Consolidation on Aisle Five: Effects of Mergers in Consumer Packaged Goods

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January 2025

Abstract

We study the effects of the typical merger in the consumer packaged goods industry, a sector making up over 10% of United States gross domestic product. Using an event-study design and linked retail scanner data from hundreds of mergers, we find that mergers raise prices at the target by approximately 1%. Under nested CES demand, we provide sufficient statistics to recover average consumer welfare effects as a function of effects on price, product availability, and exit. Accounting for availability and exit is quantitatively important. The decline in consumer welfare is equivalent to a 1.9% price increase at the target firm.

JEL Codes: L41.

Keywords: Mergers, horizontal mergers, antitrust.

*Majerovitz: University of Notre Dame, jeremy.majerovitz@gmail.com. Yu: RAND Corporation, ayu@rand.org. An older version of this paper was posted to SSRN on October 14, 2021. We thank David Atkin, David Cutler, Ed Glaeser, Robin Lee, Ariel Pakes, Nancy Rose for encouragement, helpful comments and suggestions. We also thank Xavier Jaravel and Kirill Borusyak for help linking the GS1 and NielsenIQ data. This research was funded by the MIT Shultz Fund (Majerovitz). All errors are our own. Researchers' own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researcher(s) and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein. Majerovitz and Yu worked on this paper while graduate students at MIT and Harvard, respectively.

1 Introduction

What are the effects of mergers on prices, availability, and consumer welfare? Prominent voices have emphasized the urgent need to conduct more retrospective analyses of mergers (see Federal Trade Commission 2020; Kwoka and Jarsulic 2017). Although the literature has provided many case studies, there is much less systematic evidence. To fill this gap, we study consumer packaged goods, a sector that comprises over 10% of the US economy. We conduct the most comprehensive retrospective analysis to date of mergers in this market. We link national retail scanner data between 2006 and 2017 with a complete database of mergers and acquisitions during the same time period. The typical merger is an acquisition of a small firm by a large firm, with the median acquiror five times as large as its target in terms of revenue. We estimate the effects of the typical merger employing an event-study research design. Then, through the lens of a nested CES model of demand, we calculate the impact of mergers on consumer welfare, highlighting the importance of firm exit and product availability in addition to prices.

In our main analysis of prices, we compare outcomes of the merging firm with a comparison group before and after the transaction. For each merging firm, we construct price indices for the target and the acquiror. Outcomes for control groups are then constructed using non-merging or store-brand products in the same product categories as the merging firm. We find that mergers lead to an immediate price increase of 1% at the target firm and we are able to rule out effect sizes larger than 1.5%. Consistent with theory, we find no price effect at acquirors.

We then explore effects on revenue and availability. Merger consummation is associated with a 5% decline in revenue at the target firm. This decline in revenue is not simply a movement along the demand curve, it also reflects a reduction in product availability. The number of products offered by target firms fell 2.4% and the number of stores fell 5.2% following the merger. Moreover, target firms are 3 percentage points more likely to exit in the 12 months following the merger.

Lastly, we provide a sufficient-statistics approach to recover the average effect of mergers on consumer welfare, as a function of demand parameters and of the effects we estimated. With a nested constant elasticity of substitution (CES) demand model, we show that the effect of mergers on consumer welfare can be decomposed into an effect on the target's brand-level price index and on the target's likelihood of exit. The effect on the target's brand-level price index depends on inflation for continuing products and changes to product availability. Calibrating the elasticity of substitution within and across brands with estimates from the literature, we estimate that mergers lead to a decline in consumer welfare equivalent to a

1.9% increase in the target firm's price. Without taking into account changes in product availability and exit, the price effect alone understates the effect of mergers on consumer welfare.

Our paper contributes to three strands of the literature. First, we add to retrospective merger analyses in the consumer packaged goods market. Ashenfelter and Hosken (2010) study five mergers on the enforcement margin (e.g., Pennzoil/Quaker State and P&G/Tambrands). Miller and Weinberg (2017) investigate the price effects of the Miller-Coors joint venture. Meanwhile, Ashenfelter et al. (2015) focus on the efficiency consequences of the Miller-Coors joint venture; they argue that the transaction increased efficiency through lower distribution costs, offsetting higher prices. Compared to these papers, we conduct a much larger retrospective merger analysis, covering hundreds of mergers.

Our study is also closely related in spirit to retrospective merger analyses in other industries. Economists have studied the impact of mergers of supermarkets (Hosken et al., 2018), hospitals (Dafny, 2009; Dafny et al., 2019; Brot et al., 2024), dialysis clinics (Eliason et al., 2020), pharmaceutical companies (Cunningham et al., 2021), and more. These papers typically examine price and exit outcomes at the target, acquiror, and/or rivals. See Ashenfelter et al. (2014) for a comprehensive review of 49 of these papers. Two other related papers are Kwoka (2015) and Blonigen and Pierce (2016). Kwoka (2015) performs a meta-analysis of merger effect publications; he argues that antitrust enforcement has been too lax, resulting in price hikes and lost consumer welfare. Compared to Kwoka (2015), our paper brings an internally consistent analysis of many mergers in consumer packaged goods, rather than relying on a sample of estimates from various papers, which may have different empirical strategies. Blonigen and Pierce (2016) use Census data to estimate the impact of mergers in the manufacturing sector on productivity, markups, and exit. In comparison to Blonigen and Pierce (2016), our study utilizes high-frequency data with direct observation of both prices and quantities, and we do not rely on inferred markups from an estimated production function.

The most closely related papers were developed contemporaneously: Atalay et al. (2023) and Bhattacharya et al. (2023). These papers link the same retail scanner database with the same data on merger deals, albeit with different linking procedures, sample restrictions, and research designs. Atalay et al. (2023) provides evidence on post-merger product repositioning, which we are able to replicate via our research design. Bhattacharya et al. (2023) estimate the price effects of fifty large mergers and interpret their estimates through the framework of an antitrust regulator that challenges mergers when the expected price effect exceeds some threshold. Our paper differs in two important respects. First, our sample of mergers is larger and focuses on the "typical" transaction rather than only the largest mergers that attract antitrust attention. Second, we focus on how to combine treatment effects on price, availability, and exit to estimate effects on consumer welfare.

The remainder of our paper is structured as follows. Section 2 presents a tractable model to interpret merger effects. Section 3 explains our event-study research design and describes our data. Section 4 presents the effects of mergers on prices, revenue, availability, and exit. Section 5 presents consumer welfare effects of mergers through the lens of our framework. Section 6 concludes.

2 Model for Merger Welfare Effects

In this section, we provide a tractable framework to study the effect of mergers on consumer welfare. We follow a tradition in empirical economics that connects "reduced form" estimates to structural models, in order to make statements about welfare (Chetty, 2009; Jaffe and Weyl, 2013).

2.1 Consumer Demand

We model demand as having a nested structure: each product, i, is nested in a brand, g, which is nested in a product category, c. We index time by t. We allow for a flexible utility function across categories:

$$U_t = U\left(\{U_{c,t}\}_{c=1}^{C}\right)$$

where $U_{c,t}$ is the composite good for category c. The category composite good is a CES aggregate across brands[1]

$$U_{c,t} = \left(\sum_{g\in c} Q_{g,c,t}^{(\gamma-1)/\gamma}\right)^{\gamma/(\gamma-1)}$$

where $Q_{g,c,t}$ is the composite good for brand g. Finally, the brand composite good for a brand is itself a CES aggregate across the individual products belonging to brand g

$$Q_{g,c,t} = \left(\sum_{i\in g} d_{i,g,c}^{1/\sigma}\left(q_{i,g,c,t}\right)^{(\sigma-1)/\sigma}\right)^{\sigma/(\sigma-1)}$$

[1] In addition to being the standard in the literature, nested CES is convenient because it is governed by a small number of parameters. Given the large number of products in the settings we study, estimating flexible models of demand would in general be infeasible in this environment (see discussion in Jaravel, 2021).

where $d_{i,g,c}$ is a taste shifter, $q_{i,g,c,t}$ is the quantity consumed of good i from brand g at time t, and σ is the (within-brand) elasticity of substitution. For the remainder of the paper, we will suppress the c subscript, except when necessary.

