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

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ABSTRACT

How stringent is antitrust enforcement in the United States? We address this question by documenting the effects of completed mergers in consumer packaged goods and predicting how they would change under stricter antitrust regimes. We find that mergers raise prices by 1.5% and decrease quantities sold by 2.3%, on average. Importantly, there is substantial heterogeneity in these effects: a quarter of mergers decrease prices by at least 5.1%, while another quarter increase prices by at least 5.8%. We embed these estimates into a model of antitrust enforcement and find that agencies block mergers they expect will raise prices by more than 8–9%. Many anti-competitive mergers proceed at this threshold; pro-competitive ones are rarely blocked. Lowering the threshold reduces the probability of allowing anti-competitive mergers without a substantial increase in the probability of blocking pro-competitive ones. The cost is that the number of cases the agencies must challenge could increase drastically.

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I. Introduction

Recent years have featured a debate over whether antitrust enforcement has been too lax (Kwoka, 2014; Scott Morton, 2019; Shapiro, 2021; Nocke and Whinston, 2022; Rose and Shapiro, 2022). This question is difficult, as it requires quantifying the outcomes of a representative set of consummated mergers and then predicting what outcomes would have been under stricter antitrust regimes. This paper performs both tasks. First, we document how a comprehensive set of mergers of consumer packaged goods (CPG) manufacturers has affected prices, quantities, and other equilibrium outcomes of interest. Then, through a model of agency decisions, 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. The model allows us to predict the expected price changes of consummated mergers in stricter regimes. It also quantifies the prevalence of allowed anti-competitive and blocked pro-competitive mergers, both in the status quo and counterfactual. These objects provide insights into the implications of stricter enforcement.

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.

We find that the average effect of completed mergers on prices is 1.5%. This average masks substantial heterogeneity: 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. We find that aggregate quantities decrease 2.5% on average. The first quartile of aggregate quantity changes is -6.9%, and the third quartile 2.8%. Merging parties are much more likely to reduce quantity sold: their average quantity change is -7.1%. We show that these quantity reductions are not due to temporary supply disruptions induced by the merger, but rather by changes in firm strategies. In particular, quantity reductions correlate with price increases, reductions in the number of stores served by brands and in their geographic footprint, and the elimination of products at the national level.

The Horizontal 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 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

[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.

observed among consummated mergers would be a measure of stringency.

In reality, this intuition must be adapted to the fact that at the time of making a decision, agencies have at best a noisy estimate of the impact of a merger. Agencies will 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, as it could be difficult to disentangle pro- and anti-competitive mergers ex-ante.

We develop and estimate a simple model of the agencies' decision to propose a remedy for a merger to quantify stringency. 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 the US antitrust agencies aim to propose remedies for CPG mergers with an average price increase greater than 8–9%. 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. Moving to a 5% threshold would reduce aggregate price increases by about 1 pp, have a negligible impact on the probability of blocking a pro-competitive merger, and decrease the probability of allowing anti-competitive mergers. However, this would require the agencies to challenge almost three times as many mergers. To the extent a stricter threshold prevents some parties from even proposing anti-competitive mergers, this estimate of the increase in workload is an upper bound.

Taking stock, we find that stricter antitrust enforcement in US CPG would reduce consumer prices by blocking anti-competitive mergers. The concern that this will also lead to more blocked pro-competitive mergers is unwarranted. These results thus suggest that there are benefits to expanding agencies' capacities to challenge mergers in this space.

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 to aggregate these results, but it is naturally still subject to selection into publication.

To address this issue, some papers have studied a large subset of mergers in a particular industry: Kim and Singal (1993) study 14 airline mergers from 1985–1988, and Focarelli and Panetta (2003) study 43 mergers of Italian banks from 1990–1998. A handful of 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.

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

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

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, 2021; 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 over these papers 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 increasing 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. Nielsen 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 Nielsen designated market areas (DMAs) as our geographic markets: these are collections of counties, usually centered around a major city.

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). 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 Horizontal 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). Alternatively, courts have often also resorted to *Brown Shoe* factors, such as industry

[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.

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 Nielsen categorizations. Nielsen 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 last 40 years.[6] Appendix C.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 Platinu 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

[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. Section III of the 2023 Draft Merger Guidelines lists such "practical indicia" as a method that can be used to determine relevant antitrust markets.

[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.

[6]As discussed in Appendix C.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 C.1, we document that this procedure leads to high coverage.

UPC in the same DMA in the 24 months prior to deal completion.

Table C.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 C 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 Nielsen. 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.

We make two comments about the outcome metrics. First, 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 Guidelines states that "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 data limitation of all work studying markups (Atalay et al., 2023a; Döpper et al., 2022) or mergers (Miller and Weinberg, 2017) using scanner datasets.[9] Nevertheless, by documenting effects on final consumers we pin down an object of interest to antitrust authorities.

Second, our main analysis uses the scanner dataset, which cover a large subset of stores and is skewed towards groceries. Notably, it does not cover all large retailers

[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., 2018; Arcidiacono et al., 2020; Butters et al., 2022).

in the US, and some retailers may have distinct strategies. To circumvent this issue, we re-run our analysis using the Nielsen panelist dataset, which does cover sales from these entities. Moreover, we report the distribution of merger effects restricting to food items, where we would expect better coverage by the Nielsen scanner dataset.

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 C.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. 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 approved, even in large product markets.

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

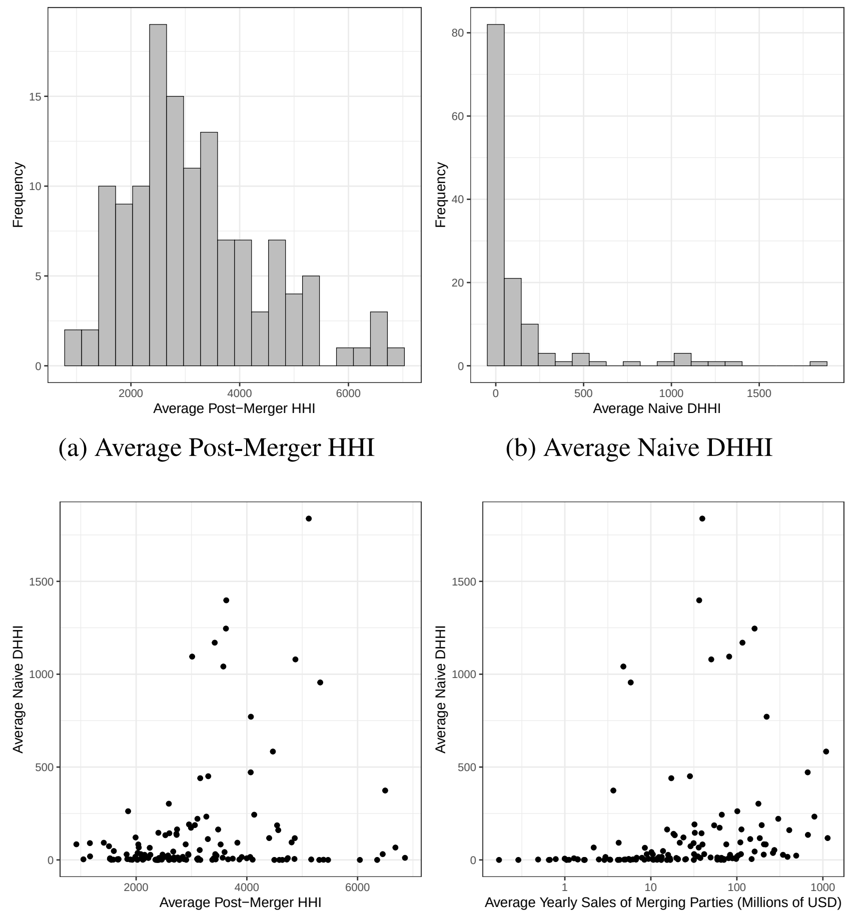
Product Group Name	N	Product Market Sales (Million USD / yr)	Merging Parties' Revenue Share	HHI	DHHI
All	130	500.6	19.8	3172.6	141.7
Baby Food	1	1436.3	12.9	4865.5	117.1
Baked Goods-Frozen	1	4.0	53.6	6683.3	66.6
Beer	2	2912.1	29.9	4270.1	527.6
Bread And Baked Goods	15	651.0	17.1	3785.8	94.9
Breakfast 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

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



(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

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

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

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

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

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. 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 C.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

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

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.

[11] We also find that mergers are not systematically completed on "special" days of the year (e.g., starts of quarters). Furthermore, Figure A.3 shows that mergers are distributed across time and are not clustered, for example, during the financial crisis.

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., 2019) 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 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

[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.

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 B shows that if the first-stage model is correctly specified, then under standard conditions this estimate recovers the sales-weighted treatment effect of the merger, 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 A.

We aggregate across mergers by weighing each uniformly, for simplicity of exposition. We verify in Appendix A 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 A.2).

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. However, there is substantial dispersion around these averages. For merging parties, 25% of mergers raise prices by over 5.9%, but also 25% of mergers lower prices by over 5.2%. 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

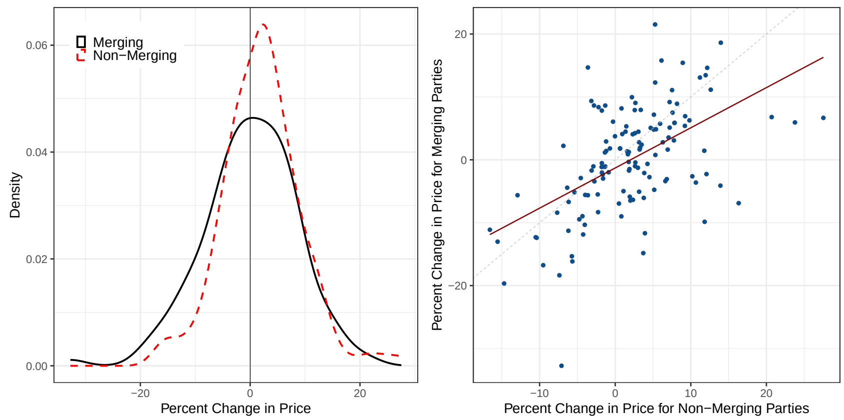
[13] For the price regressions, we use two-way clustered standard errors for the second stage by brand and DMA to account for correlation in the prediction error of the left-hand side variable. For quantities, we instead cluster by DMA, as these specifications are estimated at the merging/non-merging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

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, 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



(a) Distribution of price changes

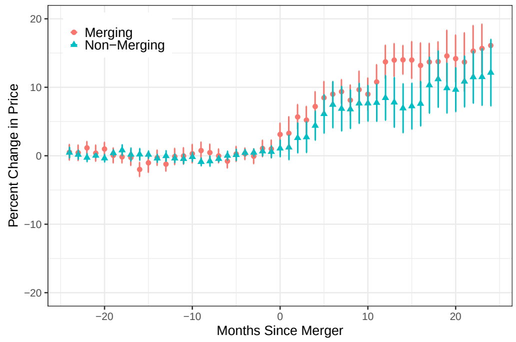
(b) Scatter of price changes

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.

