# A Large-Scale Evaluation of Merger Simulations

VIVEK BHATTACHARYA[1,2], GASTÓN ILLANES[1,2], AVNER A. KREPS[1],
JOSÉ D. SALAS[3], and DAVID STILLERMAN[4]

[1]Department of Economics, Northwestern University
[2]National Bureau of Economic Research
[3]Cornerstone Research
[4]Kogod School of Business, American University

March 6, 2026

ABSTRACT. Prospective merger simulations are a commonly used tool in industrial organization and antitrust, but evidence about their accuracy is limited. We study 101 mergers in consumer packaged goods and compare the realizations of price changes with predictions from merger simulations. Predicted price changes from merger simulations are typically larger than realized ones. We explore whether these deviations are consistent with cost synergies or driven by misspecification. Despite the overprediction, we find that merger simulations are more effective than structural presumptions at identifying mergers with large price changes.

KEYWORDS. Antitrust, Merger Simulation.

JEL CODES. D43, K21, L13, L41.

CONTACT. vivek.bhattacharya@northwestern.edu, gaston.illanes@northwestern.edu, avner@u.northwestern.edu, jdsalas@u.northwestern.edu, stillerman@american.edu. We would like to acknowledge Allan Collard-Wexler, Mike Dinerstein, Igal Hendel, JF Houde, Alessandro Iaria, Sarah Johnston, Lorenzo Magnolfi, Rob Porter, Chris Sullivan, and Daniel Yi Xu for useful feedback, as well as seminar audiences at the 2025 Econometric Society World Congress, the Chicago IO Fest, 2026 ASSA Meetings, Duke, Calgary, and Purdue. We are grateful for excellent assistance from numerous research assistants; a list can be found in the acknowledgments in Bhattacharya et al. (2025). This project was funded by grants from the Center for Equitable Growth and the National Science Foundation (SES-2116934). Researcher(s) 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. José D. Salas worked on this paper while a PhD student at Northwestern University. All errors are our own.

## I. Introduction

Antitrust agencies frequently make decisions about whether to challenge proposed mergers and thus must make prospective predictions of their effects. A commonly used tool to guide these decisions is a merger simulation. Merger simulations have become a workhorse tool in industrial organization and are prominent in both the classroom and the courtroom (Werden and Froeb, 2006). However, despite repeated calls for it (Whinston, 2007; Angrist and Pischke, 2010; Nevo and Whinston, 2010), there is very little work comparing the predictions from these simulations to realized price changes.

Prior work that has tackled this question has typically focused on a small handful of mergers at a time. This paper does so in the context of 101 mergers in US consumer packaged goods (CPG) from 2006 to 2017. These mergers are a large subset of all transactions with a deal size larger than $280 million involving CPG products sold through retail outlets. For each merger in our sample, we run every step of a textbook merger simulation (Hausman et al., 1994; Werden and Froeb, 1994; Nevo, 2000) using state-of-the-art methods. We first collect product characteristics and cost shifters for each market and estimate a mixed logit demand system (Berry et al., 1995) using pre-merger data on prices and quantities sold. We invert the first-order conditions from a Bertrand-Nash model to recover implied marginal costs, and we project these recovered costs onto cost shifters using nonparametric methods. We finally forecast demand and costs into future periods and solve the first-order conditions of the Bertrand-Nash game with the post-merger ownership structure. This procedure gives us predicted prices for about 1,000 markets (geography-month pairs) for each merger, typically with dozens of products per merger.

One main advantage of this scale is that we are able to identify systematic patterns in the comparisons between merger simulations and realizations of post-merger prices. We first analyze these results through the lens of accuracy. We find that merger simulations systematically overpredict price increases. The median overprediction is 2.6% overall, and 4.0% when considering merging party price changes. Simulations overpredict price changes in about 70% of mergers. This overprediction increases over time and is thus not due to a delay in adjusting to the next equilibrium.

Why do merger simulations overpredict price changes? We find limited support for changes to demand or product assortment driving the prediction error, and we thus use these results to understand the "supply-side residual." Under the assumption that Bertrand-Nash conduct holds after the merger, this prediction error is attributable to changes in marginal costs. Maintaining this assumption, we find that average synergies for merging parties amount on average to 2.1% of the initial price of the product. Moreover, patterns in the data offer mixed support for the hypothesis that these "implied synergies" are driven by true changes in marginal cost. First, one would not expect non-merging parties to directly realize synergies: these implied synergies are indeed smaller for non-merging parties, although they are still at around 1% in magnitude. Second, the extent of synergies increases over time since merger completion, consistent with operational changes (or contract renegotiation, if synergies are from increased buyer power) taking time to materialize. Third, consistent with distribution improvements leading to synergies, synergies for merging parties are correlated across products within-geography: DMAs explain 16% of the variation in synergies across merger-DMA-product groups. Synergies are also clustered across geographies within-product (with an $R^2$ of 0.48), consistent with operational changes at the product level. We also find, however, that under the Bertrand-Nash assumption, the implied synergy is systematically larger when the unilateral effect of the merger—the incentive to change prices due to the merger—is larger. There are some plausible explanations for this correlation at certain levels of analysis: for instance, if agencies expect an especially large incentive to increase price post-merger, they may expect the merger to generate synergies in order to avoid a challenge. We discuss potential reasons for the correlation at other levels, but we find them a priori unlikely within-merger and view this correlation as suggestive that some portion of the supply-side residual may not be due to synergies.

We thus turn to alternate explanations for this overprediction. We first consider the possibility that merged firms do not fully internalize each others profits when setting prices, which could be a consequence of organizational frictions. We estimate an imperfect internalization parameter for each merger by using the intuition that (i) correlations between implied synergies and unilateral effects, across markets within a merger, and (ii) synergies for non-merging parties might be signs of model misspecification rather than true properties of synergies. While there is significant variation in this conduct parameter across mergers, we find frequent departures from Bertrand-Nash conduct post-merger. On average, merging parties only internalize slightly less than half of the profits of their new partners. Second, we consider the potential role of retail markups through the lens of a calibrated model; we find that even under parameters previously used in the literature, merger simulations overpredict price increases. Finally, we study the possibility of demand misspecification by studying robustness to our demand estimates. In all of these exercises, we still find a role for synergies for merging parties, on average.

Even if merger simulations are biased on average, they could still be useful for predictive purposes. We find that they are directionally calibrated: a regression of a measure of the causal price change due to the merger on the unilateral effect predicted by the merger simulation yields a slope of 0.77 (s.e. 0.25) across mergers, and with similar results within-merger. However, the predictive power of the unilateral effect is quite low, as unilateral effects only explain about 8%–14% of the variation in causal effects. Nevertheless, we find that simulations are better at discriminating mergers with large price changes than predctions from just structural presumptions across a variety of metrics. We conclude by analyzing these implications of these results for merger policy through a simple statistical decision theory framework, and we find that merger simulations allow decision-makers to target a challenge rate closer to the optimal one.

*Related Literature.* The literature comparing predictions from merger simulations to realizations is sparse, and each paper in this literature studies a very small handful of mergers. Nevo (2000) studies two ready-to-eat cereal mergers, Peters (2006) studies five airline mergers, Weinberg (2011) and Weinberg and Hosken (2013) together study three mergers of consumer goods, Houde (2012) studies one merger in retail gasoline, Björnerstedt and Verboven (2016) studies one merger of pharmaceuticals, and Miller and Weinberg (2017) study one joint venture in beer.[1] With the exception of Nevo (2000) and one merger in Weinberg and Hosken (2013), most simulations depart significantly from the realizations. Reasons for the departure are varied and include unexplained supply-side issues (Peters, 2006), potential issues in demand estimation (Weinberg and Hosken, 2013; Björnerstedt and Verboven, 2016), and post-merger coordination (Björnerstedt and Verboven, 2016; Miller and Weinberg, 2017). Relative to this literature, our main contribution is scale: by studying a large set of mergers—and both the cross-merger and within-merger heterogeneity in results—we can find systematic patterns.

The literature on synergies attributable to mergers is also sparse: Asker and Nocke (2021, p. 221) note that "[e]vidence as to the typical size of merger efficiencies is also extremely limited." The extant literature can be organized into three groups. Merger retrospectives (Kim and Singal, 1993; Focarelli and Panetta, 2003; Bhattacharya et al., 2025) often attribute decreases in prices to synergies but do not provide a quantification of them. A second set of papers retrospectively estimates direct proxies for synergies, such as distribution distance in beer (Ashenfelter et al., 2015), heat rate in power plants (Demirer and Karaduman, 2025), inventory and capacity utilization in manufacturing (Braguinsky et al., 2015), or costs in hospitals (Schmitt, 2017). The final approach is closest to ours: some of the aforementioned papers evaluating merger simulations provide implied synergies required to rationalize the price effect as an auxiliary result.[2] We take a similar approach—although we interact it with our conduct estimation—and are able to provide estimates of synergies on a much broader scale than previous papers in the literature.

Moving beyond merger simulations, despite the rapidly growing importance of structural modeling in empirical economics, there are few tests of the accuracy of counterfactual predictions. A limited set of examples include an analysis of microfinance (Kaboski and Townsend, 2011), predictions of auctions models (Bajari and Hortacsu, 2005; Athey et al., 2011), discrete choice

[1] Garmon (2017) is distinct in both the focus on hospital mergers and in the quantity: he studies 28 mergers and evaluates a number of screening tools, although he assesses "reduced-form" merger simulations. Similar papers include Fournier and Gai (2007) and May and Noether (2014), which evaluate other reduced-form screens using in merger analysis, and Dranove and Ody (2016), which evaluates the effectiveness of these screens in predicting the cross-sectional variation in prices. Finally, though it does not evaluate merger simulation directly, Panhans and Taragin (2023) emphasizes the effects that a potentially misspecified demand model can have on the outcome of merger simulation and merger review.

[2] Some papers cannot be classified into these bins. Grieco et al. (2018) estimates returns to scale from a production function, makes predictions of these efficiencies based on expected changes to quantities, and verifies these predictions with post-merger data in the context of beer. Jeziorski (2014) use the decision to merge as a way to estimate expected synergies.

models (Pathak and Shi, 2021; Raval et al., 2022), spatial economics (Dingel and Tintelnot, 2025), and trade (Kehoe et al., 2017; Adão et al., 2025). We believe more work is warranted, and we view this paper as a contribution to this literature.

## II. Data and Sample Selection

### II.A. Data

Our main source of data for demand estimation is from NielsenIQ (2025). NielsenIQ provides a Retail Scanner Dataset, which provides prices and quantities at the UPC-store-week level for about 2.6–4.5 million UPCs, depending on the sample year. The dataset covers food, non-food grocery items, health and beauty aids, and select general merchandise. It also has fairly comprehensive coverage of retail outlets in the United States.[3] NielsenIQ provides an additional dataset called the Consumer Panel, which tracks shopping behavior by individual households over time and provides properties of these households. We have access to this sample from 2006 to 2018.

