Supplemental Appendix for "Merger Effects and Antitrust Enforcement: Evidence from US Consumer Packaged Goods"

Vivek Bhattacharya, Gastón Illanes, and David Stillerman

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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 0.2–0.3 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 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

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	129	0.41	4.58	-2.06	0.13	3.88
		(0.40)		(0.38)	(0.52)	(0.50)
Merging Parties	129	0.15	7.06	-3.31	0.50	3.73
		(0.62)		(0.68)	(0.60)	(0.64)
Non-Merging Parties	129	0.59	4.82	-2.15	0.09	4.46
		(0.42)		(0.38)	(0.51)	(0.48)
B. Panelist Data						
Overall	129	0.66	6.08	-1.32	1.15	3.81
		(0.53)		(0.50)	(0.42)	(0.46)
Merging Parties	129	0.83	6.46	-2.40	1.44	4.61
		(0.57)		(0.95)	(0.49)	(0.58)
Non-Merging Parties	129	0.74	6.61	-1.85	0.90	3.53
		(0.58)		(0.84)	(0.53)	(0.46)

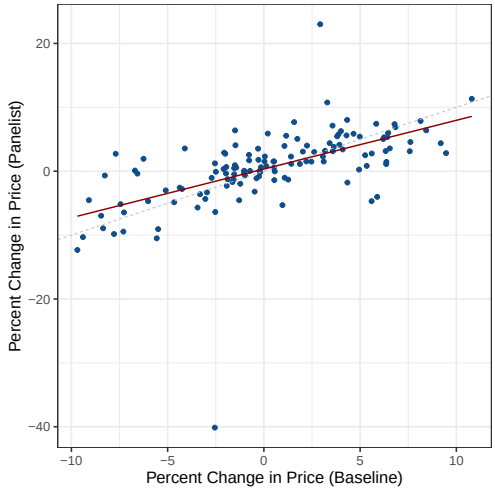
Table A.1: Price Effects using Consumer Panel Data. This table displays the distribution of transformed coefficient estimates of (2) (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for overall, merging-party, and non-merging-party price changes. Standard errors are in parentheses. Panel A displays the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

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.

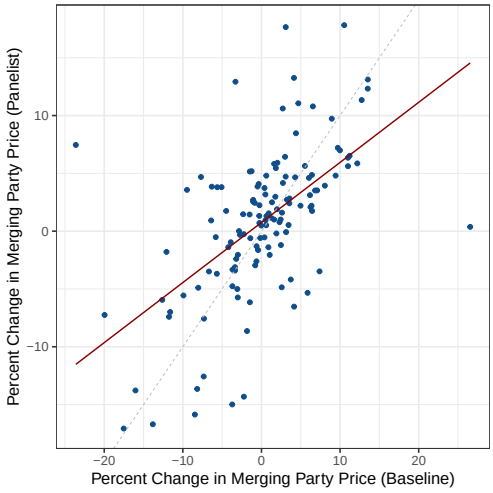
The qualitative takeaways of the analysis of the timing of price changes are also robust to the use of the panelist dataset. Figure A.3 replicates Figure 3 from the main text. We find similar patterns in timing. The main difference is that the average price decrease of non-merging firms is lower than 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 bottom left of Figure A.2.

A.2. Quantity Effects

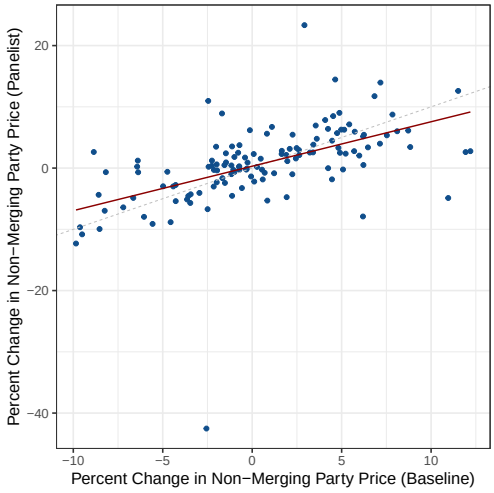
Table A.2 displays the distribution of estimated quantity effects using the Consumer Panel data. A number of the qualitative takeaways from the main text are robust to the use of the Consumer Panel dataset. Comparing Panels A and B, the distributions of overall quantity changes and those for non-merging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



(a) Aggregate



(b) Merging Parties



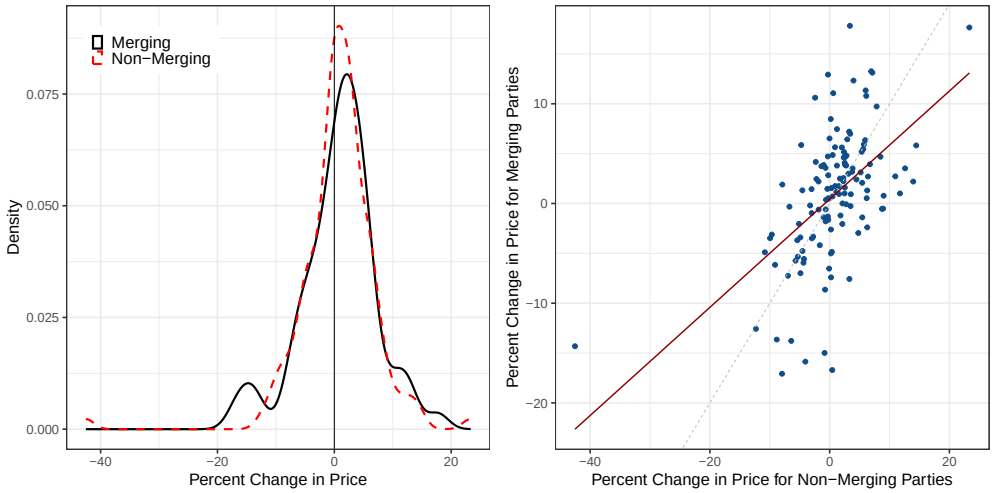
(c) Non-Merging Parties

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.

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



(a) Distribution of price changes

(b) Scatter of price changes

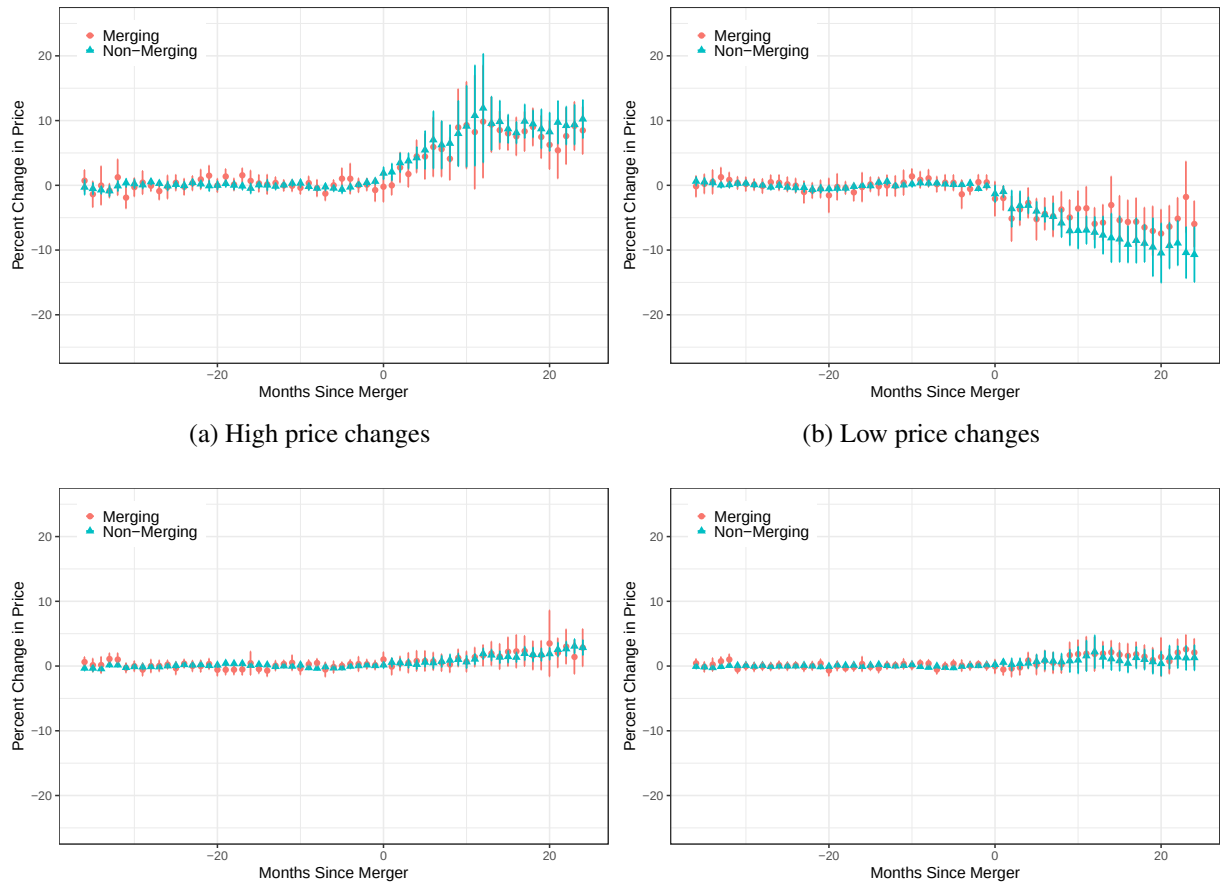
Figure A.2: Price changes for merging and non-merging parties using the NielsenIQ Consumer Panel Data. These plots display transformed coefficient estimates (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for the price change of the merging and non-merging parties.

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	129	-0.26	15.63	-9.71	-1.74	5.80
		(1.38)		(1.19)	(0.94)	(1.94)
Merging Parties	129	-6.28	32.53	-20.86	-8.49	5.53
		(2.86)		(2.79)	(2.23)	(3.10)
Non-Merging Parties	129	0.99	16.88	-7.81	-1.46	5.93
		(1.49)		(1.66)	(0.82)	(1.76)
B. Panelist Data						
Overall	129	-0.97	20.07	-12.42	-2.58	8.60
		(1.77)		(1.92)	(1.38)	(1.77)
Merging Parties	129	-0.73	46.30	-22.03	-5.95	13.28
		(4.08)		(4.82)	(2.58)	(5.53)
Non-Merging Parties	129	-0.07	20.76	-13.09	-2.63	9.74
		(1.83)		(1.44)	(2.06)	(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.

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 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) High price changes

(b) Low price changes

(c) Stable prices

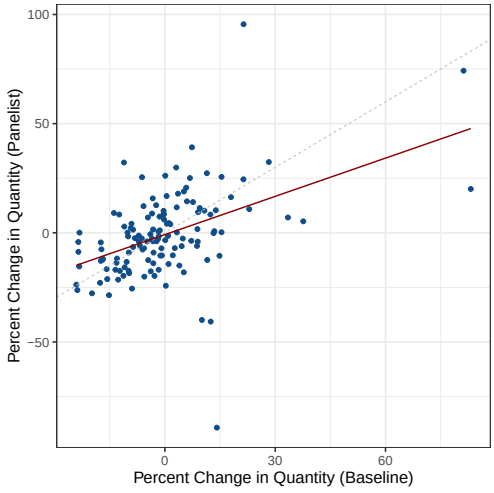
(d) All price changes

Figure A.3: Timing of price changes using the NielsenIQ Consumer Panel Data. Merging parties are denoted with red circles, and non-merging parties are denoted with blue triangles. 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.

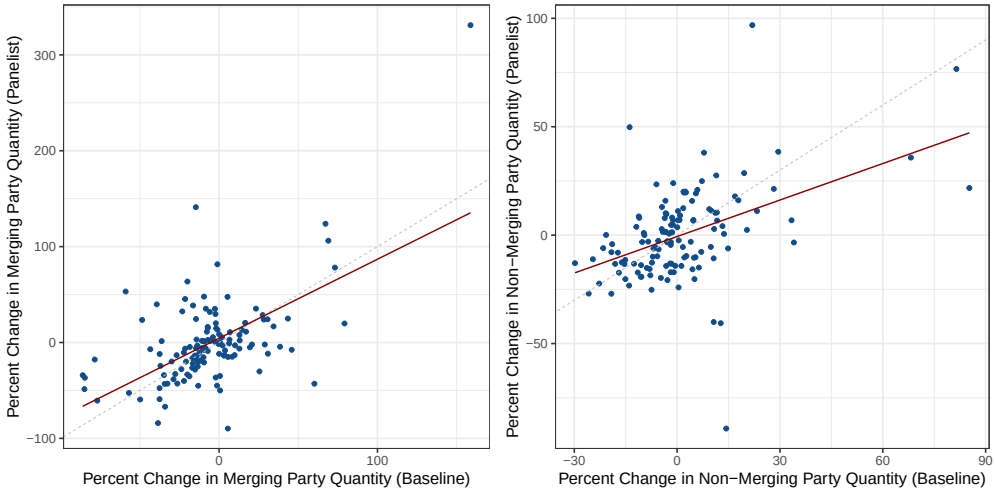
(a)) and non-merging parties (Panel (b)). The correlations between price and quantity effects are not statistically significant at conventional levels. The correlation for merging parties is -0.14 (s.e. 0.09) and for non-merging parties is -0.01 (s.e. 0.09). As in the main text, it is clear that prices 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.11, using results from the panelist data. We again see a fair bit of dispersion even in these conditional distributions, and the relationship between means



(a) Aggregate



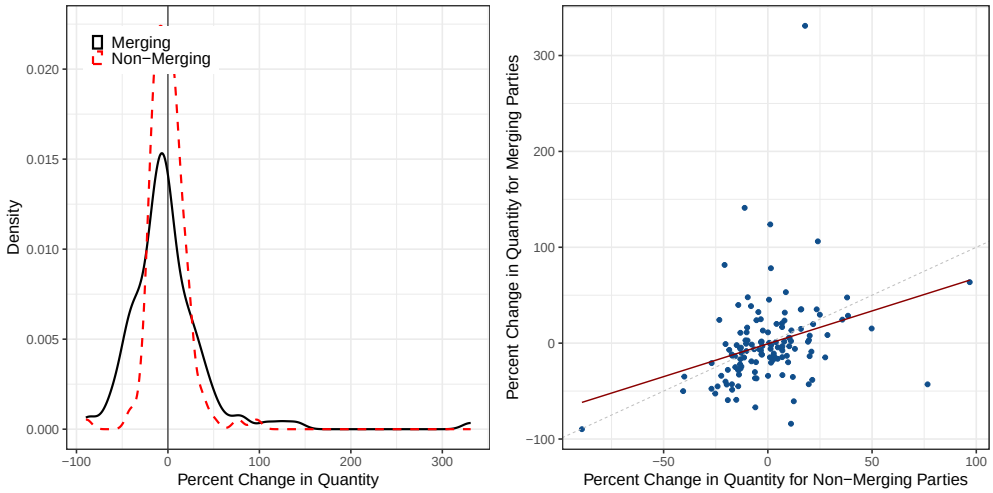
(b) Merging Parties

(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.