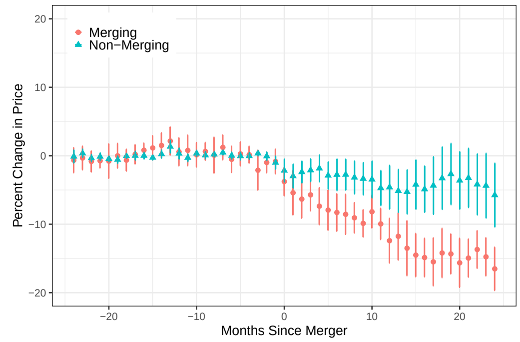
non-merging parties to lower prices for consumers. One potential explanation is that the cost synergies enjoyed by the merging parties are substantial enough to drive their prices down, and their rivals follow. On the other hand, 41% of mergers lead to higher prices from both types of firms. Strategic 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.

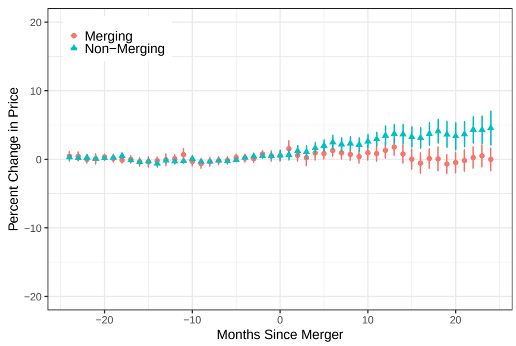
We next study the timing of these price changes. Figure 3 reports average merging and non-merging party coefficients at the monthly level for a 24-month window around the merger. Panel (a) presents results for mergers in the top quartile of price increases, Panel (b) for those in the bottom quartile, and Panel (c) for the remainder. These results shed light on how quickly merging parties begin to increase



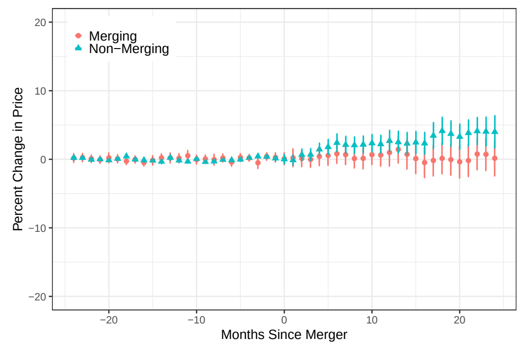
(a) High price changes



(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, 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.[14]

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.

[14] 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.

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 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, followed by a small price decrease. As in the previous panels, this is consistent with cost synergies taking effect roughly a year after completion. At the same time, non-merging parties steadily increase their prices post-merger after holding them constant for roughly two years before the completion date.

III.C. Quantities

While most merger retrospectives have focused on prices, another natural question is whether mergers have reduced transacted quantities. Conventional intuition suggests that even if a merger has a small price effect, a significant drop in quantity may indicate adverse welfare effects (Lazarev et al., 2021).

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

	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.

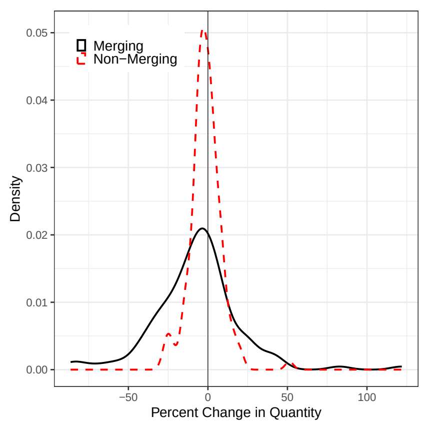
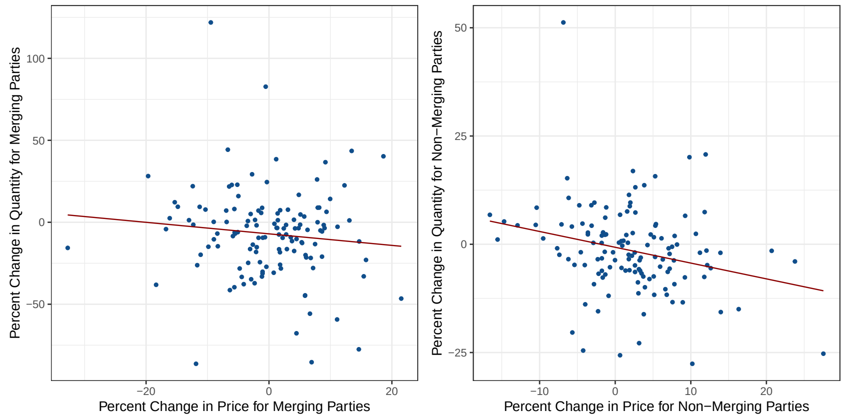


Figure 4: Quantity changes for merging and non-merging parties, as estimated by (2). This plot displays transformed coefficient estimates (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for the quantity change of the merging and non-merging parties. We use a balanced panel of stores and weigh regressions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

in quantity effects for merging parties: the standard deviation and inter-quartile range are both around 26–27 pp. The variation is much smaller for non-merging parties.

González et al. (2022) show that mergers can induce supply disruptions, which could reduce quantity. Since the welfare interpretation of a quantity decline changes if part of the drop is transitory, in Figure A.1, 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.

Are these quantity decreases driven by price increases? Figure 5 plots the estimated quantity effects against the estimated price effects for merging (Panel



(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.

(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.

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.

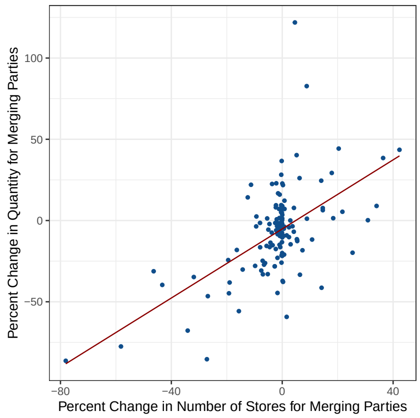
In 37% of mergers, store networks expand beyond the union of the pre-merger networks. This is consistent with the pro-competitive argument that economies of

	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)

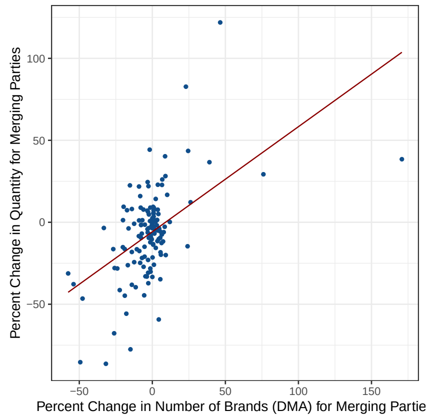
Table 4: Overall Effects on Product Availability. This table displays the distributions of product availability outcomes. Standard errors are in parentheses. Number of Stores refers to the number of unique stores in which at least one of the merging (or non-merging) parties' products is sold. Number 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.

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.

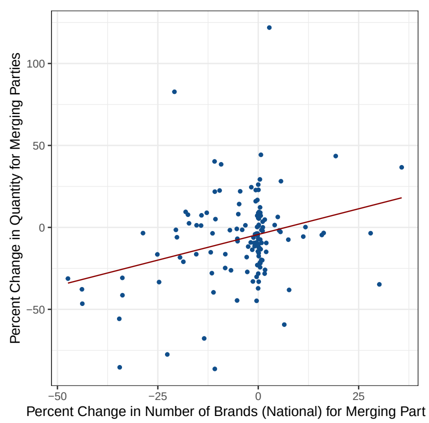
At the same time, many mergers lead to substantial contractions in the distribution network: the 25th percentile of changes to the number of stores is -4.3%. Moreover, we find that large declines in quantities sold are correlated with contractions in the store network. We find this result more surprising, as one may expect that the merged 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



(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.

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 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 the number of brands sold both in each DMA and nationally and changes in quantity.

One rationalization behind eliminating brands after a merger is that some brands are duplicative in the merged entity's portfolio. The fact that we observe quantity declines after brand removal clearly shows this is not the whole story. Instead, some of this brand removal could be due to the desire to eliminate products that cannibalize sales from more profitable alternatives. Turning our attention to brand introductions, we find that in 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, 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 a number of robustness checks to validate our empirical strategy and sample. Across our robustness checks, average price effects are 1–2 pp away from zero, average quantities drop, and the distribution of effects across both outcomes is very disperse. This latter point is of particular importance, as the distribution of effects is more informative of stringency than the mean (Carlton, 2009).

One may be concerned that the estimated effects are due to changes in cost or demand factors over time. Such shocks would have to lead to departures from the linear trend that are coincident with the timing of the merger to rationalize the time trends in Figure 3. To further deal with this concern, we consider two additional analyses. First, we control for cost and demographic control variables, see list in Appendix C.1. Panel B of Table 2 presents the distribution of price effects, and Table A.2 shows the distribution of quantity effects with these controls. Overall, these results are very similar to the baseline. Second, we leverage geographic variation in merging-party presence to form a control group. Panel C of Table 2 and Table A.2 report estimates where the merging parties do not have a presence as a control group, as in (4). For both prices and quantities, these estimates exhibit lesser, but still substantial, dispersion. However, regional pricing strategies would bias estimates from this specification towards zero.

All remaining results are presented in Tables A.1 and A.3 in Appendix A. We consider robustness to two concerns related to the selection of stores in Nielsen. First, one may be concerned that Nielsen stores are not a representative sample. We also compute effects using the NielsenIQ Consumer Panel, a random sample of households that is representative of 52 major markets. Since these households record their purchases regardless of whether the store they are purchasing from is in the NielsenIQ Retail Scanner dataset, this sample includes retailers that are excluded from our previous analysis. The mean (25th / 75th percentile) of the distribution of overall price effects using the panelist data is 2.1 pp (1.1 pp / 2.1 pp) lower than the baseline. Given the statistical error in these estimates, we do not view these differences as meaningful departures from the economic interpretation of the baseline results. Additionally, we restrict our sample of mergers to those involving food products. We find that price effects are similar, although again lower than the baseline. We also find that some especially negative quantity changes disappear in this specification. This is consistent with mergers inducing firms in non-food markets to de-emphasize or exit the grocery channels that comprise a large share of the Nielsen dataset. Another concern may be that the stores that remain in the scanner dataset throughout our sample period are selected. We verify that price effects using an unbalanced panel of stores are similar; we do not compute quantity effects since the lack of balance leads to noise when aggregating quantities.

