NBER WORKING PAPER SERIES

MERGER EFFECTS AND ANTITRUST ENFORCEMENT:
EVIDENCE FROM US CONSUMER PACKAGED GO

Vivek Bhattacharya
Gastón Illanes
David Stillerman

Working Paper 31123
http://www.nber.org/papers/w31123

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
April 2023, revised June 2024

The first version of this paper was circulated with the title "Have Mergers Raised Prices? Evidence from US Retail." We are grateful to Daniel Ackerberg, Matt Backus, 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, Bill Rogerson, Nancy Rose, Andrew Sweeting, Frank Verboven, 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. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 31123
April 2023, revised June 2024
JEL No. D43,K21,L13,L41

ABSTRACT

We document the effects of a comprehensive set of mergers of US consumer packaged goods manufacturers on prices, quantities, and product assortment. Across a range of specifications, we find a small average price effect of mergers (-0.6% to 1.6%) but substantial heterogeneity in effects, with a standard deviation between 5.3–6.7 pp. Through a model of enforcement, we find that agencies challenge mergers they expect would increase prices more than about 4%–8%. Modest increases in stringency would reduce prices and the prevalence of completed price-increasing mergers, with minimal impacts on blocked price-decreasing mergers, at a significantly greater agency burden.

Vivek Bhattacharya
Department of Economics
Northwestern University
2211 Campus Drive
Evanston, IL 60208
and NBER
vivek.bhattacharya@northwestern.edu

Gastón Illanes
Department of Economics
Northwestern University
Kellogg Global Hub, Room 3421
2211 Campus Drive
Evanston, IL 60208
and NBER
gaston.illanes@northwestern.edu

David Stillerman
Kogod School of Business
American University
4400 Massachusetts Avenue NW
Washington, DC 20016
stillerman@american.edu

A data appendix is available at http://www.nber.org/data-appendix/w31123

I. Introduction

Recent years have featured a debate over whether antitrust enforcement has been too lax (Kwoka, 2014; Scott Morton, 2019; Carlton and Heyer, 2020; Shapiro, 2021; Rose and Shapiro, 2022). We contribute to this debate by quantifying the outcomes of a representative set of consummated mergers in consumer packaged goods (CPG) and studying which mergers would have been approved under stricter antitrust regimes. More precisely, we first document how consummated mergers have affected prices, quantities, and other equilibrium outcomes of interest. Then, through a model of agency decisions under uncertainty, we investigate the relationship between these outcomes and enforcement actions. We quantify the implicit expected price increase that triggers antitrust enforcement and the uncertainty faced by the FTC and DOJ when deciding whether to challenge a merger. This allows us to provide novel insights on the stringency of antitrust enforcement by predicting the expected price changes of consummated mergers in stricter regimes and quantifying the prevalence of errors—completed price-increasing and blocked price-decreasing mergers—both in the status quo and counterfactual.

Our first contribution is to systematically analyze the effects of mergers in US CPG from 2006 to 2017. We study 130 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 CPG products sold through retail outlets. We thus avoid any bias induced by selecting which mergers to study based on interest in the popular press, data availability, and the potential for publication. This bias is large in other contexts (Shapiro et al., 2021), and it contaminates meta-analyses of papers focusing on particular mergers.

Our baseline estimates 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 affect prices. When possible, we also use the prices of products in geographic markets where the merging parties have a negligible presence as a control.

The average effect of completed mergers on prices is small. Across specifications, we estimate effects between -0.6% and 1.6%. These average effects mask substantial heterogeneity: in our baseline specification, the first quartile of price effects corresponds to a price decrease of 2.3% and the third corresponds to a price increase of 5.3%. The price changes of merging and non-merging parties are positively correlated and also show substantial heterogeneity. In the mergers with price increases in top quartile, merging parties increase prices by 10.0% and non-merging parties by 6.9%, on average. In the mergers with price changes in the bottom quartile, merging parties decrease prices by 10.7% and non-merging parties by 3.8%.

We next consider effects on total quantities. Across specifications, we find that aggregate quantities, on average, decrease between 0.4% and 2.5%. For our baseline specification, the first quartile of aggregate quantity changes is -6.9%, and the third quartile 2.8%. Aggregate price changes are negatively correlated with aggregate quantity changes. Merging parties are more likely to reduce quantity sold: in the baseline specification, their average quantity change is -7.1%, but the correlation between merging-party price changes and quantity changes is not statistically significant. This suggests that there are additional forces underlying the observed quantity effects. We find that quantity reductions for merging parties correlate with reductions in the number of stores served, with reduced product offerings across markets, and with the elimination of products at the national level.

Given the heterogeneity in effects across mergers, the agencies have a challenging task of deciding which mergers to screen. To assist in this task, both the 2010 Horizontal Merger Guidelines and the revised 2023 Merger Guidelines provide "structural presumptions," related to 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 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.

Our second contribution, which distinguishes this paper from other large-scale

[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.

analyses of merger effects, is a framework to interpret these effects in the context of antitrust enforcement. The stringency of antitrust enforcement is quantified by the marginal merger that agencies allow, whereas the distributions estimated above are those of all inframarginal mergers. Thus, as Carlton (2009) argues, one should not use a small average price change to conclude that agencies are strict: if agencies could perfectly predict the price change of a merger beforehand, the worst outcome observed among consummated mergers would be a measure of stringency.

The intuition from the previous paragraph must be adapted to the fact that agencies have at best a noisy estimate of the impact of a merger at the time of making a decision. Agencies may thus make two types of mistakes: blocking pro-competitive mergers ("type I errors") and allowing anti-competitive ones ("type II errors"). Enforcement has to balance these risks. For instance, it would be premature to conclude that agencies should be more strict even after observing a positive average price change: doing so might significantly increase the prevalence of blocked pro-competitive mergers, for instance, as it could be difficult to disentangle pro- and anti-competitive mergers ex-ante.

To quantify stringency, we develop and 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, based on this signal and its prior, it expects this merger to increase prices beyond a threshold. Using data on enforcement decisions for all mergers in our sample and estimates of the realized price changes, we estimate that on average the US antitrust agencies aim to propose remedies for CPG mergers with a mean price increase greater than 4.0–8.3%. Furthermore, our model allows us to estimate the noise in the agencies' ex-ante assessments of merger effects and thus simulate the effects of counterfactual antitrust stringency. This allows us to quantify the two sides of the above trade-off.

Moving to a 5% threshold would reduce aggregate price increases by about 1 pp and decrease the probability of allowing anti-competitive mergers. On the other side of the trade-off, a stricter threshold would naturally require the agency to challenge more mergers: we estimate that the agency would have to challenge almost three times as many. However, we find a negligible impact on the probability of type I errors, suggesting that concerns that stricter thresholds would lead to the unintended consequence of blocking more pro-competitive mergers are likely unwarranted.

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; Garmon and Bhatt, 2022) and gasoline (Simpson and Taylor, 2008; Lagos, 2018).[2] Some of these papers also compare results to merger simulations (Peters, 2006; Ivaldi and Verboven, 2005; Weinberg and Hosken, 2013; Björnerstedt and Verboven, 2016; Garmon, 2017). Kwoka (2014) provides a helpful meta-analysis and Asker and Nocke (2021) survey many of these results, but these analyses are 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 contemporaneous papers develop larger databases of M&A activity. Some studies focus on prices: in consumer packaged goods (Majerovitz and Yu, 2021), hospitals (Brand et al., 2023), 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. Majerovitz and Yu (2021) highlight the asymmetries in size between targets and acquirors, Brand et al. (2023) 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.

[2] The Federal Trade Commission manages a large bibliography of merger retrospectives at https://www.ftc.gov/policy/studies/merger-retrospective-program/bibliography.

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. (2023b), 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). Some papers have estimated causal impacts of antitrust enforcement on outcomes (Liebersohn, 2024; Chen et al., 2022; Reed et al., 2023) in industries including banking and pharmaceuticals. Others have correlated ex-post price changes with ex-ante structural presumptions (Brot-Goldberg et al., 2023) or measures of scrutiny (Brand et al., 2023). Our contribution is to directly assess and quantify the agencies' objective in how to scrutinize mergers and to study the impact of counterfactual policies on challenges and errors.

More broadly, the increased interest in documenting merger effects parallels a growing literature estimating markups and documenting concentration at a large scale, following the seminal work of De Loecker et al. (2020). Grieco et al. (2023) document decreasing markups in the automobile industry, and Miller et al. (2023) document increasing markups in cement, over several decades. Brand (2021), Döpper et al. (2022), and Atalay et al. (2023a) conduct similar exercises in consumer packaged goods. Benkard et al. (2021) document decreasing concentration in product markets. While we do not document markups or changes in concentration absent mergers, our paper sheds light into how merger activity has affected consumers.

II. Data and Sample Selection

II.A. Data Sources

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 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. NielsenIQ provides sales at the store-week level and the average transaction price for each UPC, and it also provides a classification of products into "groups" and "modules." We use NielsenIQ designated market areas (DMAs) as our geographic markets: these are collections of counties, usually centered around a major city.