We collect external sources of data to supplement the NielsenIQ dataset on three dimensions relevant for demand estimation. First, since NielsenIQ does not provide ownership of each product, we obtain this information from a combination of Euromonitor Passport (Euromonitor International, 2022) and manual searches. Second, we collect potential cost shifters for each merger by listing inputs (e.g., wheat for cereal) and obtaining commodity price indices, typically from Federal Reserve Economic Data (FRED) from the Federal Reserve Bank of St. Louis (2025). Third, the NielsenIQ dataset has limited information about product characteristics, with variables that differ at the product module level. We supplement this dataset with additional product characteristics. Some of these characteristics are from unstructured searches, but many are from decoding the abbreviations and information embedded in the NielsenIQ UPC description. Table C.1 includes a list of the instruments and product characteristics used in each merger.

### II.B. Merger Selection, Market Definition, and Product Definition

NielsenIQ's data structure provides additional information that is useful for demand estimation. First, Nielsen defines a unit of geography called a designated market area (DMAs), which is a collection of counties that is subject to the same local television stations. We follow other papers that conduct demand estimation (Nevo, 2000; Miller and Weinberg, 2017) and use the DMA to construct a geographic market. Second, NielsenIQ assigns products to "modules." We typically use modules as our definition of a product market, a decision that is aligned with other papers

[3] NielsenIQ describes this dataset as providing "scanner data from 35,000 to 50,000 grocery, drug, mass merchandise, and other stores, covering more than half the total sales volume of US grocery and drug stores and more than 30 percent of all US mass merchandiser sales volume."

that do demand estimation at scale (Atalay et al., 2025; Döpper et al., 2025). However, we have found through manual inspection that it is sometimes reasonable to combine modules together: for instance, we combine the "Wine-Domestic Dry Table" and "Wine-Imported Dry Table" modules. In a small handful of cases, we also found it reasonable to split modules based on characteristics when we think that products in each split are unlikely to be substitutes.[4] Table C.1 provides the full list of product markets used in our analysis. As discussed in Bhattacharya et al. (2025), these market definitions seem consistent with both the so-called *Brown Shoe* factors cited in the 2023 Merger Guidelines and with definitions used historically by the DOJ and FTC. We also believe that our definitions of markets are likely to contain most relevant substitutes for the products.

Our goal is to obtain a relatively large sample of mergers that are representative of typical mergers in consumer packaged goods. To do so, we follow a procedure very similar to the one in Bhattacharya et al. (2025). We start with a set of deals tracked by SDC Platinum from Thomson Reuters (2021), which provides comprehensive information on mergers, acquisitions, and joint ventures, and we restrict to deals that involve mergers of manufacturers of products sold in groceries and mass merchandisers. We consider every such deal with a transaction size larger than $280 million, and we define a merger to be a combination of a deal and a product market. We only keep mergers where both the target and the acquirer sold at least one UPC in the same market with non-negligible market share—which ensures that we retain national, seasonal, and important regional products.[5] This leads to 115 mergers over 45 deals. Finally, for computational reasons, we filter each merger to the top 50 DMAs by sales volume.

We must also take a stance on the definition of a product in our sample. Papers differ in whether the definition of a product is the UPC (Atalay et al., 2025), the brand (Döpper et al., 2025), or some level of aggregation between the two (Backus et al., 2021). The granularity of brands differs significantly across modules in the Nielsen data, so we also adopt an intermediate approach: in most markets our definition of a product is at the unique brand-characteristics level, but we sometimes define a product as a UPC when aggregating is not useful. For instance, the "Chili-Shelf Stable" module has 343 unique UPCs, only 215 of which have unique brand-characteristics, while the "Stew - Beef - Shelf Stable," "Stew - Chicken - Shelf Stable," and "Stew - Remaining - Shelf Stable" modules (which we group into one product market) have 98 UPCs, 86 of which have unique brand-characteristics, despite the ex-ante similarity of the characteristics we define on each.[6] We

[4]For instance, we split "Baby Milk and Milk Flavoring" by whether the formula is for infants. The "Seasoning-Dry" module contains many different types of spices, such as garlic powder and cinnamon, which likely do not compete with each other. Dog and cat treats likely do not compete with each other in the "Dog and Cat Treats" module.

[5]Appendix D of Bhattacharya et al. (2025) shows that this sample selection procedure leads of high coverage of UPCs within each product market.

[6]In addition to package size and store brand, which are used for every product market, we define "meat type," "is hot," "has beans," and "chili con carne" characteristics for the chili product market, and "Brunswick," "contains beef," "contains chicken," "microwave," and "regular flavor" characteristics for the stew product market.

therefore aggregate within brand-characteristics for chili but keep stew at the UPC level.

### II.C. Descriptive Statistics

Panel A of Table 1 reports summary statistics for our initial sample. The markets in our sample are fairly concentrated, with the median Herfindahl-Hirschman Index (HHI) around 2,700.[7] Many mergers have limited impacts on concentration: the median merger has a change in HHI (DHHI) around 15, and about a fourth of mergers in our sample have a DHHI larger than 100. We also have a large variety of different mergers in the dataset. Variation in the market size of the mergers is large, and there is a fair amount of variation in the number of products per merger. Finally, the most common NielsenIQ "department" for our mergers is dry groceries.

We are unable to obtain reasonable demand estimates for some of the mergers in our dataset: Section III.B provides the criteria. Panel B shows the same summary statistics for the sample for which we have estimates, and the patterns are similar. The share of large-DHHI mergers is slightly smaller, and we have a smaller share of mergers in health and beauty products.

Table C.1 shows the characteristics we collect. Since they are varied across mergers, we are unable to provide detailed distributions for each of the potential characteristics. However, given the characteristics are used to capture substitution between products, we compute distance between these characteristics. To compute this distance in a uniform way across mergers, we conduct a principal component analysis of each set of characteristics, take the first two principal components, and normalize these to $z$-scores. (In Section III.A, we note that procedure maps to the characteristics used for demand estimation.) We then compute the average Euclidean distance between competing products. Each set of three columns of Table 2 shows this distance between different sets of products. Our first observation is that the diagonal of each matrix is smaller than the elements in corresponding rows and columns: the acquirers' products are more similar to their own products than to other firms'. Second, the distance between the acquirer and the target products is smaller than that between the non-merging parties' products and either the acquirers' or targets'. This provides some evidence that firms with similar products are more likely to merge, an observation that has not been made systematically in the literature. Moreover, to the extent that these characteristics are valued by consumers, they will feed into larger diversion between merging parties than between merging and non-merging parties.

[7]The HHI is the sum of the squared market shares of each firm, and we compute this using the inside shares. We compute the change in the HHI (which we refer to as the DHHI) as the naive, or pro-forma, HHI, assuming that the market shares of merged entities would be the sum of the pre-merger market shares. Without divestitures, this is simply twice the product of the pre-merger market shares of the merging parties. We scale both these from 0 to 10,000.

| | Mean | St. Dev. | Quantiles 10% | 25% | 50% | 75% | 90% |
|---|---|---|---|---|---|---|---|
| A. Initial Sample ($N = 115$) | | | | | | | |
| HHI | 2,961 | 1,252 | 1,527 | 2,030 | 2,680 | 3,781 | 4,588 |
| DHHI | 111.7 | 220.4 | 0.0 | 1.2 | 14.6 | 163.8 | 293.6 |
| $\mathbb{1}$[DHHI $>$ 100] | 0.25 | 0.44 | 0 | 0 | 0 | 0.5 | 1 |
| Merging Party Share | 0.19 | 0.17 | 0.03 | 0.05 | 0.14 | 0.30 | 0.49 |
| Avg. Market Size ($1,000s) | 631 | 891 | 29 | 79 | 304 | 713 | 1,703 |
| Number of Products | 80 | 47 | 22 | 42 | 80 | 113 | 146 |
| Share in Department | | | | | | | |
| Dry Grocery | 0.62 | | | | | | |
| Health & Beauty Care | 0.10 | | | | | | |
| Alcoholic Beverages | 0.13 | | | | | | |
| B. Estimation Sample ($N = 101$) | | | | | | | |
| HHI | 2,961 | 1,252 | 1,527 | 2,030 | 2,680 | 3,781 | 4,588 |
| DHHI | 111.7 | 220.4 | 0.0 | 1.2 | 14.6 | 163.8 | 293.6 |
| $\mathbb{1}$[DHHI $>$ 100] | 0.29 | 0.45 | 0 | 0 | 0 | 1 | 1 |
| Merging Party Share | 0.19 | 0.17 | 0.03 | 0.05 | 0.14 | 0.30 | 0.49 |
| Avg. Market Size ($1,000s) | 610 | 908 | 29 | 65 | 242 | 655 | 1,622 |
| Number of Products | 77 | 46 | 21 | 41 | 76 | 111 | 138 |
| Share in Department | | | | | | | |
| Dry Grocery | 0.62 | | | | | | |
| Health & Beauty Care | 0.08 | | | | | | |
| Alcoholic Beverages | 0.15 | | | | | | |

Table 1: Summary statistics. HHI, DHHI, merging party share, average market size, and number of products are all averaged at the market (DMA-month) level within merger. Other departments include Frozen Food, General Merchandise, Non-Food Grocery, and Packaged Meat.

| | All Characteristics | | | Price | | | Non-Price Characteristics | | |
|---|---|---|---|---|---|---|---|---|---|
| | Acquirer | Target | Non-Merging | Acquirer | Target | Non-Merging | Acquirer | Target | Non-Merging |
| Acquirer | 1.50 | 1.77 | 2.00 | 0.52 | 0.81 | 0.88 | 1.21 | 1.35 | 1.59 |
| Target | 1.77 | 1.47 | 2.14 | 0.81 | 0.63 | 1.02 | 1.35 | 1.05 | 1.65 |
| Non-Merging | 2.00 | 2.14 | 1.95 | 0.88 | 1.02 | 0.82 | 1.59 | 1.65 | 1.57 |

Table 2: Average Euclidean distance in normalized characteristics space between competing products. All characteristics are normalized into $z$-scores before computing distances. Averages are weighted by shares within markets and then unweighted across markets and mergers.

## III. Demand Estimation

### III.A. Model

For each merger, we estimate a differentiated products demand system following Berry et al. (1995), which has become the standard model in industrial organization for demand for consumer packaged goods. We define a market to be a DMA-month pair and aggregate price and quantities to the product level across all retailers and weeks within a market, weighing these prices and quantities by the relative share in each retailer-week. For each product $j$ in DMA $d$ at time $t$, we define the indirect utility to consumer $i$ as

$$u_{ijdt} = \beta_i^0 + \alpha_i p_{jdt} + \boldsymbol{\beta}_i' \boldsymbol{x}_j + \xi_j + \xi_d + \xi_{\text{quarter}(t)} + \xi_{jdt} + \epsilon_{ijdt}, \tag{1}$$

where $p_{jdt}$ is the price of the product, $\boldsymbol{x}_j$ is a vector of product characteristics, $(\xi_j, \xi_d, \xi_{\text{quarter}(t)})$ are product, DMA, and quarter-of-year fixed effects, $\xi_{jdt}$ is an unobserved shifter of utility common across all individuals, and $\epsilon_{ijdt}$ is a Type I extreme value shock that is iid across products. The utility of the outside option is $u_{i0dt} = \epsilon_{i0dt}$, another independent draw from a Type I extreme value distribution. We parameterize the vector of random coefficients as

$$\begin{aligned} \beta_i^0 &= \Pi^{\beta^0} D_i + \sigma^0 v_i^0 \\ \alpha_i &= \alpha + \Pi^{\alpha} D_i + \sigma^{\alpha} v_i^{\alpha} \\ \beta_{ki} &= \Pi^{\beta_k} D_i + \sigma^k v_i^k, \end{aligned} \tag{2}$$

where the $v_i^{\times}$ are independent standard normal draws, and $D_i$ is a vector of demographics. We use $\boldsymbol{\theta}$ to refer to all the structural parameters to be estimated; this consists of $\alpha$, all $\Pi^{\times}$, and all $\sigma^{\times}$.