We next consider robustness to technical decisions made in the baseline specification. We estimate unweighted versions of our main specification. Price effects are slightly lower than the baseline but again lead to similar economic interpretation. Quantiles for the distribution of quantity effects are also very similar, although the mean for merging parties is considerably larger (and noisily estimated). To address churn in UPCs over time, we also consider price regressions aggregated to the brand level. Doing so lowers the upper tail of the price effects slightly.

Finally, it is important to note that the variation in baseline estimates is due to both heterogeneity of the merger effects and estimation error. While merger-level estimates are fairly precise in relation to the overall dispersion (see Figure A.2), we formally address estimation error at the merger-level by running a Bayesian shrinkage procedure. We find that while the estimated dispersion of the estimates decreases slightly, the distributions are largely the same.

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.

Section 5.3 of the Guidelines details market structures under which the agencies are likely to presume competitive harm from a merger. Mergers that increase HHI by 200 points and lead to a post-merger HHI of more than 2,500 are "presumed to be likely to enhance market power." This region is often called the "red zone" (Nocke and Whinston, 2022).[15] The "yellow zone" includes mergers outside the red zone that increase HHI by more than 100 points and lead to post-merger HHI levels above 1,500. The Guidelines note that mergers in this area "raise significant competitive concerns and often warrant scrutiny." Mergers outside this area are in the "green zone" or the "safe harbor" and are "unlikely to have adverse competitive effects."

It is a ripe time to evaluate the structural presumptions. In July 2023, the DOJ and FTC released a draft of new Merger Guidelines, following a 2021 executive order. The 2023 draft expands 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

[15] 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.

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. Column (1)–(3) use merging parties' price changes as the dependent variable. Column (1) reports that mergers with larger average HHI tend to have lower price changes. We interpret these results as likely capturing selection into 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 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 two robustness checks in Appendix A. First, computing HHI and DHHI using nationwide market shares yields similar results (Table A.5). Second, we study whether mergers price changes for mergers that proceeded with divestitures are different. Dropping these mergers from the analysis (Table A.6) dampens the

	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.

correlation with DHHI somewhat for non-merging parties. We discuss these mergers in more detail in Section V.A below when connecting price effects to antitrust enforcement. 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.

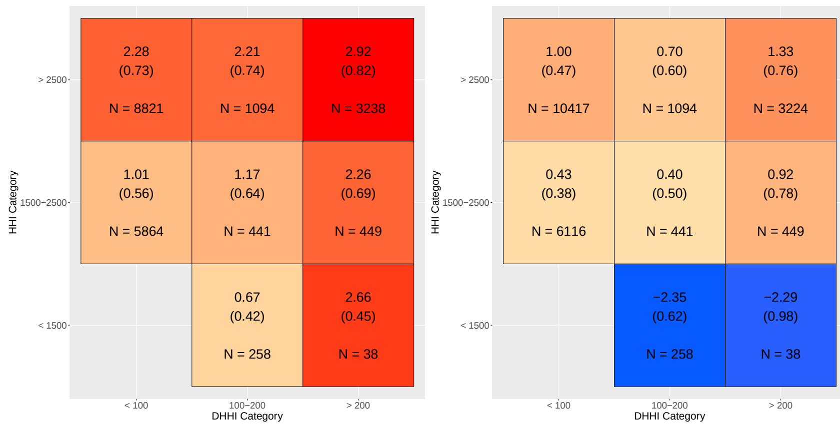
The patterns we identify cross-merger might not hold within-merger. First, if firms decide on pricing at a coarser level than the geographic market, as they would under zone pricing, DMA-level market structure may not be correlated with price changes. Second, selection into proposal and approval may 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

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

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. We make three comments about these results. 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 A.7 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 large 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"



(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.

(p. 1944). Our results are a concrete step in providing this empirical support.

Third, we investigate more granular relations with market structure than we could in the cross-merger analysis. We find that some regions in the green zone—in particular, those with high HHI or DHHI—still lead to significant price increases. These results may call into question the expansion of the green zone in the 2010 revision of the Guidelines. Additionally, we find price increases are high for mergers in the yellow region when they have either large values of HHI or DHHI, providing more evidence for arguments for increased scrutiny in this region (Rose and Shapiro, 2022). 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.[16]

Taking stock, 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

[16]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.

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 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.

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.[17] 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. 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

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

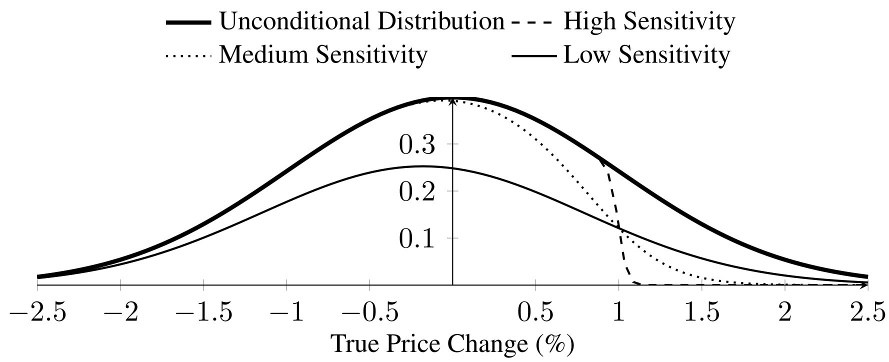


Figure 8: Illustration of the identification of model parameters. We illustrate densities of price changes of approved mergers, normalizing them to integrate to the probability of approval, for three potential sets of model parameters.

case exceeds some cost K. The net benefit and the probability of winning could 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.[18]

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

[18]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.

and size, as well as estimates of price changes for unchallenged mergers.

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. Second, we do not include measures of market structure in the threshold itself. The agencies would be more likely to challenge a merger with high DHHI, for instance, because they have a prior that it would lead to a larger price change, not because they are inherently stricter on such mergers. We assume that $p_i \sim N(p_i^*, \sigma_\epsilon^2)$, where σ_ϵ parameterizes the correlation between the true price change and the agencies' expectation.

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 $\bar{p}(Z_i)$.[19] For mergers that were allowed, 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.[20] We estimate the model via maximum likelihood.

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 with each other: we allow the mean of the prior distribution to be parameterized by dummies for whether the merger is in the "red" or "yellow" regions. We find a larger mean price change in the red region than in the yellow or the baseline, consistent with the presumption that such mergers are likely anti-competitive.

Comparing the results in Panel A with those in Table 5, we estimate 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

[19] 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.

[20] 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^*.

	Aggregate Price Changes			Merging Party Price Changes		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Prior						
Avg HHI (0–1)	-9.08			-9.60		
	(4.51)			(6.65)		
Avg DHHI (0–1)	66.02			65.45		
	(18.22)			(78.37)		
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	2.67	1.48	-0.49
	(1.53)	(2.88)	(0.64)	(2.29)	(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	8.72	5.53	18.44
	(2.83)	(1.70)	(3.12)	(6.46)	(3.02)	(29.15)
C. Threshold						
Log(Total Merging Sales)	-1.13	-0.96	-0.92	-1.37	-1.43	-0.58
	(0.55)	(0.53)	(0.56)	(0.79)	(0.71)	(0.97)
Constant	10.22	11.03	9.98	9.92	11.93	5.59
	(2.27)	(1.54)	(2.47)	(4.29)	(2.75)	(7.72)
D. Sales-Weighted Thresholds						
Average	8.34	9.45	8.46	7.65	9.57	4.63
	(2.08)	(1.50)	(2.14)	(3.48)	(2.34)	(6.25)
Q1	7.24	8.51	7.56	6.31	8.18	4.06
	(2.07)	(1.63)	(2.04)	(3.08)	(2.27)	(5.65)
Q3	9.15	10.13	9.11	8.63	10.59	5.04
	(2.08)	(1.42)	(2.20)	(3.75)	(2.42)	(6.54)

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.

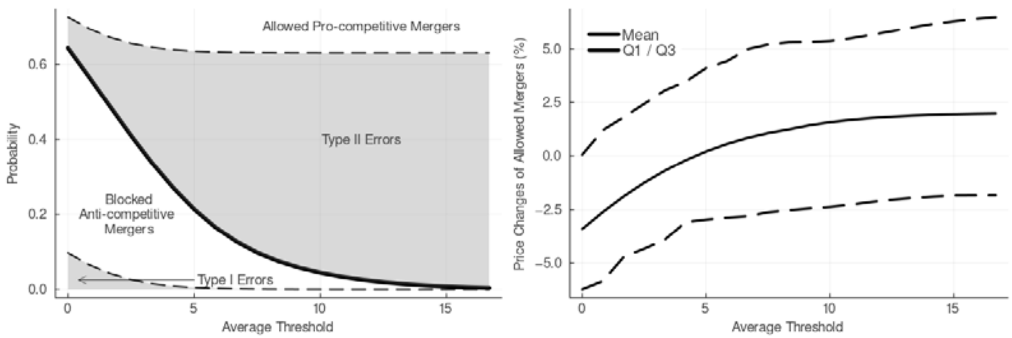
into approval: mergers with high DHHI that were proposed but did not go through likely would have had higher price changes than approved mergers with high DHHI. The agencies' actions against those with especially large price changes dampen the realized correlation. Results in Table A.4 indicate that 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 (s.e. 1.5 pp).

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 total sales is typically significant at at least 10%. 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%. Columns (4)–(6) use the price changes of the merging parties, rather than aggregate price changes, as the variable of interest. We find comparable thresholds in these specifications, especially when taking into account the standard errors.

We find that the marginal merger would have a price effect in the range of 8–9% overall. Kwoka (2014, p. 86) argues that one interpretation of the selection bias in published studies is that these studies are more likely to be of such marginal mergers: these are the deals that garnered press attention partly because of agency scrutiny. It is thus noteworthy that he arrives at a quantitatively similar conclusion, with mean price changes of mergers around 7.2% (Table 7.2 in Kwoka (2014)).

We also estimate the model using price changes from all other specifications discussed in Section III.E. Across alternate price estimates, we find sales-weighted thresholds ranging from 4.0 to 8.0 pp, slightly lower than our baseline estimate but still within the confidence interval. We find similar levels of heterogeneity across



(a) Probability of challenges

(b) Price changes of consummated mergers

Figure 9: Outcomes of counterfactual thresholds. Panel (a) shows the probability of blocking a merger (solid black) along with probabilities of type I and type II errors. Panel (b) shows price changes of consummated mergers. Figure A.4 shows confidence intervals.

mergers in the thresholds, with interquartile ranges from 0.9 pp to 2.3 pp. Moreover, the average thresholds are significantly larger than zero at at least the 10% level across all specifications. Panel (a) of Figure A.5 shows these results.

