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

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### **ABSTRACT**

We document the effects of a comprehensive set of mergers of US consumer packaged goods manufacturers on prices, quantities, and product assortment. Across specifications, we find a small average price effect of mergers (-0.6% to 1.0%) but substantial heterogeneity in effects, with a standard deviation between 4.0–7.5 pp. Through a model of enforcement, we find that agencies act as if they challenge mergers they expect would increase prices more than 4.8–6.3%. Increases in stringency would reduce prices and the prevalence of completed price-increasing mergers, with minimal impacts on blocked price-decreasing mergers, at a significantly greater agency burden.

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

Our first contribution is to systematically analyze the effects of mergers in US CPG from 2006 to 2017. We study 129 product markets (e.g., canned soup or soluble coffee) in 47 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. Naturally, however, this representativeness is limited to CPG markets.

Our baseline estimates rely on comparisons within geographies and products before and after merger completion, controlling for brand-specific time trends, seasonality, input costs, and household income. The average effect of completed mergers on prices is small. Across specifications, we estimate effects between -0.6% and 1.0%. These average effects mask substantial heterogeneity: in our baseline specification, the first quartile of price effects corresponds to a price decrease of

2.1%, and the third corresponds to a price increase of 3.7%. The price changes of merging and non-merging parties are positively correlated and also show substantial heterogeneity.

We next consider effects on total quantities. Across specifications, we find that aggregate quantities, on average, decrease between 0.4% and 1.0%. We again find substantial heterogeneity. For our baseline specification, the first quartile of aggregate quantity changes is -9.7%, and the third quartile 5.0%. Merging parties are more likely to reduce quantity sold: in the baseline specification, their average quantity change is -6.4%. We find that quantity reductions for merging parties correlate with their price increases, their competitors' price decreases, reductions in the number of stores served, reduced product offerings across markets, and the elimination of products at the national level.

Given the heterogeneity in effects across mergers, the agencies have a challenging task of deciding which mergers to screen. To assist in this task, both the 2010 Horizontal Merger Guidelines and the 2023 Merger Guidelines provide “structural presumptions”—related to the Herfindahl-Hirschman Index (HHI), its change induced by the merger (DHHI), and the merging firms' market share—that connect market structure to the likelihood that a merger raises competitive concerns.<sup>1</sup> We find evidence favoring the Guidelines' use of DHHI and merging share in screening, as price changes both across and within-merger are correlated with these metrics

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 observed among consummated mergers would be a measure of stringency. In reality, agencies have a noisy estimate of the impact of a merger at the time of making a

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<sup>1</sup>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.

decision. Agencies may thus make two types of mistakes: blocking pro-competitive mergers (“type I errors”) and allowing anti-competitive ones (“type II errors”). Enforcement has to balance these risks. For instance, it would be premature to conclude that agencies should be more strict even after observing a positive average price change: doing so might significantly increase the prevalence of blocked pro-competitive mergers, for instance, as it could be difficult to disentangle pro- and anti-competitive mergers ex-ante.

To quantify stringency, we develop and estimate a model of the agencies’ decision to propose a remedy for a merger. In the model, the agency receives a signal of the price change of the merger and proposes a remedy if, based on this signal and its prior, it expects the merger to increase prices beyond a threshold. We show that this can be viewed as the reduced form of a model that captures the trade-offs outlined above. Using data on enforcement decisions and estimates of the realized price changes, we estimate that on average the US antitrust agencies aim to propose remedies for CPG mergers with a mean price increase greater than 4.8–6.3%. Furthermore, our model allows us to estimate the noise in the agencies’ ex-ante assessments of merger effects and thus to simulate the effects of counterfactual antitrust stringency and to quantify the two sides of the aforementioned trade-off.

Moving to a 2.5% threshold would reduce aggregate price changes of consummated mergers by about 1.1 pp and decrease the probability of allowing anti-competitive mergers. On the other side of the trade-off, this threshold would require the agency to challenge almost four times as many mergers. Moreover, we find a negligible impact on the probability of type I errors, suggesting that concerns that stricter thresholds would lead to the unintended consequence of blocking more pro-competitive mergers are likely unwarranted.

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

One class of merger retrospectives involves in-depth studies of a small handful

of mergers, usually focusing on prices and quantities. Papers have studied airlines (Peters, 2006; Kwoka and Shumilkina, 2010; Luo, 2014; Das, 2019), assorted consumer products (Ashenfelter and Hosken, 2010; Weinberg and Hosken, 2013), appliances (Ashenfelter et al., 2013), beer (Ashenfelter et al., 2015; Miller and Weinberg, 2017), hospitals (Haas-Wilson and Garmon, 2011; Garmon, 2017; Garmon and Bhatt, 2022) and gasoline (Simpson and Taylor, 2008; Lagos, 2018).<sup>2</sup> Some of these papers also compare results to merger simulations (Peters, 2006; Ivaldi and Verboven, 2005; Weinberg and Hosken, 2013; Björnerstedt and Verboven, 2016; Garmon, 2017). Kwoka (2014) provides a helpful meta-analysis and Asker and Nocke (2021) survey many of these results, but these analyses are naturally still subject to selection into publication.

To address this issue, some papers have studied a large subset of mergers in a particular industry: Kim and Singal (1993) study 14 airline mergers from 1985–1988, and Focarelli and Panetta (2003) study 43 mergers of Italian banks from 1990–1998. A handful of contemporaneous papers develop larger databases of M&A activity. Some studies focus on prices: in consumer packaged goods (Majerovitz and Yu, 2025), 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 (2025) provide estimates of consumer surplus changes, 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. (2023), who study the effect of mergers on product offerings. Demirer and Karaduman (2025) show that mergers of US power plants typically improve efficiency. Benson et al. (2024) document that bank mergers lead to branch closings.

Finally, we contribute to the literature that studies the agencies’ decisions. Prior work has correlated enforcement with ex-ante merger characteristics (Bergman et al., 2005; Kwoka, 2014; Affeldt et al., 2021b) or computed required compensating

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

efficiencies using approximations leveraging ex-ante metrics of market structure (Affeldt et al., 2021a). Some papers have estimated causal impacts of antitrust enforcement on outcomes (Liebersohn, 2024; Chen et al., 2022; Reed et al., 2024) in industries including banking and pharmaceuticals. Others have correlated ex-post price changes with ex-ante structural presumptions (Brot et al., 2024) or measures of scrutiny (Brand et al., 2023). Our contribution is to directly assess and quantify the agencies’ objective in how to scrutinize mergers and to study the impact of counterfactual policies on challenges and errors.

More broadly, the increased interest in documenting merger effects parallels a growing literature estimating markups and documenting concentration at a large scale, following the seminal work of De Loecker et al. (2020). Grieco et al. (2023) document decreasing markups in the automobile industry, and Miller et al. (2025) document increasing markups in cement, over several decades. Brand (2021), Döpper et al. (2025), and Atalay et al. (2025) conduct similar exercises in consumer packaged goods. Benkard et al. (2023) document decreasing concentration in product markets. While we do not document markups or changes in concentration absent mergers, our paper sheds light into how merger activity has affected consumers.

## **II. Data and Sample Selection**

### **II.A. Data Sources**

We begin with the set of mergers tracked by SDC Platinum from Thompson Reuters, which provides comprehensive information on mergers, acquisitions, and joint ventures. We then restrict to transactions involving manufacturers of products sold in groceries and mass merchandisers, for which price and quantity data are available in the NielsenIQ Retail Scanner Dataset. NielsenIQ describes this dataset as providing “scanner data from 35,000 to 50,000 grocery, drug, mass merchandise, and other stores, covering more than half the total sales volume of US grocery and drug stores and more than 30 percent of all US mass merchandiser sales volume.” The data cover 2.6–4.5 million UPCs, depending on the year, and include food, non-food grocery items, health and beauty aids, and select general merchandise. We have access to this dataset from 2006 to 2018. NielsenIQ provides sales at the store-week level

and the average transaction price for each UPC, and it also provides a classification of products into “groups” and “modules.” We use NielsenIQ designated market areas (DMAs) as our geographic markets: these are collections of counties that are exposed to the same local television stations.

In Appendix A, we replicate our analysis with the NielsenIQ Consumer Panel Dataset, which comes from a sampling of households and therefore covers some large retailers that the scanner dataset misses. We find that results are largely similar but discuss some discrepancies below. Nevertheless, we prefer the scanner dataset as our baseline specification. Since the scanner dataset comes from a sampling of stores, it has complete coverage of UPCs sold within the store, including those with small share—which is critical when studying product assortment. Relatedly, in small product markets one does not observe many panelists making purchases in each geographic market. This can make variation over time in product sales unreliable.

Since NielsenIQ does not provide ownership of each product, we obtain this information from Euromonitor Passport.<sup>3</sup> 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 also include household income from the American Community Survey as a control.

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

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

The 2023 Merger Guidelines outline a number of ways to define antitrust markets. One such method, the “hypothetical monopolist test,” defines a market to be the

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<sup>3</sup>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, as the transfer of company prefixes can take up to a year, and there is no 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.

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

In light of this debate, we adopt the strategy of staying close to NielsenIQ categorizations to define markets. NielsenIQ divides products into groups, broad categories such as “Prepared Foods - Frozen” or “Condiments, Gravies and Sauces,” and modules, finer subcategories such as “Entrees - Meat - 1 Food - Frozen” or “Sauce Mix - Taco.” We typically use individual product modules as our markets, but after manual inspection we sometimes find it more appropriate to group product modules.<sup>5</sup> 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.<sup>6</sup> Appendix D.2 provides details.

We aim to identify all deals where the two parties competed in at least one product market-DMA during our estimation period, spanning 36 months before the deal’s announcement to 24 months past the deal’s completion. To do so, we keep deals in SDC Platinum valued at \$280 million dollars or more involving manufacturers of retail products. Second, we identify which of these transactions involve products tracked in the NielsenIQ Scanner Dataset, and check whether the parties overlapped: we look at all UPCs in the product market sold in the estimation period of the

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<sup>4</sup>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.

<sup>5</sup>Some cases are clear: the Wine product group includes as modules “Wine-Domestic Dry Table” and “Wine-Imported Dry Table.” In others, such as “Bratwurst” and “Frankfurters - Refrigerated,” the specific module definition seems arbitrary, and we find it more reasonable to group them.

<sup>6</sup>As discussed in Appendix D.2, the main difference is that DOJ/FTC market definitions sometimes exclude store brands and divide markets into quality tiers, although this is infrequent.

deal and select those with a non-negligible market share.<sup>7,8</sup> We assign each to their owners and only keep product markets where both the target and the acquirer sell at least one selected UPC in the same DMA in the estimation period.

Table D.1 presents a list of product markets for the deals in our final sample and their respective cost controls. In what follows, we refer to a product market-deal pair as a merger. For example, if X acquires Y and both sell in product markets 1 and 2, that deal generates two mergers. Our final sample consists of 129 mergers over 47 deals. Appendix D provides details about the sample and the construction procedure.

To compute outcomes, we restrict to a balanced panel of stores during our estimation period to ensure our results are not confounded by variation over time in the set of stores that report to NielsenIQ. Our price metric is the volume-weighted average monthly price by UPC and DMA, adjusted for inflation.<sup>9</sup> For non-price outcomes, we aggregate to the firm type (i.e., merging/non-merging) level and compute the following measures: (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 firm type.

## II.C. Properties of Approved Mergers

Table 1 presents summary statistics for our final sample. Each row corresponds to a NielsenIQ product group, which is coarser than our product market definitions (in Table D.1) but serves to illustrate in which broad product categories the mergers are taking place.<sup>10</sup> For each product group, we display the average—across mergers in the group—yearly product market sales in the pre-merger period, the merging parties’ and target’s revenue shares, and the average post-merger HHI and DHHI

<sup>7</sup>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.

<sup>8</sup>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 makes the number of products tractable, as we have to match ownership by hand, while respecting particularly varied product markets. In Appendix D.1, we document that this procedure leads to high coverage.

<sup>9</sup>For food mergers, we deflate by the CPI for food at home. For non-food mergers, we deflate by the CPI excluding food and energy. All dollar values are in 2010 dollars.

<sup>10</sup>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	Target's Revenue Share	HHI	DHHI
All	129	536.2	18.8	8.4	3006.3	154.8
Baby Food	1	1484.5	12.9	10.1	4912.3	119.9
Baked Goods-Frozen	1	5.5	42.6	2.0	5748.7	111.9
Beer	2	3473.5	29.7	13.5	3631.9	494.1
Bread And Baked Goods	15	633.8	17.3	5.4	3742.8	99.3
Breakfast Foods-Frozen	1	443.0	2.3	0.9	1622.6	0.9
Candy	4	1191.7	12.6	9.2	1696.5	48.5
Cereal	2	1064.0	7.3	0.9	2327.1	26.0
Coffee	2	1240.9	20.0	0.4	2048.6	42.2
Condiments, Gravies, And Sauces	11	45.0	37.8	13.1	4010.1	544.2
Cookies	1	1750.4	1.0	0.6	2450.2	0.1
Cosmetics	11	129.2	20.2	15.7	2645.3	207.3
Detergents	1	2124.6	11.6	7.3	2992.0	186.5
Fragrances - Women	1	97.1	13.7	10.2	2660.1	17.6
Grooming Aids	2	81.8	4.1	2.3	5924.8	2.4
Gum	2	708.2	47.3	46.7	3750.9	95.7
Hair Care	7	362.2	19.2	8.1	2584.0	469.8
Kitchen Gadgets	1	127.5	22.0	6.6	1102.5	107.9
Laundry Supplies	1	138.8	15.1	5.2	3170.9	263.6
Liquor	11	263.2	4.4	3.2	2109.3	20.5
Medications/Remedies/Health Aids	1	69.1	12.4	9.9	3694.8	26.7
Men's Toiletries	2	42.2	18.8	5.6	2623.8	1.0
Packaged Meats-Deli	9	834.5	8.5	4.4	1958.3	22.5
Pet Food	4	644.4	24.8	8.0	3134.5	115.9
Pickles, Olives, And Relish	3	51.7	17.8	0.7	2652.2	18.3
Pizza/Snacks/Hors D'oeuvres-Frzn	1	1496.8	42.0	38.7	2588.2	172.9
Prepared Food-Ready-To-Serve	3	93.6	9.4	2.1	4125.8	2.6
Prepared Foods-Frozen	1	260.4	3.0	2.7	1330.4	1.1
Shortening, Oil	1	122.7	16.5	16.3	3228.5	7.0
Skin Care Preparations	3	186.4	10.2	1.2	2182.6	102.1
Snacks	10	585.7	12.8	5.6	2645.9	81.5
Soft Drinks-Non-Carbonated	1	2317.4	16.7	6.0	2874.7	18.8
Spices, Seasoning, Extracts	5	134.8	48.8	15.4	3551.0	140.1
Stationery, School Supplies	2	86.5	13.6	12.3	2153.9	5.3
Tobacco & Accessories	1	4029.9	29.5	9.2	4311.9	47.3
Unprep Meat/Poultry/Seafood-Frzn	1	373.6	6.9	0.4	5168.8	0.8
Vegetables - Canned	2	30.6	8.0	7.6	4946.3	3.3
Vegetables And Grains - Dried	1	83.4	62.6	37.5	4662.2	1485.5
Wine	1	1559.4	22.0	3.5	1793.0	50.7

Table 1: Summary statistics for the final sample of mergers. This table presents summary statistics, by product group, for the 129 mergers in the final sample. The first column lists the number of mergers in each product group, followed by average yearly product market sales in the pre-merger period, merging parties' revenue share, target's revenue share, post-merger HHI, and DHHI. Average sales and revenue shares are computed across mergers. Average HHI and DHHI are computed across mergers and DMAs. HHI and DHHI are naively computed using volume shares. Product market sales are in 2010 dollars.

computed across mergers and DMAs using quantity-based shares.<sup>11</sup>

Panels (a) and (b) of Figure 1 present histograms of average post-merger HHI

<sup>11</sup>Here and in all analyses that follow, we weigh by DMA volume when computing the average HHI or DHHI across DMAs.

and DHHI. Most mergers have average (across DMAs) post-merger HHIs between 2,000 and 5,000, with some over 7,000. 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 6,000. Panel (d) presents a scatter plot of average yearly sales of the merging parties (in millions of dollars) and DHHI. Around two-thirds of the mergers with DHHI over 200 are small. These patterns are consistent with the selection process determining the completion of a merger: 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 DHHI have been consummated in large product markets.

### III. The Effects of Consummated Mergers

#### III.A. Empirical Strategy

Our approach to estimate the effect of mergers on the outcomes of interest is a before-after comparison: we compare outcomes before and after the merger controlling for trends, tastes for products, seasonality, and input costs. We implement the procedure in two steps. First, we use data for the 36 months prior to the merger and regress

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

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  $i$ ,  $\xi_{id}$  is a UPC-DMA fixed effect, and  $\xi_{m(t)}$  is a month-of-year fixed effect. The controls include the input cost series specified in Appendix D.2 as well as the log of median household income per person. This regression allows us to identify a brand-specific time trend after controlling for differences in tastes for products across cities, seasonality, and changes in input costs. We then use data for the 24 months after merger completion and regress

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

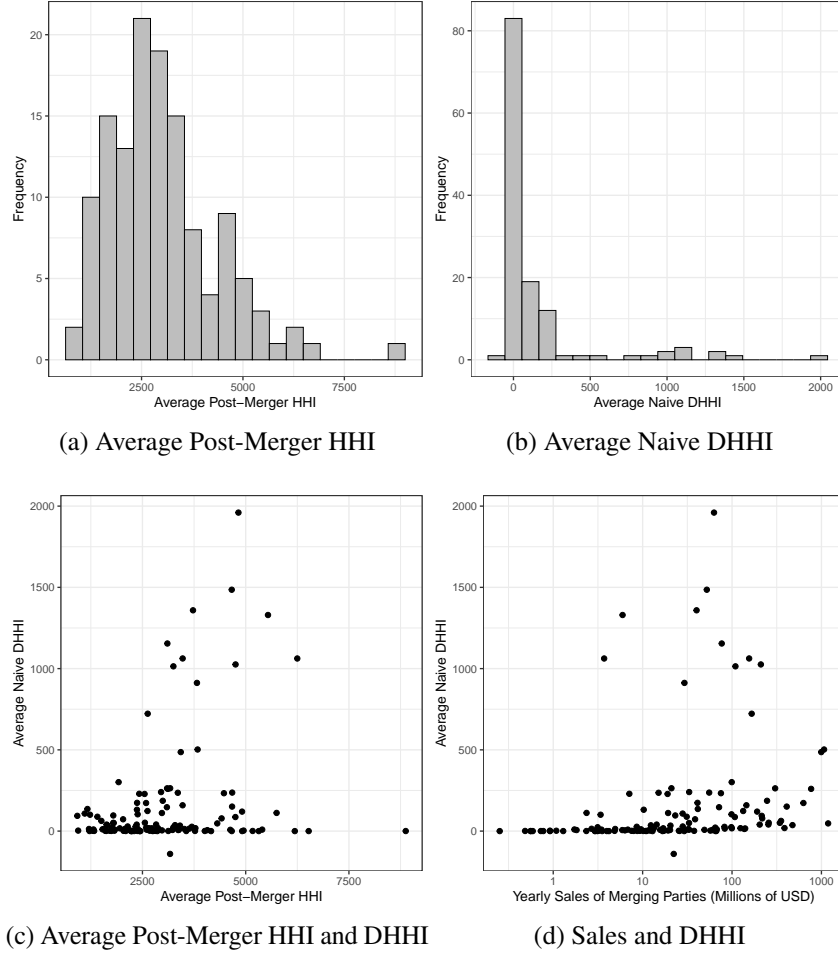


Figure 1: Distribution of post-merger HHI, naive DHHI, and merging parties' yearly sales. Panels (a) and (b) present histograms of average post-merger HHI and DHHI. Panels (c) and (d) present scatter plots of average post-merger HHI and yearly sales (in 2010 dollars) of merging parties, respectively, against average DHHI. HHI and DHHI are naively-computed using volume shares. Averages are taken across DMAs, weighed by the volume sold in each DMA.

where  $\widehat{\log y_{idt}}$  is the predicted value of the log of the outcome, obtained from (1). We use a two-step procedure so that the pre-trend is not affected by post-merger changes. The coefficients of interest are  $\beta_1$  and  $\beta_2$ , 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 weigh

all regressions by pre-merger volume at the UPC-DMA level.<sup>12</sup> We aggregate across mergers by weighing uniformly.

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, seasonality, and changes in input costs. We estimate the merger effect as the departure from the trend 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.

We interrogate this strategy in a number of different ways. First, is a linear time trend sufficient to capture changes in the environment? We address this question by augmenting (2), expanding the horizon to the full sample period around the merger and adding monthly merging and non-merging party coefficients:

$$\log y_{idt} - \widehat{\log y_{idt}} = \sum_{\tau=-36}^{24} \left( \beta_{1,\tau} \mathbb{1}[\text{Merging Party}]_i \cdot \mathbb{1}[t = \tau] + \beta_{2,\tau} \mathbb{1}[\text{Non-Merging Party}]_i \cdot \mathbb{1}[t = \tau] \right) + \epsilon_{idt}. \quad (3)$$

We then study trends in  $\beta_{1,\tau}$  and  $\beta_{2,\tau}$ , reporting averages separately for mergers in the top and bottom 25th percentile of the change in the outcome of interest and for mergers with changes between these percentiles. For example, see Figure 3 for prices. We do not find significant patterns in pre-period outcomes after controlling for the linear time trend, which is not a mechanical effect of the procedure. This provides evidence that a linear time trend is an appropriate control for the evolution of prices in the pre-period, which bolsters our confidence that it would continue to serve as an appropriate approximation to the counterfactual in the post-period.

These results also help alleviate the concern that some other event (e.g., expecting a new entrant) induced 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

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<sup>12</sup>In the second stage we use two-way clustered standard errors, at the brand and DMA level, to account for correlation in the prediction error of the left-hand side variable.

that changes happen soon after merger completion. These patterns are difficult to explain without attributing them to the merger itself, unless these other events are systematically coincident with merger completion, which we find unlikely.<sup>13</sup>

Second, we ask whether the results we find are driven by extrapolation of the pre-period trend. For example, if large positive trends are spurious we could find large but spurious negative price effects. In Figure C.1, we document that average brand trends have little explanatory power over estimated price effects. While we observe a statistically significant negative correlation between the two,<sup>14</sup> the magnitude is much smaller than what one would expect if results were driven fully by the trend.

Finally, Appendix C.1 also reports results from a set of placebo analyses where we randomize merger dates within the same markets. While we also find variation in the placebo estimates, we find that the distribution of estimated price effects is centered at zero and narrower than our baseline estimates. We also find that the mean squared prediction error (MSPE) in the post-period (relative to the pre-period) is significantly lower than in the real merger dates: the linear time trend is a better predictor in the post-period when the merger date is randomly drawn versus when the true merger date is used. This is consistent with prices departing more from the trend after a merger than after a typical date, which is further justification that this simple and transparent empirical strategy is capturing real effects of the merger.

*Alternative Approaches.* There are three alternative approaches to constructing counterfactual post-merger outcomes that we have chosen not to follow. The first is to use products of non-merging firms in the same market as the control group (Ashenfelter and Hosken, 2010; Haas-Wilson and Garmon, 2011). 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 strategies in response to the merger, and we thus avoid this approach.

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<sup>13</sup>We also find that mergers are not systematically completed on "special" days of the year (e.g., starts of quarters). Furthermore, Figure C.4 shows that mergers are distributed evenly across time and are not clustered, for example, during the financial crisis.

<sup>14</sup>A reasonable prior is that the true effect of the merger should be uncorrelated with trends. Interpreting the correlation as a test of the identification strategy requires this prior. However, this need not be the case: if mergers happen in dying industries due to market power reasons and they happen in growing ones due to synergies, we would see a negative correlation.

A second strategy is to use goods in other markets that are subject to similar cost and demand shocks (Ashenfelter et al., 2013; Kim and Singal, 1993). The justification is that we would not expect strategic responses to the merger in these markets. Thus, any outcome change for the control group is likely due to cost or demand changes. At the same time, this strategy 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, which is difficult at scale. One approach to circumvent this issue is to use synthetic controls. However, researchers still need to carefully select the donor pool to avoid overfitting (Abadie, 2021): one cannot simply use all untreated markets in the NielsenIQ database as donors. When searching for potential donors, we find that deals often treat multiple similar markets, ruling out the most natural candidates for the donor pool. Moreover, we have experimented with this approach for some mergers and have found that estimates are often sensitive to the exclusion of singular donors. Overall, we are not confident that one can scale this approach to answer the question of interest.

The third approach is to use as a control group outcome changes in geographic markets where the merging parties comprise a small share of total sales (Dafny et al., 2012). The rationale is that uncaptured changes to the post-merger environment will affect both treated and untreated markets and can be controlled for by looking at differential changes in treated markets. There are three main drawbacks to this strategy. First, we lose all national mergers. This corresponds to 39 out of 129 mergers in our sample. Second, untreated markets are not random: they are locations that the two parties involved in a large merger have chosen not to serve. In fact, we find that the difference in price trends for treated and untreated markets is often large. Finally, non-merging parties that engage in regional pricing (Adams and Williams, 2019; DellaVigna and Gentzkow, 2019; Hitsch et al., 2021) may respond to the merger in untreated markets if those markets share a pricing region with treated markets, again leading to an underestimate of the merger effect.

### **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

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
Overall	129	0.31 (0.40)	4.59	-2.09 (0.40)	0.00 (0.48)	3.65 (0.54)
Merging Parties	129	0.12 (0.62)	7.07	-3.22 (0.62)	0.38 (0.58)	3.74 (0.65)
Non-Merging Parties	129	0.48 (0.42)	4.81	-2.24 (0.38)	-0.13 (0.46)	4.24 (0.65)

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.

