

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 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. Second, this dataset specifies the DMA in which each store resides. We believe DMAs are more appropriate geographic market definitions than Scantrack markets, the geographic units at which the Consumer Panel dataset is representative.

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 lower when we use the panelist data than in the baseline. While the 25th percentile and median also decline, the most noticeable change is in the 75th percentile. Even so, we do not believe these differences change the economic interpretation of the baseline results. There is statistical error in these estimates, and, in all cases, average price effects are small (1–2 pp away from zero), and there is significant dispersion.

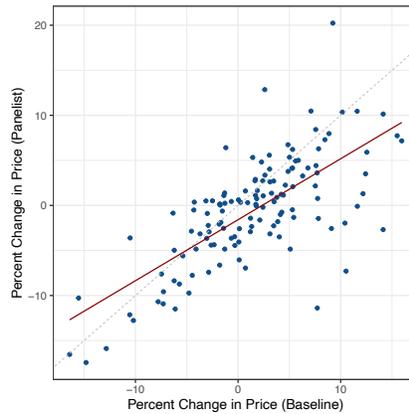
We also analyze robustness of the point estimates for each merger. Figure A.1 plots the estimated price effects in the baseline specification against the estimated effects using the Consumer Panel dataset. Panel (a) displays results for aggregate price changes, Panel (b) displays results for merging parties, and Panel (c) displays results for non-merging parties. The price effects are positively correlated and generally close to the 45° line. The main departure is that for mergers with particularly large price effects using the scanner dataset, the panelist dataset yields smaller effects.

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	130	1.51 (0.55)	6.29	-2.34 (0.58)	1.74 (0.59)	5.31 (0.57)
Merging Parties	130	0.03 (0.74)	8.47	-5.15 (0.97)	0.77 (0.97)	5.86 (0.85)
Non-Merging Parties	130	2.07 (0.62)	7.11	-2.20 (0.62)	1.93 (0.58)	6.12 (0.87)
B. Panelist Data						
Overall	130	-0.57 (0.53)	6.00	-3.61 (0.66)	0.04 (0.46)	2.89 (0.82)
Merging Parties	130	-0.86 (0.72)	8.23	-4.42 (0.62)	-0.40 (0.78)	4.17 (0.70)
Non-Merging Parties	130	-0.30 (0.56)	6.43	-3.56 (0.67)	0.09 (0.44)	3.33 (0.84)

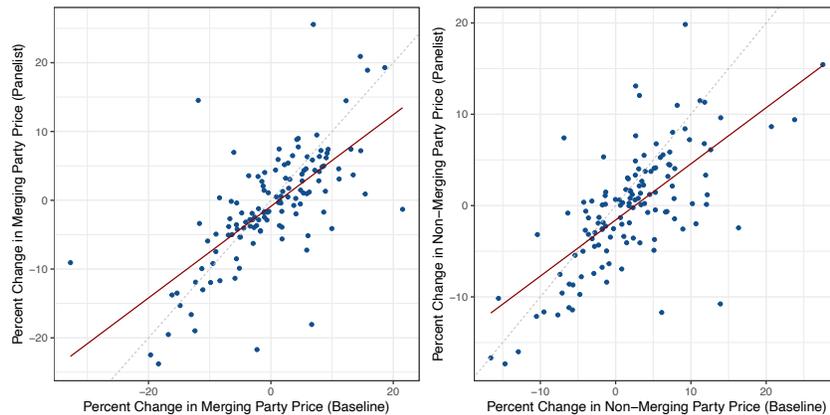
Table A.1: Price Effects using Consumer Panel Data. This table displays the distribution of transformed coefficient estimates of (2) (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for overall, merging-party, and non-merging-party price changes. Standard errors are in parentheses. Panel A displays the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

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 = 0.49, s.e. = 0.08), 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. As in the baseline specifications, we do not find evidence of pre-trends in average prices in the months leading up to the merger. Furthermore, for the mergers with the largest price increases, we again find that merging parties begin increasing prices upon the completion of the merger and undergo a further increase approximately one year after the merger. For those with the largest price decreases, merging parties begin lower prices immediately after the merger's completion.



(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.

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 25th percentile of quantity changes for aggregate, merging-, and non-merging parties does not change noticeably when moving to the panelist dataset, suggesting a similar share of mergers with large quantity declines. Furthermore, the dispersion of quantity effects are similar in aggregate and for non-merging parties.

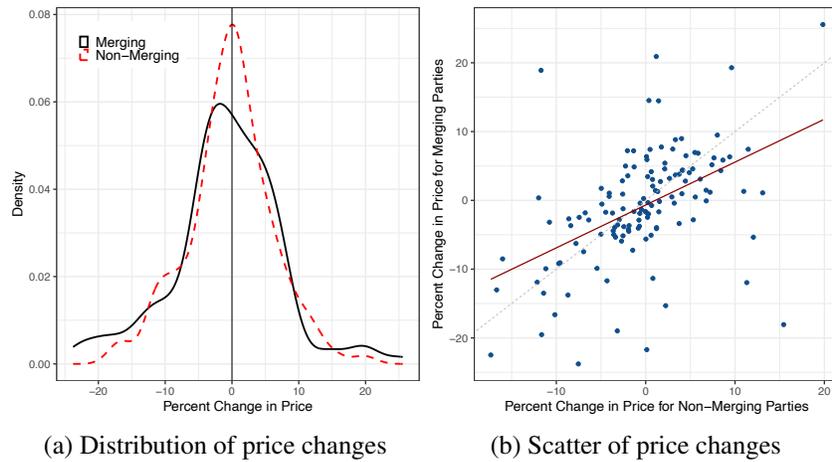


Figure A.2: Price changes for merging and non-merging parties, as estimated by (2) using the NielsenIQ Consumer Panel Data. These plots display transformed coefficient estimates (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for the price change of the merging and non-merging parties. We use a balanced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

There are, however, a few differences to note. First, the dispersion of quantity changes is notably larger for merging parties when using the panelist dataset. Second, the median and 75th percentile of the quantity effect distribution are larger. 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. Moreover, as discussed in Footnote 16, the largest estimates of quantity changes are especially noisy with the panelist data, and Bayesian shrinkage lowers the mean effect for merging parties by 1.77 pp.

As with the price effects, we are also interested in robustness of the point estimates for each merger. 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, it is important to note that there are a number of cases in which the estimated effect using the panelist data differs substantially from the corresponding effect in the baseline. As discussed above, because the

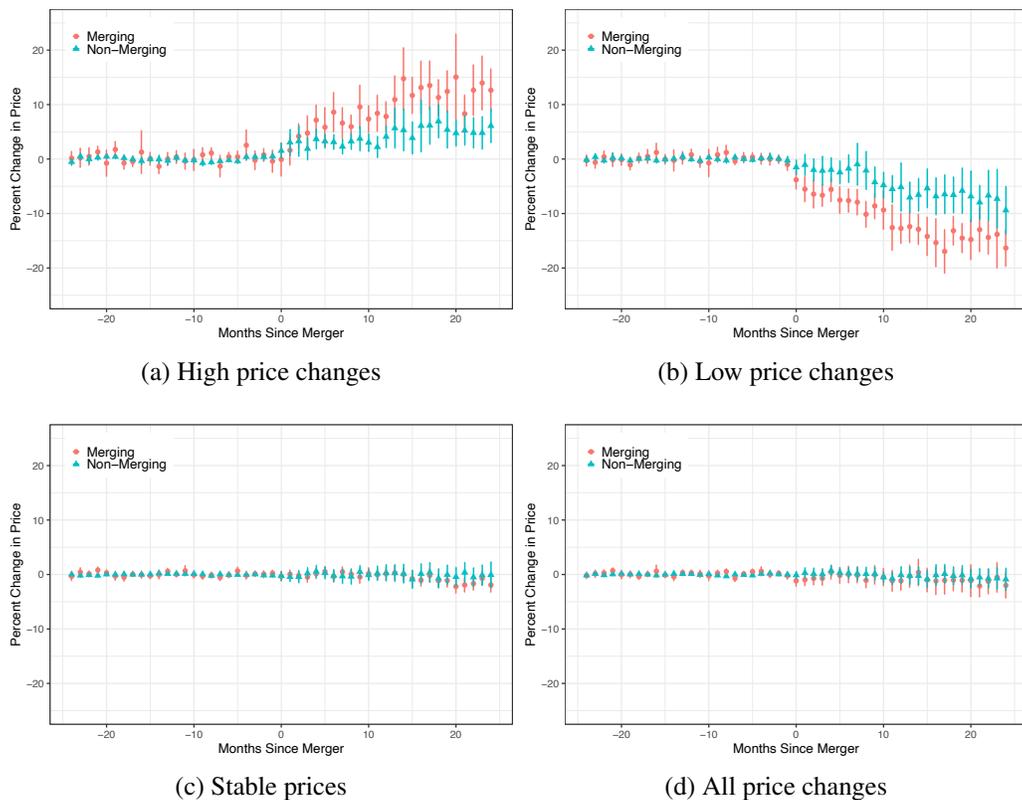


Figure A.3: Timing of price changes, for merging parties (red circle) and non-merging parties (blue triangle) using the NielsenIQ Consumer Panel Data. The marker indicates the mean price change a given number of months after the merger becomes effective, and the thick line is the 95% confidence interval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

Consumer Panel dataset has incomplete coverage of UPCs, we hesitate to draw strong conclusions about the source of these differences.