In Appendix A, we replicate our analysis with the NielsenIQ Consumer Panel Dataset, which comes from a sampling of households and therefore covers some specific large retailers that the scanner dataset misses. We find that results are largely similar but discuss some discrepancies in the body. We nevertheless prefer the scanner dataset as our baseline specification for a number of reasons. First, since the scanner dataset comes from a sampling of stores, it has complete coverage of UPCs sold within a store, including those with small share—which is critical when studying product assortment. Second, stores are mapped to DMAs, which we believe are more appropriate geographic market definitions than the often much larger "Scantrack" markets in the Consumer Panel.

Since NielsenIQ does not provide ownership of each product, we augment the dataset with information from Euromonitor Passport.[3] We also use data from other sources to account for demand and supply-side characteristics that could influence prices. For each merger, we list product inputs (e.g., wheat for cereal) and obtain commodity price indices, typically from Federal Reserve Economic Data (FRED).

[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 details.

We then collect demographic data to control for changes that may affect demand, aggregating county-level data from the American Community Survey by DMA.

Finally, for our analysis of enforcement stringency in Section V.A, 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 available on the websites of the DOJ and FTC.

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

The 2023 Merger Guidelines advocate using a "hypothetical monopolist test" to define markets, defining a market to be the smallest set of products (that includes the merging parties') such that a hypothetical monopolist would find it profitable to impose a "small but significant and nontransitory" increase in prices. Implementing it requires access to information we do not have, such as customer affidavits or surveys, or using econometric analysis beyond the scope of our paper (Harkrider, 2015). The Merger Guidelines also advocate for the use of *Brown Shoe* factors ("practical indicia"). Courts have often resorted to these factors, such as industry recognition of submarkets, when making their decisions (Baker, 2000).[4] Court cases can include protracted debates between the parties about market definition.

In light of such debate over market definition, we adopt the strategy of staying close to NielsenIQ categorizations. NielsenIQ divides products into groups, broad categories such as "Prepared Foods - Frozen" or "Condiments, Gravies and Sauces," and modules, finer subcategories such as "Entrees - Meat - 1 Food - Frozen" or "Sauce Mix - Taco." We typically use individual product modules as our markets, but after manual inspection we sometimes find it more appropriate to group product modules.[5] While there is no guarantee that these sets of modules would have corresponded to antitrust markets, we find that they generally look similar to market definitions outlined by the DOJ and FTC in competitive impact statements over the

[4]See remarks by David Lawrence at the DOJ (https://www.justice.gov/opa/speech/policy-director-david-lawrence-antitrust-division-delivers-remarks-georgetown-center), who notes that all recent district court cases have cited *Brown Shoe* factors.

[5]Some cases are obvious: the Nuts product group includes modules such as "Nuts - Cans", "Nuts - Jars," "Nuts - Bags." In others, such as "Bratwurst" and "Frankfurters - Refrigerated," the specific module definition seems arbitrary, and we find it more reasonable to group the modules.

last 40 years.[6] Appendix D.2 provides details.

We aim to 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. To do so, we keep deals in SDC Platinum 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: we look at all UPCs in the product market sold within a two-year window of the deal and select those with a non-negligible market share.[7,8] We 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.

Table D.1 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 in product markets 1 and 2, that deal generates two mergers. Our final sample consists of 130 mergers over 50 deals. Appendix D provides details about the sample and the construction procedure.

To compute outcomes, we restrict 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 set of stores that report to NielsenIQ. Our price metric is the volume-weighted average monthly price 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.

[6]As discussed in Appendix D.2, market definitions infrequently exclude store brands and divide markets into quality tiers. Removing store brands does not materially affect our estimates.

[7]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.

[8]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 D.1, we document that this procedure leads to high coverage.

Note that we estimate the effect of mergers on retail prices paid by the end consumer rather than on wholesale prices. Not only are these effects of inherent interest, but they also factor into the agencies' assessment of whether to challenge a merger: Section 1 of the 2010 Horizontal Merger Guidelines states "The Agencies examine effects on either or both of the direct customers and the final consumers. The Agencies presume, absent convincing evidence to the contrary, that adverse effects on direct customers also cause adverse effects on final consumers." We cannot provide evidence of adverse effects on direct consumers—retailers—without a model of retailer pricing. This is a common limitation of work studying markups (Atalay et al., 2023a; Döpper et al., 2022) or mergers (Miller and Weinberg, 2017) using scanner data.[9] Nevertheless, by documenting effects on final consumers we pin down an object of interest to antitrust agencies.

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 coarser than our product market definitions (in Table D.1) but serves to illustrate in which broad product categories the mergers are taking place.[10] 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, but several mergers have values over 200. Panel (c) shows that the mergers with the highest values of DHHI tend to have post-merger HHI levels between approximately 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.

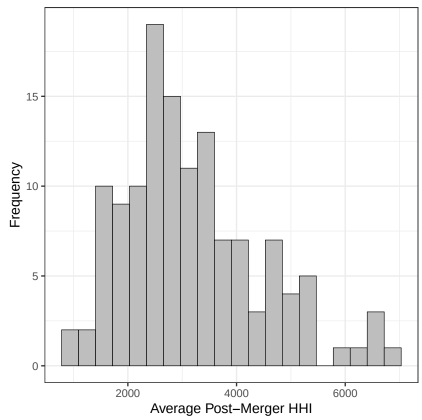
[9] At the very least, we expect retail prices to be positively correlated with wholesale ones. In fact, research has documented passive cost-plus pricing by retailers, including full passthrough of costs (De Loecker and Scott, 2022) and lack of response to demand elasticities (Anderson et al., 2023; Arcidiacono et al., 2020; Butters et al., 2022).

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

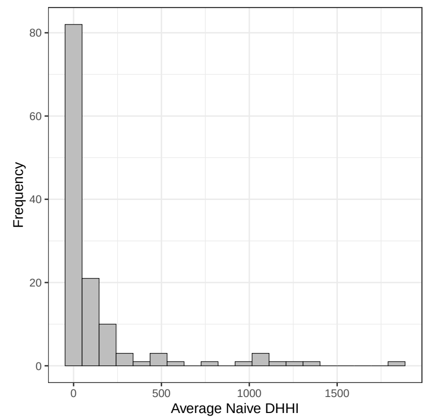
Product Group Name	N	Product Market Sales (Million USD / yr)	Merging Parties' Revenue Share	HHI	DHHI
All	130	500.6	19.8	3172.6	141.7
Baby Food	1	1436.3	12.9	4865.5	117.1
Baked Goods-Frozen	1	4.0	53.6	6683.3	66.6
Beer	2	2912.1	29.9	4270.1	527.6
Bread And Baked Goods	15	651.0	17.1	3785.8	94.9
Breakfast Foods-Frozen	1	286.9	2.9	2685.9	1.0
Candy	4	1249.7	13.0	1768.0	52.2
Cereal	2	695.7	7.5	2521.0	23.8
Coffee	2	951.0	20.0	2315.7	24.3
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	1	1765.4	11.0	3061.2	187.3
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	744.8	46.6	3858.0	106.8
Hair Care	7	351.9	21.6	2607.8	514.8
Housewares, Appliances	1	25.9	50.9	6856.3	11.2
Kitchen Gadgets	1	136.5	23.0	1164.7	90.4
Laundry Supplies	1	119.2	14.5	3157.7	440.0
Liquor	11	311.4	4.7	2512.8	25.6
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	7	779.8	10.1	2386.7	22.7
Pet Food	4	645.9	24.5	2989.6	92.6
Pickles, Olives, And Relish	3	49.7	18.1	2984.7	47.8
Pizza/Snacks/Hors Doeurves-Frzn	1	1593.9	42.1	2731.1	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	10	565.3	12.7	2738.2	35.3
Soft Drinks-Non-Carbonated	1	2328.9	16.7	2842.6	16.6
Spices, Seasoning, Extracts	5	133.7	48.7	3592.4	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	3	22.6	11.9	4554.1	6.2
Vegetables And Grains - Dried	1	80.5	62.6	4877.1	1079.8
Wine	1	1565.1	22.0	2257.0	27.5

Table 1: Summary statistics for the final sample of mergers

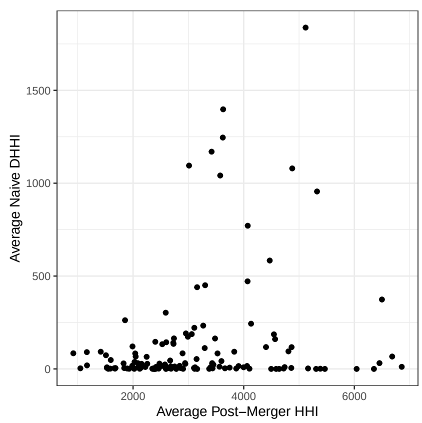
Around half of the mergers with DHHI over 500 are small, with average yearly sales for the merging parties below $100 million, but several feature DHHI near 500 and yearly sales around $1 billion. These patterns are consistent with the selection process determining merger consummation: we expect greater antitrust scrutiny on mergers involving large product markets and high values of DHHI and post-merger HHI. Nevertheless, mergers involving substantial increases in naive DHHI have been



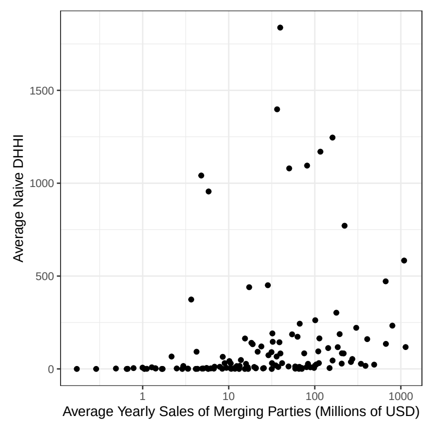
(a) Average Post-Merger HHI



(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

approved, even in large product markets.