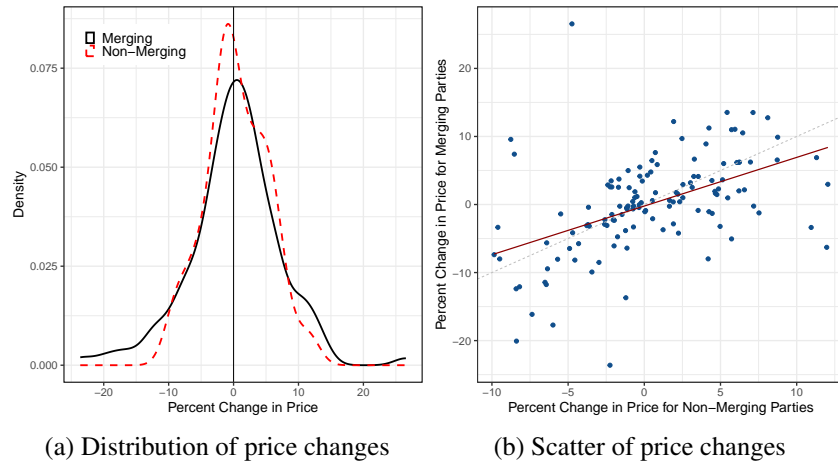


Figure 2: Price changes for merging and non-merging parties, as estimated by (2). These plots display transformed coefficient estimates (e.g.,  $100 \cdot (\exp(\hat{\beta}_1) - 1)$ ) for the price change of the merging and non-merging parties.

parties. We transform estimates from (2) to report percentage changes. Note that we estimate the effect of mergers on retail prices paid by end consumers 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.<sup>15</sup>

The results from the baseline specification (Panel A) show that mergers have modest price effects on average: the mean is 0.3%, while the averages for merging

<sup>15</sup>Section 1 of the 2010 Horizontal Merger Guidelines states “The Agencies examine effects on either or both of the direct customers and the final consumers. The Agencies presume, absent convincing evidence to the contrary, that adverse effects on direct customers also cause adverse effects on final consumers.”

and non-merging parties are 0.1% and 0.5%, respectively.<sup>16</sup> However, there is substantial dispersion around these averages. For merging parties, 25% of mergers raise prices by over 3.7%, but also 25% of mergers lower prices by over 3.2%; the first and third quartiles of non-merging price changes are -2.2% and 4.2%. Panel (a) of Figure 2 presents the distribution of price changes. Merging parties are more likely than non-merging parties to have price changes in the extremes.

Panel (b) of Figure 2 depicts the correlation between price changes for merging and non-merging parties. Price changes are positively correlated (correlation = 0.49, s.e. = 0.08), consistent with strategic complementarity, but outcomes are varied. We find that 36% of mergers lead both merging and non-merging parties to lower prices for consumers. One potential explanation is that cost synergies drive merging parties' prices down, and their rivals follow. Another 36% of mergers lead to higher prices for both groups of firms. Strategic complementarities in pricing could explain these points as well: the internalization of pricing spillovers leads merging parties to increase prices, and rivals find it optimal to follow. There are also several cases where one group of firms increases prices and the other lowers them. In particular, 13% of mergers cause merging parties to lower prices and non-merging parties to raise them, and 15% cause the opposite. 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. Panel (a) presents results for mergers in the top quartile of price increases, Panel (b) for those in the bottom quartile, and Panel (c) for the remainder. These results shed light on how quickly merging parties begin to increase prices, how long it takes their rivals to respond, and how long it takes until cost synergies are passed through. As discussed in the previous subsection, these plots also serve as a check on our identification assumptions. We do not find pre-trends in average prices before the merger for each

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<sup>16</sup>One should not interpret these means as saying that a “typical” merger leads to a price increase for non-merging parties but no price effect for merging parties. A key driver of this difference is that when prices fall on average, they fall more for merging than non-merging parties; see Figure 3.

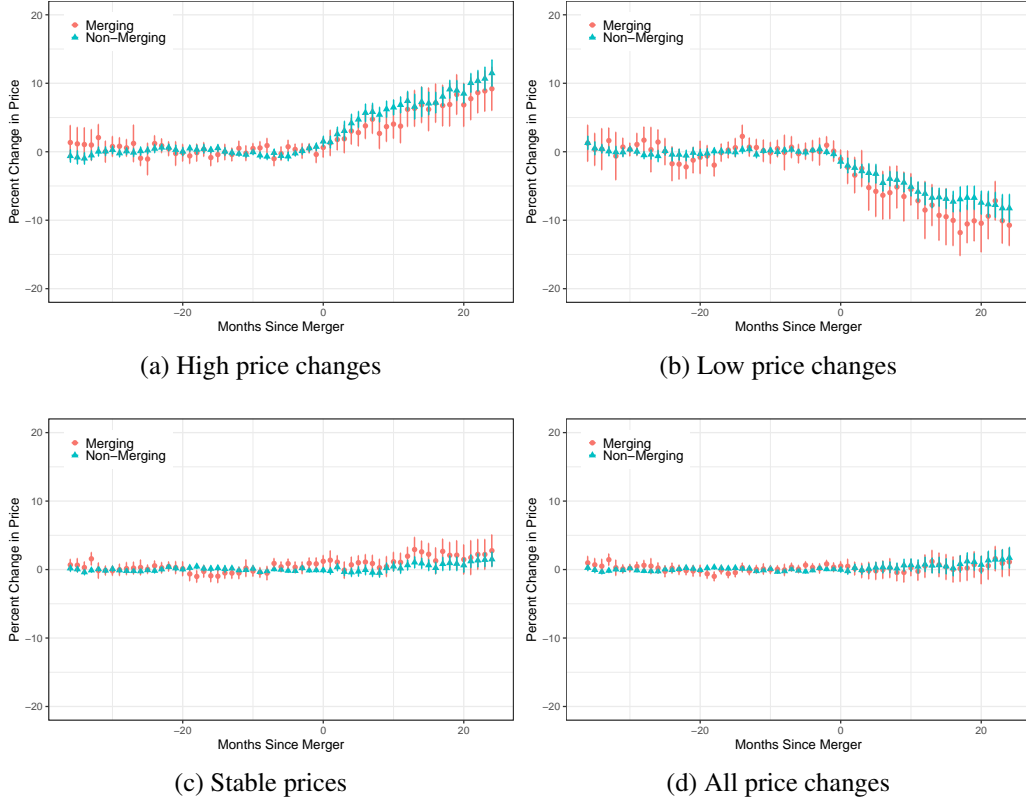


Figure 3: Timing of price changes, for merging parties (red circle) and non-merging parties (blue triangle). The marker indicates the mean price change a given number of months after the merger becomes effective, and the thick line is the 95% confidence interval of that mean. Panels (a)–(c) show subsamples: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

of the three categories of price changes.<sup>17</sup>

For the mergers that led to the largest price increases, we find that merging party prices begin increasing upon completion, are roughly 5% higher five months after the merger, and undergo a further increase roughly a year after completion. To the extent that the merged entity takes time to renegotiate contracts with supermarkets, for example, it could take 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

<sup>17</sup>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.

immediate responses for the merging parties, continuing to approximately 18 months 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. 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.

### III.C. Quantities

While most merger retrospectives have focused on prices, another natural question is whether mergers have reduced transacted quantities. 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 aggregate because 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. We weigh these regressions by the pre-merger share of the firm type-DMA and cluster at the DMA level.

Table 3 and Figure 4 show results from this analysis. We find a drop in quantities of 0.5% on average. Moreover, 60% of mergers led to total quantity reductions. Merging parties exhibit a large average quantity drop of 6.4%. The quantiles reported in Table 3 and Figure 4 indicate that distributions of quantity changes are slightly left-skewed: the third quartiles are similar, but the first quartile and median are much smaller for merging parties. There is also significant variation in quantity effects for merging parties: the standard deviation and inter-quartile range are both around 30 pp. The variation is much smaller for non-merging parties.

González et al. (2023) show that mergers can induce supply disruptions, which could reduce quantity. Since the welfare interpretation of a quantity decline changes if part of the drop is transitory, in Figure C.5, we study the time path of quantity changes. We find that quantity effects do not seem to be driven by temporary disruptions, but rather by a permanent change in strategies by the firms.

Panel (b) of Figure 4 shows that quantity changes for merging and non-merging parties have a correlation of 0.36 (s.e. 0.08). The empirical result is at odds with

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
Overall	129	-0.54 (1.37)	15.55	-9.68 (1.22)	-2.34 (0.96)	5.02 (1.92)
Merging Parties	129	-6.41 (2.86)	32.53	-20.74 (2.74)	-9.17 (2.22)	5.74 (2.67)
Non-Merging Parties	129	0.66 (1.47)	16.75	-7.98 (1.64)	-1.58 (0.73)	5.49 (1.74)

Table 3: Overall 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.

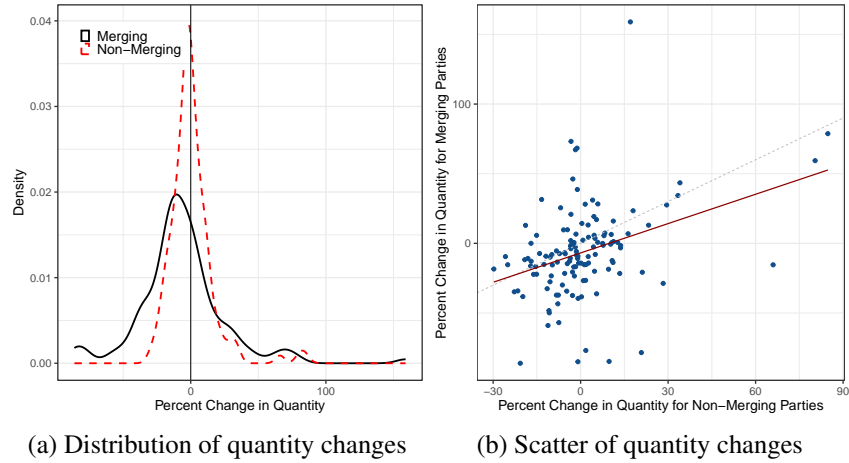


Figure 4: Quantity changes for merging and non-merging parties, as estimated by (2). This plot displays transformed coefficient estimates (e.g.,  $100 \cdot (\exp(\hat{\beta}_1) - 1)$ ) for the quantity change of the merging and non-merging parties.

results on demand systems with the “type aggregation property” (Nocke and Schutz, 2018, 2024), where mergers lead to negatively correlated quantity changes. However, we are not aware of similar predictions for general demand systems. Moreover, in our setting firms often respond on dimensions beyond price, as we document below.

Figure 5 plots the estimated quantity effects against the estimated price effects for merging (Panel (a)) and non-merging parties (Panel (b)). The unconditional correlation between price and quantity changes is statistically insignificant in both cases; moreover, many mergers exhibit price and quantity changes in the same direction. To understand this, note that the merging quantity effect depends on

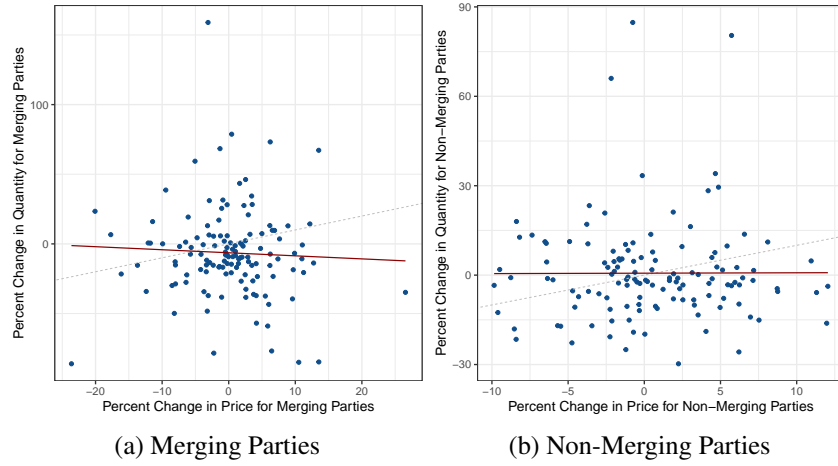


Figure 5: Scatter of price versus quantity changes for merging and non-merging parties. Panel (a) displays a scatter plot of price changes versus quantity changes for merging parties. Each blue point represents a merger, the red line is the estimated best fit. Panel (b) presents the same scatter plot, but for non-merging parties.

more than just the merging price effect. Figure 6 in Section III.D illustrates this by showing in its first panel the distribution of non-merging price effects by the quadrant of Figure 5(a): quadrant 1 refers to mergers where merging price and quantity effects are both positive, quadrant 2 refers to cases where merging price effects are negative and merging quantity effects are positive, and so on. We see that mergers in quadrant 1 also typically have positive non-merging price effects, and those in quadrant 3 typically have negative non-merging price effects. Substitution away from non-merging goods may partly explain these results, as could other strategic responses, such as changes to product assortment or distribution. We examine these other responses in the next section.

### 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 2.7% reduction in the number of stores served by the

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Number of Stores						
Overall	129	-0.08 (0.22)	2.46	-0.71 (0.14)	-0.13 (0.06)	0.16 (0.07)
Merging Parties	129	-2.74 (1.58)	17.95	-6.51 (1.62)	-0.53 (0.28)	0.79 (0.24)
Non-Merging Parties	129	0.06 (0.29)	3.26	-0.59 (0.20)	-0.00 (0.02)	0.27 (0.12)
B. Number of Brands (DMA)						
Overall	129	-3.39 (0.92)	10.40	-8.69 (1.14)	-2.60 (0.67)	1.05 (0.50)
Merging Parties	129	-2.20 (2.08)	23.57	-13.14 (2.58)	-1.96 (0.95)	4.02 (1.21)
Non-Merging Parties	129	-3.22 (0.97)	11.06	-9.12 (1.24)	-2.58 (0.72)	2.06 (1.23)
C. Number of Brands (National)						
Overall	129	1.05 (2.14)	24.30	-7.60 (0.89)	-2.57 (0.95)	2.67 (0.87)
Merging Parties	129	-0.07 (2.47)	28.09	-11.27 (1.13)	-1.36 (0.76)	3.21 (1.09)
Non-Merging Parties	129	1.19 (2.11)	23.96	-7.48 (0.86)	-2.33 (0.94)	3.46 (1.25)

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.

merging parties, on average, but there is substantial heterogeneity in these effects.

In 38% of mergers, store networks expand. This is consistent with the pro-competitive argument that economies of scale and production reallocation make it profitable to increase distribution. 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 -6.5%. 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: on average, 40.4% of these stores are eliminated from the distribution network post-merger, compared to 28.7% for stores served only by the acquirer, and 13.4% for stores served by both. This could be indicative

of contracting frictions, such as breakdowns in negotiating new agreements with retailers, restrictions imposed by exclusivity agreements, or costs of supplying certain stores.

Panels B and C of Table 4 report statistics for the changes in the number of brands sold at the DMA level and national level, respectively. We focus on brands, rather than UPCs, as we do not want to consider cases where a particular package size for an existing product is added or removed. We look at outcomes at the DMA and national level separately to study both changes in geographic footprint and the outright elimination of brands. In contrast to the findings for the number of stores, both merging and non-merging parties adjust their product portfolios. We find that merging (non-merging) parties decrease the number of brands sold in a DMA by 2.2% (3.2%) on average following a merger. Considering their national portfolios instead, we see smaller mean effects. In both cases, the left tail is long.

Which brands are being dropped? Table 5 explores this issue by regressing an indicator for whether a brand is dropped on pre-merger observables, for the set of products owned by the merged entity. Brand dropping is defined as a brand having no sales nationally or at the DMA level for at least 6 months before the end of the post-period. Columns (1) and (2) report results at the national level, so that each observation is a merger-brand. We find that products with low brand share, defined as the volume share of the brand in the merged entity's pre-merger national sales, are more likely to be dropped. Brands that comprise less than 5% of the merged entity's sales are 6 pp more likely to be dropped. Unlike Atalay et al. (2023), we do not find any differences between target and acquirer brands. Columns (3)–(6) repeat this analysis with outcomes at the brand-DMA level. We define DMA or DMA-brand shares as the volume share of the DMA or DMA-brand in the merged entity's pre-merger national sales. Brands are more likely to be dropped in small DMAs, when the brand-DMA is a small fraction of the merged entity's national sales and when it has a small share in the DMA. Again, we do not see meaningful differences between target and acquirer brands. Overall, we find that firms are more likely to drop small brands post-merger, as well as brands in small DMAs.<sup>18</sup>

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<sup>18</sup>An interesting question is whether duplicative brands are dropped after a merger. Without more characteristics of the products, we are unable to address this directly. However, we later document

	Dropped Nationally		Dropped in DMA			
	(1)	(2)	(3)	(4)	(5)	(6)
Target	0.00 (0.01)	0.00 (0.01)	-0.03 (0.02)	-0.03 (0.02)	-0.03 (0.02)	-0.03 (0.02)
Brand Share	-0.12 (0.03)					
Brand Share < 5%		0.06 (0.01)				
DMA Share			-1.00 (0.19)			
DMA Share < 0.5%				0.04 (0.01)		
DMA-Brand Share					-1.45 (0.29)	
Brand Share Within DMA					-0.17 (0.02)	
DMA-Brand Share < 0.1%						0.05 (0.01)
Brand Share Within DMA < 5%						0.08 (0.01)
Observations	860	860	118,388	118,388	118,388	118,388
R <sup>2</sup>	0.163	0.166	0.132	0.133	0.147	0.154
Dependent Mean	0.029	0.029	0.098	0.098	0.098	0.098

Table 5: Determinants of Brand Dropping. This table displays regressions of an indicator for whether a brand is dropped on pre-merger observables, for the set of products owned by the merged entity. Brand dropping is defined as a brand having no sales nationally or at the DMA level for at least 6 months before the end of the post-period. Columns (1) and (2) report results at the national level, and Columns (3)–(6) at the DMA level. Target is an indicator for whether the brand is owned by the target firm. Brand, DMA, and DMA-Brand Shares are the volume share of the brand, DMA, or brand-DMA in the merged entity’s pre-merger national sales. Brand share within DMA is the pre-merger volume share of the brand within the DMA. All specifications include merger fixed effects, and standard errors are clustered at the merger level.

Note, however, that not all mergers lead to contractions in distribution or the number of brands. The number of DMA-brands increases in 43% of mergers, and the number of brands sold nationally increases in 40% of mergers. These changes are more muted than the cases where brands are dropped.

These changes in product assortment and in distribution are important drivers of

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that the quantity declines after brand removal, which suggests that the story is not the elimination of duplicative brands but rather the elimination of small brands or brands in small markets.

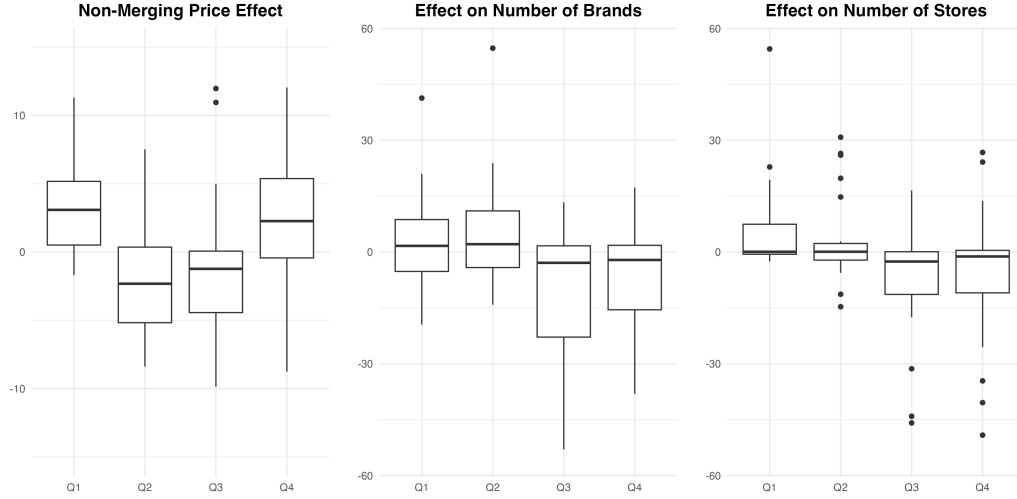


Figure 6: Boxplots for other strategic responses, by price-quantity effect quadrant. Q1 and Q4 correspond to price increases, and Q1 and Q2 correspond to quantity increases. Effect on Number of Brands refers to the analysis at the brand-DMA level.

changes in quantities for merging parties. The latter two panels of Figure 6 show the effects of the number of brands and the number of stores, binned by the quadrant of the merger. Here, we find that mergers in quadrant 3—where merging quantities drop despite a drop in merging party prices—typically have reductions in the number of brands, suggesting an important non-price explanation for the observed outcomes. We find a similar distribution for mergers in quadrant 4, which also have quantity decreases. The final panel shows that mergers in quadrant 1 are typically associated with increases in distribution for merging parties, while those in quadrants 3 and 4 are associated with decreases.<sup>19</sup>

### III.E. Robustness Checks

Our primary robustness check is to use the NielsenIQ Consumer Panel Dataset instead of the Retail Scanner Dataset. As discussed in Section II.A, by sampling

<sup>19</sup>Table C.3 in Appendix C.3 regresses quantity effects on price effects for both merging and non-merging firms as well as other effects and finds economically and statistically significant partial correlations in the expected directions for merging parties. For non-merging parties, patterns are typically statistically insignificant, and changes in distribution are the stronger predictor of quantity changes; however, quantity changes for non-merging parties are considerably smaller.

households, this dataset captures retailers that might be missing from the scanner dataset. Appendix A repeats most of the empirical analysis in the body of the paper using the panelist dataset. We find that point estimates for the average price effects are slightly higher in the panelist data than in the baseline data, with an estimate of 0.7 pp for aggregate price changes and 0.8 pp for merging party price changes. Dispersion is larger by about 2 pp for overall and non-merging party price changes and similar for merging parties.

We report a series of other robustness checks in Table C.1. Since the Retail Scanner Dataset likely has better coverage for food products, we check whether restricting to these mergers changes the main findings. We larger mean price effects (0.2–0.6 pp) and similar dispersion. To address concerns about UPC churn, we check whether aggregating products to the brand level materially affects results. We find a mean price change that is 1 pp lower for non-merging parties and standard deviations that are about 2–3 pp larger. We next consider a Bayesian shrinkage procedure; since the estimated standard deviations are typically much smaller than the dispersion in the estimates, this has limited quantitative effects on our estimates. Finally, dropping mergers with divestitures has limited effects as well. Appendix C.2 discusses these results in greater detail.

Turning to quantities, the estimates for the mean quantity effect in the aggregate or for non-merging parties are similar across the scanner and panelist datasets, and the standard deviation is about 4–5 pp larger (Table C.2). We estimate a longer upper tail for quantity effects for merging parties, which leads to a significantly larger standard deviation as well. It is worth noting, though, that a drawback of the panelist dataset is that since the data comes from a sample of households, total quantities are noisier than in the scanner dataset—especially for smaller markets, or markets with smaller merging parties. Other robustness checks for the quantity results are presented in Table C.3, and these checks are quantitatively very similar.

Overall, we find that over multiple datasets and a number of different decisions on which subsets of mergers to study and how to aggregate prices and effects, mean price changes are small for all parties, mean quantity changes are larger and negative for merging parties but small for non-merging parties, and there is substantial dispersion in these effects. The magnitudes are similar across specifications, as well.

#### IV. Connections to the Merger Guidelines

Both the 2010 and 2023 Guidelines discuss screens, based on market structure, where the agencies are likely to presume competitive harm from a merger. Section 5.3 of the 2010 Guidelines notes that 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).<sup>20</sup> 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 2010 Guidelines note that mergers in this area “raise significant competitive concerns and often warrant scrutiny.” Mergers outside this area are in the “green zone” are “unlikely to have adverse competitive effects.” The 2023 Guidelines note a presumption of illegality when DHHI exceeds 100 and either HHI exceeds 1,800 or merging parties’ market share exceeds 30%.

The theoretical basis of these 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”), and a change in HHI is often correlated with a change in the share of surplus accrued to producers (Spiegel, 2021). No such relationship exists for levels of HHI, but there may still 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. The emphasis on merging party share itself is, to our knowledge, rooted in historical precedent (e.g., *Philadelphia National Bank*) rather than standard economic theories.

We provide evidence of the real-world predictive power of these measures 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 the current enforcement landscape. Below, we will discuss enforcement as if agencies approved mergers. We do so for the sake of brevity. In

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<sup>20</sup>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>.

reality, agencies choose whether to challenge a merger or propose a remedy and do not formally approve them. This process creates selection bias: to observe a merger with large values of HHI and DHHI, 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”). Section V introduces a model that corrects for selection into approval, while this section reports the raw correlations.

#### IV.A. Price Changes and the Structural Presumptions

We begin our analysis at the merger level. Table 6 regresses price changes on HHI, DHHI, merging share, and a combination of these metrics. Columns (1) and (7) show that a 100-point increase in the average DHHI of a merger is associated with a 0.2 pp increase in price changes for merging parties and 0.3 pp for non-merging parties. Merging parties’ changes show a significant but small negative correlation with HHI as well. We interpret this as reflective of selection into proposal or approval: we may only observe mergers in high HHI markets if the agencies believe, or parties can argue, that they will not lead to large price increases. Columns (2) and (8) show that these correlations come from mergers with especially large HHI or DHHI. Columns (3) and (9) report that price effects in the red zone are larger on average than those in the green zone, although primarily for non-merging parties. Columns (4) and (10) show that price effects are significantly larger when merging share exceeds 30%. Columns (5) and (11) document noisy relationships with the 2023 presumptions. To unpack this, Columns (6) and (12) regress on a combination of HHI, DHHI, and merging share, which determine these presumptions. We find a large role for merging share, although this correlation seems to be concentrated in mergers with low DHHI; again, we speculate that this may be a indicative of selection.<sup>21</sup>

A second observation from Table 6 is that these structural presumptions have limited predictive power by themselves:  $R^2$  is low for all specifications. Figure C.6 in Appendix C.4 plots conditional distributions of price changes by bins of market

<sup>21</sup>In Section V.C we find that controlling for selection into approval increases the correlation between DHHI and price changes, and makes the correlation between HHI and price changes statistically insignificant.