2.2 Brand-Level Price Indices

To buy a unit of good i from brand g at time t, the consumer must pay the price $p_{i,g,t}$. The brand-level price index, $P_{g,t}$, is the minimum cost to obtain one unit of the composite good for brand g:

$$P_{i,g,t} = \min_{\{q_{i,g,t}\}_{i\in g}} \sum_i p_{i,g,t} \cdot q_{i,g,t}$$
$$\text{s.t.}$$
$$1 = \left(\sum_i d_{i,g}^{1/\sigma} \left(q_{i,g,t}\right)^{(\sigma-1)/\sigma}\right)^{\sigma/(\sigma-1)}$$

Since CES is homothetic, the price of buying $Q_{g,t}$ units of brand g's composite good is $P_{g,t} \cdot Q_{g,t}$.

The brand-level price index will depend on prices, $p_{i,g,t}$, and on which products are available at time t. As shown in Feenstra (1994), changes in the brand-level price index can be represented as follows:

$$\log \frac{P_{g,t}}{P_{g,t-1}} = \underbrace{\left(\sum_{i\in\mathcal{C}_{g,t}} w_{i,g,t} \cdot \log\left(\frac{p_{i,g,t}}{p_{i,g,t-1}}\right)\right)}_{\text{Inflation for Continuing Products}} + \underbrace{\frac{1}{\sigma-1} \cdot \log\left(\frac{1-s_{N,g,t}}{1-s_{E,g,t-1}}\right)}_{\text{Effect of Changing Availability}} \tag{1}$$
$$w_{i,g,t} := \frac{\left(s_{i,g,t}/s_{i,g,t-1}\right) / \log\left(s_{i,g,t}/s_{i,g,t-1}\right)}{\sum_i \left(s_{i,g,t}/s_{i,g,t-1}\right) / \log\left(s_{i,g,t}/s_{i,g,t-1}\right)}$$

where $\mathcal{C}_{g,t}$ is the set of continuing products within brand g, $s_{i,g,t} := \frac{rev_{i,g,t}}{rev_{g,t}}$ is product i's share of firm g's revenue in time t, $s_{N,g,t}$ is the share of spending in time t on new products within brand g (products that were not available in $t-1$), and $s_{E,g,t-1}$ is the share of spending in time $t-1$ on exiting products within brand g (products that will no longer be available in t). Note also that the weight $w_{i,g,t}$ will always be between the shares $s_{i,g,t}$ and $s_{i,g,t-1}$. We will refer to $\frac{1-s_{N,g,t}}{1-s_{E,g,t-1}}$ as the Feenstra ratio.

The log change in the brand's price index can thus be decomposed into a component reflecting inflation for continuing products, plus a component reflecting changes in the availability of products. The importance of this latter component is governed by the within-brand elasticity of substitution, σ. We will explore a range of values for σ, although $\sigma = 5$ is a typical value in the literature. Moreover, since these two components are additively separable, a reader can easily compute the effect on the price index for any σ, given the reported effects on the two components.

2.3 Welfare Effects of Mergers

Mergers affect welfare by affecting prices, product availability, and brand exit. We can use our framework to combine each of these effects into a single effect on consumer welfare.

To measure the effect of the merger on consumer welfare, we will focus on measuring its effect on the category-level price index, $P_{c,t}$.[2] This is justified by Shephard's lemma: up to first order, the change in the expenditure needed to achieve a given utility is equal to the percent change in $P_{c,t}$ times the baseline expenditure going to category c.[3]

We can leverage the Feenstra (1994) result to study the effect of mergers on the category-level price index. Instead of P_t and P_{t-1}, we will consider P_{Treated} and P_{Control}, where the former is the price index in the world where the merger takes place, and the latter is the counterfactual price index if the merger did not take place. We use g^* to denote the target's brand.

To approximate the effect of the merger on consumer welfare, we make a few simplifying assumptions:

Assumption 1.

a) The merger only affects prices and availability at the target.

b) The change in brand-level prices is small: $\left(\log P_{g^*,Treated} - \log P_{g^*,Control}\right)^2 \approx 0$.

c) The target is small relative to total spending: $\log\left(1 - s_{g^*}\right) \approx -s_{g^*}$.

d) Revenue-weighted average treatment effects are equal to unweighted average treatment effects.

These assumptions are broadly consistent with our empirical results. We do not find significant effects on acquirors. We find small effects on brand-level prices, and the targets we study are small relative to total spending; this enables us to use a first-order approximation. Finally, because the distribution of target sizes has a long right tail, a revenue-weighted regression would be quite noisily estimated, which requires us to instead focus on the unweighted regression. An earlier version of this paper tested for effect heterogeneity and did not find evidence for heterogeneity by target's pre-merger size.

[2] $P_{c,t}$ is defined analogously to the brand-level price index.

[3] This result goes through as usual even though we have composite goods. Because we have a well-behaved price index, expenditure on all products within category c is equal to the product of the price index and the composite good, $P_{c,t} \cdot U_{c,t}$.

For ease of interpretation, we will express the welfare cost of the merger in terms of the percentage price increase, τ, that causes the same expected welfare effect as the merger. To be precise, consider a counterfactual, $P_{\text{Control},1+\tau}$, in which the merger did not occur, but all prices at the target rose proportionally by $1+\tau$. We define τ as the solution to the following equation:

$$\mathbb{E}\left[\log \frac{P_{\text{Control},1+\tau}}{P_{\text{Control}}}\right] := \mathbb{E}\left[\log \frac{P_{\text{Treated}}}{P_{\text{Control}}}\right]$$

Proposition 1. *Let $P_{Control,1+\tau}$ denote the price index under a counterfactual where the merger does not occur, but prices at the target rise uniformly by $(1+\tau)$. Define τ such that the expected log change in consumer welfare is the same as under the merger, that is:*

$$\mathbb{E}\left[\log \frac{P_{Control,1+\tau}}{P_{Control}}\right] := \mathbb{E}\left[\log \frac{P_{Treated}}{P_{Control}}\right]$$

Then, the following expression gives the factor τ that generates the equivalent consumer welfare effect as the merger:

$$\log(1+\tau) \approx \frac{1}{Prob\,(Stays\ if\ Control)} \cdot \left(Prob\,(Stays\ in\ Both) \cdot \underbrace{\mathbb{E}\left[\log\left(\frac{P_{g^*,Treated}}{P_{g^*,Control}}\right)\right]}_{Price\ Effect} \right.$$
$$\left. + \frac{1}{\gamma-1} \cdot \underbrace{\left(Prob\,(Exits\ if\ Treated) - Prob\,(Exits\ if\ Control)\right)}_{Exit\ Effect} \right) \tag{2}$$

The importance of the price effect is mediated by the probability that the firm stays in both states. This probability is not directly observable, but we can generate tight bounds. Suppose the target is more likely to exit when treated. Then, $1 - \text{Prob}\,(\text{Exits} \mid \text{Treated}) \geq \text{Prob}\,(\text{Stays in Both}) \geq 1 - \text{Prob}\,(\text{Exits} \mid \text{Treated}) - \text{Prob}\,(\text{Exits} \mid \text{Control})$. The importance of the exit effect is determined by γ, the elasticity of substitution across brands. In contrast, Prob (Stays if Control) is something we can point-identify in the data: it is equal to the probability that the target stays following a merger, minus the treatment effect of the merger on exit.

3 Empirical Framework and Data

To examine the effects of mergers, we employ an event-study (difference-in-differences) framework. Let $y_{i,m,t}$ denote the outcome of the merging firm i in merger m at month t. We assume the following model for the outcome:

$$y_{i,m,t} = \alpha_{i,m} + \gamma_{m,t} + \sum_{\tau} \beta_\tau Merger_{i,m} 1\{t - E_m = \tau\} + \varepsilon_{i,m,t} \tag{3}$$

where $\alpha_{i,m}$ are merger-firm fixed effects, and $\gamma_{m,t}$ are merger-month fixed effects. $Merger_{i,m}$ is an indicator equal to one for the target firm and zero for the control group, and $1\{t-E_m = \tau\}$ are indicators for the event month, τ, relative to the merger effective month E_m. The residual, $\varepsilon_{i,m,t}$, is mean-zero conditional on the other regressors.