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.

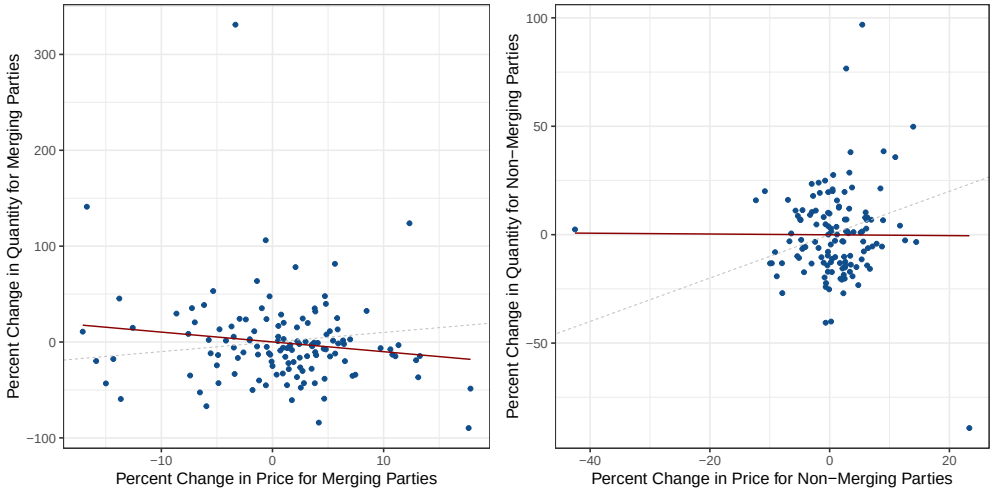
In Table A.3, we repeat the analysis of Table 6 in the main text. There are some differences: the Yellow region no longer exhibits a significant correlation with price changes, and the coefficient on the 2023 Presumptions decreases in magnitude. The negative correlation with HHI is weaker in Columns (2) and (7). Overall, however, we generally find the same patterns: price changes are correlated with DHHI and with merging share.



(a) Distribution of quantity changes

(b) Scatter of quantity changes

Figure A.5: Quantity changes for merging and non-merging parties 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.

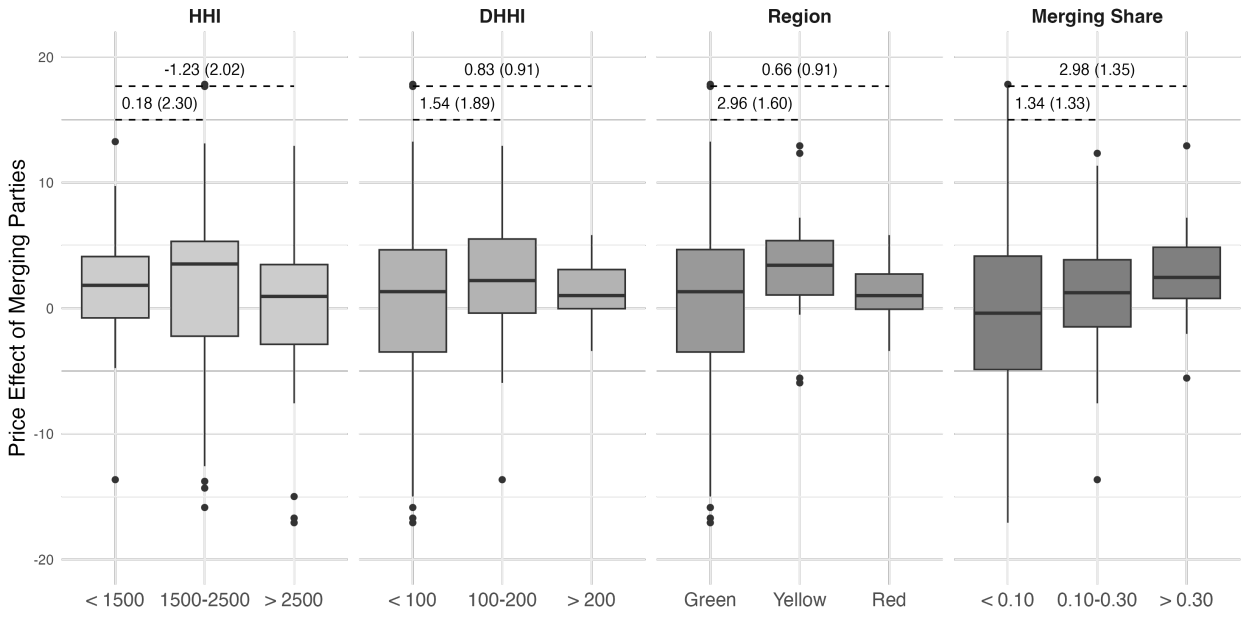


(a) Merging Parties

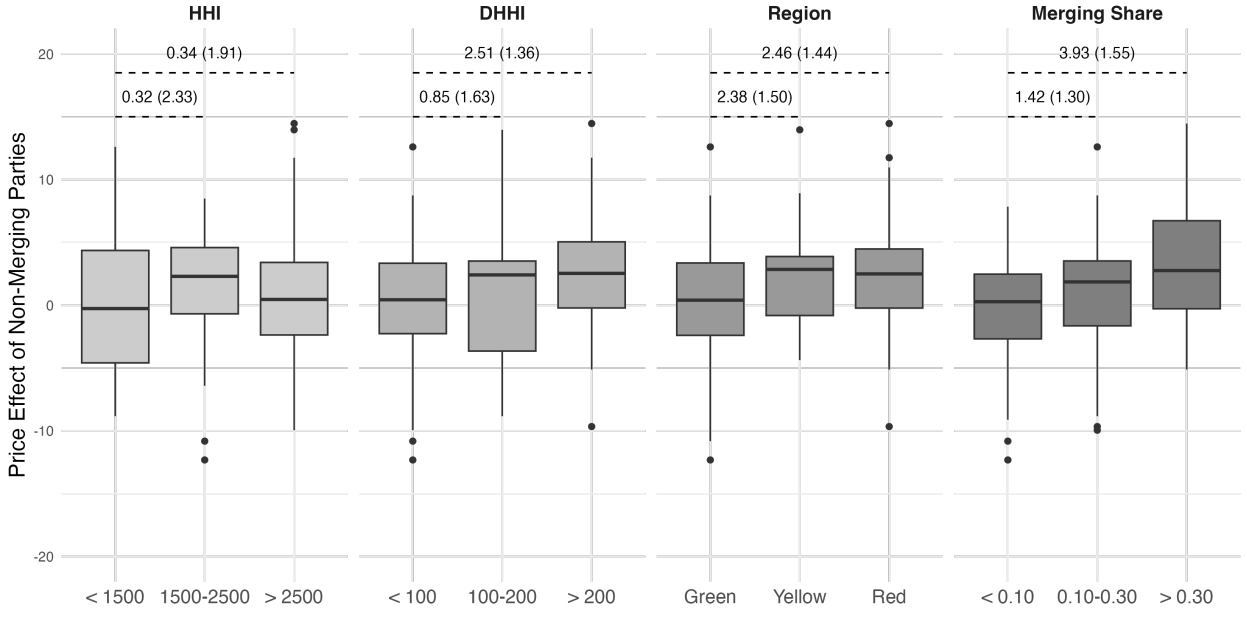
(b) Non-Merging Parties

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.

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



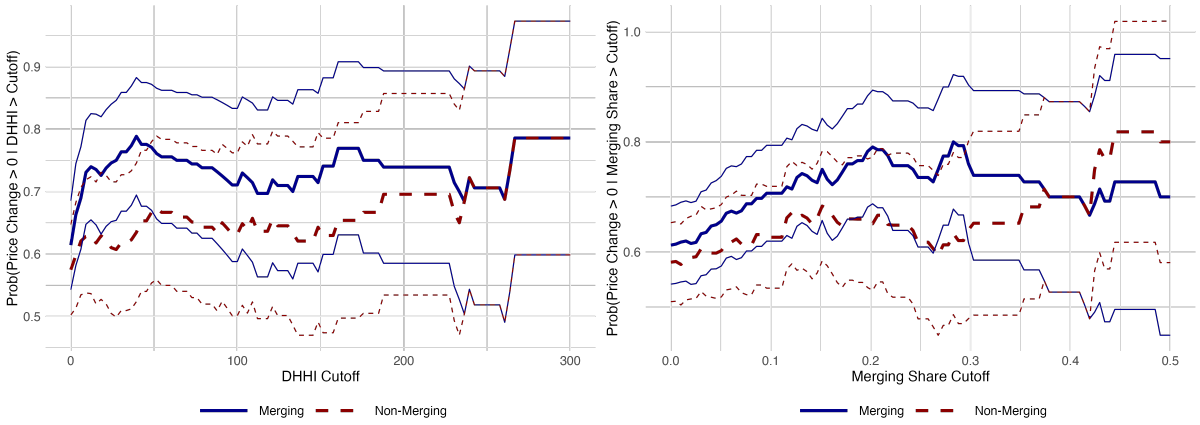
(a) Merging Parties



(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.

DMAs.



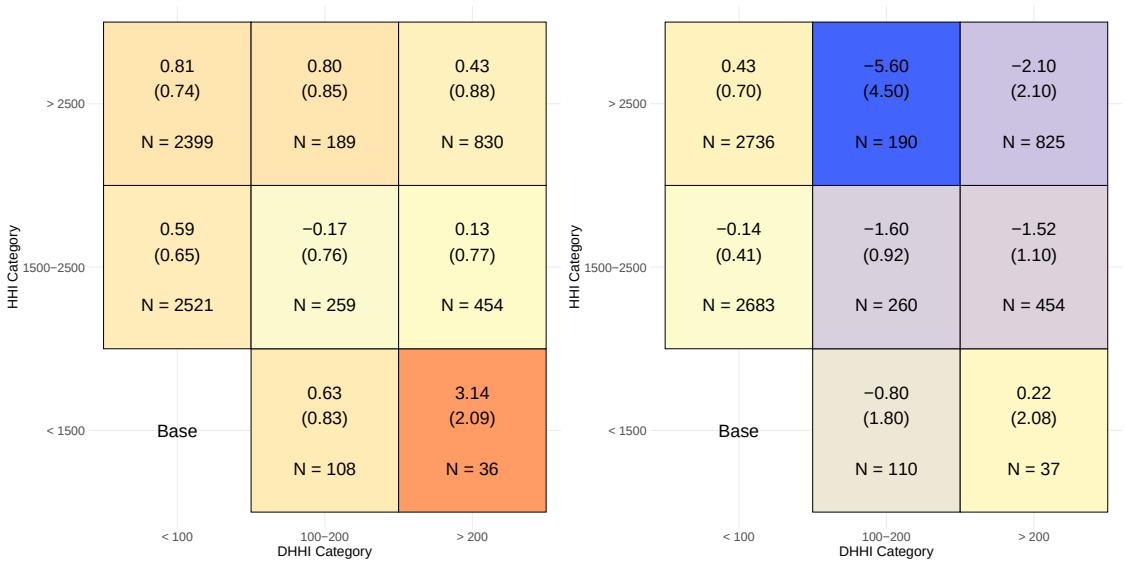
(a) DHHI

(b) Merging Share

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.

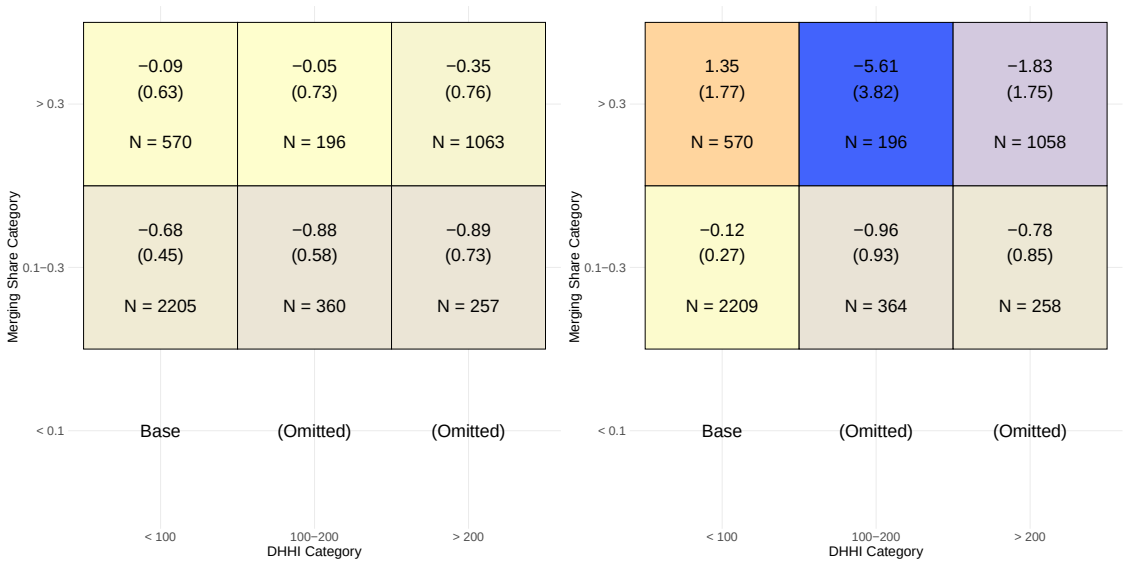
	Merging Party Price Changes						Non-Merging Party Price Changes					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
HHI (0–1)	-6.16						0.80					
	(4.38)						(4.48)					
DHHI (0–1)	15.70						39.08					
	(9.97)						(12.59)					
HHI $\in$ [1500, 2500]		0.40						0.34				
		(2.31)						(2.35)				
HHI $>$ 2500		-1.40						-0.26				
		(2.14)						(1.98)				
DHHI $\in$ [100, 200]		1.71						0.93				
		(1.85)						(1.58)				
DHHI $>$ 200		1.49						2.71				
		(0.95)						(1.28)				
Yellow			2.96						2.38			
			(1.60)						(1.50)			
Red			0.66						2.46			
			(0.91)						(1.44)			
Merg Share $\in$ [0.10, 0.30]				1.34						1.42		
				(1.33)						(1.30)		
Merg Share $>$ 0.30				2.98		5.12				3.93		1.08
				(1.35)		(1.12)				(1.55)		(1.39)
2023 Presumptions					1.58						2.43	
					(1.00)						(1.16)	
HHI $>$ 1800						-1.17						-0.01
						(1.77)						(2.48)
DHHI $>$ 100						-3.81						-3.50
						(5.05)						(3.85)
HHI $>$ 1800						5.65						4.03
$\times$ DHHI $>$ 100						(5.25)						(4.05)
Merg Share $>$ 0.30						-4.79						2.16
$\times$ DHHI $>$ 100						(1.81)						(2.19)
Constant	2.44	1.10	0.40	-0.27	0.40	1.13	-0.11	0.20	0.08	-0.58	0.08	0.14
	(1.49)	(1.95)	(0.74)	(1.13)	(0.74)	(1.51)	(1.63)	(1.90)	(0.72)	(1.09)	(0.71)	(2.38)
Observations	129	129	129	129	129	129	129	129	129	129	129	129
R^2	0.017	0.023	0.020	0.029	0.012	0.042	0.043	0.022	0.027	0.047	0.027	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.