	Merging						Non-Merging					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
HHI (0–1)	-8.95 (4.22)						-2.50 (3.52)					
DHHI (0–1)	18.82 (10.75)						29.68 (9.47)					
HHI ∈ [1500, 2500]		-1.50 (1.99)						-1.39 (1.46)				
HHI > 2500		-3.65 (1.87)						-2.45 (1.35)				
DHHI ∈ [100, 200]		-0.46 (2.11)						-0.65 (1.20)				
DHHI > 200		1.66 (1.14)						2.29 (1.09)				
Yellow			-0.38 (2.05)						-0.02 (1.18)			
Red			0.56 (1.01)						1.85 (1.14)			
Merg Share ∈ [0.10, 0.30]				1.54 (1.46)						1.23 (0.98)		
Merg Share > 0.30				2.74 (1.43)		6.61 (2.45)				2.56 (0.94)		4.90 (1.18)
2023 Presumptions					0.19 (1.16)						1.10 (0.91)	
HHI > 1800						-1.66 (1.60)						-2.90 (1.12)
DHHI > 100						-0.38 (5.20)						-3.76 (1.83)
HHI > 1800 × DHHI > 100						0.75 (5.50)						5.41 (2.35)
Merg Share > 0.30 × DHHI > 100						-5.60 (3.02)						-4.32 (2.00)
Constant	2.52 (1.35)	2.53 (1.56)	0.07 (0.80)	-1.01 (1.00)	0.07 (0.80)	0.96 (1.22)	0.77 (1.10)	2.05 (1.24)	0.18 (0.51)	-0.50 (0.54)	0.18 (0.51)	2.20 (0.92)
Observations	129	129	129	129	129	129	129	129	129	129	129	129
R <sup>2</sup>	0.028	0.033	0.001	0.022	0.000	0.037	0.041	0.047	0.020	0.040	0.010	0.088

Table 6: Regression of price changes on measures of market structure. We measure HHI and DHHI as the average across all DMAs, weighted by DMA sales. Each observation is a merger. Robust standard errors are in parentheses.

structure and complements this finding: within each bin of HHI, DHHI, merging share, and 2010 Guidelines region, we still observe substantial heterogeneity in price changes. This raises two questions. First, would more complex interactions of easily observable metrics improve predictive power? Here, our answer is negative: we have found that interactions of HHI, DHHI, merging share, share of private label brands, and market size—up to third order—never yield adjusted  $R^2$  above 10%.

Second, can these metrics be used to predict features of the price distribution? Figure 7 investigates the sign of the price increase instead of the magnitude: we ask whether there are levels of DHHI or merging share beyond which one can say with some confidence that the merger will likely lead to higher prices. We plot the

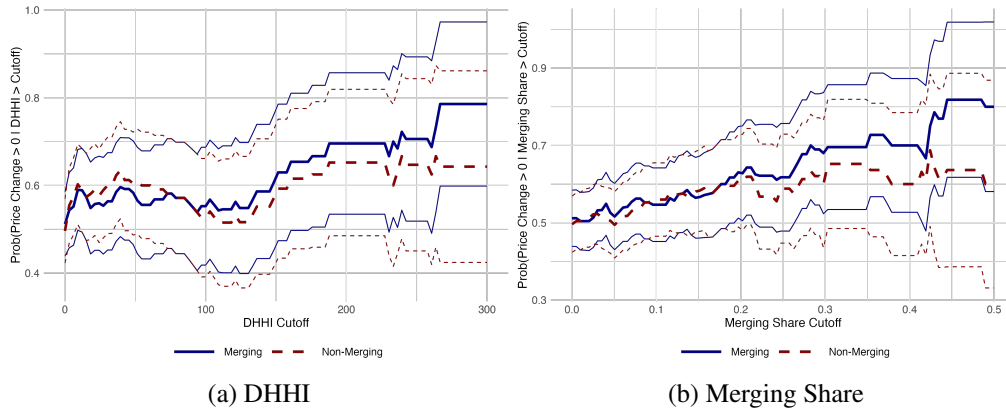


Figure 7: Share of mergers with positive price changes, conditional on measures of market structure exceeding a cutoff. The bold lines indicate means and the thinner lines outline the 90% confidence interval using Wilson standard errors.

probability of a positive price change (for either merging or non-merging parties) conditional on the merger having a DHHI or merging share above a cutoff, against the cutoff. These figures typically indicate that the probability of a positive merging party price change increases with both DHHI and merging share, although the relationship for non-merging parties is somewhat flatter. The confidence intervals are large, but we can say with 90% confidence that mergers with a DHHI above approximately 175 or a merging share above approximately 20% are more likely than not to lead to a price increase for merging parties. Additionally, the probability that a merger raises prices, for both merging and non-merging parties, increases substantially as DHHI goes from 100 to 200. The relationship between the probability of price increases and merging shares seems smoother.<sup>22</sup> Overall, this analysis suggests that extreme values of the screens can be useful in predicting price increases, although it still highlights considerable variation in outcomes for more moderate levels.

On net, we find a significant correlation between the structural presumptions and price changes as well as some additional predictive power for price increases. However, we view our results as also highlighting the importance of the agencies'

<sup>22</sup>Appendix A.3 redoes this analysis using estimates from the panelist data. Almost all results are quantitatively similar, although we find that the probability of a merging party price levels off at approximately a DHHI of 50 or a merging share of 0.2.

investigations to obtain accurate predictions of a merger’s effects. We quantify the importance of this information using data on enforcement decisions in Section V.

#### 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. Further, 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, or ones with high merging share.

We estimate price changes at the DMA-merger level as

$$\log y_{idt} - \widehat{\log y_{idt}} = \sum_{\tilde{d}} \beta_{1d} \mathbb{1}[\text{Merging Party}]_i \mathbb{1}[\tilde{d} = d] + \sum_{\tilde{d}} \beta_{2d} \mathbb{1}[\text{Non-Merging Party}]_i \mathbb{1}[\tilde{d} = d] + \epsilon_{idt}. \quad (4)$$

We then regress the transformed coefficients ( $100 \cdot (\exp(\hat{\beta}_{1d}) - 1)$ ) on merger fixed effects and measures of market structure. Figure 8 reports estimates from this analysis. In Panels (a) and (b), the top right bin represents the red region from the 2010 Guidelines, the three bins around it together form the yellow region, and all others represent the green region. In Panels (c) and (d), the top bins in the middle and right columns correspond to the 2023 presumptions. The number and color in each bin indicate the additional price changes relative to the baseline bin.

Panel (a) shows results for merging party prices, and Table C.5 provides standard errors on all pairwise differences in Figure 8. First, price changes are positively

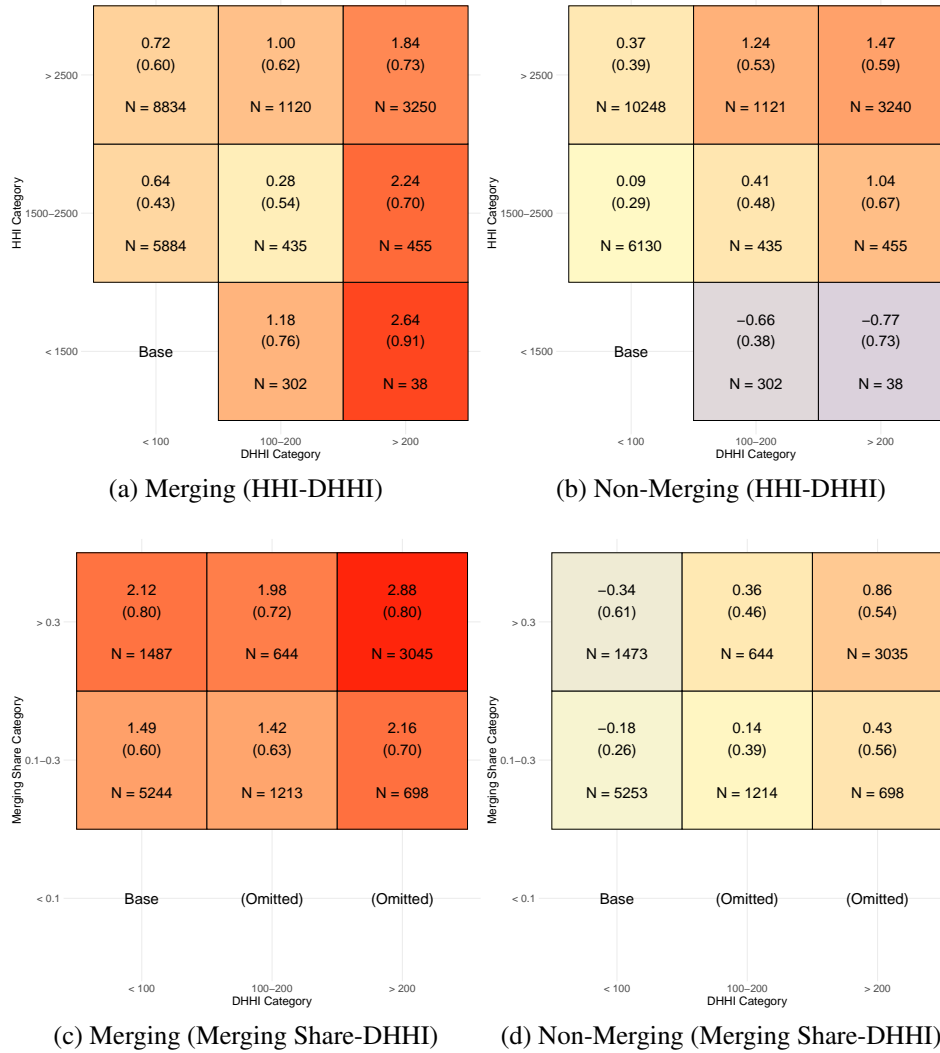


Figure 8: Within-merger price changes for bins of DMA-level HHI, DHHI, and merging party share. Each bin shows the coefficient of a regression of DMA-level price changes on bin dummies and merger fixed effects. In the top row, the base bin corresponds to low HHI and low DHHI. In the bottom, to low DHHI and low merging share, and low merging share necessarily implies that DHHI must be less than 100. Standard errors, clustered at the merger level, are in parentheses.  $N$  indicates the number of DMA-mergers in each bin.

correlated with DHHI. For each bin of HHI, we reject the null hypothesis that markets with DHHI above 200 exhibit the same price effect as those with lower DHHI with at least 90% confidence. Panel (c) illustrates this same effect when

binning by merging share and DHHI: conditioning on the bin of merging share, the price effect for high DHHI is significantly larger than that for the corresponding medium DHHI. The point estimates are also larger than for the corresponding bin with low DHHI, although this difference is not statistically significant. Overall, this result is consistent with predictions from models of unilateral effects. Panels (b) and (d) illustrate price effects for non-merging parties. While the qualitative patterns are similar, not all the analogous differences are statistically significant.

Second, we do not find any consistent evidence that HHI is correlated with price changes. Conditional on the DHHI category, the differences between bins of different HHI have varying signs, consistent with Nocke and Whinston (2022)’s finding that compensating efficiencies are not a function of HHI.

Third, Panel (c) shows a significant correlation between merging party price changes and merging share. Conditioning on any bin of DHHI, the difference between price changes in DMA with high merging share is significantly larger than that with lower merging share. We do not, however, see a significant relationship between non-merging price changes and merging share in Panel (d).

In Appendix A.3, we repeat this within-merger analysis using the Consumer Panel and do not find significant correlations of price changes with geography-level measures of market structure (Figure A.9). While this provides an important caveat to our results, we should note two differences in the analyses. The Consumer Panel requires us to define markets at the Scantrack level, which are often larger than DMAs. Furthermore, it covers fewer UPCs than the Retail Scanner Dataset.

## **V. Antitrust Enforcement**

Carlton (2009) uses a simple model to show that small average price changes do not necessarily indicate strict antitrust enforcement. Suppose that agencies can perfectly predict the price effect of a merger and can unilaterally decide whether to block mergers. Then the largest observed price effect, not the average, would indicate the maximum price increase the agencies are willing to tolerate and thus their stringency. In this section, we exposit this model formally and extend it to incorporate the fact that price forecasts are imperfect. Thus, the largest observed price change could be

due to an imprecise forecast rather than lax standards, and estimating the uncertainty in the forecast is both necessary for understanding stringency and for understanding the prevalence of the types of errors the agencies make. We then simulate outcomes under alternate stringencies, which change both the set of mergers selected into “approval” and the types of mistakes made by the agencies.

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

#### **V.A. A Model of Antitrust Enforcement**

A merger  $i$  is categorized by observable characteristics  $(X_i, Z_i)$  and a true price impact  $p_i^*$ . If agencies could observe the price change  $p_i^*$  prior to the merger taking place, they would challenge any merger where  $p_i^* \geq \bar{p}(X_i, Z_i)$ : this is the model in Section III of Carlton (2009). However, agencies do not observe  $p_i^*$  and instead learn about it through two sources. First, they have a prior  $F_{p^*}(\cdot; X_i)$  on  $p_i^*$ . Second, they obtain a signal  $p_i \sim F_p(\cdot; p_i^*)$  of  $p_i^*$  from a distribution that depends on the true price change. From the prior and the signal, the agency develops a posterior distribution on  $p_i^*$  and challenges a merger if the expectation of this posterior exceeds  $\bar{p}(X_i, Z_i)$ .

This model captures relevant features of the enforcement process. Characteristics such as those in the structural presumptions can enter the prior. The signal  $p_i$  captures what can be learned through due diligence: for example, the agency may learn that a merger is likely to lead to synergies and thus believe that prices would increase by less than would be expected given  $X_i$ . Moreover, there are observable factors  $(Z_i)$  that affect the threshold but might not affect the prior: given limited resources, agencies may choose to devote them to challenging larger mergers that may have a larger consumer surplus effect even for the same price effect.

This model can be viewed as a reduced form of the decision problem faced by the agency. Suppose there is a social cost  $S(p^*; X, Z)$  of allowing a merger that will increase prices by  $p^*$ , which is increasing in  $p^*$ . To the extent that price changes

and market size together can be used to approximate consumer surplus changes, this nests a consumer welfare standard. Assume that agencies pay a cost  $K_I$  of challenging a pro-competitive merger (a “type I error” (Kwoka, 2016)), a cost  $K_{II}$  of allowing an anti-competitive one to go through (a “type II error”), and a cost  $K_C$  of challenging at all. Then, an agency would challenge if

$$\begin{aligned} \int (S(p^*; X, Z) + K_{II} \cdot \mathbb{1}[p^* > 0]) dF_{p^*|p}(p^*|p, X) \\ \geq K_C + \int K_I \cdot \mathbb{1}[p^* \leq 0] dF_{p^*|p}(p^*|p, X), \quad (5) \end{aligned}$$

where  $F_{p^*|p}(\cdot|p, X)$  is the posterior over  $p^*$  given  $X$  and the signal  $p$ . Assuming that the family  $F_p(\cdot; p^*)$  has the monotone likelihood ratio property,  $F_{p^*|p}$  is stochastically increasing in  $p$  (Milgrom, 1981). Thus, the left-hand side is increasing in  $p$ , the right-hand side is decreasing in  $p$ , and there is a threshold  $\tilde{p}(X, Z)$ , which implicitly depends on the distributions and costs, such that the agency would challenge when  $p$  exceeds it. This threshold maps into a threshold  $\bar{p}(X, Z)$  in the posterior mean.

This microfoundation highlights potential extensions and limitations of the model. The costs  $K_\times$  could also be functions of merger characteristics  $(X, Z)$ , and the model still exhibits a threshold structure. Agencies may also take into account the probability that they win the case. In Appendix B.2 we verify that we retain a threshold structure if the probability of winning is an increasing function of the signal  $p$ . Of course, agencies may also take other factors into account when deciding to challenge a merger, such as effects on innovation or the ability of the challenge to affect case law. To the extent that these considerations are captured by observables  $(X, Z)$  in the social cost function, the reduced form of the model remains unchanged. However, an important limitation of this approach is that unobserved considerations in the agencies’ decision process would not lead to a single threshold as a function of  $(X, Z)$ .<sup>23</sup> While we anticipate that concerns like precedent-setting are limited in our setting, we cannot rule out alternative concerns that influence agency decisions. Moreover, this is an important caveat to the external validity of our estimates.

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<sup>23</sup>In Appendix B.2 we note that under some situations we can interpret the estimated signal distribution as capturing both the uncertainty in the price estimate and these alternative considerations.

The microfoundation also provides some context about the interpretation of our model. We will take the reduced-form model to the data and estimate a price threshold. However, one should remember that one need not take this model literally. We do not believe that the agencies have an explicit goal of blocking mergers above a particular price threshold. Rather, they trade off costs of challenges with costs from allowing mergers through, and they thus act as if they have a price threshold.

## V.B. Identification and Estimation

In the remainder of the section, we focus on the reduced form of the model and consider estimation of the prior  $F_{p^*}(\cdot; X)$ , the signal distribution  $F_p(\cdot; p^*)$ , and the threshold  $\bar{p}(\cdot)$ . These objects help us interpret features of the current antitrust regime even without identifying the underlying surplus function and costs. The threshold directly quantifies the effective stringency of antitrust enforcement. The signal distribution together with the prior quantify the posterior uncertainty the agency faces about the price change of any particular merger. The model also allows us to quantify the extent to which the agency commits type I and type II errors, even if we do not identify their internal costs of doing so. Moreover, we can do so even under counterfactual changes to stringency, which we can model as changes to  $\bar{p}(X, Z)$ , as long as these changes to stringency do not change the precision of the agencies' signals. For example, this rules out agencies changing how they conduct due diligence. Overall, we believe that this simple model provides us with insights regarding the stringency of antitrust enforcement.

For each merger  $i$ , our data consists of properties of the merger: notably, market structure (such as HHI and DHHI) and market size. We also observe an estimate  $\hat{p}_i$  of the true price change. Finally, we observe whether the agencies challenged a merger. Generally, a challenge could be one of many actions, such as a motion to block or a proposal for a remedy. Challenges are indeed at the merger level: agencies often propose divestitures in individual product markets without blocking the entire deal. None of the mergers in our dataset are taken to court, but we identify six mergers, from four separate deals, in which an agency proposed a remedy for a horizontal market power concern. Additionally, we find two deals, corresponding to four mergers, that were proposed and later withdrawn due to antitrust concerns

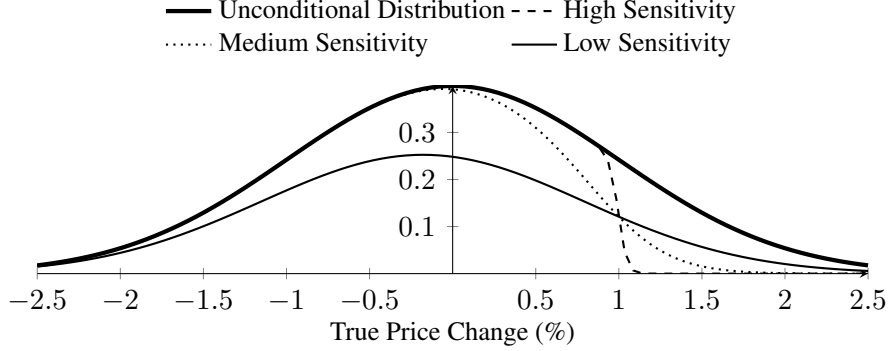


Figure 9: Illustration of the identification of model parameters. We illustrate densities of price changes of approved mergers, normalizing them to integrate to the probability of approval, for three potential sets of model parameters.

raised by the DOJ or FTC. We codify these four blocked mergers and the six mergers with remedies as being challenged.

To gain intuition for identification, suppose we observe the true price changes for consummated mergers and a merger-specific  $Z_i$  that 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 the prior  $F_{p^*}$ .

To identify the signal distribution and the threshold  $\bar{p}(\cdot)$ , consider increasing stringency by manipulating  $Z_i$ . Figure 9 plots in bold the unconditional distribution of price changes  $F_{p^*}$ , which we have already identified, and illustrates three possibilities for the distribution of price changes for approved mergers (which would be observed in the data); for illustration, we 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 acting as if they are 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,  $\sigma_\epsilon$  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  $\sigma_\epsilon$ ). 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 make parametric assumptions for estimation. In particular, we assume that the prior is normal with mean  $X_i'\beta$  and standard deviation  $\sigma_{p^*}$ . We let measures of market structure (HHI and DHHI) enter  $X_i$ , consistent with the agencies' use of structural presumptions. We assume that the signal  $p_i$  is drawn from a normal centered at  $p_i^*$  and a standard deviation  $\sigma_\epsilon$ . We parameterize the threshold as  $Z_i'\alpha$ . In our main specification, we let  $Z_i$  include log of the annual sales of merging parties. The rationale is that agencies are more likely to scrutinize markets where the merging parties are larger, as consumer surplus effects are likely to be largest. However, we do not let  $Z_i$  enter the prior directly: scaling up a market impacts the agencies' decision, but it does not inherently change its price impact conditional on market structure.<sup>24</sup> Finally, we do not include a measure of market structure in the threshold itself: agencies could be more likely to challenge a market with high DHHI, but because they have a prior that such markets have high price changes, not because they are inherently stricter on such mergers.<sup>25</sup>

We estimate the model by maximum likelihood. For unchallenged mergers, we observe a noisy measure of the true price change: we construct the likelihood by noting that (i)  $p_i^* \sim N(\hat{p}_i, \sigma_i^2)$ , where  $\hat{p}_i$  is our estimate of the price change from the descriptive analysis in Section III.B and  $\sigma_i$  is the associated standard error, (ii)  $p_i^*$  is drawn from the prior  $N(X_i'\beta, \sigma_{p^*}^2)$ , and (iii) the associated posterior mean must be lower than  $Z_i'\alpha$ . For mergers that were withdrawn, we do not observe a direct measure of  $p_i^*$ , but know that it was drawn from the prior and that the associated posterior mean must have been higher than  $Z_i'\alpha$ . For mergers that went through with a divestiture, we assume that the measured merger effect is a lower bound for the

<sup>24</sup>We are assuming that merger proposal is not affected by size. This may be implausible for especially large mergers, but since the largest mergers scrutinized by the agencies during our time period are much larger than those in our sample, we do not view this as a restrictive assumption.

<sup>25</sup>The microfoundation in Section V.A suggests that  $\bar{p}(\cdot)$  is a function of both  $X_i$  and  $Z_i$ . In Appendix B.1, we show that with one more assumption that corresponds to the agency not inherently caring about market structure, the threshold in the posterior mean does not depend on  $X_i$ .

true merger effect and that the signal would have been high enough to warrant a challenge. We present the equations behind the likelihood in Appendix B.1.

### **V.C. How Stringent is US Antitrust Enforcement?**

We estimate the model using both aggregate and merging-party price changes. The former is motivated by the fact that agencies also take into account how non-merging firms will respond to the merger.<sup>26</sup> The latter is motivated by publicly-available filings and reports, which sometimes focus solely on merging parties.

Panel A of Table 7 shows estimates of the mean of the prior under various parameterizations. Column (1) shows that price changes increase with DHHI: a 100-point increase in DHHI correlates with a 0.39 pp larger expected increase in average price. The relation between HHI and price changes is statistically insignificant. Columns (2) and (3) show that the correlation between DHHI and the prior mean comes primarily from mergers with large DHHI (above 200) or in the red zone. Column (4) shows that the prior is 3.6 pp larger for mergers with merging share above the 30% threshold noted in the 2023 Guidelines, and Column (5) shows a significant correlation with the union of the 2023 Presumptions. Results for merging parties (Columns (6)–(10)) are qualitatively similar and quantitatively larger.

Comparing the results in Panel A with those in Table 6, we estimate that DHHI and merging share correlate more strongly with the prior than with realized price changes. For instance, the coefficient on DHHI in Column (6) of Table 7 is almost three times as large as the one in Column (1) of Table 6. Similar patterns hold for our estimates of merging share and the presumptions. These results are consistent with the model controlling for selection into approval: for instance, 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. Consistent with this argument, Table C.8 indicates that enforcement is strongly correlated with DHHI, the red zone, merging share, and the 2023 presumptions.

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<sup>26</sup>The 2010 Horizontal Merger Guidelines state “Where sufficient data are available, the Agencies may construct economic models designed to quantify the unilateral price effects resulting from the merger. These models often include independent price responses by non-merging firms” (p. 21).

	Aggregate Price Changes					Merging Price Changes				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<b>A. Prior</b>										
Avg HHI (0–1)	-0.76 (3.21)					-3.80 (4.79)				
Avg DHHI (0–1)	39.27 (12.64)					51.78 (17.60)				
HHI $\in$ [1500, 2500]		-1.35 (1.48)						-1.55 (2.19)		
HHI > 2500		-1.39 (1.46)						-1.38 (2.13)		
DHHI $\in$ [100, 200]		-1.04 (1.28)						-1.45 (1.82)		
DHHI > 200		3.34 (1.14)						4.34 (1.58)		
Yellow			-0.58 (1.28)					-0.86 (1.84)		
Red			3.28 (1.13)					4.48 (1.53)		
Merg Share $\in$ [0.10, 0.30]				1.29 (0.94)					1.84 (1.36)	
Merg Share > 0.30				3.63 (1.12)					4.80 (1.60)	
2023 Presumptions					2.05 (0.90)					2.70 (1.28)
Constant	0.33 (1.03)	1.43 (1.35)	0.17 (0.47)	-0.51 (0.65)	0.15 (0.50)	1.01 (1.53)	1.32 (2.00)	-0.04 (0.70)	-1.03 (0.96)	-0.07 (0.73)
<b>B. Errors and Uncertainty</b>										
$\sigma_{p^*}$	4.53 (0.36)	4.45 (0.35)	4.45 (0.35)	4.54 (0.37)	4.59 (0.38)	6.67 (0.51)	6.56 (0.51)	6.56 (0.51)	6.66 (0.52)	6.74 (0.53)
$\sigma_{\epsilon}$	2.94 (1.83)	3.98 (2.52)	4.67 (3.03)	2.70 (2.07)	3.34 (2.38)	5.35 (2.29)	6.43 (3.20)	6.91 (3.56)	5.11 (2.29)	5.57 (2.63)
Posterior Standard Deviation	2.46 (1.07)	2.96 (1.02)	3.22 (0.97)	2.32 (1.30)	2.70 (1.23)	4.17 (1.07)	4.59 (1.13)	4.76 (1.13)	4.05 (1.12)	4.29 (1.17)
<b>C. Threshold</b>										
Log(Annual Merging Sales)	-0.56 (0.41)	-0.55 (0.37)	-0.52 (0.36)	-0.62 (0.43)	-0.57 (0.38)	-0.94 (0.59)	-0.85 (0.55)	-0.80 (0.54)	-1.01 (0.62)	-0.91 (0.57)
Constant	7.03 (1.47)	6.31 (1.80)	5.92 (2.00)	7.30 (1.63)	6.68 (1.87)	9.26 (2.04)	8.46 (2.31)	8.19 (2.43)	9.56 (2.14)	8.97 (2.25)
<b>D. Sales-Weighted Thresholds</b>										
Average	6.08 (1.53)	5.38 (1.67)	5.04 (1.79)	6.25 (1.70)	5.70 (1.84)	7.65 (1.81)	7.01 (1.99)	6.83 (2.08)	7.84 (1.81)	7.42 (1.98)
Q1	5.48 (1.58)	4.79 (1.60)	4.48 (1.65)	5.59 (1.75)	5.09 (1.79)	6.65 (1.81)	6.10 (1.87)	5.97 (1.93)	6.76 (1.78)	6.45 (1.90)
Q3	6.63 (1.43)	5.92 (1.66)	5.55 (1.81)	6.86 (1.58)	6.26 (1.77)	8.58 (1.89)	7.85 (2.11)	7.62 (2.20)	8.83 (1.96)	8.31 (2.08)

Table 7: Parameter estimates, using aggregate price changes in Columns (1)–(5) and merging party price changes in Columns (6)–(10). Standard errors are in parentheses. Log sales are demeaned.