The parameterization in (1) and (2) allows for both vertical differentiation across products and horizontal differentiation through the random coefficients. The random coefficients also allow for a departure from share-based substitution and allow for substitution towards products that are closer in characteristics space or in price. The demographic and random coefficient on the constant $\beta_i^0$ breaks share-based substitution to the outside option. Throughout our analysis, we focus on an indicator for whether household income is above \$100,000 as the demographic of interest, although in some specifications we set some $\Pi^{\times}$ to 0, and sometimes $\sigma^0$ to 0.

Papers that estimate demand at scale (Atalay et al., 2025; Döpper et al., 2025) do not use product characteristics to model substitution, likely due to both the difficulty in collecting this data and the difficulty in choosing characteristics to use in the demand system. As mentioned in Section II.C, we tackle the second problem by using the first two principal components of the set of characteristics we collected by hand, an approach used by Backus et al. (2021). A benefit of this approach in our setting is that it allows us to avoid arguably arbitrary decisions about the choice of characteristics for each market and limits researcher degrees of freedom. It also allows us to account for discretely coded characteristics in a sensible manner. This approach allows us to incorporate the most variation possible from the characteristics while respecting computational limits. There are some drawbacks. First, the first two principal components do not always capture a large share of the variation in the characteristics: they explain more than 50% of the variation in characteristics in about half of the mergers (see Table C.1). Second, this approach also limits the interpretability of random coefficients themselves. Perhaps more importantly, it is not clear that distance in the space of hand-coded characteristics is the variation that is perceived by consumers when they assess whether products are substitutes: an approach such as in Magnolfi et al. (2025) that determines characteristics from embeddings of consumer surveys would be promising, but infeasible for historical data.

Finally, we must compute market sizes (or, equivalently, shares of the outside option). Some prior papers studying individual markets take market sizes as proportional to population, where the proportion is based on an upper bound for inside purchases devised from facts about the product market (Nevo, 2000; Backus et al., 2021). The diversity of product markets in our sample makes this approach more difficult to implement in our setting. Instead, we take a simpler approach by calibrating the market sizes in each DMA as 1.5 times the largest sales quantity in that DMA throughout the sample, such that the maximum share of the outside good is 1/3. This is consistent with the approach in Miller and Weinberg (2017) and Atalay et al. (2025).

### III.B. Estimation

Demand estimation follows the algorithm of Berry et al. (1995). Given a guess of the parameters $\theta$, we solve for values of mean utilities $\delta_{jdt}$ that make observed and predicted shares match. We then project $\delta_{jdt}$ onto linear components $(\alpha, \xi_j, \xi_d, \xi_{\text{quarter}(t)})$ with two-stage least squares, instrumenting for $\alpha$, and recover $\xi_{jdt}(\theta)$. We then search for values of $\theta$ that minimize a GMM objective including both standard instrument-exclusion moments and micro-moments (Berry et al., 2004).

Identification requires at least four instruments: one for the mean price coefficient and one for each of the random coefficients on price and the first two principal components of characteristics. Our main class of instruments is the differentiation instruments formulated by Gandhi and Houde (2019). For each characteristic, we compute the quadratic distance between product $j$'s location in characteristic space and that of its competitors as $z^x_{jdt} = \sum_k (x_{jdt} - x_{kdt})^2$. Relevance comes from the notion that products that face fewer nearby competitors along a certain dimension of characteristics space tend to have higher prices, while exogeneity comes from the assumption that product characteristics are exogenous. These instruments are computed on the full set of characteristics before taking principal components. We then estimate a measure of predicted price given exogenous variables $\widehat{p}_{jdt} = \mathbb{E}[p_{jdt}|x_{jdt}, z_{jdt}]$ using a linear fixed effects regression, controlling for DMA-product fixed effects and all previously computed differentiation instruments, and construct a differentiation instrument from the fitted values $z^{\widehat{p}}_{jdt} = \sum_k (\widehat{p}_{jdt} - \widehat{p}_{kdt})^2$.

We also consider two potential alternative classes of instruments. First, for many mergers, we are able to identify inputs that have producer price indices in FRED, and we use the indices as instruments that exogenously shift costs. We interact these with package size because input costs should be expected to shift the prices of larger products by proportionately more. For a subset of these mergers, only products sharing a certain characteristic are expected to use a particular input; in these cases, we also interact the input prices with an indicator for whether the product has that value of the characteristic. Second, as an easily generalizable measure of common cost shocks, we use Hausman instruments: the average price of product $j$ in time $t$ in DMAs bordering DMA $d$. Naturally, a concern with this strategy is that bordering DMAs may have correlated demand shocks. Nevertheless, we have decided to incorporate these instruments due to their ubiquity in the literature and the dearth of cost side instruments for some markets.

For the demand estimates that enter our simulations, we want to use the most relevant set of instruments while maintaining the plausibility of the exclusion restriction and avoiding weak instruments. To that end, we prefer the differentiation and input price instruments over Hausman instruments when possible, as Hausman instruments require relatively stronger assumptions to be valid, but must occasionally rely on the Hausman instruments when input prices and differentiation instruments are too weak. The following instrument selection procedure captures these goals. We start with differentiation instruments (which are used in all specifications), and add input prices if their partial $F$-statistic in the first-stage regression of prices on differentiation instruments, input price instruments, and fixed effects is at least 20.[8] If the input prices are insufficiently predictive, then we use only the differentiation instruments if their first-stage $F$-statistic is at least 100. Finally, if the differentiation instruments, by themselves or with input prices, are too weak ($F < 100$), we rely on Hausman instruments and differentiation instruments. We run one-step GMM with the instruments resulting from this iterative procedure, then compute the feasible approximation to the optimal instruments (Chamberlain, 1987; Berry et al., 1995; Reynaert and Verboven, 2014) and run two-step GMM with these instruments. We take this set of results as successful if the objective converges within our prespecified tolerance, the probability of an individual having a positive price sensitivity is less than 5%, the median own-price elasticity is between -0.5 and -20, and the average predicted post-merger change in price is less than 25%.[9] If estimation is unsuccessful with the initial set of instruments, we try the other possible sets of instruments in the following order: differentiation with input prices, differentiation only, and differentiation with Hausman.[10]

We also attempt to use the richest utility specification possible. We start by attempting to estimate the demand system where demographics enter all the random coefficients in (2). If estimation is

[8]The input prices only vary temporally unless interacted with characteristics, and for some product markets we are unable to identify producer price indices for instruments that comprise a substantial share of input costs.

[9]In practice, the final constraint only rules out estimates predicting nonsensically large price changes. In practice, this typically comes from our cost forecasting procedure, described in Section VI.A, returning large changes in costs rather than demand estimates directly. We are currently working on further disciplining this procedure to avoid such issues.

[10]Results using different sets of instruments are correlated, although not perfectly: the pairwise correlations in estimated median own-price elasticities are 0.59 with just differentiation instruments vs. differentiation and input prices, 0.36 for just differentiation vs. differentiation and Hausman, and 0.41 for differentiation and input prices vs. differentiation and Hausman.

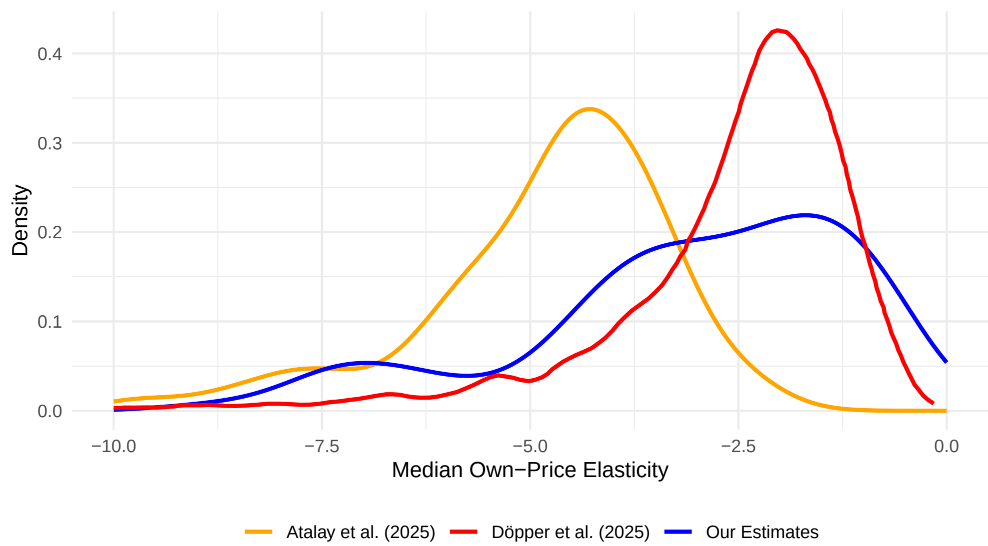



Figure 1: Comparison of our estimated elasticities with Atalay et al. (2025) and Döpper et al. (2025). Elasticities from Atalay et al. (2025) are unweighted 2010 elasticities.

unsuccessful, we consider a specification where demographics only interact with the constant and price coefficient. Finally, we consider a specification where demographics interact only with prices. If none of these specifications yield acceptable demand estimates, we set $\sigma^0 = 0$ and go through this list again. In no case do we drop random coefficients on the characteristics or the price. To identify the demographic interactions, we use the Nielsen panel data to formulate micro-moments $\mathbb{E}[x_j|D_i]$, the average value of the characteristic conditional on the demographic (Berry et al., 2004; Conlon and Gortmaker, 2025). For demographic interactions with the constant, we use $\mathbb{E}[D_i|j > 0]$; that is, the average value of each demographic level given purchase of an inside good.

To ensure we adhere to best practices, we use pyBLP (Conlon and Gortmaker, 2020) for estimation. Finally, to relieve concerns about local minima, we start estimation at two points: the result from nested logit using inside-outside nests, and the solution to the "FRAC" procedure of Salanié and Wolak (2022). If solutions from these initial points do not coincide, we restart estimation at a grid of starting points around these two values and pick the best solution. The instruments and specifications used for each merger in our estimation sample are listed in Table C.1.

