitive mergers, at a significantly greater agency burden. Our findings inform the debate over whether antitrust enforcement has been lax.

KEYWORDS. Antitrust, Merger Retrospectives, Horizontal Merger Guidelines.
JEL CODES. D43, K21, L13, L41.

CONTACT. vivek.bhattacharya@northwestern.edu, gaston.illanes@northwestern.edu, stillerman@american.edu. The first version of this paper was circulated with the title "Have Mergers Raised Prices? Evidence from US Retail." We are grateful to Christoph Carnehl, José Ignacio Cuesta, Jan De Loecker, Jan Eeckhout, Francisco Garrido, Igal Hendel, Daniel Hosken, Francine Lafontaine, Alex MacKay, Ioana Marinescu, Stephen Martin, Joe Mazur, Aviv Nevo, Ariel Pakes, Rob Porter, Mar Reguant, Nancy Rose, Andrew Sweeting, Bill Rogerson, Mike Vita, Mike Whinston, and Ali Yurukoglu for useful feedback. John Asker, Josh Feng, Gabrielle Rovigatti, and Matthew Weinberg provided helpful discussions. JD Salas provided excellent research assistance, as did Aisling Chen, Rosario Cisternas, Avner Kreps, Marina Siqueira, and Yintian Zhang. We are also grateful for help from Aaron Banks, Katherine Daehler, Ethan Nourbash, Nathan Friedle, Denis Gribenica, Tianshi Wang, and numerous other research assistants. This project was funded by grants from the Center for Equitable Growth and the National Science Foundation (SES-2116934). Researcher(s) own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researcher(s) and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein. All errors are our own.

I. Introduction

The past few years have featured a heated debate over whether antitrust enforcement has been too lax (Kwoka, 2014; Scott Morton, 2019; Shapiro, 2021; Nocke and Whinston, 2022; Rose and Shapiro, 2022). This question is difficult to answer, as it requires an understanding of the outcomes of consummated mergers and predictions of merger outcomes under counterfactual antitrust regimes. We contribute to this debate in two ways. First, we document how a comprehensive set of mergers in one sector has affected prices, quantities, and other equilibrium outcomes of interest. Second, through a model of agency decisions, we investigate the relationship between these outcomes and enforcement actions. Through the lens of our model, we quantify the implicit expected price increase that triggers antitrust enforcement and the uncertainty in agency decision-making. The model allows us to predict both the propensity to challenge mergers in counterfactual regimes and the characteristics of consummated mergers, including expected price changes and the prevalence of allowed anti-competitive mergers.

Our first contribution relates to the observation that a merger's effects are ambiguous. The standard treatment of horizontal mergers (Williamson, 1968) recognizes that cost savings due to synergies can compensate for increases in market power. Furthermore, mergers can induce changes in distribution or lead to repositioning (Sweeting, 2010; Fan, 2013). Accordingly, whether approved mergers typically increase or decrease prices, quantities, and product offerings is an important empirical question for evaluating antitrust policy. Researchers have consistently 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 large body of prior work, reviewed below, has conducted such retrospectives, studied mergers are often selected on particular dimensions. For a merger to be part of a retrospective, it must satisfy three conditions: (i) the merging parties must have proposed it, (ii) the enforcement agencies must have allowed it (or unsuccessfully challenged it), and (iii) 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 considerations whose effects are unclear:

interest in the popular press, data availability, and the potential for publication. Such selection leads 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 systematically analyzes the price and quantity effects of mergers in US consumer packaged goods from 2006 to 2017. We analyze 126 product markets (e.g., canned soup or soluble coffee) in 50 transactions (e.g., a merger between large food conglomerates). This set consists of all transactions with a deal size larger than \$280 million involving 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 proposed and approved in this industry.

Our baseline estimates of the effects of mergers rely on comparisons within geographies and products before and after merger completion, controlling for brand-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 two-thirds of our mergers, we can also use the prices of products in geographic markets where the merging parties have a negligible presence as a control.

We find that merging parties decrease prices by 0.1% on average in the two years following the merger. However, this average masks substantial heterogeneity: the first quartile of price effects corresponds to a price decrease of 5.2%, and the third quartile corresponds to a price increase of 5.9%. Non-merging parties exhibit a price increase of 2.1% on average with a slightly narrower distribution of price changes. Overall, the average effect of consummated mergers on price changes is about 1.5%.

We next consider effects on total quantities. We find that aggregate quantities decreased 2.3% on average. The first quartile of aggregate quantity changes is -6.9%, and the third quartile 3.1%. Merging parties are much more likely to reduce quantity sold: their average quantity change is -7.6%. We show that these quantity reductions are not due to temporary supply disruptions induced by the merger, but rather by changes in firm strategies. In particular, quantity reductions correlate with price increases, reductions in the number of stores served by brands and in their geographic footprint, and the elimination of products at the national level.

How do these effects correlate with antitrust enforcement? The Horizontal Merger Guidelines provide "structural presumptions," related to both the Herfindahl-Hirschman Index (HHI) and its change induced by the merger (DHHI), that connect market structure to the likelihood that a merger raises competitive concerns.[1] We find evidence favoring the merger guidelines' use of both metrics in screening. Price changes of consummated mergers are positively correlated with average DHHI across markets; within-merger, price changes in a geographic market correlate with HHI and DHHI in that market.

These results provide a systematic analysis of the effects of completed mergers in one sector. However, we make two caveats about these results. First, these distributions represent the effects of all observed mergers, not of all possible or profitable ones. Mergers that would be especially anti-competitive either do not get proposed, are successfully blocked, or go through with divestitures. Second, as Carlton (2009) illustrates, due to both this selection and the fact that the agency has, at best, a noisy signal of the future price change when the merger is proposed, the distribution of realized price changes does not inform whether an agency is too strict (blocking pro-competitive mergers with negative expected price effects) or too lax (allowing anti-competitive mergers with positive expected price effects).

Our second contribution is to evaluate the stringency of current antitrust policy and, in the process, address selection into approval. To do so, we collect data on enforcement actions for each of the mergers in our dataset. In Section IV.C, we estimate a simple model of the agencies' decision to propose a remedy for a merger. In the 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 about 8–9%. Furthermore, our model then allows us to simulate the effects of counterfactual antitrust stringency. We find

[1] The HHI is the sum of the squares of the market shares (in percentage points) of the firms in a market. Throughout the paper, when we refer to post-merger HHI and DHHI, we refer to the so-called "naive" or "pro forma" versions used by the agencies, which assume that the share of the merged entity post-merger will become the sum of the shares of the individual entities.

that moving to a 5% threshold would reduce aggregate price increases by about 1 pp, would have a negligible impact on the prevalence of blocking pro-competitive mergers ("type I errors"), and decrease the probability of allowing anti-competitive mergers ("type II errors"). However, this threshold would lead to significantly higher administrative burden, almost tripling the number of mergers the agencies must challenge. Quantifying the two sides of this trade-off provides concrete evidence to the current debate on antitrust standards.

Related Literature. Whinston (2007, p. 2425) noted that documenting the price effects of actual mergers is "clearly an area that could use more research," and Carlton (2009) highlighted the need for more data to guide antitrust reform. Since then, there have been a growing number of merger retrospectives, surveyed in Farrell et al. (2009), Hunter et al. (2008), Kwoka (2014), and Asker and Nocke (2021).

One class of merger retrospectives involves in-depth studies of a small handful of mergers, usually 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).[2] Kwoka (2014) provides a helpful meta-analysis to aggregate these results, but it is naturally still subject to selection into publication.

To address this issue, some papers have studied a large subset of mergers in a particular industry: Kim and Singal (1993) study 14 airline mergers from 1985–1988, and Focarelli and Panetta (2003) study 43 mergers of Italian banks from 1990–1998. A handful of recent contemporaneous papers develop larger databases of M&A activity, also in specific industries. Some of these studies focus on prices: in consumer packaged goods (Majerovitz and Yu, 2021), hospitals (Brand et al., 2022), and pharmaceuticals (Feng et al., 2023). The broad goal of these papers is similar to our first contribution, but each brings a new angle to the discussion on

[2]There are many more that we do not have space to cite. The Federal Trade Commission manages a large bibliography of merger retrospectives at https://www.ftc.gov/policy/studies/merger-retrospective-program/bibliography.

price changes. Majerovitz and Yu (2021) highlight the asymmetries in size between targets and acquirors. Brand et al. (2022) highlight the predictive power of metrics of substitution between hospitals, and Feng et al. (2023) show that price changes are larger for mergers below the Hart-Scott-Rodino reporting thresholds.

We also contribute to the nascent literature on large-scale retrospectives considering non-price effects. The earliest contribution to this literature is Atalay et al. (2022), who study the effect of mergers on product offerings. Demirer and Karaduman (2023) show that mergers of US power plants typically improve efficiency. Benson et al. (2022) document that bank mergers lead to branch closings.

Finally, we contribute to the literature that studies the agencies' decisions. Prior work has correlated enforcement with ex-ante merger characteristics (Bergman et al., 2005; Kwoka, 2014; Affeldt et al., 2021b) or computed required compensating efficiencies using approximations leveraging ex-ante metrics of market structure (Affeldt et al., 2021a). To our knowledge, the only other papers that connect ex-post price changes to agency actions are Brand et al. (2022), who find that mergers that were scrutinized more had higher price changes, and Chen et al. (2022), who find statistically insignificant effects of requiring a divestiture on price changes in pharmaceutical markets. However, these results do not speak to the agencies' objective in how to scrutinize mergers, nor do they study counterfactual policies.

More broadly, the increased interest in documenting the effects of mergers parallels a growing literature estimating markups at a large scale, started by the seminal contribution of De Loecker et al. (2020). Grieco et al. (2022a) document rising markups in the automobile industry over several decades. Brand (2021), Döpper et al. (2022), and Atalay et al. (2023) conduct a similar exercise over a broad set of consumer packaged goods industries. While we do not document markups, our paper sheds light into how merger activity has affected consumers.

II. Data and Sample Selection

II.A. Data Sources

To construct our sample, 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 transactions involving manufacturers of products sold in groceries and mass merchandisers, for which fine-grained 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." The data cover 2.6–4.5 million UPCs, depending on the year, and include food, non-food grocery items, health and beauty aids, and select general merchandise. We have access to this dataset from 2006 to 2019. Nielsen provides sales at the store-week level and the average transaction price for each UPC. Nielsen also provides some information about the product, including product characteristics and classification into "modules." As discussed in Section II.B, we use these modules to guide product market definitions. We use designated market areas (DMAs) as our measure of geographic markets: these are collections of counties defined by NielsenIQ, usually centered around a major city.

Since NielsenIQ does not provide ownership of each product, we augment the dataset with ownership information from Euromonitor Passport.[3] We also supplement with data from other sources to account for demand and supply-side characteristics that could influence prices. First, for each merger, we list inputs for products (e.g., wheat for cereal) and obtain commodity price indices, typically from Federal Reserve Economic Data (FRED). Second, we collect data on demographics to control for changes that could affect demand by aggregating county-level data from the American Community Survey to the DMA level.

Finally, for our analysis of enforcement stringency in Section IV.C, we recover whether the agencies required divestitures for a given deal to be approved and which product markets within that deal were subject to scrutiny. We obtain this information from publicly-available case filings, including Complaints and publicly-recorded

[3]This practice departs from prior research working with NielsenIQ data, which usually maps products to owners by looking at a UPC's first six to nine digits. These digits 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 in an acquisition can take up to a year, and there is no hard and fast rule determining whether company prefixes are transferred from the acquirer to the target after a partial divestiture. See Section 1.6 of the GS1 General Specifications, Release 22.0, for complete details.

Decision and Order documents, available on the websites of the DOJ and FTC.[4]

II.B. Market Definition, Merger Selection, and Outcomes

NielsenIQ does not categorize products into product markets suitable for antitrust analysis. Instead, it categorizes products into product groups, broad categories such as "Prepared Foods - Frozen" or "Condiments, Gravies and Sauces," and product modules, finer subcategories such as "Entrees - Meat - 1 Food - Frozen" or "Sauce Mix - Taco." Moreover, the degree of granularity varies significantly across product groups and modules. Rather than defining product markets as either categorization, we define them as sets of product modules based on our industry knowledge.[5]

Having defined product markets, we next identify all deals where the two parties competed in at least one product market-DMA during the period spanning 24 months before the deal's announcement to 24 months past the deal's completion date. We do so in two steps. First, we filter the SDC Platinum dataset to only include deals valued at $280 million dollars or more involving manufacturers of retail products. Second, we identify which of these transactions involve products tracked in the NielsenIQ Scanner Dataset, and check whether the parties overlapped in particular product and geographic markets. To check this condition, we look at all UPCs in the product market that are sold within a two-year window of the deal and select those with a non-negligible market share.[6,7] We then assign each to their owners and only keep product markets where both the target and the acquirer sell at least one selected UPC in the same DMA in the 24 months prior to deal completion.

[4]See https://www.justice.gov/atr/antitrust-case-filings-alpha (DOJ) and https://www.ftc.gov/enforcement/cases-proceedings (FTC).

[5]For example, the Nuts product group includes modules such as "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. We group all nuts into a single market but separate snacks into different markets.

[6]Throughout this paper, we compute shares using product volumes. We convert product sizes to common units (e.g., liters or kilograms) before aggregating quantities to determine market share.

[7]We define UPCs with non-negligible market share to ensure we capture all products with a national presence, seasonal versions of popular brands, and important regional products. This allows us to work with a tractable number of products, as we have to match ownership by hand, while also expanding the set of UPCs whenever the product market is remarkably varied. In Appendix C, we document that this procedure leads to high coverage.

Table C.1 in Appendix C presents a list of product markets for the deals in our final sample and their respective cost controls. In what follows, we refer to a product market-deal pair as a merger. For example, if X acquires Y and both sell products in product markets 1 and 2, that deal generates two mergers for our dataset. Our final sample consists of 126 mergers over 50 deals. Appendix C provides more details about the sample and the construction procedure.

To compute outcomes, we restrict our analysis to a balanced panel of stores within the two years around a merger to ensure our results are not confounded by variation over time in the composition of stores that report data to Nielsen. Our price metric is volume-weighted average monthly prices by UPC and DMA. For non-price outcomes, we aggregate to the firm type (i.e., merging/non-merging) level and compute the following measures separately by firm type: (i) volume sold by DMA-month, (ii) the number of unique stores in which at least one UPC was sold in a DMA-month, and (iii) the number of unique brands sold in a DMA-month. Finally, we construct a monthly panel of the number of brands sold nationwide by merging and non-merging parties.

II.C. Properties of Approved Mergers

Table 1 presents summary statistics for our final sample. Each row corresponds to a NielsenIQ Product Group, which is a coarser categorization than our product market definitions (in Table C.1) but serves to illustrate in which broad product categories the mergers are taking place.[8] For each product group, we display the average yearly product market sales in the pre-merger period, the merging parties' revenue share, and the average post-merger HHI and DHHI computed across mergers and DMAs.

Panels (a) and (b) of Figure 1 present histograms of average post-merger HHI and naive DHHI. Most mergers have average (across DMAs) post-merger HHIs between 2,000 and 4,000, with some reaching values over 6,000. Most values of DHHI are low, close to zero, but several mergers have values over 200. Panel (c) shows a scatter plot of average post-merger HHI and DHHI. The mergers with the highest values of DHHI tend to have post-merger HHI levels between approximately

[8]Our data agreement prohibits us from identifying individual companies and brands.

Product Group Name	N	Product Market Sales (Million USD / yr)	Merging Parties' Revenue Share	HHI	DHHI
All	126	592.3	19.8%	3157.7	140.3
Baby Food	1	1436.3	12.9%	4865.5	117.1
Baked Goods-Frozen	2	707.8	32.3%	3996.2	50.0
Beer	2	2916.6	30.0%	4284.3	530.5
Bread And Baked Goods	15	651.0	17.1%	3785.8	94.9
Breakfast Foods-Frozen	2	691.5	4.7%	2521.5	0.7
Candy	4	1249.7	13.0%	1768.0	52.2
Cereal	2	695.7	7.5%	2521.0	23.8
Coffee	2	1058.0	18.6%	2416.9	6.0
Condiments, Gravies, And Sauces	11	35.2	38.2%	4250.2	452.3
Cookies	1	1796.6	0.9%	2406.4	0.1
Cosmetics	11	123.5	19.5%	2690.6	207.8
Detergents	2	988.2	9.8%	2373.3	139.0
Fragrances - Women	1	99.9	13.4%	2523.6	16.1
Fresh Produce	1	75.5	42.1%	6453.7	31.1
Grooming Aids	1	142.8	4.3%	3436.5	2.9
Gum	2	841.9	41.7%	3467.4	93.4
Hair Care	7	351.9	21.6%	2607.8	514.8
Houseware, Appliances	1	25.9	50.9%	6856.3	11.2
Juice, Drinks - Canned, Bottled	1	4048.7	15.8%	2047.3	14.1
Kitchen Gadgets	1	136.5	23.0%	1164.7	90.4
Liquor	5	1129.6	3.1%	2217.6	8.1
Medications/Remedies/Health Aids	1	63.3	14.2%	3429.7	31.0
Men's Toiletries	2	41.1	19.2%	2291.7	1.3
Packaged Meats-Deli	6	869.4	9.9%	2273.8	22.6
Pet Food	3	1017.8	17.9%	3041.3	90.3
Pickles, Olives, And Relish	3	49.7	18.1%	2984.7	47.8
Pizza / Snacks / Hors D'oeuvres-Frozen	1	1593.9	42.1%	2728.7	134.8
Prepared Food-Ready-To-Serve	3	100.2	9.8%	4308.6	2.9
Prepared Foods-Frozen	1	273.7	3.9%	1661.4	3.8
Shortening, Oil	1	122.7	16.8%	3660.9	3.3
Skin Care Preparations	4	259.8	12.7%	1958.0	68.4
Snacks	11	664.8	11.9%	2865.6	32.2
Spices, Seasoning, Extracts	5	133.7	48.7%	3591.7	110.1
Stationery, School Supplies	2	89.6	15.3%	2057.7	6.4
Tobacco & Accessories	1	3616.7	31.4%	4403.1	117.6
Unprep Meat/Poultry/Seafood-Frzn	1	361.7	6.9%	5162.8	2.5
Vegetables - Canned	4	68.0	10.2%	4183.3	5.2
Vegetables And Grains - Dried	1	80.5	62.6%	4877.1	1079.8
Wine	1	2063.3	21.1%	2090.7	20.9

Table 1: Summary statistics for the final sample of mergers

3,000 and 5,000, and mergers in markets with post-merger HHI above 6,000 are only approved when DHHI is lower. Panel (d) presents a scatter plot of average yearly sales of the merging parties (in millions of dollars) and DHHI. Around half of the mergers with DHHI over 500 are small, with average yearly sales for the merging parties below \$100 million, but several feature DHHI near 500 and yearly sales around \$1 billion. These patterns are consistent with the selection process determining whether we observe a consummated merger: we expect greater antitrust scrutiny on mergers involving large product markets and high values of DHHI and post-merger HHI. Nevertheless, several mergers involving substantial increases in naive DHHI have been approved, even in large product markets.