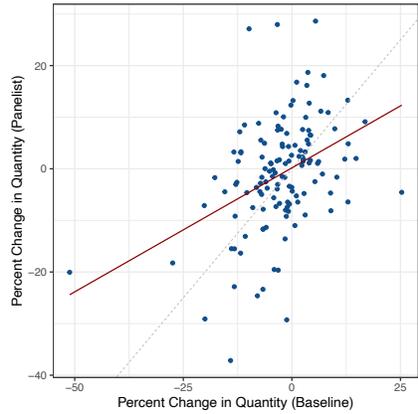
Figure A.5 replicates Figure 4 from the main text, displaying the distribution of quantity changes (Panel (a)) and a scatter of merging versus non-merging quantity changes (Panel (b)). The takeaways from Panel (a) are identical to those discussed above: merging party quantity changes exhibit more dispersion, driven by larger effects in the right tail of the distribution. In Panel (b), we do not find a statistically significant correlation between merging and non-merging quantity changes. Again, this is inconsistent with the results for demand systems with “type aggregation,”

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	130	-2.46 (0.79)	9.02	-6.87 (0.66)	-1.93 (0.74)	2.80 (0.70)
Merging Parties	130	-7.07 (2.40)	27.42	-20.96 (3.70)	-5.61 (1.95)	5.71 (1.93)
Non-Merging Parties	130	-1.45 (0.88)	10.04	-6.37 (0.72)	-1.86 (0.86)	4.09 (1.05)
B. Panelist Data						
Overall	130	-1.00 (0.94)	10.76	-6.45 (1.13)	-0.17 (0.95)	4.85 (1.19)
Merging Parties	130	-1.68 (3.43)	39.16	-22.25 (2.58)	-4.17 (2.53)	12.59 (4.80)
Non-Merging Parties	130	-0.83 (1.05)	11.92	-7.11 (1.12)	0.07 (0.96)	6.82 (1.23)

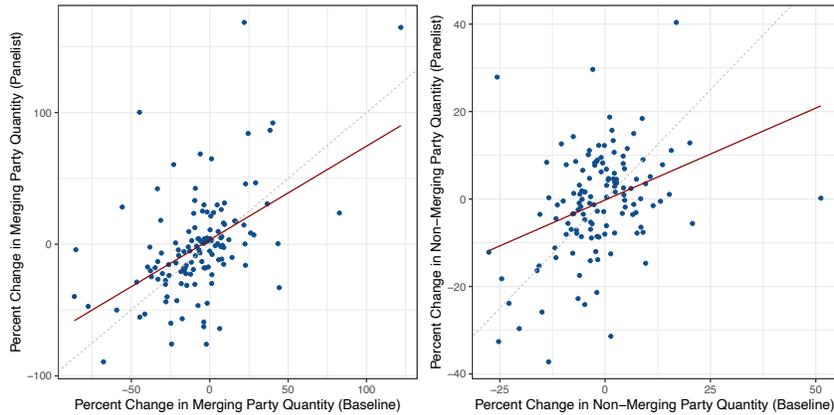
Table A.2: Quantity Effects using Consumer Panel Data. This table displays the distribution of transformed coefficient estimates of (2) (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for overall, merging-party, and non-merging-party quantity changes. Standard errors are in parentheses. Panel A displays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

which would predict a negative correlation. However, as discussed in the main text, we are not aware of such results for general demand systems.

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 negative and statistically significant with p -values of at least 0.10. The correlation for merging parties is -0.17 (s.e. 0.09) and for non-merging parties is -0.15 (s.e. 0.09). Despite these negative correlations, as in the main text, it is clear that prices 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) 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, assuming equal weights across mergers.

A.3. Connection to the Merger Guidelines

While the estimates of price effects using the panelist dataset look broadly similar to those in the baseline, one might be concerned that the departures in the right tail of the distribution might be economically meaningful for correlations with market structure. In Table A.3, we repeat the analysis of Table 5 in the main text. Results look broadly similar; however, we want to note a few differences. First, we find a significant relation between DHHI and price changes for all cases (Columns (1), (4), and (7)), but the magnitudes for aggregate and non-merging changes are about

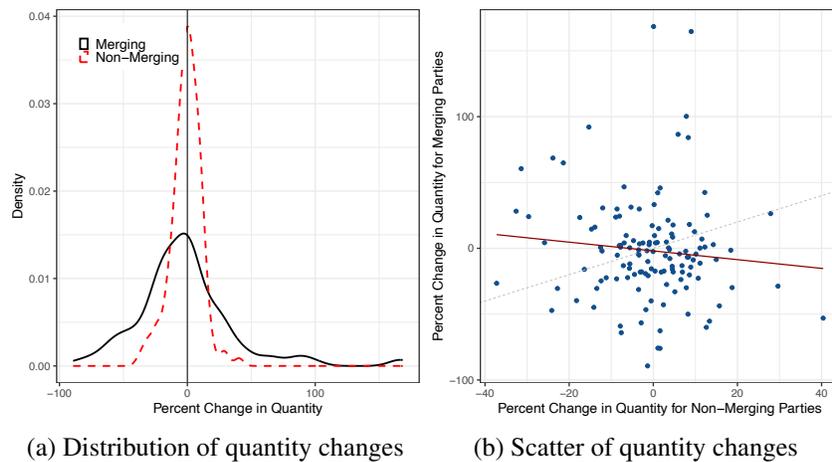


Figure A.5: Quantity changes for merging and non-merging parties, as estimated by (2), using the NielsenIQ Consumer Panel Data. This plot displays transformed coefficient estimates (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for the quantity change of the merging and non-merging parties. We use a balanced panel of stores and weigh regressions using pre-merger volume by firm type-Scantrack. The distributions in Panel (a) and best-fit line in Panel (b) assume equal weights across mergers.

one-third lower than in the baseline estimates. Similarly, the effect of high DHHI for non-merging and aggregate parties (Columns (5) and (8)) is also still significant but about 20% lower in magnitude. Second, we see a significant and large correlation between the yellow zone on price changes for non-merging parties, but still not for mergers in the red zone. Finally, the relation between HHI and price changes is muted for merging and non-merging parties. Despite these differences, we believe the takeaways from the body are robust.

Figure A.7 repeats the within-merger analysis of Figure 7. Unlike in our baseline specification, we do not find any interpretable patterns in this analysis. Note that given the structure of the panelist data, we have to define the geography as the Scantrack market: we replace DMAs in (5) with Scantracks and compute HHI and DHHI at the Scantrack level. NielsenIQ has fewer Scantracks than DMAs, and they are typically significantly larger than DMAs. We thus do not believe they comprise reasonable geographic definitions of markets, but we nevertheless report these results here so that readers can interpret the within-merger patterns in our baseline with a degree of caution.

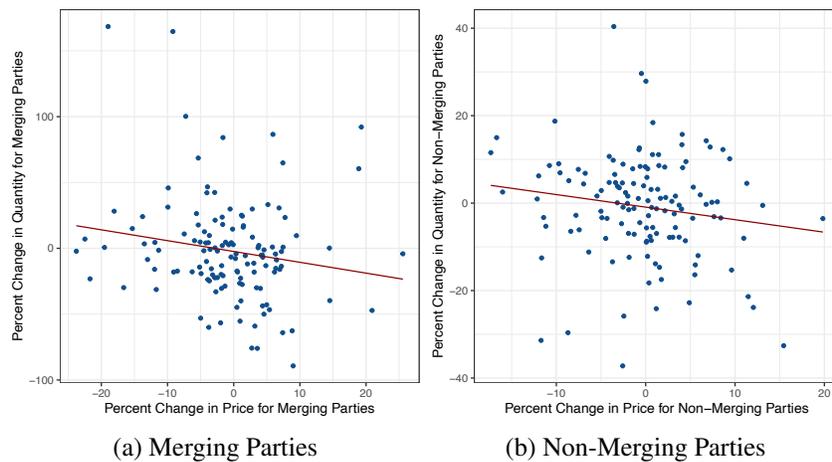


Figure A.6: Scatter of price versus quantity changes for merging and non-merging parties using the NielsenIQ Consumer Panel Data. Panel (a) displays a scatter plot of price changes versus quantity changes for merging parties. Each blue point represents a merger, the red line is the estimated best fit, assuming equal weights across mergers. Panel (b) presents the same scatter plot, but for non-merging parties. In both panels, we use a balanced panel of stores and weigh price regressions using pre-merger volume by brand-Scantrack and quantity regressions using pre-merger volume by firm type-Scantrack.

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. The dependence of the prior (Panel A) on market structures is very similar to that in the estimates with the scanner data: the coefficient of the red zone on merging party price changes is now significant as well. The posterior standard deviations (in Panel B) are similar to the baseline as well, and we still find that agencies have noisier signals about price changes for merging parties than they do about aggregate price changes. In Panel C, we see that we estimate a slightly smaller dependence of the threshold on market size, by about 15–20%. (In Columns (2) and (3), the coefficient has a p -value of 0.12.) Perhaps the most significant difference is in Panel D: the estimates thresholds are about 3 pp smaller than in the baseline. This can partly be traced back to the distribution of price effects—especially those at the right tail—being shifted left relative to the scanner data.

Table A.5 adds measures of agency funding and workload (proxied by Hart-

	Merging			Non-Merging			Aggregate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
HHI (0–1)	-15.83 (5.77)			1.65 (5.60)			-3.15 (4.71)		
DHHI (0–1)	30.44 (17.59)			46.97 (14.00)			33.80 (14.10)		
HHI ∈ [1500, 2500]		2.04 (2.55)			-1.60 (2.52)			-1.11 (2.55)	
HHI > 2500		-2.22 (2.35)			-1.99 (2.45)			-2.10 (2.45)	
DHHI ∈ [100, 200]		3.54 (1.53)			1.72 (1.38)			2.31 (1.20)	
DHHI > 200		3.46 (1.64)			4.70 (1.34)			3.73 (1.25)	
Yellow			2.85 (1.40)			1.97 (1.34)			2.40 (1.18)
Red			1.45 (1.56)			4.21 (1.25)			2.94 (1.16)
Constant	3.73 (1.92)	-0.94 (2.04)	-1.43 (0.94)	-1.49 (1.70)	0.60 (2.27)	-1.14 (0.70)	-0.04 (1.51)	0.32 (2.30)	-1.29 (0.67)
<i>N</i>	130	130	130	130	130	130	130	130	130

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)–(3) use merging party price changes, Columns (4)–(6) use non-merging party price changes, and Columns (7)–(9) use aggregate price changes. Each observation is a merger. Robust standard errors are in parentheses.

Scott-Rodino requests). Appendix B.3 discusses the data construction in more detail and provides further context for the motivation behind this analysis. Here, we simply note that using the panelist data, we find a significant negative impact agency funding on the threshold: increasing agency funding by \$10 million (about 20% of the range across years) reduces the threshold by 0.9–1.0 pp. These numbers are slightly larger than estimates with the scanner dataset (Table B.7) and estimates more precisely, but qualitatively similar. The effect of workload on thresholds is noisily estimated.