(a) Merging (HHI-DHHI)

(b) Non-Merging (HHI-DHHI)



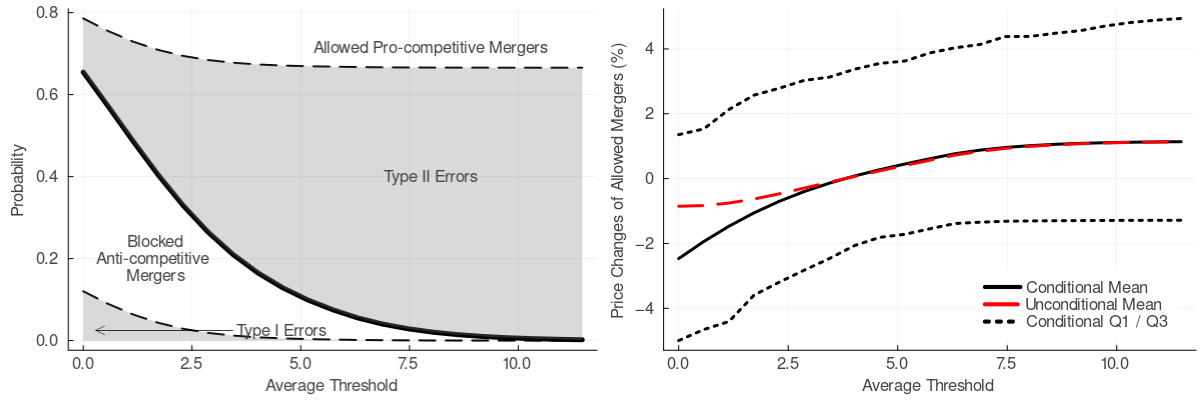
(c) Merging (Merging Share-DHHI)

(d) Non-Merging (Merging Share-DHHI)

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)
A. Prior										
Avg HHI (0–1)	-1.65					-4.23				
	(3.36)					(4.65)				
Avg DHHI (0–1)	38.10					50.79				
	(13.74)					(17.32)				
HHI $\in$ [1500, 2500]		0.48					0.35			
		(1.51)					(2.18)			
HHI $>$ 2500		0.38					-0.17			
		(1.46)					(2.08)			
DHHI $\in$ [100, 200]		-0.39					-0.14			
		(1.27)					(1.85)			
DHHI $>$ 200		2.77					4.43			
		(1.33)					(1.56)			
Yellow			0.34					1.18		
			(1.27)					(1.88)		
Red			2.94					4.53		
			(1.19)					(1.50)		
Merg Share $\in$ [0.10, 0.30]				1.84					2.45	
				(0.95)					(1.32)	
Merg Share $>$ 0.30				3.75					5.13	
				(1.20)					(1.56)	
2023 Presumptions					2.34					3.34
					(0.94)					(1.30)
Constant	1.26	0.45	0.76	-0.12	0.69	2.07	0.77	0.66	-0.40	0.68
	(1.07)	(1.36)	(0.50)	(0.68)	(0.51)	(1.50)	(1.94)	(0.69)	(0.92)	(0.70)
B. Errors and Uncertainty										
σ_{p^*}	4.88	4.82	4.83	4.83	4.89	6.45	6.41	6.41	6.40	6.50
	(0.37)	(0.36)	(0.36)	(0.37)	(0.37)	(0.50)	(0.50)	(0.50)	(0.49)	(0.50)
σ_{ϵ}	5.72	7.96	8.07	5.92	6.45	3.76	4.33	4.57	3.29	3.65
	(2.07)	(4.20)	(3.94)	(2.33)	(2.59)	(2.13)	(2.66)	(2.84)	(1.88)	(2.02)
Posterior Standard Deviation	3.71	4.12	4.15	3.74	3.90	3.25	3.59	3.72	2.92	3.18
	(0.57)	(0.60)	(0.55)	(0.59)	(0.57)	(1.36)	(1.49)	(1.50)	(1.31)	(1.32)
C. Threshold										
Log(Annual Merging Sales)	-0.79	-0.54	-0.52	-0.68	-0.63	-1.28	-1.16	-1.09	-1.29	-1.23
	(0.45)	(0.40)	(0.38)	(0.46)	(0.41)	(0.67)	(0.67)	(0.66)	(0.74)	(0.67)
Constant	7.08	5.76	5.71	6.92	6.42	10.75	10.24	10.05	11.07	10.73
	(1.73)	(2.04)	(1.91)	(1.76)	(1.75)	(2.19)	(2.40)	(2.45)	(2.17)	(2.14)
D. Sales-Weighted Thresholds										
Average	5.74	4.83	4.82	5.76	5.34	8.57	8.26	8.19	8.87	8.62
	(1.30)	(1.60)	(1.52)	(1.29)	(1.35)	(1.57)	(1.74)	(1.79)	(1.42)	(1.52)
Q1	4.90	4.25	4.27	5.04	4.66	7.20	7.03	7.02	7.50	7.31
	(1.13)	(1.33)	(1.28)	(1.10)	(1.16)	(1.40)	(1.50)	(1.53)	(1.28)	(1.37)
Q3	6.51	5.37	5.33	6.43	5.96	9.83	9.40	9.27	10.14	9.84
	(1.45)	(1.70)	(1.60)	(1.45)	(1.47)	(1.84)	(2.00)	(2.03)	(1.77)	(1.79)

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.



(a) Probability of challenges

(b) Price changes of consummated mergers

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

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 IV.C.

B. Additional Results on the Enforcement Model

B.1. Empirical Model

The model we outline in Section IV.A yields a threshold structure in the signal that depends on X and Z. The threshold we estimate in Section IV.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 IV.A.

Suppose that $p^* \sim N(\mu(X), \sigma^2_{p^*})$ and $p \sim N(p^*, \sigma^2_\epsilon)$. 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

$$S(Z) \cdot (\omega \cdot \hat{p} + (1-\omega) \cdot \mu(X)) + K_{II} \cdot \left[1 - \Phi\left(-\frac{\omega \cdot \hat{p} + (1-\omega) \cdot \mu(X)}{\sigma_{\text{post}}}\right)\right]$$

$$= K_C + K_I \cdot \Phi\left(-\frac{\omega \cdot \hat{p} + (1-\omega)\cdot \mu(X)}{\sigma_{\text{post}}}\right). \quad \text{(B.1)}$$

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 IV.[1]

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 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 σ_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 μ and standard deviation σ. 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 IV.A, we outline a model in which the antitrust agency challenges if

$$\int \left(S(p^*; X, Z) + K_{II} \cdot \mathbb{1}[p^* > 0] - K_I \cdot \mathbb{1}[p^* \leq 0]\right)\, dF_{p^*|p}(p^*|p, X) \geq K_C, \quad \text{(B.2)}$$

[1]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 (B.1) is replaced by $\mathbb{E}[S(m, Z)] + \frac{1}{2}S''(m, Z) \cdot \mathbb{E}[(p^* - m)^2] + \frac{1}{24}S^{(4)}(m, Z) \cdot \mathbb{E}[(p^* - m)^4] + \cdots$, 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.

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 (B.2) 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

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

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 σ_ϵ 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.[2] 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 (-0.30) is significantly smaller than -1. Moreover, trends do not explain much of the variation in estimated price effects: the R^2 is 0.26.

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 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.[3]

Figure C.2(a) compares the distribution of aggregate price effects under our baseline specification

[2]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.

[3]Note that some placebo dates lead to situations where we lack full coverage of cost controls. In these placebo draws, we only use the cost controls for which we have data. This prevents us from having to drop these draws, which would have lead to additional clustering in the placebo dates.

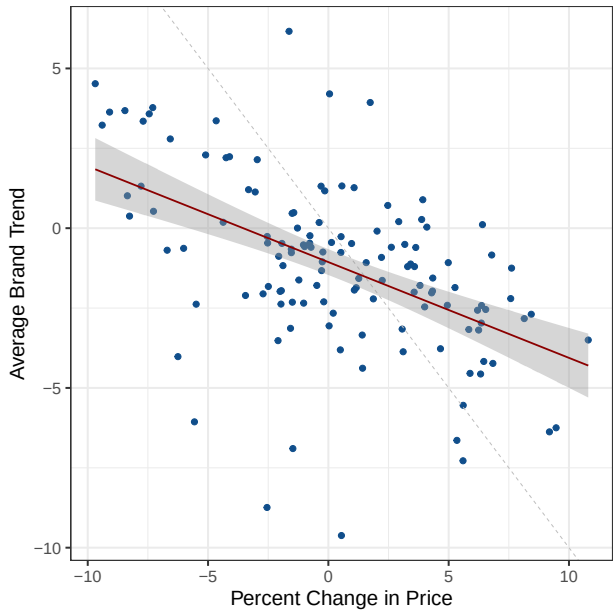
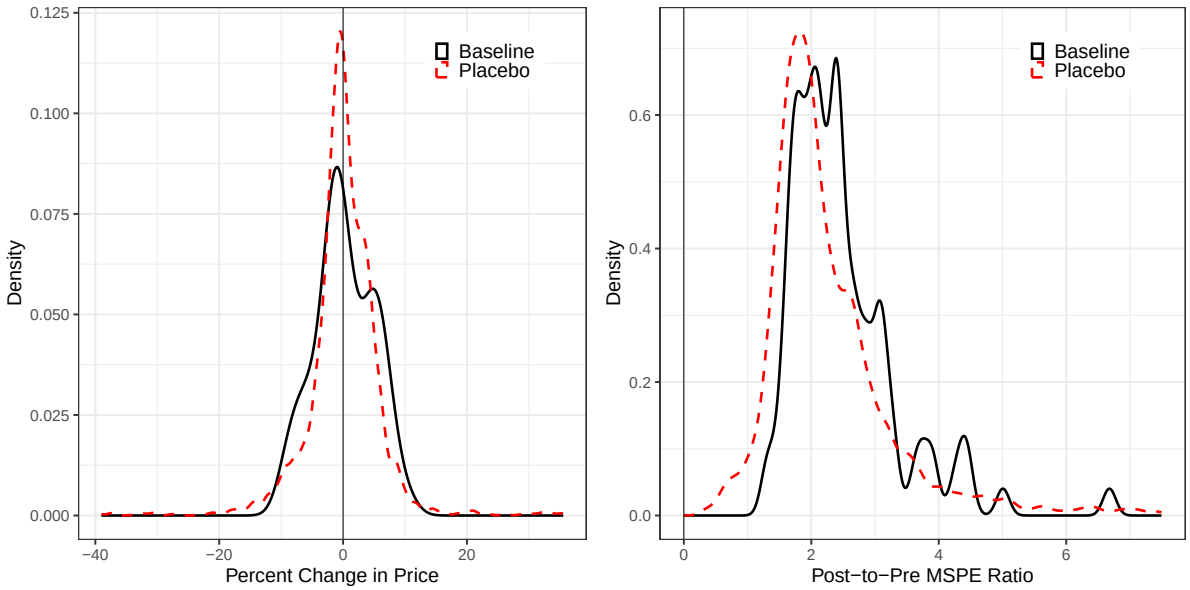


Figure C.1: Correlation between estimated and average brand trends

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 0.007, 0.096, 0.034, and 0.003, respectively.[4] Overall, this gives us confidence that placebo estimates from this procedure would be significantly closer to zero, despite their natural variation.

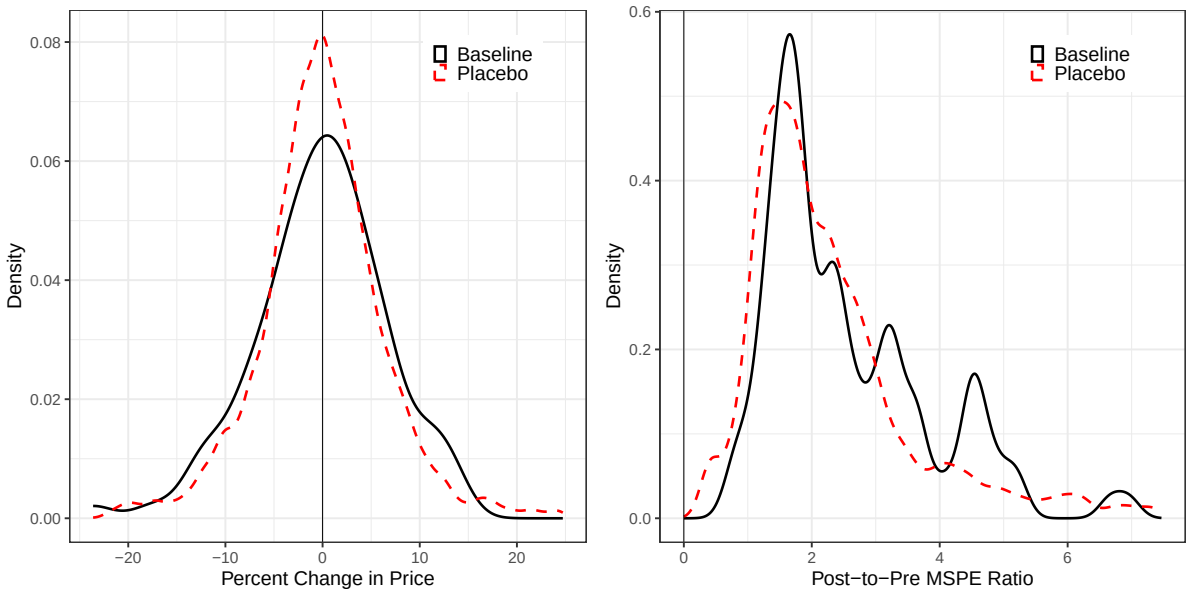
We also consider another metric to assess these placebo tests. For each date (true or placebo), we can estimate the model on the pre-period and compute the mean square prediction error (MSPE) of the model both before and after the merger. These will be positive, of course, as the model will not perfectly 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

[4]We also the studied the standard deviation. The p-values for this randomization test are much larger, as the placebo distribution has a larger standard deviation than the baseline. This is driven by outlier draws in the placebo distribution.