### III.C. Estimates

Figure 1 and Table 3 show the distribution of estimated median own-price elasticities across mergers. The median of this distribution is -2.88. The bulk of the distribution of median elasticities is between -1 and -5, with an interquartile range of [-4.10, -1.59]. However, there is a relatively sizable tail of more elastic estimates (some of which are truncated from the plot for readability); the average across all mergers is -3.51, with a $10^{\text{th}}$ percentile value of -7.07.

| | Mean | St. Dev. | Quantiles 10% | 25% | 50% | 75% | 90% |
|---|---|---|---|---|---|---|---|
| Our Estimates | -3.51 | 2.97 | -7.07 | -4.10 | -2.88 | -1.59 | -0.91 |
| Atalay et al. (2025) | -4.88 | 1.60 | -7.24 | -5.55 | -4.57 | -3.84 | -3.42 |
| Döpper et al. (2025) | -2.13 | 1.12 | -4.36 | -3.11 | -2.25 | -1.65 | -1.16 |

Table 3: Comparison of our estimated elasticities with Atalay et al. (2025) and Döpper et al. (2025). Elasticities from Atalay et al. (2025) are unweighted 2010 elasticities.

As these demand elasticities are a key input into predictions from merger simulations, we perform a number of auxiliary checks on them. First, we compare these elasticities to others in the literature. Figure 1 and Table 3 report elasticity distributions estimated by Atalay et al. (2025) and Döpper et al. (2025). The main part of our distribution is more similar to that of Döpper et al. (2025), while Atalay et al. (2025)'s distribution is more elastic in the middle. However, the left tail of our distribution is more similar to that of Atalay et al. (2025). Table A.1 provides a point-by-point comparison of elasticities when possible: our estimates are generally less elastic than those in the literature, and there are some markets where our estimates lie far from other estimates. However, for most markets, our estimates are close to the (sometimes large) range documented in other work. These comparisons are not perfect given differences in the definition of products and markets across papers, but overall these exercises provide some evidence that our estimates are not out-of-line from the literature.

Second, we ask whether our demographic interactions are sensible. Work that estimates demand for consumer packaged goods typically finds that high-income consumers are generally less price sensitive than low-income consumers (Backus et al., 2021; Döpper et al., 2025). Consistent with the literature, we find that this is the case for the majority of our demand estimates as well (see Figure A.1). The median median elasticity is -4.14 for low-income consumers, as compared to -2.17 for high-income consumers.

Finally, we ask whether the demand system picks up on the relative product similarity of the acquirer and target products. We find greater diversion between the merging parties: on average, the target to acquirer diversion ratio is 32% (1.8 pp) larger than the non-merging party to acquirer diversion ratio, while the acquirer to target diversion ratio is 36% (0.03 pp) larger than the non-merging party to target diversion ratio. Figure A.2 provides a scatter. This suggests that the similarity in characteristics among the merging parties displayed in Table 2 are economically significant when analyzing potential merger effects. Moreover, we find that the mixed logit structure of the demand system does have an impact on these estimates: Figure A.3 plots the distribution of diversion ratios between merging parties and compares them to the share-based diversion we would expect from logit. We find that the diversion ratios are noticeably higher than logit in our estimates, especially in the upper tail, demonstrating that the demand system is estimating an important (and we think sensible) role for horizontal differentiation through the random coefficients.

In sum, our demand estimates appear reasonable. The own-price elasticity estimates, and the differences in price sensitivity between low- and high-income consumers, are in line with those from the literature. Finally, the demand systems pick up on the relative similarity in product characteristics between the merging parties, manifesting in closer substitution patterns between the merging parties than their competitors. These results give us some comfort that our demand estimates will be a reliable input into the merger simulations.

## IV. The Accuracy of Merger Simulations

### IV.A. Implementing the Merger Simulation

Under the Bertrand-Nash conduct assumption, prices $\boldsymbol{p}$ in a market solve

$$D(\boldsymbol{p};\boldsymbol{\theta},\boldsymbol{\xi}) + (\boldsymbol{\Omega} \odot \boldsymbol{\Delta}(\boldsymbol{p}))(\boldsymbol{p} - \boldsymbol{c}) = 0, \tag{3}$$

where $D(\boldsymbol{p};\boldsymbol{\theta},\boldsymbol{\xi})$ is the demand function (which we parameterize with the demand parameters $\boldsymbol{\theta}$ and demand shocks $\boldsymbol{\xi}$ to connect to our procedure), $\boldsymbol{\Delta}(p)$ is the transpose of the Jacobian of demand with respect to $\boldsymbol{p}$, $\boldsymbol{c}$ is the vector of marginal costs, and $\odot$ denotes elementwise multiplication. The $ij$ entry of the ownership matrix $\boldsymbol{\Omega}$ is 1 if products $i$ and $j$ are owned by the same firm, and 0 otherwise.

The typical merger simulation uses the equilibrium conditions in (3) in two ways. First, if the researcher is willing to maintain the assumption that conduct in the pre-merger period is Bertrand-Nash, then (3) can be inverted to solve for costs $\boldsymbol{c}$ in every market. Second, the researcher can change the ownership matrix from $\boldsymbol{\Omega}$ to $\boldsymbol{\Omega}^{\text{post}}$, where $\Omega_{ij}^{\text{post}}$ is now set to 1 if products $i$ and $j$ are both owned by the merging parties. Holding demand and these recovered costs fixed, the researcher then solves (3) for the new equilibrium prices using this new ownership matrix.

In this section, we compute a forecast for the price of product $i$ in DMA $d$ in month $t$ in merger $m$. Doing so requires us to take a stance on (i) the assortment of products in the market in this future period, (ii) demand shocks $\boldsymbol{\xi}$ the firms would face, and (iii) the marginal costs in that market. For (i), we take the set of products in each DMA in the final period before the merger: we are not trying to test models of entry and exit of existing products, and predicting the introduction of new products is beyond the scope of most models. For (ii), we take a simple approach of forecasting $\boldsymbol{\xi}$ in each DMA as the average of the $\boldsymbol{\xi}$ in the three months before the merger. For (iii), for each merger $m$, we first recover marginal costs $c_{idtm}$ for each product-DMA-time combination in the pre-period. We then run a nonparametric regression of $c_{idtm}$ on product characteristics $x_{im}$, cost shifters $w_{idtm}$, and

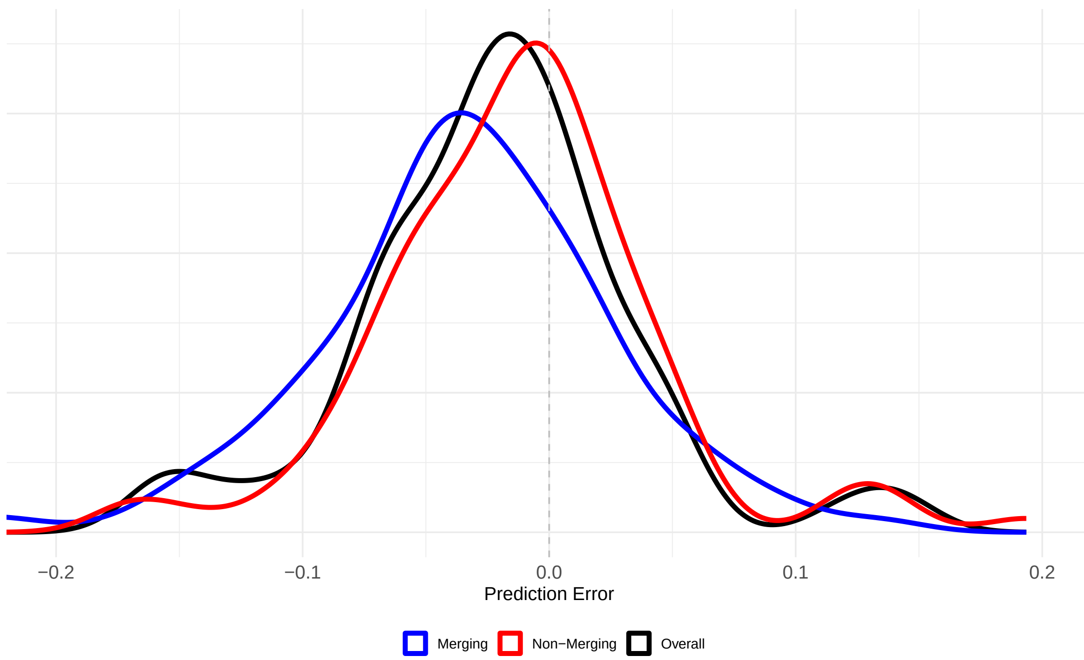



Figure 2: Distribution of prediction errors at the merger level

time. We implement this using flexible B-splines and obtain a cost function $h_m(x, w)$ and implied residuals $\omega_{idtm} \equiv c_{idtm} - h_m(x_{im}, w_{idtm})$.[11] We then forecast $c_{idtm}$ into post-merger periods $t$ as $c^{\text{Pred}}_{idtm} = h_m(x_{im}, w_{idtm}) + \bar{\omega}_{idm}$, where $\bar{\omega}_{idm}$ is the average of $\omega_{idtm}$ in the three periods before the merger. The rationale behind this procedure is that we do not want our measured prediction error to be due to large changes in input costs.

This procedure returns a prediction $p^{\text{Pred}}_{idtm}$ for the price of product $i$ in DMA $d$ in month $t$ in merger $m$. We will compare these with the actual prices $p_{idtm}$ observed in the data. We typically average post-period prices over some time period, and so we drop the $t$ subscript in the notation. We define prediction error as $\text{Error}_{idm} \equiv \log(p_{idm}) - \log(p^{\text{Pred}}_{idm})$, so that a negative number corresponds to merger simulations overpredicting the price increase from a merger. We often aggregate prediction errors further. The product-level prediction error $\text{Error}_{im}$ is computed by taking the average across DMAs, the DMA-level error $\text{Error}_{dm}$ is computed by taking the share-weighted average of prediction errors at the DMA-level, and the merger-level error $\text{Error}_m$ is computed by taking the mean of $\text{Error}_{dm}$ across DMAs.[12]