Finally, Figure A.8 shows results for the analysis of counterfactual merger thresholds. A more restrictive threshold leads to slightly higher probabilities of type I errors than in the baseline and a slightly lower increase in the probability of challenges. Reducing to 5% (very close to the estimated baseline here) increases the probability of challenges by 17% and has no appreciable impact on the probability of a type I error. Reducing to 2.5% approximately triples the probability of challenges,

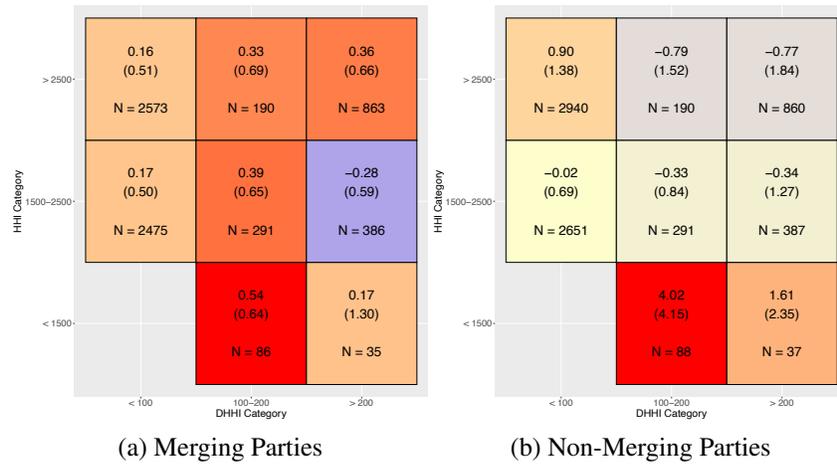


Figure A.7: Within-merger price changes for bins of Scantrack-level HHI and DHHI using the NielsenIQ Consumer Panel Data. Each bin shows the coefficient of a regression of Scantrack-level price changes on bin dummies and merging party fixed effects. The omitted bin is the one with low HHI and low DHHI.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

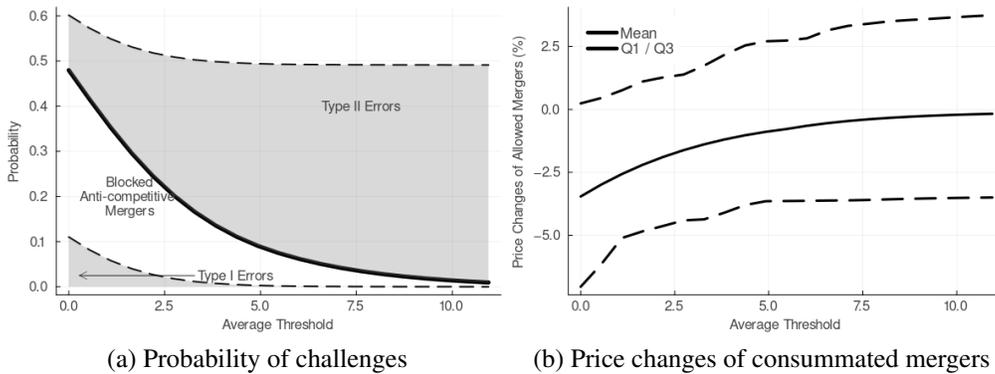


Figure A.8: Outcomes of counterfactual thresholds. This replicates Figure 9, but using estimates from the panelist data.

and about 10% of these are due to a type I error. Finally, reducing to 0% increases the probability of challenges to about 50%, and 23% of these are due to type I errors. We still find scope for modest increases in stringency without substantially changing the probability of adverse effects from type I errors.

	Aggregate Price Changes			Merging Party Price Changes		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Prior						
Avg HHI (0–1)	-2.45 (4.05)			-10.55 (5.58)		
Avg DHHI (0–1)	55.29 (18.44)			70.13 (21.49)		
HHI ∈ [1500, 2500]		-1.30 (2.65)			0.95 (3.62)	
HHI > 2500		-1.44 (2.61)			-1.00 (3.56)	
DHHI ∈ [100, 200]		2.96 (1.55)			4.12 (2.10)	
DHHI > 200		5.48 (1.58)			6.36 (1.98)	
Yellow			2.99 (1.48)			3.41 (2.03)
Red			5.21 (1.51)			5.52 (1.95)
Constant	-0.32 (1.35)	-0.05 (2.50)	-1.37 (0.58)	1.83 (1.88)	-1.51 (3.41)	-1.76 (0.79)
B. Errors and Uncertainty						
σ_{p^*}	5.51 (0.42)	5.50 (0.43)	5.49 (0.43)	7.64 (0.55)	7.70 (0.56)	7.69 (0.58)
σ_{ϵ}	4.58 (2.93)	3.54 (2.42)	4.03 (2.61)	7.96 (3.77)	6.80 (2.96)	8.38 (5.28)
Posterior Standard Deviation	3.52 (1.30)	2.98 (1.42)	3.25 (1.34)	5.51 (1.23)	5.10 (1.22)	5.67 (1.57)
C. Threshold						
Log(Total Merging Sales)	-0.95 (0.51)	-0.81 (0.51)	-0.79 (0.51)	-1.30 (0.66)	-1.16 (0.66)	-0.99 (0.71)
Constant	7.04 (2.34)	7.63 (2.10)	7.27 (2.17)	8.61 (2.69)	9.16 (2.41)	8.16 (3.41)
D. Sales-Weighted Thresholds						
Average	5.47 (2.06)	6.30 (1.84)	5.96 (1.86)	6.45 (2.21)	7.25 (1.97)	6.52 (2.72)
Q1	4.54 (2.10)	5.51 (1.91)	5.19 (1.89)	5.18 (2.22)	6.11 (2.03)	5.55 (2.60)
Q3	6.15 (2.24)	6.88 (1.99)	6.53 (2.03)	7.38 (2.48)	8.07 (2.19)	7.23 (3.06)

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

	Aggregate Price Changes				Merging Party Price Changes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Prior								
Avg HHI (0–1)	-2.20 (4.06)	-3.11 (4.09)	-1.69 (4.07)	-2.15 (4.12)	-0.06 (3.96)	-0.97 (4.02)	0.26 (4.06)	-0.11 (4.09)
Avg DHHI (0–1)	54.46 (17.47)	60.00 (17.47)	50.34 (17.94)	54.48 (18.85)	50.17 (18.98)	54.50 (20.16)	49.41 (18.02)	52.46 (19.04)
Constant	-0.41 (1.35)	-0.10 (1.36)	-0.57 (1.35)	-0.41 (1.37)	-1.08 (1.35)	-0.77 (1.37)	-1.18 (1.38)	-1.05 (1.39)
B. Errors and Uncertainty								
σ_{p^*}	5.48 (0.42)	5.57 (0.44)	5.45 (0.41)	5.50 (0.43)	5.74 (0.42)	5.81 (0.44)	5.73 (0.41)	5.77 (0.43)
σ_{ϵ}	4.82 (2.84)	3.84 (2.63)	5.33 (3.10)	4.71 (3.22)	6.24 (3.64)	5.65 (3.62)	5.65 (3.23)	5.36 (3.50)
Posterior Standard Deviation	3.62 (1.18)	3.16 (1.44)	3.81 (1.11)	3.58 (1.38)	4.22 (1.09)	4.05 (1.28)	4.02 (1.14)	3.93 (1.34)
C. Threshold								
Total Funding (\$B, Deflated)	-87.82 (52.69)		-87.65 (53.62)		-98.43 (54.92)		-100.03 (57.51)	
HSR Transactions Reported (Thousands)		0.41 (2.11)		1.21 (2.23)		0.79 (1.97)		1.69 (2.27)
Log(Total Merging Sales)			-0.91 (0.53)	-0.98 (0.52)			-1.04 (0.59)	-1.12 (0.58)
Constant	6.16 (1.97)	6.91 (2.03)	6.43 (2.28)	6.94 (2.49)	5.80 (2.28)	6.29 (2.54)	6.75 (2.42)	7.07 (2.68)
D. Sales-Weighted Thresholds								
Average	6.11 (1.96)	6.89 (2.02)	4.88 (1.88)	5.26 (2.18)	5.75 (2.27)	6.25 (2.54)	4.97 (1.87)	5.13 (2.15)
Q1	5.13 (1.89)	6.78 (1.98)	3.76 (1.76)	4.43 (2.16)	4.66 (2.07)	6.02 (2.51)	3.70 (1.68)	4.24 (2.04)
Q3	6.42 (1.97)	7.04 (2.11)	5.86 (2.12)	6.03 (2.42)	6.10 (2.33)	6.52 (2.60)	6.10 (2.18)	6.12 (2.49)

Table A.5: Parameter estimates, using aggregate price changes in Columns (1)–(4) and merging party price changes in Columns (5)–(8). Standard errors are in parentheses. All quantities used in the threshold are demeaned.

B. Additional Tables and Figures

B.1. Additional Results Related to Price Effects

Table B.1 performs additional robustness tests on the distribution of estimated price effects documented in Section III.B. Panels A reproduces the estimates from the baseline specification in Panel A of Table 5 for convenience. Panel B reproduces the results from the baseline specification using the Consumer Panel; these results were discussed in Appendix A.1.

Panel C 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. The mean price effects are close to the ones from the panelist data but the distributions are slightly wider. Comparing to the baseline, the highest estimated price increases come from non-food mergers.

Panel D reports results obtained using all stores in the Retail Scanner dataset, not just those that are in the sample throughout the entirety of the merger’s analysis window. The distribution of price effects are almost identical to those used in the baseline (with the balanced panel of stores). Panel E weighs regressions using equal weights for all UPCs rather than the pre-merger volume. Comparing to Panel A, we find that on aggregate and for non-merging parties, equal weighting lowers the right tail of price effects, leading to estimates of the mean price change about 1 pp lower. For merging parties, equal weighting leads to slight contraction of both tails: the mean is almost the same, and the estimated standard deviation is 1 pp lower.