3.1 Estimation with Merger-Specific Control Groups

For our main results on price and availability, we use a "stacked" approach by construct a control group for each merger. To estimate this model, we subtract the outcome of the comparison group from the outcome of the target firm, to yield our estimating equation:

$$\Delta y_{m,t} = \alpha_m + \sum_{\tau \neq -1} \beta_\tau 1\{t - E_m = \tau\} + \varepsilon_{m,t} \tag{4}$$

$\Delta y_{m,t}$ is the difference in the outcome of the merging firm relative to the control in calendar month t. The merger fixed effect and merger-time residual are defined similarly: $\alpha_m = \Delta\alpha_m$, and $\varepsilon_{m,t} = \Delta\varepsilon_{m,t}$. β_τ are the coefficients of interest and plot the average impact of a merger on the merging firm's outcome over time. We normalize the coefficients to zero the month before the merger: that is, $\beta_{-1} = 0$ and cluster standard errors at the merger level. When reporting pooled estimates, we average across dynamic effects: $\beta_{\text{pool}} = \frac{1}{\sum_\tau 1\{\tau \geq 0\}} \sum_{\tau \geq 0} \beta_\tau$.[4] As shown by Baker et al. (2022), this research design will estimate an average treatment effect across mergers, and circumvents the negative weighting issues raised by Goodman-Bacon (2021).

The main identification assumption for these regressions is parallel trends of the counterfactual. For each merger, we construct control outcomes using similar non-merging products to the target's products. Identifying suitable control groups is also challenging due to competitive interactions, which may lead to violations of the SUTVA assumption (no spillovers). However, we note that, because the target firms tend to be small, the effects on prices of firms other than the targets will also tend to be quite small. Following Ashenfelter and Hosken (2010), we show that our results that are robust to different control definitions, such as private label products similar to the target's products.

[4]For other examples of this research design, see Gormley and Matsa (2011), Cengiz et al. (2019) and Deshpande and Li (2019).

3.2 Estimation Using Staggered Timing

An alternative estimation approach is to leverage the staggered timing of mergers, comparing firms that merge at different times. We use this approach to measure the effect of mergers on exit, which cannot be sensibly measured for the control group in our previous strategy, and as a robustness check for our other outcomes. We modify equation 3 to assume that the merger time fixed effect, $\gamma_{m,t}$, is the same across mergers. This yields the staggered timing equation:

$$y_{i,m,t} = \alpha_i + \gamma_t + \sum_{\tau} \beta_\tau 1\{t - E_m = \tau\} + \varepsilon_{i,m,t} \tag{5}$$

Under staggered timing, identification comes from the assumption that yet-to-be-treated firms that will eventually merge are good controls for firms that have already been treated. In the context of equation 3, this rules out anticipation effects and also requires that all of the firms in the sample would, on average, be on similar time trends but for the merger (that is, $\gamma_{m,t} = \gamma_t$ for all mergers, m). We believe these assumptions are plausible, and we are able to partially test them by testing for pre-trends.

As noted by Borusyak et al. (2024), the collinearity of firm, month, and event time indicators requires that at least two of the β_τ coefficients be normalized to zero. We normalize β_τ to zero for $\tau = -1$ and $\tau = -12$. We estimate the above equation using ordinary least squares with two-way fixed effects. However, if the effects of mergers are heterogeneous, then traditional two-way fixed effect estimates may yield a non-convex average of the true merger effects (Goodman-Bacon, 2021). For robustness, we also estimate the model with the estimator provided in Borusyak et al. (2024), with merger-specific time trends.[5]

3.3 Price Indices

For each merger target and corresponding comparison group, we construct monthly price indices using the CES price index (see "inflation for continuing products" in Equation 1) and Tornqvist price index. We focus on superlative price indices because they solve the substitution bias issue, which heavily biases the more common Laspeyres and Paasche indices (see discussion in Hausman, 2003). For each product, we construct its average price as national total revenue divided by national total quantity for a given month. We drop products that are not available for the full year: this helps us deal with seasonally available products that would otherwise distort the results.

[5] A variety of other methods are also available, such as Callaway and Sant'Anna (2021); Sun and Abraham (2021); de Chaisemartin and D'Haultfœuille (2020). However, we favor the methods of Borusyak et al. (2024) because they show that, under homoskedastic and uncorrelated residuals, their method is efficient within the class of imputation estimators that avoid the negative weights issue.

To construct the Feenstra ratio (see "effect of changing availability" in Equation 1), we use the same sample of products, measuring monthly national revenue for each product. To study the cumulative effects of changing product availability, we take the running product of the Feenstra ratio, mirroring the construction of price indices from monthly inflation.

3.4 Data Sources

The data for our analysis come from two sources: SDC Platinum and Kilts retail scanner data from NielsenIQ. We collect horizontal mergers and acquisitions from the SDC Platinum database between 2006 and 2017. For each merger, we observe the date effective and target/acquiror identifiers (e.g. name and SIC industry code). The NielsenIQ scanner data contain barcode (UPC) level sales data from over 35,000 stores at participating grocery, pharmacy, and mass-merchandise chains. These data comprise over half of total sales of US grocery and drug stores as well as over 30% of all US mass merchandiser sales volume. These products are organized into departments, product groups, and product modules, which are increasing in detail. An example of a department, product group, and product module is "Non-food grocery," "Detergent,", and "Detergent-Packaged," respectively. We treat each UPC as a distinct product. We collapse the sample to a UPC by month sample, obtaining data on monthly total revenue, quantities, and number of stores.

We link acquisitions from the SDC Platinum database with Kilts scanner data from NielsenIQ. To identify firms, we merge the Kilts scanner data with manufacturer identifiers from GS1, the organization in charge of allocating bar code prefixes.[6] For each merger in the SDC data, we identify GS1 prefixes corresponding to the target and acquiror. To connect the SDC data and GS1 prefixes, we use a variety of fuzzy matching techniques and manual review, outlined in the Supplementary Appendix.[7]

We are able to identify 465 mergers in the SDC Platinum data to the NielsenIQ data. Our analysis on the Tornqvist price index uses a balanced panel of 271 mergers for which we have data one year before through one year after the merger's effective date. Mergers that occur before February 2007 and after December 2016 are dropped because these mergers cannot have one year of pre- and post-merger data. Since NielsenIQ grocery store coverage changed significantly in 2017, some of our analyses further subset to mergers that occur before the end of December 2015, leaving us with 241 mergers. This gap can be explained by two reasons. First, firm exit can happen either before or after the merger. Exit is defined

[6] The first few digits of a UPC barcode are the company prefix, while the later digits identify the specific product.

[7] We use standardization and linkage tools from Wasi and Flaaen (2015). We also thank Kirill Borusyak for providing us the code for reclink4, a tool developed for Borusyak and Jaravel (2021).

as the firm no longer having any sales for associated UPC codes for the remaining months of the panel. Second, and to a lesser extent, we need to be able to construct outcomes for the comparison group for each merger. Specifically, our preferred control group contains all non-merging products that are in the same product modules as the products of the target firm. If this set is empty, a control price index cannot be constructed.

Table 1 presents summary statistics for the our main sample. On average, target firms generate $788,000 in sales per month in the NielsenIQ panel. The median target firm revenue share of the combined firm is 16%, so the acquiror is about five times larger. Target firms are small relative to the market: in their top product module, targets average 6% market share by revenue, with a median below 1%. In our sample, the most common mergers are for dry grocery products (40%), alcoholic beverages (13%), frozen food (12%), and dairy (9%). Examples of dry grocery products include bread, pasta, and cereal.

4 Effects of Mergers

4.1 Effects on Prices

We begin by estimating Equation 4 on the balanced panel of mergers, focusing on target firms. Figure 1 plots the estimated event-study coefficients and corresponding 95% confidence intervals for the effect of mergers on the log CES price index and log Tornqvist index. We find an average price effect of roughly 1%, beginning immediately following the merger transaction. The pooled effect is precisely estimated, with a 95% confidence interval from 0.2% to 1.4% for the CES price index, and 0.3% to 1.5% for the Tornqvist index. In the Supplemental Appendix, we present various alternative specifications for robustness: the point estimates are extremely similar, although some specifications are less precisely estimated. We also show effects for acquirors: these are approximately zero and not statistically significant, as expected given that targets tend to be small compared to their acquiror.

4.2 Effects on Revenue and Product Availability

We next study the effects of mergers on revenue and product availability at target firms. We measure availability by the total number of products (unique UPC codes) sold, the number of stores selling the target's products, and the product of Feenstra ratios (see Section 3.3). The number of stores is constructed from the target's most widely available product in the NielsenIQ data. We normalize outcomes to 1 twelve months prior to the merger, and then winsorize revenue, stores, and products at the top and bottom 1%.

Figure 2 plots the estimated effects of mergers on revenue and availability. On average, revenues fall by 5% following the merger. Although there is some evidence of a positive pre-trend, there is a sharp reversal at the time of the merger, suggesting that our estimates are, if anything, underestimates of the true magnitude of the revenue effect. Our result on the number of products replicates Atalay et al. (2023) using a different research design. We find that the number of products at the target firm fell by 2.4%, though the effect is not immediately realized at the time of the merger. It takes over half a year before the effects on the number of products are statistically distinguishable from zero. The number of stores also falls upon merger consummation, declining 5.2% on average in the 12 months after the merger. The product of Feenstra ratios increases by 2.0% on average, consistent with the decline in overall product availability. In the Supplemental Appendix, we present robustness of these results as well as effects on acquiror outcomes.