III. The Effects of Consummated Mergers

III.A. Empirical Strategy

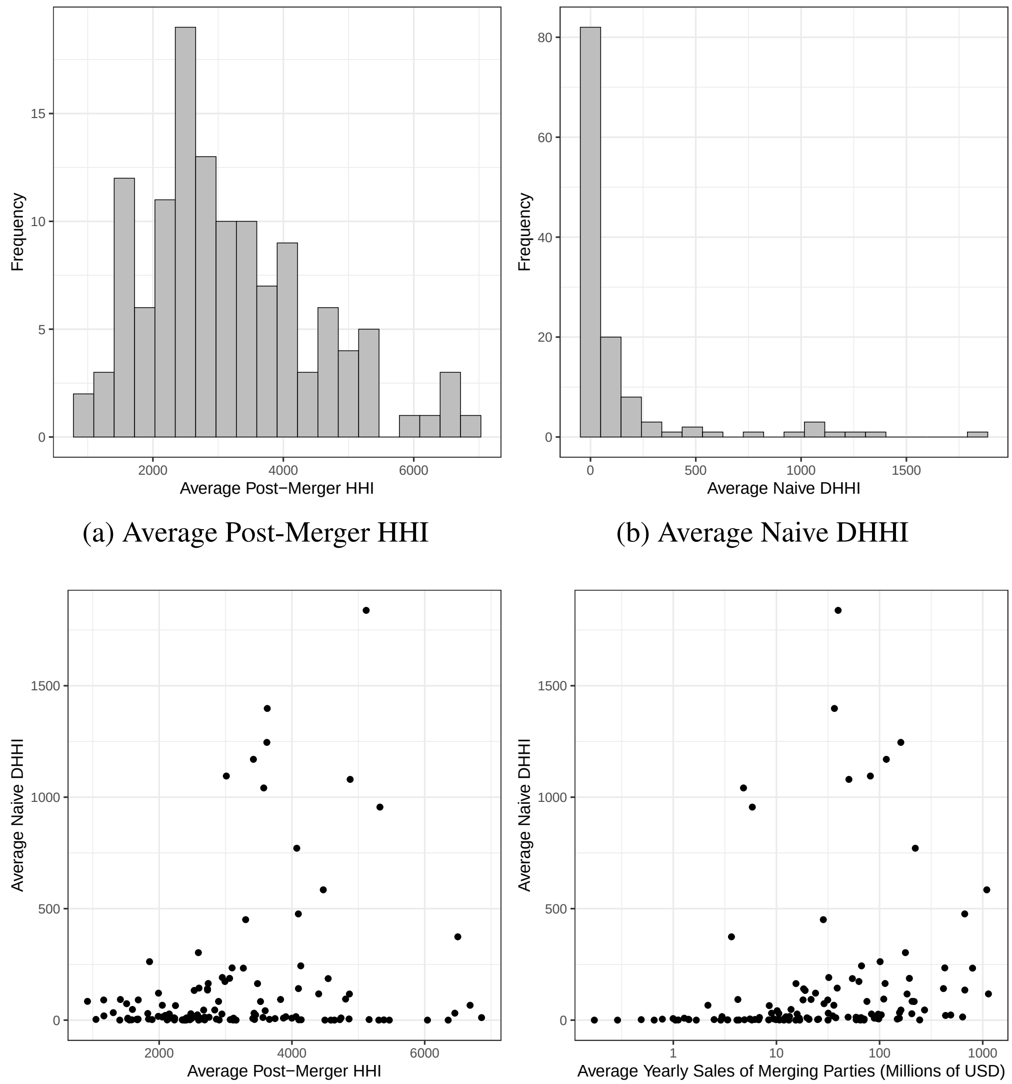
We take two broad approaches to estimate the effect of mergers on the outcomes of interest. The first approach is at its heart a before-after comparison: we compare outcomes before and after the merger controlling for trends, tastes for products, and seasonality. We implement the procedure in two steps. In the first step, we use data for the 24 months prior to the merger and regress

$$\log y_{idt} = \alpha_{b(i)} \cdot t + \xi_{id} + \xi_{m(t)} + \text{Controls}_{idt} + \epsilon_{idt}, \tag{1}$$

where i is a UPC, d is a DMA, and t is a month. In this specification, $\alpha_{b(i)} \cdot t$ is a linear time trend for the brand $b(i)$ of product i, ξ_{id} is a UPC-DMA fixed effect, and $\xi_{m(t)}$ is a month of the year fixed effect. This regression allows us to identify a brand-specific time trend after controlling for idiosyncratic differences in tastes for products across cities and for seasonality. In some specifications, we also add demographic and cost controls. We then use data for the 24 months after merger completion, and regress

$$\log y_{idt} - \widehat{\log y_{idt}} = \beta_1 \mathbb{1}[\text{Merging Party}]_i + \beta_2 \mathbb{1}[\text{Non-Merging Party}]_i + \epsilon_{idt}, \tag{2}$$

where $\widehat{\log y_{idt}}$ is the predicted value of the log of the outcome of interest, obtained from (1). The coefficients of interest are β_1 and β_2, which give the average difference in the outcome of interest between the realized value and its prediction using pre-merger data, separately for merging and non-merging parties. In some specifications,



(a) Average Post-Merger HHI

(b) Average Naive DHHI

(c) Average Post-Merger HHI and DHHI

(d) Sales and DHHI

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

the outcome of interest is an aggregate of both parties. In these cases, the right hand side of (2) is a constant.

We interpret (1) as giving us the counterfactual outcome had there not been a merger. The main assumption is that outcomes would have continued on the same trend after controlling for city-level tastes for individual products and seasonality.

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) and (2) are an event study.

This identification strategy is based on the idea that any secular trends in demand or cost are gradual, so outcome data at the monthly level lets us estimate them well. Is a linear time trend sufficient to capture post-merger changes in the environment? We address this question by augmenting (2). We expand the horizon to a 24-month window around the merger and add monthly merging and non-merging party coefficients

$$\log y_{idt} - \widehat{\log y_{idt}} = \sum_{\tau=-24}^{24} \Big(\beta_{1,\tau} \mathbb{1}[\text{Merging Party}]_i \cdot \mathbb{1}[t=\tau] \\ + \beta_{2,\tau} \mathbb{1}[\text{Non-Merging Party}]_i \cdot \mathbb{1}[t=\tau] \Big) + \epsilon_{idt}. \quad (3)$$

We then study trends in $\beta_{1,\tau}$ and $\beta_{2,\tau}$. Since plotting 126 trends will not produce clear insights, we report averages separately for mergers in the top and bottom 25th percentile of the change in the outcome of interest and for mergers with changes between these percentiles. For example, see Figure 3 for prices. We condition on the magnitude of the post-merger change in the outcome to show that trends in the pre-period do not drive effects for mergers with the most extreme changes: positive estimated price effects are not due to inappropriately controlling for positive pre-trends, for instance. We do not find significant patterns in pre-period outcomes.

As a robustness check, we control for log income per household at the DMA level and for input prices (see Table C.1). Additionally, we consider an alternative identification strategy. This second approach uses outcome changes in geographic markets where the merging parties comprise a small share of total sales as a control group. In this approach, we leave (1) unchanged, but replace (2) with

$$\log y_{idt} - \widehat{\log y_{idt}} = \beta_1 \mathbb{1}[\text{Merging Party}]_i + \beta_2 \mathbb{1}[\text{Non-Merging Party}]_i \\ + \beta_3 \mathbb{1}[\text{Merging Party}]_i \mathbb{1}[\text{Treated}]_d \\ + \beta_4 \mathbb{1}[\text{Non-Merging Party}]_i \mathbb{1}[\text{Treated}]_d + \epsilon_{idt}, \quad (4)$$

where the "Treated" dummy corresponds to a market where the merging parties combine for a market share of at least 2%. The objects of interest in this specification are β_3 and β_4, the merging and non-merging party difference between treated and untreated markets in the difference between realized outcomes and outcomes as predicted by the coefficients in (1). The rationale for this specification is that any uncaptured changes to the post-merger environment will affect both treated and untreated markets and thus can be controlled by looking for differential changes in treated markets beyond what takes place in untreated markets. Dafny et al. (2012) follow this approach to study the price effects of insurance mergers by using the price changes in markets with low predicted changes in concentration as a control.

There are three main drawbacks to applying this strategy in our setting. First, merging parties can lower prices even in untreated markets if the merger creates cost synergies at the national level. These price changes may also lead non-merging parties to respond. Thus, controlling for what happens in untreated markets underestimates the effect of the merger. Second, non-merging parties that engage in regional pricing (Adams and Williams, 2019; DellaVigna and Gentzkow, 2019; Hitsch et al., 2019) may respond to the merger in untreated markets if those markets share a pricing region with treated markets. Again, this leads to an underestimate of the effect of the merger.[9] Despite these concerns, we present results from this specification because they are robust to changes in market conditions that are not captured by our time trend. If these changes played an important role, estimates obtained with this strategy would differ significantly from those obtained using our baseline specification, which would be a cause for concern. This is not the case in the estimates presented below. Third, this strategy does not allow for the identification of merger effects for either national mergers, where all markets are treated, or especially small mergers, where no markets are treated. As a result, we lose 39 out of 126 mergers when using this identification strategy.

There are two canonical approaches to constructing counterfactual post-merger outcomes that we have chosen not to follow. The first is to use changes in the

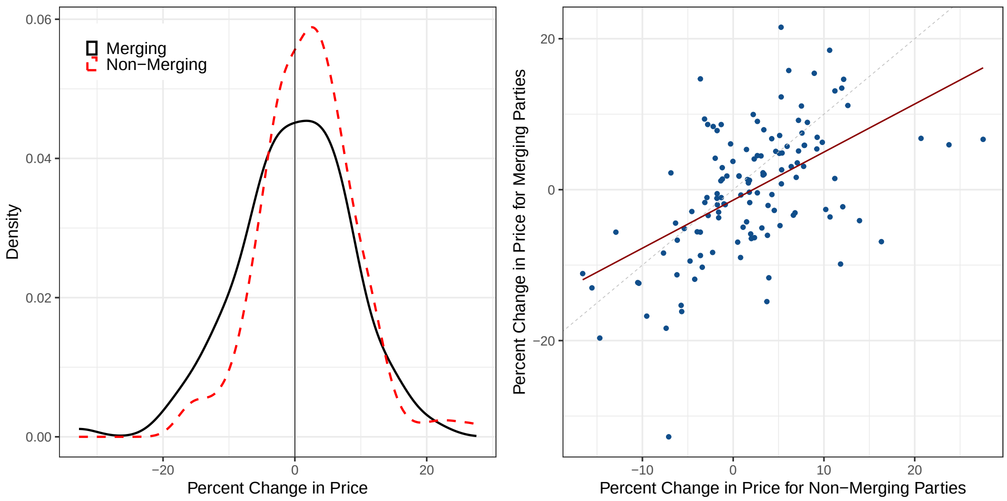
[9] Kim and Mazur (2022) present another concern: mergers may induce changes in prices in untreated markets by affecting the threat of entry in those markets, even if no new entry occurs. This effect is sizable in their setting of airlines, as airport presence presents a direct channel through which entry probabilities in other markets can be affected.

outcome of interest for products of non-merging firms in the same market as a control group. 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 (2011) use prices of non-merging hospitals. The rationale is that these products are likely subject to the same cost and demand shocks as merging parties' products. However, non-merging firms are competitors and may adjust their prices or any other outcome of interest in response to the merger. Because of this concern, we avoid using outcomes for non-merging firms as a control.

A second strategy is to use outcome 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 by using prices of 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 we would not expect strategic responses to the merger in these markets. Thus, any outcome change for the control group is likely due to cost or demand changes. At the same time, the challenge with this strategy is that it requires threading the needle between finding industries that are untreated by the merger yet similar enough to be subject to the same cost and demand shocks. This makes it difficult to find control groups that fit the bill, especially at the scale at which this paper conducts the analysis, which is why we have chosen not to follow this approach.

We weigh all regressions by pre-merger volume at the brand-DMA level. Appendix B shows that if the first-stage model is correctly specified, then under standard conditions this estimate recovers the sales-weighted treatment effect of the merger (on prices or quantities), even in the presence of unmodelled heterogeneity in treatment effects. We believe this to be a quantity of interest, especially when treatment effects are estimated in percentage terms. Nevertheless, we also follow prescriptions in the literature about weighting (Solon et al., 2015) and report results from unweighted regressions in Appendix A.

When aggregating results across mergers, we present unweighted summary statistics in the body of the paper. Vita and Osinski (2018) suggest aggregating merger effects using the precision of the estimate. This method is consistent if one assumes that estimates from different mergers are all noisy estimates of the same



(a) Distribution of price changes

(b) Scatter of price changes

Figure 2: Price changes for merging and non-merging parties, as estimated by (2). These plots display transformed coefficient estimates (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for the price change of the merging and non-merging parties. We use a balanced panel of stores and weigh regressions using pre-merger volume by brand-DMA. The distributions in Panel (a) and best-fit line in Panel (b) assume equal weights across mergers.

parameter (DerSimonian and Laird, 1986). We do not want impose this assumption, but we nevertheless report results with this aggregation in Appendix A.[10]

III.B. Prices

Table 2 presents summary statistics for the distribution of price effects across mergers for all products and separately for products owned by merging and non-merging parties. We transform estimates from (2) to report percentage changes, e.g. $100 \cdot (\exp(\hat{\beta}_1) - 1)$. Panel A displays results for the baseline specification, Panel B reports results using cost shifters and demographics as controls, and Panel C presents estimates using markets without merging party presence as a control group.

[10] DerSimonian and Laird (1986) provide a third method of aggregating across noisy estimates, which they refer to as the "random effects" aggregation, that does not depend on assuming a homogeneous treatment effect across mergers. We have found that the random effects aggregator in our setting is very similar to the uniformly weighted one. Thus, we choose to report the easier-to-interpret uniform weighting procedure and omit the random effects version from the paper.

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	126	1.51	6.33	-2.34	1.65	5.32
		(0.56)		(0.61)	(0.81)	(0.51)
Merging Parties	126	-0.06	8.55	-5.15	0.77	5.86
		(0.76)		(0.98)	(0.99)	(0.78)
Non-Merging Parties	126	2.09	7.17	-2.20	1.93	6.40
		(0.64)		(0.63)	(0.67)	(0.83)
B. Cost and Demographic Controls						
Overall	126	1.58	6.64	-2.34	1.31	5.83
		(0.59)		(0.59)	(0.62)	(0.90)
Merging Parties	126	0.21	9.05	-5.01	0.04	5.62
		(0.81)		(0.99)	(0.84)	(1.27)
Non-Merging Parties	126	2.16	7.38	-2.56	1.77	7.06
		(0.66)		(0.67)	(0.47)	(0.84)
C. Treated/Untreated						
Overall	87	-0.33	3.29	-2.09	0.03	1.40
		(0.35)		(0.70)	(0.33)	(0.38)
Merging Parties	87	-0.39	5.17	-2.51	-0.24	2.41
		(0.55)		(0.40)	(0.48)	(0.56)
Non-Merging Parties	87	-0.21	3.43	-2.19	0.15	1.48
		(0.37)		(0.72)	(0.35)	(0.35)

Table 2: Overall Price Effects. This table displays summary statistics and standard errors for the distribution of transformed coefficient estimates of (2) (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for overall, merging-party, and non-merging-party price changes. In all cases, we use a balanced panel of stores and weigh regressions using pre-merger volume by brand-DMA. We aggregate across mergers using equal weights.

The results from the baseline specification show that mergers have modest price effects: the mean is 1.5%, while the averages for merging and non-merging parties are -0.1% and 2.1%, respectively. However, there is substantial dispersion around these averages. For merging parties, 25% of mergers raise prices by over 5.9%, but also 25% of mergers lower prices by over 5.2%. Note that the 75th percentile of price changes is similar for non-merging parties, but the 25th percentile is much larger. To complete the picture, Panel (a) of Figure 2 presents the distribution of price changes. We find that merging parties are much more likely to lower prices drastically than non-merging parties, while the probability of substantial price increases is more or less similar across the two groups of firms. This discrepancy drives the difference in average price effects across merging and non-merging firms; differences in median price changes are more muted. One potential explanation for this phenomenon is cost synergies that are large enough to induce the merging parties to lower prices.

Panel (b) of Figure 2 depicts the correlation between price changes for merging and non-merging parties. Price changes are positively correlated, consistent with strategic complementarity. For example, non-merging parties lower prices by 7.3%, on average, when merging parties lower their prices by 10% or more, and non-merging parties raise prices by 8.0% on average when merging parties increase their prices by 10% or more. We also find that 28% of mergers lead both merging and nonmerging parties to lower prices for consumers. One potential explanation is that the cost synergies enjoyed by the merging parties are substantial enough to drive their prices down, and their rivals follow. On the other hand, 40% of mergers lead to higher prices from both types of firms. Strategic complementarities in pricing could explain these points as well: the internalization of pricing spillovers induced within the merging parties leads them to increase prices, and rivals find it optimal to follow.

There are several cases where one group of firms increases prices and the other lowers them. In particular, 21% of mergers cause merging parties to lower prices and non-merging parties to raise them, and 11% cause the converse. Changes in the product portfolio can explain this result, as can changes in market segmentation. For example, when merging parties lower prices due to a cost synergy, rivals may find it optimal to concede price-sensitive consumers and focus on those with more inelastic demand for their products.

We next study the timing of these price changes. Figure 3 reports average merging and non-merging party coefficients at the monthly level for a 24-month window around the merger. Panel (a) presents results for mergers in the top 25th percentile of price increases, Panel (b) for those in the bottom 25th percentile, and Panel (c) for the remainder. These results shed light on how quickly merging parties begin to increase prices after the deal, how long it takes their rivals to respond, and how long it takes until cost synergies are passed through to prices. As discussed in the previous subsection, these plots also serve as a check on our identification assumptions. We do not find pre-trends in average prices before merger completion for each of the three categories of price changes.[11]

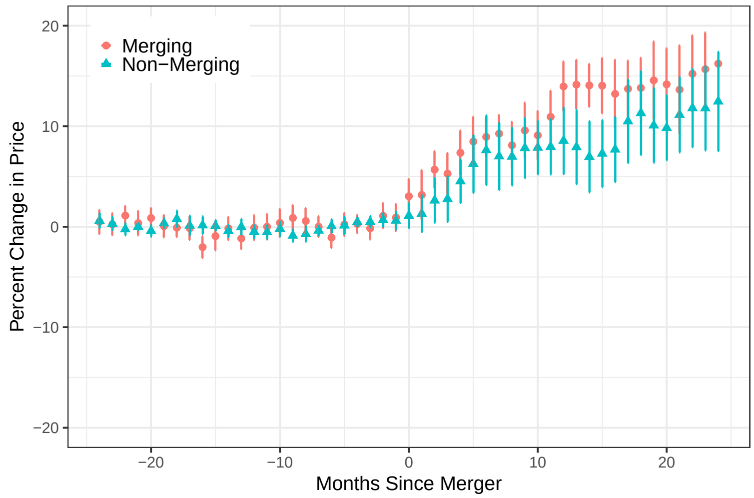
For mergers that led to the largest price increases, we find that merging party prices begin increasing upon completion, are roughly 10% higher five months after the merger, and undergo a further increase approximately a year after completion. Rival prices follow suit, although their price increase is smaller. To the extent that the merged entity takes time to renegotiate contracts with supermarkets, for instance, it stands to reason that it takes some time for it to be able to exert market power.