| | | | Quantiles | | | | |
|---|---|---|---|---|---|---|---|
| | Mean | St. Dev. | 10% | 25% | 50% | 75% | 90% |
| A. Baseline | | | | | | | |
| Merging | -0.043 | 0.089 | -0.127 | -0.079 | -0.040 | 0.011 | 0.039 |
| Overall | -0.024 | 0.068 | -0.101 | -0.056 | -0.026 | 0.006 | 0.043 |
| Non-Merging | -0.016 | 0.072 | -0.095 | -0.056 | -0.016 | 0.019 | 0.056 |
| B. Using Post-Merger Demand and Assortment | | | | | | | |
| Merging | -0.049 | 0.103 | -0.182 | -0.088 | -0.040 | 0.005 | 0.052 |
| Overall | -0.024 | 0.100 | -0.137 | -0.066 | -0.018 | 0.010 | 0.052 |
| Non-Merging | -0.017 | 0.099 | -0.099 | -0.059 | -0.019 | 0.016 | 0.064 |
| C. Adding Partial Best Responses | | | | | | | |
| Merging | -0.040 | 0.088 | -0.137 | -0.086 | -0.042 | 0.008 | 0.051 |
| Overall | -0.023 | 0.097 | -0.120 | -0.064 | -0.018 | 0.010 | 0.049 |
| Non-Merging | -0.014 | 0.099 | -0.088 | -0.051 | -0.018 | 0.018 | 0.065 |
| D. Incomplete Internalization | | | | | | | |
| Merging | -0.047 | 0.125 | -0.152 | -0.086 | -0.036 | 0.012 | 0.063 |
| Overall | -0.020 | 0.098 | -0.121 | -0.065 | -0.013 | 0.014 | 0.059 |
| Non-Merging | -0.016 | 0.100 | -0.093 | -0.059 | -0.019 | 0.018 | 0.064 |
| E. Adding a Retail Markup ($\lambda = 0.2$) | | | | | | | |
| Merging | -0.054 | 0.140 | -0.165 | -0.093 | -0.038 | 0.008 | 0.082 |
| Overall | -0.044 | 0.167 | -0.173 | -0.094 | -0.023 | 0.020 | 0.069 |
| Non-Merging | -0.038 | 0.173 | -0.174 | -0.073 | -0.018 | 0.027 | 0.082 |
| F. At Optimal Scaling of Price Coefficients | | | | | | | |
| Merging | -0.031 | 0.052 | -0.088 | -0.057 | -0.025 | 0.002 | 0.025 |
| Overall | -0.011 | 0.038 | -0.054 | -0.023 | -0.000 | 0.000 | 0.025 |
| Non-Merging | -0.004 | 0.042 | -0.049 | -0.021 | -0.001 | 0.010 | 0.035 |
| G. At Optimal Scaling of Non-Price Coefficients | | | | | | | |
| Merging | -0.036 | 0.065 | -0.098 | -0.061 | -0.036 | 0.001 | 0.031 |
| Overall | -0.016 | 0.040 | -0.062 | -0.033 | -0.008 | 0.000 | 0.022 |
| Non-Merging | -0.007 | 0.045 | -0.058 | -0.025 | -0.002 | 0.013 | 0.035 |

Table 4: Summary statistics of prediction error (log actual price minus log predicted price) on market characteristics, at different levels of observation.

### IV.B. The Distribution of Prediction Errors

Figure 2 plots the distribution of the merger-level prediction error $\text{Error}_m$, both overall and split by merging and non-merging parties. Panel A of Table 4 provides summary statistics of this distribution. Our main finding is that while there is a wide distribution of prediction errors, they are typically negative: merger simulations frequently overpredict price increases. The average prediction error for merging parties is -4.3%, and merger simulations overpredict in approximately three-quarters of mergers. The overprediction is smaller for overall price changes, with a mean of -2.4% and an error distribution that is shifted to the right. This provides a more consistent pattern in results than the limited literature on evaluating merger simulations has provided thus far: Weinberg (2011), Björnerstedt and Verboven (2016), and Miller and Weinberg (2017) document underprediction; Weinberg and Hosken (2013) study one merger with overprediction and one with underprediction, and the sign in Peters (2006) depends on the chosen demand system.

We check two potential sources of this prediction error that would be unrelated to merger simulations. The difference is not due to an initial prediction error that fades as firms adjust to the new equilibrium: the prediction error grows in magnitude over time (Figure A.4). We also compute the prediction error from running this procedure at a point when a merger did not occur. We conduct a "merger simulation" with no ownership changes at a date one year before the merger and predict prices for one year after the placebo date. We find that the prediction error for all parties is centered around zero (Figure A.5), providing evidence that the prediction error we observe is not due to secular trends in costs or demand that are not captured by our forecasting models.

Errors in predictions are clustered within-merger. About one-fourth of all mergers have a realized price increase lower than the predicted one in at least 90% of the DMAs (about one-fifth when studying product-level overprediction); see Figure A.6. While the clustering suggests merger-level misspecification that could lead to these prediction errors, we have not found merger-level observables that are predictive of the prediction error. Table A.2 shows correlations in prediction error within-merger with DHHI. In Section V, we turn to supply-side explanations for our results, and such within-merger patterns inform our estimates.

### IV.C. Decomposing Prediction Errors

The merger simulations in Section IV.B are conducted assuming that neither demand nor product assortment changes after the merger. Using post-merger data, we now evaluate how much of the prediction error is due to changes in demand and assortment. To do so, we take the post-merger

[11]We regularize the cost function by adding a ridge penalty term, and select hyperparameters—the number of knots, degree of the basis polynomial, and weight on the ridge penalty—by minimizing out-of-sample squared error using five-fold cross validation.

[12]Alternate methods of aggregating results, using different forms of price indices, lead to similar conclusions.

assortment in each market as fixed. We take the estimated demand parameters $\boldsymbol{\theta}$ as fixed as well to avoid the (rather expensive) process of re-estimating demand. We then use the post-merger market shares and invert the demand system to recover the post-merger demand shocks $\boldsymbol{\xi}$, which differ from the forecasted shocks used to simulate the merger in Section VI. We use the same marginal cost prediction as in Section VI and simulate post-merger prices as if demand were also known. We still use pre-merger shares to aggregate the prices so that we are comparing the same set of products. Note that while there is some evidence (Atalay et al., 2024; Bhattacharya et al., 2025) that mergers lead to changes in assortment, and mergers may also lead to other changes that affect demand (e.g., advertising, product restocking, etc.), this decomposition does not require us to assume that the changes are causally due to the merger.

Panel B of Table 4 shows the distribution of these prediction errors. Our interpretation is that changes in demand are not a main factor in explaining the prediction error. The mean prediction error for merging parties increases in magnitude to 3.6%, and the mean remains essentially unchanged for merging and non-merging parties. The distribution of prediction error becomes slightly wider in all cases. The limited effect of demand changes on prediction error mirrors the findings in Peters (2006), where this procedure was first proposed, and Weinberg and Hosken (2013).

Another explanation for our findings is that an error in predicting the costs of one of the parties propagates to prediction errors for other parties. Merging parties may realize synergies, say, and non-merging parties in the market may be aware of these synergies and price lower than our model (without synergies) would expect. To evaluate this, we conduct an exercise where we compute the best response of non-merging parties as a whole to the observed post-merger price of the merging parties, and vice versa.[13] We refer to these results as "partial best responses" in Panel C of Table 4. We find some effects in the left tail of the prediction error distribution: the 10$^{\text{th}}$ percentile increases, as does the 25$^{\text{th}}$ for non-merging parties. However, the bulk of the distribution does not change appreciably, reflecting the low diversion estimated between merging and non-merging parties.[14]

Our interpretation of these results collectively is that they point towards the important role of what Peters (2006) calls the "supply-side residual," which captures everything not explained by demand. In the subsequent section, we explore this supply-side residual and evaluate potential explanations for it.

[13] This exercise has similarities to Table 8 of Weinberg and Hosken (2013), which evaluates whether the post-merger first-order conditions are satisfied at the observed prices, to within statistical error.

[14] The limited effect of partial best responses on non-merging party prices is also consistent with prices of non-merging parties responding more to prices of merging parties than one may expect from standard models. While this is not an explanation we pursue further in this draft, we see merit in doing so, as it mirrors an observation in Björnerstedt and Verboven (2016).

## V. Why Are Merger Simulations Biased?

The typical pro-competitive argument for merger simulations is that they would lead to cost synergies which would then be passed through to consumers. The systematic overprediction we document in Section IV is consistent with such synergies being realized, and we investigate these implied synergies in Section V.A. However, they could also point to other departures from the standard model, and we investigate alternative conduct models, retail competition, and demand misspecification in the subsequent subsections.

### V.A. Synergies

In this section, we take the Bertrand-Nash conduct assumption as given and analyze the structure of the marginal cost synergies they imply. Conditional on the assumption, this exercise is informative of the structure of the synergies, but these patterns can also provide a roadmap to assess the plausibility of the conduct assumption itself, as we discuss in Section V.B. With estimates of post-merger demand, we can invert (3) using the realized prices at the realized product assortment and demand shocks $\boldsymbol{\xi}$ and obtain the implied marginal cost $c_{idtm}$. Then, we can define $\text{Synergy}_{idtm} \equiv (c^{\text{Pred}}_{idtm} - c_{idtm})/p^0_{idm}$, a quantity that is positive if marginal costs are below our prediction. We normalize this quantity by the original price of the good to bring units into the same scale and our synergies are thus measured as a proportion of price. As before, we aggregate this quantity over different units.

Figure 3 plots the distribution of implied synergies at the merger level, and Panel A of Table 6 provides summary statistics. The average synergy overall is essentially zero. Merging parties realize an average synergy of 2.1%, with them realizing positive synergies on average in slightly fewer than three-quarters of mergers. Non-merging parties realize negative synergies on average of about 1%, which correspond to cost increases. It is in principle possible for non-merging parties to realize cost changes from a merger: the merger may change bargaining with input suppliers, quantity changes may lead to changes in marginal costs if there are non-constant returns to scale (although we assume them away in our analysis in this paper), and the non-merging parties may be strategically induced into investing in cost reductions if their newly merged competitors realize them (Motta and Tarantino, 2021). However, these channels are certainly less direct than the ones typically cited for merging parties.[15]

Figure 4 shows the distribution of synergies over time, split by merging and non-merging parties. The main observation is that synergies for merging parties increase slightly over time, to a median

[15]To our knowledge, there is no direct evidence of cost changes for non-merging parties in the literature: documenting that the price of non-merging parties could fall after a merger, which has been done in some papers cited elsewhere in this draft, could be attributed to strategic complementarities in pricing.