V.B. Counterfactual Outcomes Under Alternative Stringencies

Given the estimated threshold in Section V.A, is antitrust scrutiny excessively lax? Answering this question requires elements outside the scope of this study: a full welfare calculation, knowledge of the agencies' budget constraints, the costs of challenging mergers, the likelihood that challenges would hold up in court, and a social objective function. However, we can use the model to inform important elements that would go into the cost-benefit calculation for adjusting antitrust scrutiny which, to our knowledge, have not been quantified before. 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.[21] We also compute the distribution of price effects for allowed mergers.

Panel (a) of Figure 9 plots the probability of challenging a merger against

[21] 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.

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 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.

Another way to tackle this question is to document errors under different thresholds. A blocked merger could have been anti-competitive (leading to a price increase) or pro-competitive (leading to a price decrease). The latter situation is called a "type I error" (Kwoka, 2016). Tightening the threshold must lead to more type I errors since the agencies only operate based on a prediction of the price effect; the relevant question is by how much. Panel (a) shows that type I errors are infrequent at the current threshold. Recall that agencies block pro-competitive mergers if their signal exceeds the threshold and that 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. The opposite mistake—allowing an anti-competitive merger—is called a "type II error." Panel (a) also splits the region where mergers are allowed (above the solid line) into type II errors and situations where pro-competitive mergers are allowed. At the current threshold, about three-fifths of allowed mergers are due to Type II errors. The ratio becomes about one-half at a threshold of 5% and one-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 A.5 show these results and those for price changes for consummated mergers.

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

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

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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ONLINE APPENDICES

A. Additional Tables and Figures

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

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

- Table A.1 performs additional robustness tests on the distribution of estimated price effects.
  - Panel A reproduces the estimates from the baseline specification in Panel A of Table 5 for convenience.
  - Panel B reports results obtained using the Nielsen Consumer Panel dataset instead of the Retail Scanner dataset. The sample of Nielsen panelists is constructed to be representative of a coarser market definition called a Scantrack market, rather than a DMA. Thus, these regressions are done at the UPC-Scantrack-month level, and we aggregate to this level using the projection weights provided by Nielsen.
  - Panel C reports results for mergers involving manufacturers of food products, markets for which Nielsen coverage is likely better.
  - Panel D reports results obtained using all stores in the Retail Scanner dataset, not just those that are in the sample throughout the entirety of the merger's analysis window.
  - Panel E reports results from regressions where observations are weighted equally rather than using the pre-merger volume.
  - Panel F reports results of a specification where prices are computed as the sales-weighted average at the brand-DMA-month level to address potential churn in the set of UPCs over time.
  - Panel G reports the distribution of merger effects obtained using Bayesian shrinkage. We implement a "random effects" model where the price

effect of merger i is $p_i \sim N(\mu, \sigma^2)$, and our estimate is $\hat{p}_i \sim N(p_i, \sigma_i^2)$, where σ_i is our estimated second-stage standard error (taking into account estimation error from the first stage). We implement this procedure using the `brms` package of Bürkner (2017), which can provide posterior distributions on each p_i through a Hamiltonian Monte Carlo procedure.[22] We report distributions of the posterior mean of each p_i in the tables.

- Tables A.2 replicates all panels of Table 2 in the body, but for quantity effects. In particular, it adds results involving cost and demographic controls (Panel B) as well as untreated markets as a control (Panel C).
- Figure A.1 shows the path of quantity changes over time through event study diagrams, separately for mergers in the top and bottom quartiles of quantity changes, for remaining mergers, and for all mergers. This is the analogue of Figure 3 in the body, but for quantities.
- Table A.3 provides the same set of robustness checks for the quantity results that Table A.1 does for the price results. We omit the unbalanced panel of stores: given quantity is aggregated across stores, quantity effects from an unbalanced panel would be hard to interpret. We also omit the brand-level specification, as the results in this section rely on tests of aggregate quantity changes.
- Figure A.2 displays merger-level estimates and 95% confidence intervals for aggregate, merging-party, and non-merging-party price changes.
- Figure A.3 analyzes the timing of mergers. It displays, both at the merger and the deal level, histograms of the dates at which mergers became effective.

The remaining tables and figures pertain to Sections IV and V.

- Table A.4 reports regressions of enforcement on merger-level market structure. This table corroborates that enforcement correlates with DHHI and the red region of the merger guidelines.

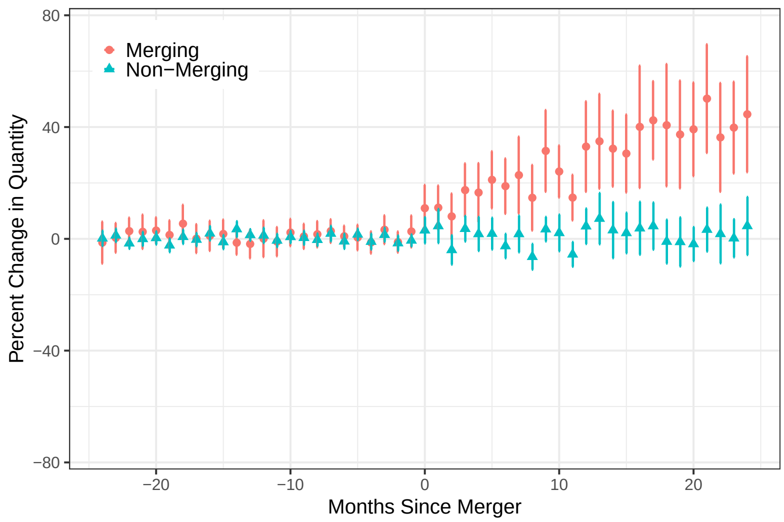
[22] For each merger, we run a chain with a burn-in of 5,000 iterations and draw 20,000 more iterations. We have checked that chains converge given the diagnostics reported by the package.

- Table A.5 replicates Table 5 but uses nationwide HHI and DHHI as the metrics for market structure, rather than the average of DMA-level HHI and DHHI.
- Table A.6 replicates Table 5 but drops mergers in which the parties had to divest at least one brand.
- Table A.7 presents standard errors on all pairwise differences in Figure 7.
- Figure A.4 replicates Figure 7, but adds confidence regions.
- Figure A.5 shows results of the structural estimates using alternate methods to estimate price effects. It reports sales-weighted thresholds in Panel (a), the analogue of Panel D (Column (1)) of Table 6. Panel (b) reports probabilities of challenges and probabilities of Type I and Type II errors as a function of alternate thresholds. Panel (c) reports price changes for consummated mergers. These panels are the analogues of Figure 9.

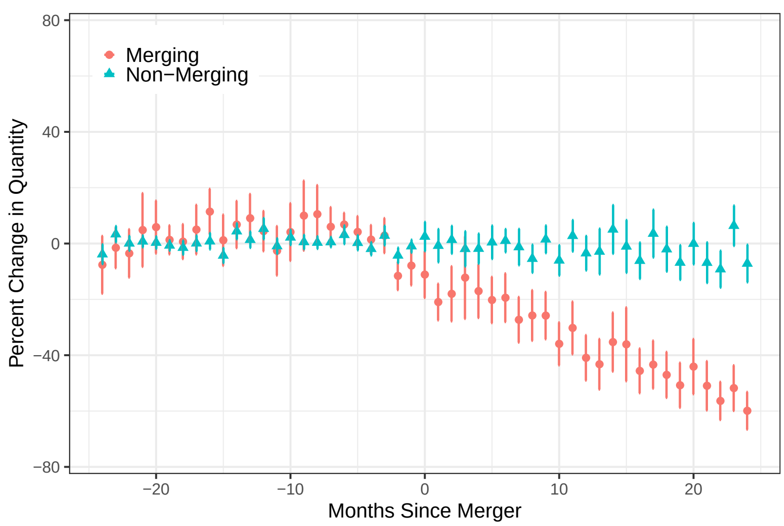
	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	130	1.51	6.29	-2.34	1.74	5.31
		(0.55)		(0.58)	(0.59)	(0.57)
Merging Parties	130	0.03	8.47	-5.15	0.77	5.86
		(0.74)		(0.97)	(0.97)	(0.85)
Non-Merging Parties	130	2.07	7.11	-2.20	1.93	6.12
		(0.62)		(0.62)	(0.58)	(0.87)
B. Panelist Data						
Overall	130	-0.59	5.96	-3.44	-0.08	3.19
		(0.52)		(0.44)	(0.51)	(0.66)
Merging Parties	130	-1.02	8.04	-4.69	-0.86	4.20
		(0.71)		(0.68)	(0.75)	(0.71)
Non-Merging Parties	130	-0.26	6.46	-3.45	0.03	3.74
		(0.57)		(0.64)	(0.44)	(0.72)
C. Food Mergers Only						
Overall	75	-0.06	6.69	-4.46	0.25	4.87
		(0.77)		(1.32)	(1.14)	(0.55)
Merging Parties	75	-1.07	9.65	-6.96	-1.05	5.39
		(1.11)		(1.62)	(1.25)	(2.05)
Non-Merging Parties	75	0.14	6.85	-4.55	0.65	4.69
		(0.79)		(1.21)	(1.13)	(0.85)
D. Unbalanced Panel of Stores						
Overall	130	1.56	6.20	-2.11	1.76	5.11
		(0.54)		(0.79)	(0.68)	(0.71)
Merging Parties	130	-0.05	8.54	-5.45	0.45	5.76
		(0.75)		(1.09)	(1.16)	(0.70)
Non-Merging Parties	130	2.10	7.04	-2.28	1.97	6.40
		(0.62)		(0.81)	(0.72)	(0.62)
E. Equally-Weighted Regressions						
Overall	130	0.50	5.31	-2.36	1.10	4.10
		(0.47)		(0.41)	(0.59)	(0.77)
Merging Parties	130	-0.04	7.50	-4.14	0.65	4.48
		(0.66)		(0.93)	(0.77)	(0.80)
Non-Merging Parties	130	0.82	5.57	-2.42	1.18	4.20
		(0.49)		(0.49)	(0.52)	(0.66)

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
F. Brand Level						
Overall	130	-0.60	6.72	-4.14	0.03	2.83
		(0.59)		(0.79)	(0.53)	(0.55)
Merging Parties	130	-0.69	12.04	-7.89	-1.13	3.69
		(1.06)		(1.62)	(0.71)	(0.98)
Non-Merging Parties	130	-0.26	7.48	-4.36	0.32	2.70
		(0.66)		(0.77)	(0.42)	(0.47)
G. Bayesian Shrinkage						
Overall	130	1.16	5.53	-1.81	1.74	4.84
		(0.48)		(0.58)	(0.54)	(0.53)
Merging Parties	130	-0.08	7.42	-4.92	0.77	5.07
		(0.65)		(1.02)	(0.91)	(0.66)
Non-Merging Parties	130	1.49	5.85	-1.87	1.88	5.61
		(0.51)		(0.53)	(0.51)	(0.80)

Table A.1: Robustness of 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-party, and non-merging-party price changes. Standard errors are in parentheses. Panel A displays the baseline results from the main text, Panel B displays results using the NielsenIQ Consumer Panel Data, Panel C displays results for food mergers only, Panel D displays results using an unbalanced panel of stores, Panel E displays results assuming equal weights across UPC/DMAs when estimating (1) and (2), Panel F displays results for brand-level regressions, and Panel G displays results using Bayesian shrinkage.