In the case of the mergers that led to the largest price decreases (Panel (b)), we also find evidence for immediate responses for the merging parties, with a further decline a year after completion. We expect cost synergies to take time to materialize (Focarelli and Panetta, 2003; Whinston, 2007). Indeed, there is evidence for immediate frictions upon merging (González et al., 2022), which may lead to cost increases in the short run. Therefore, it is surprising that prices fall immediately. One potential explanation is price changes in anticipation of the cost decrease, as would be the case under dynamic pricing. Another possible explanation is that some of these mergers lead to lower prices because some products now face a more elastic demand function, as could happen for products whose distribution network shifted significantly. The additional price decrease a year after completion is consistent with cost synergies taking effect.

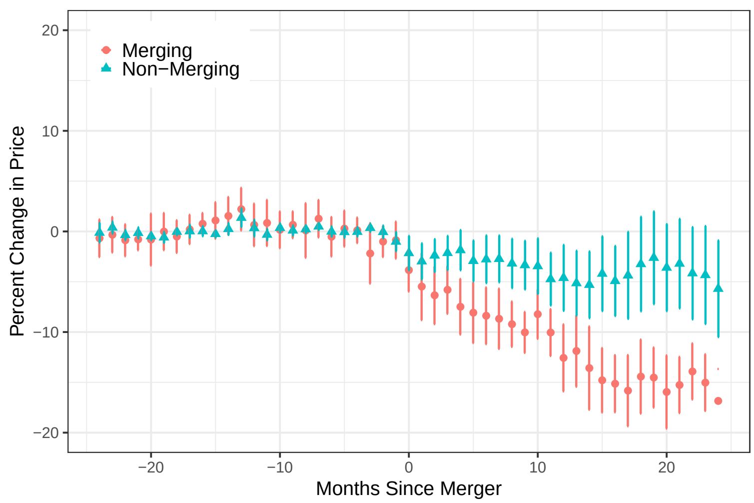
Finally, mergers with price changes between the 25th and the 75th percentile (Panel (c)) exhibit modest price increases for the merging party until a year after completion, followed by a small price decrease. As in the previous panels, this is consistent with cost synergies taking effect roughly a year after completion. At the same time, non-merging parties steadily increase their prices post-merger after holding them constant for roughly two years before the completion date.

Robustness Checks. Panel B in Table 2 presents estimates obtained using cost and demographic control variables (see list in Appendix C.1), while Panel C reports estimates using DMAs where the merging parties do not have a presence as a control group, as in (4). Overall, these results do not offer meaningful departures from

[11]By construction, the average of $\beta_{1,\tau}$ and $\beta_{2,\tau}$ for $\tau \leq 0$ is 0. However, the procedure does not place any mechanical constraints on the pattern in these pre-merger coefficients.

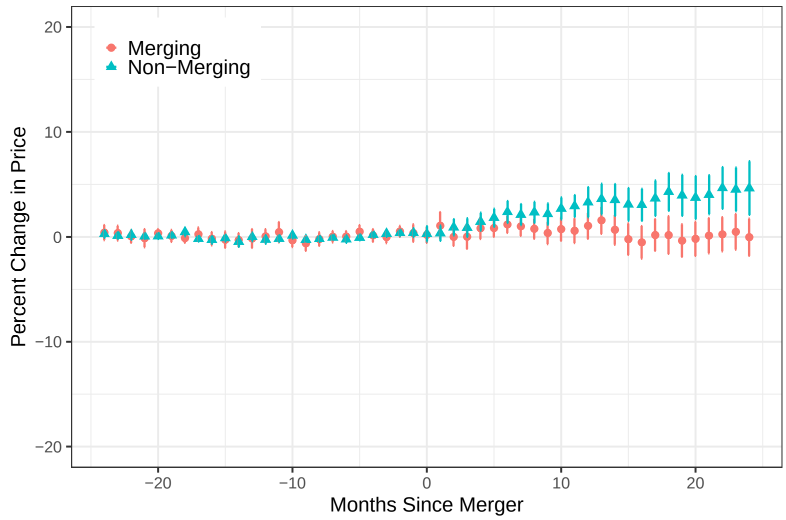


(a) High price changes

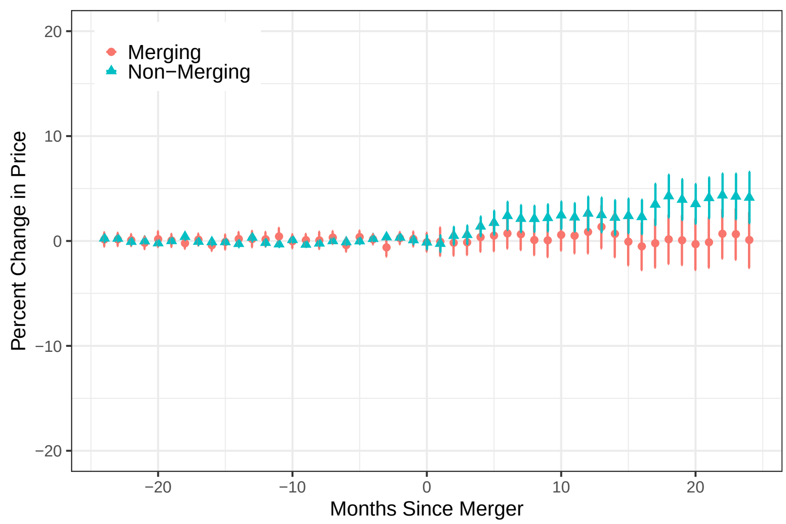


(b) Low price changes

Figure 3: Timing of price changes, for merging parties (red circle) and non-merging parties (blue triangle). The marker indicates the mean price change the given number of months after the merger becomes effective, and the thick line is the 95% confidence interval of that mean. Panels (a)–(c) show 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.)



(c) Stable prices



(d) All price changes

Figure 3: (Continued from last page)

the economic interpretation of the results in Panel A, except for the fact that price effects in Panel C seem to have lower dispersion that in the other specifications. As discussed above, regional pricing strategies would bias the estimates from this specification towards zero.

We also study the sensitivity of our results to the composition of the stores that enter the NielsenIQ Retail Scanner dataset. Since the set of stores is selected and their identity is masked, one may be concerned that they are not a representative sample. Table A.1 in Appendix A presents results obtained by computing price effects using the NielsenIQ Consumer Panel, a random sample of households meant to be representative of 52 major markets. Since households who participate in this dataset are asked to record their purchases regardless of whether the store they are purchasing from is in the NielsenIQ Retail Scanner dataset, this sample includes all the retailers that are excluded from our previous analysis. We do not observe meaningful departures from the economic interpretation of the above results when working with this alternative dataset.

Another concern may be that the stores that remain in the scanner dataset throughout our sample period are selected in some way; Panel C in Table A.1 presents estimates obtained using the entire set of stores in the data rather than the balanced panel. This table also reports results obtained under a specification where observations are weighted uniformly within regression (Panel D), and summary statistics obtained when aggregating merger estimates using the precision of the estimate (Panel E). These robustness checks do not yield any economically meaningful departures from our baseline results.

III.C. Quantities

While most merger retrospectives have focused on prices, another natural question is whether mergers have reduced transacted quantities. Conventional intuition suggests that even if a merger has a small price effect, a significant drop in quantity may indicate adverse welfare effects. Lazarev et al. (2021) formalize this intuition: they argue that under certain conditions, including an assumption that the welfare effect of a merger has the same sign for all customers, 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 in this section should be interpreted as welfare effects, theoretical results like in Lazarev et al. (2021) indicate that documenting quantity effects is of direct interest.

To compute quantity effects, we aggregate to the DMA-month-firm type level, where a firm type is whether that firm is a merging party or not, and use as the

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
Overall	126	-2.31	9.14	-6.91	-1.47	3.05
		(0.81)		(0.80)	(0.73)	(0.64)
Merging Parties	126	-7.57	27.52	-21.46	-6.02	4.75
		(2.45)		(3.64)	(2.09)	(2.38)
Non-Merging Parties	126	-1.17	10.10	-6.17	-1.46	4.29
		(0.90)		(0.75)	(0.93)	(1.08)

Table 3: Quantity Effects. This table displays summary statistics and standard errors for the distribution of transformed coefficient estimates of (2) (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for overall, merging-party, and non-merging-party quantity changes. In all cases, we use a balanced panel of stores and weigh regressions using pre-merger volume by firm type-DMA. We aggregate across mergers using equal weights.

outcome of interest the log of total volume sold. We conduct this aggregation for two reasons. First, we are not interested in whether the merger led to the redistribution of quantities between UPCs of the same firm but whether total sales changed. Second, results like the one in Lazarev et al. (2021) rely on tests of changes in total quantity.

Table 3 and Figure 4 show results from this analysis. We find a drop in quantities of about 2.3% on average across all mergers in the sample. Moreover, 62% of mergers lead to total quantity reductions, suggesting reductions in total welfare under the assumptions of Lazarev et al. (2021). Merging parties exhibit larger quantity drops than non-merging parties, with averages of 7.6% versus 1.2%. The quantiles reported in Table 3 and Figure 4 indicate that distributions of quantity changes tend to be skewed slightly to the left: the median decrease in quantities for merging parties is about 6.0%, for instance. There is also significant variation in quantity effects for merging parties: the standard deviation and inter-quartile range are both around 26–27 pp. The variation is much smaller for non-merging parties.

González et al. (2022) show that mergers can induce supply disruptions, which could lead to reductions in quantity. Since the welfare interpretation of the decline in quantities changes if part of the drop is transitory, in Figure A.1 we study the time path of quantity changes through event study diagrams. We find that quantity effects do not seem to be driven by temporary disruptions, but rather by a permanent change in strategies by the firms.

Are these quantity decreases driven by increases in prices? Figure 5 plots the

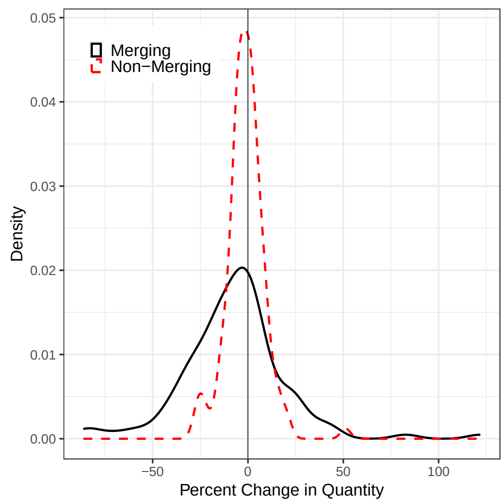
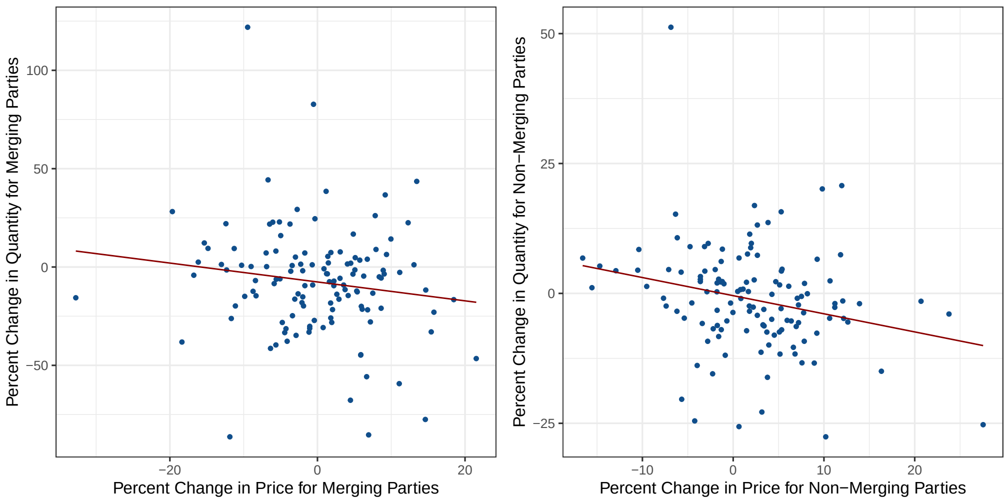


Figure 4: Quantity changes for merging and non-merging parties, as estimated by (2). These plots display transformed coefficient estimates (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for the quantity change of the merging and non-merging parties. We use a balanced panel of stores and weigh regressions using pre-merger volume by firm type-DMA. This distribution is computed with equal weights across mergers.

estimated quantity effects against the estimated price effects for merging parties (Panel (a)) and non-merging parties (Panel (b)). We find that price and quantity changes are indeed negatively correlated. However, the fact that in many mergers average prices and total quantities move in the same direction clearly highlight that average prices do not tell the whole story.

Robustness Checks. Table A.2 compares our baseline estimates to those obtained using cost and demographic controls and to those obtained using untreated markets as a control group. Adding controls does not change estimates in a meaningful way. Using untreated markets as a control group increases the standard deviation of merging party quantity effects, but the economic interpretation of the results is unchanged.

Table A.3 presents results obtained using alternative weighting schemes and changing samples to the NielsenIQ Consumer Panel dataset. The latter results exhibit similar means to the baseline, but the distribution of quantity effects is more disperse. Regressions obtained by equally-weighting firm types and cities shift the upper tail



(a) Merging Parties (b) Non-Merging Parties

Figure 5: Scatter of price versus quantity changes for merging and non-merging parties. Panel (a) displays a scatter plot of price changes versus quantity changes for merging parties. Each blue point represents a merger, the red line is the estimated best fit, assuming equal weights across mergers. Panel (b) presents the same scatter plot, but for non-merging parties. In both panels, we use a balanced panel of stores and weigh price regressions using pre-merger volume by brand-DMA and quantity regressions using pre-merger volume by firm type-DMA.

of the distribution of quantity effects to the right, suggesting that quantity effects either for smaller brands or in smaller DMAs are larger. Finally, summary statistics obtained using inverse-variance weights are indicative of a distribution of quantity effects that is somewhat shifted to the left.

III.D. Other Strategic Responses

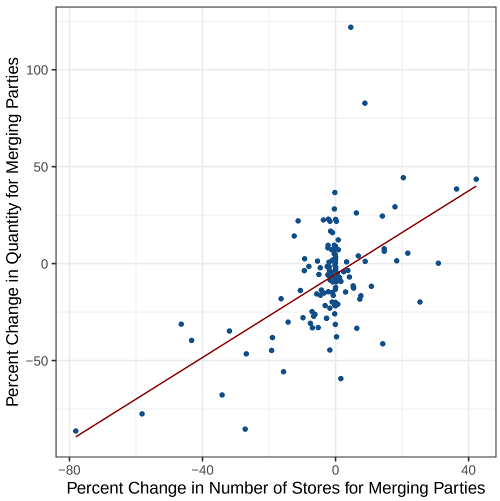
Product assortments and distribution networks are two other levers merging parties and their rivals have at their disposal. Focusing on distribution networks, Panel A in Table 4 displays summary statistics for changes in the number of stores in which at least one product was sold. Non-merging parties make minimal changes to their network of stores. In contrast, mergers lead to a 2.0% reduction in the number of stores served by the merging parties, on average, but there is substantial heterogeneity in these effects.

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Number of Stores						
Overall	126	-0.29	1.97	-0.68	-0.14	0.08
		(0.18)		(0.12)	(0.06)	(0.04)
Merging Parties	126	-1.97	14.73	-4.25	-0.39	1.18
		(1.31)		(1.24)	(0.11)	(0.35)
Non-Merging Parties	126	-0.12	2.23	-0.23	0.00	0.08
		(0.20)		(0.07)	(0.01)	(0.02)
B. Number of Brands (DMA)						
Overall	126	-3.27	8.92	-7.88	-3.32	1.11
		(0.79)		(1.07)	(0.68)	(1.23)
Merging Parties	126	-2.19	22.62	-9.11	-1.81	3.09
		(2.02)		(1.45)	(0.88)	(1.11)
Non-Merging Parties	126	-3.04	9.88	-9.08	-3.13	2.58
		(0.88)		(1.84)	(0.76)	(1.21)
C. Number of Brands (National)						
Overall	126	-3.07	6.78	-6.74	-1.97	0.87
		(0.60)		(0.99)	(0.44)	(0.71)
Merging Parties	126	-4.49	12.84	-9.62	-0.45	0.46
		(1.14)		(2.29)	(0.18)	(0.19)
Non-Merging Parties	126	-2.75	6.77	-7.04	-2.19	0.68
		(0.60)		(1.16)	(0.66)	(0.38)

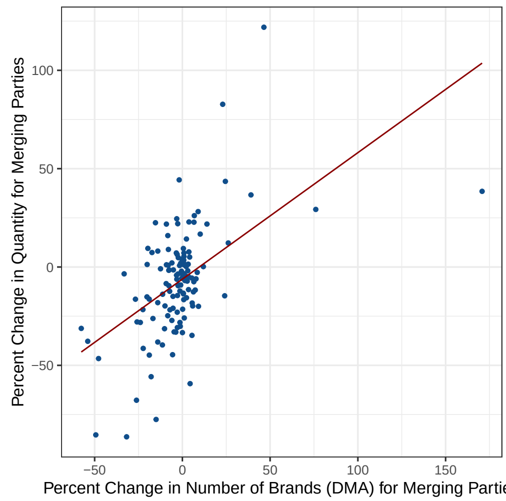
Table 4: Overall Effects on Product Availability. This table displays summary statistics and standard errors for the distributions of product availability outcomes. Number of Stores refers to the number of unique stores in which at least one of the merging (or non-merging) parties' products is sold. Number of Brands refers to the number of unique brands, as defined by NielsenIQ, sold by the merging (or non-merging) parties. In all cases, we use a balanced panel of stores and weigh regressions using pre-merger volume by firm type-DMA. We aggregate across mergers using equal weights.

In 36% of mergers, store networks expand beyond the union of the pre-merger networks. This is consistent with the pro-competitive argument that economies of scale and production reallocation may make it profitable to increase the set of stores where products are offered. Panel (a) in Figure 6 depicts a scatter plot of the change in quantity on the change in the number of stores served. It is in fact the case that large increases in the distribution network correlate with quantity increases.

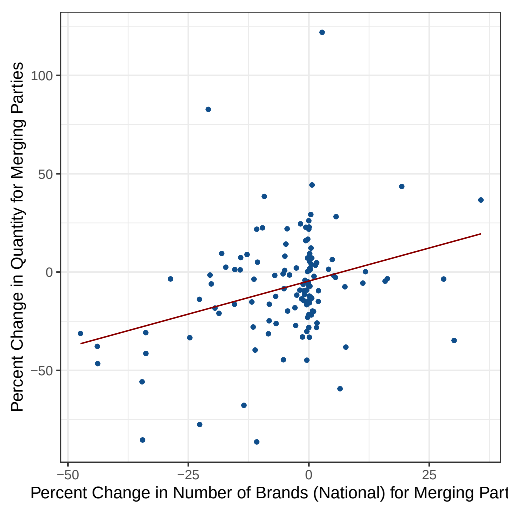
At the same time, many mergers lead to substantial contractions in the distribution network: the 25th percentile of changes to the number of stores is -4.3%. Moreover,



(a) Number of Stores



(b) Number of Brands (DMA)



(c) Number of Brands (National)

Figure 6: Determinants of quantity changes for merging parties. Each panel displays a scatter of merging-party quantity changes against a different outcome. Panel (a) shows quantity against the number of stores, Panel (b) shows quantity against number of brands at the DMA level, and Panel (c) shows quantity against the number of brands (national). Each blue point represents a merger, and the red line is the estimated best fit, assuming equal weights across mergers. For each merger, we use a balanced panel of stores and weigh regressions using pre-merger volume by firm type-DMA.

we find that large declines in quantities sold are correlated with contractions in the store network. We find this result more surprising, as one may expect that the merged entity should have replicated the distribution network of the merging parties if not doing so induces fewer sales. This could be indicative of contracting frictions, such as breakdowns in negotiating new agreements with retailers, restrictions imposed by exclusivity agreements, or costs of supplying certain stores. Consistent with these frictions, we have found that in mergers that lead to bottom-quartile changes in the number of stores served, stores served only by the target pre-merger are more likely to be dropped: 40.5% of stores served only by target brands pre-merger are eliminated from the distribution network post-merger, compared to 26.4% for stores served only by the acquirer, and 12.6% for stores served by both. Thus, mergers of firms with non-overlapping distribution networks often lead to the disappearance of products from shelves and reductions in quantities sold, outcomes that suggest the possibility of consumer harm.