4.3 Effects on Firm Exit

So far, we have investigated the impact of mergers on target firms on the intensive margin, focusing on the balanced panel of firms. However, these estimates may understate the full effect on revenue and availability, as these will also decrease due to the exit of target firms. We now estimate the effects of mergers on exit, leveraging the staggered timing of320 mergers.[8] The top panel of Figure 3 plots the raw trends. Target exit is (mechanically) on an upward trajectory prior to the effective date, but the slope increases around the time of the merger. Roughly 9% of targets have already exited the sample a year before the merger, 11% have exited by the time of the merger, and 14% have exited one year later. In the bottom panel, we plot the dynamic effects from estimating (5).. Across the two estimation approaches, the average effect of merger on exit is around 3 percentage points one year following the merger. Although the estimates are noisy; we are able to rule out exit effects larger than 7 percentage points.[9]

5 Estimated Effects on Consumer Welfare

In this section, we use the estimated effects on prices, availability, and exit to compute the approximate effect of the merger on consumer welfare. We express these welfare effects in terms of an equivalent price increase: what uniform price increase at the target firm would

[8]This is larger than our main sample because it includes targets that exit.

[9]We focus on the impact after twelve months, rather than the pooled effect, because the effect on exit appears to be naturally cumulative.

have an equivalent effect on consumer welfare to the merger's actual effects on prices, availability, and exit? Equation 2 of Proposition 1, provides the formula for this price increase. To compute the welfare effect of the merger we must calibrate the elasticity of substitution within (σ) and across brands (γ). We anchor our discussion around the calibration $\sigma = \gamma = 5$, based on consensus estimates from the literature (see Broda and Weinstein 2006, 2010; Jaravel 2019, 2021), but we also explore how alternative values affect our results.

First, we compute the brand-level price effect, following Equation 1. For the price effect on continuing products, we use our CES price index estimate of 0.8%. For the effect of changing availability, we use our estimate of the effect on the product of Feenstra ratios, which is 2.0%. Under a calibration of $\sigma = 5$, the brand-level price effect is 1.3%:

$$\underbrace{\text{Brand-Level Price Effect}}_{1.3\%} = \underbrace{0.8\%}_{\text{Price Effect for Continuing Products}} + \underbrace{0.25}_{\frac{1}{\sigma-1}} \times \underbrace{2.0\%}_{\text{Effect of Changing Availability}}$$

We now can compute the welfare effect of the merger, using Equation 2. For the probability that the target firm survives without the merger, Prob(Stays if Control), we combine the exit probability, approximately 0.144, and our estimated 0.026 exit effect to arrive at a survival probability of 0.882.The probability that the target would survive regardless of the merger, Prob (Stays in Both), is not directly observable, but we can use the same information to generate bounds:a lower bound of 0.738 and an upper bound of 0.856. We adopt the midpoint 0.797. Calibrating $\sigma = \gamma = 5$, we find that the average welfare effect of the merger is equal to a uniform price increase of 1.9% at the target firm:

$$\underbrace{\text{Welfare Effect}}_{1.9\%} \approx \left(\underbrace{0.797}_{\text{Prob(Stays in Both)}} \times \underbrace{1.3\%}_{\text{Brand Price Effect}} + \underbrace{0.25}_{\frac{1}{\gamma-1}} \times \underbrace{2.6\%}_{\text{Exit Effect}} \right) \div \underbrace{0.882}_{\text{Prob(Stays if Control)}}$$

Accounting for effects on exit and product availability more than doubles the welfare effect of the merger. This calibration implies a plausible brand-level markup of 25% ($\frac{P-MC}{P} = \frac{-1}{-\gamma+1} = \frac{-1}{-5+1}$). Note that given σ, the brand-level elasticity of demand, $-\gamma$, can be identified from the brand-level price effect and revenue effect for surviving firms: $-\gamma = \frac{\text{Revenue Effect}}{\text{Brand-Level Price Effect}} - 1$. For $\sigma = 5$, this yields a point estimate of $\hat{\gamma} = 4.73$ (95% CI: $[0.79, 8.69]$), which is close to our calibrated value of $\gamma = 5$.

To explore how other values of the demand parameters affect the welfare effect of mergers, Figure 4 reports estimates of equation 2 for various σ and γ. Higher values of σ reduce the importance of product availability, and higher values of γ reduce the importance of exit.

For target firms facing very inelastic demand (e.g., $\gamma, \sigma \approx 2$), a uniform increase to the target's prices of over 5% would generate the equivalent consumer welfare decline. For extremely elastic demand (e.g., $\gamma, \sigma = 100$), the average decline in consumer surplus is well approximated by the "naive" price effect for continuing products. However, such elasticities are implausibly high, and would imply near-perfect competition (a markup of just 1%). Restricting attention to the high end of the elasticities in the literature ($\sigma = \gamma = 10$), we find a welfare effect of 1.3%, which is still 50% higher than the naive price effect.

6 Discussion and Conclusion

Concerns about rising concentration and market power put the spotlight on antitrust policy and mergers. In this paper, we study a comprehensive sample of hundreds of mergers in the consumer packaged goods industry, the vast majority of which are not subject to antitrust scrutiny. We find that mergers cause target firms to increase prices by 1%, reduce various measures of product availability, and increase the probability of exit by 3 percentage points twelve months after the merger. We show how to combine these estimates to measure the full effect of the merger on consumer welfare. We find that mergers cause a decline in consumer welfare equivalent to a 1.9% increase in prices at the target firm. This effect is twice the size of the raw price effect, highlighting the importance of accounting for exit and product availability in merger analysis.

Should antitrust authorities have blocked these mergers? The median merger target in our sample has annual revenue of roughly \$888,000 (monthly revenue of \$37,000 in our data, times twelve months, times two to account for Nielsen's incomplete coverage). Our estimated welfare effect of 1.9% implies that this merger reduces consumer welfare by \$17,000 the year following the merger. Taking the net present value, discounting at 9% (2% discount rate plus 7% exit rate), yields a benefit of \$190,000. The benefits of blocking such a merger are unlikely to justify the cost of enforcement. In contrast, a merger with \$25.3 million in annual revenue at the target firm (the current size-of-persons threshold under Hart-Scott-Rodino) would reduce consumer welfare over the next year \$481,000 (NPV=\$5.3 million). Our estimates thus provide some rationale for the selective approach of antitrust regulators. Blocking a "typical" merger is unlikely to justify the cost, but enforcement may be worthwhile if the merger is very large or if the welfare costs are likely to be well above average.

Proof of Proposition 1

Proof. First, we compute $\mathbb{E}\left[\log \frac{P_{\text{Treated}}}{P_{\text{Control}}}\right]$. Applying the Feenstra (1994) result:

$$\log \frac{P_{\text{Treated}}}{P_{\text{Control}}} = \left(\sum_{g\in\mathcal{C}} w_g \cdot \log\left(\frac{P_{g,\text{Treated}}}{P_{g,\text{Control}}}\right)\right) + \frac{1}{\gamma-1}\cdot\log\left(\frac{1-s_N}{1-s_E}\right)$$

where $\mathcal{C}$ denotes brands available in both states, s_N is the revenue share of brands that are available in the treated state but not the control state, and s_E is the revenue share of brands that are available in the control state but not the treated state.

There are four cases, depending on whether the target exits in the treated and/or control state.