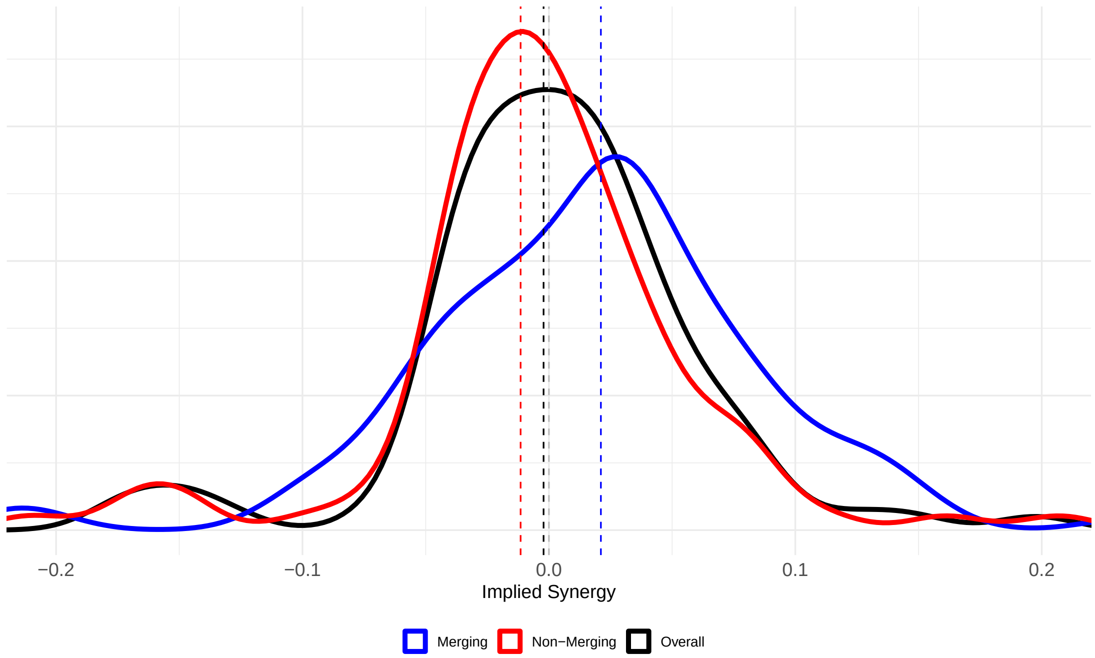



Figure 3: Distribution of implied synergies at the merger level, assuming Bertrand-Nash conduct

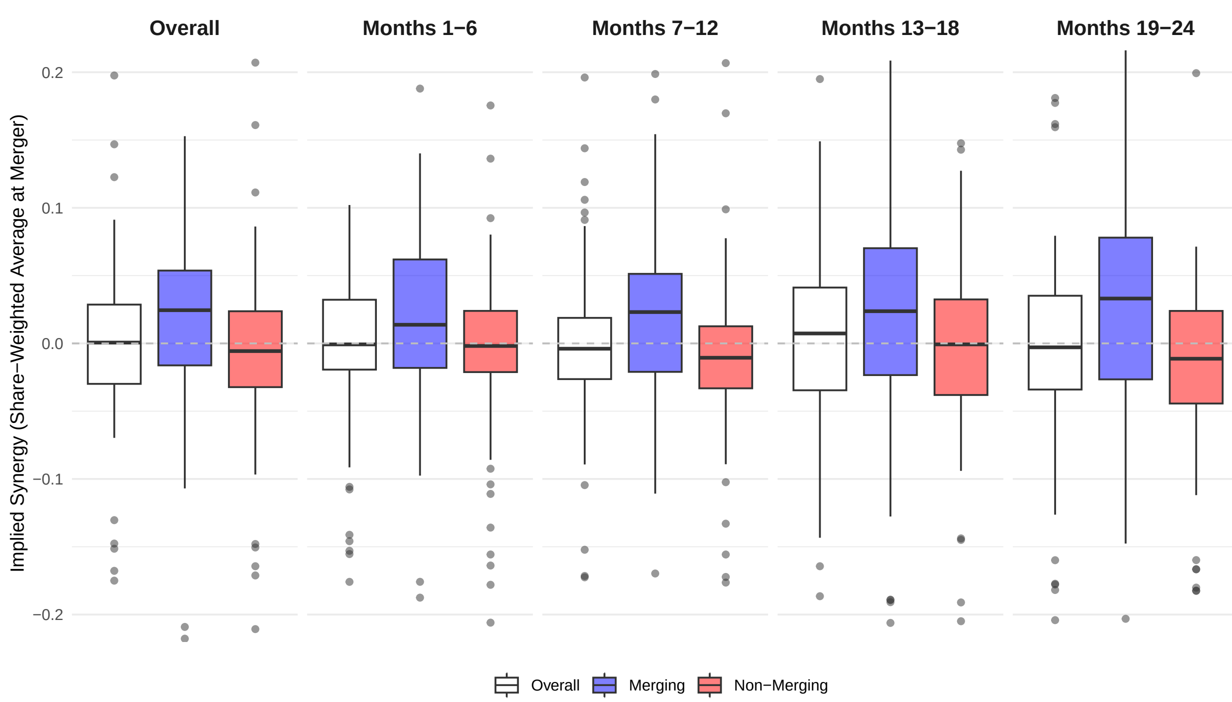



Figure 4: Distribution of implied synergies at the merger level, assuming Bertrand-Nash conduct, over time

of almost 3% 18 months after the merger. That synergies take time to materialize is consistent with plausible economic mechanisms for the source of these synergies—e.g., changes in distribution networks, production processes, or negotiations with upstream suppliers. Prior work documenting synergies after mergers also note that synergies take time to materialize, sometimes even beyond the time period of this analysis.[16] At the same time, the box plots under "Months 1–6" document small immediate synergies for merging parties, with a median synergy of almost 2% and approximately three-fourths of all mergers experiencing synergies. While immediate changes in costs are not implausible,[17] we would also see this pattern if firms do not take into account their market power immediately after the merger, perhaps if pricing must be renegotiated with supermarkets over many months. Alternatively, we would expect this pattern if the conduct model predicts that firms take "more advantage of" their market power than they actually do. The median nonmerging party synergy starts at 0 and changes slightly over the time horizon.

The mechanisms hypothesized above—for merging parties but also for non-merging parties—imply a structure for the observed synergies. First, we would expect synergies to be correlated within-merger. Second, if synergies are due to efficiencies in distribution, we would expect them to be correlated within DMA across products. If they are due to improvements in production costs, perhaps because of unifying production lines, we would expect them to be correlated across DMAs within products or within similar types of products. We study this by regressing Synergy$_{idm}$ on various sets of fixed effects and computing the adjusted $R^2$ of the fixed effects.

Figure 5 reports results from this exercise. For merging parties, about 10% of the variation in synergies across merger-DMA-products is explained by mergers themselves. About 16% is explained by merger-DMA fixed effects, suggesting that synergies are somewhat clustered within DMA after a merger, consistent with the importance of distribution efficiencies. We use brands as a proxy for whether products are similar: to the extent that products within the same brand umbrella benefit from the same production synergies, we would expect merger-brand fixed effects to have substantial predictive power over synergies. We see that they explain about 23% of the variation for merging parties. Finally, merger-product fixed effects explain about 48% of the the variation.[18]

[16] In the case of beer, Ashenfelter et al. (2015) uses distance to market as a measure of efficiencies and finds reductions about two years after the merger, stemming from the time it takes to reorganize production and distribution. Schmitt (2017) uses explicit measures of costs after hospital mergers and documents that about half the measured cost reduction happens 1–2 years after the merger but continues up to 4 years after the merger. Focarelli and Panetta (2003) and Sheen (2014) both use prices as a proxy for synergies: the former find price decreases in bank mergers starting approximately two years after consummation, and the latter finds a decrease in prices in consumer goods that speeds up after a year from the merger.

[17] Demirer and Karaduman (2025) document efficiencies in power plants within a few months after the merger. While it is a priori unlikely that their mechanism applies to our setting of consumer packaged goods, it still provides evidence that efficiencies need not take years to realize.

[18] Of course, these mechanisms need not be the only reasons that implied synergies are correlated within DMAs or within product lines. However, it would have been more difficult to justify them as synergies if we saw very little correlation within these groups.

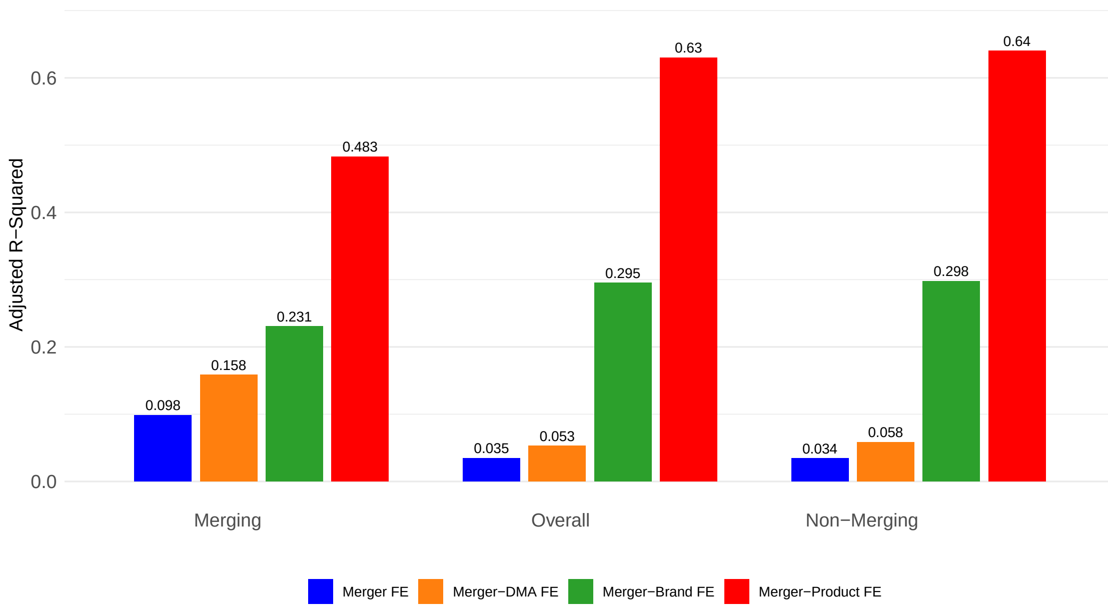



Figure 5: Adjusted $R^2$ values of regressions of fixed effects of various levels on implied synergies

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Merging | 0.63*** | 0.45** | 0.35** | 0.74*** | 0.74** |
| | (0.23) | (0.20) | (0.16) | (0.25) | (0.34) |
| Overall | 1.81** | 1.85** | 1.87* | 0.90*** | 0.89*** |
| | (0.75) | (0.90) | (0.99) | (0.26) | (0.27) |
| Level | Merger | DMA | DMA | Product | Product |
| Merger FE | No | No | Yes | No | Yes |

Table 5: Regression of implied synergies on unilateral effects, at various levels. Each cell corresponds to the coefficient on the unilateral effect in a separate regression. Results in the first row restrict to merging parties, and results in the second row use all parties. Standard errors are heteroskedasticity-robust in Column (1) and clustered at the merger level in all other columns.

By comparison, mergers and merger-DMAs do not explain much of the variation in what we are calling synergies for non-merging parties, although more of the variation is explained by fixed effects related to products.

Which products realize the largest synergies? We take one specific approach to this question. We first compute $\text{Unilateral}_{idtm}$ as the unilateral effect of the merger for product $i$ in DMA $d$ in post-merger period $t$ and merger $m$; we define the specifics of this quantity in Section VI.A, but it quantifies the incentives of the parties to raise prices due to the merger under Bertrand-Nash. We aggregate as usual and regress $\text{Synergy}_{\times}$ on $\text{Unilateral}_{\times}$ at various levels, with and without merger fixed effects. The results in Table 5 show that synergies and unilateral effects are typically significantly positively correlated overall and for merging parties, for a variety of different levels.

There are reasons to believe that this correlation could be positive for merging parties. At the merger level, it may be that selection into merging is such that a merger with large unilateral effect is only allowed through if there are cognizable synergies. Within a merger across products, the products with the largest unilateral effects are closer substitutes for consumers, and this may be due to them being similar on dimensions that allow for production processes to be harmonized between them; this could lead to product-specific synergies for products with large unilateral effects. DMAs where one expects large effects are typically ones with a large presence of both merging parties: the merged entity may have an incentive to relocate distribution closer to these markets. However, another interpretation is that these potential mechanisms are somewhat unlikely within-merger. We entertain this viewpoint in the next subsection to calibrate departures from Bertrand-Nash.