As for product assortment, theory has ambiguous predictions regarding how the merged entity's optimal product portfolio will differ from the combined portfolios of the merging parties. On the one hand, mergers create incentives to remove duplicative products. They also create incentives to remove products that cannibalize sales from more profitable alternatives, even if there are some lost sales. In the limit, it is possible that an acquirer's main goal is to eliminate the target's product line, as in the killer acquisitions literature (Cunningham et al., 2021). In the long run, the incentive to innovate by designing new products changes as well.

Panels B and C in Table 4 report statistics for the changes in the number of brands sold at the DMA level and national level, respectively. We look at each quantity separately because the former allows us to discuss changes in products' geographic footprint, while the latter allows us to address the outright elimination of brands.

In contrast to the findings for the number of stores, both merging and non-merging parties adjust their product portfolios. We find that merging (non-merging) parties decrease the number of brands sold in a DMA by 2.2% (3.0%) on average following a merger. Considering their national portfolios instead, we estimate that merging parties decrease the number of brands sold by 4.5%, while their rivals decrease the number by 2.8%. Panels (b) and (c) in Figure 6 correlate these changes with changes in quantity. We find a positive correlation between changes in the number of brands sold both in each DMA and nationally and changes in quantity.

One rationalization behind eliminating brands after a merger is that some brands are duplicative in the merged entity's portfolio. The fact that we observe quantity declines after brand removal clearly shows this is not the whole story. Instead, some of this brand removal could be due to the desire to eliminate products that cannibalize sales from more profitable alternatives. Turning our attention to brand introductions, we find that in 41% of mergers, the merged entity introduces brands to new DMAs. This result is consistent with the idea that the merged entity can exploit synergies in distribution to expand the geographic footprint of some brands and that this leads to increases in consumption. We also observe that 39% of mergers lead to national brand introductions, but quantity effects in this case are much more muted.

Taking stock, we find that reductions in quantity correlate with price increases, reductions in the number of stores served by the merged entity, and reductions in the number of brands sold in a DMA and nationally. These correlations support the notion that these reductions in quantity are due to strategic responses by the merged entity that are leading to lower consumer surplus. At the same time, it is important to return to Tables 3 and 4 and highlight that many mergers lead to quantity expansions, to the merged entity serving more stores, and to DMAs where consumers face broader variety. An important takeaway from these facts is the heterogeneity in outcomes after a merger. In the next section, we study the interplay between this heterogeneity and the presumptions encoded in the merger guidelines.

IV. Connection to the Merger Guidelines

A striking feature of the previous results is their dispersion. This dispersion highlights the difficulty of the agencies' task of deciding which mergers to scrutinize and challenge. The Horizontal Merger Guidelines codify agency practices in making these assessments. Section 5.3 of the guidelines detail market structures under which the agencies are likely to presume competitive harm from a merger. Mergers that increase HHI by 200 points and lead to a post-merger HHI of more than 2,500 are "presumed to be likely to enhance market power." This region is often called the "red zone" (Nocke and Whinston, 2022).[12] The "yellow zone" includes mergers outside the red zone that increase HHI by more than 100 points and lead to post-merger HHI levels above 1,500. The guidelines note that mergers in this area "raise significant competitive concerns and often warrant scrutiny." Mergers outside this area are in the "green zone" or the "safe harbor" and are "unlikely to have adverse competitive effects." This section investigates the relationship between these structural presumptions and realized price changes. We focus our attention on price changes, in keeping with the emphasis the guidelines and the previous literature have given to this outcome.

It is a ripe time to look back and evaluate the effectiveness of the structural presumptions. On July 2021, President Biden issued an executive order encouraging the review of the horizontal and vertical merger guidelines. In response to this review, various research economists issued public comments about the presumptions.[13] Moreover, the theoretical basis of the agencies' structural presumptions has been a focus of recent work in the academic literature. Theoretical results (Nocke and Schutz, 2018; Nocke and Whinston, 2022) show a relationship between DHHI and the efficiencies required to make a merger neutral to consumer surplus ("compensating efficiencies"). Nocke and Whinston (2022) 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, HHI does not determine a merger's compensating efficiencies. Nevertheless, one may imagine alternate reasons that HHI would play a role in the effects of mergers: for instance, Loertscher and Marx (2021) and Nocke and Whinston (2022) note that competition authorities have historically used HHI as a measure of the potential for coordinated effects. However, they also call this practice into question, arguing that more empirical evidence on HHI screens is needed.

We approach this task through two lenses. First, in Sections IV.A and IV.B, we take a descriptive approach and compute correlations between price changes and structural presumptions. Our goal is to learn about the current state of the world,

[12] See also remarks by Carl Shapiro while Deputy Assistant Attorney General for Economics at the DOJ in 2010, available at https://www.justice.gov/atr/file/518246/download.

[13] See a list compiled by the Yale School of Management at https://som.yale.edu/centers/thurman-arnold-project-at-yale/merger-guidelines, last accessed 2/24/2023.

holding fixed selection into merger proposal and approval. This analysis teaches us how consummated mergers' average price effect changes across market structures given today's enforcement landscape. Second, in Section IV.C, we analyze what our estimates imply for the discussion of whether antitrust scrutiny has been too lax or strict. We begin by noting that marginal price effects, and not average, indicate the stringency of antitrust enforcement. We then estimate a model of agency decision-making that allows us to quantify the expected price effect of the marginal merger. This section aims to provide concrete numbers to inform the current debate.

IV.A. Price Changes and the Structural Presumptions

We begin our analysis at the merger level. To evaluate the correlation between the screens and realized merger effects, we regress average price changes on average DMA-level HHI and DHHI. This procedure delivers the relationship between the screens and each price change given the observed selection into proposal and approval. For us to observe a merger with large values of HHI and DHHI, the merging parties must have thought there was a good argument for approval even though the screens flagged it as problematic. Further, the agencies must have allowed the merger to proceed. Therefore, these correlations are informative of the variation in price effects for mergers with different observables given the current regulatory landscape.

Table 5 regresses average price changes at the merger level on average measures of market structure. Column (1)–(3) use merging parties' price changes as the dependent variable. Column (1) reports that mergers with larger average HHI tend to have lower price changes. We interpret these results as likely capturing selection into merger proposal and approval. As discussed above, the relation between HHI and price changes is zero in some theories or potentially positive in others. However, the data-generating process likely selects high HHI mergers that will not result in drastic price increases; for example, ones with plausible synergies. We find that mergers with larger average changes in HHI have large price changes: a 100-point increase in average DHHI across DMAs is associated with a 0.3 pp larger price increase. While this is expected, the selection channels could still have dampened this estimate. Column (2) uses bins of HHI and DHHI, and the takeaways are similar: price changes are larger when DHHI is especially large, and they tend to the smaller

	Merging			Non-Merging			Aggregate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
HHI (0–1)	-14.77			-10.12			-13.45		
	(5.82)			(7.21)			(4.63)		
DHHI (0–1)	26.40			69.76			48.81		
	(17.81)			(28.08)			(21.00)		
HHI $\in$ [1500, 2500]		0.14			-1.57			-1.40	
		(3.37)			(2.40)			(2.48)	
HHI $>$ 2500		-5.76			-5.52			-5.80	
		(3.25)			(2.29)			(2.33)	
DHHI $\in$ [100, 200]		3.49			2.59			2.93	
		(2.00)			(1.38)			(1.12)	
DHHI $>$ 200		3.92			6.27			5.06	
		(1.75)			(1.94)			(1.47)	
Yellow			1.68			1.30			1.49
			(1.76)			(1.29)			(1.10)
Red			1.19			4.46			2.98
			(1.72)			(1.98)			(1.52)
Constant	4.24	2.66	-0.42	4.31	4.93	1.33	5.08	4.62	0.93
	(1.95)	(3.02)	(0.97)	(2.06)	(2.09)	(0.76)	(1.50)	(2.19)	(0.70)
N	126	126	126	126	126	126	126	126	126

Table 5: Regression of price changes on measures of market structure. We measure HHI and DHHI as the average across all DMAs. Columns (1)–(3) use merging party price changes, Columns (4)–(6) use non-merging party price changes, and Columns (7)–(9) use aggregate price changes. Each observation is a merger. Robust standard errors are in parentheses.

when HHI is especially large. Finally, Column (3) regresses against dummies for the average market structure being in either the yellow or the red region. While point estimates are positive, the magnitudes are smaller, and the results are noisier.

Columns (4)–(6) repeat the exercise with the price changes of non-merging parties, and Columns (7)–(9) do so for aggregate price changes. The point estimates suggest that these price changes are (i) less strongly correlated with average HHI and (ii) more strongly correlated with the red region. However, the differences with the estimates for merging parties are not statistically significant, and the takeaways are broadly similar.

In Appendix A, we explore two robustness checks to this analysis. Using HHI and DHHI computed using nationwide market shares, which are sometimes reported in agency documents, yields similar results (Table A.5). One may also be concerned that price changes for mergers that proceeded with divestitures would be systematically different; we discuss such mergers in more detail in Section IV.C below when connecting the price effects to antitrust enforcement. Dropping these mergers from the analysis (Table A.6) dampens the correlation with DHHI somewhat, but mostly for non-merging parties. Taking stock, we find over a broad range of specifications that mergers with higher average DHHI lead to larger price increases, consistent with the presumption that these mergers are more likely to enhance market power.

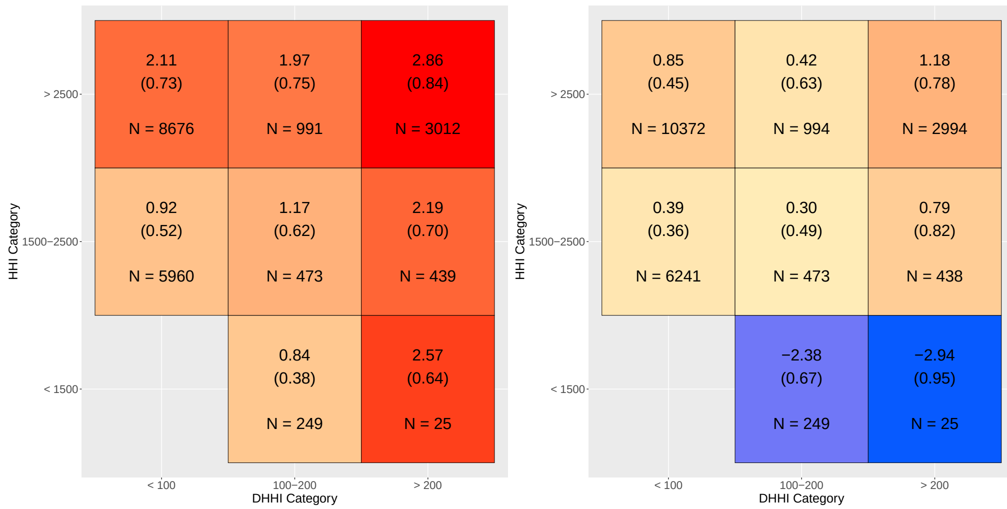
IV.B. Within-Merger Analysis of Price Changes

We next investigate price changes within merger across DMAs. Even with modest average effects, mergers could induce consumer harm in a subset of geographic markets, and the agencies thus also consider heterogeneity in price changes across geography. In some cases (only one in our sample), the agencies even propose geography-specific remedies. Understanding whether the same structural presumptions can guide these decisions is policy-relevant.

Moreover, it is unclear whether the same patterns we identify cross-merger would also hold within-merger. For instance, if firms decide on pricing at a coarser level than the geographic market, as they would under zone pricing, DMA-level market structure may not be correlated with price changes. However, if pricing is local, the appropriate test is whether the DMA-level market structure correlates with price changes. Second, selection into proposal and approval may operate differently at the market level than at the merger level. In particular, if geography-specific remedies are not always feasible, approved mergers that fall in the green or yellow regions at the national level can feature cities where the merger is in the red region. National price effects could mask the worst implications of some mergers.

We estimate price changes at the DMA-merger level as

$$\log y_{idt} - \widehat{\log y_{idt}} = \sum_{\tilde{d}} \beta_{1d} \mathbb{1}[\text{Merging Party}]_i \mathbb{1}[\tilde{d} = d] + \sum_{\tilde{d}} \beta_{2d} \mathbb{1}[\text{Non-Merging Party}]_i \mathbb{1}[\tilde{d} = d] + \epsilon_{idt}. \quad (5)$$



(a) Merging Parties (b) Non-Merging Parties

Figure 7: Within-merger price changes for bins of DMA-level HHI and DHHI. Each bin shows the coefficient of a regression of DMA-level price changes on bin dummies and merging party fixed effects. The omitted bin is the one with low HHI and low DHHI. Standard errors, clustered at the merger level, are in parentheses. N indicates the number of DMA-mergers in each bin.

We then regress the transformed coefficients (i.e., $100 \cdot (\exp(\hat{\beta}_{1d}) - 1)$) on a set of merger fixed effects as well as dummies denoting the region of (HHI, DHHI) plane in which the DMA lies. Figure 7 reports estimates for these market structure dummies. The top right bin represents the red region, the three bins around it together form the yellow region, and all other bins represent the green region. The number and the color in each bin indicate the additional price changes relative to the baseline bin of low HHI and low DHHI.

Panel (a) shows results for merging party prices. We make three comments about these results. First, price changes are positively correlated with DHHI. For each bin of HHI, we can reject the null hypothesis that mergers with DHHI greater than 200 have the same price effect as mergers with DHHI between 100 and 200 with at least 95% confidence. (Table A.7 in Appendix A provides standard errors on all pairwise differences in Figure 7.) This result is consistent with predictions from models of unilateral effects.

Second, price changes are typically correlated with HHI. We find particularly large price increases for high levels of HHI, regardless of the value of DHHI. These findings lend credence to the current guidelines' use of HHI screens, which may be surprising since Nocke and Whinston (2022) find that compensating efficiencies are not a function of the HHI. However, the same authors state that "we do not discount the possibility that, in some circumstances, screening mergers in part based (on) their resulting post-merger level of the HHI may make sense. Yet, at the same time, we view our results as raising the bar for the level of theoretical and empirical support that should back up any such claim" (p. 1944). Our results are a concrete step in providing this empirical support.

Third, the larger number of data points lets us investigate more granular relations with market structure than we could in the cross-merger analysis. We find that some regions in the green zone of antitrust still lead to significant price increases. In particular, mergers with high levels of HHI and low levels of DHHI and mergers with high DHHI and low HHI lead to larger price increases than other mergers in the green zone. The 2010 revision of the Horizontal Merger Guidelines expanded the green region considerably, [14] and these results may call into question this expansion. Additionally, Rose and Shapiro (2022) argue for increased scrutiny of mergers in the yellow region. We find that price increases are exceptionally high for mergers in this region when they have either high values of HHI or of DHHI.

Panel (b) shows results for price changes of non-merging parties. The qualitative relationships with HHI and DHHI described above are also typically consistent with the point estimates in this panel. However, the difference in price changes is more muted and often not statistically significant. Somewhat surprisingly, increases in DHHI for mergers with low HHI are associated with lower price increases. However, note that the result does not indicate that prices decrease on average in this bucket: the mean price change is still positive.

Looking back at the results of these two sections, we find a consistent relationship between DHHI and price changes both across-merger and within-merger. Within-

[14] The 1982 Guidelines defined the green zone as consisting of (i) mergers with DHHI less than 50, (ii) mergers with HHI less than 1000, and (iii) mergers with HHI in 1,000–1,800 and DHHI between 50 and 100.

merger, we also find a positive correlation between price changes and HHI of the geographic market. This is not the case across mergers. The difference between these two results could be due to differences in the selection process. In particular, it may be the case that mergers with high HHI levels in some DMAs are less scrutinized than mergers with high HHI levels on average.

IV.C. How Stringent is US Antitrust Enforcement?

Carlton (2009) points out that small average price changes, like those documented in the previous section, do not necessarily indicate strict antitrust enforcement by the agencies. Consider a world where merger effects are perfectly predictable a priori and agencies can unilaterally decide whether to approve or reject a merger. In that case, the largest observed price effect, not the average, would indicate the maximum price increase the agencies are willing to tolerate. With uncertainty, of course, the largest observed price change could be due to an imprecise forecast rather than lax standards. However, the point remains that one needs to identify the price effects of the marginal merger to discuss how stringent antitrust enforcement is.

In this section, we estimate this level of stringency through the lens of a simple empirical model of the agencies choosing whether to challenge a merger. Conceptually, we model the agencies as choosing to challenge mergers that they believe to be sufficiently anti-competitive—that they expect will lead to significant price increases. Denote by (X_i, Z_i) the observable characteristics of merger i and by p_i^* its true price impact, averaged across geographic markets.[15] Agencies learn about the true price impact through two sources. First, they have a prior on the price impact $F_{p^*}(X_i)$ that could depend on characteristics such as the structural presumptions. Second, through due diligence and review of the documents that parties provide, they also learn a noisy signal p_i of p_i^*. Based on this noisy signal and their price, they form a posterior on p_i^*. They challenge a merger if the expected value of the posterior distribution exceeds a threshold $\bar{p}(X_i, Z_i)$. If $p_i = p_i^*$, this would be exactly the model sketched in Section III of Carlton (2009).[16]

[15] We average price changes across DMAs since only one challenge in our sample features a geography-specific remedy.

[16] Note that this model is an interpretable parameterization of a more general model in which the

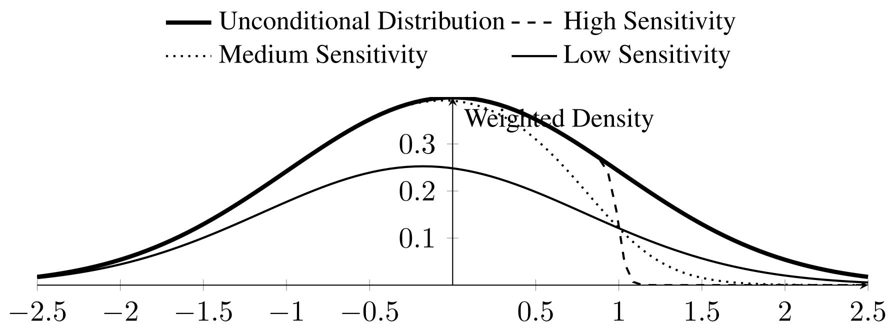


Figure 8: Illustration of the identification of model parameters

As discussed earlier, merger retrospectives can face three levels of selection: selection into proposal, selection into approval, and selection into publication. By studying the universe of mergers in an industry, we have addressed the final selection layer. This model addresses the second layer by directly modeling selection into approval. We aim to draw lessons regarding the strictness of antitrust enforcement by estimating the thresholds for agency challenges.