Panel B reports the standard deviation of the prior ( $\sigma_{p^*}$ ) and the error in the agencies' assessment of the price change ( $\sigma_\epsilon$ ). These estimates together give us a sense of the value of the signal the agency collects: that  $\sigma_\epsilon$  is smaller than the prior standard deviation  $\sigma_{p^*}$  means that the agency places relatively more weight on the signal than on the prior. Together, we estimate a posterior standard deviation of 2.5–3.2 pp when using aggregate price changes and 4.1–4.8 pp when using merging party price changes. To get a sense of the additional information gained from the signal, suppose that the agency had to classify each merger as anti-competitive (positive price change) or pro-competitive (negative price change) purely based on the prior. Using the estimates in Column (1), we would predict that their accuracy would be 56% (s.e. 3 pp); using the signal in addition increases the accuracy to 83% (s.e. 9 pp).<sup>27</sup> Another way to interpret the signal is that the correlation between the prior mean and the true price change is 0.29 (s.e. 0.08); the correlation between the posterior mean and the true price change is 0.85 (s.e. 0.15). That is, the signal is much more predictive of the true price change than the flexible functions of observables discussed in Section IV.A.

Panel C reports estimates of the threshold function. A 10% increase in merging party sales leads to approximately a 0.06 pp decrease in the threshold when using aggregate price changes and 0.09 pp when using merging price changes. This is consistent with the intuition that agencies are stricter for larger mergers. The dependence of the threshold on sales typically has  $p$ -values around 0.15 when using aggregate price changes and 0.11 when using merging.

Panel D summarizes these estimates. We find a sales-weighted average threshold of between 5.0% and 6.3% 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 4.5% and 5.6%. The third quartile (i.e., for the smaller mergers in our dataset) amounts to between 5.6% and 6.9%. Using merging party price changes as the outcome, we find slightly higher thresholds, around 6.6–7.8%.

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<sup>27</sup>We could also conduct the exercise of categorizing every agency decision—based on comparing the posterior mean and the threshold, rather than on comparing the posterior mean to zero—into type I errors, type II errors, and accurate decisions. Here, the probability of an accurate decision is 0.45 (s.e. 0.04) without the signal and 0.59 (s.e. 0.09) with.

*Robustness and Discussion.* In Appendix A.4, we re-estimate this model using estimates from the panelist data. We find that average thresholds are in line with the ones we find using the scanner data: the threshold in aggregate price is 4.8–5.7%, while the estimated threshold for merging prices is about 1 pp larger than than estimated here. We find a stronger dependence of the threshold of sales, with associated  $p$ -values slightly smaller. One difference is that our estimate of the precision of the signal decreases: to put that into context, we find that the correlation between the prior mean and the true price change is 0.25 (s.e. 0.08) while that between the posterior mean and the true price change is 0.66 (s.e. 0.14)—the latter of which is lower than our baseline estimates. This difference has implications for the prevalence of type I and type II errors, which we discuss in Section V.D.

To our knowledge, this is the first direct estimate of this threshold, so benchmarking it is difficult. One possibility is to use published merger retrospectives: Kwoka (2014, p. 86) argues that one interpretation of the selection bias in published studies is that these studies are more likely to be of such marginal mergers, as these are the deals that garnered press attention partly because of agency scrutiny. It is thus noteworthy that he arrives at a quantitatively similar conclusion, with mean price changes of mergers around 7.2% (Table 7.2 in Kwoka (2014)), although the industries under consideration are quite different. Another option is to compare our estimates to publicly available predictions of price effects by the agencies and their experts. We find that thresholds given market sizes are typically lower than the predicted price effects presented by the agencies. This analysis comes with many caveats—industries are different and market sizes are often larger than those in the sample, agency predictions may not have taken into account synergies, and other factors may affect agency decisions—but it provides another benchmark for comparison. Appendix C.5 provides further details.

#### **V.D. Counterfactual Outcomes Under Alternate Stringencies**

Given these estimates, is antitrust scrutiny excessively lax? Tightening the threshold leads to more challenges, improving the outcomes of the mergers that go through while increasing the workload faced by the agencies. It also increases type I errors while reducing type II errors. Our model allows us to quantify these effects in both

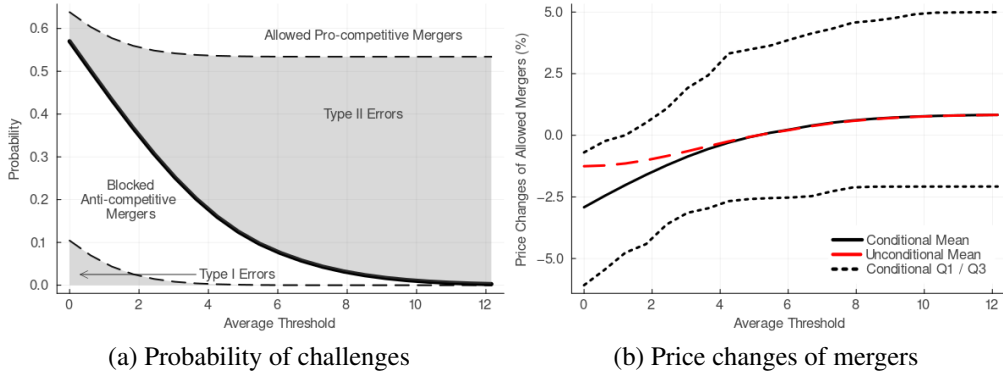


Figure 10: Outcomes of counterfactual thresholds. Panel (a) shows the probability of blocking a merger (solid black) along with probabilities of type I and type II errors. Panel (b) shows price changes conditional on consummation in solid black and the changes unconditional on consummation in a dashed red line. Figure C.8 shows confidence intervals.

current and counterfactual regimes. In this section, we consider scaling the thresholds by a factor, e.g., all thresholds become 10% smaller. For each counterfactual threshold, we compute the probability of challenging a merger in our sample as well as probabilities of making errors.<sup>28</sup> We also compute the distribution of price effects for allowed mergers.

Panel (a) of Figure 10 plots the probability of challenging a merger against counterfactual thresholds in solid black, using the estimates in Column (1) of Table 7. To begin quantifying outcomes, consider agency workload. Moving to a threshold of 2.5% from the current average of 6.1% (5.7% for the panelist data) would increase the probability of challenges to 0.30 in both specifications—approximately quadrupling it from the current probability of 0.075. Reducing it to 0% would lead the agencies to challenge 57% of mergers (66% using the panelist data). 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.

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

<sup>28</sup>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.

price changes of consummated mergers for different threshold levels. Tightening the threshold to 2.5% would reduce the aggregate price change for consummated mergers by about 1.4 pp, to -1.2% (a reduction of 1.1 pp, to -0.5%, in the panelist data). Moving to a 0% threshold would lead to 60–70% of consummated mergers causing price decreases. The cost of loosening the threshold is more limited: average price changes level off to about 1% 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. For context, Panel (b) also reports unconditional price changes in red, leveraging that price changes in unconsummated mergers are 0.

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 mergers from being proposed, our estimated increase in administrative burden is an upper bound.

Turning to errors, Panel (a) shows that type I errors are infrequent at the current threshold. Recall that agencies block pro-competitive mergers if their signal exceeds the threshold and that pro-competitive mergers have negative price effects. Therefore, with a threshold around 5%, 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 3%. At a threshold of 0%, 10% of blocked mergers are type I errors (13% using the panelist data). 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 50% of allowed mergers are due to type II errors. The ratio becomes 35% at a threshold of 2.5% and 16% at 0%. Given the relatively less precise signal estimated using the panelist data, these percentages are considerably larger in that specification: the current probability of a type II error given a challenge is 64%, and it reduces to 56% and 40% at threshold of 2.5% and 0%.

Our estimates indicate that small increases in antitrust stringency would reduce price effects of the average consummated merger modestly and would reduce the prevalence of type II errors. The main cost is not an increase in type I errors but

rather a significant increase in burden on the agencies—unless increased stringency leads to fewer mergers being proposed. How an agency balances these trade-offs is a function of the weights they place on errors and the cost of challenging mergers.

## 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 2.1%, and another 25% have raised them by more than 3.7%. Second, through a model of agency decisions, we investigate the stringency of antitrust enforcement. We find that current levels of antitrust enforcement are such that the probability of blocking a pro-competitive merger is very low, while the probability of allowing anti-competitive mergers is substantial. Tightening standards would lead to fewer type II errors without a corresponding increase in the prevalence of type I errors. However, it would result in a significantly higher burden on the agencies.

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## ONLINE APPENDICES

### A. Results using Consumer Panel Dataset

The baseline results in the main text rely on the NielsenIQ Scanner Dataset. We prefer this dataset over the NielsenIQ Consumer Panel for the two main reasons discussed in the text. First, it contains all UPCs sold in a sample of stores and includes UPCs with small shares. For our analysis of product assortment, this is critical. Moreover, in small markets, larger coverage is important to estimate stable price and quantity effects. Second, the scanner dataset specifies the DMA in which each store is located. By contrast, the sampling procedure in the Consumer Panel is representative of the “Scantrack,” which is often larger than the DMA.

However, it is important to reiterate that there is a drawback to using the scanner data: it does not cover the universe of retailers. For this reason, we replicate our analysis using the NielsenIQ Consumer Panel dataset, which contains a sample of households—rather than stores—and therefore covers the subset of retailers that the scanner dataset does not.

#### A.1. Price Effects

Table A.1 displays the distribution of the estimated price effects using the Consumer Panel dataset. The point estimates for the average price effects are slightly higher in the panelist data than in the baseline data—by about 0.3–0.4 pp for overall and non-merging party price changes, and 0.7 pp for merging party price changes. We see changes throughout the distribution of price effects, as almost all quantiles tend to be shifted up. We also estimate slightly larger standard deviations for the overall effects and non-merging party effects, but similar interquantile ranges. We show later that the larger change in standard deviations is related to a handful of datapoints with a significantly different value in the panelist data. Overall, we do not believe these differences change the economic interpretation of the baseline results. In all cases, average price effects are small, and there is significant dispersion.

We also analyze robustness of the point estimates for each merger. Figure A.1 plots the estimated price effects in the baseline specification against the estimated

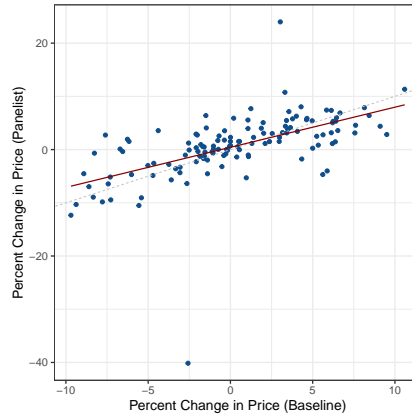
	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	129	0.31 (0.40)	4.59	-2.09 (0.40)	0.00 (0.48)	3.65 (0.54)
Merging Parties	129	0.12 (0.62)	7.07	-3.22 (0.62)	0.38 (0.58)	3.74 (0.65)
Non-Merging Parties	129	0.48 (0.42)	4.81	-2.24 (0.38)	-0.13 (0.46)	4.24 (0.65)
B. Panelist Data						
Overall	129	0.67 (0.54)	6.10	-1.32 (0.50)	1.15 (0.42)	3.81 (0.46)
Merging Parties	129	0.84 (0.57)	6.47	-2.40 (0.95)	1.44 (0.48)	4.61 (0.58)
Non-Merging Parties	129	0.74 (0.58)	6.64	-1.85 (0.84)	0.90 (0.53)	3.53 (0.46)

Table A.1: Price Effects using Consumer Panel Data. 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 for comparison purposes, and Panel B displays results using the NielsenIQ Consumer Panel Data.

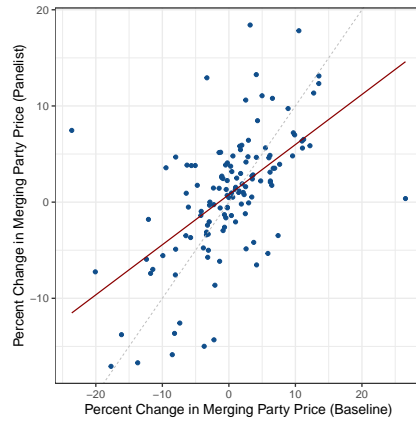
effects using the Consumer Panel dataset. Panel (a) displays results for aggregate price changes, Panel (b) displays results for merging parties, and Panel (c) displays results for non-merging parties. The price effects are positively correlated and, with the exception of a handful of datapoints, generally close to the 45° line.

Figure A.2 illustrates the distribution of price changes for merging and non-merging parties in Panel (a) and scatters the changes for merging and non-merging parties against each other in Panel (b). Consistent with the results in the main text, there is a positive correlation between merging and non-merging price changes (correlation = 0.56, s.e. = 0.07), indicating strategic complementarity.

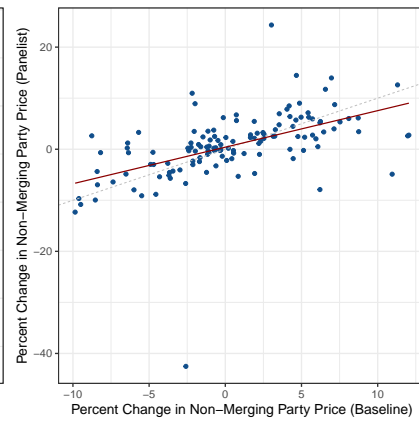
The qualitative takeaways of the analysis of the timing of price changes are also robust to the use of the panelist dataset. Figure A.3 replicates Figure 3 from the main text. We find similar patterns in timing. The main difference is that the average price decrease of non-merging firms is lower than for merging firms when the aggregate price change is in the bottom quartile. Note that the average for non-merging firms is estimated noisily, and it is influenced by the one outlier estimate shown in the



(a) Aggregate



(b) Merging Parties



(c) Non-Merging Parties

Figure A.1: Comparison of baseline price effects and price effects using the NielsenIQ Consumer Panel Data. The dashed line is the 45-degree line, and the red line is the estimated best fit, assuming equal weights across mergers.

bottom left of Figure A.2.

## A.2. Quantity Effects

Table A.2 displays the distribution of estimated quantity effects using the Consumer Panel data. A number of the qualitative takeaways from the main text are robust to the use of the Consumer Panel dataset. Comparing Panels A and B, the distributions of overall quantity changes and those for non-merging parties are generally similar. Using the panelist data, we generally similar means, standard deviations that are

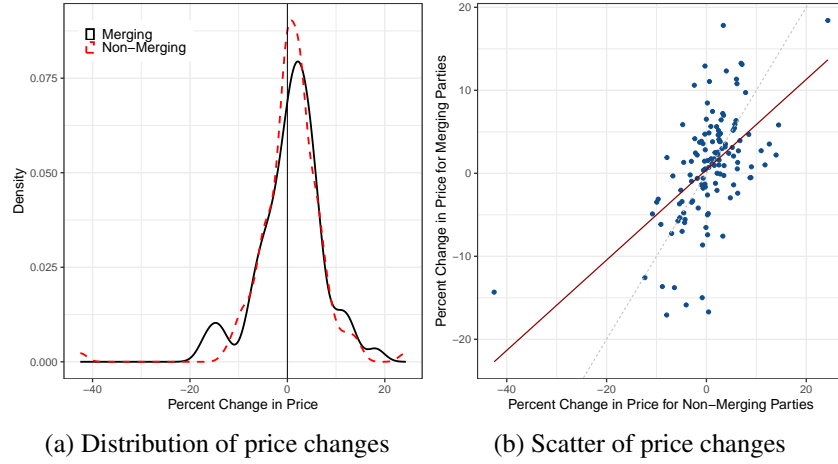


Figure A.2: Price changes for merging and non-merging parties, as estimated by (2) using the NielsenIQ Consumer Panel Data. These plots display transformed coefficient estimates (e.g.,  $100 \cdot (\exp(\hat{\beta}_1) - 1)$ ) for the price change of the merging and non-merging parties.

about 25% larger, and slightly more extreme first and third quartiles for non-merging parties. The distribution for merging parties exhibits considerably more dispersion when using the panelist data and a larger mean (although the mean is measured noisily). This difference can be traced back to a longer upper tail of quantity changes. We hesitate to draw strong conclusions about these differences: they could be driven by incomplete coverage of UPCs or by quantity increases in the set of stores covered by the panelist dataset but not the scanner dataset.

Figure A.4 scatters the estimated quantity effects in the baseline against the estimated effects using the Consumer Panel dataset. Panel (a) displays results for aggregate quantity changes, Panel (b) displays the merging-party effects, and Panel (c) displays the non-merging party effects. The quantity changes are positively correlated. However, there are some cases in which the estimated effect using the panelist data differs substantially from the corresponding effect in the baseline. As discussed above, because the Consumer Panel dataset has incomplete coverage of UPCs, we hesitate to draw strong conclusions about the source of these differences.

Figure A.5 replicates Figure 4 from the main text, displaying the distribution of quantity changes (Panel (a)) and a scatter of merging versus non-merging quantity changes (Panel (b)). The takeaways from Panel (a) are identical to those discussed

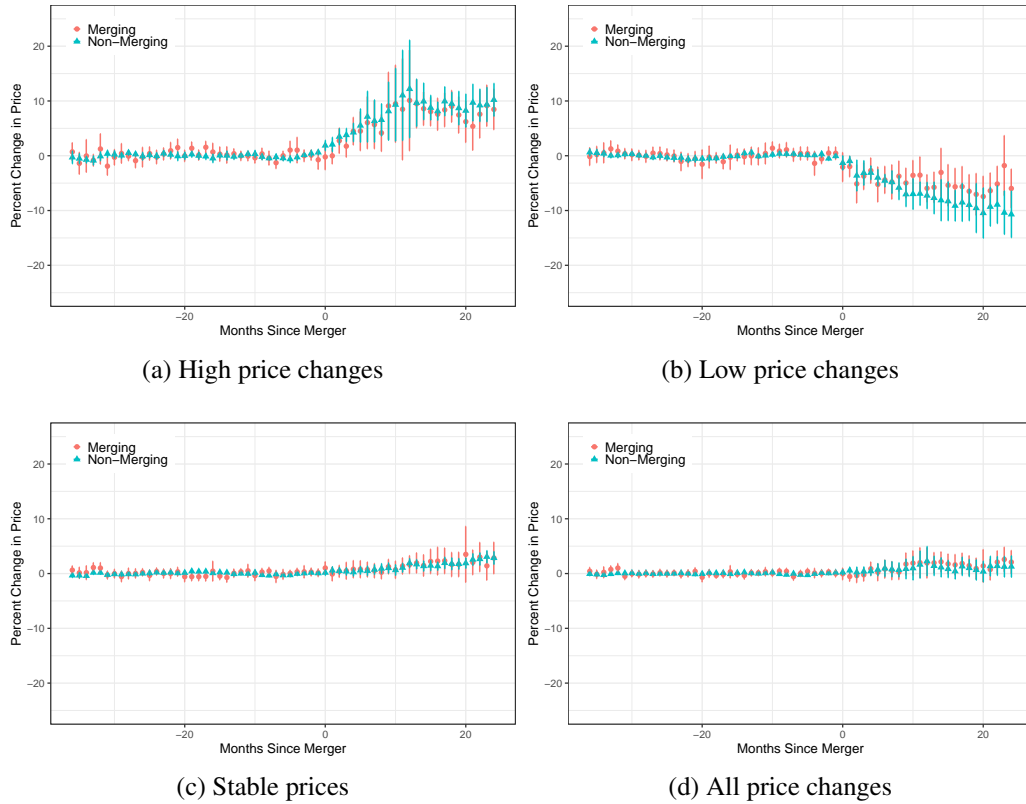


Figure A.3: Timing of price changes, for merging parties (red circle) and non-merging parties (blue triangle) using the NielsenIQ Consumer Panel Data. The marker indicates the mean price change a given number of months after the merger becomes effective, and the thick line is the 95% confidence interval of that mean. Panels (a)–(c) show subsamples: Panel (a) restricts to mergers with merging-party 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.

above: merging party quantity changes exhibit more dispersion, driven by larger effects in the right tail of the distribution. In Panel (b), we find a statistically significant positive correlation between merging and non-merging quantity changes.

We next analyze whether the quantity changes are explained by movement in prices. Figure A.6 scatters the estimated quantity changes against the corresponding price changes for merging (Panel (a)) and non-merging parties (Panel (b)). The correlations between price and quantity effects are not statistically significant at conventional levels. The correlation for merging parties is  $-0.14$  (s.e.  $0.09$ ) and for non-merging parties is  $-0.01$  (s.e.  $0.09$ ). As in the main text, it is clear that prices

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	129	-0.54 (1.37)	15.55	-9.68 (1.22)	-2.34 (0.96)	5.02 (1.92)
Merging Parties	129	-6.41 (2.86)	32.53	-20.74 (2.74)	-9.17 (2.22)	5.74 (2.67)
Non-Merging Parties	129	0.66 (1.47)	16.75	-7.98 (1.64)	-1.58 (0.73)	5.49 (1.74)
B. Panelist Data						
Overall	129	-0.97 (1.77)	20.07	-12.42 (1.92)	-2.58 (1.38)	8.60 (1.77)
Merging Parties	129	-0.73 (4.08)	46.30	-22.03 (4.82)	-5.95 (2.58)	13.28 (5.53)
Non-Merging Parties	129	-0.07 (1.83)	20.76	-13.09 (1.44)	-2.63 (2.06)	9.74 (1.68)

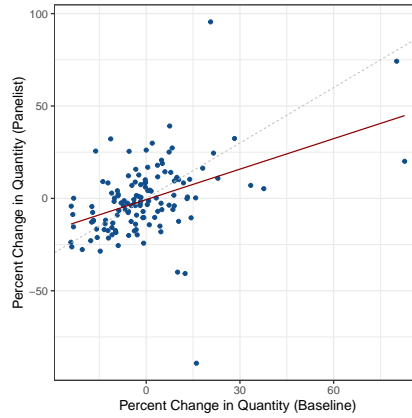
Table A.2: Quantity Effects using Consumer Panel Data. This table displays the distribution of transformed coefficient estimates of (2) (e.g.,  $100 \cdot (\exp(\hat{\beta}_1) - 1)$ ) for overall, merging-party, and non-merging-party quantity changes. Standard errors are in parentheses. Panel A displays the baseline results from the main text for comparison purposes, and Panel B displays results using the NielsenIQ Consumer Panel Data.

do not tell the whole story. However, given the incomplete coverage of UPCs, we do not believe the product availability results are credible when using the panelist data. We therefore do not replicate Table 4 and Figure 6 from the main text in this appendix.

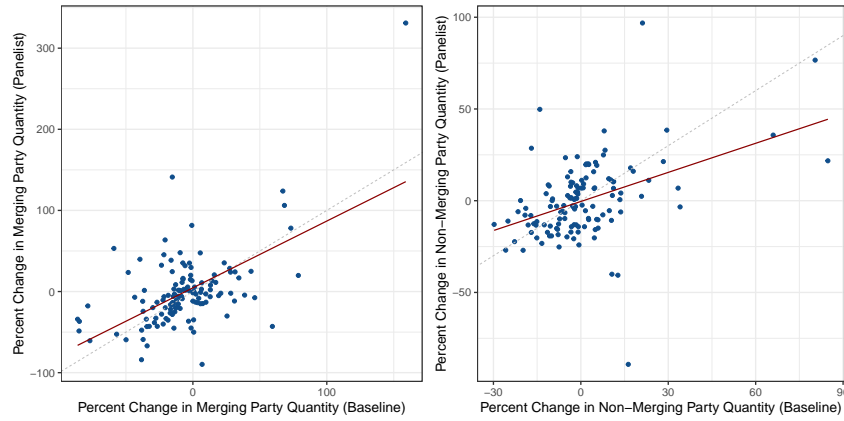
### A.3. Connection to the Merger Guidelines

Figure A.7 shows the equivalent of Figure C.6, using results from the panelist data. We again see a fair bit of dispersion even in these conditional distributions, and the relationship between means and bins of market structure are similar to the baseline estimates and somewhat stronger for the non-merging price effects. Figure A.8 shows the probability of a positive price change as a function of DHHI and merging share. As discussed in the body, the pattern with merging share is broadly similar. The main difference is that pattern with DHHI levels off at a much smaller value than when using the scanner dataset.