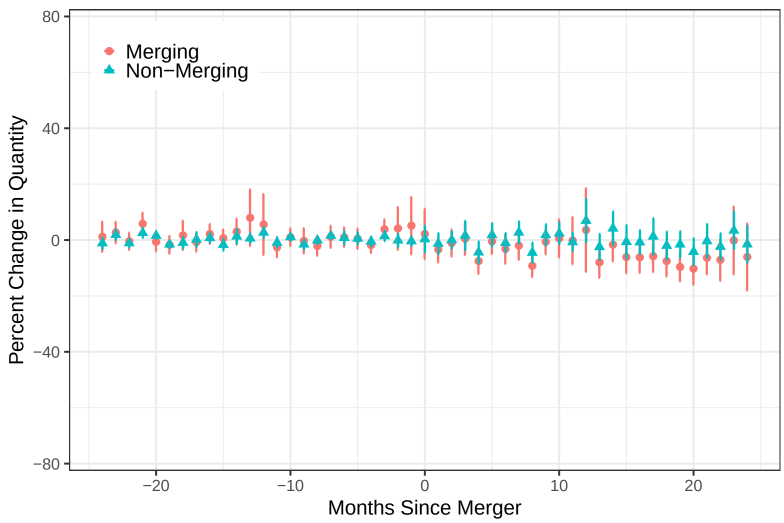


(a) High quantity changes

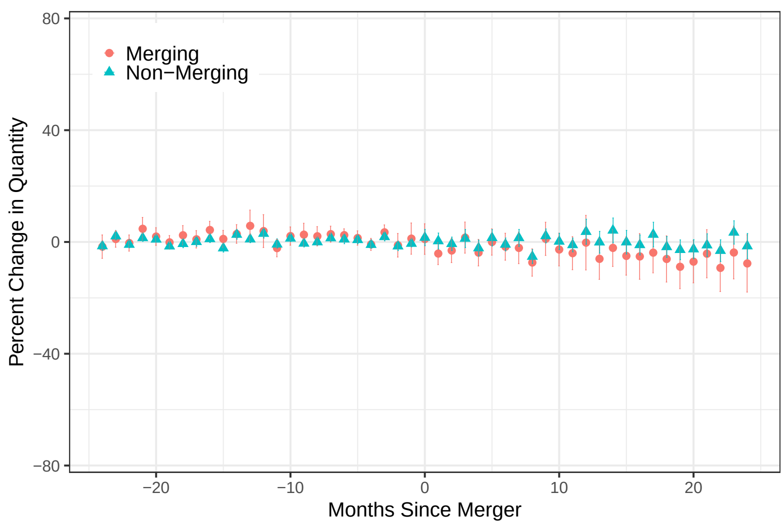


(b) Low quantity changes

Figure A.1: Timing of quantity changes, for merging parties (red circle) and non-merging parties (blue triangle). The marker indicates the mean quantity change the given number of months after the merger becomes effective, and the thick line is the 95% confidence interval of that mean. Panels (a)–(c) shows subsamples: Panel (a) restricts to mergers with quantity changes in the top quartile, Panel (b) restricts to mergers with changes in the bottom quartile, while Panel (c) displays the remaining mergers. Panel (d) shows all mergers. (Continued on next page.)



(c) Stable quantities



(d) All quantity changes

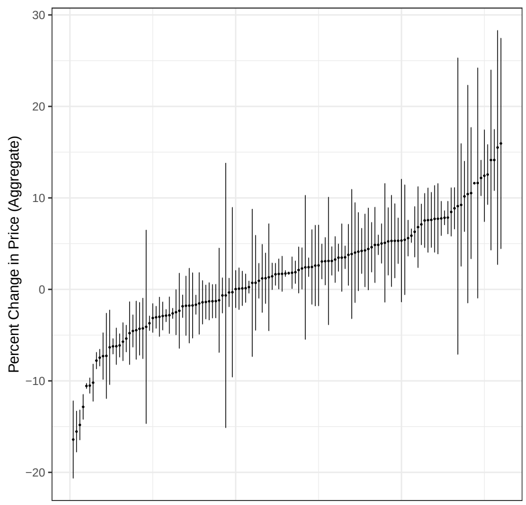
Figure A.1: (Continued from last page)

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	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)
B. Cost and Demographic Controls						
Overall	130	-2.45	9.08	-6.87	-1.74	2.85
		(0.80)		(0.65)	(0.77)	(0.79)
Merging Parties	130	-7.06	27.45	-21.00	-5.73	5.60
		(2.41)		(3.29)	(2.05)	(1.99)
Non-Merging Parties	130	-1.44	10.09	-6.49	-1.82	4.30
		(0.88)		(0.72)	(0.82)	(1.13)
C. Treated/Untreated						
Overall	90	-1.22	13.37	-4.89	-1.12	3.22
		(1.41)		(0.97)	(0.67)	(0.64)
Merging Parties	90	-5.06	40.44	-28.19	-6.12	11.99
		(4.26)		(5.93)	(3.61)	(5.18)
Non-Merging Parties	90	-0.36	12.65	-3.70	0.29	3.52
		(1.33)		(0.96)	(0.63)	(0.81)

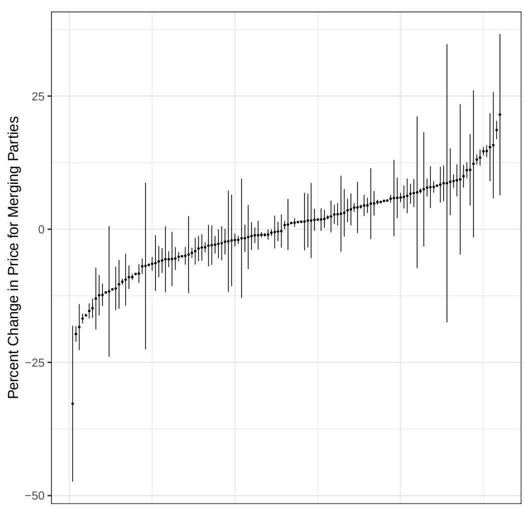
Table A.2: Quantity Effects with Controls. This table displays the distribution of transformed coefficient estimates of (2) (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for overall, merging-party, and non-merging-party quantity changes. Standard errors are in parentheses. We use a balanced panel of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

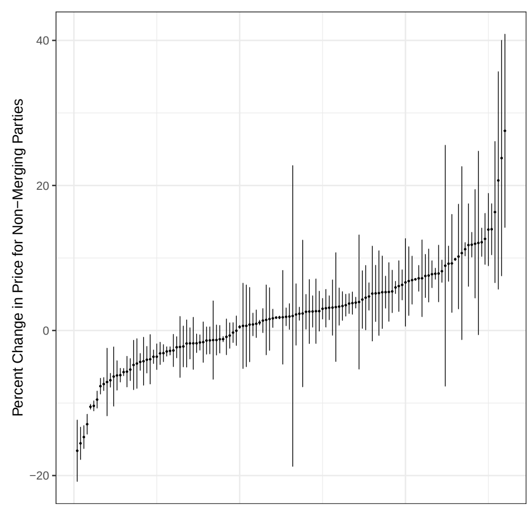
	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	130	-2.46	9.02	-6.87	-1.93	2.80
		(0.79)		(0.66)	(0.74)	(0.70)
Merging Parties	130	-7.07	27.42	-20.96	-5.61	5.71
		(2.40)		(3.70)	(1.95)	(1.93)
Non-Merging Parties	130	-1.45	10.04	-6.37	-1.86	4.09
		(0.88)		(0.72)	(0.86)	(1.05)
B. Panelist Data						
Overall	130	-3.79	15.08	-13.09	-1.71	4.55
		(1.32)		(2.04)	(1.21)	(1.27)
Merging Parties	130	-7.16	41.30	-30.48	-11.37	10.91
		(3.62)		(3.25)	(3.41)	(5.11)
Non-Merging Parties	130	-3.35	15.81	-13.41	-1.35	5.89
		(1.39)		(1.66)	(1.17)	(1.88)
C. Food Mergers Only						
Overall	75	-0.36	6.99	-3.64	0.10	3.84
		(0.81)		(1.13)	(0.85)	(0.56)
Merging Parties	75	-1.99	28.23	-15.64	-2.75	8.93
		(3.26)		(3.81)	(2.52)	(2.12)
Non-Merging Parties	75	0.33	10.19	-3.73	0.38	4.66
		(1.18)		(0.72)	(0.90)	(1.09)
D. Equally-Weighted Regressions						
Overall	130	-0.74	25.77	-11.83	-2.08	6.73
		(2.26)		(1.58)	(1.54)	(1.06)
Merging Parties	130	5.23	77.23	-18.84	-5.00	11.45
		(6.77)		(3.53)	(2.32)	(3.10)
Non-Merging Parties	130	-0.23	13.61	-6.71	-1.76	5.61
		(1.19)		(0.84)	(1.21)	(1.18)
E. Bayesian Shrinkage						
Overall	130	-2.40	8.84	-6.84	-1.96	2.75
		(0.78)		(0.65)	(0.72)	(0.66)
Merging Parties	130	-7.76	25.31	-20.91	-5.62	5.34
		(2.22)		(3.60)	(1.95)	(2.42)
Non-Merging Parties	130	-1.35	9.27	-6.31	-1.86	3.99
		(0.81)		(0.70)	(0.86)	(1.01)

Table A.3: Robustness of 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-party, and non-merging-party quantity changes. Standard errors are in parentheses. Panel A displays the baseline results from the main text, Panel B displays results using the NielsenIQ Consumer Panel Data, Panel C displays results for food mergers only, Panel D displays results assuming equal weights across firm type/DMAs when estimating (1) and (2), and Panel E displays results using Bayesian shrinkage.