Our data include whether the agencies challenged a merger. Generally, a challenge could be one of many actions, such as a motion to block the merger or a proposal for a remedy. In our setting, we identify five deals, corresponding to nine mergers, in which an agency proposed a remedy. Additionally, SDC Platinum identifies two deals, corresponding to four mergers, that were proposed and later withdrawn due to antitrust concerns raised by either the DOJ or the FTC. We codify these four blocked mergers and the nine mergers with remedies as being challenged. We also have various merger observables, such as market structure and size, as well as estimates of price changes for unchallenged mergers.[17]

To gain intuition for how the data inform the parameters of this model, suppose that we observe the true price changes for consummated mergers. In addition,

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. Both these sources would be captured in our estimate of the correlation between p_i and p_i^* below.

[17] We also observe estimates of price changes of mergers with a proposed remedy. However, using them in estimation here would require a model for the price change without the remedy.

suppose (as indicated in the above notation) that a merger-specific property Z_i affects the agencies' threshold $\bar{p}(\cdot)$ but not the prior distribution of expected price changes and condition on all other observables. When Z_i is such that the agency does not challenge any merger, we observe the unfiltered distribution of price changes: this identifies F_{p^*}. Now consider increasing the agencies' stringency by manipulating Z_i. Figure 8 plots in bold the unconditional distribution of price changes F_{p^*} and illustrates three possibilities for the distribution of price changes conditional on agency approval. The dashed line depicts a case where all mergers that would have led to large price increases were filtered out, but ones that led to lower price changes were allowed. Here, we would estimate that the agency is trying to prevent mergers with price changes above 1% and that they are successful, as p_i correlates strongly with p_i^*. On the other extreme, the weaker solid distribution shows a case where the distribution of price changes looks like a scaled version of the prior. Here we would conclude that p_i is a very noisy measure of p_i^*. If the probability of challenging a merger is high, we would further conclude that there is a strict threshold. The dotted line illustrates an intermediate case between the two.

We impose parametric forms on the objects of interest to take this model to the data. We assume the prior is normal with mean $X_i'\beta$ and standard deviation σ_{p^*}, and let X_i include measures of market structure such as HHI and DHHI; this is consistent with the agencies' use of structural presumptions in determining whether a merger is likely to cause competitive harm. We also parameterize the threshold as $Z_i'\alpha$, where Z_i includes the log of total sales in the market for merging parties. We make two comments about this choice. First, mergers in which merging parties are larger (in absolute terms) are more likely to draw the agencies' scrutiny but would not change their prior on the price change to expect: simply scaling a market up will change the welfare impact of the merger, which we expect would impact the agencies' decision, but not its price impact. Second, we do not include measures of market structure in the threshold itself. The agencies would be more likely to challenge a merger with high DHHI, for instance, because they have a prior that it would lead to a larger price change, not because they are inherently stricter on such mergers. We assume that $p_i \sim N(p_i^*, \sigma_\epsilon^2)$, where σ_ϵ parameterizes the correlation between the true price change and the agencies' expectation of it. Finally, we do not assume that we observe p_i^*; instead, we assume that $p_i^* \sim N(\hat{p}_i, \sigma_i^2)$, where $\hat{p}_i$ is our estimate of the price change in the data and σ_i is the standard error of this estimate. We estimate the model via maximum likelihood.

Table 6 presents model estimates. Panel A shows estimates of the mean of the prior, using the same parameterizations as in Table 5. Column (1) shows that the unselected price changes[18] increase with DHHI: a 100-point increase in DHHI would correlate with a 0.63 pp larger expected increase in price. We also find a negative relationship between the HHI and price changes, although this effect is small: a 1,000-point increase in post-merger HHI would correspond to a 1.2 pp price decline. Column (2) shows qualitatively similar results using bins of HHI and DHHI. Finally, in Column (3) we use bins that effectively interact HHI and DHHI changes with each other: we allow the mean of the prior distribution to be parameterized by dummies for whether the merger is in the "red" or "yellow" regions. We find a larger mean price change in the red region than in the yellow or the baseline, consistent with the presumption that such mergers are likely anti-competitive.

Comparing the results in Panel A with those in Table 5, we estimate a larger effect of DHHI on the prior than on the realized price changes. For instance, the coefficient on average DHHI in Column (1) of Table 6 is about 33% larger in Column (7) of Table 5. These results are consistent with the model controlling for selection into approval: mergers with high DHHI that were proposed but did not go through likely would have had higher price changes than approved mergers with high DHHI. However, the agencies' actions against those with especially large price changes dampen the realized correlation. Table A.4 shows estimates from a regression of enforcement actions on measures of market structure. Results indicate that enforcement is strongly correlated with DHHI and the red zone in particular, consistent with this argument.

Panel B reports the standard deviation of the prior (σ_{p^*}) as well as the error in the agencies' assessment of the price change (σ_ϵ). First, note that the prior has a large standard deviation: the estimates of σ_{p^*} are consistent with the empirical standard deviation of aggregate price changes reported in Table 2. Compared to the variance

[18]These mergers are unselected in the sense of selection into approval; there is still selection into proposal.

	Aggregate Price Changes			Merging Party Price Changes		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Prior						
HHI (0–1)	-12.16			-15.14		
	(4.46)			(6.71)		
DHHI (0–1)	62.69			42.73		
	(17.57)			(108.51)		
HHI $\in$ [1500, 2500]		0.35			1.53	
		(2.50)			(3.61)	
HHI $>$ 2500		-4.10			-4.35	
		(2.47)			(3.54)	
DHHI $\in$ [100, 200]		3.63			4.16	
		(1.84)			(2.64)	
DHHI $>$ 200		6.40			7.76	
		(1.66)			(2.36)	
Yellow			2.15			2.41
			(1.80)			(2.58)
Red			4.62			5.54
			(1.68)			(2.39)
Constant	5.26	3.58	1.44	5.04	2.06	0.23
	(1.51)	(2.32)	(0.68)	(2.29)	(3.34)	(0.96)
B. Errors and Uncertainty						
σ_{p^*}	6.30	6.20	6.50	9.02	8.81	9.19
	(0.48)	(0.45)	(0.50)	(0.67)	(0.64)	(0.69)
σ_ϵ	3.48	2.87	3.42	2.97	2.75	3.17
	(1.75)	(1.16)	(1.67)	(1.79)	(1.54)	(1.87)
C. Threshold						
Log(Total Merging Sales)	-1.20	-1.12	-1.09	-2.24	-2.19	-2.09
	(0.50)	(0.47)	(0.50)	(0.70)	(0.66)	(0.73)
Constant	10.57	10.90	10.52	14.13	14.14	13.81
	(1.64)	(1.26)	(1.60)	(2.01)	(1.84)	(2.08)
D. Sales-Weighted Thresholds						
Average	8.63	9.08	8.74	10.50	10.59	10.42
	(1.53)	(1.23)	(1.46)	(1.59)	(1.47)	(1.59)
Q1	7.21	7.75	7.45	7.84	7.99	7.93
	(0.48)	(0.43)	(0.45)	(0.53)	(0.51)	(0.52)
Q3	9.47	9.87	9.51	12.07	12.13	11.89
	(0.31)	(0.26)	(0.31)	(0.41)	(0.40)	(0.39)

Table 6: Parameter estimates, using aggregate price changes as the metric of interest in Columns (1)–(3) and merging party price changes in Columns (4)–(6). Standard errors are in parentheses. Log sales are demeaned.

of the prior, the variance of the signal (σ_ϵ^2) is markedly smaller: the precision of the signal relative to the prior (given by $\sigma_\epsilon^{-2}/(\sigma_\epsilon^{-2} + \sigma_{p^*}^{-2})$) is 0.77 (s.e. 0.19) in the specification in Column (1), for instance. Nevertheless, σ_ϵ is still sizable—between 2.9 and 3.5 pp depending on the specification.

We report the parameter estimates of this threshold in Panel C. We find that a 10% increase in the sales of the merging parties leads to a 0.11–0.12 pp decrease in the threshold across specifications, consistent with the intuition that the agencies are more stringent for larger mergers.

Panel D puts these estimates together to summarize the agencies' threshold. We find a sales-weighted average threshold of between 8.6% and 9.1% in our sample: on average, agencies challenge mergers where they expect a price increase larger than this value. The first quartile of the distribution of thresholds across mergers is between 7.2% and 7.8%. In contrast, the third quartile (i.e., for the smaller mergers in our dataset) amounts to between 9.5% and 9.9%. Columns (4)–(6) use the price changes of the merging parties, rather than aggregate price changes, as the variable of interest to the agencies. We find similar but somewhat larger thresholds in these specifications—around 10.5%—albeit with more variation across mergers.

An interpretation of this result is that the marginal merger would have a price effect in the range of 8–9% overall. 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)), which would be within the confidence interval of our estimate.

IV.D. Adjusting Antitrust Stringency

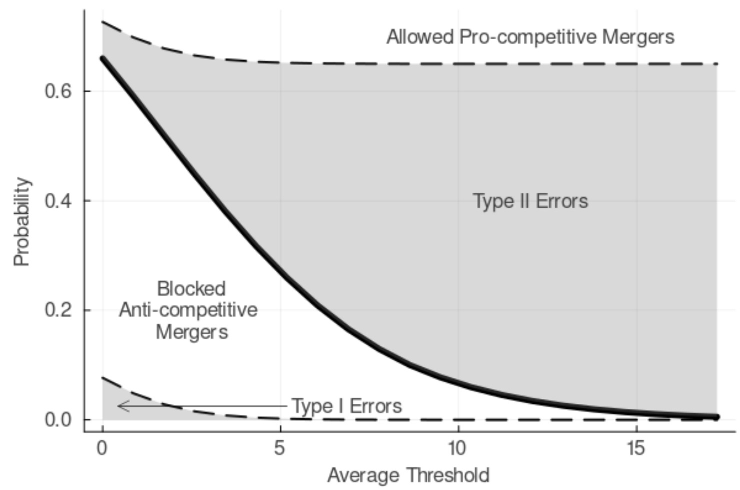
Given the estimated threshold in Section IV.C, is antitrust scrutiny excessively lax? We do not take a stance on this question since such an analysis would require a full welfare calculation and a deeper understanding of the agencies' budget constraints, the costs of challenging mergers, and the likelihood that challenges would hold up in court. However, we can use the model to inform the elements that would go into the cost-benefit calculation for adjusting antitrust scrutiny. In this section, we consider scaling the thresholds by a common factor, e.g., all thresholds become 10% smaller. For each counterfactual threshold, we can compute the probability of challenging a merger in our sample.[19] We can also compute the distribution of price effects for allowed (and blocked) mergers.

Panel (a) of Figure 9 plots the probability of challenging a merger against counterfactual thresholds in solid black, using the baseline estimates in Column (1) of Table 6. Moving to a threshold of 5% compared to the current average of 8.6% would increase challenges almost three times. Reducing the threshold to 0% would lead the agencies to challenge over 60% of mergers. These observations align with the distributions presented in Table 2: for instance, over half of the mergers in our sample have a positive aggregate price impact. The main takeaway of this line is to highlight the additional burden to the agencies from tightening stringency.

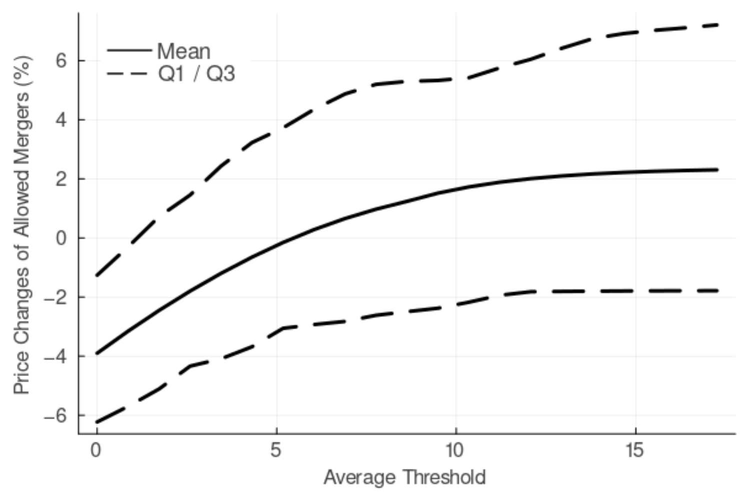
Which mergers would get screened out from a change in the threshold? Panel (b) provides one answer to this question by plotting the mean price change of consummated mergers and the first and third quartiles of the price change distribution for different threshold levels. Tightening the threshold to 5% would lead to an aggregate price change of about 0% for consummated mergers, compared to about 1.5% in the current regime. Moving to a 0% threshold would lead to over 75% of consummated mergers causing price decreases. The cost of loosening the threshold seems somewhat more limited: average price changes level off to about 2% even if the threshold doubles, although we see increases in the third quartile of the price change distribution. At these thresholds, challenge probabilities are so low that we recover the unconditional distribution of price changes for proposed mergers. One caveat is that we assume selection into merger proposal does not change even as the threshold changes. Thus, laxer thresholds may induce the proposal of worse mergers. If this is the case, our estimated price effects are lower bounds. Conversely, stronger thresholds may dissuade some of the observed mergers from being proposed, in which case our estimated increase in administrative burden is an upper bound.

Another way to tackle this question is to document mistakes the agencies may

[19] We conduct the exercise in-sample. That is, we compute counterfactual outcomes for merger i not just conditional on X_i and Z_i but also conditioning on distributions of unobservables (i.e., the true price change p_i^* and the agencies' estimate p_i) that would be consistent with the decision in the data as well as our estimate of the price effect.



(a) Probability of challenges



(b) Price changes of consummated mergers

Figure 9: Outcomes of counterfactual thresholds. Panel (a) shows the probability of blocking a merger (solid black) along with probabilities of type I and type II errors. Panel (b) shows price changes of consummated mergers. Figure A.2 shows confidence intervals.

make under different thresholds. A blocked merger could have been anti-competitive (leading to a price increase) or pro-competitive (leading to a price decrease). The latter situation is called a "type I error" (Kwoka, 2016). Tightening the threshold must lead to more type I errors since the agencies only operate based on a prediction of the price effect; the relevant question is by how much. Panel (a) shows that type I errors are infrequent at the current threshold. Recall that agencies block pro-competitive mergers if their signal exceeds the threshold and that, by definition, pro-competitive mergers have negative price effects. Therefore, with an 8–9% threshold, only very adverse signals can induce the agencies to block these mergers. Given our estimated variance of the signal, this event is unlikely. Type I errors only become non-trivial starting at a threshold of around 5%. At a threshold of 0%, 12% of blocked mergers are type I errors. The opposite mistake—allowing an anti-competitive merger—is called a "type II error." Panel (a) also splits the region where mergers are allowed (above the solid line) into type II errors and situations where pro-competitive mergers are allowed. At the current threshold, about three-fifths of allowed mergers are due to Type II errors. The ratio becomes about one-half at a threshold of 5% and one-fifth at 0%.

Our estimates indicate that modest increases in antitrust stringency would reduce prices and the prevalence of type II errors while having minimal impacts on type I errors. However, they may come with a significant additional burden on the antitrust agencies unless the increased stringency leads to drastically fewer proposed mergers. An important caveat of this analysis is that we are solely focusing on price effects. Perhaps other margins of response, such as product assortments or distribution networks, can lead to different welfare implications. Nevertheless, these findings provide relevant data points regarding the current debate on antitrust stringency and the future of enforcement.

V. Conclusion

This paper has two main contributions. First, we document how a comprehensive set of mergers in US retail have affected prices, quantities, and other equilibrium outcomes of interest. Our most striking result is the variance in observed outcomes for mergers in this industry. For example, we estimate that 25% of the mergers have lowered prices by more than 2.3%, and another 25% have raised them by more than 5.3%. Second, through a model of agency decisions, we investigate the stringency of antitrust enforcement. We find that the current levels of antitrust enforcement are such that the probability of blocking a pro-competitive merger is very low, while the probability of allowing anti-competitive mergers is substantial. However, tightening standards would lead to a drastically higher burden on the agencies.

The first contribution is a description of the current state of the world, depicting what mergers have done in this industry in the last 15 years. The second sheds light on what alternative regulatory regimes would do. Both contributions are important additions to the current debate on antitrust standards.

Several avenues for future work stem from these results. First, recall that we study the effects of mergers of manufacturers on prices paid by consumers at the supermarket. An interesting question is whether these mergers affect the split of surplus between manufacturers and retailers. We cannot answer it, as we do not observe the contracts between these parties. As part of our selection process, we have encountered many deals without product market overlap. This question may be connected to the prevalence of such deals, as they may alter the bargaining positions of manufacturers. Second, we document that the merged entity often drops stores from its distribution network post-merger. The decision of which stores to serve in a given geographic market, and its interaction with market power, seems like a promising avenue for future research. Finally, we argue that making antitrust enforcement more strict can reduce the prevalence of approved anti-competitive mergers without increasing the likelihood of blocked pro-competitive mergers. However, it would severely increase the administrative burden faced by the agencies. We are not aware of a quantification of how much it would cost to expand the agencies' capacity to challenge more cases, a central input into determining whether stricter antitrust enforcement is worthwhile.

References

ADAMS, B. AND K. R. WILLIAMS (2019): "Zone Pricing in Retail Oligopoly," *American Economic Journal: Microeconomics*, 11, 124–56.

AFFELDT, P., T. DUSO, K. GUGLER, AND J. PIECHUCKA (2021a): "Assessing EU Merger Control through Compensating Efficiencies," Tech. rep., DIW Berlin.

AFFELDT, P., T. DUSO, AND F. SZÜCS (2021b): "25 Years of European Merger control," *International Journal of Industrial Organization*, 76, 102720.

ASHENFELTER, O. C. AND D. S. HOSKEN (2010): "The Effect of Mergers on Consumer

Prices: Evidence from Five Mergers on the Enforcement Margin," *Journal of Law and Economics*, 53, 417–466.

ASHENFELTER, O. C., D. S. HOSKEN, AND M. C. WEINBERG (2013): "The Price Effects of a Large Merger of Manufacturers: A Case Study of Maytag-Whirlpool," *American Economic Journal: Economic Policy*, 5, 239–61.

——— (2014): "Did Robert Bork Understate the Competitive Impact of Mergers? Evidence from Consummated Mergers," *Journal of Law and Economics*, 57, S67–S100.

——— (2015): "Efficiencies Brewed: Pricing and Consolidation in the US Beer Industry," *RAND Journal of Economics*, 46, 328–361.

ASKER, J. AND V. NOCKE (2021): "Collusion, Mergers, and Related Antitrust Issues," in *Handbook of Industrial Organization*, Elsevier, vol. 5, 177–279.

ATALAY, E., E. FROST, A. SORENSEN, C. SULLIVAN, AND W. ZHU (2023): "Scalable Demand and Markups," Tech. rep., University of Wisconsin.

ATALAY, E., A. SORENSEN, C. SULLIVAN, AND W. ZHU (2022): "Post-Merger Product Repositioning: An Empirical Analysis," Tech. rep., University of Wisconsin.

BENSON, D., S. BLATTNER, S. GRUNDL, Y. S. KIM, AND K. ONISHI (2022): "Concentration and Geographic Proximity in Antitrust Policy: Evidence from Bank Mergers," Tech. rep., Federal Reserve.

BERGMAN, M. A., M. JAKOBSSON, AND C. RAZO (2005): "An Econometric Analysis of the European Commission's Merger Decisions," *International Journal of Industrial Organization*, 23, 717–37.

BRAND, J. (2021): "Differences in Differentiation: Rising Variety and Markups in Retail Food Stores," Tech. rep., Microsoft Research.