Case I (Stays in Both)

$$\begin{aligned}
& 1\,(\text{Stays in Both}) \log \frac{P_{\text{Treated}}}{P_{\text{Control}}} \\
& = 1\,(\text{Stays in Both})\left(\left(\sum_{g\in\mathcal{C}} w_g \cdot \log\left(\frac{P_{g,\text{Treated}}}{P_{g,\text{Control}}}\right)\right) + \frac{1}{\gamma-1}\cdot\log\left(\frac{1-s_N}{1-s_E}\right)\right) \\
\text{Assumption 1a:}\quad & = 1\,(\text{Stays in Both})\cdot w_{g^*}\cdot\log\left(\frac{P_{g^*,\text{Treated}}}{P_{g^*,\text{Control}}}\right) \\
& \implies \mathbb{E}\left[1\,(\text{Stays in Both})\log\frac{P_{\text{Treated}}}{P_{\text{Control}}}\right] \\
& = \text{Prob}\,(\text{Stays in Both})\cdot\mathbb{E}\left[w_{g^*}\cdot\log\left(\frac{P_{g^*,\text{Treated}}}{P_{g^*,\text{Control}}}\right) \mid \text{Stays in Both}\right] \\
\text{Assumption 1d:}\quad & \approx \text{Prob}\,(\text{Stays in Both})\cdot\mathbb{E}\left[w_{g^*}\right]\cdot\mathbb{E}\left[\log\left(\frac{P_{g^*,\text{Treated}}}{P_{g^*,\text{Control}}}\right)\right]
\end{aligned}$$

Note that $\mathbb{E}\left[\log\left(\frac{P_{g^*,\text{Treated}}}{P_{g^*,\text{Control}}}\right)\right]$ is only defined for this case.

Since only the target is affected by Assumption 1a we have, by the mean value theorem:

$$s_{g^*,\text{Treated}} - s_{g^*,\text{Control}} = \varepsilon\cdot\log\left(\frac{P_{g^*,\text{Treated}}}{P_{g^*,\text{Control}}}\right)$$

where ε is the semi-elasticity $\frac{ds_{g^*}}{d\log P_{g^*}}$, evaluated at some price level between $P_{g^*,\text{Treated}}$ and

$P_{g^*,\text{Control}}$. This yields:

$$\begin{aligned}
&\mathbb{E}\left[s_{g^*,\text{Treated}}\right]\cdot\mathbb{E}\left[\log\left(\frac{P_{g^*,\text{Treated}}}{P_{g^*,\text{Control}}}\right)\right]\\
&=\mathbb{E}\left[s_{g^*,\text{Control}}\right]\cdot\mathbb{E}\left[\log\left(\frac{P_{g^*,\text{Treated}}}{P_{g^*,\text{Control}}}\right)\right]+\varepsilon\cdot\left(\mathbb{E}\left[\log\left(\frac{P_{g^*,\text{Treated}}}{P_{g^*,\text{Control}}}\right)\right]\right)^2\\
\text{Assumption 1b:}\quad&\approx\mathbb{E}\left[s_{g^*,\text{Control}}\right]\cdot\mathbb{E}\left[\log\left(\frac{P_{g^*,\text{Treated}}}{P_{g^*,\text{Control}}}\right)\right]
\end{aligned}$$

Since w_{g^*} is between $s_{g^*,\text{Treated}}$ and $s_{g^*,\text{Control}}$ by construction, we can simply write:

$$\mathbb{E}\left[1\left(\text{Stays in Both}\right)\log\frac{P_{\text{Treated}}}{P_{\text{Control}}}\right]\approx\text{Prob}\left(\text{Stays in Both}\right)\cdot\mathbb{E}\left[s_{g^*}\right]\cdot\mathbb{E}\left[\log\left(\frac{P_{g^*,\text{Treated}}}{P_{g^*,\text{Control}}}\right)\right]$$

Case II (Exits Only if Treated)

$$\begin{aligned}
&1\left(\text{Exits Only if Treated}\right)\log\frac{P_{\text{Treated}}}{P_{\text{Control}}}\\
&=1\left(\text{Exits Only if Treated}\right)\left(\left(\sum_{g\in\mathcal{C}}w_g\cdot\log\left(\frac{P_{g,\text{Treated}}}{P_{g,\text{Control}}}\right)\right)+\frac{1}{\gamma-1}\cdot\log\left(\frac{1-s_N}{1-s_E}\right)\right)\\
\text{Assumption 1a:}\quad&=1\left(\text{Exits Only if Treated}\right)\cdot\frac{1}{\gamma-1}\cdot\log\left(\frac{1}{1-s_{g^*,\text{Control}}}\right)
\end{aligned}$$

Case III (Exits Only if Control)

The result is analogous to Case II. Adding up both cases and taking expectations:

$$\begin{aligned}
&\mathbb{E}\left[1\left(\text{Exits Only if Treated OR Exits Only if Control}\right)\log\frac{P_{\text{Treated}}}{P_{\text{Control}}}\right]\\
&=\frac{1}{\gamma-1}\cdot\mathbb{E}\left[1\left(\text{Exits Only if Treated}\right)\cdot\log\left(\frac{1}{1-s_{g^*,\text{Control}}}\right)\right]\\
&+\frac{1}{\gamma-1}\cdot\mathbb{E}\left[1\left(\text{Exits Only if Control}\right)\cdot\log\left(\frac{1-s_{g^*,\text{Treated}}}{1}\right)\right]\\
&=\frac{1}{\gamma-1}\cdot\mathbb{E}\left[\log\left(\frac{1}{1-s_{g^*}}\right)\left(1\left(\text{Exits if Treated}\right)-1\left(\text{Exits if Control}\right)\right)\right]\\
\text{Assumption 1d:}\quad&=\frac{1}{\gamma-1}\cdot\mathbb{E}\left[\log\left(\frac{1}{1-s_{g^*}}\right)\right]\cdot\left(\text{Prob}\left(\text{Exits if Treated}\right)-\text{Prob}\left(\text{Exits if Control}\right)\right)\\
\text{Assumption 1c:}\quad&\approx\frac{1}{\gamma-1}\cdot\mathbb{E}\left[s_{g^*}\right]\cdot\left(\text{Prob}\left(\text{Exits if Treated}\right)-\text{Prob}\left(\text{Exits if Control}\right)\right)
\end{aligned}$$

Note that s_{g^*} refers to the share in whichever state the target stays.

Case IV (Exits in Both)

The merger has no effect in this case, trivially.

We can then add up each case:

$$\begin{aligned}
\mathbb{E}\left[\log \frac{P_{\text{Treated}}}{P_{\text{Control}}}\right] &= \mathbb{E}\left[(1(\text{Stays in Both}) + 1(\text{Exits Only if Treated})\right.\\
&\left. +1(\text{Exits Only if Control}) + 1(\text{Exits in Both})) \cdot \log \frac{P_{\text{Treated}}}{P_{\text{Control}}}\right]\\
&\approx \mathbb{E}[s_{g^*}] \cdot \left(\text{Prob}(\text{Stays in Both}) \cdot \mathbb{E}\left[\log\left(\frac{P_{g^*,\text{Treated}}}{P_{g^*,\text{Control}}}\right)\right]\right.\\
&\left. + \frac{1}{\gamma-1} \cdot (\text{Prob}(\text{Exits if Treated}) - \text{Prob}(\text{Exits if Control}))\right)
\end{aligned}$$

Next, we compute $\mathbb{E}\left[\log \frac{P_{\text{Control},1+\tau}}{P_{\text{Control}}}\right]$ for an arbitrary τ. We then have:

$$\begin{aligned}
&\mathbb{E}\left[\log \frac{P_{\text{Control},1+\tau}}{P_{\text{Control}}}\right]\\
&= \mathbb{E}\left[1(\text{Stays if Control}) \cdot w_{g^*} \log\left(\frac{P_{g^*,\text{Control},1+\tau}}{P_{g^*,\text{Control}}}\right)\right]\\
\text{Homotheticity:}\quad &= \text{Prob}(\text{Stays if Control}) \cdot \mathbb{E}[w_{g^*}] \cdot \log(1+\tau)\\
\text{Assumption 1b:}\quad &\approx \text{Prob}(\text{Stays if Control}) \cdot \mathbb{E}[s_{g^*}] \cdot \log(1+\tau)
\end{aligned}$$

where the last line uses the argument from Case I.