(a) Aggregate

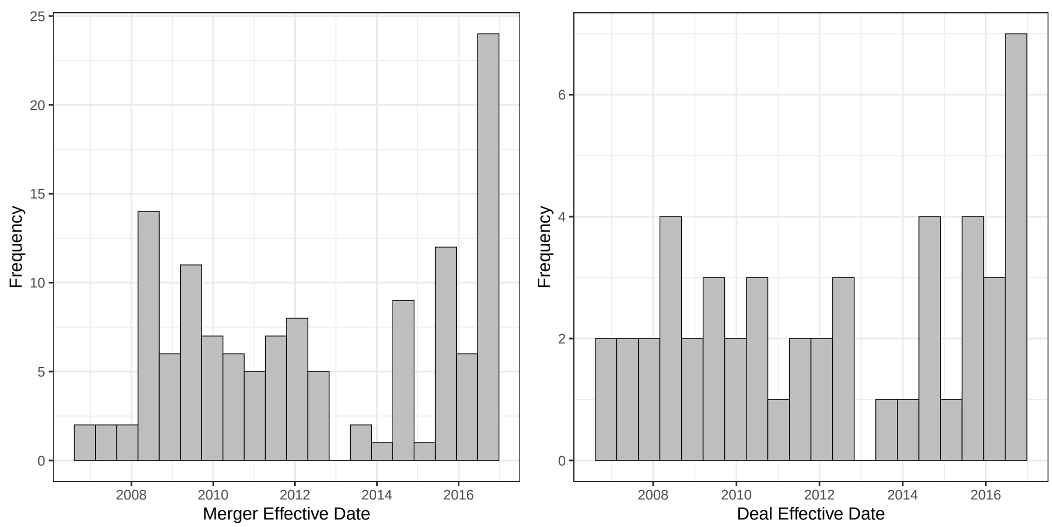




(b) Merging

(c) Non-Merging

Figure A.2: Merger-level estimates and confidence intervals. Panel (a) displays aggregate price changes, Panel (b) displays merging party price changes, and Panel (c) displays non-merging party price changes. 95% confidence intervals are calculated using standard errors two-way clustered by brand and DMA.



(a) Merger Level

(b) Deal Level

Figure A.3: Timing of mergers. These plots display histograms of the dates at which mergers became effective. The unit of observation of the left-hand panel is a merger, while the unit of observation of the right-hand panel is a deal.

	(1)	(2)	(3)	(4)	(5)	(6)
HHI (0–1)	-0.08			0.00		
	(0.13)			(0.15)		
DHHI (0–1)	3.04			2.70		
	(1.20)			(1.23)		
HHI $\in$ [1500, 2500]		0.04			0.06	
		(0.04)			(0.04)	
HHI $>$ 2500		0.06			0.09	
		(0.03)			(0.04)	
DHHI $\in$ [100, 200]		0.07			0.02	
		(0.09)			(0.09)	
DHHI $>$ 200		0.22			0.19	
		(0.10)			(0.11)	
Yellow			0.07			0.02
			(0.08)			(0.08)
Red			0.25			0.21
			(0.11)			(0.11)
Log(Total Merging Sales)				0.03	0.03	0.02
				(0.01)	(0.01)	(0.01)
N	134	134	134	134	134	134

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

	Merging			Non-Merging			Aggregate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
HHI (0–1)	-14.66 (7.25)			-3.32 (10.01)			-9.54 (5.91)		
DHHI (0–1)	28.31 (16.97)			38.57 (23.49)			33.49 (15.85)		
HHI $\in$ [1500, 2500]		0.23 (2.36)			0.10 (2.09)			0.40 (2.04)	
HHI $>$ 2500		-4.32 (2.43)			-1.68 (2.19)			-2.55 (2.06)	
DHHI $\in$ [100, 200]		1.40 (1.92)			1.23 (1.58)			1.26 (1.32)	
DHHI $>$ 200		4.33 (1.65)			4.62 (1.69)			4.28 (1.30)	
Yellow			1.64 (1.67)			1.09 (1.21)			1.22 (1.03)
Red			1.55 (1.67)			4.17 (1.91)			2.91 (1.45)
Constant	3.36 (1.86)	1.02 (2.04)	-0.39 (0.95)	2.28 (2.31)	1.87 (1.92)	1.36 (0.75)	3.42 (1.56)	1.65 (1.87)	0.96 (0.69)
N	130	130	130	130	130	130	130	130	130

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

	Merging			Non-Merging			Aggregate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
HHI (0–1)	-14.44			-8.66			-12.66		
	(6.01)			(7.41)			(4.77)		
DHHI (0–1)	16.16			38.88			27.50		
	(20.74)			(23.07)			(17.34)		
HHI $\in$ [1500, 2500]		-0.53			-4.21			-3.85	
		(4.39)			(2.50)			(2.77)	
HHI $>$ 2500		-5.46			-7.37			-7.40	
		(4.33)			(2.39)			(2.64)	
DHHI $\in$ [100, 200]		1.32			1.85			1.77	
		(1.97)			(1.48)			(1.23)	
DHHI $>$ 200		3.32			3.86			3.39	
		(1.86)			(1.47)			(1.20)	
Yellow			0.40			1.09			0.95
			(1.72)			(1.35)			(1.14)
Red			1.05			2.22			1.56
			(1.85)			(1.48)			(1.22)
Constant	4.26	3.03	-0.28	4.01	7.12	1.34	4.93	6.62	0.95
	(1.99)	(4.16)	(0.96)	(2.12)	(2.21)	(0.75)	(1.54)	(2.52)	(0.70)
N	124	124	124	124	124	124	124	124	124

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

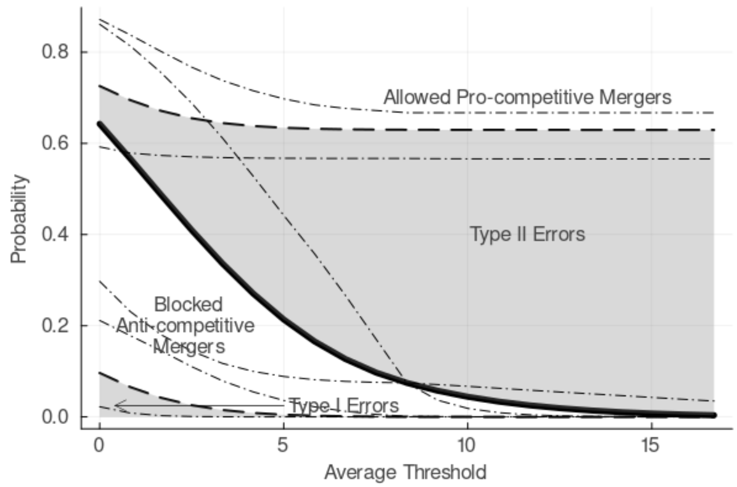
	Green				Yellow			Red
	LM	LH	ML	HL	MM	MH	HM	HH
LL	0.67	2.66	1.01	2.28	1.17	2.26	2.21	2.92
	(0.42)	(0.45)	(0.56)	(0.73)	(0.64)	(0.69)	(0.74)	(0.82)
LM		1.99	0.34	1.61	0.50	1.58	1.54	2.25
		(0.38)	(0.63)	(0.78)	(0.62)	(0.71)	(0.78)	(0.85)
LH			-1.65	-0.38	-1.49	-0.41	-0.45	0.26
			(0.54)	(0.64)	(0.46)	(0.55)	(0.64)	(0.74)
ML				1.27	0.16	1.24	1.20	1.91
				(0.64)	(0.54)	(0.61)	(0.61)	(0.71)
HL					-1.11	-0.02	-0.07	0.64
					(0.55)	(0.62)	(0.38)	(0.50)
MM						1.09	1.04	1.75
						(0.44)	(0.52)	(0.66)
MH							-0.05	0.66
							(0.57)	(0.69)
HM								0.71
								(0.34)

(a) Merging parties

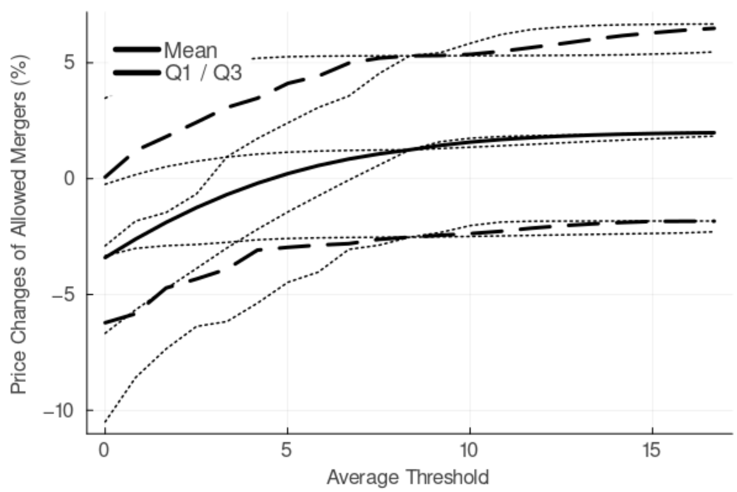
	Green				Yellow			Red
	LM	LH	ML	HL	MM	MH	HM	HH
LL	-2.35	-2.29	0.43	1.00	0.40	0.92	0.70	1.33
	(0.62)	(0.98)	(0.38)	(0.47)	(0.50)	(0.78)	(0.60)	(0.76)
LM		0.06	2.78	3.34	2.75	3.27	3.05	3.68
		(0.72)	(0.77)	(0.80)	(0.74)	(0.82)	(0.85)	(0.95)
LH			2.72	3.28	2.69	3.21	2.99	3.62
			(1.00)	(1.02)	(0.98)	(1.03)	(1.03)	(1.12)
ML				0.56	-0.04	0.48	0.26	0.89
				(0.25)	(0.39)	(0.75)	(0.48)	(0.67)
HL					-0.60	-0.08	-0.30	0.33
					(0.41)	(0.76)	(0.49)	(0.70)
MM						0.52	0.30	0.93
						(0.56)	(0.45)	(0.59)
MH							-0.22	0.41
							(0.62)	(0.61)
HM								0.63
								(0.42)