Panel F reports results of a specification where prices are computed as the sales-weighted average at the brand-DMA-month level. This specification is designed to address potential churn in UPCs the dataset. While we still find that average price effects are near zero and that merging parties have lower price changes than non-merging ones, we find a negative shift in price effects across the distribution.

Panel G reports the distribution of merger effects obtained using Bayesian shrinkage. We implement a “random effects” model where the price effect of merger i is $p_i \sim N(\mu, \sigma^2)$, and our estimate is $\hat{p}_i \sim N(p_i, \sigma_i^2)$, where σ_i is our estimated second-stage standard error (taking into account estimation error from the first stage).

We implement this procedure using the `brms` package of Bürkner (2017), which can provide posterior distributions on each p_i through a Hamiltonian Monte Carlo procedure.²⁷ 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 0.4–0.5 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 B.1 illustrated this by displaying merger-level estimates and 95% confidence intervals for aggregate, merging-party, and non-merging-party price changes. Note that for aggregate and non-merging parties, the estimates with the largest standard errors are large positive price estimates, explaining why Bayesian shrinkage leads to a small reduction in the mean for these cases.

Finally, Panel H drops the mergers in our dataset where there was a divestiture. There is a small contraction of the right tail of price effects for merging and non-merging parties. This may reflect the fact that divestitures are enforced when the merger by itself would be especially anticompetitive, and they divestiture makes the mergers just “acceptable” to the antitrust agencies. Otherwise, the results are very similar.

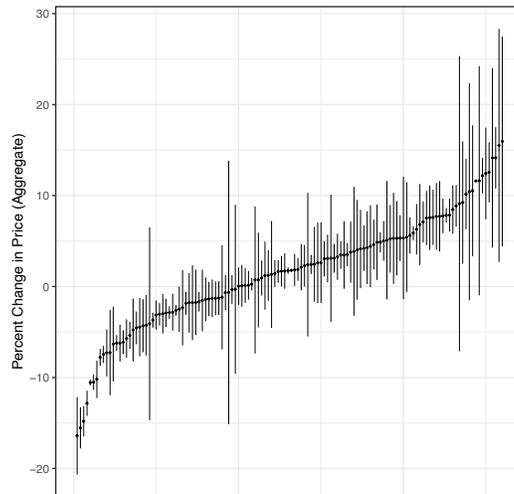
Figure B.2 is not directly related to price effects. It reports the timing of mergers. It displays, both at the merger and the deal level, histograms of the dates at which mergers became effective. Figure 3 shows that price changes appear to be coincident with the timing of the merger. The fact that mergers do not seem to be completed on special dates of the year helps rule out one alternative explanation for that observation.

²⁷For 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.

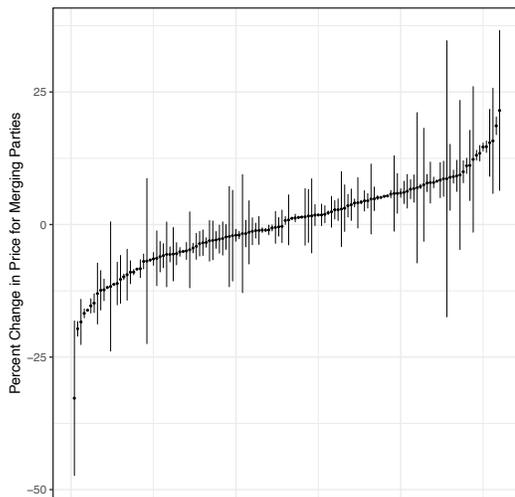
	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	130	1.51 (0.55)	6.29	-2.34 (0.58)	1.74 (0.59)	5.31 (0.57)
Merging Parties	130	0.03 (0.74)	8.47	-5.15 (0.97)	0.77 (0.97)	5.86 (0.85)
Non-Merging Parties	130	2.07 (0.62)	7.11	-2.20 (0.62)	1.93 (0.58)	6.12 (0.87)
B. Panelist Data						
Overall	130	-0.57 (0.53)	6.00	-3.61 (0.66)	0.04 (0.46)	2.89 (0.82)
Merging Parties	130	-0.86 (0.72)	8.23	-4.42 (0.62)	-0.40 (0.78)	4.17 (0.70)
Non-Merging Parties	130	-0.30 (0.56)	6.43	-3.56 (0.67)	0.09 (0.44)	3.33 (0.84)
C. Food Mergers Only						
Overall	75	-0.06 (0.77)	6.69	-4.46 (1.32)	0.25 (1.14)	4.87 (0.55)
Merging Parties	75	-1.07 (1.11)	9.65	-6.96 (1.62)	-1.05 (1.25)	5.39 (2.05)
Non-Merging Parties	75	0.14 (0.79)	6.85	-4.55 (1.21)	0.65 (1.13)	4.69 (0.85)
D. Unbalanced Panel of Stores						
Overall	130	1.56 (0.54)	6.20	-2.11 (0.79)	1.76 (0.68)	5.11 (0.71)
Merging Parties	130	-0.05 (0.75)	8.54	-5.45 (1.09)	0.45 (1.16)	5.76 (0.70)
Non-Merging Parties	130	2.10 (0.62)	7.04	-2.28 (0.81)	1.97 (0.72)	6.40 (0.62)
E. Equally-Weighted Regressions						
Overall	130	0.50 (0.47)	5.31	-2.36 (0.41)	1.10 (0.59)	4.10 (0.77)
Merging Parties	130	-0.04 (0.66)	7.50	-4.14 (0.93)	0.65 (0.77)	4.48 (0.80)
Non-Merging Parties	130	0.82 (0.49)	5.57	-2.42 (0.49)	1.18 (0.52)	4.20 (0.66)

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
F. Brand Level						
Overall	130	-0.60 (0.59)	6.72	-4.14 (0.79)	0.03 (0.53)	2.83 (0.55)
Merging Parties	130	-0.69 (1.06)	12.04	-7.89 (1.62)	-1.13 (0.71)	3.69 (0.98)
Non-Merging Parties	130	-0.26 (0.66)	7.48	-4.36 (0.77)	0.32 (0.42)	2.70 (0.47)
G. Bayesian Shrinkage						
Overall	130	1.16 (0.48)	5.53	-1.81 (0.58)	1.74 (0.54)	4.84 (0.53)
Merging Parties	130	-0.08 (0.65)	7.42	-4.92 (1.02)	0.77 (0.91)	5.07 (0.66)
Non-Merging Parties	130	1.49 (0.51)	5.85	-1.87 (0.53)	1.88 (0.51)	5.61 (0.80)
H. Mergers without Divestitures						
Overall	124	1.24 (0.55)	6.16	-2.49 (0.63)	1.69 (0.63)	5.30 (0.60)
Merging Parties	124	-0.11 (0.77)	8.57	-5.15 (0.95)	-0.34 (1.04)	5.39 (0.81)
Non-Merging Parties	124	1.72 (0.61)	6.78	-2.27 (0.61)	1.89 (0.61)	5.39 (0.89)

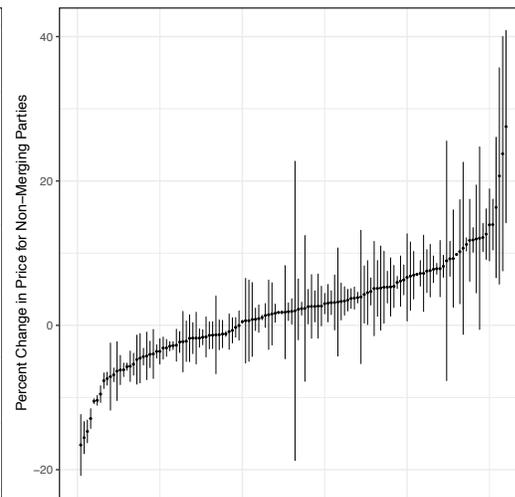
Table B.1: Robustness of Price Effects. This table displays the distribution of transformed coefficient estimates of (2) (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for overall, merging-party, and non-merging-party price changes. Standard errors are in parentheses. Panel A displays the baseline results from the main text, Panel B displays results using the NielsenIQ Consumer Panel Data, Panel C displays results for food mergers only, Panel D displays results using an unbalanced panel of stores, Panel E displays results assuming equal weights across UPC/DMA's when estimating (1) and (2), Panel F displays results for brand-level regressions, Panel G displays results using Bayesian shrinkage, and Panel H displays results for mergers without divestitures.



(a) Aggregate



(b) Merging



(c) Non-Merging

Figure B.1: 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.

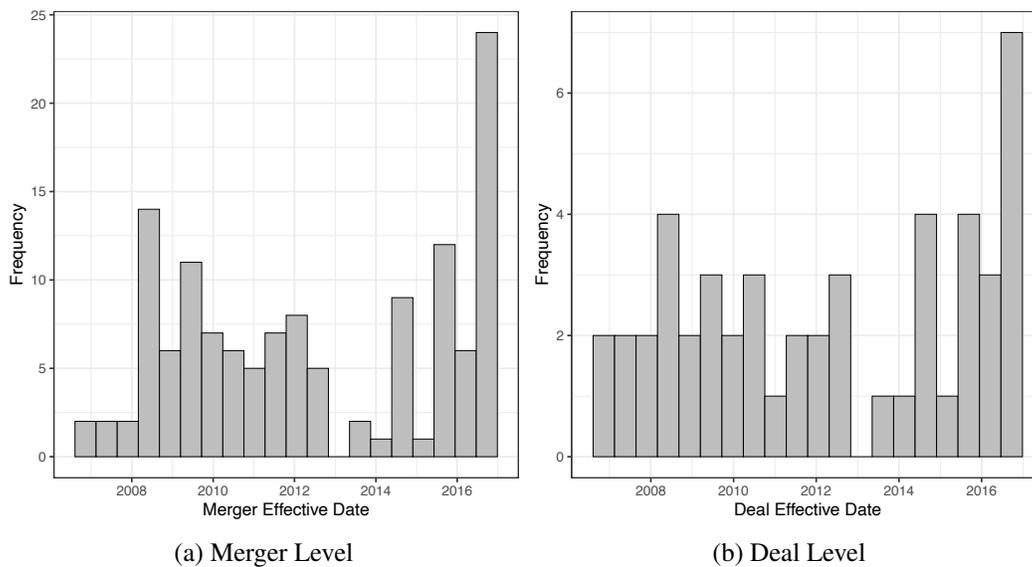


Figure B.2: Timing of mergers. These plots display histograms of the dates at which mergers became effective. The unit of observation of the left-hand panel is a merger, while the unit of observation of the right-hand panel is a deal.