(a) Price effect distributions - Overall

(b) Post-to-pre MSPE ratio - Overall



(c) Price effect distributions - Merging

(d) Post-to-pre MSPE ratio - Merging

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

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 0.25. The p-value for the Kolmogorov-Smirnov test statistic on the MSPE distributions is below 0.05.

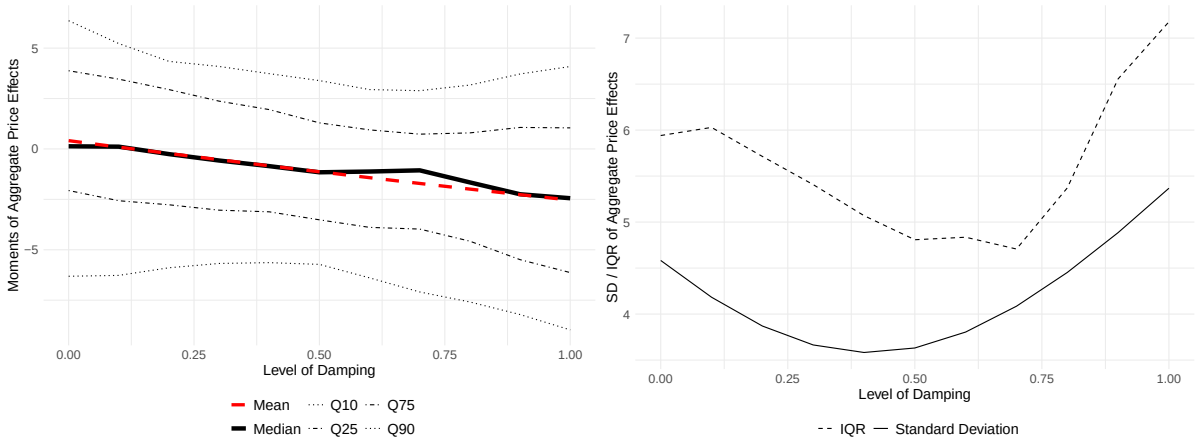
C.2. Robustness to Alternate Assumptions about the Trend

A potential concern about the strategy is that the trend estimated from the pre-period may overshoot the true counterfactual prices in the post-period. While we have provided some auxiliary evidence in defense of this identifying assumption, in this subsection we directly test the ramifications of alternate forecasting models. In particular, we consider the "damping" the trend from the pre-period back to a baseline level and reassess our main results.

We do so in two steps. We estimate (1) in the paper as is. When we estimate (2), we forecast $\widehat{\log y_{idt}}$ by damping the estimated brand level trends to $(1-\lambda)$ times the estimated brand trend plus λ times a baseline. The edge case $\lambda = 0$ would be our baseline of extrapolating the prices out using the estimated pre-trend at the brand level. The edge case $\lambda = 1$ would assume that at the time of the merger, the price trend would have flattened to our assumed baseline had it not been for the merger. We consider two potential baselines: (i) zero trend and (ii) a trend equal to the average trend in the data (across all mergers). For instance, $\lambda = 1$ with a baseline of zero would correspond to an extreme level of zero-reversion. Intermediate levels would would of course dampen the trend partially during forecasting.

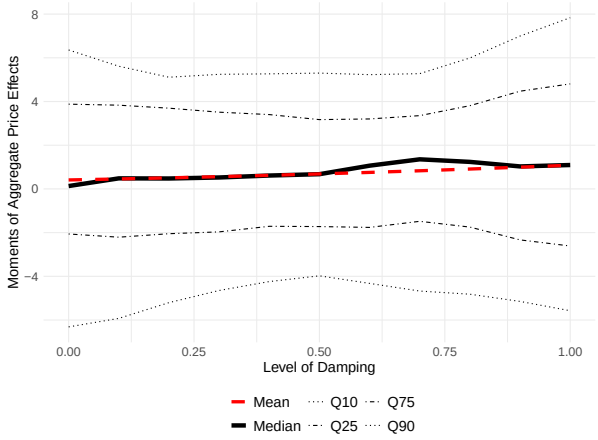
Of course, this procedure will mechanically change the estimated price effect of each merger. However, it does not have a mechanical impact on the distribution of price effects, how these price effects correlate with measures of market structure, or the estimates from the model. We investigate these in this section.

Figures C.3 shows properties of the distribution of aggregate price changes as a function of the level of damping. Focusing on Panel (a), we see that flattening the trend to zero after the merger (damping of 1) has a large impact on the mean and median price change. The mean aggregate price change after shrinking to zero is -2.5% . However, an important result is that the tails of the distribution do not shrink towards each other appreciably when moving from the baseline to full damping. We can see this in the behavior of the 10th and 90th quantiles in Panel (a) of each figure. Panel (b) shows that IQR and standard deviations increase for aggregate price changes, and the standard deviation of merging party price changes drops by about half a percentage point. Over the course of the entire range of λ, IQR for both price effects and the standard deviation of aggregate price effects do not drop by more than 1 pp from their baseline values; standard deviation of merging party price changes does drop by 2 pp when using λ around 0.5, but it still remains large. Our interpretation is the extrapolation of the trend is not responsible for the large width of the distribution, which is a salient feature of our results.

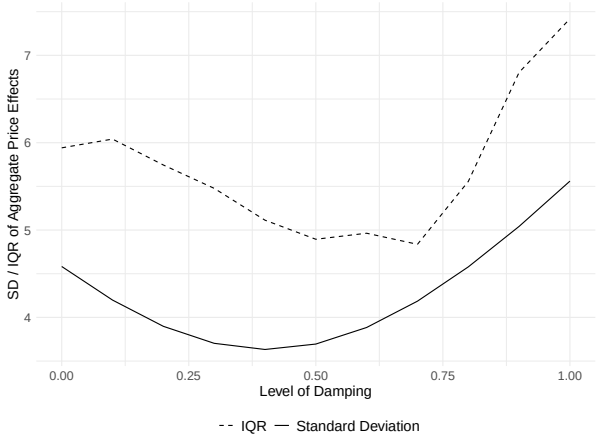


(a) Mean and quantiles, zero baseline

(b) Dispersion, zero baseline



(c) Mean and quantiles, average baseline



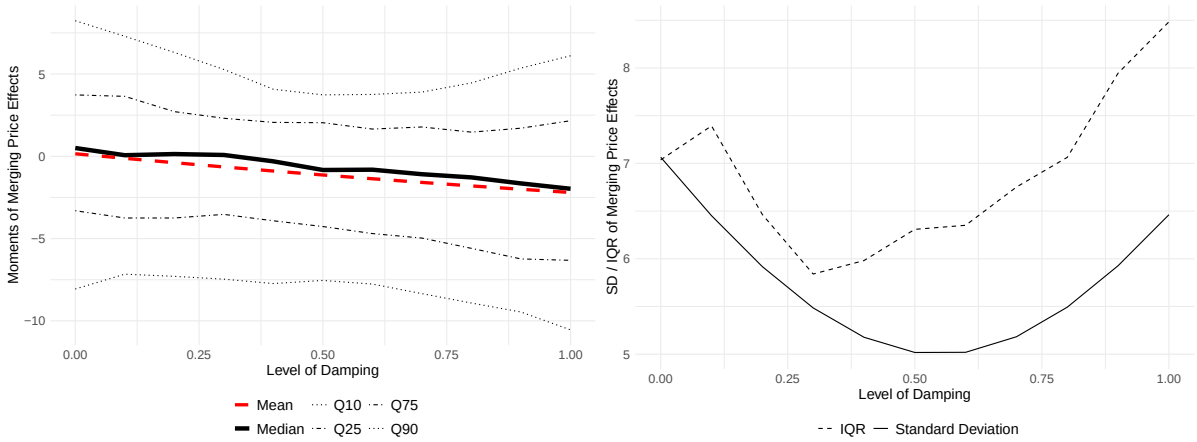
(d) Dispersion, average baseline

Figure C.3: Distribution of aggregate price changes, as a function of the damping parameter. Panels (a) and (b) use zero as the baseline, and Panels (c) and (d) use the average trend.

The average trend in our sample is negative, and thus damping to zero may not be the right baseline. Panels (c) and (d) investigate the effects of damping to the average trend, so that at $\lambda = 1$, the counterfactual price path is assumed to grow at the average trend in the dataset immediately after the merger. Here, we find that the effect on the mean is significantly small: even at $\lambda = 1$, the mean and median are at about 1 pp. Moreover, the large variance of the distribution survives across all levels of damping.

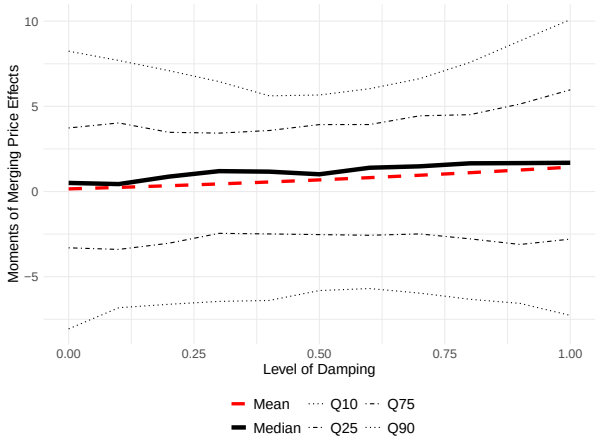
Figure C.4 repeats this exercise for merging party price changes, and we find very similar patterns. Damping affects the mean price change (and the magnitude of the change is more similar when damping to the mean versus damping to zero),[5] but the dispersion is not due to the trend.

[5]It is worth noting that we are not damping to the average merging trend in the data, as we wanted to keep the analysis transparent. This may explain why, unlike for aggregate effects, the magnitude of the effect for merging parties is not smaller when using the average as the baseline.

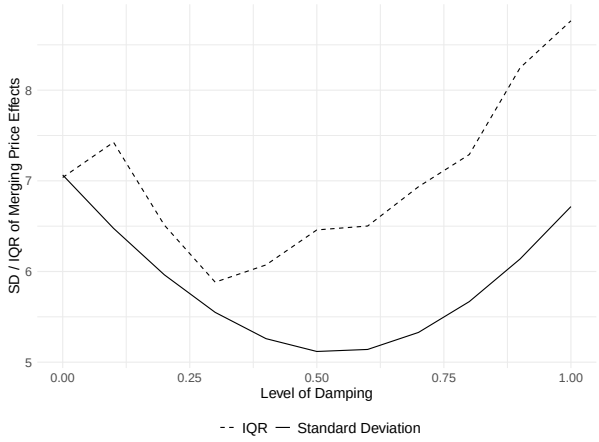


(a) Mean and quantiles, zero baseline

(b) Dispersion, zero baseline



(c) Mean and quantiles, average baseline

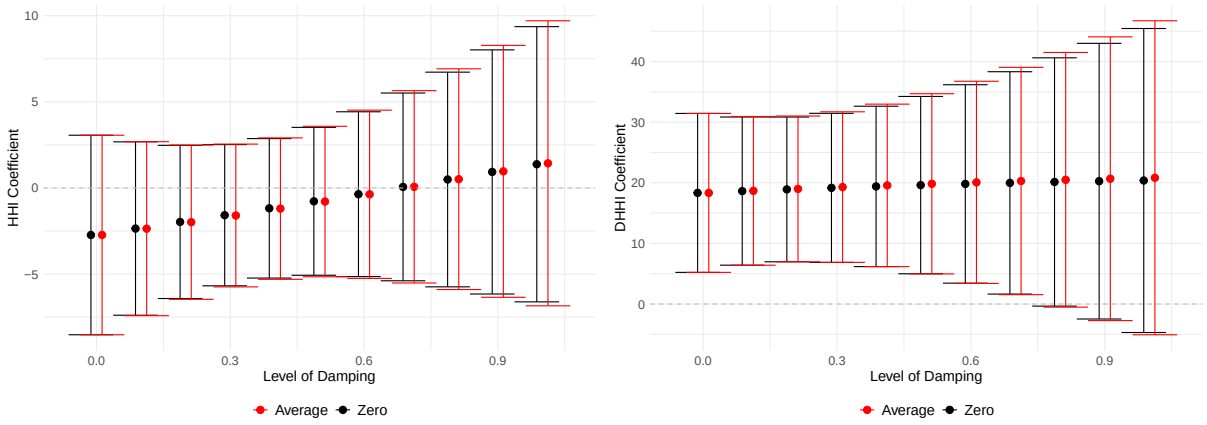


(d) Dispersion, average baseline

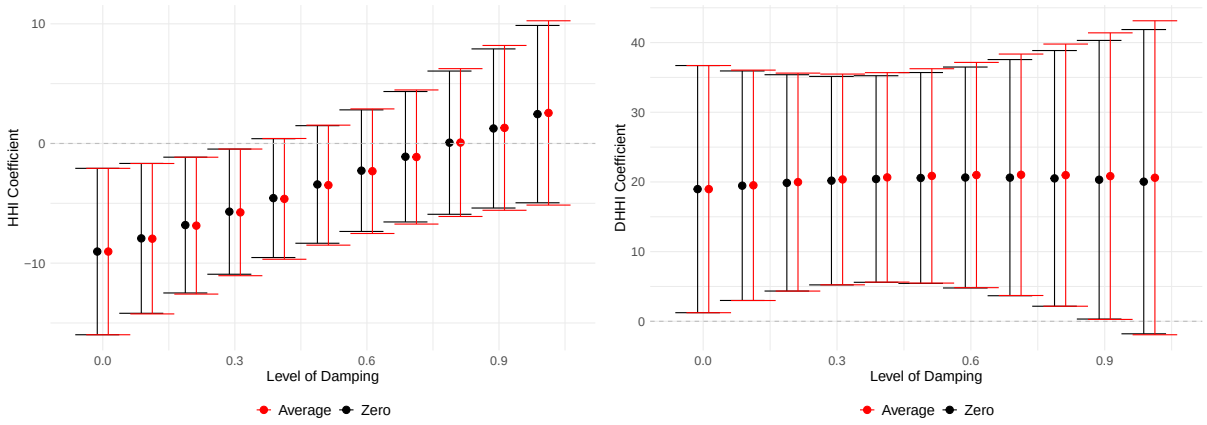
Figure C.4: Distribution of merging party price changes, as a function of the damping parameter. Panels (a) and (b) use zero as the baseline, and Panels (c) and (d) use the average trend.