III. The Effects of Consummated Mergers

III.A. Empirical Strategy

We take two approaches to estimate the effect of mergers on the outcomes of interest. The first approach is 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. First, 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-year fixed effect. This regression allows us to identify a brand-specific time trend after controlling for 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, obtained from (1). We use a two-step process so that the pre-trend is not contaminated by post-merger changes. The coefficients of interest are β_1 and β_2, which give the average difference in the outcome between the realized value and its prediction using pre-merger data for merging and non-merging parties. In some specifications, the outcome of interest is an aggregate of both parties and 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 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 130 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. First, we do not find significant patterns in pre-period outcomes after controlling for the linear time trend, which is not a mechanical effect of this procedure. This provides evidence that a linear time trend is an appropriate control for the evolution of prices (due to demand and cost changes) in the pre-period, which bolsters our confidence that this linear time trend would continue to serve as an appropriate approximation to the counterfactual in the post-period. Second, when conditioning on the magnitude of the post-merger change in the outcome, we find that pre-period trends do not drive the most extreme changes: positive estimated price effects are not due to inappropriately controlling for positive pre-trends, for instance.

These timing results also help alleviate endogeneity concerns that some other event (e.g., expecting a new entrant) precipitated both the merger and the outcome changes we document. Not only do we find no departure from a linear trend in the pre-period, but we also find that changes happen soon after the merger is consummated. We find these patterns difficult to explain without attributing them to the merger itself, unless the other events one may be concerned about are systematically coincident with the merger completion dates, which we find unlikely.[11]

As a robustness check, we control for log income per household at the DMA level and for input prices (see Table D.1). Additionally, we use 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

$$\log y_{idt} - \widehat{\log y_{idt}} = \beta_1 \mathbb{1}[\text{Merging Party}]_i + \beta_2 \mathbb{1}[\text{Non-Merging Party}]_i$$
$$+ \beta_3 \mathbb{1}[\text{Merging Party}]_i \mathbb{1}[\text{Treated}]_d$$

[11] We also find that mergers are not systematically completed on "special" days of the year (e.g., starts of quarters). Furthermore, Figure B.2 shows that mergers are distributed across time and are not clustered, for example, during the financial crisis.

$$+ \beta_4 \mathbb{1}[\text{Non-Merging Party}]_i \mathbb{1}[\text{Treated}]_d + \epsilon_{idt}, \quad (4)$$

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 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 a similar approach to study the price effects of insurance mergers.

There are three main drawbacks to applying this strategy in our setting. First, merging parties can lower prices in untreated markets if the merger creates cost synergies at the national level, which 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., 2021) may respond to the merger in untreated markets if those markets share a pricing region with treated markets, again leading to to an underestimate of the merger effect.[12] Despite these concerns, we present results from this specification because they are robust to changes in market conditions that may not be captured by our time trend. 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 none are treated. As a result, we lose 40 out of 130 mergers when using this 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 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

[12] Kim and Mazur (2022) present another concern: mergers may induce changes in prices in untreated markets by affecting the threat of entry. This effect is sizable in their setting of airlines.

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 we conduct our analysis.

We weigh all regressions by pre-merger volume at the brand-DMA level. Appendix C 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, even in the presence of unmodeled heterogeneity in treatment effects. This is the case because the second stage regression does not have covariates. We believe this to be a quantity of interest, especially when 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 B.

We aggregate across mergers by weighing each uniformly, for simplicity of exposition. We verify in Appendix B that results are very similar when using a Bayesian shrinkage procedure to account for estimation error.[13] This is because the magnitude of the standard error on each estimate is considerably less than the variance across estimates for different mergers (see Figure B.1).

[13]For the price regressions, we use two-way clustered standard errors for the second stage by brand and DMA to account for correlation in the prediction error of the left-hand side variable. For quantities, we instead cluster by DMA, as these specifications are estimated at the merging/non-merging level.

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	130	1.51	6.29	-2.34	1.74	5.31
		(0.55)		(0.58)	(0.59)	(0.57)
Merging Parties	130	0.03	8.47	-5.15	0.77	5.86
		(0.74)		(0.97)	(0.97)	(0.85)
Non-Merging Parties	130	2.07	7.11	-2.20	1.93	6.12
		(0.62)		(0.62)	(0.58)	(0.87)
B. Cost and Demographic Controls						
Overall	130	1.68	6.96	-2.54	1.19	5.82
		(0.61)		(0.69)	(0.73)	(0.64)
Merging Parties	130	0.30	9.19	-5.36	0.22	5.53
		(0.81)		(1.07)	(1.07)	(1.02)
Non-Merging Parties	130	2.26	7.64	-2.54	1.78	6.57
		(0.67)		(0.70)	(0.54)	(0.90)
C. Treated/Untreated						
Overall	90	-0.39	3.39	-2.09	-0.25	1.23
		(0.36)		(0.63)	(0.38)	(0.28)
Merging Parties	90	-0.20	5.38	-2.51	0.04	2.66
		(0.57)		(0.41)	(0.53)	(0.51)
Non-Merging Parties	90	-0.28	3.52	-2.19	-0.09	1.20
		(0.37)		(0.64)	(0.41)	(0.22)

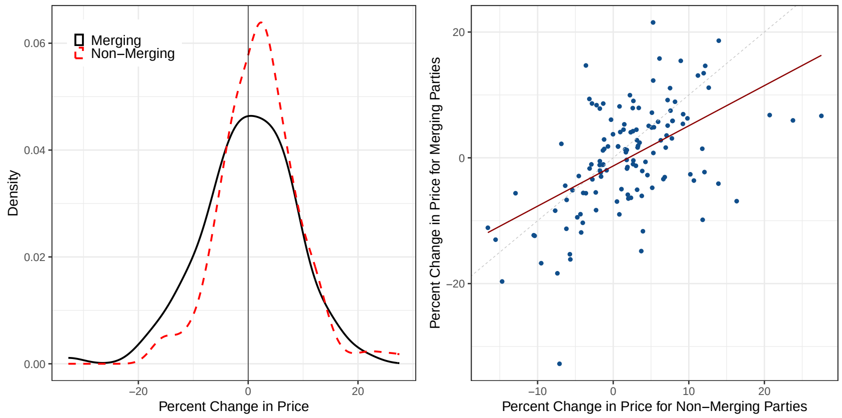
Table 2: Overall Price Effects. This table displays the distribution of transformed coefficient estimates of (2) (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for overall, merging-, and non-merging-party price changes. Standard errors are in parentheses. We use a balanced panel of stores, weigh regressions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

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.

The results from the baseline specification (Panel A) show that mergers have modest price effects: the mean is 1.5%, while the averages for merging and non-merging parties are 0.0% and 2.1%, respectively.[14] However, there is substantial

[14]One should not interpret these means as saying that a "typical" merger leads to a price increase for non-merging parties but no price effect for merging parties. Figures 2 and 3 show that when prices increase after a merger, merging parties typically have larger price changes. When prices decrease, merging parties also have larger price decreases. As we discuss below, this is consistent with a world where mergers lead to both market power and synergies, heterogeneously across mergers.



(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.

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%. The 75th percentile of price changes is similar for non-merging parties, but the 25th is much larger. To complete the picture, Panel (a) of Figure 2 presents the distribution of price changes. Merging parties are more likely to lower prices drastically than non-merging parties, while the probability of substantial price increases is similar across the two groups. This discrepancy drives the difference in average price effects; differences in median price changes are more muted. One potential explanation is cost synergies that are large enough to induce the merging parties to lower prices.