In Table A.3, we repeat the analysis of Table 6 in the main text. There are some



(a) Aggregate



(b) Merging Parties

(c) Non-Merging Parties

Figure A.4: Comparison of baseline quantity effects and quantity effects using the NielsenIQ Consumer Panel Data. The dashed line is the 45-degree line, and the red line is the estimated best fit.

differences: the Yellow region no longer exhibits a significant correlation with price changes, and the coefficient on the 2023 Presumptions decreases in magnitude. The negative correlation with HHI is weaker in Columns (2) and (7). Overall, however, we generally find the same patterns: price changes are correlated with DHHI and with merging share.

Figure A.9 repeats the within-merger analysis of Figure 8. Unlike in our baseline specification, we do not find any interpretable patterns in this analysis, and all results are statistically noisy. Note that given the sampling procedure of the panelist data, we have to define the geography as the Scantrack market: we replace DMAs in (4)

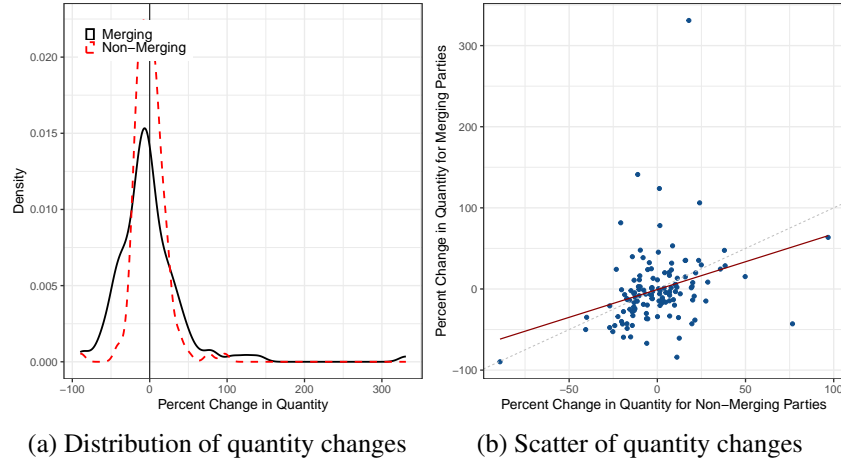


Figure A.5: Quantity changes for merging and non-merging parties, as estimated by (2), using the NielsenIQ Consumer Panel Data. This plot displays transformed coefficient estimates (e.g.,  $100 \cdot (\exp(\hat{\beta}_1) - 1)$ ) for the quantity change of the merging and non-merging parties.

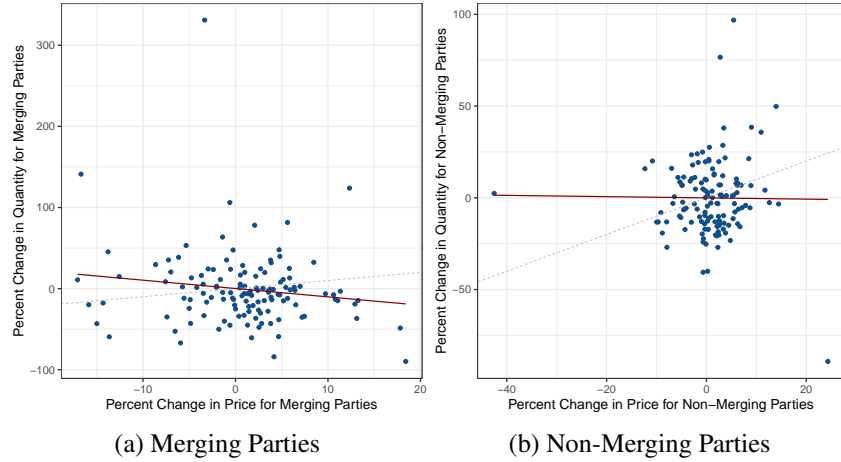
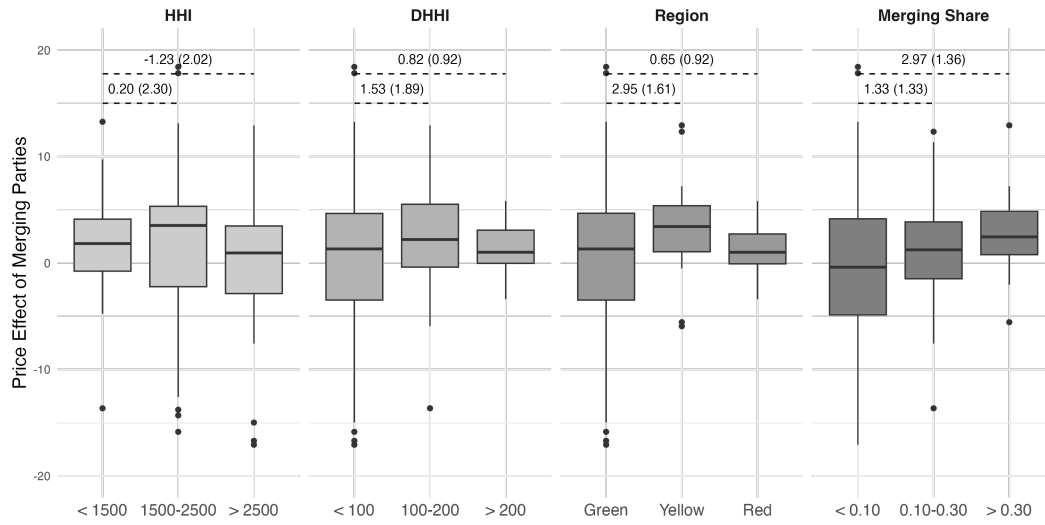
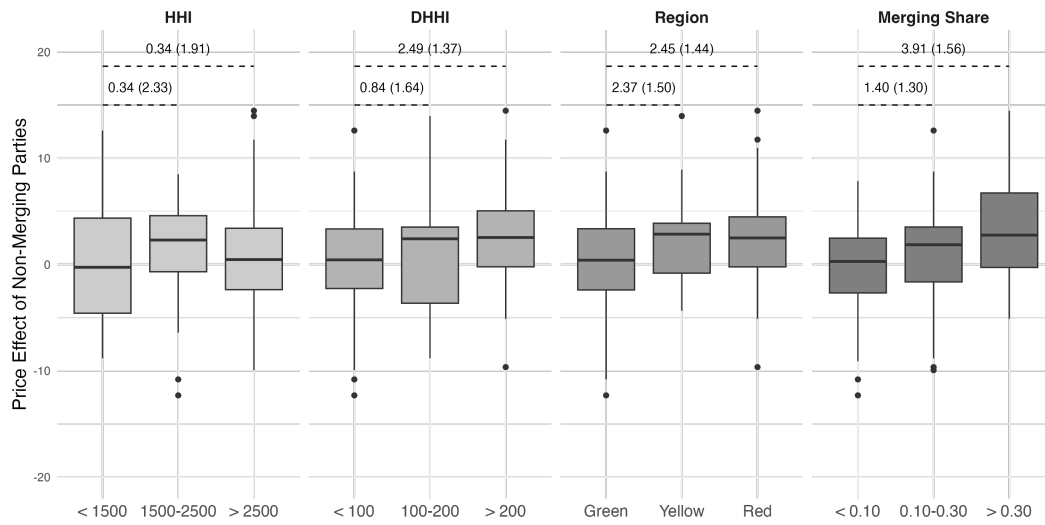


Figure A.6: Scatter of price versus quantity changes for merging and non-merging parties using the NielsenIQ Consumer Panel Data. Panel (a) displays a scatter plot of price changes versus quantity changes for merging parties. Each blue point represents a merger, the red line is the estimated best fit, assuming equal weights across mergers. Panel (b) presents the same scatter plot, but for non-merging parties.

with Scantracks and compute HHI and DHHI at the Scantrack level. NielsenIQ has fewer Scantracks than DMAs, and they are typically larger than DMAs.



(a) Merging Parties



(b) Non-Merging Parties

Figure A.7: Boxplots for price changes of (a) merging and (b) non-merging parties, by market characteristics, using estimates from the panelist dataset. The numbers over the dotted lines indicate the difference in means with the baseline group, with standard errors in parentheses. Some outliers are excluded due to truncation of the plots.

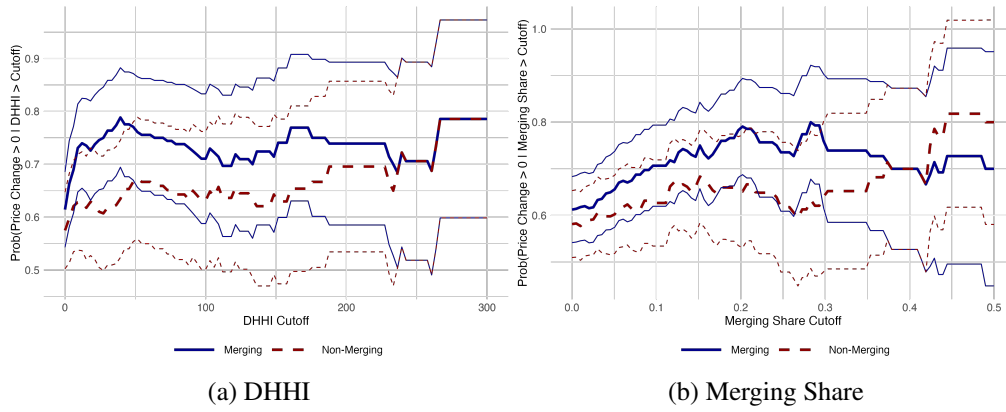


Figure A.8: Share of mergers with positive price changes, conditional on measures of market structure exceeding a cutoff. The bold lines indicate means and the thinner lines outline the 90% confidence interval using Wilson standard errors.

#### A.4. Antitrust Stringency

Table A.4 presents results of the model of antitrust enforcement using the panelist data as the source of estimates of price change. Figure A.10 shows results for the analysis of counterfactual merger thresholds. The results are broadly similar, and the differences are outlined in detail in Section V.C.

	Merging Party Price Changes						Non-Merging Party Price Changes					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
HHI (0–1)	-6.19 (4.38)						0.76 (4.49)					
DHHI (0–1)	15.65 (9.98)						39.02 (12.60)					
HHI ∈ [1500, 2500]		0.41 (2.31)						0.37 (2.36)				
HHI > 2500		-1.40 (2.14)						-0.26 (1.98)				
DHHI ∈ [100, 200]		1.70 (1.85)						0.92 (1.58)				
DHHI > 200		1.48 (0.95)						2.70 (1.28)				
Yellow			2.95 (1.61)						2.37 (1.50)			
Red			0.65 (0.92)						2.45 (1.44)			
Merg Share ∈ [0.10, 0.30]				1.33 (1.33)						1.40 (1.30)		
Merg Share > 0.30				2.97 (1.36)		5.11 (1.13)				3.91 (1.56)		1.06 (1.39)
2023 Presumptions					1.57 (1.00)						2.42 (1.16)	
HHI > 1800						-1.16 (1.77)						0.01 (2.48)
DHHI > 100						-3.81 (5.05)						-3.50 (3.85)
HHI > 1800 × DHHI > 100						5.64 (5.25)						4.01 (4.05)
Merg Share > 0.30 DHHI > 100						-4.78 (1.81)						2.17 (2.19)
Constant	2.46 (1.49)	1.10 (1.94)	0.41 (0.74)	-0.25 (1.14)	0.41 (0.74)	1.13 (1.51)	-0.09 (1.64)	0.20 (1.90)	0.09 (0.72)	-0.56 (1.10)	0.09 (0.72)	0.14 (2.38)
Observations	129	129	129	129	129	129	129	129	129	129	129	129
R <sup>2</sup>	0.017	0.023	0.020	0.029	0.012	0.042	0.043	0.022	0.026	0.047	0.026	0.050

Table A.3: Regression of price changes on measures of market structure using the NielsenIQ Consumer Panel Data. We measure HHI and DHHI as the average across all DMAs. Columns (1)–(6) use merging party price changes, and Columns (7)–(12) use non-merging party price changes. Each observation is a merger. Robust standard errors are in parentheses.

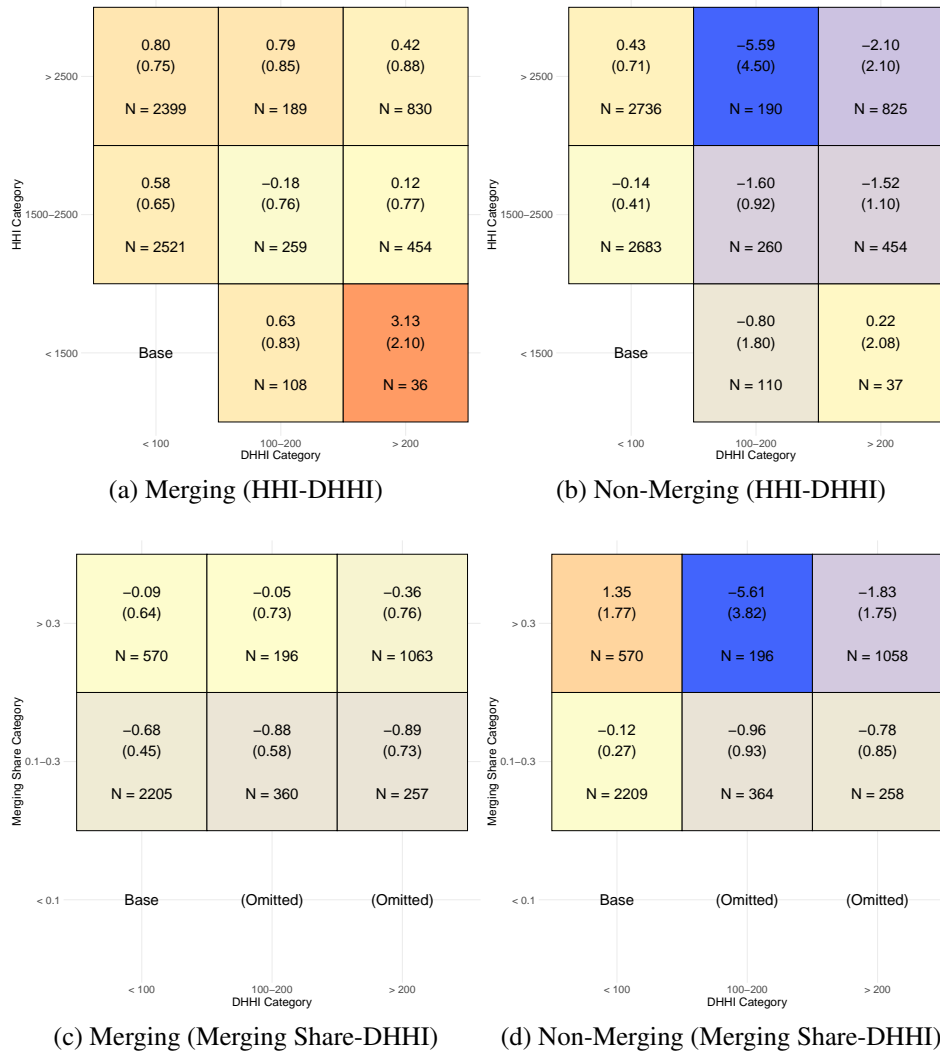


Figure A.9: Within-merger price changes for bins of Scantrack-level HHI, DHHI, and merging party share using the NielsenIQ Consumer Panel Data. Each bin shows the coefficient of a regression of Scantrack-level price changes on bin dummies and merger fixed effects. In the top row, the base bin corresponds to low HHI and low DHHI. In the bottom, to low DHHI and low merging share, and low merging share necessarily implies that DHHI must be less than 100. Standard errors, clustered at the merger level, are in parentheses.  $N$  indicates the number of Scantrack-mergers in each bin.

	Aggregate Price Changes					Merging Price Changes				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<b>A. Prior</b>										
Avg HHI (0–1)	-1.57 (3.37)					-4.21 (4.65)				
Avg DHHI (0–1)	37.45 (13.77)					50.78 (17.32)				
HHI $\in$ [1500, 2500]		0.47 (1.51)						0.35 (2.18)		
HHI > 2500		0.40 (1.46)						-0.17 (2.08)		
DHHI $\in$ [100, 200]		-0.42 (1.27)						-0.15 (1.84)		
DHHI > 200		2.71 (1.34)						4.43 (1.56)		
Yellow			0.30 (1.27)					1.17 (1.88)		
Red			2.90 (1.20)					4.53 (1.50)		
Merg Share $\in$ [0.10, 0.30]				1.84 (0.95)					2.45 (1.32)	
Merg Share > 0.30				3.72 (1.20)					5.13 (1.57)	
2023 Presumptions					2.31 (0.94)					3.34 (1.29)
Constant	1.25 (1.07)	0.45 (1.36)	0.77 (0.51)	-0.11 (0.68)	0.70 (0.51)	2.06 (1.50)	0.77 (1.94)	0.67 (0.69)	-0.40 (0.92)	0.69 (0.70)
<b>B. Errors and Uncertainty</b>										
$\sigma_{p^*}$	4.91 (0.37)	4.86 (0.36)	4.87 (0.36)	4.87 (0.37)	4.92 (0.37)	6.46 (0.50)	6.42 (0.50)	6.41 (0.50)	6.41 (0.49)	6.50 (0.50)
$\sigma_{\epsilon}$	6.03 (2.16)	8.40 (4.46)	8.44 (4.08)	6.25 (2.45)	6.78 (2.70)	3.85 (2.16)	4.45 (2.72)	4.70 (2.90)	3.36 (1.91)	3.73 (2.06)
Posterior Standard Deviation	3.81 (0.56)	4.21 (0.58)	4.22 (0.53)	3.84 (0.57)	3.98 (0.55)	3.31 (1.35)	3.66 (1.48)	3.79 (1.49)	2.97 (1.32)	3.24 (1.33)
<b>C. Threshold</b>										
Log(Annual Merging Sales)	-0.77 (0.45)	-0.53 (0.40)	-0.51 (0.37)	-0.66 (0.45)	-0.62 (0.41)	-1.28 (0.68)	-1.15 (0.67)	-1.09 (0.66)	-1.29 (0.75)	-1.23 (0.67)
Constant	6.98 (1.73)	5.65 (2.04)	5.63 (1.89)	6.82 (1.76)	6.32 (1.74)	10.72 (2.21)	10.18 (2.43)	10.00 (2.47)	11.05 (2.20)	10.70 (2.16)
<b>D. Sales-Weighted Thresholds</b>										
Average	5.66 (1.31)	4.75 (1.60)	4.76 (1.50)	5.69 (1.30)	5.26 (1.35)	8.54 (1.58)	8.22 (1.76)	8.14 (1.80)	8.85 (1.43)	8.59 (1.54)
Q1	4.83 (1.16)	4.19 (1.36)	4.22 (1.30)	4.99 (1.14)	4.60 (1.18)	7.17 (1.45)	6.99 (1.55)	6.98 (1.58)	7.48 (1.33)	7.28 (1.42)
Q3	6.42 (1.54)	5.27 (1.76)	5.27 (1.65)	6.34 (1.54)	5.88 (1.54)	9.80 (1.94)	9.35 (2.14)	9.21 (2.17)	10.12 (1.86)	9.81 (1.89)

Table A.4: Parameter estimates, using aggregate price changes in Columns (1)–(5) and merging party price changes in Columns (6)–(10). Standard errors are in parentheses. Log sales are demeaned.

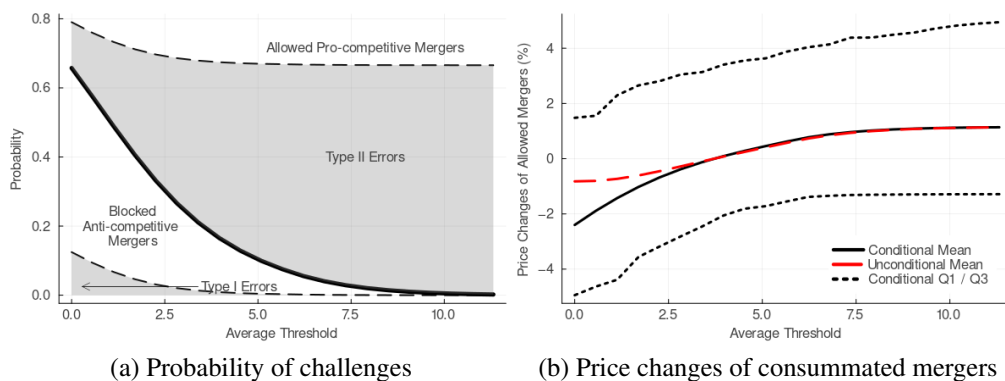


Figure A.10: Outcomes of counterfactual thresholds. This replicates Figure 10, but using estimates from the panelist data.

## B. Additional Results on the Enforcement Model

### B.1. Empirical Model

The model we outline in Section V.A yields a threshold structure in the signal that depends on  $X$  and  $Z$ . The threshold we estimate in Section V.C is in the posterior mean and depends only on  $Z$ . We do this partly to deal with limited data but partly because this is a sensible restriction: the agency does not care about the market structure only insofar as it affects its posterior. Here, we note that we can derive this parameterization in an internally consistent manner from Section V.A.

Suppose that  $p^* \sim N(\mu(X), \sigma_{p^*}^2)$  and  $p \sim N(p^*, \sigma_\epsilon^2)$ . Let  $\omega \equiv \sigma_\epsilon^{-2} / (\sigma_{p^*}^{-2} + \sigma_\epsilon^{-2})$  be the relative precision of the signal and  $\sigma_{\text{post}} \equiv (\sigma_{p^*}^{-2} + \sigma_\epsilon^{-2})^{-1/2}$  denote the standard deviation of the posterior. Suppose further that the cost  $S(p^*; X, Z)$  of allowing a merger through can be written as  $S(Z) \cdot p^*$ : this captures that the agency does not inherently care about the market structure that leads to the price increase. Then, we can compute the integrals in (5) explicitly and the marginal signal  $\hat{p}$  must satisfy

$$\begin{aligned} S(Z) \cdot (\omega \cdot \hat{p} + (1 - \omega) \cdot \mu(X)) + K_{II} \cdot \left[ 1 - \Phi \left( -\frac{\omega \cdot \hat{p} + (1 - \omega) \cdot \mu(X)}{\sigma_{\text{post}}} \right) \right] \\ = K_C + K_I \cdot \Phi \left( -\frac{\omega \cdot \hat{p} + (1 - \omega) \cdot \mu(X)}{\sigma_{\text{post}}} \right). \end{aligned} \quad (6)$$

Of course,  $\hat{p}$  itself depends on  $X$ . However, the posterior mean that  $\hat{p}$  implies (i.e.,  $\omega \cdot \hat{p} + (1 - \omega) \cdot \mu(X)$ ) only depends on  $S(Z)$ ,  $K_C$ ,  $K_I$ , and  $K_{II}$ . To the extent that none of these quantities depend on  $X$  directly, we can estimate a threshold *in the posterior mean* that depends only on  $Z$ . This is the empirical model we estimate in Section V.<sup>29</sup>

Our parameterization is in terms of  $(\beta, \alpha, \sigma_{p^*}, \sigma_\epsilon)$ , where the posterior threshold is  $Z'_i \alpha$  and the prior mean is  $X'_i \beta$ . For most mergers  $i$ , we also see a (noisy) estimate

<sup>29</sup>The important assumption is that  $S(\cdot)$  does not depend directly on  $X$ . If  $S(p^*, Z)$  is not multiplicative, we can approximate it by its Taylor expansion around the posterior mean  $m$ . Then, the first term in (6) is replaced by  $\mathbb{E}[S(m, Z)] + \frac{1}{2} S''(m, Z) \cdot \mathbb{E}[(p^* - m)^2] + \frac{1}{24} S^{(4)}(m, Z) \cdot \mathbb{E}[(p^* - m)^4] + \dots$ , where the derivative notation refers to partial derivatives with respect to the first argument and expectation denote expectations over the posterior. Since the posterior is normal, all moments are a function of just the mean  $m$  and the standard deviation (which does not depend on  $X$ ). Thus, we still have a threshold in  $m$ .

of  $p_i^*$ , which we denote  $\hat{p}_i$ . We treat  $p_i^* \sim N(\hat{p}_i, \sigma_i^2)$  when constructing the likelihood, where  $\sigma_i$  is the standard error of the estimate. Let  $\tau_i \equiv [Z_i' \alpha - (1 - \omega) X_i' \beta] / \omega$  be the “transformed” threshold in terms of the signal when  $Z_i' \alpha$  is the threshold in terms of the posterior mean. Then, the likelihood of observation  $i$  is

$$L_i = \begin{cases} \int \phi(p_i^*; X_i' \beta, \sigma_{p^*}) \cdot \Phi(\tau_i - p_i^*; 0, \sigma_\epsilon) \cdot \phi(p_i^*; \hat{p}_i, \sigma_i) dp_i^* & \text{if } i \text{ is not challenged} \\ \int [1 - \Phi(\tau_i - p_i^*; 0, \sigma_\epsilon)] \cdot \phi(p_i^*; X_i' \beta, \sigma_{p^*}) dp_i^* & \text{if } i \text{ is withdrawn} \\ \int \left( \int_{p_i^*}^\infty \phi(\tilde{p}; X_i' \beta, \sigma_{p^*}) \cdot [1 - \Phi(\tau_i - \tilde{p}; 0, \sigma_\epsilon)] d\tilde{p} \right) \cdot \phi(p_i^*; \hat{p}_i, \sigma_i) dp_i^* & \text{if } i \text{ has a divestiture} \end{cases}.$$

Here,  $\phi(\cdot; \mu, \sigma)$  and  $\Phi(\cdot; \mu, \sigma)$  are the pdf and cdf of a normal with mean  $\mu$  and standard deviation  $\sigma$ . If the merger proceeds without a challenge, then the observed price change informs us about the prior, and we know that the signal must have been lower than the (transformed) threshold. If the merger is withdrawn, we know that the true price change was drawn from the prior and the signal exceeded the transformed threshold. If there was a divestiture, we assume that the true price change would have been larger than the one we estimate and that the signal given this true price change was larger than the transformed threshold. To compute the likelihood in the first case, we use Gaussian quadrature over the distribution of  $p_i^*$ . In the second case, we can write the integral in closed form. In the final case, we use adaptive quadrature for the inner integral and Gaussian quadrature for the outer.