We next turn to evaluating how the coefficients of the regressions of price changes on measures of market structure change. We focus on the analogues of specifications (1) and (4) in Table 6, as they are easier to illustrate in graphical format. Figure C.5 plots the coefficient on HHI and DHHI for (a) aggregate price effects and (b) merging price effects. We see that the coefficient on HHI increases with the level of damping, but it is not statistically significant for aggregate price changes throughout. Coefficients on DHHI are remarkably constant throughout the range of λ: the coefficients lose statistical significance at $\lambda = 0.8$ for both aggregate and merging party price changes, but this comes from lower precision. These results are consistent with the estimated trend being independent of DHHI.

Figure C.6 shows results from a regression of price effects on bins of merging share. The coefficient of large merging share on aggregate price changes is remarkably similar across levels of



(a) Aggregate Price Effects



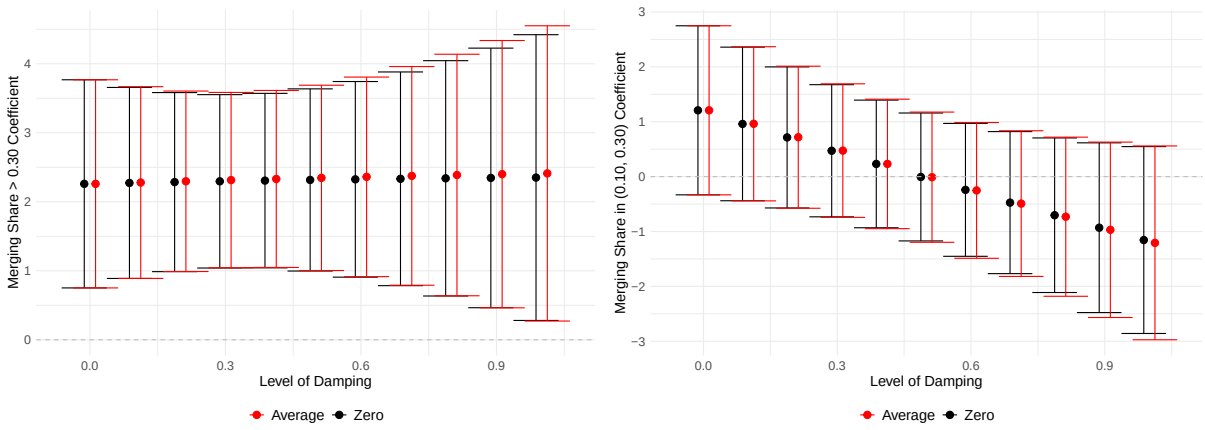
(b) Merging Price Effects

Figure C.5: Results of a regression of price effects on HHI and DHHI. Bars indicate 90% confidence intervals.

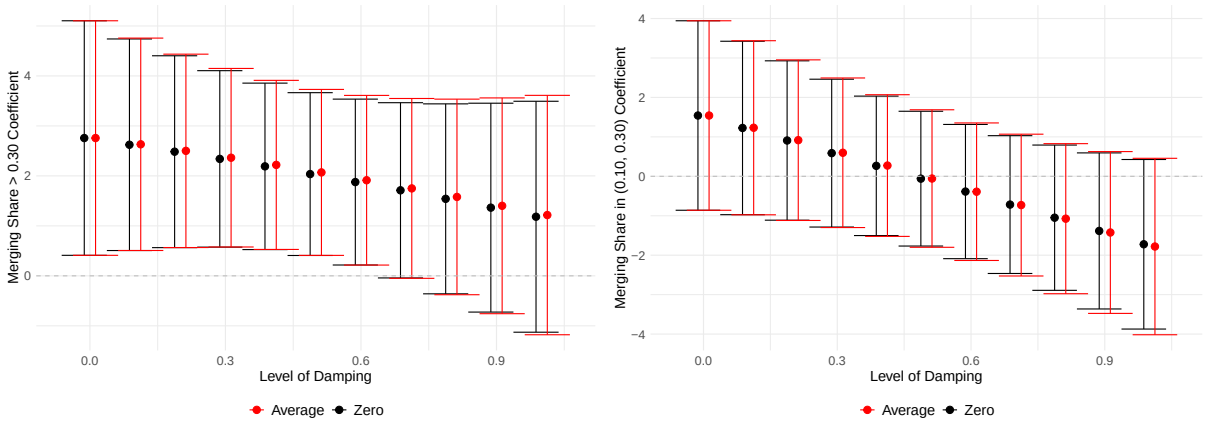
damping. For both merging and aggregate price changes, the coefficient of medium merging share remains statistically insignificant throughout the range of damping. We find that the coefficient of high merging share does decrease for merging parties as we dampen the trend.

Overall, our takeaway is that some of these results on market structure—especially for aggregate prices—are robust to the decision of how to dampen the trend. The results on merging party price changes are robust to up to large levels of damping the trend ($\lambda = 0.7$). Given that $\lambda = 1$ is an extreme benchmark, as we would not expect trends to have decayed to 0 or to the average immediately at the time of the merger, this robustness check gives us confidence that our results on market structure are not driven by the trend itself.

Finally, we re-estimate the model for different levels of λ. Figure C.7 plots moments of the threshold distribution. The threshold decreases with damping, by up to 2 pp for the aggregate threshold and up to 4 pp for merging parties'. Part of this is due to the leftward shift in the distribution



(a) Aggregate Price Effects

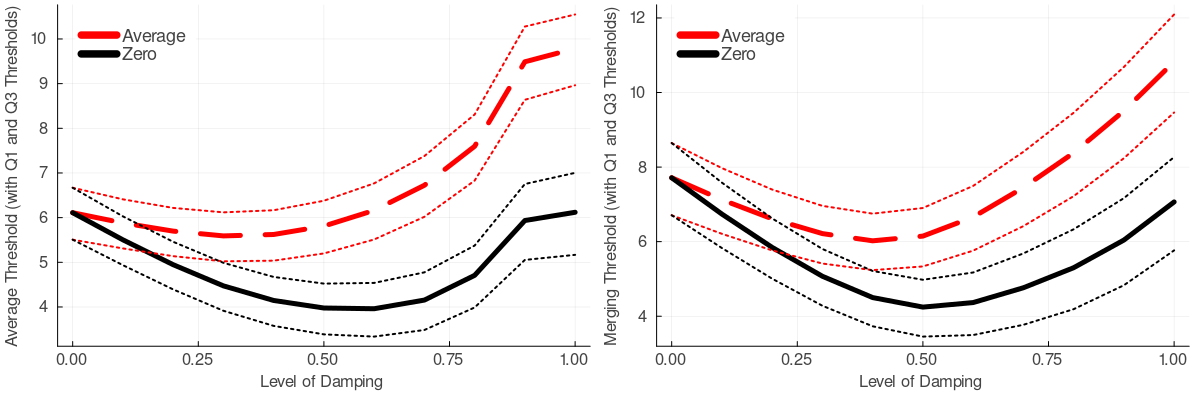


(b) Merging Price Effects

Figure C.6: Results of a regression of price effects on high merging share (> 0.3) and medium merging share (between 0.1 and 0.3). Bars indicate 90% confidence intervals.

documented in Figures C.3 and C.4 due to damping the trend towards 0 and the rightward shift (particular for merging parties) when damping to the average. Aside from a large increase in the estimated threshold when damping to the average and using values of λ above 0.8—which we find to be an extreme assumption anyway—we do not see a large economic difference between estimated thresholds for low and high levels of damping. Changes in the thresholds are generally within both the statistical and the model uncertainty (from different specifications) of our baseline estimates.

To summarize, our broad takeaway from this exercise is that despite the fact that damping the trend for forecasting affects the individual estimate for mergers, the majority of our results are robust to up to moderate levels of damping, using two separate implementations of the procedure. Some of our results are robust to all levels of damping. This was not mechanical, and it reflects that the trend itself is not driving our findings.



(a) Aggregate Threshold (b) Merging Threshold

Figure C.7: Average thresholds as a function of damping, using the baseline specification of the model (Columns (1) and (6) in Table 7). The dashed red line corresponds to damping to the average, and the solid black one corresponds to damping to the zero. Dotted lines are the Q1 and Q3 thresholds.

C.3. Additional Results Related to Price Effects

Table C.1 performs additional robustness tests on the distribution of estimated price effects documented in Section II.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 0.2–0.7 pp—and dispersion is also slightly larger (by approximately 0.1–0.2 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 1.2 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 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.[6] 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.8 illustrates this by displaying merger-level estimates and 95% confidence intervals for aggregate, merging-party, and non-merging-party price changes.

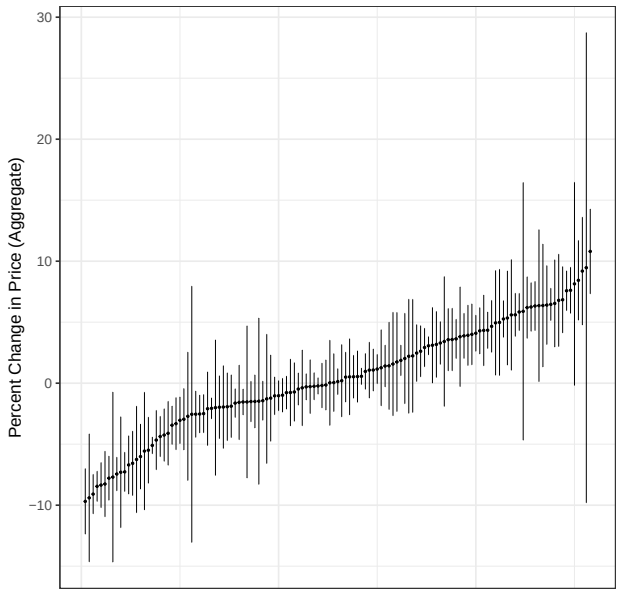
Finally, Panel E drops the mergers in our dataset where there was a divestiture. The difference with Panel A is minimal.

Figure C.9 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.

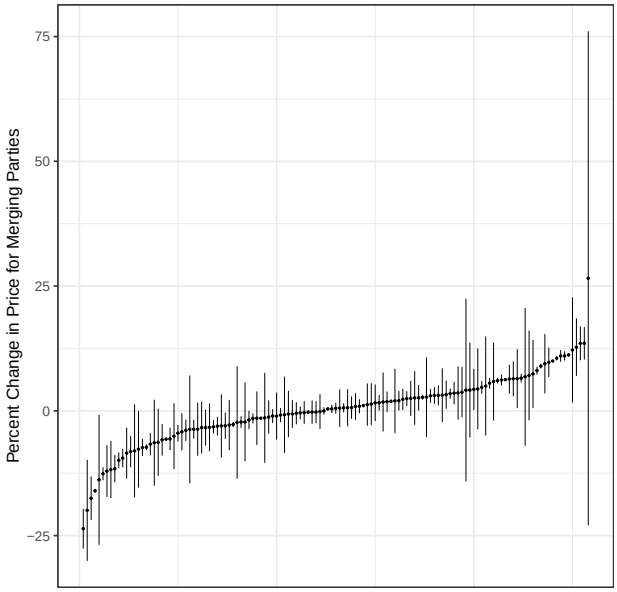
[6] For each merger, we run a chain with a burn-in of 5,000 iterations and draw 20,000 more iterations. We have checked that chains converge given the diagnostics reported by the package.

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	129	0.41	4.58	-2.06	0.13	3.88
		(0.40)		(0.38)	(0.52)	(0.50)
Merging Parties	129	0.15	7.06	-3.31	0.50	3.73
		(0.62)		(0.68)	(0.60)	(0.64)
Non-Merging Parties	129	0.59	4.82	-2.15	0.09	4.46
		(0.42)		(0.38)	(0.51)	(0.48)
B. Food Mergers Only						
Overall	75	1.11	4.87	-2.52	2.02	4.66
		(0.56)		(1.09)	(0.75)	(0.45)
Merging Parties	75	0.40	7.17	-3.02	0.92	6.04
		(0.83)		(0.90)	(0.95)	(1.22)
Non-Merging Parties	75	1.29	5.00	-2.15	2.10	4.88
		(0.58)		(0.95)	(1.06)	(0.41)
C. Brand Level						
Overall	129	-0.53	7.56	-2.76	-0.44	3.70
		(0.67)		(0.34)	(0.51)	(0.67)
Merging Parties	129	0.30	9.06	-4.21	-0.06	4.16
		(0.80)		(1.09)	(0.62)	(0.67)
Non-Merging Parties	129	-0.57	7.73	-3.23	-0.53	3.75
		(0.68)		(0.59)	(0.46)	(0.79)
D. Bayesian Shrinkage						
Overall	129	0.30	3.94	-1.76	0.13	3.29
		(0.35)		(0.36)	(0.41)	(0.48)
Merging Parties	129	0.04	5.82	-2.86	0.49	3.03
		(0.51)		(0.69)	(0.53)	(0.38)
Non-Merging Parties	129	0.40	4.02	-1.93	0.11	3.37
		(0.35)		(0.42)	(0.43)	(0.54)
E. Mergers without Divestitures						
Overall	123	0.34	4.67	-2.49	0.03	4.00
		(0.42)		(0.52)	(0.55)	(0.65)
Merging Parties	123	0.06	7.19	-3.37	0.39	4.14
		(0.65)		(0.66)	(0.67)	(0.68)
Non-Merging Parties	123	0.52	4.91	-2.25	-0.13	4.47
		(0.44)		(0.35)	(0.45)	(0.52)