B.2. Additional Results Related to Quantity Effects

Figure B.3 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. First, 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. Second, the changes again seem coincident with the timing of the merger. Third, for mergers with positive estimated quantity effects, quantity effects for merging parties tend to flatten out about a year after merger consummation. We do not see evidence of flattening out for negative quantity changes. Fourth, quantity changes for merging and non-merging parties are not strongly correlated, which was also evidenced in Figure 4(b).

Table B.2 provides robustness checks for quantity effects. Cost and demographic controls (Panel B) have very little impact on the estimated effects. When using untreated markets as a control (Panel C), we estimate wider distributions of effects.

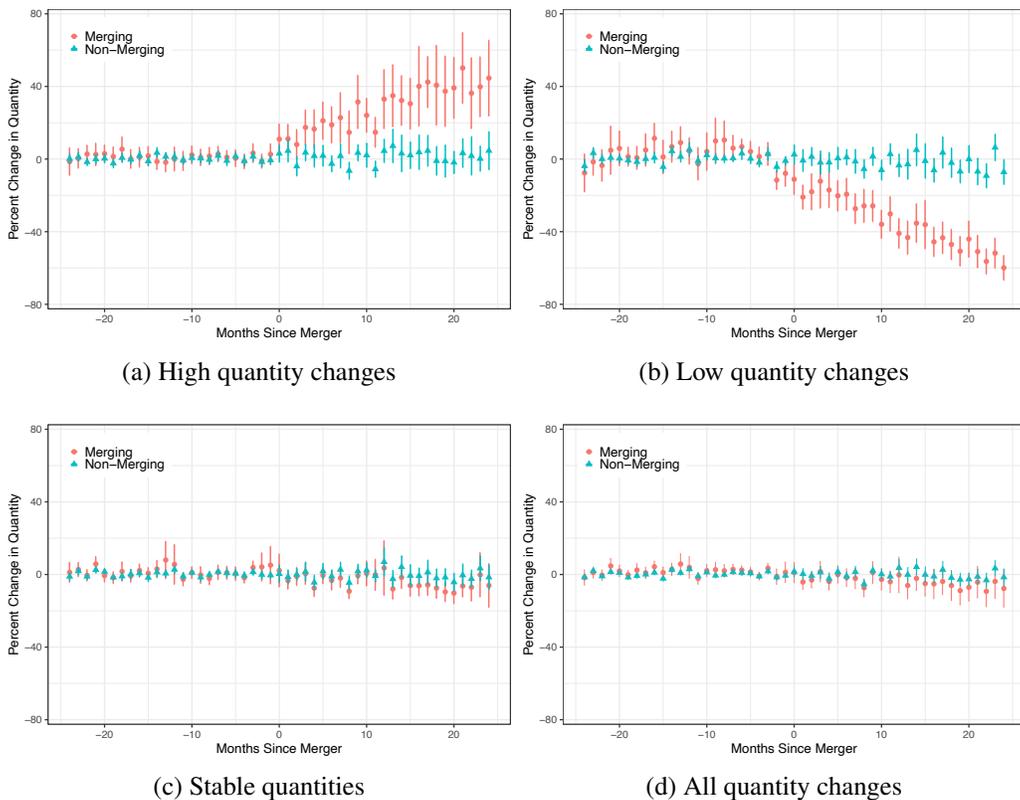


Figure B.3: Timing of quantity changes, for merging parties (red circle) and non-merging parties (blue triangle). The marker indicates the mean quantity change the given number of months after the merger becomes effective, and the thick line is the 95% confidence interval of that mean. Panels (a)–(c)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

We do not read much into the fact that means are slightly less negative (by 1–2 pp), since standard errors on the mean are large. We still find that merging parties drop quantities more than non-merging parties do on average. Panel D replicates the results with the Consumer Panel for convenience, but we refer the reader to Appendix A.2 for a more detailed discussion.

Panel E shows that non-food mergers tend to be ones with large quantity declines: restricting to food mergers leads to less negative quantity changes. It could be that distribution changes are less likely with food mergers.

We do not show results with an unbalanced panel of stores: interpreting total

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	130	-2.46 (0.79)	9.02	-6.87 (0.66)	-1.93 (0.74)	2.80 (0.70)
Merging Parties	130	-7.07 (2.40)	27.42	-20.96 (3.70)	-5.61 (1.95)	5.71 (1.93)
Non-Merging Parties	130	-1.45 (0.88)	10.04	-6.37 (0.72)	-1.86 (0.86)	4.09 (1.05)
B. Cost and Demographic Controls						
Overall	130	-2.45 (0.80)	9.08	-6.87 (0.65)	-1.74 (0.77)	2.85 (0.79)
Merging Parties	130	-7.06 (2.41)	27.45	-21.00 (3.29)	-5.73 (2.05)	5.60 (1.99)
Non-Merging Parties	130	-1.44 (0.88)	10.09	-6.49 (0.72)	-1.82 (0.82)	4.30 (1.13)
C. Treated/Untreated						
Overall	90	-1.22 (1.41)	13.37	-4.89 (0.97)	-1.12 (0.67)	3.22 (0.64)
Merging Parties	90	-5.06 (4.26)	40.44	-28.19 (5.93)	-6.12 (3.61)	11.99 (5.18)
Non-Merging Parties	90	-0.36 (1.33)	12.65	-3.70 (0.96)	0.29 (0.63)	3.52 (0.81)
D. Panelist Data						
Overall	130	-1.00 (0.94)	10.76	-6.45 (1.13)	-0.17 (0.95)	4.85 (1.19)
Merging Parties	130	-1.68 (3.43)	39.16	-22.25 (2.58)	-4.17 (2.53)	12.59 (4.80)
Non-Merging Parties	130	-0.83 (1.05)	11.92	-7.11 (1.12)	0.07 (0.96)	6.82 (1.23)
E. Food Mergers Only						
Overall	75	-0.36 (0.81)	6.99	-3.64 (1.13)	0.10 (0.85)	3.84 (0.56)
Merging Parties	75	-1.99 (3.26)	28.23	-15.64 (3.81)	-2.75 (2.52)	8.93 (2.12)
Non-Merging Parties	75	0.33 (1.18)	10.19	-3.73 (0.72)	0.38 (0.90)	4.66 (1.09)

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
F. Equally-Weighted Regressions						
Overall	130	-0.74 (2.26)	25.77	-11.83 (1.58)	-2.08 (1.54)	6.73 (1.06)
Merging Parties	130	5.23 (6.77)	77.23	-18.84 (3.53)	-5.00 (2.32)	11.45 (3.10)
Non-Merging Parties	130	-0.23 (1.19)	13.61	-6.71 (0.84)	-1.76 (1.21)	5.61 (1.18)
G. Bayesian Shrinkage						
Overall	130	-2.40 (0.78)	8.84	-6.84 (0.65)	-1.96 (0.72)	2.75 (0.66)
Merging Parties	130	-7.76 (2.22)	25.31	-20.91 (3.60)	-5.62 (1.95)	5.34 (2.42)
Non-Merging Parties	130	-1.35 (0.81)	9.27	-6.31 (0.70)	-1.86 (0.86)	3.99 (1.01)
H. Mergers without Divestitures						
Overall	124	-2.34 (0.82)	9.13	-6.64 (0.72)	-1.63 (0.70)	2.80 (0.67)
Merging Parties	124	-7.02 (2.51)	27.99	-20.96 (3.98)	-5.61 (2.14)	5.71 (2.16)
Non-Merging Parties	124	-1.46 (0.92)	10.24	-6.40 (0.78)	-1.86 (0.95)	4.09 (1.07)

Table B.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 with cost and demographic controls, Panel C displays results using geographic markets where merging parties comprise a small share of total sales as a control, Panel D displays results using the NielsenIQ Consumer Panel Data, Panel E displays results for food mergers only, Panel F displays results assuming equal weights across firm type/DMA's when estimating (1) and (2), Panel G displays results using Bayesian shrinkage, and Panel H displays results for mergers without divestitures.

quantity changes when the number of stores is changing is difficult. Panel F shows results using equally weighted regressions. Unlike for prices, this does have a large impact on the estimated mean: for instance, we now estimate that merging parties increase quantities by 5.2%. It is worth noting that this comes from an increase in the right tail of estimated quantity impacts: the first quartile and the median are very similar. Moreover, the standard deviation of price changes increases significantly, and the mean is also rather imprecisely estimated, with a 95% confidence interval

	Green				Yellow			Red		Green				Yellow			Red
	LM	LH	ML	HL	MM	MH	HM	HH		LM	LH	ML	HL	MM	MH	HM	HH
LL	0.67 (0.42)	2.66 (0.45)	1.01 (0.56)	2.28 (0.73)	1.17 (0.64)	2.26 (0.69)	2.23 (0.74)	2.92 (0.82)	LL	-2.35 (0.62)	-2.29 (0.98)	0.43 (0.38)	1.00 (0.47)	0.39 (0.50)	0.91 (0.78)	0.65 (0.60)	1.31 (0.76)
LM		1.99 (0.38)	0.34 (0.63)	1.60 (0.78)	0.50 (0.62)	1.58 (0.71)	1.55 (0.78)	2.25 (0.85)	LM		0.06 (0.72)	2.78 (0.77)	3.35 (0.80)	2.74 (0.74)	3.26 (0.82)	3.00 (0.85)	3.66 (0.95)
LH			-1.65 (0.54)	-0.39 (0.64)	-1.49 (0.46)	-0.41 (0.55)	-0.44 (0.64)	0.26 (0.74)	LH			2.73 (1.00)	3.29 (1.02)	2.68 (0.98)	3.20 (1.03)	2.94 (1.03)	3.61 (1.12)
ML				1.27 (0.64)	0.16 (0.54)	1.25 (0.61)	1.21 (0.61)	1.91 (0.71)	ML				0.56 (0.25)	-0.04 (0.39)	0.48 (0.75)	0.21 (0.48)	0.88 (0.67)
HL					-1.11 (0.55)	-0.02 (0.62)	-0.05 (0.38)	0.64 (0.50)	HL					-0.61 (0.41)	-0.09 (0.76)	-0.35 (0.49)	0.32 (0.70)
MM						1.09 (0.44)	1.05 (0.52)	1.75 (0.66)	MM						0.52 (0.56)	0.25 (0.46)	0.92 (0.59)
MH							-0.03 (0.57)	0.67 (0.69)	MH							-0.26 (0.62)	0.40 (0.61)
HM								0.70 (0.34)	HM								0.67 (0.42)

(a) Merging parties

(b) Nonmerging parties

Table B.3: Differences in DMA-level price effects across bins of DMA-level market structures. The first letter denotes the HHI bin (Low, Medium, or High), and the second letter denotes the DHHI bin. Each cell indicates the difference between the column bin and the row bin. Standard errors of the difference, clustered at the merger level, are in parentheses.

of (-8.0%, 18.5%). Finally, we do not think that weighing percentage changes in quantity for small-share brands equally with percent changes for large-share brands is especially meaningful. Overall, this is not our preferred specification, but we nevertheless report results for completeness.