Equating $\mathbb{E}\left[\log \frac{P_{\text{Control},1+\tau}}{P_{\text{Control}}}\right]$ and $\mathbb{E}\left[\log \frac{P_{\text{Treated}}}{P_{\text{Control}}}\right]$ leads directly to our result:

$$\begin{aligned}
\log(1+\tau) \approx \frac{1}{\text{Prob}(\text{Stays if Control})} \cdot \left(\text{Prob}(\text{Stays in Both}) \cdot \mathbb{E}\left[\log\left(\frac{P_{g^*,\text{Treated}}}{P_{g^*,\text{Control}}}\right)\right]\right.\\
\left. + \frac{1}{\gamma-1} \cdot (\text{Prob}(\text{Exits if Treated}) - \text{Prob}(\text{Exits if Control}))\right)
\end{aligned}$$

□

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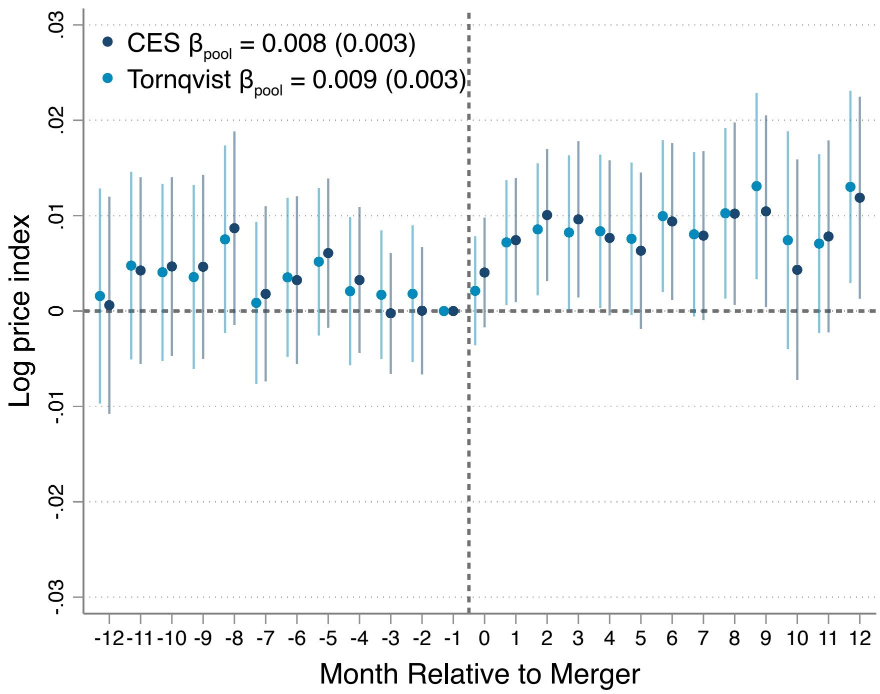
Exhibits

Table 1: Summary Statistics of Mergers Sample

A. Descriptive Statistics of Target Firms			
Variable	Mean	Std. Dev.	Median
Monthly Revenue (Million USD)	0.788	(4.734)	0.037
Log Price (Tornqvist) Growth	0.064	(0.627)	0.017
Log Monthly Revenue Growth	0.047	(0.137)	0.034
Revenue Share of Top Product Module	0.063	(0.145)	0.007
Revenue Share of Top Product Group	0.013	(0.044)	0.001
Revenue Share of Top Department	0.002	(0.008)	0.000
Target Firm Revenue Share if Acquiror Matched	0.336	(0.364)	0.156
Revenue-Weighted Predicted ΔHHI if Acquiror Matched	5.484	(20.604)	0.045
B. Number of Mergers by Department	Count	%	
Dry Grocery	109	40%	
Alcoholic Beverages	42	15%	
Frozen Foods	31	11%	
Dairy	24	9%	
General Merchandise	18	7%	
Health & Beauty Care	15	6%	
Packaged Meat	13	5%	
Non-Food Grocery	8	3%	
Deli	7	3%	
Fresh Produce	4	1%	
All	271	100%	
Number of Observations (Target-Month)	6775		

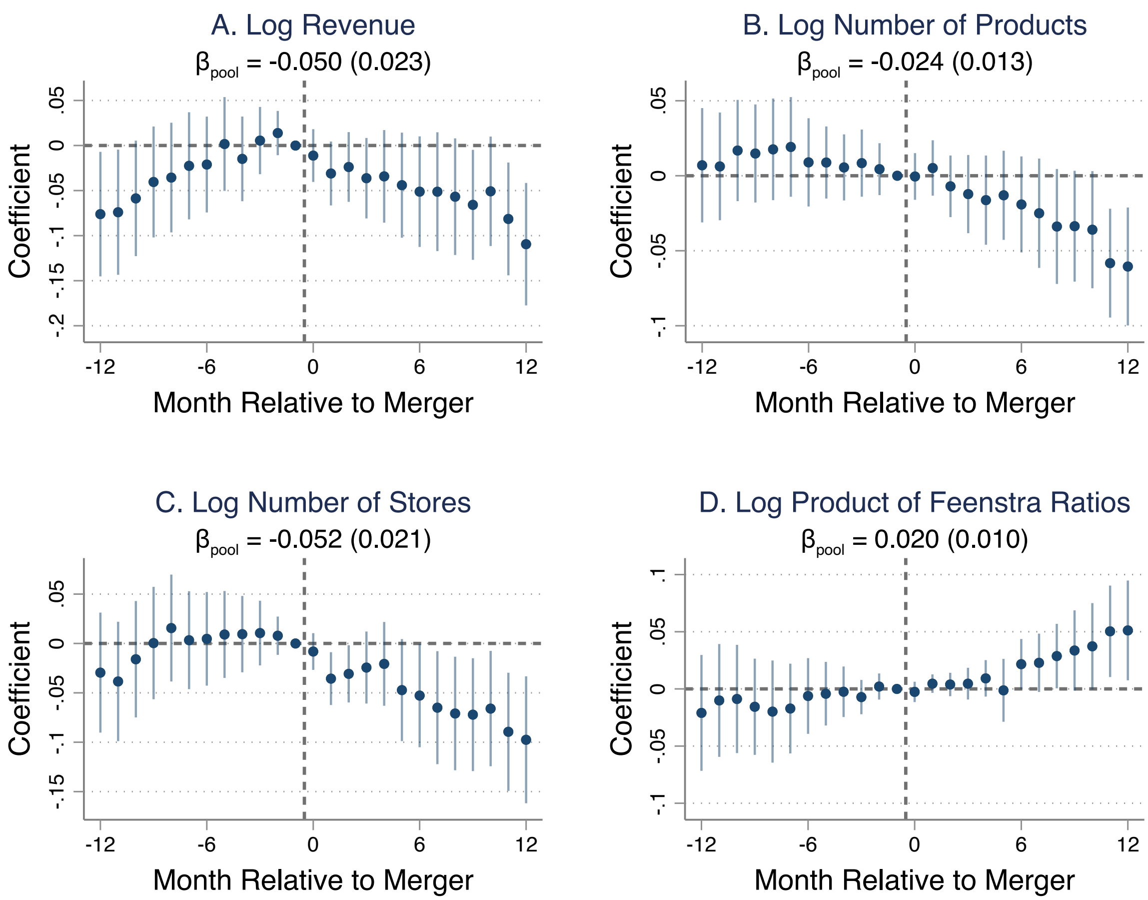
Notes: Panel A presents summary statistics (mean and standard deviation) of the target firms in the analysis sample -12 to -7 months before the merger effective date. Target firm revenue share and revenue-weighted predicted change in HHI are caclulated on mergers where we have acquiror data. Panel B shows the number of mergers by department in NielsenIQ scanner data. The first and third columns present the number of target firms matched to NielsenIQ data where we observe data one year before and one year after the merger's effective date.