BRAND, K., C. GARMON, AND T. ROSENBAUM (2022): "In the Shadow of Antitrust Enforcement: Price Effects of Hospital Mergers from 2009–2016," Tech. rep., Federal Trade Commission.

CARLTON, D. (2009): "The Need to Measure the Effect of Merger Policy and How to Do It," *Competition Policy International*, 5, Article 6.

CHEN, V., C. GARMON, K. RIOS, AND D. SCHMIDT (2022): "The Competitive Efficacy of Divestitures: An Empirical Analysis of Generic Drug Markets," Tech. rep., Federal Trade Commission.

CUNNINGHAM, C., F. EDERER, AND S. MA (2021): "Killer Acquisitions," *Journal of Political Economy*, 129, 649–702.

DAFNY, L., M. DUGGAN, AND S. RAMANARAYANAN (2012): "Paying a Premium on Your Premium? Consolidation in the US Health Insurance Industry," *American Economic Review*, 102, 1161–85.

DAS, S. (2019): "Effect of Merger on Market Price and Product Quality: American and US Airways," *Review of Industrial Organization*, 55, 339–374.

DE LOECKER, J., J. EECKHOUT, AND G. UNGER (2020): "The Rise of Market Power and the Macroeconomic Implications," *Quarterly Journal of Economics*, 135, 561–644.

DELLAVIGNA, S. AND M. GENTZKOW (2019): "Uniform Pricing in US Retail Chains," *Quarterly Journal of Economics*, 134, 2011–2084.

DEMIRER, M. AND O. KARADUMAN (2023): “Do Mergers and Acquisitions Improve Efficiency: Evidence from Power Plants,” Tech. rep., MIT Sloan.

DERSIMONIAN, R. AND N. LAIRD (1986): “Meta-Analysis in Clinical Trials,” *Controlled Clinical Trials*, 7, 177–188.

DÖPPER, H., A. MACKAY, N. H. MILLER, AND J. STIEBALE (2022): “Rising Markups and the Role of Consumer Preferences,” Tech. rep., HBS.

FAN, Y. (2013): “Ownership Consolidation and Product Characteristics: A Study of the US Daily Newspaper Market,” *American Economic Review*, 103, 1598–1628.

FARRELL, J., P. A. PAUTLER, AND M. G. VITA (2009): “Economics at the FTC: Retrospective Merger Analysis with a Focus on Hospitals,” *Review of Industrial Organization*, 35, 369.

FENG, J., T. HWANG, Y. LIU, AND L. MAINI (2023): “Mergers that Matter: The Impact of M&A Activity in Prescription Drug Markets,” Tech. rep., University of Utah.

FOCARELLI, D. AND F. PANETTA (2003): “Are Mergers Beneficial to Consumers? Evidence from the Market for Bank Deposits,” *American Economic Review*, 93, 1152–1172.

GARMON, C. (2017): “The Accuracy of Hospital Merger Screening Methods,” *RAND Journal of Economics*, 48, 1068–1102.

GONZÁLEZ, J., J. LEMUS, AND G. MARSHALL (2022): “Mergers and Organizational Disruption: Evidence from the US Airline Industry,” Tech. rep., University of Illinois.

GRIECO, P., C. MURRY, AND A. YURUKOGLU (2022a): “The Evolution of Market Power in the US Auto Industry,” Tech. rep., Pennsylvania State University.

GRIECO, P. L., C. MURRY, J. PINKSE, AND S. SAGL (2022b): “Conformant and Efficient Estimation of Discrete Choice Demand Models,” Tech. rep., Pennsylvania State University.

HAAS-WILSON, D. AND C. GARMON (2011): “Hospital Mergers and Competitive Effects: Two Retrospective Analyses,” *International Journal of the Economics of Business*, 18, 17–32.

HITSCH, G. J., A. HORTAÇSU, AND X. LIN (2019): “Prices and Promotions in U.S. Retail Markets: Evidence from Big Data,” Working Paper 26306, National Bureau of Economic Research.

HUNTER, G., G. K. LEONARD, AND G. S. OLLEY (2008): “Merger Retrospective Studies: A Review,” *Antitrust*, 23, 34.

KIM, E. H. AND V. SINGAL (1993): “Mergers and Market Power: Evidence from the Airline Industry,” *American Economic Review*, 549–569.

KIM, M. AND J. MAZUR (2022): “The Dual Effects of Mergers on Peripheral Markets: Evidence from the U.S. Airline Industry,” Tech. rep., Purdue University.

KWOKA, J. (2014): *Mergers, Merger Control, and Remedies: A Retrospective Analysis of US Policy*, MIT Press.

——— (2016): “The Structural Presumption and the Safe Harbor in Merger Review: False Positives or Unwarranted Concerns,” *Antitrust Law Journal*, 81, 837.

KWOKA, J. AND E. SHUMILKINA (2010): “The Price Effect of Eliminating Potential Competition: Evidence from an Airline Merger,” *Journal of Industrial Economics*, 58, 767–793.

LAGOS, V. (2018): "Effectiveness of Merger Remedies: Evidence from the Retail Gasoline Industry," *Journal of Industrial Economics*, 66, 942–979.

LAZAREV, J., A. NEVO, AND B. TOWN (2021): "What Can We Learn from Merger Retrospectives? Lessons from the Airline Industry," Tech. rep., University of Pennsylvania.

LOERTSCHER, S. AND L. M. MARX (2021): "Coordinated Effects in Merger Review," *Journal of Law and Economics*, 64, 705–744.

LUO, D. (2014): "The Price Effects of the Delta/Northwest Airline Merger," *Review of Industrial Organization*, 44, 27–48.

MAJEROVITZ, J. AND A. YU (2021): "Consolidation on Aisle Five: Effects of Mergers in Consumer Packaged Goods," Tech. rep., MIT.

MILLER, N. H. AND M. C. WEINBERG (2017): "Understanding the Price Effects of the MillerCoors Joint Venture," *Econometrica*, 85, 1763–1791.

NOCKE, V. AND N. SCHUTZ (2018): "Multiproduct-Firm Oligopoly: An Aggregative Games Approach," *Econometrica*, 86, 523–557.

NOCKE, V. AND M. D. WHINSTON (2022): "Concentration Thresholds for Horizontal Mergers," *American Economic Review*, 112, 1915–1948.

PETERS, C. (2006): "Evaluating the Performance of Merger Simulation: Evidence from the US Airline Industry," *Journal of Law and Economics*, 49, 627–649.

ROSE, N. AND C. SHAPIRO (2022): "What Next for the Horizontal Merger Guidelines," *Antitrust*, 36.

SCOTT MORTON, F. (2019): "Modern U.S. Antitrust Theory and Evidence Amid Rising Concerns of Market Power and its Effects: An Overview of Recent Academic Literature," Tech. rep., Center for Equitable Growth.

SHAPIRO, B. T., G. J. HITSCH, AND A. E. TUCHMAN (2021): "TV Advertising Effectiveness and Profitability: Generalizable Results from 288 Brands," *Econometrica*, 89, 1855–1879.

SHAPIRO, C. (2021): "Antitrust: What Went Wrong and How to Fix It," *Antitrust*, 35, 33–45.

SIMPSON, J. AND C. TAYLOR (2008): "Do Gasoline Mergers Affect Consumer Prices? The Marathon Ashland petroleum and Ultramar Diamond Shamrock Transaction," *The Journal of Law and Economics*, 51, 135–152.

SOLON, G., S. J. HAIDER, AND J. WOOLDRIDGE (2015): "What Are We Weighting For?" *Journal of Human Resources*, 50.

SWEETING, A. (2010): "The Effects of Mergers on Product Positioning: Evidence from the Music Radio Industry," *RAND Journal of Economics*, 41, 372–397.

VITA, M. AND F. D. OSINSKI (2018): "John Kwoka's Mergers, Merger Control, and Remedies: A Critical Review," *Antitrust Law Journal*, 82, 361–88.

WEINBERG, M. C. AND D. HOSKEN (2013): "Evidence on the Accuracy of Merger Simulations," *Review of Economics and Statistics*, 95, 1584–1600.

WHINSTON, M. D. (2007): "Antitrust Policy Toward Horizontal Mergers," in *Handbook of Industrial Organization*, ed. by M. Armstrong and R. Porter, Elsevier, vol. 3, 2369–2440.

WILLIAMSON, O. E. (1968): "Economies as an Antitrust Defense: The Welfare Tradeoffs," *American Economic Review*, 58, 18–36.

A. Additional Tables and Figures

This appendix includes additional tables and figures. We first begin with a description of the exhibits in this section, and the exhibits follow.

The first set of exhibits provides additional results related to the analysis of price and quantity effects in Section III.

- Table A.1 performs additional robustness tests on the distribution of estimated price effects.
  - Panel A reproduces the estimates from the baseline specification in Panel A of Table 5 for convenience.
  - Panel B reports results obtained using the Nielsen Consumer Panel dataset instead of the Retail Scanner dataset. The sample of Nielsen panelists is constructed to be representative of a coarser market definition called a Scantrack market, rather than a DMA. Thus, these regressions are done at the UPC-Scantrack-month level, and we aggregate to this level using the projection weights provided by Nielsen.
  - Panel C reports results obtained using all stores in the Retail Scanner dataset, not just those that are in the sample throughout the entirety of the merger's analysis window.
  - Panel D reports results from regressions where observations are weighted equally rather than using the pre-merger volume.
  - Panel E report results obtained aggregating the baseline specification across mergers using the inverse variance of the second-stage estimate.
- Tables A.2 replicates all panels of Table 2 in the body, but for quantity effects. In particular, it adds results involving cost and demographic controls (Panel B) as well as untreated markets as a control (Panel C).
- Figure A.1 shows the path of quantity changes over time through event study diagrams, separately for mergers in the top and bottom quartiles of quantity changes, for remaining mergers, and for all mergers. This is the analogue of Figure 3 in the body, but for quantities.

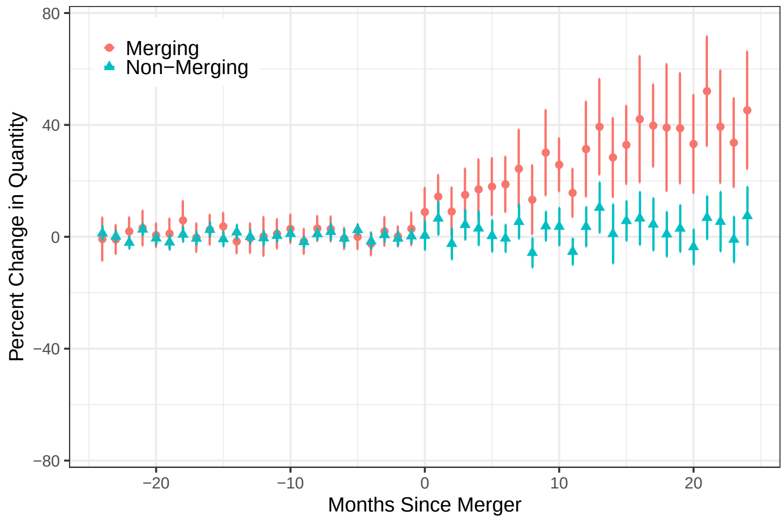
- Table A.3 provides the same set of robustness checks for the quantity results that Table A.1 does for the price results. The only one we omit is the unbalanced panel of stores: given quantity is aggregated across stores, quantity effects from an unbalanced panel would be hard to interpret.

The remaining tables and figures pertain to Section IV.

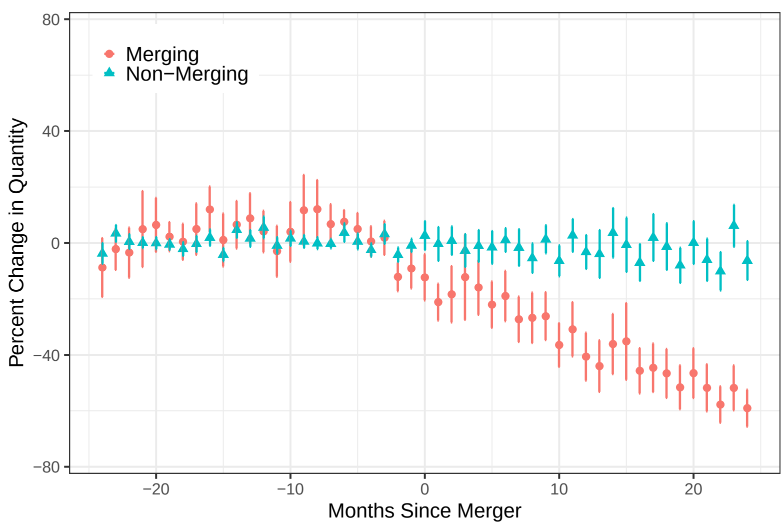
- Table A.4 reports regressions of enforcement on merger-level market structure. This table corroborates that enforcement correlates with DHHI and the red region of the merger guidelines.
- Table A.5 replicates Table 5 but uses nationwide HHI and DHHI as the metrics for market structure, rather than the average of DMA-level HHI and DHHI.
- Table A.6 replicates Table 5 but drops mergers in which the parties had to divest at least one brand.
- Table A.7 presents standard errors on all pairwise differences in Figure 7.
- Figure A.2 replicates Figure 7, but adds confidence regions.

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	126	1.51	6.33	-2.34	1.65	5.32
		(0.56)		(0.61)	(0.81)	(0.51)
Merging Parties	126	-0.06	8.55	-5.15	0.77	5.86
		(0.76)		(0.98)	(0.99)	(0.78)
Non-Merging Parties	126	2.09	7.17	-2.20	1.93	6.40
		(0.64)		(0.63)	(0.67)	(0.83)
B. Panelist Data						
Overall	126	-0.76	6.01	-3.61	-0.63	2.75
		(0.54)		(0.50)	(0.48)	(0.85)
Merging Parties	126	-1.41	8.08	-5.11	-1.10	3.45
		(0.72)		(0.79)	(0.80)	(0.86)
Non-Merging Parties	126	-0.40	6.54	-3.74	-0.23	3.74
		(0.58)		(0.58)	(0.51)	(0.82)
C. Unbalanced Panel of Stores						
Overall	126	-0.76	6.01	-3.61	-0.63	2.75
		(0.54)		(0.50)	(0.48)	(0.85)
Merging Parties	126	-1.41	8.08	-5.11	-1.10	3.45
		(0.72)		(0.79)	(0.80)	(0.86)
Non-Merging Parties	126	-0.40	6.54	-3.74	-0.23	3.74
		(0.58)		(0.58)	(0.51)	(0.82)
D. Equally-Weighted Regressions						
Overall	126	0.43	5.35	-2.44	0.89	4.12
		(0.48)		(0.32)	(0.70)	(0.68)
Merging Parties	126	-0.15	7.59	-4.43	0.08	4.48
		(0.68)		(0.94)	(0.97)	(0.83)
Non-Merging Parties	126	0.75	5.61	-2.46	1.16	4.25
		(0.50)		(0.30)	(0.60)	(0.65)
E. Inverse-Variance Weighting Across Mergers						
Overall	126	1.80	5.66	-1.30	3.18	5.02
		(0.51)		(0.07)	(0.14)	(0.12)
Merging Parties	126	-0.30	6.69	-3.43	-1.05	4.51
		(0.60)		(0.15)	(0.17)	(0.14)
Non-Merging Parties	126	2.13	6.65	-1.77	2.66	5.34
		(0.59)		(0.12)	(0.12)	(0.11)

Table A.1: Robustness of Price Effects. This table displays summary statistics and standard errors for the distribution of transformed coefficient estimates of (2) (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for overall, merging-party, and non-merging-party price changes. Panel A displays the baseline results from the main text, Panel B displays results using the NielsenIQ Consumer Panel Data, Panel C displays results using an unbalanced panel of stores, Panel D displays results assuming equal weights across UPC/DMAs when estimating (1) and (2), and Panel E displays results when merger-level coefficient estimates are weighed by the inverse variance of the transformed second-stage estimate.

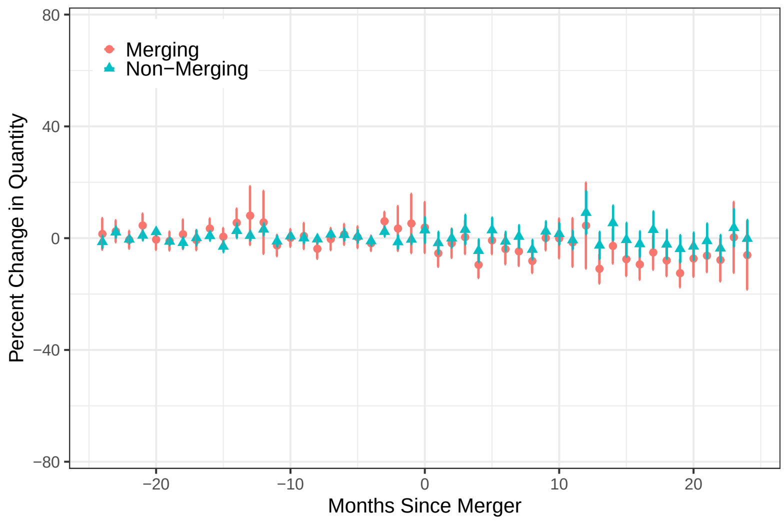


(a) High quantity changes

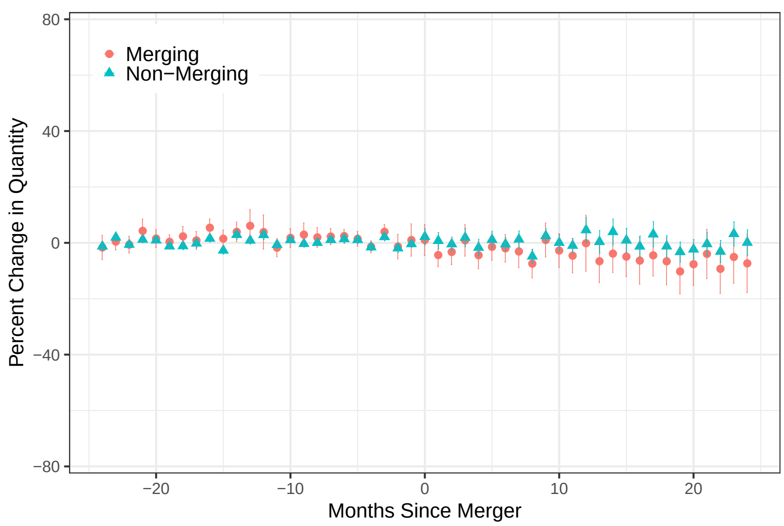


(b) Low quantity changes

Figure A.1: Timing of quantity changes, for merging parties (red circle) and non-merging parties (blue triangle). The marker indicates the mean quantity change the given number of months after the merger becomes effective, and the thick line is the 95% confidence interval of that mean. Panels (a)–(c) shows subsamples: Panel (a) restricts to mergers with quantity 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.)