(b) Nonmerging parties

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

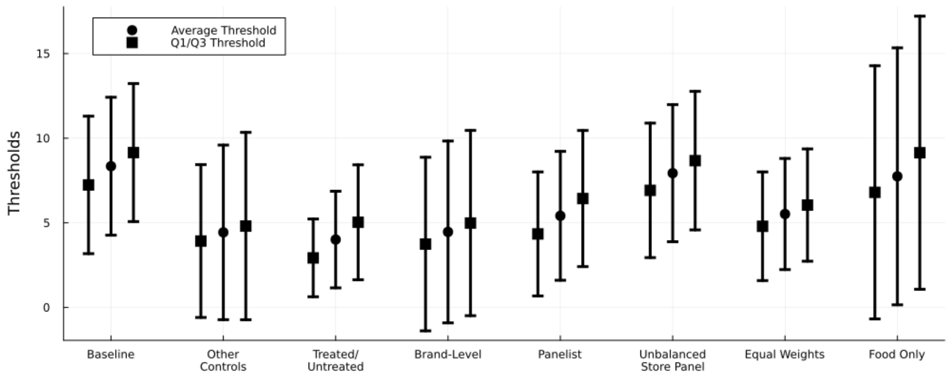


(a) Probability of challenges

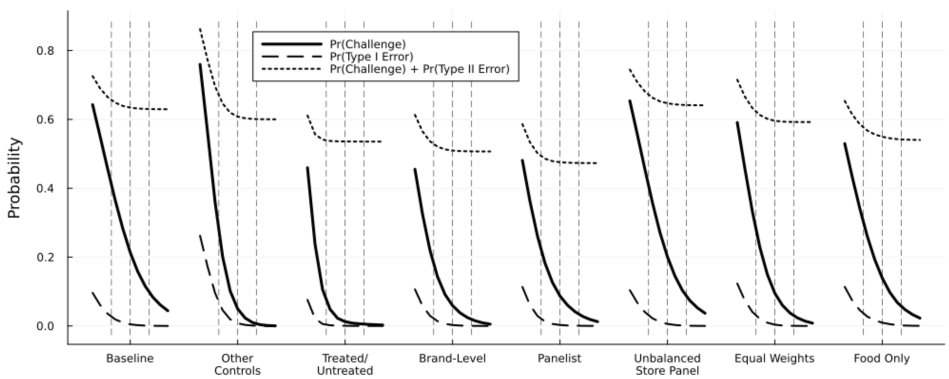


(b) Price changes of consummated mergers

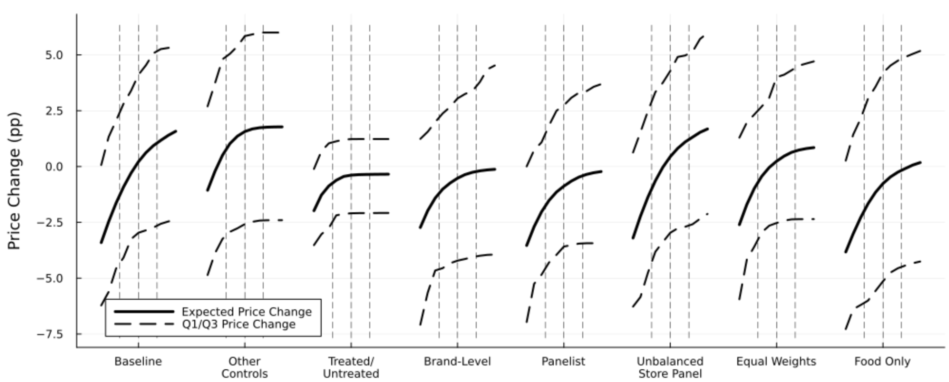
Figure A.4: Outcomes of counterfactual thresholds. This replicates Figure 9, but the dotted lines surrounding each line represent 95% confidence intervals.



(a) Thresholds



(b) Probability of challenge as a function of threshold



(c) Price changes conditional on consummation as a function of threshold

Figure A.5: Robustness of structural results to different estimates of price effects. Panel (a) shows sales-weighted thresholds and 95% confidence intervals, using the baseline specification of the structural model (Column (1) of Table 6). Panels (b) and (c) shows probabilities of challenge, probabilities of errors, and price changes for consummated mergers as a function of the threshold. The lines range from thresholds of 0 to 10 pp, and the dotted vertical lines correspond to thresholds of 2.5%, 5.0%, and 7.5%.

B. Estimation and Heterogeneous Treatment Effects

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

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

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

In the first stage, we estimate

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

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

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

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

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

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

The weighted OLS estimator for β_b is

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

We recover the remaining parameters as

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

In the second stage, we take weighted averages of

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

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

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

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

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

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

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

C. Details on Sample Construction and Market Definition

C.1. Sample Construction

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

Most deals that survive this initial filtering process either involve firms that do not sell retail products or only sell products not tracked in the NielsenIQ Scanner Dataset. To identify relevant deals, we analyze each deal's press release and the merging parties' SEC filings for the year before the merger and identify their retail brands, if they have any. We then search for those brands in the Product files of the NielsenIQ Scanner Dataset.

As described in Section II.B, we next check whether the parties overlapped in particular product and geographic markets by computing whether they each owned at least one UPC with a non-negligible share in the same geographic market. To do so, we compute shares at the DMA-month level and begin by considering all UPCs that have a share of at least 1% in any DMA-month in a two-year window around the merger. If this is more than 100 UPCs, we only keep the 100 best-selling UPCs. To ensure we do not miss any regional brands, we then add all UPCs with more than a 5% share in any region-month pair. With this initial sample of products, we check market coverage: the fraction of sales volume in the product market captured by this sample. If the 10th percentile of the distribution of market coverage across DMA-months is smaller than 60%, we repeat this exercise with 200 UPCs. If this continues to be the case, we expand the universe to 300 UPCs. If coverage continues to be too low, we drop the initial share cutoff from 1% to 0.5% and finally to 0.1%. Finally, to ensure we do not miss seasonal products affiliated with a popular brand, we add all UPCs associated with a brand included in our original list and all UPCs associated with brands that have a market share of at least 5%.

This procedure yields a sample that covers a large share of each relevant market. The average value (across mergers) of the 10th percentile of market coverage (within merger, across DMAs) is 92.2%, and the average value of the median coverage is 95.1%. This reassures us that we are capturing the relevant products in each product market.

C.2. Market Definition

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

How do these market definitions compare to ones posited by the agencies? Tables C.2 through C.4 list all market definitions from public competitive impact statements or complaints posted online by either the FTC or DOJ for mergers that were challenged or where divestitures were proposed, going back to 1990 for the FTC and 1982 for the DOJ.[23] We restrict our attention to horizontal mergers for goods that may have been in our Nielsen sample.

Our interpretation of these markets is that they are quite similar to markets that we have defined using combinations of Nielsen modules. Some of these markets are identical to ones in our sample (e.g., dry cat food, ready-to-eat cereal, or beer), and others (e.g., fine fabric wash products) correspond to candidate markets that we defined but that ended up having no overlap for our deals. A priori, one may have been concerned that the agencies select substantively narrower market definitions that product modules following their implementation of the hypothetical monopolist test, and we generally do not find this concern—at least among the set of mergers for which we have public information about their deliberations.

We find two caveats to the above discussion. First, agencies sometimes exclude

[23]For the FTC, we start from https://www.ftc.gov/legal-library/browse/cases-proceedings, filter by “Competition” as the Mission and “Horizontal” as the merger type. For the DOJ, we start from https://www.justice.gov/atr/antitrust-case-filings and filter for “Civil Mergers.” For both lists, we then manually inspect each of the results to find mergers that involve consumer packaged goods.

generic brands from the market. Second, the agencies sometimes separate products into quality tiers that (based on the text of the competitive impact statements) are based primarily on price.[24] However, both departures are more the exception than the rule. We find one instance where non-branded products are excluded entirely (DFA/SODIAAL in 2000), one in which only private labels are considered (Post/TreeHouse, although this is because TreeHouse only manufactured private label cereals), and one where both the entire market and the branded products are listed as markets (Crisco/Wesson). Separating into quality tiers also seems to be rare and happens in only three cases (ice cream, wine, and shampoo/conditioner). There are numerous market definitions where separating might have seemed plausible a priori: for instance, the competitive impact statements for beer mergers reference multiple segments but then group them into one market.[25] At a practical level, separating products into tiers is especially difficult since subdividing Nielsen modules into smaller groups at scale would necessitate somewhat arbitrary decisions on how to make the split—and would be almost impossible to do by hand. Thus, we are comfortable with the market definitions in the paper.

[24] We also see one case—refrigerated pickles—where the stated market definition would be narrower than a Nielsen module. However, in this case, the agency complaint acknowledged that there is "sufficient substitution" so that shelf-stable pickles act as a competitive constraint to products in the market. See https://www.ftc.gov/sites/default/files/documents/cases/2002/10/hickscmp.pdf.

[25] See https://www.justice.gov/atr/case-document/file/1331221/download for the competitive impact statement for the Anheuser-Busch InBev and Craft Brew Alliance merger.

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

Market	NielsenIQ Product Group	NielsenIQ Product Modules in Product Market	Cost Controls
30	Cookies	Cookies	Wheat Flour, Cocoa Beans, Sugar, Oats
31	Cosmetics	Cosmetic Kits	Fatty Acids, Starch Vegetable Fats Oils
32	Cosmetics	Cosmetics - Concealers	Fatty Acids, Starch Vegetable Fats Oils
33	Cosmetics	Cosmetics-Blushers	Fatty Acids, Starch Vegetable Fats Oils
34	Cosmetics	Cosmetics-Eye Shadows	Fatty Acids, Starch Vegetable Fats Oils
35	Cosmetics	Cosmetics-Eyebrow & Eye Liner	Fatty Acids, Starch Vegetable Fats Oils
36	Cosmetics	Cosmetics-Face Powder	Fatty Acids, Starch Vegetable Fats Oils
37	Cosmetics	Cosmetics-Foundation-Liquid, Cosmetics-Foundation-Cream And Powder	Fatty Acids, Starch Vegetable Fats Oils
38	Cosmetics	Cosmetics-Lipsticks	Fatty Acids, Starch Vegetable Fats Oils
39	Cosmetics	Cosmetics-Mascara	Fatty Acids, Starch Vegetable Fats Oils
40	Cosmetics	Cosmetics-Remaining	Fatty Acids, Starch Vegetable Fats Oils
41	Cosmetics	Talcum & Dusting Powder	Talc, Corn Starch
42	Detergents	Detergents-Packaged, Detergents - Light Duty, Detergents - Heavy Duty - Liquid	Surfactants
43	Fragrances - Women	Cologne & Perfume-Women's	Ethanol, Coal, Soybeans, Other Grains
44	Fresh Produce	Fresh Fruit-Remaining	Fertilizer
45	Grooming Aids	Cosmetic And Nail Grooming Accessory	Stainless Steel, Aluminum, Plastic
46	Gum	Gum-Bubble, Gum-Chewing, Gum-Chewing-Sugarfree, Gum-Bubble-Sugarfree	Sugar, Resin And Synthetic Rubber
47	Hair Care	Creme Rinses & Conditioners	Fatty Acids, Surfactants
48	Hair Care	Hair Preparations - Other Than Men's	Ethanol, Basic Organic Compounds
49	Hair Care	Hair Spray - Women's	Ethanol, Basic Organic Compounds
50	Hair Care	Shampoo-Aerosol/ Liquid/ Lotion/ Powder, Shampoo-Combinations	Fatty Acids, Surfactants
51	Hair Care	Wave Setting Products	Ethanol, Basic Organic Compounds
52	Housewares, Appliances	Oral Hygiene Appliance And Accessory	Nylon, Plastic
53	Kitchen Gadgets	Beverage Storage Container	Plastic, Stainless Steel
54	Laundry Supplies	Detergent Boosters	Surfactants
55	Liquor	Alcoholic Cocktails	Barley, Wheat, Corn
56	Liquor	Bourbon-Straight/Bonded, Bourbon-Blended, Canadian Whiskey, Irish Whiskey, Remaining Whiskey, Scotch	Barley, Wheat, Corn
57	Liquor	Cordials & Proprietary Liqueurs	Barley, Wheat, Corn
58	Liquor	Gin	Barley, Wheat
59	Liquor	Rum	Sugar
60	Liquor	Tequila	Sugar
61	Liquor	Vodka	Wheat, Russet Potatoes
62	Medications/Remedies/Health Aids	Foot Preparations-Athlete's Foot	Basic Organic Compounds
63	Men's Toiletries	Cologne/Lotion-Men's	Ethanol, Coal, Soybeans, Other Grains
64	Packaged Meats-Deli	Bacon-Refrigerated	Slaughter Hogs, Slaughter Poultry, Slaughter Hogs,Slaughter Poultry