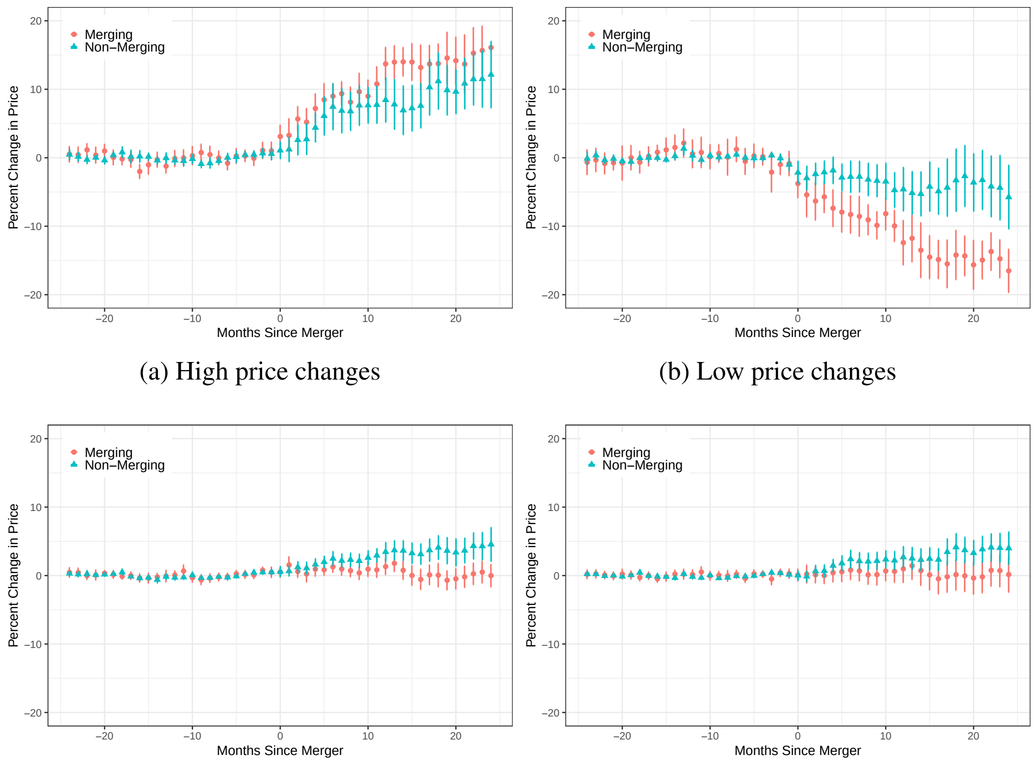
Panel (b) of Figure 2 depicts the correlation between price changes for merging and non-merging parties. Price changes are positively correlated (correlation = 0.54, s.e. = 0.07), consistent with strategic complementarity. For example, non-merging parties lower prices by 7.3%, on average, when merging parties lower their prices by 10% or more, and non-merging parties raise prices by 8.3% on average when merging parties increase their prices by 10% or more. We also find that 28% of mergers lead both merging and non-merging 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, 41% 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, 22% of mergers cause merging parties to lower prices and non-merging parties to raise them, and 10% cause the converse. Changes in the product portfolio or market segmentation can explain this result. 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.

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 quartile of price increases, Panel (b) for those in the bottom quartile, and Panel (c) for the remainder. These results shed light on how quickly merging parties begin to increase prices, how long it takes their rivals to respond, and how long it takes until cost synergies are passed through. 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 the merger for each of the three categories of price changes.[15]

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. 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 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). Heterogeneity in the time required to realize synergies could explain the gradual decline in prices. In both cases, rival

[15]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

(c) Stable prices

(d) All 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 a 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.

prices follow suit, although their price changes are smaller.

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. Non-merging parties steadily increase their prices post-merger after holding them constant for roughly two years before the completion date.

III.C. Quantities

While most merger retrospectives have focused on prices, another natural question is whether mergers have reduced transacted quantities. Conventional intuition suggests

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
Overall	130	-2.46	9.02	-6.87	-1.93	2.80
		(0.79)		(0.66)	(0.74)	(0.70)
Merging Parties	130	-7.07	27.42	-20.96	-5.61	5.71
		(2.40)		(3.70)	(1.95)	(1.93)
Non-Merging Parties	130	-1.45	10.04	-6.37	-1.86	4.09
		(0.88)		(0.72)	(0.86)	(1.05)

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

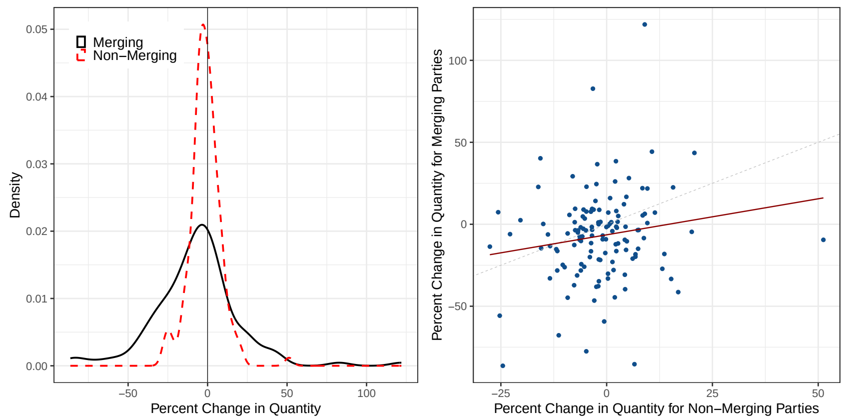
that even if a merger has a small price effect, a significant drop in quantity may indicate adverse welfare effects (Lazarev et al., 2021).

To compute quantity effects, we aggregate to the DMA-month-firm type level, where a firm type is merging or non-merging, and use as the 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 2.5% on average. Moreover, 64% of mergers lead to total quantity reductions. Merging parties exhibit larger quantity drops than non-merging parties, with averages of 7.1% versus 1.5%. The quantiles reported in Table 3 and Figure 4 indicate that distributions of quantity changes are slightly left-skewed: the median decrease for merging parties is 5.6%, 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. (2023) show that mergers can induce supply disruptions, which could reduce quantity. Since the welfare interpretation of a quantity decline changes if part of the drop is transitory, in Figure B.3, we study the time path of quantity changes. 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.

Panel (b) of Figure 4 shows that quantity changes for merging and non-merging

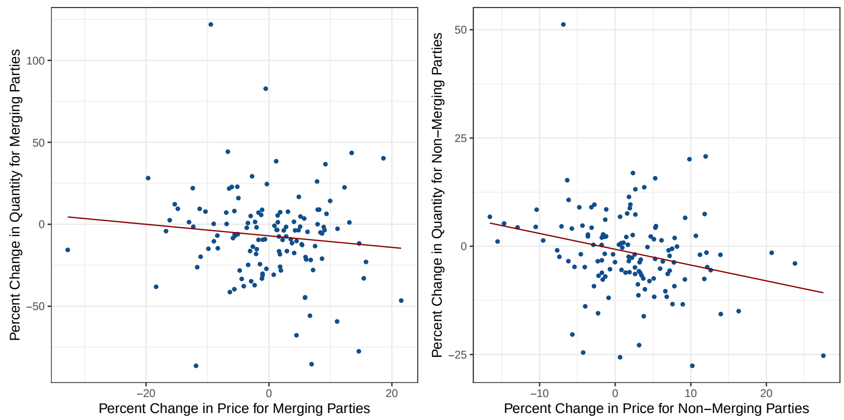


(a) Distribution of quantity changes (b) Scatter of quantity changes

Figure 4: Quantity changes for merging and non-merging parties, as estimated by (2). This plot displays 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. The distributions in Panel (a) and best-fit line in Panel (b) assume equal weights across mergers.

parties have a slight positive correlation (correlation = 0.16, s.e. = 0.09). The empirical result is at odds with results on demand systems with the "type aggregation property" (Nocke and Schutz, 2018, 2024), where one can show that mergers would lead to negatively correlated quantity changes. However, we are not aware of predictions for how mergers affect quantities of merging parties and competitors in multiproduct Bertrand pricing games with general demand systems. Furthermore, in our setting, firms can respond on dimensions beyond simply price, as we document below.

Are these quantity decreases driven by price increases? Figure 5 plots the estimated quantity effects against the estimated price effects for merging (Panel (a)) and non-merging parties (Panel (b)). We find that price and quantity changes are negatively correlated, although not significantly so for merging parties. The correlation for merging parties is -0.11 (s.e. 0.09) and for non-merging parties is -0.26 (s.e. 0.09). Moreover, the fact that in many mergers average prices and total quantities move in the same direction highlights that average prices do not tell the whole story, particularly for merging parties. We investigate other effects next.



(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.

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 results for changes in the number of stores in which at least one product was sold. Non-merging parties minimally change their network of stores. In contrast, mergers lead to a 1.8% reduction in the number of stores served by the merging parties, on average, but there is substantial heterogeneity in these effects.