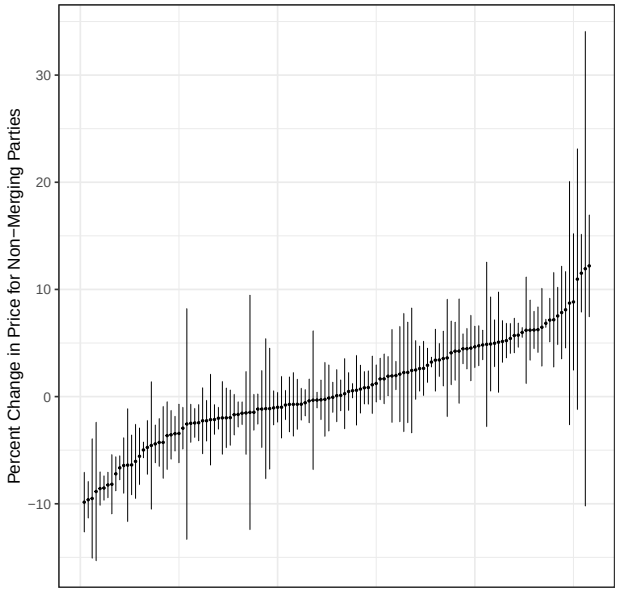
Table C.1: Robustness of Price Effects. This table displays the distribution of transformed coefficient estimates of (2) (e.g., $100 \cdot (\exp(\hat{\beta}_1) - 1)$) for overall, merging-party, and non-merging-party price changes. Standard errors are in parentheses. Panel A displays the baseline results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



(a) Aggregate

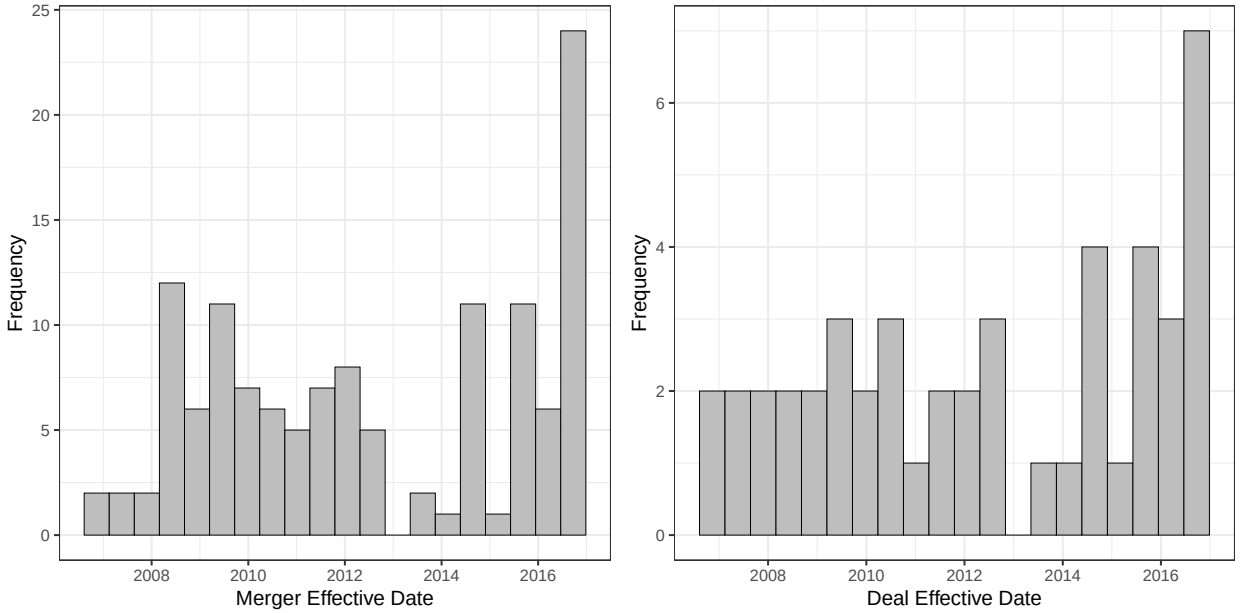


(b) Merging



(c) Non-Merging

Figure C.8: Merger-level estimates and confidence intervals. Panel (a) displays aggregate price changes, Panel (b) displays merging party price changes, and Panel (c) displays non-merging party price changes. 95% confidence intervals are calculated using standard errors two-way clustered by brand and DMA.



(a) Merger Level (b) Deal Level

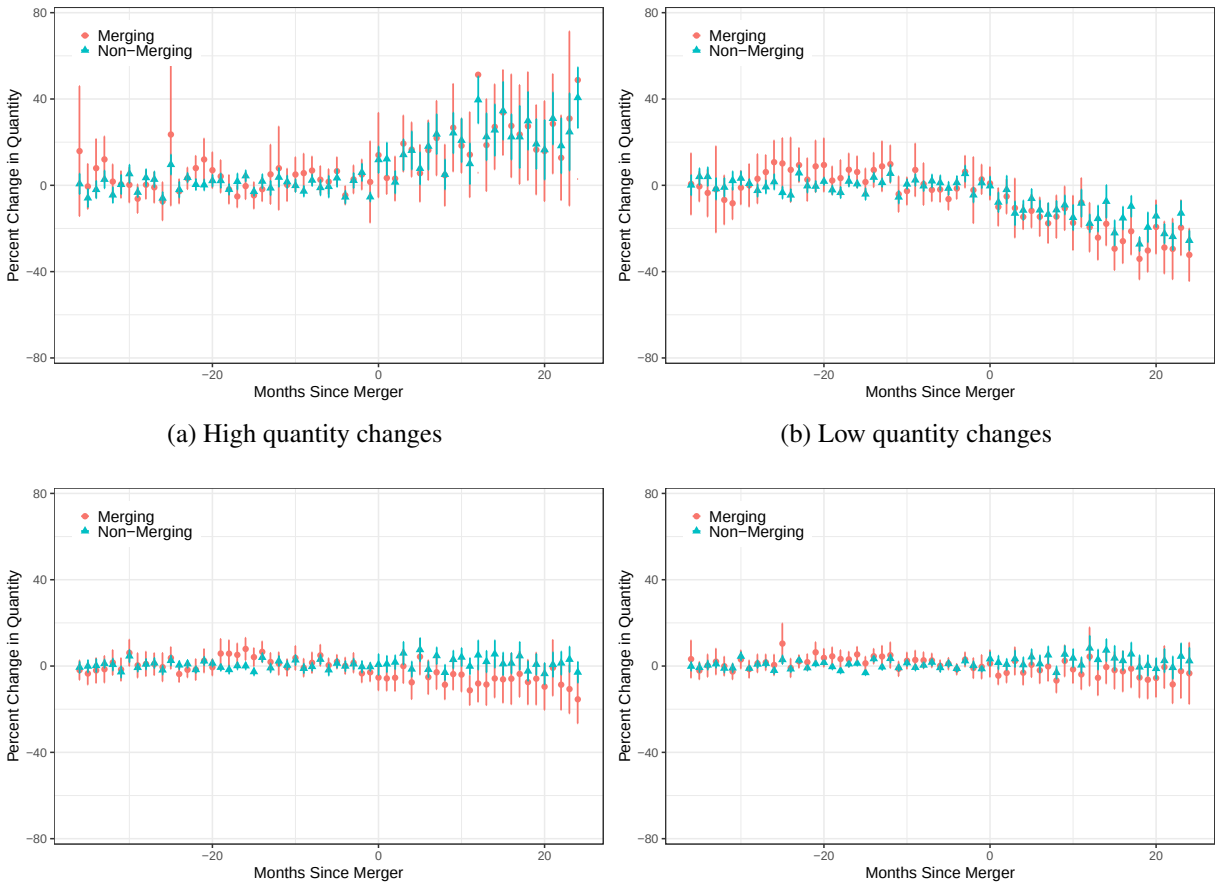
Figure C.9: Timing of mergers. These plots display histograms of the dates at which mergers became effective. The unit of observation of the left-hand panel is a merger, while the unit of observation of the right-hand panel is a deal.

C.4. Additional Results Related to Quantity Effects

Figure C.10 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 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 II.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



(a) High quantity changes

(b) Low quantity changes

(c) Stable quantities

(d) All quantity changes

Figure C.10: 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.

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 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 parties).

	N	Mean	S.D.	25th Pct.	Median	75th Pct.
A. Baseline						
Overall	129	-0.26	15.63	-9.71	-1.74	5.80
		(1.38)		(1.19)	(0.94)	(1.94)
Merging Parties	129	-6.28	32.53	-20.86	-8.49	5.53
		(2.86)		(2.79)	(2.23)	(3.10)
Non-Merging Parties	129	0.99	16.88	-7.81	-1.46	5.93
		(1.49)		(1.66)	(0.82)	(1.76)
B. Food Mergers Only						
Overall	75	-0.00	11.78	-8.16	-0.82	6.80
		(1.36)		(1.93)	(1.19)	(2.10)
Merging Parties	75	-2.61	34.88	-19.97	-5.52	12.83
		(4.03)		(2.89)	(3.71)	(6.11)
Non-Merging Parties	75	1.52	14.15	-6.31	-1.13	7.24
		(1.63)		(2.00)	(1.18)	(2.42)
C. Bayesian Shrinkage						
Overall	129	-0.27	15.53	-9.70	-1.74	5.78
		(1.37)		(1.13)	(0.93)	(1.97)
Merging Parties	129	-6.84	30.59	-20.01	-8.49	5.22
		(2.69)		(2.65)	(2.19)	(2.77)
Non-Merging Parties	129	0.95	16.63	-7.76	-1.46	5.91
		(1.46)		(1.65)	(0.82)	(1.74)
D. Mergers without Divestitures						
Overall	123	-0.09	15.95	-9.87	-1.70	6.80
		(1.44)		(1.43)	(0.95)	(1.92)
Merging Parties	123	-6.31	33.29	-21.44	-9.23	6.33
		(3.00)		(2.90)	(2.22)	(3.60)
Non-Merging Parties	123	1.12	17.25	-8.01	-1.46	7.05
		(1.56)		(1.71)	(0.84)	(2.06)

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.

C.5. Additional Results Related to Antitrust Enforcement

Figure C.11 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 III.B.

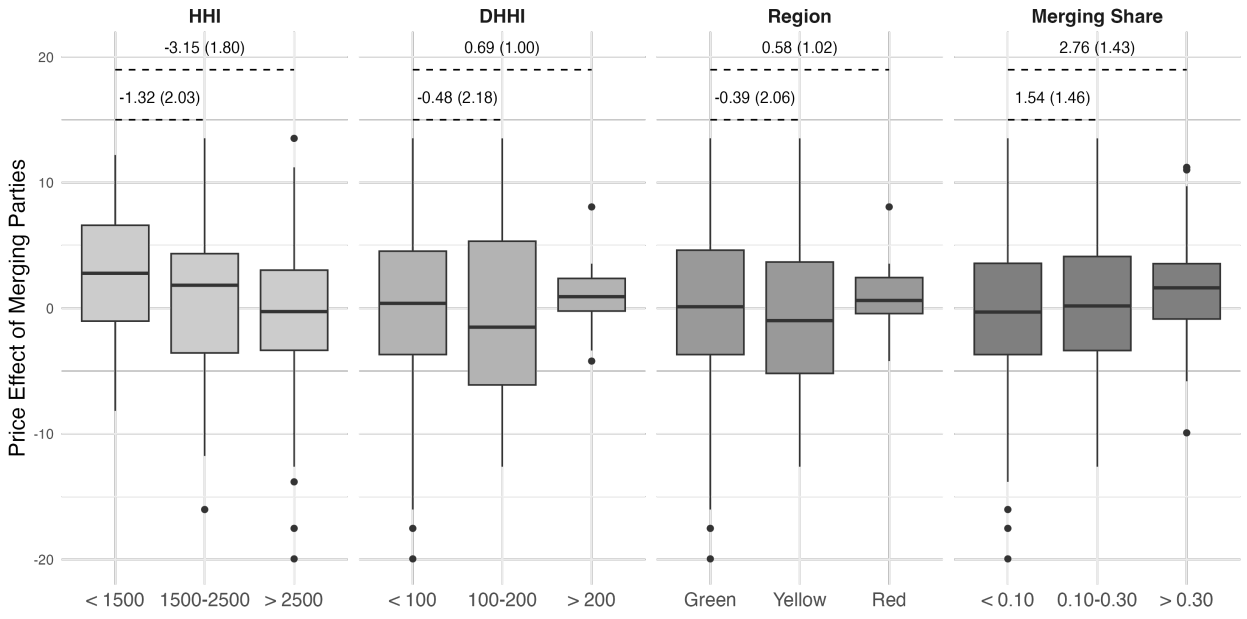
	Merging Quantity			Non-Merging Quantity		
	(1)	(2)	(3)	(4)	(5)	(6)
Merging Price Effect	-0.25	-0.50	-0.95			-0.10
	(0.50)	(0.29)	(0.29)			(0.25)
Merging Brands Effect		0.44	0.51			0.11
		(0.19)	(0.24)			(0.12)
Merging Stores Effect		0.75	0.77			-0.11
		(0.16)	(0.17)			(0.11)
Non-Merging Price Effect			1.23	-0.01	0.12	0.18
			(0.49)	(0.27)	(0.28)	(0.35)
Non-Merging Brands Effect			-0.35		0.28	0.18
			(0.34)		(0.21)	(0.14)
Non-Merging Stores Effect			0.68		1.36	1.44
			(0.60)		(0.47)	(0.48)
Constant	-6.25	-3.11	-4.75	1.00	1.78	1.39
	(2.87)	(2.47)	(2.32)	(1.49)	(1.75)	(1.54)
Observations	129	129	129	129	129	129
R^2	0.003	0.402	0.438	0.000	0.124	0.143

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.