Figure 1: Effects of Mergers on Prices



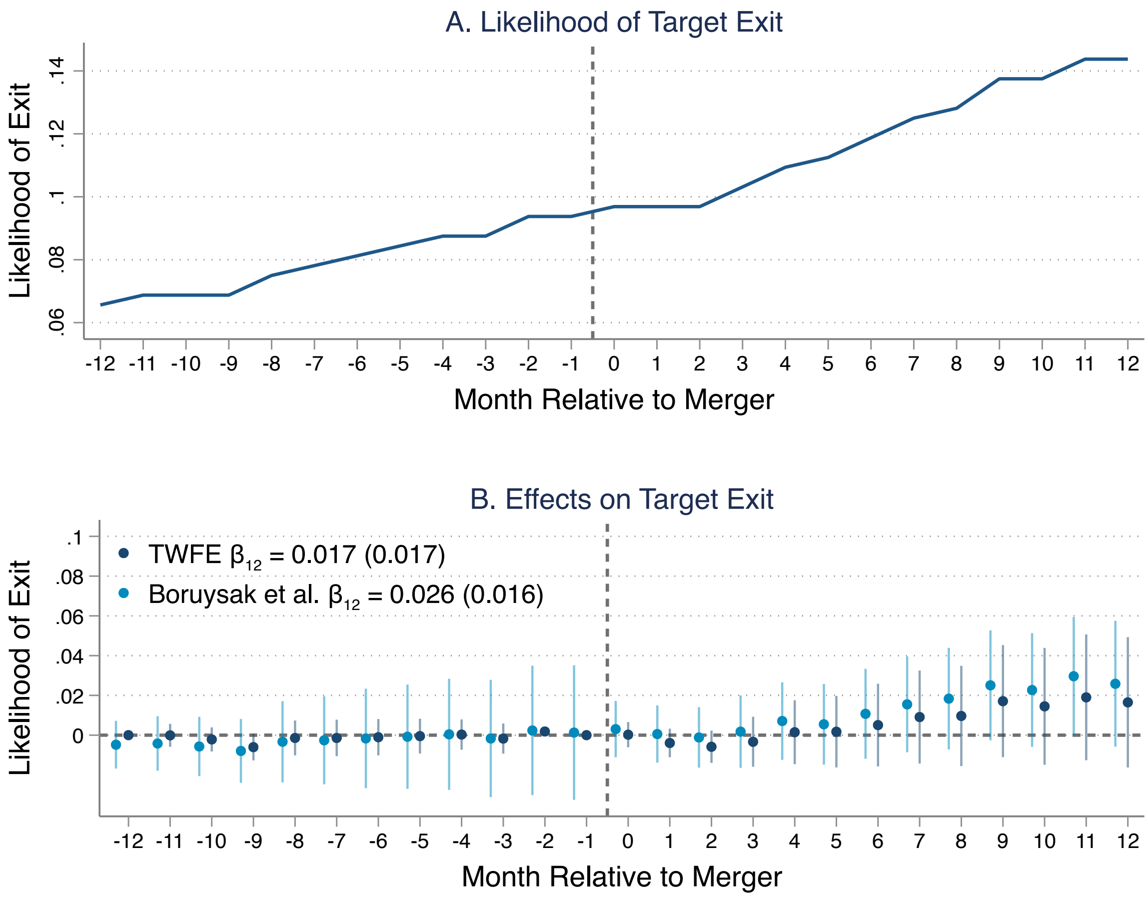
Notes: This figure plots event-study coefficients for log price indices at target firms. 95% confidence intervals are plotted around each solid line. Standard errors are clustered at the merger level. The control group for each merger is constructed using products in the same product modules offered by the target firm. The pooled estimate across the entire 0-12 month period along with standard errors is reported on the graph area.

Figure 2: Effects of Mergers on Revenue and Product Availability



Notes: This figure plots event-study coefficients for log revenue (a), number of products (b), number of stores (c), and the product of Feenstra ratios (d) at target firms. 95% confidence intervals are plotted around each solid line. Standard errors are clustered at the merger level. The control group for each merger is constructed using products in the same product modules offered by the target firm. The pooled estimate across the entire 0-12 month period along with standard errors is reported on the graph area.

Figure 3: Effects of Mergers on Firm Exit



Notes: Subfigure a plots raw likelihood of target exit for targets that are present in the NielsenIQ data prior to the merger announcement date. Subfigure b plots event-study coefficients for target exit. The estimate at $\tau = 12$ is reported alongside its standard error in parentheses, clustered at the merger level. The two-way fixed effects (TWFE) specification normalizes β_τ at $\tau = -12, -1$ to zero. The DID-Imputation estimation procedure follows Borusyak et al. (2024), including merger-specific time trends.

Figure 4: Consumer Welfare Equivalent Price Changes by σ, γ

		Within-Brand Elasticity of Substitution (σ)				
		2	3	5	10	100
Elasticity of Substitution Across Brands (γ)	2	5.5%	4.6%	4.1%	3.9%	3.7%
	3	4.0%	3.1%	2.6%	2.4%	2.2%
	5	3.3%	2.4%	1.9%	1.7%	1.5%
	10	2.9%	2.0%	1.5%	1.3%	1.1%
	100	2.6%	1.7%	1.2%	1.0%	0.8%

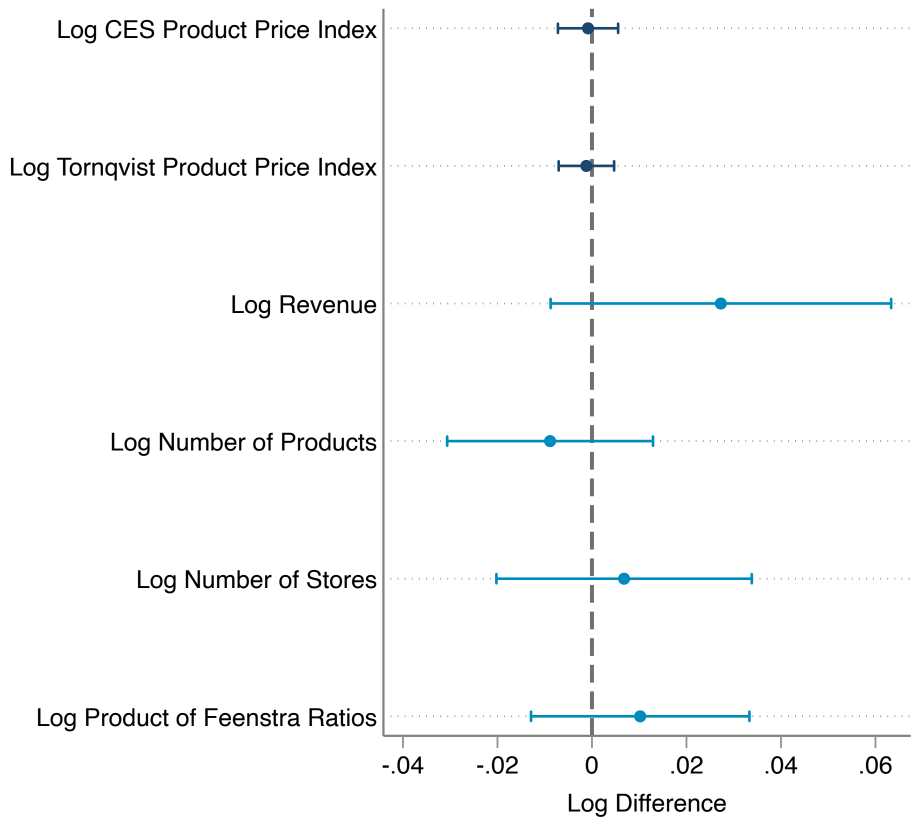
Notes: This figure plots the average effect of mergers on consumer welfare, computed using Equation 2 from Proposition 1, as a function of the elasticity of substition across brands (γ) and within brands (σ). We compute the uniform price change, τ, such that the change in consumer welfare is equivalent to the average welfare effect of the merger, accounting for effects on prices, product availability, and firm exit. In particular, we use the formula $\tau \approx \left[\text{Prob}\,(\text{Stays in Both}) \times \left(\text{Price Effect} + \frac{1}{\sigma-1} \times \text{Availability Effect}\right) + \frac{1}{\gamma-1} \times \text{Exit Effect}\right] \div \text{Prob}\,(\text{Stays in Control})$. The price effect (0.8%) and availability effect (2.0%) are estimated from the stacked difference-in-differences design. The exit effect (2.6%) is based on the effect estimated at 12 months after the merger using the DID imputation estimator from Boruysak et al. (2024) with merger-specific trends. The probability that the target survives in the absence of the merger, Prob (Stays in Control), is 0.88, computed from the 14% exit probability 12 months after the merger, combined with our estimated exit effect. For the probability that the firm would survive regardless of the merger, Prob (Stays in Both), we use these estimates to bound the probability within $[0.74, 0.86]$, and use the midpoint of this range, 0.80, as our estimate.

For Online Publication: Appendix to "Consolidation on Aisle Five: Effects of Mergers in Consumer Packaged Goods"

Jeremy Majerovitz and Anthony Yu[1]

1 Supplemental Figures

Figure 1: Effects of Mergers on Acquiror Outcomes



Note: This figure presents pooled event study estimates of the impact of mergers on log acquiror outcomes. The whiskers indicate 95% confidence intervals, constructed using standard errors clustered by merger.