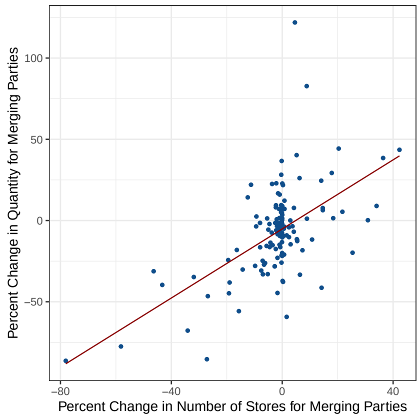
In 37% of mergers, store networks expand beyond the union of the pre-merger networks. This is consistent with the pro-competitive argument that economies of scale and production reallocation may make it profitable to increase the set of stores where products are offered. Panel (a) in Figure 6 shows that 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, 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

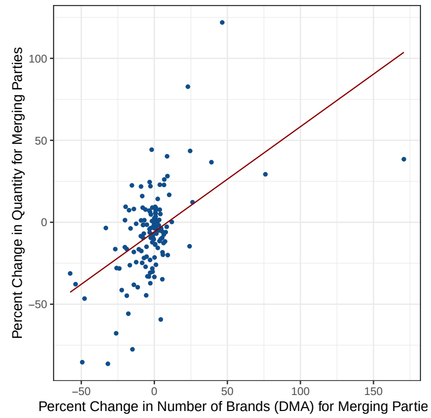
	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Number of Stores						
Overall	130	-0.30	2.01	-0.67	-0.16	0.06
		(0.18)		(0.13)	(0.05)	(0.03)
Merging Parties	130	-1.79	14.90	-4.25	-0.35	1.60
		(1.31)		(1.26)	(0.12)	(0.72)
Non-Merging Parties	130	-0.16	2.25	-0.23	0.00	0.07
		(0.20)		(0.05)	(0.01)	(0.01)
B. Number of Brands (DMA)						
Overall	130	-3.26	8.79	-7.98	-3.49	0.95
		(0.77)		(1.16)	(0.65)	(1.04)
Merging Parties	130	-2.03	22.23	-8.89	-1.40	3.50
		(1.95)		(1.41)	(0.85)	(1.14)
Non-Merging Parties	130	-3.08	9.75	-9.08	-3.13	1.88
		(0.86)		(1.54)	(0.72)	(1.03)
C. Number of Brands (National)						
Overall	130	-3.04	6.82	-6.55	-1.97	0.87
		(0.60)		(0.98)	(0.41)	(0.75)
Merging Parties	130	-4.42	12.77	-10.65	-0.28	0.53
		(1.12)		(2.34)	(0.12)	(0.21)
Non-Merging Parties	130	-2.70	6.82	-6.48	-2.22	0.98
		(0.60)		(1.19)	(0.59)	(0.65)

Table 4: Overall Effects on Product Availability. This table displays the distributions of product availability outcomes. Standard errors are in parentheses. 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. We use a balanced panel of stores, weigh regressions using pre-merger volume by firm type-DMA, and aggregate across mergers using equal weights.

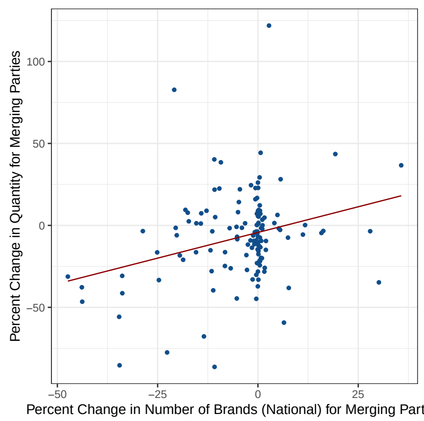
entity should have replicated the merging parties' distribution network if not doing so decreases sales. This could be indicative of contracting frictions, such as breakdowns in negotiating new agreements with retailers, restrictions imposed by exclusivity agreements, or costs of supplying certain stores. Consistent with these frictions, we find that in mergers that lead to bottom-quartile changes in the number of stores, stores served only by the target pre-merger are more likely to be dropped: 37.8% of stores served only by target brands pre-merger are eliminated from the distribution network post-merger, compared to 26.0% for stores served only by the acquirer, and 12.1% for stores served by both. Thus, mergers of firms with non-overlapping distribution networks often lead to the disappearance of products from shelves and



(a) Number of Stores



(b) Number of Brands (DMA)



(c) Number of Brands (National)

Figure 6: Correlates 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.

reductions in quantities sold, suggesting the possibility of consumer harm.

Theory has ambiguous predictions regarding how the merged entity's optimal product portfolio will differ from the combined portfolios of the merging parties. Mergers create incentives to remove duplicative products or ones that cannibalize sales from more profitable alternatives, even if there are some lost sales. An acquirer's goal could even be to eliminate the target's product line, as in a "killer acquisition" (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 2.0% (3.1%) on average following a merger. Considering their national portfolios instead, we estimate that merging parties decrease the number of brands sold by 4.4%, while their rivals decrease the number by 2.7%. Panels (b) and (c) in Figure 6 correlate these changes with changes in quantity. We find a positive correlation between changes in quantity and changes in the number of brands sold both in each DMA (correlation = 0.52, s.e. = 0.08) and nationally (correlation = 0.29, s.e. = 0.08).

One rationalization behind eliminating brands after a merger is that some brands are duplicative in the merged entity's portfolio. Alternatively, the merged entity may desire to eliminate products that cannibalize sales from more profitable alternatives. The fact that we observe quantity declines after brand removal provides evidence in support of the second explanation.

Turning our attention to brand introductions, we find that in 42% 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 41% of mergers lead to national brand introductions, but quantity effects in this case are much more muted.

In summary, we find that reductions in quantity correlate with price increases (albeit insignificantly so for merging parties), reductions in stores served by the merged entity, and reductions in brands sold in a DMA and nationally. These correlations suggest that these reductions in quantity are due to strategic responses by the merged entity. 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 Sections IV and V, we study the interplay between this heterogeneity and the presumptions encoded in the merger guidelines.

III.E. Robustness Checks

We consider two classes of robustness checks to validate our empirical strategy and sample. First, as discussed above, one may be concerned that the stores covered by the NielsenIQ Retail Scanner Dataset are not a representative sample. In Appendix A, we replicate our analysis using the NielsenIQ Consumer Panel Dataset, which covers a sample of households—rather than stores—and therefore captures the retailers that might be missing from the scanner dataset. Second, we examine robustness to a number of other sample restrictions and technical assumptions. Appendix B.1 displays results for prices, and Appendix B.2 does the same for quantities.

Results using the panelist data are broadly consistent with those from the baseline specifications, and we discuss each result in detail in Appendix A. Price effects are modest on average but disperse, price changes for merging and non-merging parties are positively correlated, and the effects begin to materialize upon completion of the merger. However, it is important to note that mergers for which we estimate large price effects using the scanner dataset tend to have smaller effects when using the panelist dataset. This leads to lower average price effects in Appendix A: the mean price change across all mergers is -0.57%. That said, the point remains that the average price effect is small.

Turning to the quantity effects, we estimate a similar share of mergers with large quantity declines using the panelist and scanner datasets, but large quantity increases are more prevalent in the panelist data. Consequently, the average, median, and 75th percentile of the quantity effect distribution are larger (see Table A.2). This difference could potentially be the result of sparse coverage of UPCs in the panelist data for a subset of mergers or be driven by quantity increases in the stores absent from the scanner data.[16]

[16] We also note that the largest quantity effects are estimated noisily. A Bayesian shrinkage procedure aggregating these estimates would lead to a mean quantity change for merging parties that is -3.45% instead of -1.68%.

The second class of robustness checks examines results under different sample restrictions and technical assumptions. We provide a summary of these results here but defer a complete discussion to Appendices B.1 and B.2. Across these robustness checks, price effects are modest on average but disperse: mean price effects are about 1–2 pp away from zero, and the standard deviation of the distribution is between 5–7 pp. Quantity results are noisier: across all but one specification, average quantities drop, although not all are statistically significant. The distributions are still very disperse. This dispersion in effects is of particular importance, as the distribution of effects is more informative of stringency than the mean (Carlton, 2009).

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. To assist in this task, the agencies rely on measures of market structure. Notably, these so-called "structural presumptions" are not enforcement prescriptions but rather meant to be predictive of the potential harm for a merger. 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.

Both the 2023 and 2010 Guidelines detail market structures under which the agencies are likely to presume competitive harm from a merger. The exact thresholds differ across versions of the Guidelines, and we focus our analysis on the thresholds specified in Section 5.3 of the 2010 Guidelines, given these were the presumptions used during our sample period. 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).[17] 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

[17] 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.

"safe harbor" and are "unlikely to have adverse competitive effects."

It is a ripe time to evaluate the structural presumptions. The 2023 Guidelines expand the red zone to mergers with HHI at least 1,800 and DHHI above 100, returning to the values of the 1982 Guidelines. Moreover, the theoretical basis of the structural presumptions has been a focus of recent work. Some 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"), but no such relationship exists for levels of HHI. Nevertheless, there may be reasons HHI would play a role in the effects of mergers: for instance, Loertscher and Marx (2021) and Nocke and Whinston (2022) note that HHI has been used to indicate the potential for coordinated effects. However, they also question this practice, arguing that more evidence on HHI screens is needed.