### V.B. Partial Internalization of Within-Firm Profits

Given that overprediction is the modal finding, we focus on conduct models that would deliver a lower price increase due to the merger than Bertrand-Nash. We consider a model of conduct in which merging firms internalize a fraction $\tau_m \in [0, 1]$ of their merging partners' profits in the post-merger period. With $\tau_m = 1$, this model coincides with Bertrand-Nash, and at $\tau_m = 0$ the model captures that the two units of merged entity continue to price separately. Intermediate values of $\tau_m$ capture a trade-off between a division's own profits and its new partner's profits that is less than one-for-one: similar models are commonly used in the literature (Crawford et al., 2018; Backus et al., 2021), and in our setting this may come from, for instance, managerial incentives that depend both on firm and division profits.

We calibrate $\tau_m$ by building off the observation in Table 5 and the associated discussion. In particular, our procedure chooses the value of $\tau_m$ that minimizes the correlation between expected markup changes following the merger and unobservable marginal cost synergies, while maintaining zero synergies for non-merging parties, on average. We assume throughout that conduct in the pre-merger period was Bertrand-Nash.

Under our conduct assumption, the first-order conditions of the firms' price-setting problems imply

$$c_{idtm} = p_{idtm} - \eta_{idtm}(\tau_m), \tag{4}$$

where $c_{idtm}$ is the marginal cost, $p_{idtm}$ is the price, and $\eta_{idtm}(\tau_m)$ is the markup for product $i$ in DMA $d$ in month $t$ and merger $m$ under a particular model of conduct parameterized by $\tau_m$. Note that this expression holds for both the pre- and post-merger periods, and we impose that $\tau_m = 0$ prior to the merger. We specify the change in marginal costs between the period immediately preceding the merger ($t = 0$) and a post-merger period $t$ as

$$c_{idtm} - c_{id0m} = \Delta h(x_{idtm}, w_{idtm}) + \Delta\omega_{idtm}, \text{ where } \mathbb{E}[\Delta\omega_{idtm}|\mathbf{z}_{dtm}] = 0. \tag{5}$$

In the above, $x_{idtm}$ captures product characteristics that affect both demand and marginal costs, $w_{idtm}$ capture variables that affect marginal cost and are excluded from demand, and $\mathbf{z}_{dtm}$ is the full set of exogenous product characteristics, including those that affect both demand and marginal costs, those that affect only demand, and those that affect only marginal costs. We flexibly model the change in marginal costs through $\Delta h(\cdot)$. This function captures changes in the mapping from product characteristics and cost shifters to marginal costs between the pre- and post-merger periods, which could stem from factors such as improvements in the efficiency of input usage.

The conditions (4) and (5) together imply

$$(p_{idtm} - p_{id0m}) - (\eta_{idtm}(\tau_m) - \eta_{id0m}(0)) = \Delta h(x_{idtm}, w_{idtm}) + \Delta\omega_{idtm}. \tag{6}$$

In (6), prices are observed, as are markups, given a value of $\tau_m$ and the estimated demand system. We can therefore treat the left-hand side of the expression as data. We then calibrate the model of conduct by choosing the value of $\tau_m$ that best satisfies two moment restrictions: a conditional moment restriction for merging-party products, $\mathbb{E}[\Delta\omega_{idtm}|\mathbf{z}_{dtm}] = 0$, as well as an unconditional restriction for products sold by non-merging parties, $\mathbb{E}[\Delta\omega_{idtm}] = 0$. In practice, we weaken the conditional moment restriction for merging parties and instead use $\mathbb{E}[\Delta\omega'_{idtm}A(\mathbf{z}_{dtm})] = 0$. Our choice of $A(\mathbf{z}_{dtm})$ is aligned with the discussion of Table 5: we choose $\tau_m$ to minimize the correlation of unobservable synergies ($\Delta\omega_{idtm}$) and expected unilateral effects, conditional on the full set of exogenous product characteristics, $\mathbf{z}_{dtm}$. Specifically, we choose $A(\mathbf{z}_{dtm}) = \mathbb{E}\left[\Delta\eta_{idtm}(\tau_m)|\mathbf{z}_{dtm}\right]$. To the extent that differences in internalization imply different structures of unilateral effects, this function allows us to distinguish among alternative models of conduct.

For each merger, we choose $\tau_m$ by minimizing the GMM objective function under the two moment restrictions described above. For each candidate $\tau_m$, this procedure requires three steps. First, we estimate the function that determines changes in marginal costs and recover $\hat{\Delta\omega_{idtm}}$. Second, we estimate $\Delta\eta_{idtm}(\tau_m)$ as a function of $\mathbf{z_{dtm}}$ and obtain the fitted values, $\hat{g}(\mathbf{z_{dtm}})$. Backus et al. (2021) highlight the need to flexibly model both of these functions. To maintain this flexibility, we regress cost or markup changes on B-spline bases in the specified covariates. Finally, we compute the vector of moments:

$$\hat{m}(\tau_m) = \begin{bmatrix} \frac{1}{|\mathcal{J}_M|}\sum_{idt\in\mathcal{J}_M} \hat{\Delta\omega_{idtm}}\hat{g}(\mathbf{z_{dtm}}) \\ \frac{1}{|\mathcal{J}_{NM}|}\sum_{idt\in\mathcal{J}_{NM}} \hat{\Delta\omega_{idtm}} \end{bmatrix}, \tag{7}$$

where $\mathcal{J}_M$ and $\mathcal{J}_{NM}$ are the sets of merging and non-merging products, respectively. We choose $\tau_m$ to minimize $\hat{m}(\tau_m)'\hat{W}\hat{m}(\tau_m)$. We implement this using two-step GMM, where $\hat{W}$ is the estimate of the optimal weighting matrix.

In addition to $x_{idtm}$ and $w_{idtm}$, there are a number of classes of instruments that can enter $\mathbf{z}_{dtm}$. These instruments must be excluded from marginal costs and explain differences in merger-induced changes in markups across different models of conduct (i.e., across different values of $\tau$). In our case, we rely on Gandhi-Houde instruments, which leverage the intuition that merging-party products with few close substitutes in characteristics space would experience larger changes in markups were the parties to internalize pricing spillovers following the merger. We specify $x_{idtm}$ to include the first two principal components of product characteristics, package size, and month-of-year dummies, and $w_{idtm}$ to include input costs.

Before moving to the results, we compare our approach with others in the literature. Backus et al. (2021) relies upon detecting correlation between markup differences across models and marginal cost shocks. We model our exercise after this approach with one distinction: instead of running our procedure purely using post-period data, we exploit the merger itself and difference across the pre- and post-merger periods. We view this as an advantage in our setting. By differencing, our procedure absorbs unobserved cost shocks that are persistent across time and instead relies on the orthogonality of cost changes and excluded demand shifters. Of course, it comes with a different assumption that changes to markups not explained by product-level factors are orthogonal to synergies, which is not directly comparable to assumption used by Backus et al. (2021).[19,20]

Figure 6 shows results of internalization coefficients. We find an average internalization coefficient around 0.5, with substantial bunching at the extreme values of 1 (Bertrand-Nash) or 0 (no internalization). First, as a means of validating this procedure, we also estimate $\tau_m$ in a set of "placebo" mergers, following the same procedure as in Section IV.B: Figure A.7 shows the distribution, and while it also exhibits bunching at both 0 and 1, the mass near 0 is considerably larger than in the true estimates. Second, to interpret these estimates, we regress $\tau_m$ on measures of market structure and market size (see Table A.4). We find a positive correlation of the internalization coefficient with market size, a negative correlation with the merging party share, and no significant

[19]We note that if one is concerned about synergies being correlated with factors that determine unilateral effects, then the exclusion restriction used in Backus et al. (2021) also would not hold—as the residual from the regression of costs in the post-period on cost shifters and product characteristics would incorporate the synergy and thus be correlated with the instruments they use.

[20]This approach relates to the one proposed in Adão et al. (2025). Adão et al. (2025) studies tests of quantitative trade models and proposes a test for model-based predictions of policy effects using the goodness-of-fit measure: $\mathbb{E}[z(\Delta y - \Delta x)]$, where $\Delta y$ is the observed change in outcome following the policy and $\Delta x$ is the model-based prediction of the policy effect. In our setting, we could implement an analogous test, imposing that price changes following the merger are orthogonal to our set of instruments. However, the required structural assumption is that the instrument must be orthogonal to both changes in markups and changes in synergies.

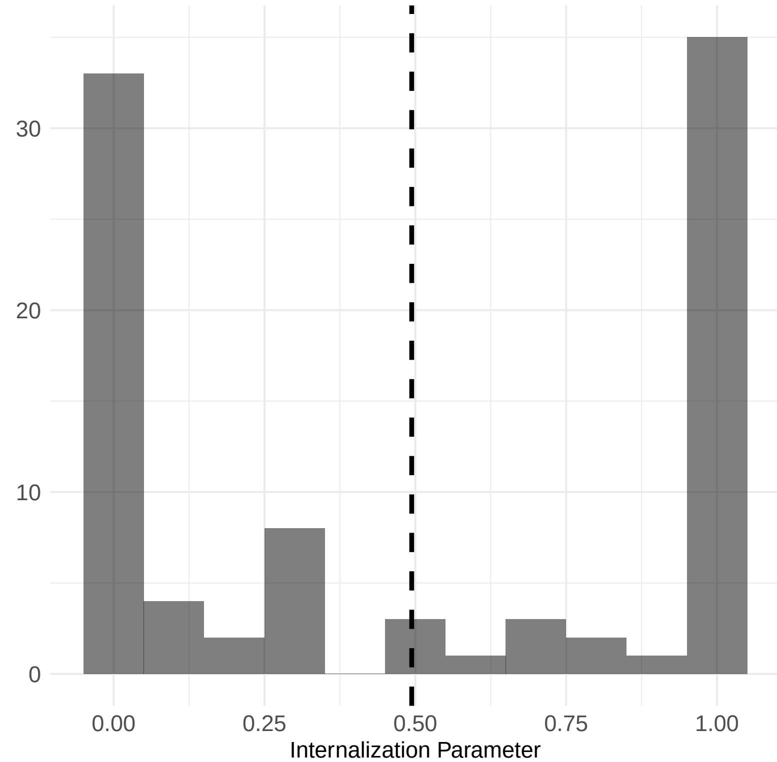



Figure 6: Estimates of internalization coefficients

correlation with DHHI.[21] While we hesitate to read too much into these results, the former could be a signal that merging parties are more likely to reoptimize prices when the stakes are higher.

These results also allow us to reassess our estimates of synergies. Table 6 shows summary statistics of the distribution of implied synergies assuming partial internalization in Panel B. First, we find that the mean synergy for merging parties drops from 2.1% of price to 1.6% of price. This is driven by a drop in the right tail of estimated synergies: the modified conduct model attributes these overpredictions to departures from Bertrand-Nash conduct instead of large reductions in marginal costs. Synergies for non-merging parties are even smaller on average. While lower than our baseline synergy estimates, the fact that residual synergies remain after allowing for imperfect internalization suggests that this particular deviation from Nash-Bertrand conduct is not sufficient to explain the consistent overprediction of merger effects.