(c) Stable quantities



(d) All quantity changes

Figure A.1: (Continued from last page)

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	126	-2.31	9.14	-6.91	-1.47	3.05
		(0.81)		(0.80)	(0.73)	(0.64)
Merging Parties	126	-7.57	27.52	-21.46	-6.02	4.75
		(2.45)		(3.64)	(2.09)	(2.38)
Non-Merging Parties	126	-1.17	10.10	-6.17	-1.46	4.29
		(0.90)		(0.75)	(0.93)	(1.08)
B. Cost and Demographic Controls						
Overall	126	-2.30	9.20	-6.91	-1.49	3.21
		(0.82)		(0.80)	(0.76)	(0.70)
Merging Parties	126	-7.55	27.53	-21.52	-6.21	5.00
		(2.45)		(3.07)	(2.35)	(2.26)
Non-Merging Parties	126	-1.16	10.14	-6.21	-1.39	4.31
		(0.90)		(0.79)	(0.97)	(1.01)
C. Treated/Untreated						
Overall	87	-1.38	13.49	-5.34	-1.12	2.82
		(1.45)		(1.08)	(0.71)	(0.77)
Merging Parties	87	-5.28	40.56	-23.06	-8.11	9.32
		(4.35)		(4.18)	(3.06)	(5.80)
Non-Merging Parties	87	-0.43	12.78	-3.70	0.34	3.43
		(1.37)		(0.77)	(0.58)	(0.91)

Table A.2: Quantity Effects with Controls. This table displays summary statistics and standard errors for the distribution of transformed coefficient estimates of (2) (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for overall, merging-party, and non-merging-party quantity changes. In all cases, we use a balanced panel of stores and weigh regressions using pre-merger volume by firm type-DMA. We aggregate across mergers using equal weights.

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	126	-2.31	9.14	-6.91	-1.47	3.05
		(0.81)		(0.80)	(0.73)	(0.64)
Merging Parties	126	-7.57	27.52	-21.46	-6.02	4.75
		(2.45)		(3.64)	(2.09)	(2.38)
Non-Merging Parties	126	-1.17	10.10	-6.17	-1.46	4.29
		(0.90)		(0.75)	(0.93)	(1.08)
B. Panelist Data						
Overall	126	-3.17	15.05	-11.53	-1.65	5.31
		(1.34)		(2.38)	(1.20)	(1.55)
Merging Parties	126	-6.19	39.64	-27.48	-10.97	10.23
		(3.53)		(3.26)	(3.11)	(4.69)
Non-Merging Parties	126	-2.69	15.80	-12.02	-0.99	6.22
		(1.41)		(2.06)	(1.23)	(1.89)
C. Equally-Weighted Regressions						
Overall	126	-0.68	26.19	-12.96	-1.39	7.05
		(2.33)		(1.68)	(1.82)	(1.04)
Merging Parties	126	5.16	78.49	-20.90	-3.99	11.47
		(6.99)		(3.88)	(2.86)	(2.83)
Non-Merging Parties	126	0.22	13.64	-6.28	-1.27	5.67
		(1.22)		(0.97)	(1.13)	(1.01)
D. Inverse-Variance Weighting Across Mergers						
Overall	126	-3.90	8.11	-9.01	-3.64	1.99
		(0.72)		(0.41)	(0.24)	(0.21)
Merging Parties	126	-16.64	19.80	-25.91	-14.93	-5.75
		(1.76)		(0.20)	(0.24)	(0.12)
Non-Merging Parties	126	-4.09	9.09	-9.21	-4.22	2.19
		(0.81)		(0.39)	(0.27)	(0.11)

Table A.3: Robustness of Quantity Effects. This table displays summary statistics and standard errors for the distribution of transformed coefficient estimates of (2) (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for overall, merging-party, and non-merging-party quantity changes. Panel A displays the baseline results from the main text, Panel B displays results using the NielsenIQ Consumer Panel Data, Panel C displays results assuming equal weights across firm type/DMAs when estimating (1) and (2), and Panel D displays results when merger-level coefficient estimates are weighed by the inverse variance of the transformed second-stage estimate.

	(1)	(2)	(3)	(4)	(5)	(6)
HHI (0–1)	-0.11			-0.02		
	(0.13)			(0.15)		
DHHI (0–1)	3.08			2.77		
	(1.20)			(1.23)		
HHI $\in$ [1500, 2500]		0.07			0.08	
		(0.05)			(0.05)	
HHI $>$ 2500		0.05			0.07	
		(0.03)			(0.04)	
DHHI $\in$ [100, 200]		0.09			0.06	
		(0.10)			(0.10)	
DHHI $>$ 200		0.25			0.22	
		(0.11)			(0.11)	
Yellow			0.08			0.05
			(0.09)			(0.09)
Red			0.26			0.23
			(0.11)			(0.11)
Log(Total Merging Sales)				0.03	0.02	0.02
				(0.02)	(0.01)	(0.01)
N	130	130	130	130	130	130

Table A.4: Determinants of Enforcement. Coefficients indicate output from a linear probability model of enforcement on merger-level market structure. HHI and DHHI as measured as the average across all DMAs within a merger. Robust standard errors are in parentheses.

	Merging			Non-Merging			Aggregate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
HHI (0–1)	-15.18			-3.94			-10.06		
	(7.42)			(10.23)			(6.03)		
DHHI (0–1)	30.24			39.51			34.48		
	(17.19)			(23.73)			(15.99)		
HHI $\in$ [1500, 2500]		-0.91			-0.20			0.01	
		(2.27)			(2.07)			(2.01)	
HHI $>$ 2500		-5.36			-1.93			-2.92	
		(2.35)			(2.20)			(2.04)	
DHHI $\in$ [100, 200]		2.34			1.56			1.74	
		(2.02)			(1.69)			(1.39)	
DHHI $>$ 200		4.50			4.75			4.35	
		(1.68)			(1.75)			(1.35)	
Yellow			1.90			0.98			1.23
			(1.75)			(1.25)			(1.08)
Red			1.40			4.28			2.88
			(1.71)			(2.00)			(1.52)
Constant	3.37	1.74	-0.47	2.44	2.08	1.40	3.54	1.92	0.98
	(1.89)	(1.89)	(0.97)	(2.35)	(1.87)	(0.76)	(1.58)	(1.82)	(0.70)
N	126	126	126	126	126	126	126	126	126

Table A.5: Regression of price changes on measures of market structure. We use HHI and DHHI as computed using nationwide shares. Columns (1)–(3) use merging party price changes, Columns (4)–(6) use non-merging party price changes, and Columns (7)–(9) use aggregate price changes. Each observation is a merger. Robust standard errors are in parentheses.

	Merging			Non-Merging			Aggregate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
HHI (0–1)	-13.86			-8.94			-12.69		
	(5.88)			(7.31)			(4.70)		
DHHI (0–1)	17.24			38.97			27.31		
	(20.90)			(23.28)			(17.45)		
HHI $\in$ [1500, 2500]		0.13			-1.32			-1.17	
		(3.33)			(2.39)			(2.46)	
HHI $>$ 2500		-5.78			-5.55			-5.83	
		(3.26)			(2.30)			(2.33)	
DHHI $\in$ [100, 200]		2.72			2.56			2.67	
		(2.08)			(1.52)			(1.20)	
DHHI $>$ 200		3.65			4.19			3.64	
		(1.89)			(1.54)			(1.23)	
Yellow			1.20			1.36			1.37
			(1.84)			(1.42)			(1.18)
Red			0.98			2.25			1.47
			(1.89)			(1.56)			(1.27)
Constant	3.90	2.66	-0.54	4.12	4.93	1.34	4.95	4.62	0.92
	(1.93)	(3.03)	(0.94)	(2.08)	(2.09)	(0.75)	(1.50)	(2.20)	(0.69)
N	117	117	117	117	117	117	117	117	117

Table A.6: Regression of price changes on measures of market structure, dropping mergers with a divestiture. HHI and DHHI as measured as the average across all DMAs within a merger. Columns (1)–(3) use merging party price changes, Columns (4)–(6) use non-merging party price changes, and Columns (7)–(9) use aggregate price changes. Each observation is a merger. Robust standard errors are in parentheses.

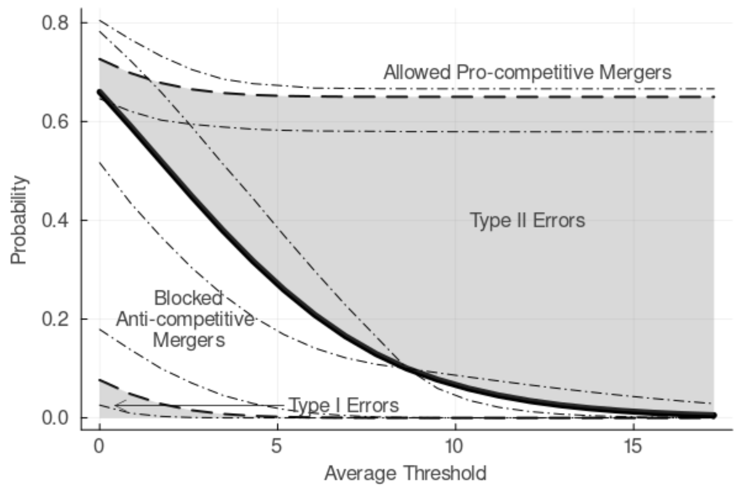
	Green				Yellow			Red
	LM	LH	ML	HL	MM	MH	HM	HH
LL	0.84	2.57	0.92	2.11	1.17	2.19	1.97	2.86
	(0.38)	(0.64)	(0.52)	(0.73)	(0.62)	(0.70)	(0.75)	(0.84)
LM		1.73	0.08	1.27	0.33	1.35	1.13	2.02
		(0.52)	(0.57)	(0.76)	(0.59)	(0.69)	(0.77)	(0.85)
LH			-1.65	-0.46	-1.40	-0.38	-0.60	0.29
			(0.67)	(0.78)	(0.66)	(0.75)	(0.79)	(0.89)
ML				1.19	0.25	1.27	1.05	1.94
				(0.66)	(0.53)	(0.63)	(0.64)	(0.75)
HL					-0.93	0.08	-0.14	0.76
					(0.57)	(0.64)	(0.42)	(0.54)
MM						1.02	0.80	1.69
						(0.44)	(0.54)	(0.69)
MH							-0.22	0.67
							(0.59)	(0.72)
HM								0.89
								(0.38)

(a) Merging parties

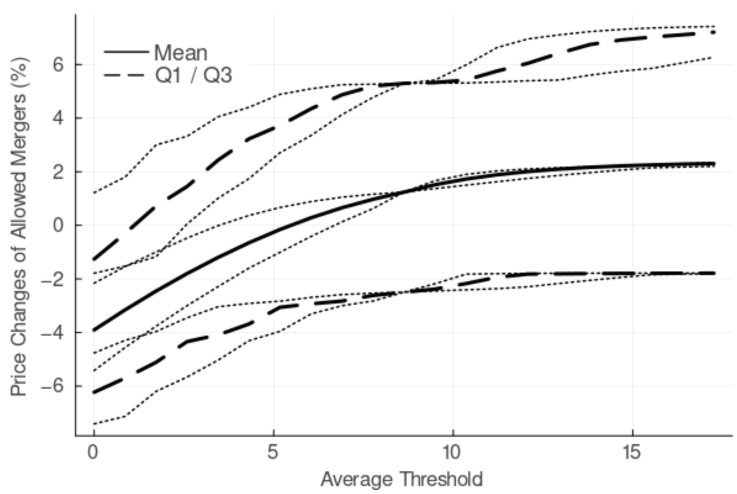
	Green				Yellow			Red
	LM	LH	ML	HL	MM	MH	HM	HH
LL	-2.38	-2.94	0.39	0.85	0.30	0.79	0.42	1.18
	(0.67)	(0.95)	(0.36)	(0.45)	(0.49)	(0.82)	(0.63)	(0.78)
LM		-0.56	2.77	3.23	2.68	3.17	2.80	3.56
		(0.71)	(0.81)	(0.86)	(0.83)	(0.92)	(0.92)	(1.02)
LH			3.33	3.79	3.24	3.73	3.37	4.13
			(1.02)	(1.06)	(1.02)	(1.06)	(1.07)	(1.16)
ML				0.46	-0.09	0.40	0.04	0.80
				(0.24)	(0.38)	(0.78)	(0.52)	(0.70)
HL					-0.55	-0.06	-0.43	0.33
					(0.40)	(0.79)	(0.53)	(0.73)
MM						0.49	0.13	0.89
						(0.60)	(0.48)	(0.61)
MH							-0.37	0.39
							(0.65)	(0.61)
HM								0.76
								(0.45)

(b) Nonmerging parties

Table A.7: Differences in DMA-level price effects across bins of DMA-level market structures. The first letter denotes the HHI bin (Low, Medium, or High), and the second letter denotes the DHHI bin. Each cell indicates the difference between the column bin and the row bin. Standard errors of the difference, clustered at the merger level, are in parentheses.



(a) Probability of challenges



(b) Price changes of consummated mergers

Figure A.2: Outcomes of counterfactual thresholds. This replicates Figure 9, but the dotted lines surrounding each line represent 95% confidence intervals.

B. Estimation and Heterogeneous Treatment Effects

In this appendix, we check conditions under which we recover the weighted average treatment effect of the merger across UPC-DMAs in our baseline specification. Suppose each UPC i belongs to a single brand $b(i)$ and that each brand belong to either a merging or a non-merging party. Suppose that the data generating process the potential outcomes $Y_{idt}(0)$ and $Y_{idt}(1)$, i.e., with and without the merger, for UPC i in DMA d in time period t satisfies

$$\begin{aligned} Y_{idt}(0) &= \beta_{b(i)} \cdot t + \xi_{id} + \xi_{m(t)} + \epsilon_{idt} \\ Y_{idt}(1) &= Y_{idt}(0) + \delta_{idt}. \end{aligned}$$

That is, δ_{idt} is the effect of the merger on UPC i in DMA d and period t. We do not take a stand on the structure of δ_{idt}. Rather, we show that in our baseline specification, our estimation routine recovers the appropriately weighted average of these treatment effects even in the presence of unmodeled heterogeneity in δ_{idt}.

In the first stage, we estimate

$$Y_{idt} = \beta_{b(i)} \cdot t + \xi_{id} + \xi_{m(t)} + \epsilon_{idt},$$

where $m(t)$ is the month corresponding to time period t. We will construct the weighted OLS estimator for each β_b in this case, with weights w_{id}. Note that we do not allow weights to vary with time as we never do so in the empirical application. In what follows, we assume we a balanced panel of UPCs for notational simplicity. We have checked that our results go through in an unbalanced panel with random attrition.

To begin, we apply the Frisch-Waugh Theorem twice to partial out UPC-DMA fixed effects and month-of-the-year fixed effects. Since weights are constant within UPC-DMA and there are no further covariates that are common across UPC-DMAs, for each application of the theorem we can simply demean within the appropriate fixed effect. First, the UPC-DMA average and the deviation between the outcome and this average are

$$
\begin{aligned}
\bar{Y}_{id} &\equiv \beta_{b(i)} \cdot \bar{t} + \xi_{id} + \bar{\xi}_m + \bar{\epsilon}_{id} \\
Y_{idt} - \bar{Y}_{id} &= \beta_{b(i)} \cdot (t - \bar{t}) + \xi_{m(t)} - \bar{\xi}_m + \epsilon_{idt} - \bar{\epsilon}_{id},
\end{aligned}
$$

where $\bar{\xi}_m \equiv T^{-1} \sum_t \xi_{m(t)}$ and $\bar{\epsilon}_{id} \equiv T^{-1} \sum_t \epsilon_{idt}$. To partial out the month-of-the-year average, let $T_m \equiv \sum_t \mathbb{1}\left[m\left(t\right) = m\right]$ denote the number of months in the sample that correspond to month-of-the-year m. For month-of-the-year m, let

$$
\begin{aligned}
\bar{Y}_{idm} &\equiv T_m^{-1} \sum_t \mathbb{1}\left[m\left(t\right) = m\right] \left(Y_{idt} - \bar{Y}_{id}\right) \\
&= \beta_{b(i)} \left(\bar{t}_m - \bar{t}\right) + \xi_m - \bar{\xi}_m + \bar{\epsilon}_{idm} - \bar{\epsilon}_{id} \\
\tilde{Y}_{idt} &\equiv Y_{idt} - \bar{Y}_{id} - \bar{Y}_{idm} \\
&= \beta_{b(i)} \left(t - \bar{t}_{m(t)}\right) + \epsilon_{idt} - \bar{\epsilon}_{idm(t)}.
\end{aligned}
$$

The weighted OLS estimator for β_b is

$$
\begin{aligned}
\hat{\beta}_b &= \frac{N_b^{-1} \cdot D^{-1} \cdot T^{-1} \sum_{i \in b,d,t} w_{id} \left(t - \bar{t}_{m(t)}\right) \tilde{Y}_{idt}}{N_b^{-1} \cdot D^{-1} \cdot T^{-1} \sum_{i \in b,d,t} w_{id} \left(t - \bar{t}_{m(t)}\right)^2} \\
&= \beta_b + \frac{N_b^{-1} \cdot D^{-1} \cdot T^{-1} \sum_{i \in b,d,t} w_{id} \left(t - \bar{t}_{m(t)}\right) \left(\epsilon_{idt} - \bar{\epsilon}_{idm(t)}\right)}{N_b^{-1} \cdot D^{-1} \cdot T^{-1} \sum_{i \in b,d,t} w_{id} \left(t - \bar{t}_{m(t)}\right)^2}.
\end{aligned}
$$

We recover the remaining parameters as

$$
\begin{aligned}
\widehat{\xi_{m(t)} - \bar{\xi}_m} &= N^{-1} D^{-1} \sum_{i,d} w_{id} \left(\bar{Y}_{idm(t)} - \hat{\beta}_{b(i)} \left(\bar{t}_{m(t)} - \bar{t}\right)\right) \\
&= \xi_{m(t)} - \bar{\xi}_m + N^{-1} D^{-1} \sum_{i,d} w_{id} \left[\left(\beta_{b(i)} - \hat{\beta}_{b(i)}\right) \left(\bar{t}_{m(t)} - \bar{t}\right) + \bar{\epsilon}_{idm(t)} - \bar{\epsilon}_{id}\right] \\
\widehat{\xi_{id} + \bar{\xi}_m} &= \bar{Y}_{id} - \hat{\beta}_{b(i)} \cdot \bar{t} = \xi_{id} + \bar{\xi}_m + \left(\beta_{b(i)} - \hat{\beta}_{b(i)}\right) \bar{t} + \bar{\epsilon}_{id}
\end{aligned}
$$

In the second stage, we take weighted averages of

$$
Y_{idt} - \hat{Y}_{idt} = \delta_{idt} + \left(\beta_{b(i)} - \hat{\beta}_{b(i)}\right) \cdot (t - \bar{t}) + \epsilon_{idt} - \bar{\epsilon}_{id}
$$

$$- N^{-1}D^{-1}\sum_{\tilde{i},\tilde{d}} w_{\tilde{i},\tilde{d}}\left[\left(\beta_{b(\tilde{i})}-\hat{\beta}_{b(\tilde{i})}\right)\left(\bar{t}_{m(t)}-\bar{t}\right)+\bar{\epsilon}_{\tilde{i}\tilde{d}m(t)}-\bar{\epsilon}_{\tilde{i}\tilde{d}}\right].$$

Note that $\sum_t (t-\bar{t}) > 0$ as $\bar{t}$ is the mean time in the pre-period and the summation is over the post-period. Moreover, $\sum_t \left(\bar{t}_{m(t)}-\bar{t}\right)$ may or may not be 0, depending on the number of times each month of the year appears in the pre and post periods. We will not assume it is zero either. Instead, note that

$$\begin{aligned}
&\frac{\sum_{i,d,t}\mathbb{1}\left[i\text{ is merging}\right]w_{id}\left(Y_{idt}-\hat{Y}_{idt}\right)}{\sum_{idt}\mathbb{1}\left[i\text{ is merging}\right]w_{id}} \\
&\quad=\frac{\sum_{i,d,t}\mathbb{1}\left[i\text{ is merging}\right]w_{id}\left(\delta_{idt}+\left(\beta_{b(i)}-\hat{\beta}_{b(i)}\right)\cdot(t-\bar{t})+\epsilon_{idt}-\bar{\epsilon}_{id}\right)}{\sum_{i,d,t}\mathbb{1}\left[i\text{ is merging}\right]w_{id}} \\
&\qquad-\frac{N^{-1}D^{-1}\sum_{\tilde{i},\tilde{d}}w_{\tilde{i}\tilde{d}}\left[\left(\beta_{b(\tilde{i})}-\hat{\beta}_{b(\tilde{i})}\right)\left(\bar{t}_{m(t)}-\bar{t}\right)+\bar{\epsilon}_{\tilde{i}\tilde{d}m(t)}-\bar{\epsilon}_{\tilde{i}\tilde{d}}\right]}{\sum_{i,d,t}\mathbb{1}\left[i\text{ is merging}\right]w_{id}}.
\end{aligned}$$

If $\mathbb{E}\left[\epsilon_{id\tilde{t}}|w_{id},t\right]=0$ for all i, d, and $\tilde{t}$ satisfying $m(\tilde{t})=m(t)$, then

$$\begin{aligned}
&\text{plim}_{N\cdot D\to\infty}\frac{\sum_{i,d,t}1\left[i\text{ is merging}\right]w_{id}\left(Y_{idt}-\hat{Y}_{idt}\right)}{\sum_{i,d,t}\mathbb{1}\left[i\text{ is merging}\right]w_{id}} \\
&\qquad\qquad=\text{plim}_{N\cdot D\to\infty}\frac{\sum_{i,d,t}\mathbb{1}\left[i\text{ is merging}\right]w_{id}\cdot\delta_{idt}}{\sum_{i,d,t}\mathbb{1}\left[i\text{ is merging}\right]w_{id}}. \quad (6)
\end{aligned}$$

Thus, the estimate from our baseline procedure converges to the weighted average treatment effect of the merger. We write the right-hand side of (6) as a probability limit as $N \cdot D \to \infty$ since we are adding new treatment effects δ_{idt} and weights w_{id} as this limit happens and some regularity conditions are needed for this sum to converge. For instance, we can follow Grieco et al. (2022b) and assume that δ_{idt} and w_{id} are i.i.d. draws from a superpopulation, in which case the probability limit converges to the appropriate weighted average for the superpopulation.