## B.2. Extensions to the Model

In Section V.A, we outline a model in which the antitrust agency challenges if

$$\int (S(p^*; X, Z) + K_{II} \cdot \mathbb{1}[p^* > 0] - K_I \cdot \mathbb{1}[p^* \leq 0]) dF_{p^*|p}(p^*|p, X) \geq K_C, \quad (7)$$

where  $S(\cdot)$  is the cost of allowing a merger that has a price change  $p^*$ ,  $K_C$  is the cost of a challenge, and  $K_I$  and  $K_{II}$  are costs of type I and type II errors, respectively. Note that (7) is a rearrangement of (5), and the left-hand side can be interpreted as the net benefit of a challenge. Here, we outline an extension and some limitations of this model.

First, this model assumes that challenges are successful with probability one. In reality, the probability of a challenge being successful could be a function of  $p$  if, say, the strength of the case depends on the evidence that the agency is able to collect. Call this probability  $w(p; X, Z)$ . For simplicity, suppress all dependence on  $X$  and  $Z$ . Then, the agency would challenge if

$$w(p) \cdot \int (S(p^*) + K_{II} \cdot \mathbb{1}[p^* > 0] - K_I \cdot \mathbb{1}[p^* \leq 0]) dF_{p^*|p}(p^*|p) \geq K_C.$$

If the left-hand side is increasing in  $p$ , we would have a threshold. A sufficient condition for this is that  $w(p)$  is increasing in  $p$ . This could be a sensible assumption: higher  $p$  could correspond to a stronger documentary record suggesting price increases, which could lead to a higher probability of winning. Note that we could also have a model in which  $w(\cdot)$  depends on  $p^*$  rather than  $p$ , perhaps if more evidence is revealed during the challenge process; again, if it is increasing in its argument, the threshold rule would still apply. Thus, the fact that agencies take into account the probability of winning a challenge does not automatically preclude a threshold structure.

Second, a minor comment is that the costs of type I and type II errors need not be incurred based on a comparison to 0. The agency could have an internal threshold where it incurs costs for not blocking mergers that have a price change larger than some  $p_0$ . The left-hand side would still be increasing in  $p$  and we would still have a threshold structure (which may not coincide with  $p_0$ ). Indeed, the broader point is that any increasing function of  $p^*$  on the left-hand side would yield a threshold structure, which leads to many potential microfoundations.

An important limitation of the threshold rule is that in practice, the agency considers other unobserved issues when making challenge decisions. In general, this leads to a two-dimensional screening problem, and without variation that affects the distribution of unobserved non-price concerns, we cannot decide whether the agency chose to challenge because of these unobserved concerns or because they got an unusually large estimate of the price change. However, this provides some intuition that in the presence of these unobserved concerns, we would be misestimating  $\sigma_\epsilon$  to be larger than it is and thus underestimating the precision of the signal. To get

some more intuition on this, consider the likelihood in Appendix B.1 and suppose that the threshold is  $\tau_i + \eta_i$ , where  $\eta_i \sim N(0, \sigma_\eta^2)$  captures the unobserved factors the agency considers for merger  $i$ . For simplicity, assume that  $\sigma_{p^*} \gg \sigma_\epsilon$ , so that  $\omega \approx 1$ . Then, the likelihood would replace every instance of  $\Phi(\tau_i - p_i^*; 0, \sigma_\epsilon)$  with  $\int \Phi(\tau_i + \eta - p_i^*; 0, \sigma_\epsilon) \cdot \phi(\eta; 0, \sigma_\eta) d\eta = \Phi(\tau_i - p_i^*; 0, \sqrt{\sigma_\epsilon^2 + \sigma_\eta^2})$ . In this case, the estimated value of the standard deviation of the signal would be inflated by exactly the magnitude of the non-price considerations.

## C. Additional Tables and Figures

### C.1. Further Validation of the Empirical Specification

In this section, we discuss two checks for our empirical specification. The first is a test of the correlation between the trend and the estimated price effects. The second is a series of placebo tests.

Figure C.1 reports the relationship between the average brand trend from the model, weighted by post-merger volume, and estimated price effects. The concern that this test is meant to address is the fact that negative pre-trends extrapolate to decreasing predicted prices in the no-merger counterfactual, leading mechanically to positive price effects.<sup>30</sup> To benchmark this, we compare the line of best fit with the negative 45 degree line. While we find a negative relationship, the magnitude of the slope (-0.31) is significantly smaller than -1. Moreover, trends do not explain much of the variation in estimated price effects: the  $R^2$  is 0.27.

We next compare the distributions of merger effects obtained under our baseline model with those obtained from placebo mergers. To compute placebo effects for each merger, we begin by selecting a set of placebo merger dates. Our baseline research design uses three years for the pre-period of the merger, and two years for the post-period. We have decided to exclude the five years immediately after the merger and the two years immediately before from the set of potential placebo dates. Excluding the five years immediately after the merger ensures that the placebo merger's three year pre-period does not overlap with the real merger's two year post-period. Excluding the two years immediately before ensures that the placebo merger's two year post-period does not overlap with the real merger's post-period. Additionally, we exclude dates that lead to a pre-period that is shorter than 36 and a post period that is shorter than 24.

These restrictions imply that some mergers do not have many candidate placebo merger months, and thus placebo effects drawn for that merger would be very

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<sup>30</sup>Of course, it could be that a merger rejuvenates a dying product line, which would be a situation where one would expect a negative pre-trend following by a positive price effect. A case where a merger allows the merged entities to control increasing costs would be captured by a positive pre-trend and a negative price effect. Thus, while a reasonable prior is that estimated price effects should not be correlated with pre-trends, this need not be the case.

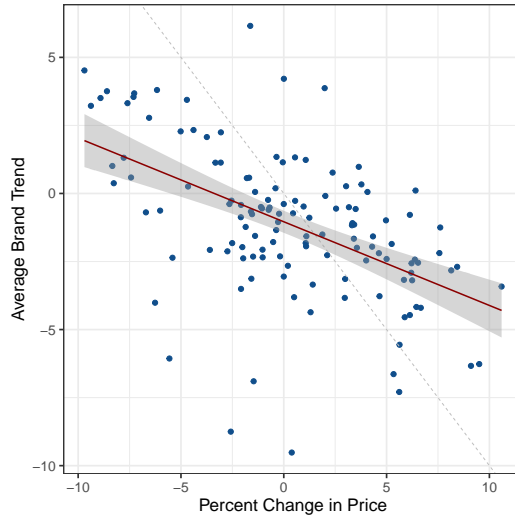


Figure C.1: Correlation between estimated and average brand trends

correlated with each other. We thus focus on mergers with at least 24 candidate placebo months, which gives us 99 mergers on which we conduct this placebo analysis. For each merger, we draw 100 placebo merger dates, and compute placebo outcome effects by estimating (1) and (2). We use the same set of cost controls used in the baseline specification for each merger.

Figure C.2(a) compares the distribution of aggregate price effects under our baseline specification to the placebo price effect distribution: the dashed red distribution is the mixture of the placebo distributions for all mergers. The distribution of placebo effects is centered around 0. We also observe visually that it is tighter than the distribution of baseline effects and has more mass centered around 0 than the placebos. To evaluate this quantitatively, we consider a number of different measures of central tendency: interquartile range, the difference between the 90th and 10th percentile, the mass between price changes of  $\pm 2.5\%$ , and the mass between price changes of  $\pm 5\%$ . For each of these measures, we conduct a randomization test to compare the test statistic of the true distributions to the test statistics of “pseudo-distributions” constructed by drawing one effect from the union of true and placebo effects for each merger. The  $p$ -values for these tests are 0.008, 0.109, 0.018, and 0.001, respectively.<sup>31</sup> Overall, this gives us confidence that placebo estimates from

<sup>31</sup>We also studied the standard deviation. The  $p$ -values for this randomization test are much larger,

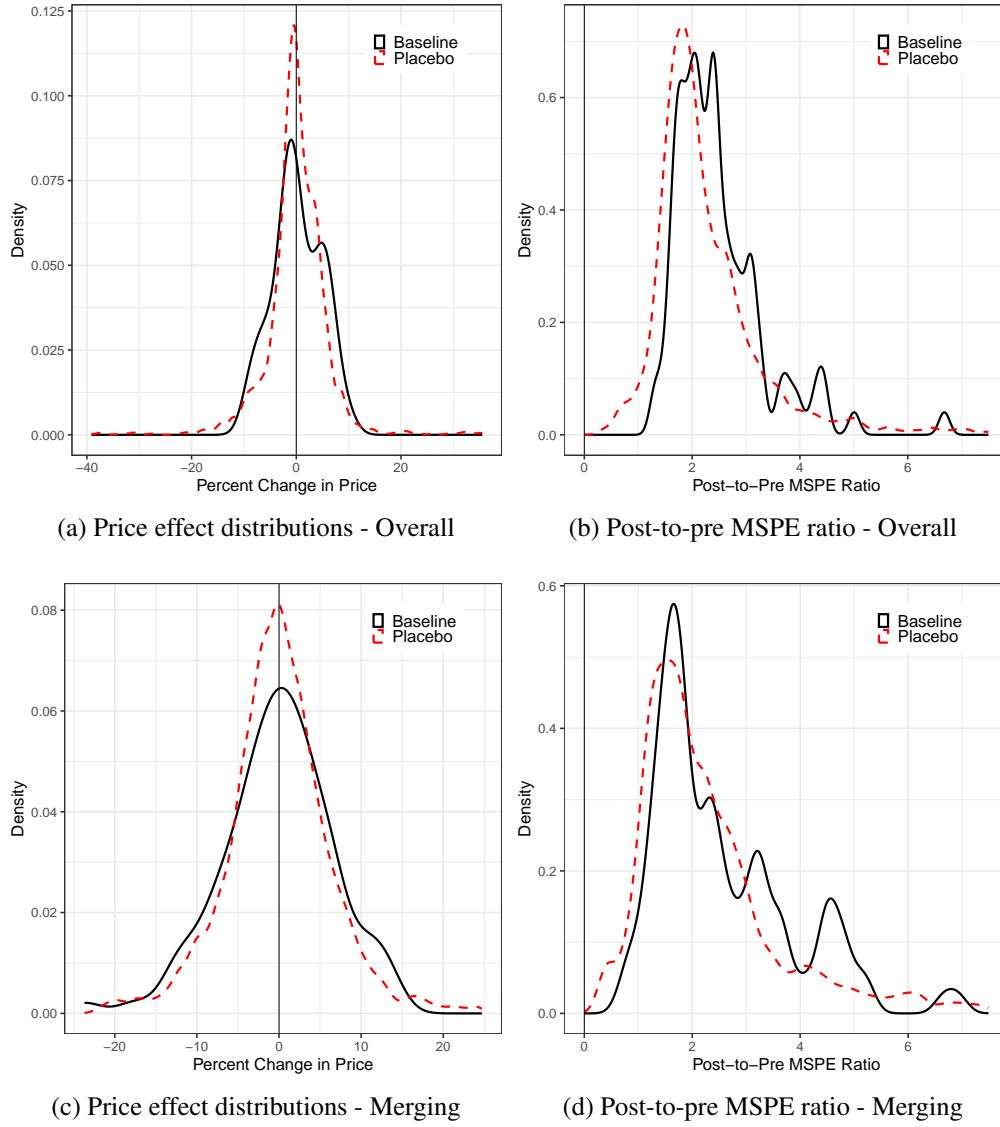


Figure C.2: Comparison of baseline and placebo results.

this procedure would be significantly closer to zero, despite their natural variation.

We also consider another metric to assess these placebo tests. For each date (true or placebo), we can estimate the model on the pre-period and compute the mean square prediction error (MSPE) of the model both before and after the merger.

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as the placebo distribution has a larger standard deviation than the baseline. This is driven by outlier draws in the placebo distribution.

These will be positive, of course, as the model will not perfectly predict the prices. However, we would expect that the model will be a worse predictor of prices in the post-period of the true merger than it will in the post-period of the placebo mergers. Thus, we would expect the ratio of post-MSPE to the pre-MSPE to be larger in true mergers than in placebos. This comparison of post to pre MSPEs is often used to assess the validity of the model in synthetic control designs (Abadie et al., 2015). The results in Panel (b) show the distributions of these ratios, and the placebo distribution is clearly shifted to the left of the true distribution. We do a permutation test using the one-sided Kolmogorov-Smirnov statistic as our test statistic and find a  $p$ -value less than 0.01. Thus, the model does a relatively worse job at fitting prices after a true merger than after a placebo merger, consistent with it capturing true merger effects.

Panels (c) and (d) repeat this exercise for merging party price changes. Similar patterns hold visually. The  $p$ -values for the randomization tests related to the share within a cutoff are 0.106 and 0.095 (for  $\pm 2.5\%$  and  $\pm 5\%$ , respectively). However, the  $p$ -values for the IQR and the 90-10 range test statistics are around 0.34. The  $p$ -value for the Kolmogorov-Smirnov test statistic on the MSPE distributions is again below 0.01.

## C.2. Additional Results Related to Price Effects

Table C.1 performs additional robustness tests on the distribution of estimated price effects documented in Section III.B. Panel A reproduces the estimates from the baseline specification in Table 2 for convenience.

Panel B reports results for mergers involving manufacturers of food products. The rationale for looking at this subset of mergers is that NielsenIQ coverage may be better for this set of products, and food products are less likely to be sold through non-NielsenIQ channels, so the NielsenIQ dataset may have better coverage for food products. We find that mean effects are slightly larger for food mergers—by 0.2–0.6 pp—and dispersion is also slightly larger (by approximately 0.2 pp when looking at standard deviations). The most salient difference is that the upper tail of the distribution of food mergers is longer, as the medians and upper quartiles are larger. Note that it may simply be that non-food mergers had more muted price

effects; this need not be indicative of an issue with the coverage of the NielsenIQ dataset. Overall, we find the patterns to be similar.

Panel C reports results of a specification where prices are computed as the sales-weighted average at the brand-DMA-month level. That is, we collapse to the brand level when computing the regressions. This specification is designed to address potential churn in UPCs the dataset. Mean price effects are still small, although they are up to 1 pp lower for overall and non-merging prices (and 0.1 pp different for merging). We find wider standard deviations too, due to a longer left tail for non-merging parties and wider extremes for merging parties. It is worth noting that despite these differences in point estimates, these point estimates are within the confidence intervals of the baseline.

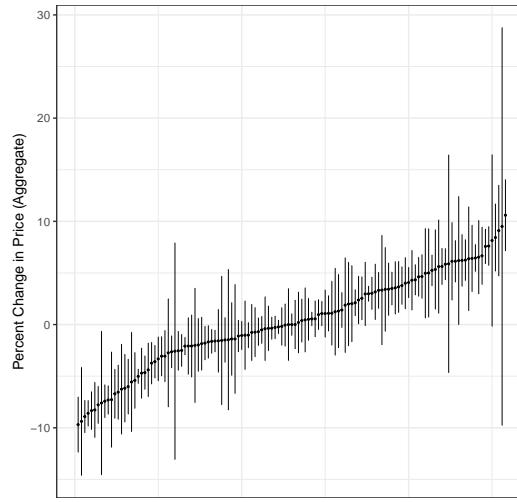
Panel D reports the distribution of merger effects obtained using Bayesian shrinkage. We implement a “random effects” model where the price effect of merger  $i$  is  $p_i \sim N(\mu, \sigma^2)$ , and our estimate is  $\hat{p}_i \sim N(p_i, \sigma_i^2)$ , where  $\sigma_i$  is our estimated second-stage 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.<sup>32</sup> We report distributions of the posterior mean of each  $p_i$  in the tables. As expected, the distributions are tighter, with standard deviations decreasing by about 1 pp. The estimated mean effects for aggregate and non-merging price changes reduce by about 0.1–0.2 pp. The fact that the price effects are largely similar can be traced back to the fact that estimated standard errors for the effects are typically much smaller than the standard deviation of the distribution of estimates. Figure C.3 illustrates this by displaying merger-level estimates and 95% confidence intervals for aggregate, merging-party, and non-merging-party price changes.

Finally, Panel E drops the mergers in our dataset where there was a divestiture. The difference with Panel A is minimal.

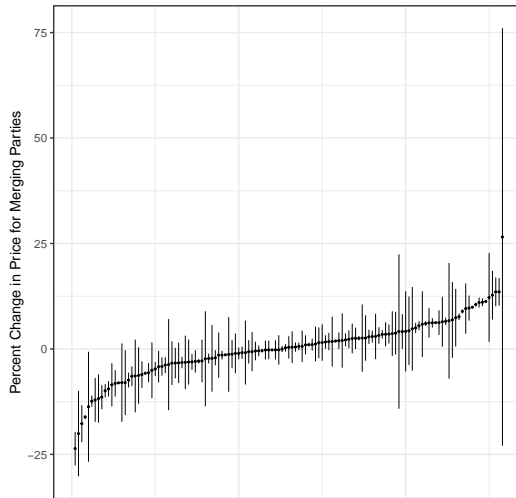
Figure C.4 is not directly related to price effects. It reports the timing of mergers. It displays, both at the merger and the deal level, histograms of the dates at which mergers became effective. Figure 3 shows that price changes appear to be coincident

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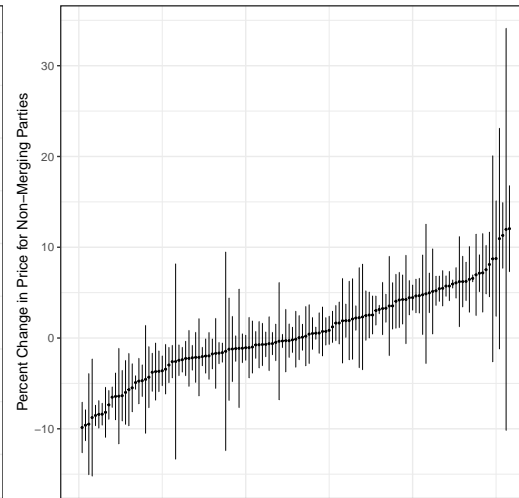
<sup>32</sup>For each merger, we run a chain with a burn-in of 5,000 iterations and draw 20,000 more iterations. We have checked that chains converge given the diagnostics reported by the package.



(a) Aggregate



(b) Merging



(c) Non-Merging

Figure C.3: 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.

with the timing of the merger. The fact that mergers do not seem to be completed on special dates of the year helps rule out one alternative explanation for that observation.

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	129	0.31 (0.40)	4.59	-2.09 (0.40)	0.00 (0.48)	3.65 (0.54)
Merging Parties	129	0.12 (0.62)	7.07	-3.22 (0.62)	0.38 (0.58)	3.74 (0.65)
Non-Merging Parties	129	0.48 (0.42)	4.81	-2.24 (0.38)	-0.13 (0.46)	4.24 (0.65)
B. Food Mergers Only						
Overall	75	0.95 (0.57)	4.91	-2.74 (1.12)	1.99 (0.70)	4.66 (0.53)
Merging Parties	75	0.35 (0.83)	7.18	-3.11 (0.90)	1.00 (0.91)	6.02 (1.09)
Non-Merging Parties	75	1.10 (0.58)	5.00	-2.17 (0.92)	1.90 (1.07)	4.87 (0.57)
C. Brand Level						
Overall	129	-0.60 (0.66)	7.52	-2.62 (0.24)	-0.64 (0.52)	3.70 (0.72)
Merging Parties	129	0.25 (0.79)	9.02	-4.18 (1.04)	0.06 (0.62)	4.15 (0.71)
Non-Merging Parties	129	-0.65 (0.68)	7.68	-3.24 (0.64)	-0.63 (0.42)	3.48 (0.76)
D. Bayesian Shrinkage						
Overall	129	0.20 (0.35)	3.95	-1.85 (0.36)	0.02 (0.39)	3.26 (0.49)
Merging Parties	129	-0.01 (0.51)	5.82	-2.86 (0.69)	0.36 (0.53)	3.00 (0.40)
Non-Merging Parties	129	0.28 (0.35)	4.00	-2.01 (0.45)	-0.08 (0.34)	3.26 (0.57)
E. Mergers without Divestitures						
Overall	123	0.24 (0.42)	4.68	-2.54 (0.47)	-0.05 (0.50)	4.01 (0.67)
Merging Parties	123	0.02 (0.65)	7.20	-3.35 (0.60)	-0.03 (0.58)	4.12 (0.65)
Non-Merging Parties	123	0.41 (0.44)	4.90	-2.39 (0.34)	-0.29 (0.42)	4.25 (0.69)

Table C.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 for food mergers only, Panel C displays results for brand-level regressions, Panel D displays results using Bayesian shrinkage, and Panel E displays results for mergers without divestitures.

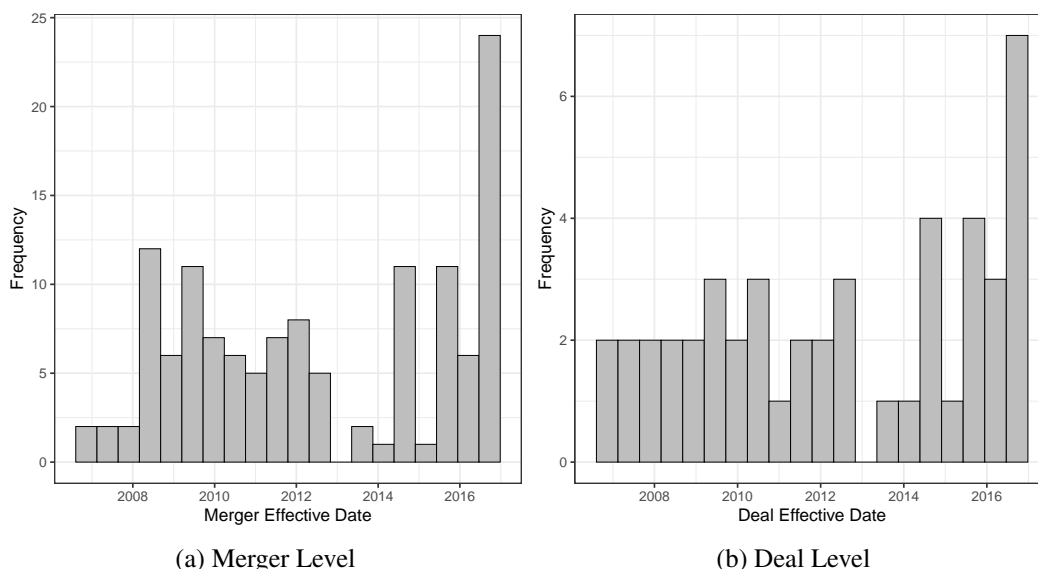


Figure C.4: Timing of mergers. These plots display histograms of the dates at which mergers became effective. The unit of observation of the left-hand panel is a merger, while the unit of observation of the right-hand panel is a deal.

### C.3. Additional Results Related to Quantity Effects

Figure C.5 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. Like for prices, we do not see any evidence of pre-trends: mergers where we estimate a positive quantity effect are not coming from an increase in quantities leading up to the merger, say. Moreover, the changes again seem coincident with the timing of the merger. The time path of quantity increases is noisier than that for quantity decreases, but there does not seem to be clear evidence of quantities flattening out within the two years after merger completion. Finally, quantity changes are correlated with each other, which was also evidenced in Figure 4(b).

Table C.2 provides robustness checks for quantity effects. We use the same robustness checks as the ones in Tables C.1, except for the brand aggregation since the quantity analysis is already aggregated to the merging/non-merging level. Results are quite similar across all panels, except for the fact that dropping the non-food

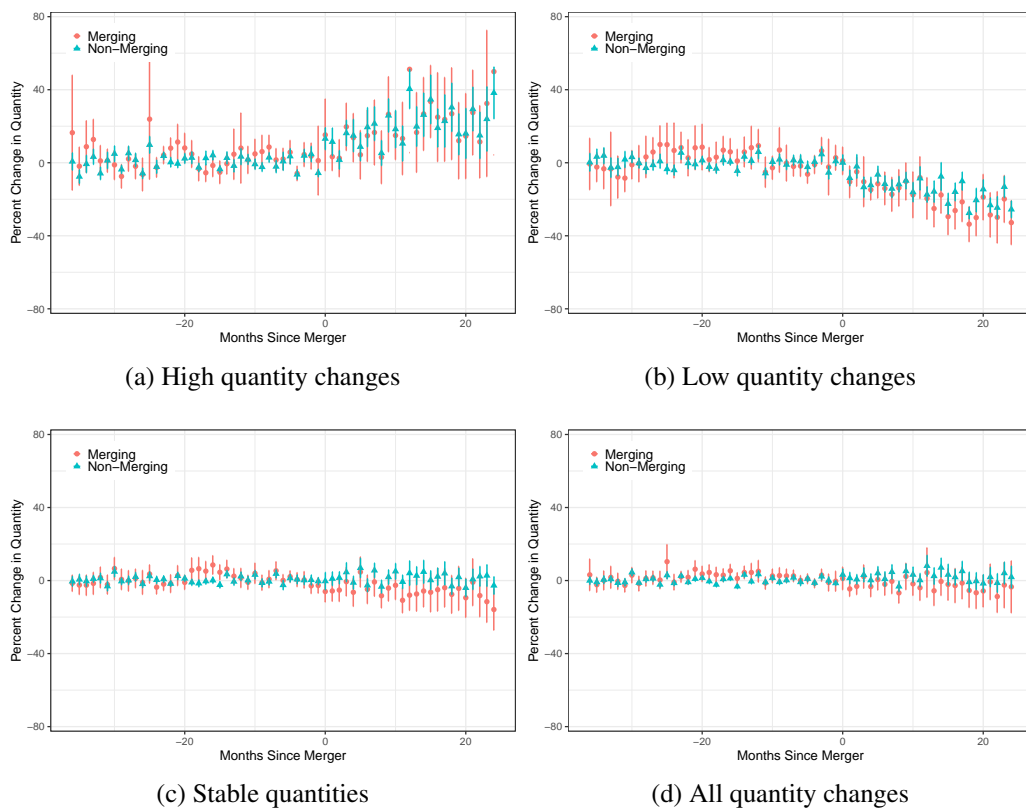


Figure C.5: Timing of quantity changes, for merging parties (red circle) and non-merging parties (blue triangle). The marker indicates the mean quantity change the given number of months after the merger becomes effective, and the thick line is the 95% confidence interval of that mean. Panels (a)–(c) 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.

mergers increases the upper tail (and the mean) for merging party changes. Overall, we find that qualitative and quantitative takeaways to be very similar.