We provide such evidence by computing correlations between price changes and the structural presumptions. This analysis teaches us how consummated mergers' average price effects change across market structures given today's enforcement landscape, holding fixed the process that leads to parties proposing mergers and the agencies "approving" them (i.e., allowing them to complete, or challenging them unsuccessfully). For us to observe a merger with large values of HHI and DHHI, say, the parties must have thought this merger would both be profitable and likely to be approved ("selection into proposal"), and the agencies or a court must have agreed that the merger would not harm consumers ("selection into approval").

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. Table 5 displays the results. Columns (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 proposal and approval. As discussed above, the relation between HHI and price changes is zero in some theories or positive in others. However, the data-generating process likely selects high-HHI mergers that will not result in drastic price increases (e.g., ones with plausible synergies). We

	Merging			Non-Merging			Aggregate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
HHI (0–1)	-14.69			-9.73			-13.32		
	(5.88)			(7.33)			(4.70)		
DHHI (0–1)	25.10			68.93			48.22		
	(17.62)			(27.81)			(20.78)		
HHI $\in$ [1500, 2500]		-0.58			-4.26			-3.90	
		(4.39)			(2.49)			(2.76)	
HHI $>$ 2500		-5.43			-7.34			-7.37	
		(4.33)			(2.39)			(2.64)	
DHHI $\in$ [100, 200]		2.08			1.91			2.04	
		(1.92)			(1.36)			(1.16)	
DHHI $>$ 200		3.50			5.80			4.69	
		(1.74)			(1.86)			(1.41)	
Yellow			0.99			1.06			1.11
			(1.67)			(1.24)			(1.07)
Red			1.29			4.31			2.97
			(1.68)			(1.89)			(1.45)
Constant	4.34	3.03	-0.28	4.19	7.12	1.34	5.06	6.62	0.95
	(1.97)	(4.16)	(0.96)	(2.11)	(2.21)	(0.75)	(1.52)	(2.52)	(0.70)
N	130	130	130	130	130	130	130	130	130

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.

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 aforementioned selection could dampen 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 be smaller when HHI is especially large. Finally, Column (3) regresses against dummies for the average market structure being in 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. These price changes are more strongly correlated with average DHHI and with the red region.

We explore robustness checks in Appendices A and B. First, computing HHI and DHHI using nationwide market shares yields similar results (Table B.4), although the coefficient on DHHI for non-merging parties' and aggregate price changes declines in magnitude. Second, we study whether price changes for mergers that proceeded with divestitures are different. Dropping these mergers from the analysis (Table B.5) dampens the correlation with DHHI for merging and non-merging parties, and the correlation of price changes with the red zone becomes statistically insignificant. We discuss these mergers in more detail in Section V.A below when connecting price effects to antitrust enforcement. Third, using the panelist data to estimate price changes dampens correlations with HHI but correlations with DHHI and the red zone are broadly similar (Table A.3). Overall, 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.

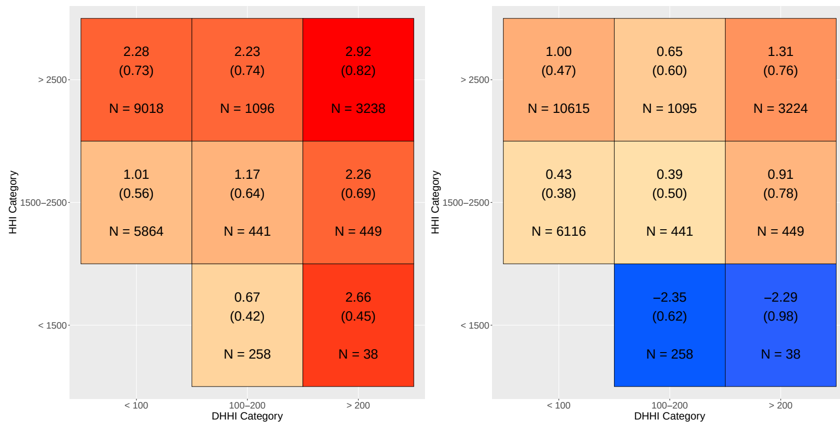
IV.B. Within-Merger Analysis of Price Changes

We next investigate price changes within merger across DMAs. Agencies can take into account damages in specific markets even when a merger has small effects elsewhere. This includes geography-specific remedies, which we observe once in our sample. Exploring whether the same structural presumptions can guide these decisions is policy-relevant.

The patterns we identify cross-merger might not hold within-merger. First, 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. 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.

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 $(100 \cdot (\exp(\hat{\beta}_{1d}) - 1))$ on merger fixed effects and dummies for the region of (HHI, DHHI) plane in which the DMA lies. Figure 7 reports estimates for these dummies. The top right bin represents the red region, the three bins around it together form the yellow region, and all others represent the green region. The number and 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. First, price changes are positively correlated with DHHI. For each bin of HHI, we reject the null hypothesis that markets with DHHI above 200 have the same price effect as those with DHHI between 100 and 200 with at least 95% confidence. Table B.3 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 larger price increases for high levels of HHI, regardless of DHHI. These findings lend credence to the use of HHI screens, which may be surprising since Nocke and Whinston (2022) find that compensating efficiencies are not a function of 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.

The qualitative relationships with HHI and DHHI for non-merging parties (Panel (b)) are typically consistent with those for merging parties. However, the difference in price changes is more muted and often not significant.[18]

In Appendix A.3, we repeat this within-merger analysis using the Consumer Panel and do not find significant correlations of price changes with geography-level measures of market structure (Figure A.7). While this provides an important caveat to our results, we should note that the Consumer Panel analysis requires us to define markets at the Scantrack level, which are often significantly larger than DMAs and we believe are less likely to be representative of geographic markets.

Taking stock, in the baseline specifications, we find a consistent relationship between DHHI and price changes both across- and within-merger. Within-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. 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.

V. Antitrust Enforcement

Carlton (2009) points out that small average price changes do not necessarily indicate strict antitrust enforcement. Consider a world where merger effects are 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

[18] 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.

effects of the marginal merger to discuss the stringency of antitrust enforcement. We estimate this level of stringency through the lens of an empirical model of the agencies' decision to challenge a merger. We then simulate outcomes under alternate stringencies, which change both the set of mergers selected into "approval" and the types of mistakes made by the agencies.

We focus on price changes as the variable of interest as agency filings and court exhibits highlight that they are a primary focus of antitrust analysis in this industry, and the literature on mergers retrospectives has commonly focused on prices (Asker and Nocke, 2021). Agencies may also consider other aspects when deciding whether to challenge a merger. A natural alternative would be to focus on consumer surplus changes instead. However, estimating consumer surplus requires demand estimation, which is outside the scope of this paper.

V.A. How Stringent is US Antitrust Enforcement?

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.[19] 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, they also learn a noisy signal p_i of p_i^* through due diligence: for instance, the agency may learn that a particular merger is especially likely to lead to synergies and thus have a sense that prices would increase by less than would be expected only given X_i. Based on this 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 in Section III of Carlton (2009).

One could view this as a reduced-form of a model in which agencies choose to challenge if the net benefit of winning a case times the probability of winning the case exceeds some cost K. The net benefit and the probability of winning could

[19]We average across DMAs since only one challenge in our sample has a geography-specific remedy.

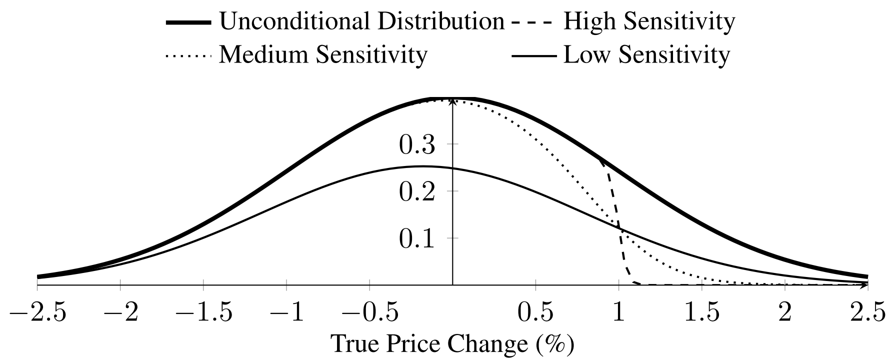


Figure 8: Illustration of the identification of model parameters. We illustrate densities of price changes of approved mergers, normalizing them to integrate to the probability of approval, for three potential sets of model parameters.

depend on the posterior mean $\mathcal{E}$ as well as (X_i, Z_i). This cost K could capture both legal and administrative costs and any shadow cost from an agency budget constraint. If the expected net benefit (i.e., probability times the net benefit) is increasing in $\mathcal{E}$ conditional on (X_i, Z_i), which we expect is the case, then we arrive at a model where the agencies challenge mergers with sufficiently large expected price changes.[20]

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. Moreover, challenges are indeed at the merger level: agencies can (and do) propose divestitures in individual product markets without blocking the entire deal. In our setting, we identify six mergers (from four separate deals) in which an agency proposed a remedy for a horizontal market power concern. Additionally, SDC Platinum identifies two deals, corresponding to four mergers, that were proposed and later withdrawn due to antitrust concerns raised by the DOJ or FTC. We codify these four blocked mergers and the six 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.