Market	NielsenIQ Product Group	NielsenIQ Product Modules in Product Market	Cost Controls
65	Packaged Meats-Deli	Bratwurst & Knockwurst, Sausage-Dinner, Frankfurters-Refrigerated	Slaughter Hogs, Slaughter Cattle, Slaughter Poultry
66	Packaged Meats-Deli	Lunchmeat-Deli Pouches-Refrigerated	Slaughter Cattle, Slaughter Poultry, Slaughter Hogs, Beef And Veal
67	Packaged Meats-Deli	Lunchmeat-Sliced-Refrigerated	Slaughter Hogs, Slaughter Poultry, Slaughter Cattle
68	Packaged Meats-Deli	Sausage-Breakfast	Slaughter Hogs, Slaughter Poultry
69	Pet Food	Cat Food - Dry Type	Soybeans, Other Grains, Slaughter Poultry, Slaughter Cattle, Unprocessed Finfish
70	Pet Food	Dog & Cat Treats	Soybeans, Other Grains, Slaughter Hogs, Slaughter Poultry, Slaughter Cattle, Unprocessed Finfish
71	Pet Food	Dog Food - Dry Type	Soybeans, Other Grains, Slaughter Poultry, Slaughter Cattle, Unprocessed Finfish
72	Pet Food	Dog Food - Wet Type, Dog Food - Moist Type	Soybeans, Other Grains, Slaughter Poultry, Slaughter Cattle, Unprocessed Finfish
73	Pickles, Olives, And Relish	Pickles - Sweet	Vinegar, Sugar, Cucumbers
74	Pickles, Olives, And Relish	Relishes	Vinegar, Sugar, Cucumbers, Mangoes, Corn
75	Pizza/Snacks/Hors Doeurves-Frzn	Pizza-Frozen	Cheese, Wheat Flour, Wheat, Refrigerated Storage
76	Prepared Food-Ready-To-Serve	Chicken - Shelf Stable	Poultry Processing
77	Prepared Food-Ready-To-Serve	Chili-Shelf Stable	Beef And Veal, Beans City Average
78	Prepared Food-Ready-To-Serve	Stew - Beef - Shelf Stable, Stew - Remaining - Shelf Stable, Stew - Chicken - Shelf Stable	Poultry Processing, Beef And Veal
79	Prepared Foods-Frozen	Entrees - Meat - 1 Food - Frozen	Slaughter Cattle, Slaughter Poultry, Slaughter Hogs, Beef And Veal
80	Shortening, Oil	Cooking Sprays	Olive Oil, Soybean Oil, Vegetable Oil, Sunflower Oil, Rapeseed Oil
81	Skin Care Preparations	Hand & Body Lotions	Fatty Acids, Starch Vegetable Fats Oils
82	Skin Care Preparations	Hand Cream	Fatty Acids, Starch Vegetable Fats Oils
83	Skin Care Preparations	Skin Cream-All Purpose	Fatty Acids, Starch Vegetable Fats Oils
84	Snacks	Dip - Mixes	Dry Onions, Salt Pepper Spices
85	Snacks	Popcorn - Popped, Snacks - Caramel Corn	Cheese, Cocoa Beans, Corn
86	Snacks	Snacks - Health Bars & Sticks	Whey, Corn Starch, Sugar, Vegetable Oil, Peanuts, Almonds
87	Snacks	Snacks - Potato Chips, Snacks - Potato Sticks	Corn Starch, Salt Pepper Spices, Russet Potatoes, Vegetable Oil
88	Snacks	Snacks - Pretzel	Wheat Flour, Eggs, Sugar
89	Snacks	Snacks - Remaining	Russet Potatoes, Corn, Wheat, Vinegar
90	Soft Drinks-Non-Carbonated	Water-Bottled	Plastic Bottles
91	Spices, Seasoning, Extracts	Meat Marinades & Tenderizers	Salt Pepper Spices, Spices

Market	NielsenIQ Product Group	NielsenIQ Product Modules in Product Market	Cost Controls
92	Spices, Seasoning, Extracts	Pepper	Spices, Salt Pepper Spices
93	Spices, Seasoning, Extracts	Salt - Cooking/Edible/Seasoned	
94	Spices, Seasoning, Extracts	Seasoning-Dry	Spices, Salt Pepper Spices
95	Spices, Seasoning, Extracts	Vegetables - Onions - Instant	Dry Onions
96	Stationery, School Supplies	Dry Erase Bulletin Board And Accesory	Aluminum
97	Stationery, School Supplies	Personal Planners Binders And Folders	Pulp Paper, Plastic
98	Tobacco & Accessories	Cigarettes	Tobacco, Pulp Paper
99	Unprep Meat/Poultry/Seafood-Frzn	Frozen Poultry	Poultry Processing, Slaughter Poultry, Processed Foods And Feeds
100	Vegetables - Canned	Mushrooms - Shelf Stable	Vinegar
101	Vegetables - Canned	Vegetables-Mixed-Canned	Carrots, Vinegar, Beans City Average
102	Vegetables - Canned	Vegetables-Peas-Remaining-Canned, Vegetables-Peas-Canned, Vegetables-Peas & Carrots-Canned	Vinegar, Green Peas, Carrots, Pinto Beans
103	Vegetables And Grains - Dried	Rice - Instant	Fertilizer
104	Wine	Wine-Domestic Dry Table, Wine-Imported Dry Table	Wine Grapes, Us Aud Conversion, Us Euro Conversion

Table C.1: Product Market Definitions and Cost Controls

Parties	Year	Agency	Markets
Lactalis / Kraft-Heinz	2021	DOJ	Feta cheese
			Ricotta cheese
Dairy Farmers Association / Dean's	2020	DOJ	Fluid milk
Foremost / Dean's	2011		
Country Lake / Superior	1990		
Danone / WhiteWave	2017	DOJ	Fluid organic milk
Dairy Farmers Association / SODIAAL	2000	DOJ	Branded whipped butter
			Branded stick butter
Post / TreeHouse	2019	FTC	Private label RTE cereal
General Mills / Ralcorp	1996	FTC	All RTE cereal
Bimbo / Sara Lee	2011	DOJ	Sliced bread
Earthgrains / Specialty Foods	1999	DOJ	Plain white bread
US Sugar / Imperial	2021	DOJ	Refined sugar
Crisco / Wesson	2018	FTC	Canola and vegetable oils
			Branded canola and vegetable oils
Connors / Bumble Bee	2004	DOJ	Mainstream sardine snacks
Nestle / Dryer	2003	FTC	Superpremium ice cream
Vlassic / Claussen	2002	FTC	Refrigerated pickles
Nestle / Ralston Purina	2001	FTC	Dry cat food
Heinz / Beech-Nut	2000	FTC	Prepared baby food, including jarred
Philip Morris / Nabisco	2000	FTC	Dry-mix gelatin
			Dry-mix pudding
			No-bake dessers
			Baking powder
			Intense mints

Table C.2: Market definitions for assorted food products

Parties	Year	Agency	Markets
P&G / Billie	2020	FTC	Wet shave system razors
			Disposable razors
Unilever / Alberto	2011	DOJ	Value shampoo
			Value conditioner
			Hairspray
Gilette / Eemland	1989	DOJ	Wet shaving razor blades
American Safety Razor / Ardell	1989	DOJ	Single-edge razor blades
P&G / Gilette	2005	FTC	At-home teeth whitening products
			Adult battery-powered toothbrushes
			Rechargeable toothbrushes
			Men's antiperspirant
Loreal / Carson	2000	DOJ	Adult women's hair relaxer kits
Kimberly-Clark / Scott	1995	DOJ	Facial tissues
			Baby wipes
Elanco / Bayer	2020	FTC	Oral canine flea medication
J&J / Pfizer	2006	FTC	OTC H-2 blockers
			OTC hydrocortisone anti-itch products
			OTC nighttime sleep aids
			OTC diaper rash treatments
P&G / Rorer	1990	DOJ	OTC stomach remedies
Jarden / K2	2007	FTC	Monofilament fishing line
Reckitt and Coleman / Benckiser	1999	FTC	Hard surface bathroom cleaners
			Fine fabric wash products
Rohm & Haas / Morton	1999	FTC	Water-based floor care polymers
SC Johnson / Dow	1998	FTC	Soil and stain removers
			Glass cleaners

Table C.3: Market definitions for assorted cosmetic products, medicine, and household products

Parties	Year	Agency	Markets
Altria / JUUL	2020	FTC	Closed-system e-cigarettes
Reynolds / Lorillard	2015	FTC	Combustible cigarettes
American Maize Products / Bayuk	1981	DOJ	Cigars
Gallo / Constellation	2021	FTC	Low-priced sparkling wine
			Low-priced brandy
			Low-priced port and sherry
AB InBev / Craft Brew Alliance	2020	DOJ	Beer
AB InBev / SAB Miller	2016		
Anheuser-Busch / InBev	2008		
Heileman / Pabst	1982		

Table C.4: Market definitions for tobacco and alcohol products