Table C.3 regresses the estimated quantity effect on price effects and other effects estimated in Section III.D. Column (1) confirms the merging price and quantity changes do not exhibit a significant correlation. However, the results in Column (2) confirm that once we control for merging brand changes and distribution changes—both of which are positively correlated with quantity changes—the coefficient on price changes increases. Controlling for competitors’ responses increases it even further; the competitors’ price effect has a significant coefficient, while the brand

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	129	-0.54 (1.37)	15.55	-9.68 (1.22)	-2.34 (0.96)	5.02 (1.92)
Merging Parties	129	-6.41 (2.86)	32.53	-20.74 (2.74)	-9.17 (2.22)	5.74 (2.67)
Non-Merging Parties	129	0.66 (1.47)	16.75	-7.98 (1.64)	-1.58 (0.73)	5.49 (1.74)
B. Food Mergers Only						
Overall	75	-0.41 (1.36)	11.75	-8.74 (1.99)	-0.99 (1.27)	6.01 (1.97)
Merging Parties	75	-2.77 (4.03)	34.91	-20.57 (2.87)	-7.31 (4.18)	12.93 (6.15)
Non-Merging Parties	75	1.03 (1.62)	14.00	-6.85 (2.19)	-1.12 (1.12)	7.57 (2.41)
C. Bayesian Shrinkage						
Overall	129	-0.55 (1.36)	15.45	-9.67 (1.21)	-2.34 (0.95)	4.99 (1.91)
Merging Parties	129	-6.96 (2.69)	30.56	-20.01 (2.67)	-9.17 (2.16)	5.46 (2.63)
Non-Merging Parties	129	0.61 (1.45)	16.51	-7.97 (1.63)	-1.57 (0.73)	5.45 (1.73)
D. Mergers without Divestitures						
Overall	123	-0.39 (1.43)	15.87	-10.15 (1.41)	-1.94 (0.98)	6.01 (1.80)
Merging Parties	123	-6.44 (3.00)	33.28	-21.61 (2.91)	-9.23 (2.27)	6.40 (3.36)
Non-Merging Parties	123	0.76 (1.54)	17.11	-8.29 (1.78)	-1.58 (0.75)	5.92 (1.98)

Table C.2: 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 for food mergers only, Panel C displays results using Bayesian shrinkage, and Panel D displays results for mergers without divestitures.

and store effects are noisily estimated. Overall, these results complement the ones in Figure 6 and explain the relationship between merging price and quantity changes. Columns (4)–(6) repeat this exercise for non-merging quantity changes. Results are not conclusive, although we do find that the largest quantity changes are associated with increases in distribution networks (which is still relatively rare for non-merging

	Merging Quantity			Non-Merging Quantity		
	(1)	(2)	(3)	(4)	(5)	(6)
Merging Price Effect	-0.22 (0.49)	-0.46 (0.29)	-0.95 (0.29)			-0.06 (0.25)
Merging Brands Effect		0.44 (0.19)	0.51 (0.24)			0.13 (0.13)
Merging Stores Effect		0.74 (0.16)	0.77 (0.17)			-0.12 (0.12)
Non-Merging Price Effect			1.28 (0.51)	0.01 (0.27)	0.15 (0.30)	0.16 (0.36)
Non-Merging Brands Effect			-0.36 (0.33)		0.18 (0.21)	0.07 (0.15)
Non-Merging Stores Effect			0.68 (0.60)		1.41 (0.49)	1.48 (0.50)
Constant	-6.39 (2.87)	-3.35 (2.46)	-4.88 (2.28)	0.65 (1.48)	1.08 (1.75)	0.70 (1.55)
Observations	129	129	129	129	129	129
R <sup>2</sup>	0.002	0.398	0.440	0.000	0.103	0.127

Table C.3: Regression of quantity effects of merging parties (Columns (1)–(3)) and non-merging parties (Columns (4)–(6)) on other effects. Robust standard errors are in parentheses.

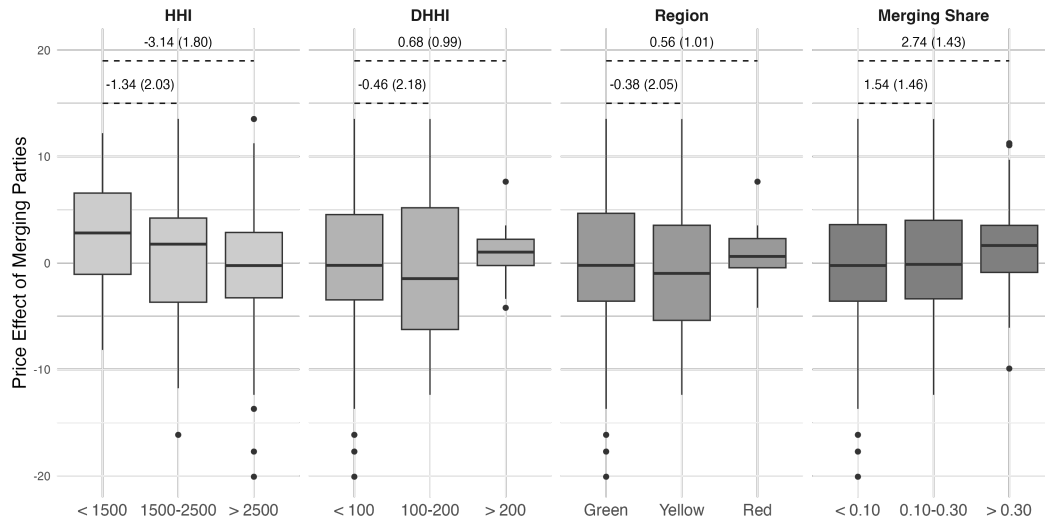
parties).

#### C.4. Additional Results Related to Antitrust Enforcement

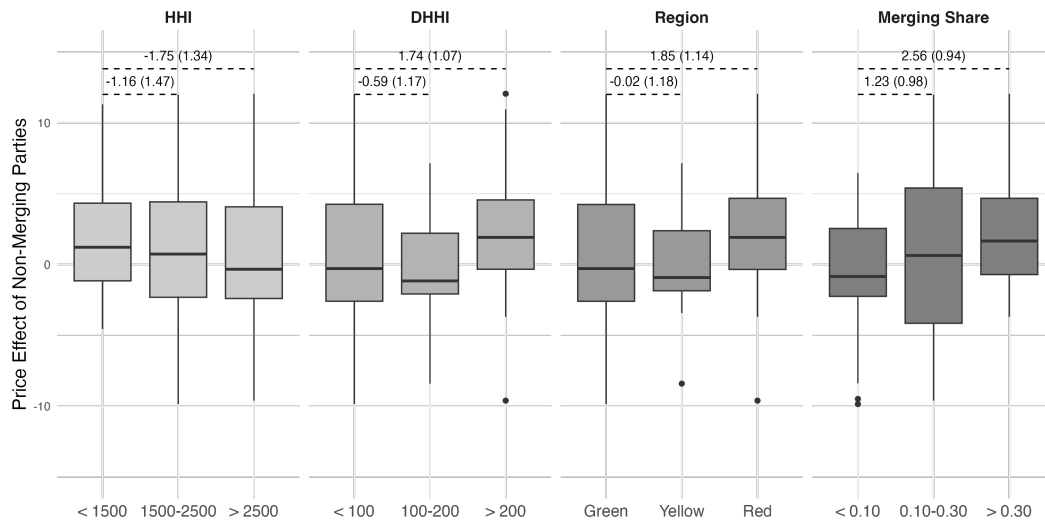
Figure C.6 shows conditional distributions of price changes by bins of HHI, DHHI, 2010 Region, and merging share. The variance in the conditional distributions is large, although the only statistically significant difference in means is between the bin with the largest merging share and that with the smallest. Table C.4 shows the equivalent of Table 6, but using aggregate prices. As before, we find that controlling for multiple measures of market structure adds precision to the estimates, and the patterns are like the ones in Table 6.

Table C.5 presents standard errors on all pairwise differences in Figure 8. This supports the statements made about these differences in Section IV.B.

We now turn to analysis of robustness. We first study robustness of the analysis of the structural presumptions. Table C.6 replicates Table 6 but uses nationwide HHI and DHHI as the metrics for market structure, rather than the average of DMA-

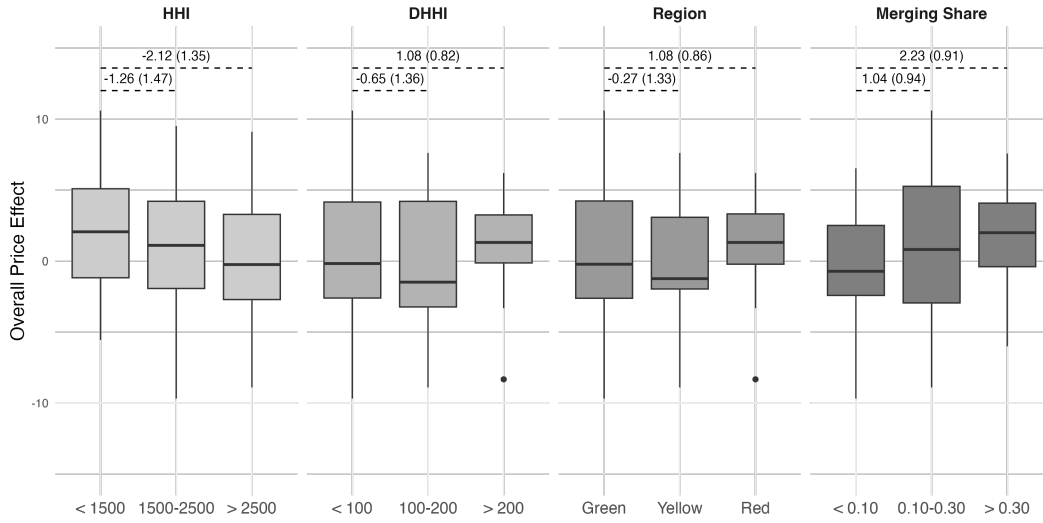


(a) Merging Parties



(b) Non-Merging Parties

Figure C.6: Boxplots for price changes of (a) merging, (b) non-merging parties, and ((c), on subsequent page) all parties, by market characteristics. The numbers over the dotted lines indicate the difference in means with the baseline group, with standard errors in parentheses. Some outliers are excluded due to truncation of the plots.



(c) Aggregate Parties

Figure C.6: (Continued)

level HHI and DHHI. While there are some differences that are noticeable (e.g., the coefficient on the large DHHI bin is slightly larger, and the coefficient on the red zone for merging party price changes is smaller), magnitudes are quantitatively similar and are always within the confidence intervals of the baseline estimates.

A second robustness test drops mergers with divestitures. We do this for two reasons. First, we use the realized price changes of mergers with divestitures differently in the model (treating them as a lower bound of the true price change). Second, mergers in which the agencies forced divestitures are special, and checking whether similar patterns related to the structural presumptions hold for other mergers is informative. Table C.7 replicates Table 6 but drops mergers in which the parties had to divest at least one brand. Once again, we find that results are quantitatively quite similar to our baseline. This suggests that comparison between the results in Table 6 and those of the prior mean in Table 7 are not due to difference in the sample and are instead due to the selection correction embedded in the model.

Related to this robustness check, we can rerun the within-merger analysis restricting to mergers without divestitures. Figure C.7 replicates Figure 8 but drops mergers in which parties had to divest at least one brand. Estimates are very similar,

	(1)	(2)	(3)	(4)	(5)	(6)
HHI (0–1)	-2.82 (3.54)					
DHHI (0–1)	19.11 (7.94)					
HHI $\in$ [1500, 2500]		-1.47 (1.45)				
HHI > 2500		-2.67 (1.38)				
DHHI $\in$ [100, 200]		-0.71 (1.36)				
DHHI > 200		1.69 (0.87)				
Yellow			-0.27 (1.33)			
Red			1.08 (0.86)			
Merg Share $\in$ [0.10, 0.30]				1.04 (0.94)		
Merg Share > 0.30				2.23 (0.91)		5.44 (1.41)
2023 Presumptions					0.54 (0.82)	
HHI > 1800						-2.70 (1.08)
DHHI > 100						-2.74 (2.83)
HHI > 1800 × DHHI > 100						3.85 (3.12)
Merg Share > 0.30 × DHHI > 100						-4.87 (1.98)
Constant	0.87 (1.08)	2.16 (1.25)	0.17 (0.50)	-0.52 (0.54)	0.17 (0.50)	1.97 (0.89)
Observations	129	129	129	129	129	129
R <sup>2</sup>	0.020	0.044	0.008	0.033	0.003	0.088

Table C.4: Regression of aggregate price changes on measures of market structure. Each observation is a merger. Robust standard errors are in parentheses.

even quantitatively. Thus, our observation that prices are correlated with DHHI and merging share within merger is not driven by mergers with divestitures.

Finally, recall that in Appendix A.3, we present both cross- and within-merger correlations with structural presumptions when using the Consumer Panel. We refer the reader to the discussion there rather than reproducing results here.

	Green				Yellow			Red		Green				Yellow			Red
	LM	LH	ML	HL	MM	MH	HM	HH		LM	LH	ML	HL	MM	MH	HM	HH
LL	1.18 (0.76)	2.64 (0.91)	0.64 (0.43)	0.72 (0.60)	0.28 (0.54)	2.24 (0.70)	1.00 (0.62)	1.84 (0.73)	LL	-0.66 (0.38)	-0.77 (0.73)	0.09 (0.29)	0.37 (0.39)	0.41 (0.48)	1.04 (0.67)	1.24 (0.53)	1.47 (0.59)
LM		1.46 (0.83)	-0.55 (0.83)	-0.46 (0.93)	-0.91 (0.86)	1.06 (0.95)	-0.19 (0.93)	0.66 (1.00)	LM		-0.12 (0.68)	0.75 (0.43)	1.02 (0.48)	1.06 (0.55)	1.69 (0.67)	1.89 (0.59)	2.13 (0.64)
LH			-2.00 (0.96)	-1.92 (1.04)	-2.36 (0.98)	-0.40 (0.92)	-1.64 (1.03)	-0.80 (1.08)	LH			0.87 (0.75)	1.14 (0.78)	1.18 (0.83)	1.81 (0.96)	2.01 (0.82)	2.25 (0.89)
ML				0.08 (0.42)	-0.36 (0.43)	1.61 (0.64)	0.36 (0.49)	1.20 (0.63)	ML				0.27 (0.20)	0.31 (0.38)	0.94 (0.61)	1.14 (0.42)	1.38 (0.49)
HL					-0.44 (0.52)	1.52 (0.69)	0.27 (0.36)	1.12 (0.52)	HL					0.04 (0.37)	0.67 (0.63)	0.87 (0.44)	1.10 (0.51)
MM						1.96 (0.50)	0.72 (0.46)	1.56 (0.62)	MM						0.63 (0.60)	0.83 (0.51)	1.07 (0.50)
MH							-1.25 (0.59)	-0.40 (0.63)	MH							0.20 (0.59)	0.44 (0.55)
HM								0.84 (0.38)	HM								0.24 (0.31)

(a) Merging parties (HHI-DHHI)							(b) Non-merging parties (HHI-DHHI)						
	ML	MM	MH	HL	HM	HH		ML	MM	MH	HL	HM	HH
LL	1.49 (0.60)	1.42 (0.63)	2.16 (0.70)	2.12 (0.80)	1.98 (0.72)	2.88 (0.80)	LL	-0.18 (0.26)	0.14 (0.39)	0.43 (0.56)	-0.34 (0.61)	0.36 (0.46)	0.86 (0.54)
ML		-0.07 (0.39)	0.67 (0.48)	0.63 (0.52)	0.49 (0.42)	1.40 (0.57)	ML		0.32 (0.29)	0.61 (0.50)	-0.16 (0.58)	0.54 (0.39)	1.04 (0.49)
MM			0.74 (0.38)	0.70 (0.58)	0.56 (0.39)	1.46 (0.51)	MM			0.29 (0.40)	-0.48 (0.62)	0.22 (0.35)	0.72 (0.45)
MH				-0.04 (0.60)	-0.18 (0.40)	0.73 (0.34)	MH				-0.77 (0.83)	-0.07 (0.51)	0.43 (0.59)
HL					-0.14 (0.46)	0.77 (0.59)	HL					0.70 (0.59)	1.20 (0.71)
HM						0.91 (0.40)	HM						0.50 (0.36)

(c) Merging parties (Merging Share-DHHI)							(d) Non-merging parties (Merging Share-DHHI)						
	ML	MM	MH	HL	HM	HH		ML	MM	MH	HL	HM	HH
LL	1.49 (0.60)	1.42 (0.63)	2.16 (0.70)	2.12 (0.80)	1.98 (0.72)	2.88 (0.80)	LL	-0.18 (0.26)	0.14 (0.39)	0.43 (0.56)	-0.34 (0.61)	0.36 (0.46)	0.86 (0.54)
ML		-0.07 (0.39)	0.67 (0.48)	0.63 (0.52)	0.49 (0.42)	1.40 (0.57)	ML		0.32 (0.29)	0.61 (0.50)	-0.16 (0.58)	0.54 (0.39)	1.04 (0.49)
MM			0.74 (0.38)	0.70 (0.58)	0.56 (0.39)	1.46 (0.51)	MM			0.29 (0.40)	-0.48 (0.62)	0.22 (0.35)	0.72 (0.45)
MH				-0.04 (0.60)	-0.18 (0.40)	0.73 (0.34)	MH				-0.77 (0.83)	-0.07 (0.51)	0.43 (0.59)
HL					-0.14 (0.46)	0.77 (0.59)	HL					0.70 (0.59)	1.20 (0.71)
HM						0.91 (0.40)	HM						0.50 (0.36)

Table C.5: 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 in Panels (a) and (b). In Panels (c) and (d), the first letter denotes the bin of merging share. 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.

An estimate that feeds into the model of antitrust enforcement is the probability of challenging a merger. In the context of the model, this should be a function of both the prior and the elements of the threshold. Table C.8 reports linear probability models of enforcement on merger-level market structure (in Columns (1)–(5)) and these measures together with elements of the threshold (in Columns (6)–(10)). This table corroborates that enforcement correlates with DHHI (linearly, or looking at the high bin), the red region of the merger guidelines, high merging share, and the 2023 presumptions. It is also correlated with the high region of HHI. These estimates,

	Merging Party Price Changes						Non-Merging Party Price Changes					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
HHI (0–1)	-9.91 (3.92)						-4.70 (4.12)					
DHHI (0–1)	20.17 (9.36)						28.71 (9.57)					
HHI ∈ [1500, 2500]		0.06 (1.75)						-0.95 (1.26)				
HHI > 2500		-3.52 (1.73)						-2.39 (1.18)				
DHHI ∈ [100, 200]		1.46 (1.97)						0.56 (1.04)				
DHHI > 200		2.23 (1.14)						2.62 (1.09)				
Yellow			1.32 (1.72)						0.80 (0.91)			
Red			0.12 (0.98)						2.02 (1.28)			
Merg Share ∈ [0.10, 0.30]				1.54 (1.46)						1.23 (0.98)		
Merg Share > 0.30				2.74 (1.43)		6.57 (2.47)				2.56 (0.94)		4.72 (1.23)
2023 Presumptions					0.48 (1.13)						1.32 (0.91)	
HHI > 1800						-0.82 (1.51)						-1.22 (1.05)
DHHI > 100						4.97 (2.60)						0.89 (1.22)
HHI > 1800 × DHHI > 100						-4.36 (2.98)						1.07 (1.73)
Merg Share > 0.30 × DHHI > 100						-5.95 (2.95)						-4.61 (1.98)
Constant	2.35 (1.12)	1.24 (1.30)	-0.06 (0.80)	-1.01 (1.00)	0.00 (0.79)	0.00 (0.99)	1.21 (1.08)	1.44 (1.01)	0.10 (0.51)	-0.50 (0.54)	0.15 (0.50)	0.63 (0.80)
Observations	129	129	129	129	129	129	129	129	129	129	129	129
R <sup>2</sup>	0.029	0.063	0.004	0.022	0.001	0.054	0.048	0.056	0.022	0.040	0.014	0.059

Table C.6: Regression of price changes on measures of market structure. We use HHI and DHHI as computed using nationwide shares. Columns (1)–(6) use merging party price changes, and Columns (7)–(12) use non-merging party price changes. Each observation is a merger. Robust standard errors are in parentheses.

together with the correlation of prices with market structure, inform our estimate of the prior in Panel A of Table 7. Notably, Columns (6)–(10) show that enforcement is correlated with our measure of market size (with  $p$ -values typically between 0.10 and 0.15), which feeds into the estimates of the effect of market size on the threshold in Panel C of Table 7.

We now move to robustness checks on the model of antitrust enforcement. For completeness, we start by simply reporting confidence intervals on our analysis of counterfactual antitrust policies. Figure C.8 reports Figure 10 but adds 95% confidence intervals as dotted lines. The main takeaway that reducing the threshold

	Merging Party Price Changes						Non-Merging Party Price Changes					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
HHI (0–1)	-9.20 (4.33)						-2.74 (3.61)					
DHHI (0–1)	21.08 (12.19)						32.06 (10.98)					
HHI ∈ [1500, 2500]		-1.47 (1.99)						-1.39 (1.46)				
HHI > 2500		-3.69 (1.89)						-2.47 (1.35)				
DHHI ∈ [100, 200]		-0.43 (2.11)						-0.64 (1.21)				
DHHI > 200		1.20 (1.15)						2.24 (1.30)				
Yellow			-0.38 (2.06)						-0.02 (1.18)			
Red			0.04 (1.04)						1.82 (1.41)			
Merg Share ∈ [0.10, 0.30]				1.48 (1.53)						1.06 (1.04)		
Merg Share > 0.30				2.58 (1.46)		6.63 (2.46)				2.72 (0.99)		4.92 (1.19)
2023 Presumptions					-0.15 (1.24)						0.96 (1.01)	
HHI > 1800						-1.68 (1.62)						-2.92 (1.13)
DHHI > 100						-0.38 (5.21)						-3.76 (1.83)
HHI > 1800 × DHHI > 100						0.33 (5.60)						4.81 (2.52)
Merg Share > 0.30 × DHHI > 100						-5.50 (3.21)						-3.57 (2.24)
Constant	2.50 (1.37)	2.53 (1.56)	0.06 (0.81)	-1.01 (1.00)	0.06 (0.81)	0.96 (1.22)	0.81 (1.12)	2.05 (1.24)	0.17 (0.51)	-0.50 (0.54)	0.17 (0.51)	2.20 (0.92)
Observations	123	123	123	123	123	123	123	123	123	123	123	123
R <sup>2</sup>	0.029	0.033	0.000	0.019	0.000	0.038	0.040	0.043	0.016	0.041	0.007	0.089

Table C.7: 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)–(6) use merging party price changes, and Columns (7)–(12) use non-merging party price changes. Each observation is a merger. Robust standard errors are in parentheses.

will limited impact on type I errors holds.

Finally, Figure C.9 reruns the counterfactual analysis of Section V.D but uses merging party price changes as the outcome of interest—following the specification in Column (6) of Table 7. The broad patterns remain the same, although the quantification changes slightly. Moving to a 2.5% threshold would lead to a probability of challenge of 0.33. Moving to a 0% threshold would lead to a probability of challenge of 0.56. The type I error rates are still low. However, they are quantitatively larger than when looking at aggregate price changes: at thresholds of 2.5% and 0%, about 11% and 22% of blocked mergers would be a type I error (respectively), compared to 5% and 18% when using aggregate price changes. These larger numbers can be

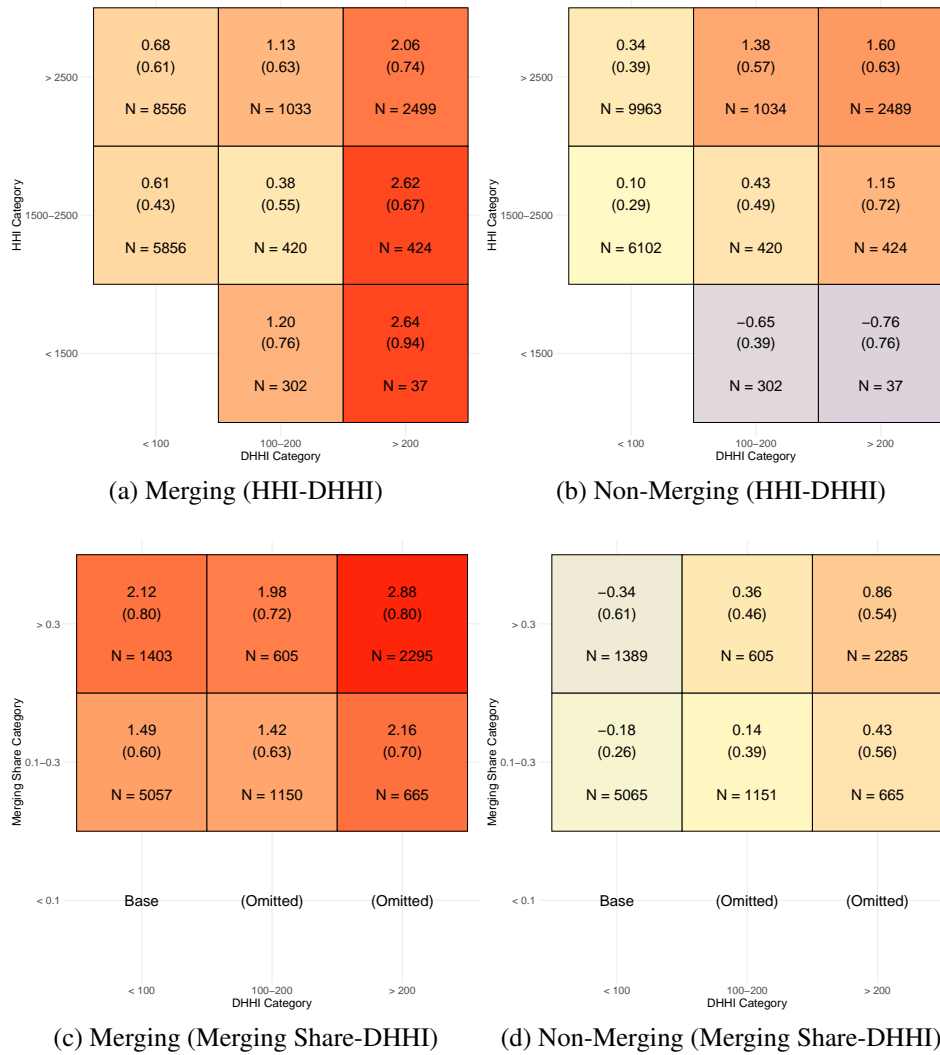


Figure C.7: Within-merger price changes for bins of DMA-level HHI and DHHI for mergers without divestitures. Each bin shows the coefficient of a regression of DMA-level price changes on bin dummies and merging party fixed effects. The omitted bin is the one with low HHI and low DHHI. Standard errors, clustered at the merger level, are in parentheses.  $N$  indicates the number of DMA-mergers in each bin.