[20]Note that this model is an interpretable parameterization of a more general model in which the agency effectively has a probability $\lambda(p_i^*, X_i, Z_i)$ of challenging a merger with true price change is p_i^* and observable characteristics (X_i, Z_i). The randomness in this decision, from the perspective of the econometrician, could come from two sources: (i) noise between p_i^* and $\mathcal{E}$ or (ii) characteristics that are unobserved to the econometrician but used in the agencies' decision. Both sources would be captured in our estimate of the correlation between p_i and p_i^*, using the notation below.

To gain intuition for identification, suppose we observe the true price changes for consummated mergers and that a merger-specific property Z_i affects the agencies' threshold $\bar{p}(\cdot)$ but not the prior distribution of expected price changes. 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 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 for approved mergers (which would be observed in the data); we can normalize this distribution so that it integrates to the probability of approval given Z_i. The dashed distribution 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: the probability of challenging a merger is very low to the left of 1% and rises sharply at 1% to nearly 1. Here, we would estimate that the agency is trying to prevent mergers with price changes above 1% and that they are successful: p_i correlates strongly with p_i^*, and the threshold is about 1%. In the parameterization introduced below, σ_ϵ would be small and $\bar{p}(Z) = 1\%$. 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; the probability of challenging a merger is fairly flat as a function of the true price change. Here we would conclude that p_i is a very noisy measure of p_i^* (large σ_ϵ). If the probability of challenging a merger is high, we would further conclude that there is a strict threshold (low $\bar{p}(Z)$). The dotted line illustrates an intermediate case.

We impose parametric restrictions for estimation. 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. We 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: scaling a market up changes the welfare impact of the merger, which we expect to impact the agencies' decision, but not its price impact.[21] Second, we do

[21] We are assuming that merger proposal is not affected by size. This may be implausible for especially

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.

If a divestiture was imposed by the agencies or the merger was blocked, then all we know is that the agencies' posterior mean based on the signal p_i exceeds the threshold $\bar{p}(Z_i)$.[22] For unchallenged mergers, the reverse is true. Moreover, for these mergers, we observe a noisy measure of the true price change from the exercise conducted in Section III, where the noise is due to statistical error. 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.[23] We estimate the model via maximum likelihood.

The outcome variable of interest is either aggregate price changes or price changes for merging party products. The former is motivated by the fact that agencies also take into account how non-merging firms will respond to the merger.[24] The latter is motivated by our reading of publicly-available filings and reports, which sometimes focus solely on merging parties.

Panel A of Table 6 shows estimates of the mean of the prior, using the same parameterizations as in Table 5. Column (1) shows that the unselected price changes (i.e., correcting for selection into approval) increase with DHHI: a 100-point increase in DHHI correlates with a 0.66 pp larger expected increase in price. We also find a negative relationship between the HHI and price changes, although this correlation is small: a 1,000-point increase in post-merger HHI corresponds to a 0.9 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

large mergers, but since the largest mergers scrutinized by the agencies during our time period are much larger than those in our sample, we do not view this as a restrictive assumption.

[22] We also observe noisy 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.

[23] In this sense, the model has similarities to a Bayesian shrinkage procedure. Although not the object of interest, the model's posterior expectation of the true change p_i^* will be a combination of of $X_i'\beta$ and $\hat{p}_i$, where the relative weights depend on σ_i as well as the estimate of σ_p^*.

[24] The 2010 Horizontal Merger Guidelines state "Where sufficient data are available, the Agencies may construct economic models designed to quantify the unilateral price effects resulting from the merger. These models often include independent price responses by non-merging firms" (p. 21).

	Aggregate Price Changes			Merging Party Price Changes		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Prior						
Avg HHI (0–1)	-9.08			-9.50		
	(4.51)			(6.29)		
Avg DHHI (0–1)	66.02			68.68		
	(18.22)			(26.16)		
HHI $\in$ [1500, 2500]		-3.53			-0.11	
		(3.01)			(3.75)	
HHI $>$ 2500		-5.61			-3.11	
		(2.99)			(3.73)	
DHHI $\in$ [100, 200]		2.91			3.19	
		(1.71)			(2.26)	
DHHI $>$ 200		6.55			6.97	
		(1.64)			(2.15)	
Yellow			2.31			1.64
			(1.61)			(2.06)
Red			5.35			3.77
			(1.67)			(4.35)
Constant	3.70	5.34	0.71	2.63	1.48	-0.49
	(1.53)	(2.88)	(0.64)	(2.14)	(3.57)	(0.79)
B. Errors and Uncertainty						
σ_{p^*}	5.97	5.99	6.04	7.92	8.07	7.81
	(0.45)	(0.44)	(0.47)	(0.61)	(0.62)	(0.58)
σ_ϵ	4.26	2.75	4.27	8.75	5.53	18.44
	(2.83)	(1.70)	(3.12)	(6.46)	(3.02)	(29.15)
Posterior Standard Deviation	3.47	2.50	3.49	5.87	4.56	7.19
	(1.50)	(1.27)	(1.67)	(1.88)	(1.66)	(1.64)
C. Threshold						
Log(Total Merging Sales)	-1.13	-0.96	-0.92	-1.36	-1.43	-0.58
	(0.55)	(0.53)	(0.56)	(0.78)	(0.71)	(0.97)
Constant	10.22	11.03	9.98	9.90	11.93	5.59
	(2.27)	(1.54)	(2.47)	(4.28)	(2.75)	(7.72)
D. Sales-Weighted Thresholds						
Average	8.34	9.45	8.46	7.64	9.57	4.63
	(2.08)	(1.50)	(2.14)	(3.49)	(2.34)	(6.25)
Q1	7.24	8.51	7.56	6.31	8.18	4.06
	(2.20)	(1.73)	(2.17)	(3.29)	(2.43)	(5.84)
Q3	9.15	10.13	9.11	8.62	10.59	5.04
	(2.22)	(1.53)	(2.33)	(3.93)	(2.58)	(6.65)

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

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. Results for the prior for merging party price changes (Columns (4)–(6)) are similar, but estimates are noisier, particularly for the effect of the red zone.

Comparing the results in Panel A with those in Table 5, we estimate that DHHI correlates more strongly with the prior than with realized price changes. For instance, the coefficient on average DHHI in Column (1) of Table 6 is 37% 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. The agencies' actions against those with especially large price changes dampen the realized correlation. Results in Table B.6 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). In the baseline specification, these estimates together imply that the agencies' ex-ante prediction of the price change of any merger—a combination of both the information from the prior and the signal—has a standard deviation of 3.5 pp. Instead using merging party price changes, the agencies' prediction is noisier, with a standard deviation of 5.9 pp. This may be consistent with an additional difficulty in predicting other effects that may be specific to merging parties, such as merger-specific synergies. These estimates feed into our analysis of type I and type II errors in counterfactual antitrust enforcement in Section V.B.

Panel C reports estimates of the threshold function. A 10% increase in merging party sales leads to a 0.09–0.11 pp decrease in the threshold, consistent with the intuition that agencies are stricter for larger mergers. The dependence of the threshold on sales is typically significant at at least the 10% level. Note, however, that merging party sales in Column (6) do not shift the threshold significantly. Since the model is identified leveraging variation in the threshold induced by this shifter, we will not draw further conclusions from this specification.

Panel D summarizes these estimates. We find a sales-weighted average threshold of between 8.3% and 9.5% in our sample: on average, agencies challenge mergers in CPG 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 8.5%. The third quartile (i.e., for the smaller mergers in our dataset) amounts to between 9.1% and 10.2%. Using merging party price changes as the outcome, we find comparable thresholds in Columns (4) and (5).

Robustness and Discussion. Our main finding is an estimate of antitrust stringency: an average-sized merger in our sample would warrant a challenge if the agencies expect it to have a price effect in the range of 8–9%. In Panel (a) of Figure B.6, we study how these estimates change when using price changes from all the alternative specifications discussed in Section III.E. We find sales-weighted thresholds ranging from 4.0 to 7.9 pp, slightly lower than our baseline estimate but still within the confidence interval. Moreover, we find similar levels of heterogeneity in thresholds across mergers, with interquartile ranges from 0.9 pp to 2.3 pp. Finally, the average thresholds are significantly larger than zero at at least the 10% level across all specifications.

To our knowledge, this is the first direct estimate of this threshold, so benchmarking it is difficult. One possibility is to use published merger retrospectives: 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, as 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)), although the industries under consideration are quite different.