C. Details on Sample Construction

As discussed in Section II.B, we first filter the SDC Platinum dataset to only include deals valued at \$280 million dollars or more involving manufacturers of retail products. In particular, we restrict the dataset to completed deals that took place on or after 2007, where (i) either the target or acquirer is in the United States, (ii) the acquirer is not classified as "Investment and Commodity Firms, Dealers, Exchanges," (iii) the deal involves SIC codes that satisfy a broad interpretation of retail products, and (iv) the deal size is above \$280 million.

Most deals that survive this initial filtering process either involve firms that do not sell retail products or only sell products not tracked in the NielsenIQ Scanner Dataset. To identify relevant deals, we analyze each deal's press release and the merging parties' SEC filings for the year before 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.

As described in Section II.B, we next check whether the parties overlapped in particular product and geographic markets by computing whether they each owned at least one UPC with a non-negligible share in the same geographic market. To do so, we compute 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 around the merger. If this is more than 100 UPCs, we only keep the 100 best-selling UPCs. To ensure we do not miss any regional brands, we then add all UPCs with more than a 5% share in any region-month pair. With this initial sample of products, we check market coverage: the fraction of sales volume in the product market captured by this sample. 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 this continues to be the case, we expand the universe to 300 UPCs. If coverage continues to be too low, we drop the initial share cutoff from 1% to 0.5% and finally to 0.1%. Finally, to ensure we do not miss seasonal products affiliated with a popular brand, we add all UPCs associated with a brand included in our original list and all UPCs associated with brands that have a market share of at least 5%.

This procedure yields a sample that covers a large share of each relevant market.

The average value (across mergers) of the 10th percentile of market coverage (within merger, across DMAs) is 92.2%, and the average value of the median coverage is 97.3%. This reassures us that we are capturing the relevant products in each product market.

Table C.1 shows the market definitions we use in our merger: it lists the product group as well as the set of product modules that constitute each market. Note that there are fewer market definitions than there are mergers since multiple mergers can happen in the same product market at different points in time. For each of these markets, we also list the union of cost controls used for their respective mergers.

Market	NielsenIQ Product Group	NielsenIQ Product Modules in Product Market	Cost Controls
1	Baby Food	Baby Milk And Milk Flavoring	Canned Baby Foods (PPI), Formula Feeds (PPI)
2	Baked Goods-Frozen	Bakery-Bagels-Frozen	Wheat, Other Grains
3	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, Frozen/Refrigerated Breakfasts, Frozen Waffles & Pancakes & French Toast	Oats, Wheat, Sugar, Cocoa Beans
4	Beer	Beer, Near Beer/Malt Beverage, Stout And Porter, Light Beer (Low Calorie/Alcohol), Ale, Malt Liquor	Barley, Wheat
5	Beer	Beer, Stout And Porter, Light Beer (Low Calorie/Alcohol), Ale, Malt Liquor	Barley, Wheat
6	Bread And Baked Goods	Bakery - Bread - Fresh	Wheat, Other Grains
7	Bread And Baked Goods	Bakery-Bagels-Fresh	Sugar, Wheat, Wheat Flour, Other Grains
8	Bread And Baked Goods	Bakery-Breakfast Cakes/Sweet Rolls-Fresh	Wheat Flour, Sugar, Vegetable Oil
9	Bread And Baked Goods	Bakery-Buns-Fresh	Wheat, Other Grains
10	Bread And Baked Goods	Bakery-Cheesecake-Fresh	Cheese, Wheat Flour, Wheat, Eggs, Sugar
11	Bread And Baked Goods	Bakery-Doughnuts-Fresh	Wheat Flour, Sugar, Vegetable Oil
12	Bread And Baked Goods	Bakery-Muffins-Fresh	Wheat, Other Grains
13	Bread And Baked Goods	Bakery-Pies-Fresh	Wheat Flour, Pecans, Lemons, Apples
14	Bread And Baked Goods	Bakery-Rolls-Fresh	Wheat, Other Grains
15	Breakfast Foods-Frozen	Bacon-Refrigerated	Slaughter Hogs, Processed Meat, Slaughter Poultry
16	Breakfast Foods-Frozen	Frozen/Refrigerated Breakfasts	Eggs, Slaughter Poultry, Slaughter Cattle, Beef And Veal, Cheese, Russet Potatoes
17	Candy	Candy-Chocolate-Miniatures, Candy-Chocolate, Candy-Chocolate-Special	Cocoa Beans, Sugar
18	Candy	Candy-Dietetic - Non-Chocolate, Candy-Dietetic - Chocolate	Sugar, Cocoa Beans
19	Candy	Candy-Hard Rolled, Candy-Non-Chocolate-Miniatures, Candy-Non-Chocolate, Candy-Lollipops	Sugar, Cocoa Beans
20	Cereal	Cereal - Granola & Natural Types	Sugar, Oats
21	Cereal	Cereal - Ready To Eat	Barley, Corn, Oats, Rough Rice, Sugar, Wheat
22	Coffee	Coffee - Soluble Flavored, Coffee - Soluble	Coffee Beans
23	Coffee	Ground And Whole Bean Coffee, Coffee - Liquid	Coffee Beans
24	Condiments, Gravies, And Sauces	Cooking Sauce	Sugar, Tomatoes, Sauces (PPI), Corn
25	Condiments, Gravies, And Sauces	Fish & Seafood & Cocktail Sauce	Tomatoes, Mayonnaise And Dressing, Shrimp, Unprocessed Finfish, Pickles And Horseradish
26	Condiments, Gravies, And Sauces	Meat Sauce, Worcestershire Sauce	Beef And Veal, Tomatoes, Vinegar
27	Condiments, Gravies, And Sauces	Mustard	
28	Condiments, Gravies, And Sauces	Sauce & Seasoning Mix-Remaining	Sauces (PPI), Salt Pepper Spices, Spices, Vinegar, Dry Onions
29	Condiments, Gravies, And Sauces	Sauce Mix - Spaghetti	Salt Pepper Spices, Spices, Sauces (PPI)

Market	NielsenIQ Product Group	NielsenIQ Product Modules in Product Market	Cost Controls
30	Condiments, Gravies, And Sauces	Sauce Mix - Taco, Sauce & Seasoning Mix-Remaining Mexican	Sauces (PPI)
31	Condiments, Gravies, And Sauces	Seasoning Mix - Chili	Salt Pepper Spices, Spices, Sugar, Vinegar
32	Condiments, Gravies, And Sauces	Seasoning Mix - Sloppy Joe	Spices, Sauces (PPI), Salt Pepper Spices, Tomatoes, Slaughter Hogs
33	Cookies	Cookies	Wheat Flour, Cocoa Beans, Sugar, Oats
34	Cosmetics	Cosmetic Kits	Cosmetics (PPI)
35	Cosmetics	Cosmetics - Concealers	Cosmetics (PPI)
36	Cosmetics	Cosmetics-Blushers	Cosmetics (PPI)
37	Cosmetics	Cosmetics-Eye Shadows	Cosmetics (PPI)
38	Cosmetics	Cosmetics-Eyebrow & Eye Liner	Cosmetics (PPI)
39	Cosmetics	Cosmetics-Face Powder	Cosmetics (PPI)
40	Cosmetics	Cosmetics-Foundation-Liquid, Cosmetics-Foundation-Cream And Powder	Cosmetics (PPI)
41	Cosmetics	Cosmetics-Lipsticks	Cosmetics (PPI)
42	Cosmetics	Cosmetics-Mascara	Cosmetics (PPI)
43	Cosmetics	Cosmetics-Remaining	Cosmetics (PPI)
44	Cosmetics	Talcum & Dusting Powder	Talc, Corn Starch
45	Detergents	Detergents-Packaged, Detergents - Light Duty, Detergents - Heavy Duty - Liquid	Soap Detergents (PPI)
46	Detergents	Packaged Soap, Laundry Treatment Aids, Fabric Washes - Special, Detergent Boosters	Soap Detergents (PPI)
47	Fragrances - Women	Cologne & Perfume-Women's	Perfume Chemicals (PPI), Ethanol, Coal, Soybeans, Other Grains
48	Fresh Produce	Fresh Fruit-Remaining	Fertilizer
49	Grooming Aids	Cosmetic And Nail Grooming Accessory	Cosmetics (PPI)
50	Gum	Gum-Bubble, Gum-Chewing, Gum-Chewing-Sugarfree, Gum-Bubble-Sugarfree, Breath Sweeteners	Sugar, Resin And Synthetic Rubber, Gum (PPI)
51	Hair Care	Creme Rinses & Conditioners	Hair Chemicals (PPI), Hair Conditioner
52	Hair Care	Hair Preparations - Other Than Men's	Essential Oil, Hair Chemicals (PPI)
53	Hair Care	Hair Spray - Women's	Hair Chemicals (PPI)
54	Hair Care	Shampoo-Aerosol/ Liquid/ Lotion/ Powder, Shampoo-Combinations	Hair Chemicals (PPI)
55	Hair Care	Wave Setting Products	Hair Chemicals (PPI), Ethanol
56	Houseware, Appliances	Oral Hygiene Appliance And Accessory	Nylon, Plastic
57	Juice, Drinks - Canned, Bottled	Fruit Drinks-Canned, Fruit Drinks-Other Container, Water-Bottled	Bottled Water (PPI), Sugar
58	Kitchen Gadgets	Beverage Storage Container	Plastic, Stainless Steel
59	Liquor	Alcoholic Cocktails	Barley, Wheat, Corn
60	Liquor	Bourbon-Straight/Bonded, Bourbon-Blended, Canadian Whiskey, Irish Whiskey, Remaining Whiskey, Scotch, Gin, Vodka, Rum, Tequila, Brandy/Cognac	Barley, Wine Grapes, Wheat, Corn
61	Liquor	Bourbon-Straight/Bonded, Bourbon-Blended, Canadian Whiskey, Irish Whiskey, Remaining Whiskey, Scotch, Gin, Vodka, Rum, Tequila, Brandy/Cognac, Cordials & Proprietary Liqueurs	Barley, Wheat, Corn
62	Liquor	Bourbon-Straight/Bonded, Bourbon-Blended, Canadian Whiskey, Irish Whiskey, Remaining Whiskey, Scotch, Gin, Vodka, Rum, Tequila, Brandy/Cognac, Cordials & Proprietary Liqueurs, Alcoholic Cocktails	Barley, Wheat, Corn
63	Liquor	Vodka	Wheat, Corn, Russet Potatoes, Other Grains, Ethanol
64	Medications/Remedies/ Health Aids	Foot Preparations-Athlete's Foot	

Market	NielsenIQ Product Group	NielsenIQ Product Modules in Product Market	Cost Controls
65	Men's Toiletries	Cologne/Lotion-Men's	Perfume Chemicals, Ethanol, Coal, Soybeans, Other Grains
66	Packaged Meats-Deli	Bacon-Refrigerated	Slaughter Hogs, Slaughter Poultry
67	Packaged Meats-Deli	Bratwurst & Knockwurst, Sausage-Dinner, Frankfurters-Refrigerated	Slaughter Hogs, Slaughter Cattle, Slaughter Poultry
68	Packaged Meats-Deli	Lunchmeat-Deli Pouches-Refrigerated	Slaughter Cattle, Slaughter Poultry, Slaughter Hogs, Beef And Veal
69	Packaged Meats-Deli	Lunchmeat-Sliced-Refrigerated	Slaughter Hogs, Slaughter Poultry, Slaughter Cattle
70	Packaged Meats-Deli	Sausage-Breakfast	Slaughter Hogs, Slaughter Poultry, Processed Meat (PPI)
71	Pet Food	Cat Food - Wet Type, Cat Food - Moist Type, Cat Food - Dry Type	Soybeans, Other Grains, Slaughter Poultry, Slaughter Cattle, Unprocessed Finfish
72	Pet Food	Dog & Cat Treats	Soybeans, Other Grains, Slaughter Hogs, Slaughter Poultry, Slaughter Cattle, Unprocessed Finfish
73	Pet Food	Dog Food - Wet Type, Dog Food - Moist Type, Dog Food - Dry Type	Soybeans, Other Grains, Slaughter Poultry, Slaughter Cattle, Unprocessed Finfish
74	Pickles, Olives, And Relish	Pickles - Sweet	Pickles And Pickled Products (PPI), Pickles and Horseradish (PPI), Sauces
75	Pickles, Olives, And Relish	Relishes	Sauces, Pickles And Pickled Products (PPI), Sugar, Corn, Pickles and Horseradish (PPI), Mangoes
76	Pizza / Snacks / Hors D'oeuvres-Frozen	Pizza-Frozen	Cheese, Wheat Flour, Wheat, Refrigerated Storage
77	Prepared Food-Ready-To-Serve	Chicken - Shelf Stable	Poultry Processing
78	Prepared Food-Ready-To-Serve	Chili-Shelf Stable	Beef And Veal, Beans City Average
79	Prepared Food-Ready-To-Serve	Stew - Beef - Shelf Stable, Stew - Remaining - Shelf Stable, Stew - Chicken - Shelf Stable	Poultry Processing, Beef And Veal
80	Prepared Foods-Frozen	Entrees - Meat - 1 Food - Frozen	Slaughter Cattle, Slaughter Poultry, Slaughter Hogs, Beef And Veal
81	Shortening, Oil	Cooking Sprays	Olive Oil, Soybean Oil, Vegetable Oil, Sunflower Oil, Rapeseed Oil
82	Skin Care Preparations	Hand & Body Lotions	Creams Lotions (PPI)
83	Skin Care Preparations	Hand Cream	Creams Lotions (PPI)
84	Skin Care Preparations	Skin Cream-All Purpose	Creams Lotions (PPI)
85	Snacks	Dip - Mixes	Dry Onions, Salt Pepper Spices
86	Snacks	Popcorn - Popped, Snacks - Caramel Corn	Cheese, Cocoa Beans, Corn
87	Snacks	Snacks - Health Bars & Sticks	Whey, Corn Starch, Sugar, Vegetable Oil, Peanuts, Almonds
88	Snacks	Snacks - Potato Chips	Corn Starch, Salt Pepper Spices, Russet Potatoes, Sunflower Oil
89	Snacks	Snacks - Potato Chips, Snacks - Potato Sticks	Corn Starch, Salt Pepper Spices, Russet Potatoes, Vegetable Oil, Cheese, Wheat
90	Snacks	Snacks - Pretzel	Wheat Flour, Eggs, Sugar
91	Snacks	Snacks - Remaining	Wheat Flour, Spices, Corn, Other Grains, Russet Potatoes, Wheat, Vinegar

Market	NielsenIQ Product Group	NielsenIQ Product Modules in Product Market	Cost Controls
92	Spices, Seasoning, Extracts	Meat Marinades & Tenderizers	Salt Pepper Spices, Sauces (PPI), Spices
93	Spices, Seasoning, Extracts	Pepper	Spices, Salt Pepper Spices
94	Spices, Seasoning, Extracts	Salt - Cooking/Edible/Seasoned	
95	Spices, Seasoning, Extracts	Seasoning-Dry	Spices, Salt Pepper Spices
96	Spices, Seasoning, Extracts	Vegetables - Onions - Instant	Dry Onions
97	Stationery, School Supplies	Dry Erase Bulletin Board And Accesory	Aluminum
98	Stationery, School Supplies	Personal Planners Binders And Folders	Pulp Paper, Plastic
99	Tobacco & Accessories	Cigarettes	Tobacco, Pulp Paper
100	Unprep Meat/Poultry/Seafood-Frzn	Frozen Poultry	Poultry Processing, Slaughter Poultry, Processed Foods And Feeds
101	Vegetables - Canned	Mushrooms - Shelf Stable	
102	Vegetables - Canned	Vegetables-Beans-Green-Canned, Vegetables-Beans-Waxed-Canned, Vegetables-Beans-White/Northern/Navy-Canned, Vegetables-Beans-Vegetarian-Shelf Stable, Vegetables-Beans-Remaining-Canned, Vegetables-Beans-Garbanzo - Canned, Vegetables-Beans-Lima-Canned, Vegetables-Beans-Kidney/Red-Canned, Vegetables-Beans-Pinto-Canned, Vegetables-Beans-Chili-Canned	Vinegar
103	Vegetables - Canned	Vegetables-Mixed-Canned	Carrots, Vinegar, Beans City Average
104	Vegetables - Canned	Vegetables-Peas-Remaining-Canned, Vegetables-Peas-Canned, Vegetables-Peas & Carrots-Canned	Vinegar, Green Peas, Carrots, Pinto Beans
105	Vegetables And Grains - Dried	Rice - Instant	Fertilizer
106	Wine	Wine-Vermouth, Wine-Aperitifs, Wine-Domestic Dry Table, Wine-Imported Dry Table, Wine-Flavored/Refreshment, Wine-Kosher Table, Wine-Sake, Wine-Sangria, Wine-Sparkling, Wine-Sweet Dessert-Domestic, Wine-Sweet Dessert-Imported, Wine - Non Alcoholic	Wine Grapes, US/AUD Conversion, US/Euro Conversion

Table C.1: Product Market Definitions and Cost Controls