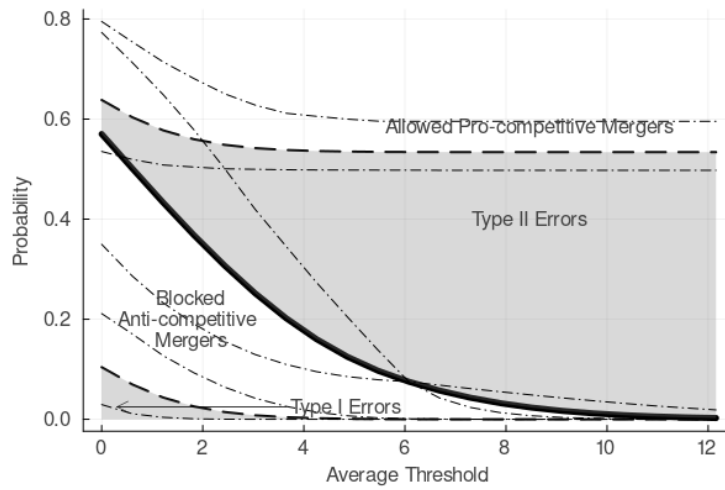
traced back to our estimates that agencies have a less precise signal of merging party price changes.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
HHI (0–1)	0.07 (0.12)					0.13 (0.13)				
DHHI (0–1)	2.42 (1.20)					2.10 (1.23)				
HHI ∈ [1500, 2500]		-0.02 (0.01)					-0.02 (0.02)			
HHI > 2500		0.05 (0.03)					0.05 (0.03)			
DHHI ∈ [100, 200]		-0.04 (0.02)					-0.06 (0.04)			
DHHI > 200		0.22 (0.09)					0.20 (0.10)			
Yellow			-0.03 (0.02)					-0.06 (0.03)		
Red			0.27 (0.10)					0.25 (0.10)		
Merg Share ∈ [0.10, 0.30]				0.10 (0.04)					0.06 (0.03)	
Merg Share > 0.30				0.18 (0.07)					0.15 (0.09)	
2023 Presumptions					0.16 (0.07)					0.13 (0.07)
Log(Annual Merging Sales)						0.02 (0.01)	0.02 (0.01)	0.02 (0.01)	0.02 (0.01)	0.02 (0.01)
Constant	0.01 (0.03)	0.01 (0.01)	0.03 (0.02)	0.00 (0.00)	0.03 (0.02)	-0.38 (0.24)	-0.32 (0.21)	-0.29 (0.22)	-0.26 (0.23)	-0.28 (0.22)
Observations	133	133	133	133	133	133	133	133	133	133
R <sup>2</sup>	0.114	0.156	0.159	0.068	0.072	0.137	0.173	0.175	0.077	0.087

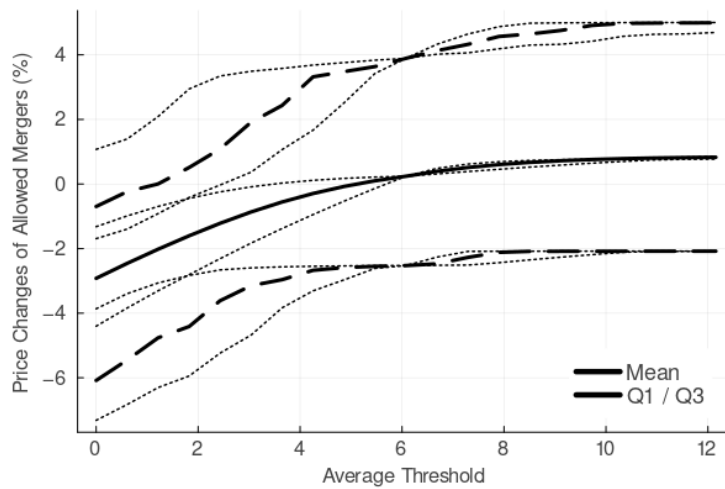
Table C.8: Correlates of Enforcement. Coefficients indicate output from a linear probability model of enforcement on merger-level market structure. HHI and DHHI as measured as the average across all DMAs within a merger. Robust standard errors are in parentheses.

## C.5. Comparison of Predicted Price Effects of Computed Thresholds

As mentioned in Section V.C, we compare our estimated thresholds to agencies’ predicted price effects to put them into context: thresholds lower than agency predictions provide evidence in favor of our model. Information about agency predictions is typically not publicly available, so we have to rely on the limited public material we can find. In particular, we go through all DOJ documents about merger cases going back to 2006 and look for trial exhibits (e.g., opening and closing statements, or expert slides), expert reports, or court opinions that may reference reports. Since this information is rare, we do not restrict to horizontal mergers in CPG. We collect all price predictions mentioned in any of these documents in



(a) Probability of challenges



(b) Price changes of consummated mergers

Figure C.8: Outcomes of counterfactual thresholds. This replicates Figure 10, but the dotted lines surrounding each line represent 95% confidence intervals. To avoid clutter, we omit the confidence intervals on unconditional price change (dashed red line in Figure 10(b)).

Table C.9.<sup>33</sup> When expert analysis is not publicly available, we have found situations where the agencies cite intended price increases from internal firm documents.

Our threshold depends on market size, proxied by the total annual sales of the

<sup>33</sup>In the closing argument for one of these mergers (Penguin Random House / Simon & Schuster), the DOJ cited predicted price effects from other cases in the same district, even those by the FTC.

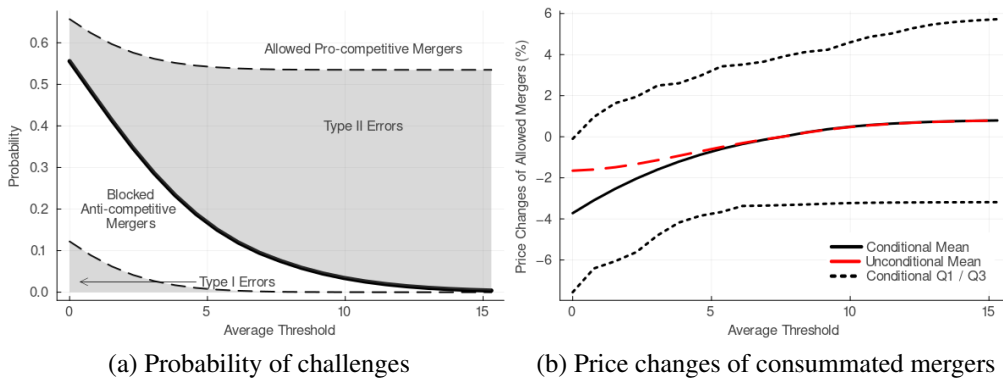


Figure C.9: Outcomes of counterfactual thresholds. This replicates Figure 10, but uses merging party price changes as the outcome variable of interest.

Merging Parties	Year	Market Size	Predicted Price Effects	Threshold (%)	
				Aggregate	Merging
H&R Block / TaxACT	2011	\$3B	2.2–2.5% (H&R); 10.5–12.2% (TaxACT)	4.91 (1.98)	5.67 (2.22)
GE / Electrolux	2015	\$2.7B	5–11% (ranges); 3–21% (cooktops); 4–15% (wall ovens), all for merging parties	4.97 (1.95)	5.77 (2.19)
Aetna / Humana	2015	—	60% premium increase (10–74%)	—	—
Sysco / US Foods	2015	\$56B	3–4% marketwide; 4.2–4.5% for merging parties	3.28 (2.94)	2.91 (3.51)
Energy Solutions / WCS	2017	\$100M	15% (from internal documents) for merging parties	6.80 (1.45)	8.88 (1.94)
AT&T / Time Warner <sup>a</sup>	2018	\$68B	27¢ per month (10–50¢) on a typical cable bill, for merging parties	3.17 (3.01)	2.73 (3.60)
		\$3.6B	16% in carriage fees charged by merging parties	4.81 (2.02)	5.50 (2.29)
Hackensack Meridian / Englewood	2021	—	5.7% for merging parties	—	—
US Sugar / Imperial	2022	\$1.2B	5.4–5.7% for merging parties, with coordinated effects; 1.9–2.1% for competitors	5.42 (1.74)	6.54 (1.96)
Penguin Random House / Simon & Schuster	2022	\$490M	4.3–11.6% (up to 7.3–19.2%) for merging parties	5.92 (1.57)	7.38 (1.82)
JetBlue / Spirit	2023	\$9.3B	30% (from internal documents) for merging parties	4.28 (2.32)	4.61 (2.66)

Table C.9: Predicted price effects of various mergers, compared to calculated thresholds using our baseline specifications (Columns (1) and (4) in Table 7). Market size refers to our best estimate of total sales of merging parties in the year prior to the merger. Standard errors are in parentheses. (a) AT&T / Time Warner is a vertical merger, and we report estimated effects on final cable consumers in the first row and effects on carriage fees in the second.

Merger	Merging Party Revenue	Price Effects
H&R Block / TaxACT	H&R revenue from tax services of \$2.9B ( <a href="#">2011 10-K</a> ); TaxACT revenues are \$78M for FY 2011, from <a href="#">company press release</a> .	DOJ <a href="#">closing argument</a> for Penguin (p. 39), citing 833 F. Supp. 2d 36, 87 (D.D.C. 2011)
GE / Electrolux	Appliance and lighting were 5.7% of total GE revenue ( <a href="#">10-K</a> ) of \$146B. Electrolux's sales of major appliances in North America was 32B Krona (exchange rate of 1 SEK = 0.15 USD in 2013), from <a href="#">2013 Annual Report</a> (p. 58). We take each appliance market to be 20% of total appliance sales.	Unilateral price changes from UPP with different demand functions, from <a href="#">Michael Whinston's testimony</a> (p. 59)
Aetna / Humana	—	Changes in rebate-adjusted premiums, from demonstrative used in <a href="#">Aviv Nevo's testimony</a> (p. 65)
Sysco / US Foods	Total broadline sales for Sysco are \$37.7B ( <a href="#">2014 10-K</a> , p. 97), and 87% of overall sales are in US (p. 98). Total US Foods revenue is \$23B ( <a href="#">10-K</a> ), all of which is assumed to be broadline.	DOJ <a href="#">closing argument</a> for Penguin (p. 39), citing 113 F. Supp. 3d 1, 66 (D.D.C. 2015)
Energy Solutions / WCS	Approximated from estimates of total market-wide disposal costs of nuclear waste ( <a href="#">DOJ opening statement</a> , p. 10) and HHI nearly 10,000 for merging parties ( <a href="#">DOJ closing statement</a> , p. 30)	Energy Solutions' internal model shows 15% price increases per year for two years, from <a href="#">DOJ closing statement</a> (p. 38)
AT&T / Time Warner	Time Warner's operating revenue was \$31.2B ( <a href="#">2017 10-K</a> , p. 52), and AT&T's video entertainment operating revenue was \$36.7B ( <a href="#">2017 10-K</a> , p. 135). Market size related to carriage fees are from scaling annual impact on carriage fees by percent impact (Figure 13 of <a href="#">expert report</a> ).	Cable price effects are from Figures 15 and 16 of <a href="#">Carl Shapiro's expert report</a> . Carriage fee increases are in Figure 13.
Hackensack / Englewood	—	DOJ <a href="#">closing argument</a> for Penguin (p. 39), citing 2021 WL 4145062, 22 n.26 (D.N.J. 2021)
US Sugar / Imperial	<a href="#">DOJ press release</a>	Price changes from a second-score model accounting for coordinated effects, from <a href="#">Dov Rothman's testimony</a> (p. 28)
Penguin / S&S	Total contracted in top-seller market is over \$1 billion (p. 20), about 49% of which is for merging parties (p. 22); both from DOJ <a href="#">closing argument</a>	DOJ <a href="#">closing argument</a> (pp. 38–39)
JetBlue / Spirit	DOJ <a href="#">closing argument</a> (p. 10)	JetBlue internal assumption on price effect of Spirit exit, from DOJ <a href="#">closing argument</a> (p. 49)

Table C.10: Details of data sources and justification for merging party revenue and price effects. The electronic version of this document has hyperlinks to the relevant data sources.

merging parties. In some of these cases, the agency documents themselves have estimates of total sales or revenues in the relevant markets (e.g., US Sugar / Imperial and JetBlue / Spirit), and in others we can infer it from other exhibits in agency documents (e.g., Energy Solutions / WCS, Penguin / Simon & Schuster, and total carriage revenues for AT&T / Time Warner). In all other cases, we use revenues from 10-K reports, restricting to the relevant product market. Table C.10 describes the source of the data for market size and also the source of the estimate of price effects.<sup>34</sup>

As mentioned in Section V.C, in most cases, our estimated thresholds are either lower than or comparable to the predicted price changes—even when the agency estimates a modest change. In H&R Block / TaxACT, the predicted effects for H&R Block are lower than our merging threshold (and the sales-weighted average of effects would be closer to the H&R Block effect). In Penguin / Simon & Schuster, the lower end of baseline price estimates (from a second-score model) are lower than our threshold, but they are within the range of predictions from a multi-round GUPPI model.<sup>35</sup> In US Sugar / Imperial, merging price effects are slightly lower than the threshold for merging parties, and competitors are expected to have even smaller price changes.<sup>36</sup> Perhaps the most salient exception is AT&T / Time Warner (when looking at cable bills), where estimated price effect is very small. Given the different setting (different industry, much larger size, and a vertical structure), we hesitate to extrapolate our results to this merger.

Of course, these results must be interpreted with some caution. The industries are very different from the ones under consideration in our setting, and we have no reason to believe that agencies would use the same decision rule across industries.

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<sup>34</sup>We do not have market sizes for two mergers. In Aetna / Humana, the market of interest was Medicare Advantage plans in a specific set of counties, and we do not have easy access to the data needed to compute sales in those markets. Any reasonable threshold would be below the predicted price effect of 60% here. Hackensack Meridian / Englewood was a hospital merger, and the point of contention was negotiations with insurance companies. We are not sure what the appropriate definition of sales would be here.

<sup>35</sup>This is also a unique case in that the harm was alleged to authors (input suppliers), and the DOJ chose to restrict to a narrow market of top-selling authors.

<sup>36</sup>We should note that the expert report cites merging party price increases of 3.3–3.6% when ignoring coordinated effects, but the existence of these effects (with American Crystal Sugar, a non-merging party) was part of the DOJ's argument.

The predictions cited usually do not take into account synergies, and while the position of the agency in many of these cases is that there are no cognizable merger-specific efficiencies, it may still be the case that the agencies expect price changes lower than the predictions. Even if so, in many cases, expected synergies would have to be quite large for some of the price effects to drop below our thresholds. These mergers are also taken to trial, so they may be especially far past the “enforcement margin”: for comparison, all challenges in our setting lead to divestitures, and none of our cases are taken to course. Fourth, considerations such as precedent-setting are surely important in some of these mergers. Finally, from a statistical standpoint, our estimates of the threshold are noisy. Despite these limitations, this analysis provides evidence that the estimates from our model are not at odds with the historical behavior of the agencies.

## **D. Details on Sample Construction and Market Definition**

### **D.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.<sup>37</sup>

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

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<sup>37</sup>Twenty of the 129 mergers in our final sample occur within two years of the completion of another merger in the same module. We chose to keep these mergers in our final sample to improve the representativeness of the sample. We checked that dropping these mergers from our sample has limited quantitative impacts on the estimates distributions of effects or on their correlation with the structural presumptions.

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 91.2%, and the average value of the median coverage is 95.6%. This reassures us that we are capturing the relevant products in each product market.

## **D.2. Market Definition**

Table D.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 D.2 through D.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.<sup>38</sup> We restrict our attention to horizontal mergers for goods that may have been in our NielsenIQ sample.

Our interpretation of these markets is that they are quite similar to markets that we have defined using combinations of NielsenIQ 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

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<sup>38</sup>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.

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 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.<sup>39</sup> However, both departures are more the exception than the rule. We find one instance where non-branded products are excluded entirely (the 2000 merger on butter reviewed by the DOJ), one in which only private labels are considered (the 2019 RTE cereal mergers, although this is because one company only manufactured private label cereals), and one where both the entire market and the branded products are listed as markets (a 2018 merger reviewed by the FTC). 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.<sup>40</sup> At a practical level, separating products into tiers is especially difficult since subdividing NielsenIQ 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.

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<sup>39</sup>We also see one case—refrigerated pickles—where the stated market definition would be narrower than a NielsenIQ 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>.

<sup>40</sup>See <https://www.justice.gov/atr/case-document/file/1331221/download> for the competitive impact statement for one beer merger that has such a discussion.

Market	NielsenIQ Group	Product	NielsenIQ Product Modules in Product Market	Cost Controls
1	Baby Food		Baby Milk And Milk Flavoring	Dry Milk
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
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	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, And Sauces	Gravies,	Cooking Sauce	Sugar, Tomatoes, Corn
22	Condiments, And Sauces	Gravies,	Fish & Seafood & Cocktail Sauce	Tomatoes, Mayonnaise And Dressing, Shrimp, Unprocessed Finfish, Pickles And Horseradish
23	Condiments, And Sauces	Gravies,	Meat Sauce, Worcestershire Sauce	Beef And Veal, Tomatoes, Vinegar
24	Condiments, And Sauces	Gravies,	Mustard	Vinegar, Salt Pepper Spices
25	Condiments, And Sauces	Gravies,	Sauce & Seasoning Mix-Remaining	Salt Pepper Spices, Spices, Vinegar, Dry Onions, Tomatoes
26	Condiments, And Sauces	Gravies,	Sauce Mix - Spaghetti	Salt Pepper Spices, Tomatoes
27	Condiments, And Sauces	Gravies,	Sauce Mix - Taco, Sauce & Seasoning Mix-Remaining Mexican	Salt Pepper Spices, Spices, Vinegar, Dry Onions, Tomatoes
28	Condiments, And Sauces	Gravies,	Seasoning Mix - Chili	Salt Pepper Spices, Tomatoes, Spices, Vinegar, Dry Onions
29	Condiments, And Sauces	Gravies,	Seasoning Mix - Sloppy Joe	Salt Pepper Spices, Tomatoes

Market	NielsenIQ Group	Product	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	Pigments
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	Starch Vegetable Fats Oils
39	Cosmetics		Cosmetics-Mascara	Fatty Acids, Starch Vegetable Fats Oils
40	Cosmetics		Cosmetics-Remaining	
41	Cosmetics		Talcum & Dusting Powder	Corn Starch
42	Detergents		Detergents-Packaged, Detergents - Light Duty, Detergents - Heavy Duty - Liquid	Surfactants
43	Fragrances - Women		Cologne & Perfume-Women's	Ethanol, Coal
44	Grooming Aids		Cosmetic And Nail Grooming Accessory	Stainless Steel, Aluminum, Plastic
45	Grooming Aids		Cosmetics - Noncotton Aplctrs/Puffs/Etc.	Plastic, Polyester
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
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
51	Hair Care		Wave Setting Products	Ethanol, Basic Organic Compounds
52	Kitchen Gadgets		Beverage Storage Container	Stainless Steel
53	Laundry Supplies		Detergent Boosters	Surfactants
54	Liquor		Alcoholic Cocktails	Barley, Wheat, Corn
55	Liquor		Bourbon-Straight/Bonded, Bourbon-Blended, Canadian Whiskey, Irish Whiskey, Remaining Whiskey, Scotch	Barley, Wheat, Corn
56	Liquor		Cordials & Proprietary Liqueurs	Barley, Wheat, Corn
57	Liquor		Gin	Barley, Wheat
58	Liquor		Rum	Sugar
59	Liquor		Tequila	Sugar
60	Liquor		Vodka	Wheat, Russet Potatoes
61	Medications/Remedies/Health Aids		Foot Preparations-Athlete's Foot	Basic Organic Compounds
62	Men's Toiletries		Cologne/Lotion-Men's	Ethanol, Coal, Soybeans, Other Grains
63	Packaged Meats-Deli		Bacon-Refrigerated	Slaughter Hogs
64	Packaged Meats-Deli		Bratwurst & Knockwurst, Sausage-Dinner, Frankfurters-Refrigerated	Slaughter Hogs, Slaughter Cattle, Slaughter Poultry

Market	NielsenIQ Group	Product	NielsenIQ Product Modules in Product Market	Cost Controls
65	Packaged Meats-Deli		Lunchmeat-Deli Pouches-Refrigerated	Slaughter Cattle, Slaughter Poultry, Slaughter Hogs
66	Packaged Meats-Deli		Lunchmeat-Nonsliced-Refrigerated	Slaughter Cattle, Slaughter Poultry, Slaughter Hogs, Beef And Veal
67	Packaged Meats-Deli		Lunchmeat-Sliced-Refrigerated	Slaughter Cattle, Slaughter Poultry, Slaughter Hogs, Beef And Veal
68	Packaged Meats-Deli		Sausage-Breakfast	Slaughter Hogs
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 Doeuvres-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	Dry Pinto Beans
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	Corn
86	Snacks		Snacks - Health Bars & Sticks	Corn Starch, Sugar
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 Group	Product	NielsenIQ Product Modules in Product Market	Cost Controls
92	Spices, Seasoning, Ex-tracts		Pepper	Spices, Salt Pepper Spices
93	Spices, Seasoning, Ex-tracts		Salt - Cooking/Edible/Seasoned	
94	Spices, Seasoning, Ex-tracts		Seasoning-Dry	Spices, Salt Pepper Spices
95	Spices, Seasoning, Ex-tracts		Vegetables - Onions - Instant	Dry Onions
96	Stationery, School Sup-plies		Dry Erase Bulletin Board And Accesory	Stainless Steel
97	Stationery, School Sup-plies		Personal Planners Binders And Folders	
98	Tobacco & Accessories		Cigarettes	Tobacco, Pulp Paper
99	Unprep Meat/Poultry/Seafood-Frzn		Frozen Poultry	Poultry Processing, Processed Foods And Feeds
100	Vegetables - Canned		Mushrooms - Shelf Stable	Vinegar
101	Vegetables - Canned		Vegetables-Mixed-Canned	Carrots, Vinegar, Beans City Average
102	Vegetables And Grains - Dried		Rice - Instant	
103	Wine		Wine-Domestic Dry Table, Wine-Imported Dry Table	Wine Grapes, US/AUD Con- version, US/Euro Conversion

Table D.1: Product Market Definitions and Cost Controls

Year	Agency	Markets
2021	DOJ	Feta cheese Ricotta cheese
2020	DOJ	Fluid milk
2011	DOJ	Fluid milk
1990	DOJ	Fluid milk
2017	DOJ	Fluid organic milk
2000	DOJ	Branded whipped butter Branded stick butter
2019	FTC	Private label RTE cereal
1996	FTC	All RTE cereal
2011	DOJ	Sliced bread
1999	DOJ	Plain white bread
2021	DOJ	Refined sugar
2018	FTC	Canola and vegetable oils Branded canola and vegetable oils
2004	DOJ	Mainstream sardine snacks
2003	FTC	Superpremium ice cream
2002	FTC	Refrigerated pickles
2001	FTC	Dry cat food
2000	FTC	Prepared baby food, including jarred
2000	FTC	Dry-mix gelatin Dry-mix pudding No-bake desserts Baking powder Intense mints

Table D.2: Market definitions for assorted food products. Each listed year corresponds to one deal, and separate product markets are listed in each row. Merging parties cannot be identified due to our data agreement.

Year	Agency	Markets
2020	FTC	Wet shave system razors Disposable razors
2011	DOJ	Value shampoo Value conditioner Hairspray
1989	DOJ	Wet shaving razor blades
1989	DOJ	Single-edge razor blades
2005	FTC	At-home teeth whitening products Adult battery-powered toothbrushes Rechargeable toothbrushes Men's antiperspirant
2000	DOJ	Adult women's hair relaxer kits
1995	DOJ	Facial tissues Baby wipes
2020	FTC	Oral canine flea medication
2006	FTC	OTC H-2 blockers OTC hydrocortisone anti-itch products OTC nighttime sleep aids OTC diaper rash treatments
1990	DOJ	OTC stomach remedies
2007	FTC	Monofilament fishing line
1999	FTC	Hard surface bathroom cleaners Fine fabric wash products
1999	FTC	Water-based floor care polymers
1998	FTC	Soil and stain removers Glass cleaners

Table D.3: Market definitions for assorted cosmetic products, medicine, and household products. Each listed year corresponds to one deal, and separate product markets are listed in each row. Merging parties cannot be identified due to our data agreement.

Year	Agency	Markets
2020	FTC	Closed-system e-cigarettes
2015	FTC	Combustible cigarettes
1981	DOJ	Cigars
2021	FTC	Low-priced sparkling wine Low-priced brandy Low-priced port and sherry
2020	DOJ	Beer
2016	DOJ	Beer
2008	DOJ	Beer
1982	DOJ	Beer

Table D.4: Market definitions for tobacco and alcohol products. Each listed year corresponds to one deal, and separate product markets are listed in each row. Merging parties cannot be identified due to our data agreement.