Another option is to compare to estimates of publicly available predictions of price effects by the agencies and their experts. Since we only observe these predictions when a merger goes to court, the predicted price effects must be larger than our estimated thresholds. We have gone through DOJ case filings to 2006 and aggregated any such prediction we can find—from expert reports, trial exhibits, and court opinions. Table 7 presents these predicted price effects and our estimated price

Merging Parties	Year	Market Size	Predicted Price Effects	Threshold (%) Aggregate	Threshold (%) Merging
H&R Block / TaxACT	2011	\$3B	2.2–2.5% (H&R); 10.5–12.2% (TaxACT)	5.49 (2.50)	4.20 (3.02)
GE / Electrolux	2015	\$2.7B	5–11% (ranges); 3–21% (cooktops); 4–15% (wall ovens), all for merging parties	5.61 (2.47)	4.35 (3.01)
Aetna / Humana	2015	—	60% premium increase (10–74%)	—	—
Sysco / US Foods	2015	\$56B	3–4% marketwide; 4.2–4.5% for merging parties	2.18 (3.64)	0.22 (3.89)
Energy Solutions / WCS	2017	\$100M	15% (from internal documents) for merging parties	9.34 (2.14)	8.85 (3.88)
AT&T / Time Warner [a]	2018	\$68B	27¢ per month (10–50¢) on a typical cable bill, for merging parties	1.96 (3.73)	-0.05 (3.98)
		\$3.6B	16% in carriage fees charged by merging parties	5.29 (2.55)	3.96 (3.03)
Hackensack Meridian / Englewood	2021	—	5.7% for merging parties	—	—
US Sugar / Imperial	2022	\$1.2B	5.4–5.7% for merging parties, with coordinated effects; 1.9–2.1% for competitors	6.53 (2.26)	5.46 (3.05)
Penguin Random House / Simon & Schuster	2022	\$490M	4.3–11.6% (up to 7.3–19.2%) for merging parties	7.54 (2.12)	6.69 (3.25)
JetBlue / Spirit	2023	\$9.3B	30% (from internal documents) for merging parties	4.21 (2.89)	2.67 (3.19)

Table 7: Predicted price effects of various mergers, compared to calculated thresholds using our baseline specifications (Columns (1) and (4) in Table 6). Market size refers to our best estimate of total sales of merging parties in the year prior to the merger. Standard errors are in parentheses. (a) AT&T / Time Warner is a vertical merger, and we report estimated effects on final cable consumers in the first row and effects on carriage fees in the second.

thresholds, given the relevant market size;[25] Appendix B.4 describes the underlying data collection procedure and provides additional details. For almost all the mergers for which we can find information, we find that our estimated threshold is lower than the prediction of the price change from the agency—even when the agency estimates a modest change. Of course, these comparisons must be made with caution: industries are different and market sizes are often larger than those in the sample, agency predictions may not have taken into account synergies (although the agencies'

[25]We do not have market sizes for two mergers. In Aetna / Humana, the market of interest was Medicare Advantage plans in a specific set of counties, and we do not have easy access to the data needed to compute sales in those markets. Any reasonable threshold would be below the predicted price effect of 60% here. Hackensack Meridian / Englewood was a hospital merger, and the point of contention was negotiations with insurance companies. We are not sure what the appropriate definition of sales would be here.

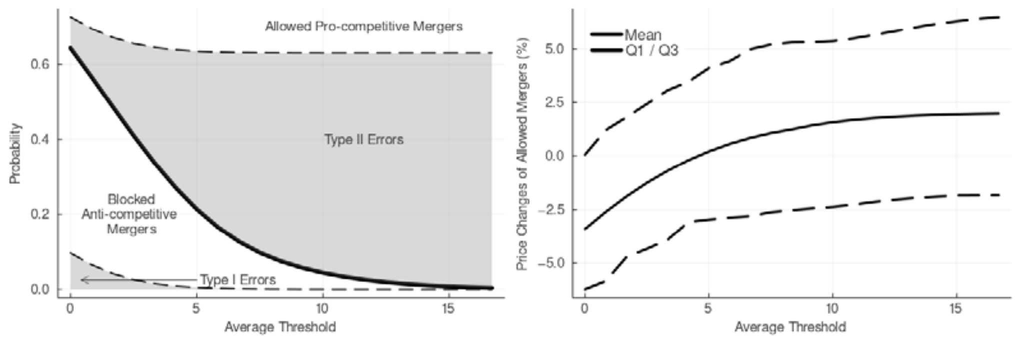
position is often that there are no cognizable efficiencies), and other factors surely affect agency decisions. Moreover, cases brought to trial may be especially far from the relevant threshold. Finally, the estimated thresholds are noisy. Nevertheless, we believe that this lends more credence that our analysis is not at odds with agency behavior.

Finally, readers and seminar participants have asked whether aspects of the political environment affect the threshold. We cannot include the administration in our threshold as almost all divestitures in our sample happen during one administration. Instead, we use funding of the agencies as a proxy for attitudes on enforcement and find suggestive evidence that thresholds becomes tighter as funding increases: point estimates are similar when using the scanner (Table B.7) or the panelist dataset (Table A.5) but only significant when using the panelist data. Proxies of workload, such as the number of Hart-Scott-Rodino filings in that fiscal year, are only noisily correlated with the threshold.

V.B. Counterfactual Outcomes Under Alternative Stringencies

Given the estimated threshold in Section V.A, is antitrust scrutiny excessively lax? In a world where the agencies can perfectly predict the price effects of mergers, a stricter threshold yields a direct trade-off between higher costs of enforcement and blocking more anti-competitive mergers. Without any costs of enforcement, any positive threshold would thus be too lax (to the extent that price changes are the object of interest for the agencies).

This logic does not extend directly to the case where agencies have imprecise forecasts of price changes: beyond the trade-off above, a stricter threshold would change the probability of errors. 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). The opposite mistake of letting an anti-competitive merger go unchallenged is called a "type II error." Tightening the threshold necessarily leads to the (unintended) consequence of type I errors and the potential benefit of fewer type II errors; the relevant question is by how much. An agency would then have to choose the relative weights it places on making each of these errors. Our model allows us to quantify elements of the



(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 B.5 shows confidence intervals.

trade-off in both current and counterfactual regimes, which we believe is a novel contribution to the study of antitrust enforcement.

In this section, we consider scaling the thresholds by a factor, e.g., all thresholds become 10% smaller. For each counterfactual threshold, we compute the probability of challenging a merger in our sample as well as probabilities of making errors.[26] We also compute the distribution of price effects for allowed 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 8.3% would almost triple the number of challenges. Reducing the threshold to 0% would lead the agencies to challenge almost two-thirds of proposed mergers. These observations align with the distributions presented in Table 2, as over half of the mergers in our sample have a positive aggregate price impact. This quantifies the additional burden to the agencies from tightening stringency.

Which mergers would get screened out from a change in the threshold? Panel (b) answers this question by plotting the mean and first and third quartiles of the price changes of consummated mergers for different threshold levels. Tightening the

[26] We conduct the exercise in-sample by computing 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.

threshold to 5% would reduce the aggregate price change for consummated mergers by about 1 pp, to 0.2%. Moving to a 0% threshold would lead to almost 75% of consummated mergers causing price decreases. The cost of loosening the threshold is 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 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 with the threshold. If laxer thresholds induce the proposal of worse mergers, our estimated price effects are lower bounds. Conversely, if stronger thresholds dissuade some of the observed mergers from being proposed, our estimated increase in administrative burden is an upper bound.

Turning to errors, 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 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%, 15% of blocked mergers are type I errors. 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-fourth at 0%.

These main observations hold across different specifications of the estimates of price changes. The probability of type I errors is rare and generally predicted to be less than 10% even at a threshold of 0% (except in one specification). At a threshold of 5%, about 43–58% of approved mergers are due to type II errors, and this number is generally in 20–30% at a threshold of 0%. Panels (b) and (c) of Figure B.6 show these results and those for price changes for consummated mergers.

Our estimates indicate that small increases in antitrust stringency would reduce price effects of the average consummated merger modestly and would reduce the prevalence of type II errors. The main cost is not an increase in type I errors as one may have feared but rather the "direct" cost of a significant increase in burden on the agencies—unless increased stringency leads to fewer mergers being proposed. How an agency balances these trade-offs is a function of the weights they place on errors and the cost of challenging mergers, and the quantification we perform here informs such a cost-benefit analysis.

VI. Conclusion

This paper has two main contributions. First, we document how a comprehensive set of mergers in US CPG have affected prices, quantities, and other outcomes. 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 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. Tightening standards would lead to fewer type II errors without a corresponding increase in the prevalence of type I errors. However, it would result in a significantly higher burden on the agencies.

Several avenues for future work stem from these results. First, an interesting question is how 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. The decision of which stores to serve and its interaction with market power seems like a promising avenue for future research.

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