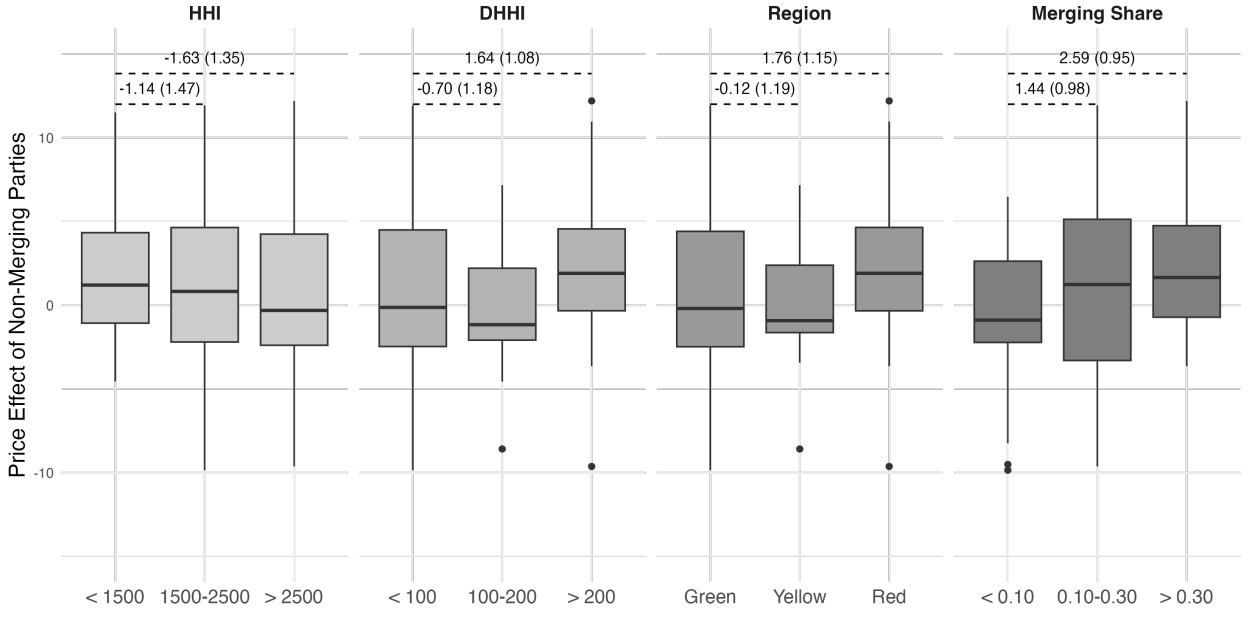
We now turn to analysis of robustness. We first study robustness of the analysis of the structural presumptions. Table C.6 replicates Table 6 but uses nationwide HHI and DHHI as the metrics for market structure, rather than the average of DMA-level HHI and DHHI. While there are some differences that 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.12 replicates Figure 8 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 DHHI and merging share within merger is not driven by mergers with divestitures.



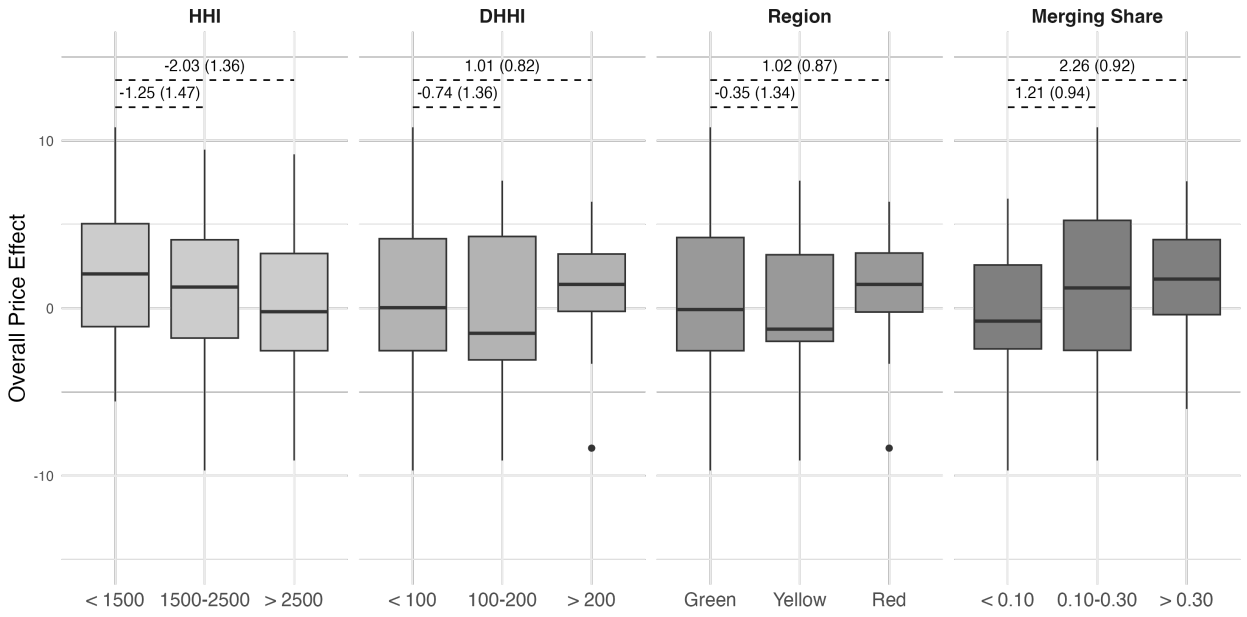
(a) Merging Parties



(b) Non-Merging Parties

Figure C.11: 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.

Finally, recall that in Appendix A.3, we present both cross- and within-merger correlations with



(c) Aggregate Parties

Figure C.11: (Continued)

structural presumptions when using the Consumer Panel. We refer 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 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, 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 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.13 reports Figure 10 but adds 95% confidence intervals as dotted lines. The main takeaway that reducing the threshold will limited impact on type I errors holds.

Finally, Figure C.14 reruns the counterfactual analysis of Section IV.D but uses merging party price changes as the outcome of interest—following the specification in Column (6) of Table 7.

	(1)	(2)	(3)	(4)	(5)	(6)
HHI (0–1)	-2.73					
	(3.52)					
DHHI (0–1)	18.33					
	(7.97)					
HHI $\in$ [1500, 2500]		-1.47				
		(1.45)				
HHI $>$ 2500		-2.56				
		(1.38)				
DHHI $\in$ [100, 200]		-0.81				
		(1.37)				
DHHI $>$ 200		1.58				
		(0.88)				
Yellow			-0.35			
			(1.34)			
Red			1.02			
			(0.87)			
Merg Share $\in$ [0.10, 0.30]				1.21		
				(0.94)		
Merg Share $>$ 0.30				2.26		5.36
				(0.92)		(1.42)
2023 Presumptions					0.47	
					(0.83)	
HHI $>$ 1800						-2.60
						(1.08)
DHHI $>$ 100						-2.78
						(2.83)
HHI $>$ 1800						3.77
$\times$ DHHI $>$ 100						(3.12)
Merg Share $>$ 0.30						-4.76
$\times$ DHHI $>$ 100						(1.99)
Constant	0.95	2.22	0.28	-0.50	0.28	2.02
	(1.07)	(1.25)	(0.50)	(0.55)	(0.50)	(0.89)
Observations	129	129	129	129	129	129
R^2	0.019	0.040	0.008	0.035	0.002	0.084

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

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 21% of blocked mergers would be a type I error (respectively), compared to 5% and 19% when using aggregate price changes. These larger numbers can be traced back to our estimates that agencies have a less precise signal of merging party price changes.

	Green				Yellow			Red
	LM	LH	ML	HL	MM	MH	HM	HH
LL	1.19	2.62	0.63	0.71	0.27	2.23	0.99	1.83
	(0.77)	(0.91)	(0.43)	(0.60)	(0.54)	(0.70)	(0.62)	(0.73)
LM		1.43	-0.56	-0.47	-0.92	1.04	-0.19	0.65
		(0.83)	(0.83)	(0.93)	(0.86)	(0.95)	(0.93)	(1.00)
LH			-1.99	-1.91	-2.35	-0.39	-1.63	-0.79
			(0.96)	(1.04)	(0.98)	(0.92)	(1.03)	(1.08)
ML				0.08	-0.36	1.60	0.36	1.20
				(0.42)	(0.43)	(0.63)	(0.49)	(0.63)
HL					-0.45	1.51	0.28	1.12
					(0.52)	(0.69)	(0.36)	(0.52)
MM						1.96	0.73	1.56
						(0.50)	(0.46)	(0.62)
MH							-1.23	-0.39
							(0.59)	(0.63)
HM								0.84
								(0.38)

(a) Merging parties (HHI-DHHI)

	Green				Yellow			Red
	LM	LH	ML	HL	MM	MH	HM	HH
LL	-0.66	-0.79	0.09	0.36	0.40	1.01	1.23	1.46
	(0.39)	(0.73)	(0.29)	(0.39)	(0.48)	(0.68)	(0.53)	(0.59)
LM		-0.13	0.75	1.02	1.05	1.67	1.89	2.12
		(0.68)	(0.43)	(0.49)	(0.56)	(0.67)	(0.59)	(0.64)
LH			0.88	1.15	1.19	1.81	2.02	2.25
			(0.75)	(0.79)	(0.83)	(0.96)	(0.82)	(0.89)
ML				0.27	0.31	0.92	1.14	1.37
				(0.20)	(0.38)	(0.61)	(0.42)	(0.49)
HL					0.04	0.66	0.87	1.10
					(0.38)	(0.63)	(0.44)	(0.51)
MM						0.62	0.84	1.07
						(0.61)	(0.51)	(0.50)
MH							0.22	0.45
							(0.59)	(0.56)
HM								0.23
								(0.31)

(b) Non-merging parties (HHI-DHHI)

	ML	MM	MH	HL	HM	HH
LL	1.48	1.41	2.14	2.09	1.96	2.87
	(0.60)	(0.63)	(0.69)	(0.80)	(0.71)	(0.80)
ML		-0.07	0.66	0.61	0.49	1.39
		(0.39)	(0.48)	(0.52)	(0.42)	(0.57)
MM			0.73	0.68	0.55	1.45
			(0.38)	(0.58)	(0.39)	(0.51)
MH				-0.06	-0.18	0.72
				(0.60)	(0.39)	(0.34)
HL					-0.12	0.78
					(0.45)	(0.59)
HM						0.90
						(0.40)

(c) Merging parties (Merging Share-DHHI)

	ML	MM	MH	HL	HM	HH
LL	-0.16	0.15	0.44	-0.33	0.37	0.86
	(0.26)	(0.39)	(0.56)	(0.61)	(0.46)	(0.54)
ML		0.32	0.60	-0.17	0.54	1.03
		(0.29)	(0.50)	(0.58)	(0.40)	(0.49)
MM			0.28	-0.48	0.22	0.71
			(0.40)	(0.62)	(0.36)	(0.45)
MH				-0.77	-0.06	0.43
				(0.83)	(0.51)	(0.59)
HL					0.71	1.20
					(0.59)	(0.71)
HM						0.49
						(0.36)

(d) Non-merging parties (Merging Share-DHHI)

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.

	Merging Party Price Changes						Non-Merging Party Price Changes					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
HHI (0–1)	-10.03						-4.41					
	(3.92)						(4.04)					
DHHI (0–1)	20.41						27.59					
	(9.36)						(9.68)					
HHI $\in$ [1500, 2500]		0.10						-0.88				
		(1.75)						(1.27)				
HHI $>$ 2500		-3.51						-2.18				
		(1.73)						(1.19)				
DHHI $\in$ [100, 200]		1.48						0.45				
		(1.97)						(1.04)				
DHHI $>$ 200		2.25						2.46				
		(1.15)						(1.10)				
Yellow			1.35						0.72			
			(1.72)						(0.92)			
Red			0.13						1.92			
			(0.98)						(1.29)			
Merg Share $\in$ [0.10, 0.30]				1.54						1.44		
				(1.46)						(0.98)		
Merg Share $>$ 0.30				2.76		6.57				2.59		4.63
				(1.43)		(2.46)				(0.95)		(1.24)
2023 Presumptions					0.49						1.23	
					(1.14)						(0.92)	
HHI $>$ 1800						-0.83						-1.07
						(1.51)						(1.05)
DHHI $>$ 100						4.97						0.88
						(2.59)						(1.22)
HHI $>$ 1800						-4.35						0.92
$\times$ DHHI $>$ 100						(2.97)						(1.74)
Merg Share $>$ 0.30						-5.93						-4.50
$\times$ DHHI $>$ 100						(2.94)						(2.00)
Constant	2.42	1.26	-0.03	-0.98	0.03	0.04	1.27	1.46	0.23	-0.47	0.29	0.68
	(1.12)	(1.30)	(0.80)	(1.00)	(0.78)	(0.99)	(1.06)	(1.02)	(0.51)	(0.54)	(0.50)	(0.81)
Observations	129	129	129	129	129	129	129	129	129	129	129	129
R^2	0.030	0.064	0.004	0.022	0.001	0.054	0.044	0.048	0.020	0.043	0.012	0.054

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.

C.6. Comparison of Predicted Price Effects of Computed Thresholds

As mentioned in Section IV.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 Table C.9.[7] When expert analysis is not publicly

[7]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.