Performing Bayesian shrinkage on the estimates (Panel G) yields numerically very similar estimates, as does dropping mergers with divestitures (Panel H).

B.3. Additional Results Related to Antitrust Enforcement

Table B.3 presents standard errors on all pairwise differences in Figure 7. This supports the statements made about these differences in Section IV.B.

We first study robustness of the analysis of the structural presumptions. Table B.4 replicates Table 5 but uses nationwide HHI and DHHI as the metrics for market structure, rather than the average of DMA-level HHI and DHHI. Some quantitative differences are notable. Price changes are correlated with DHHI for merging, non-merging, and overall changes at the 10% level, but the magnitudes of the effect for non-merging parties and overall are about one-half to one-third as large as in the baseline. The correlation with the largest bin of DHHI is significant for all cases, but is about 20% smaller for non-merging parties. The correlation with the red

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

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

zone is similar. We also find that non-merging parties and aggregate effects are less correlated with HHI than in the baseline. Despite these differences, our overall reading is that results are broadly similar, and most of the point estimates in the baseline lie in the confidence intervals of the estimates in Table B.4.

A second robustness test drops mergers with divestitures. We do this for two reasons. First, we do not use the realized price changes of mergers with divestitures to estimate the structural model, as thus the prior is informed by price changes of mergers without divestitures. 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 B.5 replicates Table 5 but drops mergers in which the parties had to divest at least one brand. The correlation with DHHI (linearly) decreases for all three cases. It remains significant at 10%

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

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

for non-merging parties, but we find a p -value less than 0.11 for aggregate changes. Correlations with the largest bin of DHHI are quantitatively smaller (by about one-fourth to one-third) for non-merging and aggregate changes, but significant for all three cases. However, while we still see a robust correlation with DHHI, the correlation with the red zone becomes smaller statistically insignificant for all three cases.

Related to this robustness check, we can rerun the within-merger analysis restricting to mergers without divestitures. Figure B.4 replicates Figure 7 but drops mergers in which parties had to divest at least one brand. Estimates are very similar, even quantitatively. Thus, our observation that prices are correlated with both HHI and DHHI 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

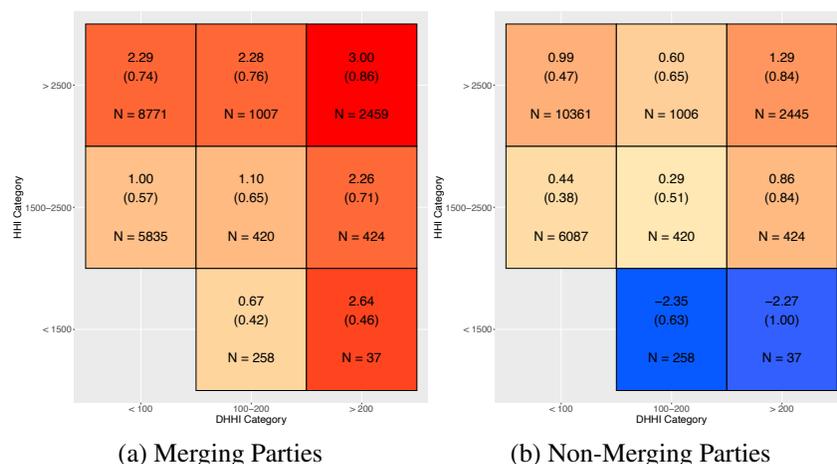


Figure B.4: Within-merger price changes for bins of DMA-level HHI and DHHI for mergers without divestitures. Each bin shows the coefficient of a regression of DMA-level price changes on bin dummies and merging party fixed effects. The omitted bin is the one with low HHI and low DHHI. Standard errors, clustered at the merger level, are in parentheses. N indicates the number of DMA-mergers in each bin.

the reader to the discussion there rather than reproducing results here.

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 B.6 reports linear probability models of enforcement on merger-level market structure (in Columns (1)–(3)) and these measures together with elements of the threshold (in Columns (4)–(6)). This table corroborates that enforcement correlates with DHHI (linearly, or looking at the high bin) and the red region of the merger guidelines. It is also correlated with the high region of HHI. These estimates, together with the correlation of prices with market structure, inform our estimate of the prior in Panel A of Table 6. Notably, Columns (4)–(6) show that enforcement is significantly correlated with our measure of market size, which feeds into a significant estimate of the effect of market size on the the threshold in Panel C of Table 6.

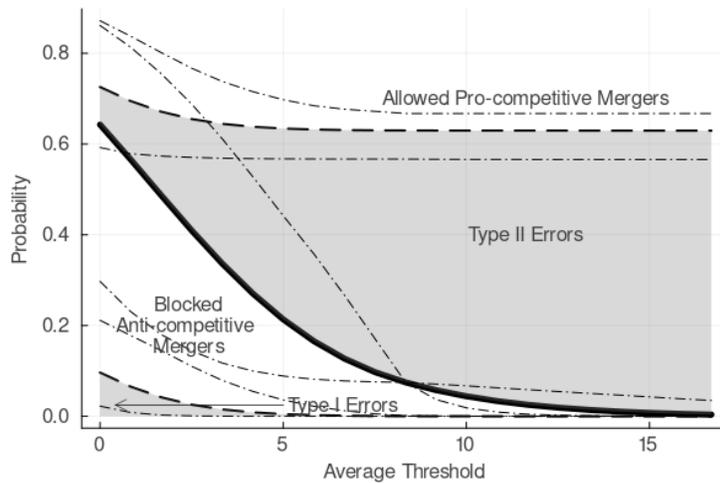
We now move to robustness checks on the model of antitrust enforcement. For completeness, we start by simply reporting confidence intervals on our analysis of counterfactual antitrust policies. Figure B.5 reports Figure 9 but adds 95% confidence intervals as dotted lines. The main takeaway that reducing the threshold

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

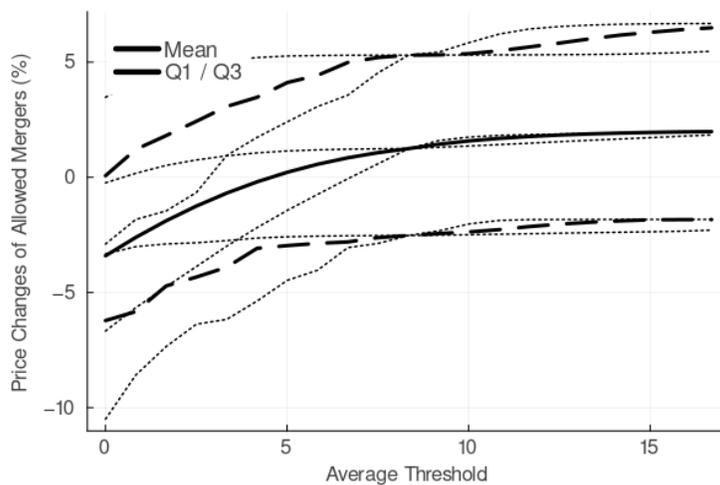
Table B.6: Correlates of Enforcement. Coefficients indicate output from a linear probability model of enforcement on merger-level market structure. HHI and DHHI as measured as the average across all DMAs within a merger. Robust standard errors are in parentheses.

will limited impact on type I errors holds. The upper bound of the confidence interval on probability of a type I error is 5%.

Figure B.6 shows results of the structural estimates using alternate methods to estimate price effects. It reports sales-weighted thresholds in Panel (a), the analogue of Panel D (Column (1)) of Table 6. Panel (b) reports probabilities of challenges and probabilities of type I and type II errors as a function of alternate thresholds. Panel (c) reports price changes for consummated mergers. These panels are the analogues of Figure 9. (Note that we do not have results with the Bayesian shrinkage robustness check since the model of antitrust enforcement effectively incorporates shrinkage by taking into account that the point estimate is noisy.) In most of our robustness checks, the estimate of the threshold is smaller than in our baseline, consistent with the observation that many robustness checks led to dampening the right tail of price estimates. Thresholds could be reduced to even 2.5% and the probability of a type