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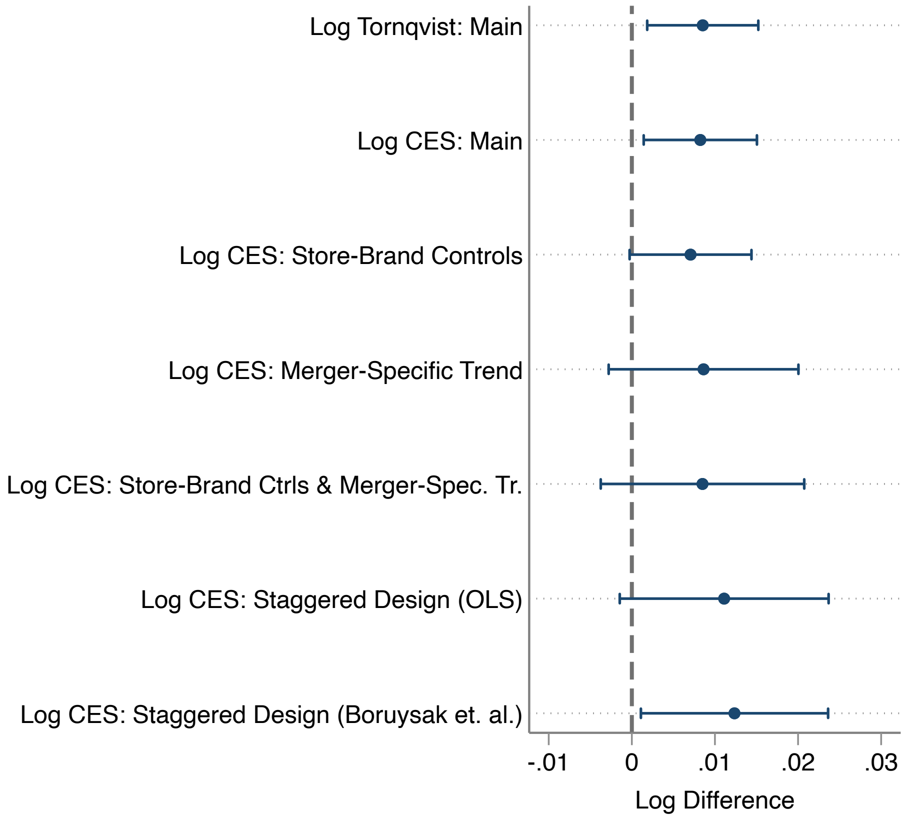
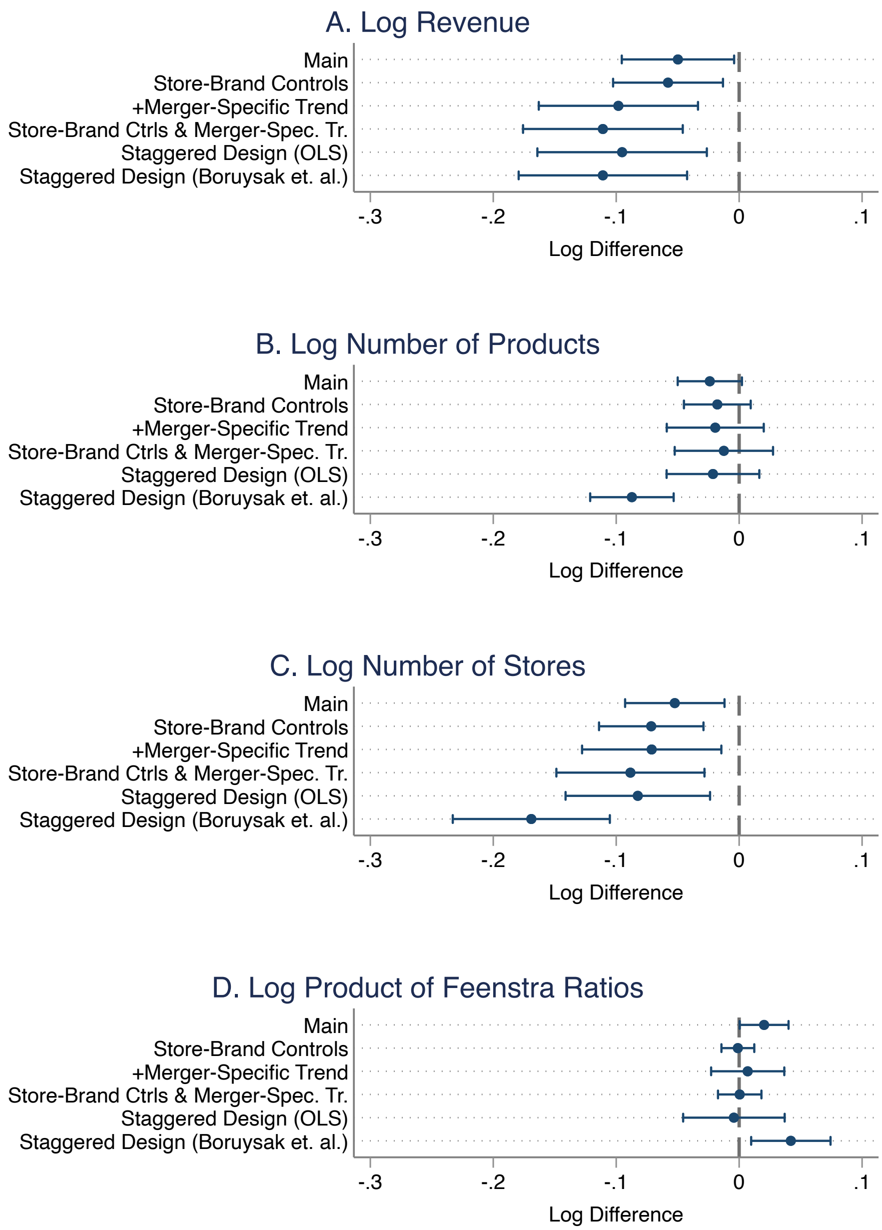
Researchers' own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researcher(s) and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

Figure 2: Robustness of the Effect of Mergers on Prices



Note: This figure presents pooled (month 0-12) event study estimates of the impact of mergers on log prices. The whiskers indicate 95% confidence intervals, constructed using standard errors clustered by merger. The main results implement a stacked event study approach using prices of non-merging products in the target firms product modules as control groups. Staggered designs leveraging the timing of mergers over time to estimate effects. Staggered OLS estimates require two normalizations; we normalize $\tau = -12, -1$ to zero. Borusyak et al. (2023) propose an alternative estimation strategy to circumvent treatment effect estimation issues outlined in Goodman-Bacon (2021). We present estimates using their imputation approach with merger-specific trends.

Figure 3: Robustness of the Effect of Mergers on Revenue and Availability



Note: This figure presents pooled (month 0-12) event study estimates of the impact of mergers on log revenue, number of products, number of stores, and the product of Feenstra ratios. The whiskers indicate 95% confidence intervals, constructed using standard errors clustered by merger. The main results implement a stacked event study approach using prices of non-merging products in the target firms product modules as control groups. Staggered designs leveraging the timing of mergers over time to estimate effects. Staggered OLS estimates require two normalizations; we normalize $\tau = -12, -1$ to zero. Borusyak et al. (2023) propose an alternative estimation strategy to circumvent treatment effect estimation issues outlined in Goodman-Bacon (2021). We present estimates using their imputation approach with merger-specific trends.

2 Data Construction

2.1 Linking SDC Platinum with NielsenIQ data

To link the SDC Platinum mergers data with NielsenIQ scanner data, we use a GS1 crosswalk that was downloaded in July 2017. The GS1 crosswalk provides firm identifiers for each UPC prefix, which we then merge on party names in the mergers database.

We pre-process the SDC and GS1 data prior to merging the two datasets. In the SDC Platinum data, we create two datasets based on acquirers and their targets. Then, we rename the variables so that they are the same in both datasets and stack. Our final dataset contains following variables: name, street address, cities, states, and any covariates of the mergers. Names, street address, city, and zip codes are standardized using the Stata command `stn_compname` and `stn_address` (Wasi and Flaaen, 2015). In the GS1 data, we keep only the UPC prefix, name, address, city, state and zip code, standardizing the names using the commands described above.

For each merger and name (target or acquiror), we perform fuzzy merges between the SDC Platinum dataset and GS1 using the Stata commands `reclink2` and `reclink4` (Borusyak and Jaravel, 2021). Table B1 shows the exact merges we performed.

Table B1: Fuzzy Merges

	Exact match	Fuzzy match	Weights	Threshold for Review
1	-	Name, Address, City, State, Zip-5	(10, 5, 7, 8, 7)	0.9
2	Zip-5	Name, Address	10 10 5	0.75
3	Zip-5	Name	10 10	0.95
4	Name	State	10 3	0.95
5	Name (first word)	Name, State	10 10 3	0.95

After the fuzzy merges, we manually reviewed the matches with scores exceeding the thresholds outlined in the table. Within these potential matches, we focus on transactions that are most likely to be in the NielsenIQ dataset coverage. With the merged SDC-GS1 dataset, we link the dataset with the NielsenIQ scanner data by matching on UPC prefixes.

2.2 Merger List

Due to our data use agreement with Kilts regarding research around antitrust, we are unable release the list of mergers for this draft.

References

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