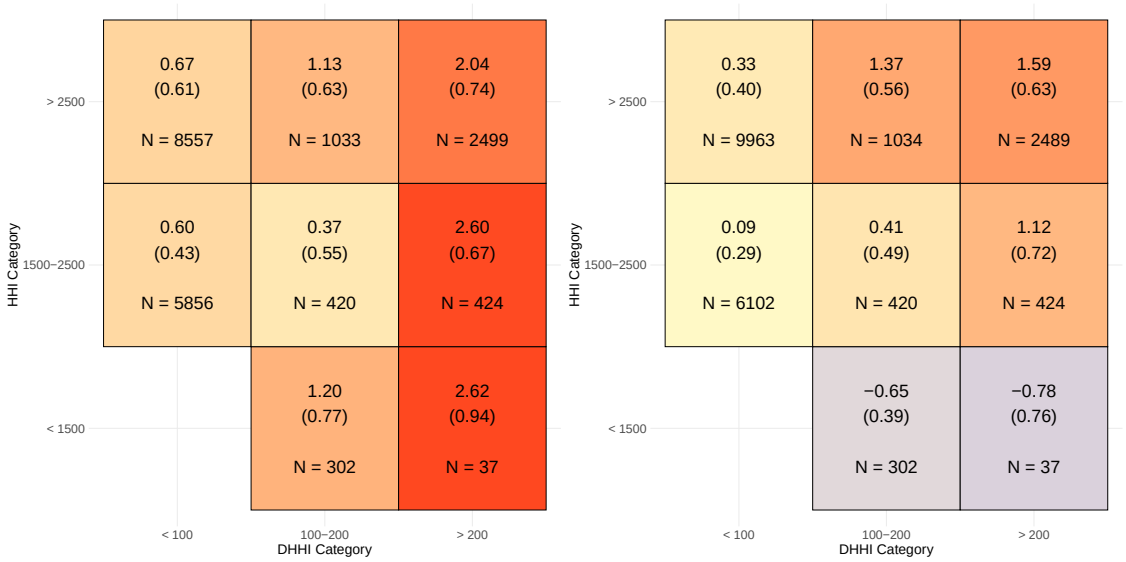
	Merging Party Price Changes						Non-Merging Party Price Changes					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
HHI (0–1)	-9.27						-2.58					
	(4.34)						(3.58)					
DHHI (0–1)	21.09						30.76					
	(12.23)						(11.11)					
HHI $\in$ [1500, 2500]		-1.46						-1.38				
		(1.99)						(1.46)				
HHI $>$ 2500		-3.70						-2.33				
		(1.89)						(1.36)				
DHHI $\in$ [100, 200]		-0.44						-0.76				
		(2.12)						(1.22)				
DHHI $>$ 200		1.19						2.06				
		(1.15)						(1.31)				
Yellow			-0.39						-0.12			
			(2.06)						(1.20)			
Red			0.02						1.70			
			(1.04)						(1.42)			
Merg Share $\in$ [0.10, 0.30]				1.47						1.29		
				(1.53)						(1.04)		
Merg Share $>$ 0.30				2.58		6.65				2.75		4.80
				(1.46)		(2.44)				(1.00)		(1.20)
2023 Presumptions					-0.17						0.85	
					(1.24)						(1.02)	
HHI $>$ 1800						-1.69						-2.79
						(1.61)						(1.12)
DHHI $>$ 100						-0.42						-3.80
						(5.21)						(1.83)
HHI $>$ 1800						0.33						4.67
$\times$ DHHI $>$ 100						(5.60)						(2.53)
Merg Share $>$ 0.30						-5.49						-3.42
$\times$ DHHI $>$ 100						(3.20)						(2.26)
Constant	2.55	2.57	0.10	-0.98	0.10	1.01	0.89	2.11	0.31	-0.47	0.31	2.25
	(1.37)	(1.56)	(0.81)	(1.00)	(0.81)	(1.22)	(1.11)	(1.24)	(0.51)	(0.54)	(0.51)	(0.92)
Observations	123	123	123	123	123	123	123	123	123	123	123	123
R^2	0.030	0.033	0.000	0.019	0.000	0.038	0.036	0.038	0.014	0.043	0.006	0.083

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.

available, we have found situations where the agencies cite intended price increases from internal firm documents.

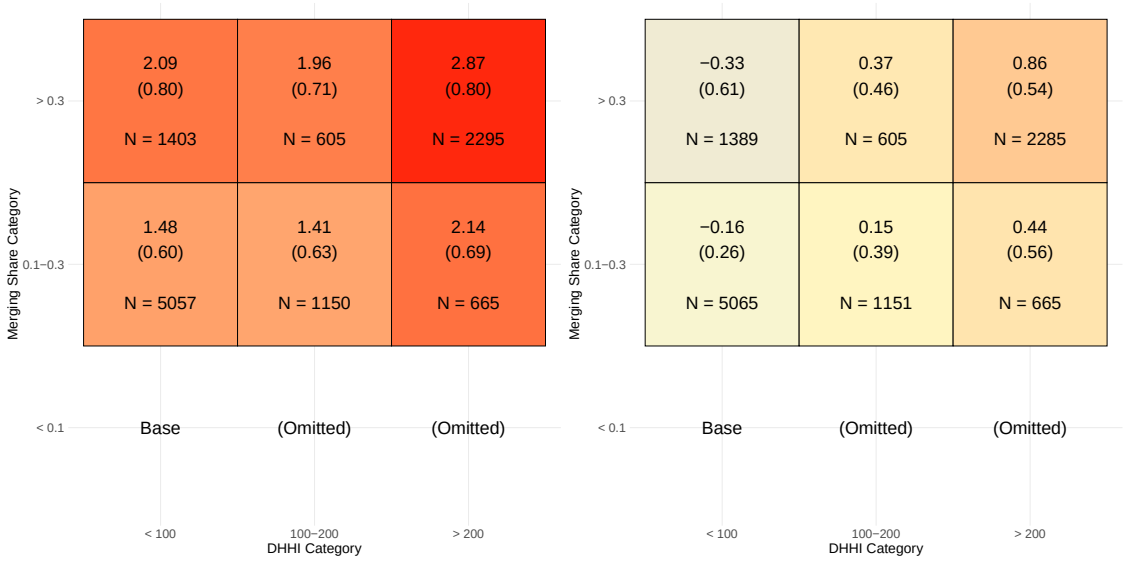
Our threshold depends on market size, proxied by the total annual sales of the merging parties. In some of these cases, the agency documents themselves have estimates of total sales or revenues in the relevant markets (e.g., US Sugar / Imperial and JetBlue / Spirit), and in others we can infer it from 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.[8]

[8]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.



(a) Merging (HHI-DHHI)

(b) Non-Merging (HHI-DHHI)



(c) Merging (Merging Share-DHHI)

(d) Non-Merging (Merging Share-DHHI)

Figure C.12: 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.

As mentioned in Section IV.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

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.

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

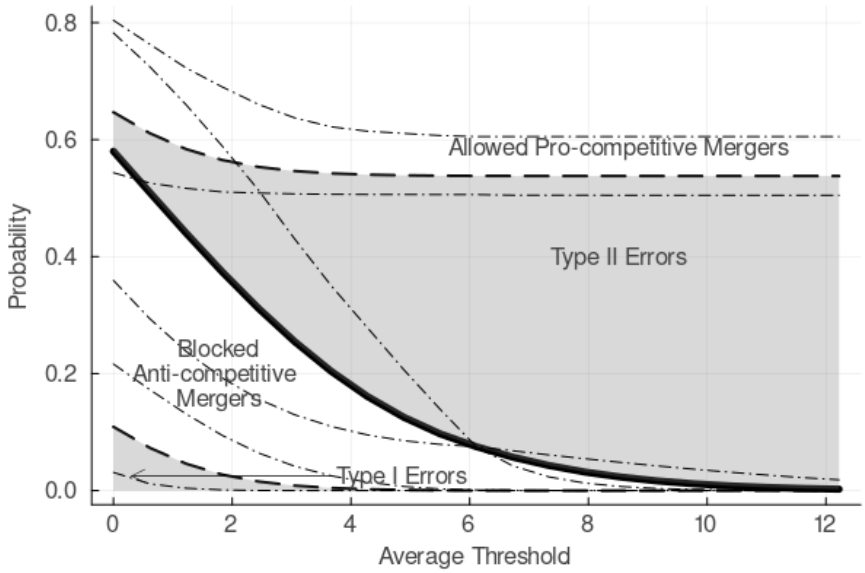
Table C.8: Correlates of Enforcement. Coefficients indicate output from a linear probability model of enforcement on merger-level market structure. HHI and DHHI as measured as the average across all DMAs within a merger. Robust standard errors are in parentheses.

(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.[9] 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.[10] 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.

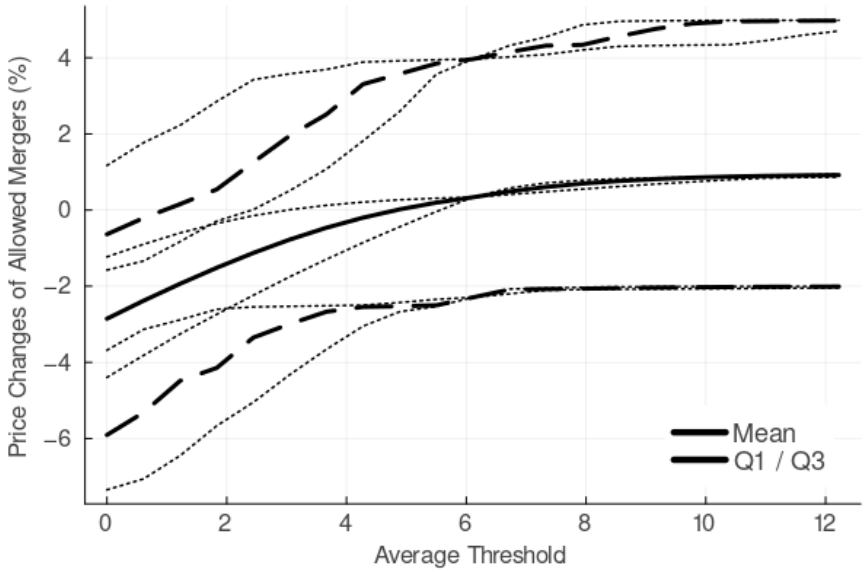
Of course, these results must be interpreted with some caution. The industries are very different

[9]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.

[10]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.



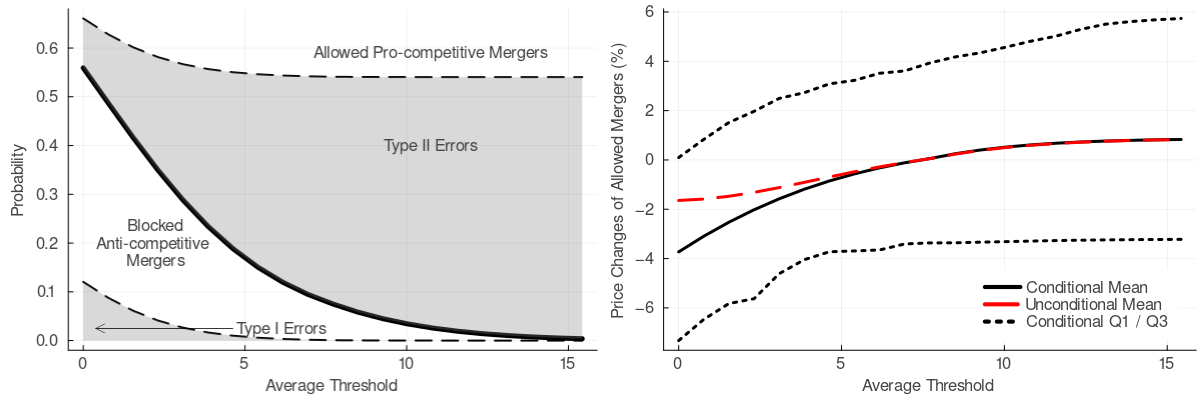
(a) Probability of challenges



(b) Price changes of consummated mergers

Figure C.13: 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)).

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 some of the price effects to drop below our thresholds. These mergers are also



(a) Probability of challenges

(b) Price changes of consummated mergers

Figure C.14: Outcomes of counterfactual thresholds. This replicates Figure 10, but uses merging party price changes as the outcome variable of interest.

				Threshold (%)	
Merging Parties	Year	Market Size	Predicted Price Effects	Aggregate	Merging
H&R Block / TaxACT	2011	$3B	2.2–2.5% (H&R); 10.5–12.2% (TaxACT)	4.92 (1.95)	5.73 (2.23)
GE / Electrolux	2015	$2.7B	5–11% (ranges); 3–21% (cooktops); 4–15% (wall ovens), all for merging parties	4.98 (1.92)	5.83 (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	3.26 (2.90)	2.97 (3.53)
Energy Solutions / WCS	2017	$100M	15% (from internal documents) for merging parties	6.85 (1.44)	8.95 (1.93)
AT&T / Time Warner [a]	2018	$68B	27¢ per month (10–50¢) on a typical cable bill, for merging parties	3.15 (2.97)	2.79 (3.63)
		$3.6B	16% in carriage fees charged by merging parties	4.82 (2.00)	5.56 (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	5.44 (1.71)	6.60 (1.96)
Penguin Random House / Simon & Schuster	2022	$490M	4.3–11.6% (up to 7.3–19.2%) for merging parties	5.95 (1.54)	7.44 (1.82)
JetBlue / Spirit	2023	$9.3B	30% (from internal documents) for merging parties	4.28 (2.29)	4.67 (2.68)

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.

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

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 effectts, 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 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.

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 I.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.[11]

As described in Section I.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 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.

[11] 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.

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.[12] 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 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.[13] 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

[12] 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.

[13] 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.

them into one market.[14] 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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BÜRKNER, P.-C. (2017): "brms: An R Package for Bayesian Multilevel Models using Stan," *Journal of Statistical Software*, 80, 1–28.

[14] 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 Product Group	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, Gravies, And Sauces	Cooking Sauce	Sugar, Tomatoes, Corn
22	Condiments, Gravies, And Sauces	Fish & Seafood & Cocktail Sauce	Tomatoes, Mayonnaise And Dressing, Shrimp, Unprocessed Finfish, Pickles And Horseradish
23	Condiments, Gravies, And Sauces	Meat Sauce, Worcestershire Sauce	Beef And Veal, Tomatoes, Vinegar
24	Condiments, Gravies, And Sauces	Mustard	Vinegar, Salt Pepper Spices
25	Condiments, Gravies, And Sauces	Sauce & Seasoning Mix-Remaining	Salt Pepper Spices, Spices, Vinegar, Dry Onions, Tomatoes
26	Condiments, Gravies, And Sauces	Sauce Mix - Spaghetti	Salt Pepper Spices, Tomatoes
27	Condiments, Gravies, And Sauces	Sauce Mix - Taco, Sauce & Seasoning Mix-Remaining Mexican	Salt Pepper Spices, Spices, Vinegar, Dry Onions, Tomatoes
28	Condiments, Gravies, And Sauces	Seasoning Mix - Chili	Salt Pepper Spices, Tomatoes, Spices, Vinegar, Dry Onions
29	Condiments, Gravies, And Sauces	Seasoning Mix - Sloppy Joe	Salt Pepper Spices, Tomatoes

Market	NielsenIQ Product Group	NielsenIQ Product Modules in Product Market	Cost Controls
30	Cookies	Cookies	Wheat Flour, Cocoa Beans, Sugar, Oats
31	Cosmetics	Cosmetic Kits	Fatty Acids, Starch Vegetable Fats Oils
32	Cosmetics	Cosmetics - Concealers	Fatty Acids, Starch Vegetable Fats Oils
33	Cosmetics	Cosmetics-Blushers	Fatty Acids, Starch Vegetable Fats Oils
34	Cosmetics	Cosmetics-Eye Shadows	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 Product Group	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 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	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, 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 Product Group	NielsenIQ Product Modules in Product Market	Cost Controls
92	Spices, Seasoning, Extracts	Pepper	Spices, Salt Pepper Spices
93	Spices, Seasoning, Extracts	Salt - Cooking/Edible/Seasoned	
94	Spices, Seasoning, Extracts	Seasoning-Dry	Spices, Salt Pepper Spices
95	Spices, Seasoning, Extracts	Vegetables - Onions - Instant	Dry Onions
96	Stationery, School Supplies	Dry Erase Bulletin Board And Accesory	Stainless Steel
97	Stationery, School Supplies	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 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 dessers
		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.