(a) Probability of challenges

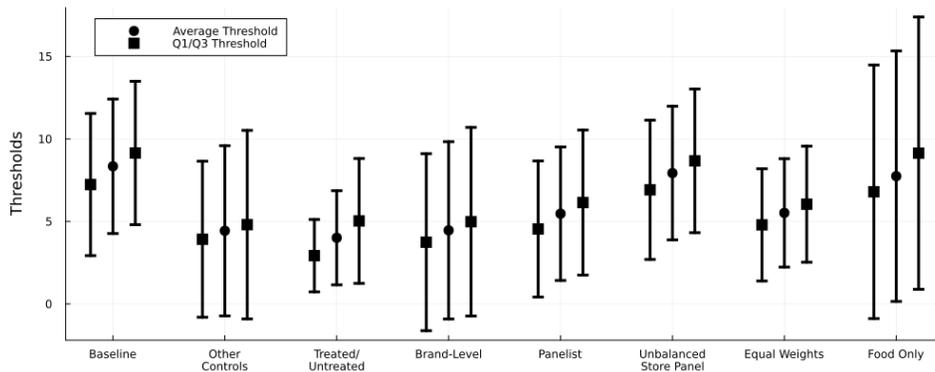


(b) Price changes of consummated mergers

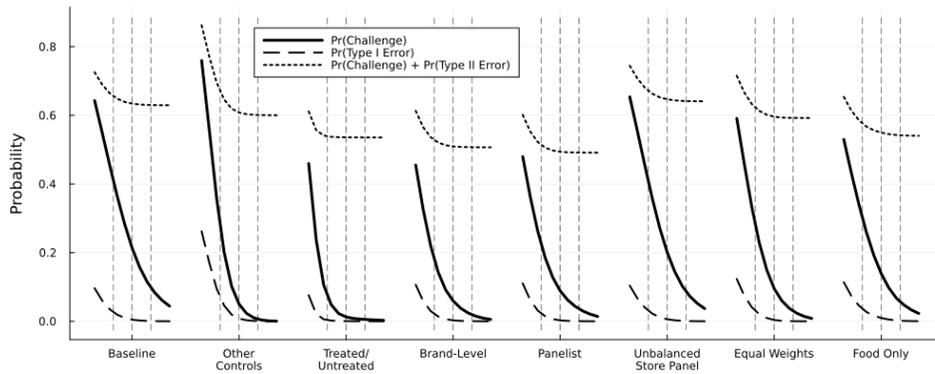
Figure B.5: Outcomes of counterfactual thresholds. This replicates Figure 9, but the dotted lines surrounding each line represent 95% confidence intervals.

I error would stay bounded by 5%—although the probability of challenges would increase to as much as 40% if the agencies choose to do so.

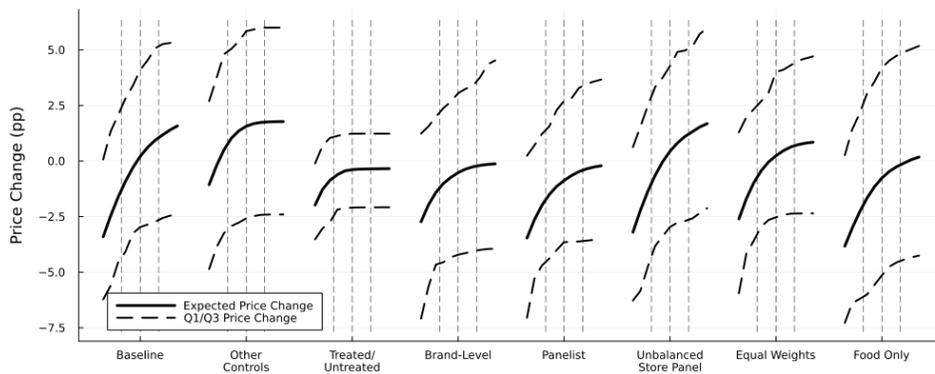
Next, we add additional covariates to the threshold to capture funding and workload of the agencies. The DOJ Antitrust Division provides yearly appropriations at <https://www.justice.gov/atr/appropriation-figures-antitrust-division>. We collect data on FTC appropriations by manually going through annual congressional budget



(a) Thresholds



(b) Probability of challenge as a function of threshold



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

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

	Aggregate Price Changes				Merging Party Price Changes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Prior								
Avg HHI (0–1)	-8.71 (4.77)	-9.50 (4.61)	-9.21 (4.60)	-9.11 (4.58)	-6.84 (4.66)	-7.83 (4.58)	-7.90 (4.53)	-7.94 (4.49)
Avg DHHI (0–1)	66.33 (19.01)	70.52 (18.19)	65.27 (17.84)	66.70 (18.09)	57.31 (20.01)	62.83 (19.07)	59.91 (17.76)	61.45 (17.89)
Constant	3.59 (1.64)	3.89 (1.58)	3.76 (1.57)	3.73 (1.57)	3.10 (1.58)	3.44 (1.57)	3.44 (1.56)	3.46 (1.54)
B. Errors and Uncertainty								
σ_{p^*}	6.00 (0.48)	6.10 (0.49)	5.99 (0.46)	6.00 (0.47)	5.87 (0.45)	5.95 (0.48)	5.88 (0.46)	5.90 (0.46)
σ_{ϵ}	4.36 (3.36)	3.22 (2.62)	3.72 (2.81)	3.82 (2.93)	6.18 (3.79)	4.85 (3.23)	4.72 (3.07)	4.61 (3.00)
Posterior Standard Deviation	3.53 (1.74)	2.85 (1.80)	3.16 (1.70)	3.22 (1.74)	4.26 (1.20)	3.76 (1.46)	3.68 (1.42)	3.63 (1.44)
C. Threshold								
Total Funding (\$B, Deflated)	-74.81 (59.56)		-101.98 (68.45)		-75.95 (54.81)		-96.19 (64.43)	
HSR Transactions Reported (Thousands)		1.01 (2.65)		1.70 (2.85)		0.89 (2.28)		1.61 (2.56)
Log(Total Merging Sales)			-1.35 (0.67)	-1.19 (0.58)			-1.31 (0.67)	-1.23 (0.58)
Constant	9.51 (2.59)	10.31 (2.18)	10.84 (2.63)	10.53 (2.39)	8.29 (2.57)	9.15 (2.52)	10.06 (2.72)	9.96 (2.47)
D. Sales-Weighted Thresholds								
Average	9.47 (2.59)	10.26 (2.14)	8.55 (2.05)	8.47 (2.12)	8.25 (2.56)	9.10 (2.50)	7.84 (2.04)	7.86 (2.03)
Q1	8.64 (2.56)	9.97 (2.06)	7.28 (1.88)	7.50 (2.14)	7.41 (2.44)	8.85 (2.43)	6.42 (1.81)	6.84 (1.98)
Q3	9.74 (2.57)	10.61 (2.34)	9.64 (2.40)	9.47 (2.35)	8.52 (2.58)	9.41 (2.65)	8.88 (2.40)	8.84 (2.33)

Table B.7: Parameter estimates of the model of antitrust enforcement, with measure of funding and workload in the threshold. Columns (1)–(3) document thresholds based on aggregate price changes, and Columns (5)–(8) document thresholds based on merging party price changes. All covariates in the threshold are demeaned.

justifications: budget justifications include information on prior years’ budgets devoted to its mission of “Maintaining Competition.” We deflate these by the GDP deflator from FRED. We use yearly Hart-Scott-Rodino filings as a proxy for agency workload, collected from annual HSR reports.

The odd columns of Table B.7 show results related to total funding of the agencies. A \$10 million increase in the budget of the agencies reduces the threshold by 0.7–1.0 pp. (The variation across years in funding in our sample is about \$50 million.) One

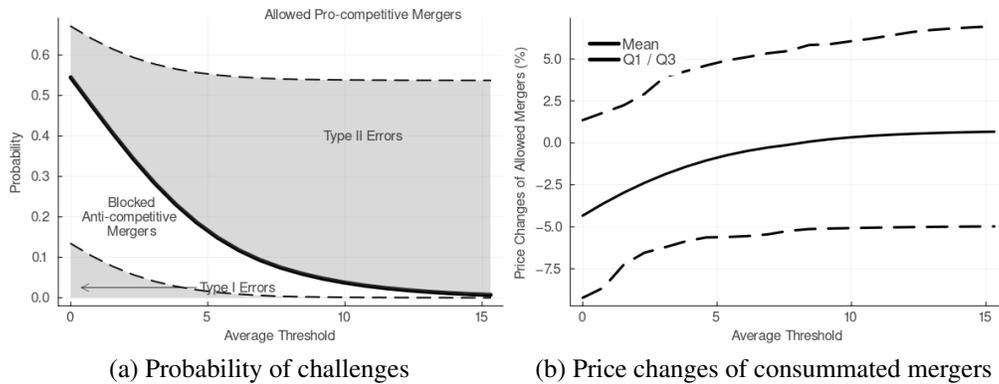


Figure B.7: Outcomes of counterfactual thresholds. This replicates Figure 9, but uses merging party price changes as the outcome variable of interest.

could interpret total funding as a proxy for the “stance” of the administration or political climate for antitrust enforcement. One could also interpret this as a proxy for workload: higher funding gives agencies the resources to target stricter cases. It is important to note that the variation in this thresholds comes entirely temporally. This result is suggestive: we are unable to reject that this is distinct from 0 at any reasonable significance level when using the scanner dataset, but we do find that the analogous estimates are significant when using the panelist dataset (Table A.5).

The even columns show no significant relationship between the number of HSR filings in a year and the threshold. For comparison, we have between 700 and 2,200 filings per year over our sample period, so the estimate should be interpreted as noisy rather than zero. We have checked that other proxies for workload—including monthly HSR filings, full-time equivalent workers, or filings per employee—also return noisy results. Overall, we cannot draw definitive conclusions about how workload influences the threshold.

In the last two columns of each section, we see that market size still remains a significant shifter of the threshold. Moreover, the thresholds in Panel D are not influenced significantly by the inclusion of these additional variables.

Finally, Figure B.7 reruns the counterfactual analysis of Section V.B but uses merging party price changes as the outcome of interest—following the specification in Column (4) of Table 6. The broad patterns remain the same, although the

quantification changes. Moving to a 5% threshold would lead to a probability of challenge of 0.17, about 2.2 times the baseline at the current threshold. Moving to a 0% threshold would lead to a probability of challenge of 0.55. The type I error rates are still low. However, they are quantitatively larger than when looking at aggregate price changes: at thresholds of 5% and 0%, about 10% and 25% of blocked mergers would be a type I error (respectively). These larger numbers can be traced back to our estimates that agencies have a less precise signal of merging party price changes.

B.4. Comparison of Predicted Price Effects of Computed Thresholds

As discussed in Section V.A, we compare our estimated thresholds to predicted price effects to put them into context: our thresholds should be lower than all predictions. 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 try to restrict to horizontal mergers in CPG. We collect all price predictions mentioned in any of these documents in Table 7.²⁸ 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 sales of the 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 B.8 describes the source of the data for market size and also the source of the estimate of price effects.

As discussed in Section V.A, in most cases, our estimated thresholds are lower than the predicted price changes. In H&R Block / TaxACT, the predicted effects for

²⁸In 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.

Merger	Merging Party Revenue	Price Effects
H&R Block / TaxACT	H&R revenue from tax services of \$2.9B (2011 10-K); TaxACT revenues are \$78M for FY 2011, from company press release .	DOJ closing argument 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 (10-K) 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 2013 Annual Report (p. 58). We take each appliance market to be 20% of total appliance sales.	Unilateral price changes from UPP with different demand functions, from Michael Whinston’s testimony (p. 59)
Aetna / Humana	—	Changes in rebate-adjusted premiums, from demonstrative used in Aviv Nevo’s testimony (p. 65)
Sysco / US Foods	Total broadline sales for Sysco are \$37.7B (2014 10-K , p. 97), and 87% of overall sales are in US (p. 98). Total US Foods revenue is \$23B (10-K), all of which is assumed to be broadline.	DOJ closing argument 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 (DOJ opening statement , p. 10) and HHI nearly 10,000 for merging parties (DOJ closing statement , p. 30)	Energy Solutions’ internal model shows 15% price increases per year for two years, from DOJ closing statement (p. 38)
AT&T / Time Warner	Time Warner’s operating revenue was \$31.2B (2017 10-K , p. 52), and AT&T’s video entertainment operating revenue was \$36.7B (2017 10-K , p. 135). Market size related to carriage fees are from scaling annual impact on carriage fees by percent impact (Figure 13 of expert report).	Cable price effects are from Figures 15 and 16 of Carl Shapiro’s expert report . Carriage fee increases are in Figure 13.
Hackensack / Englewood	—	DOJ closing argument for Penguin (p. 39), citing 2021 WL 4145062, 22 n.26 (D.N.J. 2021)
US Sugar / Imperial	DOJ press release	Price changes from a second-score model accounting for coordinated effects, from Dov Rothman’s testimony (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 closing argument	DOJ closing argument (pp. 38–39)
JetBlue / Spirit	DOJ closing argument (p. 10)	JetBlue internal assumption on price effect of Spirit exit, from DOJ closing argument (p. 49)

Table B.8: 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.

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.²⁹ In US Sugar / Imperial, price effects are lower than thresholds we estimate, but only when considering the aggregate threshold.³⁰

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. 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 price effects to drop below our thresholds. These mergers are also taken to trial, so they may be especially far past the “enforcement margin.” 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.

²⁹This 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.

³⁰We 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.

C. Estimation and Heterogeneous Treatment Effects

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

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

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

In the first stage, we estimate

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

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

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

and this average are

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

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

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

The weighted OLS estimator for β_b is

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

We recover the remaining parameters as

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

In the second stage, we take weighted averages of

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

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

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

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

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

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

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

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.

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

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

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.³¹ We restrict our attention to horizontal mergers for goods that may have been in our NielsenIQ sample. Our data agreement prevents us from listing the names of the merging parties.

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 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.

³¹For 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.

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.³² 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.³³ 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.

³²We 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>.

³³See <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	Corn Sweeteners, Starch Vegetable Fats Oils, Vitamin Nutrient Hematinic Human
2	Baked Goods-Frozen		Bakery-Bagels-Frozen	Wheat, Other Grains
3	Beer		Beer, Stout And Porter, Light Beer (Low Calorie/Alcohol), Ale	Barley, Wheat
4	Bread And Baked Goods		Bakery - Bread - Fresh	Wheat, Other Grains
5	Bread And Baked Goods		Bakery-Bagels-Fresh	Wheat, Other Grains
6	Bread And Baked Goods		Bakery-Breakfast Cakes/Sweet Rolls-Fresh	Wheat Flour, Sugar, Vegetable Oil
7	Bread And Baked Goods		Bakery-Buns-Fresh	Wheat, Other Grains
8	Bread And Baked Goods		Bakery-Cheesecake-Fresh	Cheese, Wheat Flour, Wheat, Eggs, Sugar
9	Bread And Baked Goods		Bakery-Doughnuts-Fresh	Wheat Flour, Sugar, Vegetable Oil
10	Bread And Baked Goods		Bakery-Muffins-Fresh	Wheat, Other Grains
11	Bread And Baked Goods		Bakery-Pies-Fresh	Wheat Flour, Pecans, Lemons, Apples
12	Bread And Baked Goods		Bakery-Rolls-Fresh	Wheat, Other Grains
13	Breakfast Foods-Frozen		Frozen/Refrigerated Breakfasts	Eggs, Slaughter Poultry, Slaughter Cattle, Beef And Veal, Cheese, Russet Potatoes
14	Candy		Candy-Chocolate-Miniatures, Candy-Chocolate, Candy-Chocolate-Special	Cocoa Beans, Sugar
15	Candy		Candy-Dietetic - Non-Chocolate, Candy-Dietetic - Chocolate	Sugar, Cocoa Beans
16	Candy		Candy-Hard Rolled, Candy-Non-Chocolate-Miniatures, Candy-Non-Chocolate, Candy-Lollipops	Sugar
17	Cereal		Cereal - Granola & Natural Types	Sugar, Oats
18	Cereal		Cereal - Ready To Eat	Barley, Corn, Oats, Rough Rice, Sugar, Wheat
19	Coffee		Coffee - Soluble Flavored, Coffee - Soluble	Coffee Beans
20	Coffee		Ground And Whole Bean Coffee	Coffee Beans
21	Condiments, 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, Spices, Vinegar, Dry Onions, 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, Spices, Vinegar, Dry Onions, Tomatoes
29	Condiments, And Sauces	Gravies,	Seasoning Mix - Sloppy Joe	Salt Pepper Spices, Spices, Vinegar, Dry Onions, 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	Fatty Acids, Starch Vegetable Fats Oils
35	Cosmetics		Cosmetics-Eyebrow & Eye Liner	Fatty Acids, Starch Vegetable Fats Oils
36	Cosmetics		Cosmetics-Face Powder	Fatty Acids, Starch Vegetable Fats Oils
37	Cosmetics		Cosmetics-Foundation-Liquid, Cosmetics-Foundation-Cream And Powder	Fatty Acids, Starch Vegetable Fats Oils
38	Cosmetics		Cosmetics-Lipsticks	Fatty Acids, Starch Vegetable Fats Oils
39	Cosmetics		Cosmetics-Mascara	Fatty Acids, Starch Vegetable Fats Oils
40	Cosmetics		Cosmetics-Remaining	Fatty Acids, Starch Vegetable Fats Oils
41	Cosmetics		Talcum & Dusting Powder	Talc, Corn Starch
42	Detergents		Detergents-Packaged, Detergents - Light Duty, Detergents - Heavy Duty - Liquid	Surfactants
43	Fragrances - Women		Cologne & Perfume-Women's	Ethanol, Coal, Soybeans, Other Grains
44	Fresh Produce		Fresh Fruit-Remaining	Fertilizer
45	Grooming Aids		Cosmetic And Nail Grooming Accessory	Stainless Steel, Aluminum, Plastic
46	Gum		Gum-Bubble, Gum-Chewing, Gum-Chewing-Sugarfree, Gum-Bubble-Sugarfree	Sugar, Resin And Synthetic Rubber
47	Hair Care		Creme Rinses & Conditioners	Fatty Acids, Surfactants
48	Hair Care		Hair Preparations - Other Than Men's	Ethanol, Basic Organic Compounds
49	Hair Care		Hair Spray - Women's	Ethanol, Basic Organic Compounds
50	Hair Care		Shampoo-Aerosol/ Liquid/ Lotion/ Powder, Shampoo-Combinations	Fatty Acids, Surfactants
51	Hair Care		Wave Setting Products	Ethanol, Basic Organic Compounds
52	Housewares, Appliances		Oral Hygiene Appliance And Accessory	Nylon, Plastic
53	Kitchen Gadgets		Beverage Storage Container	Plastic, Stainless Steel
54	Laundry Supplies		Detergent Boosters	Surfactants
55	Liquor		Alcoholic Cocktails	Barley, Wheat, Corn
56	Liquor		Bourbon-Straight/Bonded, Bourbon-Blended, Canadian Whiskey, Irish Whiskey, Remaining Whiskey, Scotch	Barley, Wheat, Corn
57	Liquor		Cordials & Proprietary Liqueurs	Barley, Wheat, Corn
58	Liquor		Gin	Barley, Wheat
59	Liquor		Rum	Sugar
60	Liquor		Tequila	Sugar
61	Liquor		Vodka	Wheat, Russet Potatoes
62	Medications/Remedies/Health Aids		Foot Preparations-Athlete's Foot	Basic Organic Compounds
63	Men's Toiletries		Cologne/Lotion-Men's	Ethanol, Coal, Soybeans, Other Grains
64	Packaged Meats-Deli		Bacon-Refrigerated	Slaughter Hogs, Slaughter Poultry, Slaughter Hogs, Slaughter Poultry

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

Market	NielsenIQ 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 Supplies	Dry Erase Bulletin Board And Accessory		Aluminum
97	Stationery, School Supplies	Personal Planners Binders And Folders		Pulp Paper, Plastic
98	Tobacco & Accessories	Cigarettes		Tobacco, Pulp Paper
99	Unprep Meat/Poultry/Seafood-Frzn	Frozen Poultry		Poultry Processing, Slaughter Poultry, Processed Foods And Feeds
100	Vegetables - Canned	Mushrooms - Shelf Stable		Vinegar
101	Vegetables - Canned	Vegetables-Mixed-Canned		Carrots, Vinegar, Beans City Average
102	Vegetables - Canned	Vegetables-Peas-Remaining-Canned, Canned, Vegetables-Peas & Carrots-Canned	Vegetables-Peas-	Vinegar, Green Peas, Carrots, Pinto Beans
103	Vegetables And Grains - Dried	Rice - Instant		Fertilizer
104	Wine	Wine-Domestic Dry Table, Wine-Imported Dry Table		Wine Grapes, Us Aud Conversion, Us Euro Conversion

Table 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.