[21]We have also run restricted versions of the regressions in Table A.4, limited the sample to mergers with sufficient high DHHI. The correlation with market size remains positive but becomes statistically insignificant, and we also find a stronger negative correlation with HHI.

| | Mean | St. Dev. | Quantiles | | | | |
|---|---|---|---|---|---|---|---|
| | | | 10% | 25% | 50% | 75% | 90% |
| A. Bertrand-Nash | | | | | | | |
| Merging | 0.021 | 0.077 | -0.056 | -0.016 | 0.024 | 0.054 | 0.103 |
| Overall | -0.002 | 0.123 | -0.050 | -0.030 | 0.001 | 0.029 | 0.068 |
| Non-Merging | -0.011 | 0.124 | -0.068 | -0.032 | -0.006 | 0.024 | 0.061 |
| B. Partial Internalization | | | | | | | |
| Merging | 0.016 | 0.060 | -0.048 | -0.019 | 0.017 | 0.047 | 0.081 |
| Overall | 0.000 | 0.113 | -0.050 | -0.028 | -0.000 | 0.030 | 0.056 |
| Non-Merging | -0.003 | 0.116 | -0.060 | -0.029 | -0.004 | 0.028 | 0.061 |
| C. Adding a Retail Markup ($\lambda = 0.2$) | | | | | | | |
| Merging | 0.029 | 0.147 | -0.068 | -0.018 | 0.032 | 0.064 | 0.131 |
| Overall | -0.004 | 0.221 | -0.147 | -0.042 | 0.005 | 0.066 | 0.206 |
| Non-Merging | -0.014 | 0.227 | -0.153 | -0.044 | 0.003 | 0.051 | 0.157 |
| D. At Optimal Scaling of Price Coefficients | | | | | | | |
| Merging | 0.013 | 0.059 | -0.049 | -0.020 | 0.020 | 0.048 | 0.079 |
| Overall | -0.016 | 0.090 | -0.057 | -0.033 | -0.006 | 0.019 | 0.040 |
| Non-Merging | -0.024 | 0.093 | -0.084 | -0.037 | -0.010 | 0.015 | 0.041 |
| E. At Optimal Scaling of Non-Price Coefficients | | | | | | | |
| Merging | 0.017 | 0.077 | -0.058 | -0.018 | 0.026 | 0.057 | 0.087 |
| Overall | -0.017 | 0.114 | -0.058 | -0.031 | -0.008 | 0.021 | 0.053 |
| Non-Merging | -0.028 | 0.116 | -0.085 | -0.040 | -0.012 | 0.012 | 0.049 |

Table 6: Summary statistics for synergies

### V.C. Retail Markups

Our procedure assumes that retail prices are set by the manufacturers of products. This assumption is common in work in consumer packaged goods, dating back to Nevo (2000), and underlies the above cited papers studying markups at scale as well. However, in a model with retailer price setting, the retail markup would be a function of customer demand and the wholesale prices set by upstream firms, and the retail passthrough would be a function of the upstream prices. If passthrough is less than one-for-one, then a large predicted change in wholesale prices would lead to a smaller change in retail prices. Of course, in this environment, the implied prices in the pre-period would also lead to different implied costs in the pre-merger period.

Estimating a model of retailer competition is beyond the scope of this paper. We instead follow an approach, proposed in Appendix E of Miller and Weinberg (2017), of calibrating a one-parameter model of competition in the retail sector. Given a set of upstream prices $\boldsymbol{p}^U_{dtm}$, a representative retailer will set a retail markup

$$\boldsymbol{\mu}^R_{dtm}(\lambda, \boldsymbol{p}^U_{dtm}) \equiv \lambda \cdot \left[ \left( \frac{\partial \boldsymbol{D}_{dtm}(\boldsymbol{p}^R_{dtm}(\boldsymbol{p}^U_{dtm}))}{\partial \boldsymbol{p}^R_{dtm}} \right)' \right]^{-1} \boldsymbol{D}_{dtm}(\boldsymbol{p}^R_{dtm}(\boldsymbol{p}^U_{dtm})), \tag{8}$$

where the retail price is defined by

$$\boldsymbol{p}^R_{dtm} \equiv \boldsymbol{p}^U_{dtm} + \boldsymbol{\mu}^R_{dtm}(\lambda, \boldsymbol{p}^U_{dtm}). \tag{9}$$

In (8), $\lambda$ parameterizes the competitiveness of the retail sector, with $\lambda = 0$ corresponding to a perfectly competitive retail sector, and $\lambda = 1$ a monopolistic one. When setting wholesale prices, the upstream manufacturers know that the the retail prices will follow (9), which assumes away marginal costs from the retailer.

Fixing a $\lambda$, we implement the merger simulation by recovering costs from the first-order condition corresponding to the upstream manufacturer's problem in this model. We use the same forecasting procedure for costs as in Section VI.A, and we solve this two-stage model in the post-merger period. This is a more intricate model to solve, but a modification of the algorithm from Morrow and Skerlos (2011) allows for efficient computation (Duarte, 2025).

We report results with $\lambda = 0.2$, which is within the range of robustness checks analyzed by Miller and Weinberg (2017). Panel E of Table 4 shows that this does not correct for the misprediction on average: the average prediction error becomes more negative for all parties, as does the median. It is important to note, however, that this is not a uniform shift to the left: the distribution of prediction errors does grow wider, and the upper quantiles increase. Panel C of Table 6 shows the distribution of synergies under this model. Here, the patterns are similar too: implied synergies are larger in magnitude. We conclude that the systematic overprediction of price changes would not be eliminated by adding a vertical structure.

A different angle in vertical relationships is bargaining between the manufacturers and the retailers. Manufacturer bargaining power may increase after a merger, and they could be able to negotiate a lower retail price to increase upstream profits. To the extent that bargaining power increases more in mergers with larger DHHI, this explanation would be consistent with patterns in prediction error. However, it is beyond the scope of this paper to evaluate this approach. Estimation would require a strategy to empirically distinguish synergies from changes in bargaining power, which we suspect is difficult without restrictions on how bargaining power can change due to the merger. Even a calibration would require a model of retailer bargaining, which would be difficult to solve at our scale.

### V.D. Incorrect Demand Estimates

A fourth explanation for the systematic overprediction is not related to the "supply-side residual": a main input into the system, demand, itself could be incorrect. Given our scale, it is infeasible for us to reestimate demand to evaluate robustness to each of the decisions we made. We instead take the approach of peturbing our demand system in a (moderately large) neighborhood of the baseline estimates and picking the one that minimizes prediction error.

We consider two types of perturbations. First, we scale all price-related coefficients, including the mean coefficient, random coefficient, and demographic interactions, by a common factor.[22] This changes both average price sensitivity as well as the intensity of competition among products with different price levels. Second, we scale all non-price-related coefficients, including random coefficients and demographic interactions on the constant and product characteristics. These affect the intensity of differentiation and competition in characteristics space. For each set of coefficients, we recover pre-period costs and follow the same procedure to simulate the merger for scaling factors in a range from 0.5 to 3, and then select the scaling factors minimizing absolute overall prediction error separately for each merger.

Panels F and G of Table 4 show the distribution of prediction errors at the merger level at the optimal values of the scaling coefficients. By construction, the distributions are significantly tighter than the baseline. However, we observe that the overprediction still persists, and the mean is not appreciably different from the baseline. Scaling the pricing coefficient has a larger effect, but this leads to significantly more elastic demand systems than estimated at baseline (Figure A.10). This also leads to merging party synergies that are up to 40% lower on average (see Panels D and E of Table 6), but these estimates do lead to substantially more negative synergies for non-merging parties—which we may think are a priori unlikely.

This robustness check is naturally "local" to mixed logit demand systems of the form we estimate. Some recent work has suggested that these demand systems may be too restrictive for experimental elasticities (Goldszmidt et al., 2025; Bray and Stamatopoulos, 2026), or that they may have restrictive curvature (Miravete et al., 2026). Given that BLP is the workhorse demand system in industrial organization, we leave evaluating alternate classes of demand systems to future work.

## VI. The Predictive Power of Merger Simulations

A practitioner of merger simulations would be concerned with not just accuracy but also precision. Moreover, even if merger simulations are biased on average, their predictions could still

[22]We scale these together because the mean price coefficient needs to be sufficiently negative relative to the random coefficient and demographic interaction to ensure that consumers almost surely have downward-sloping demand, a requirement for equilibrium existence. Scaling these coefficients together does not change the relative magnitudes.

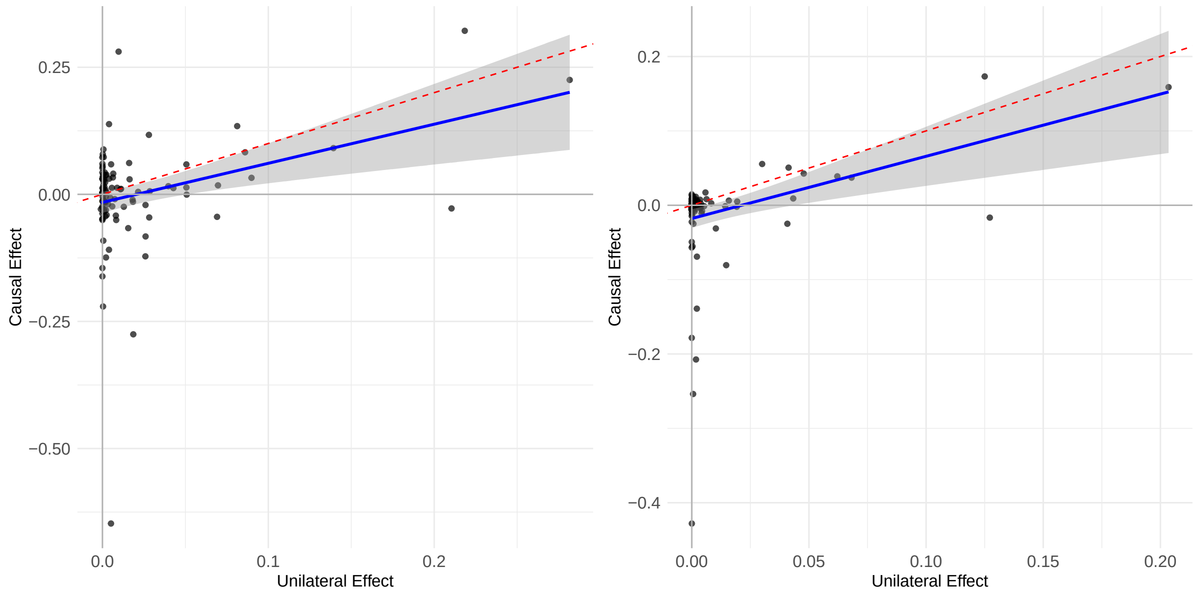
be informative of the true price change. In this section, we consider the question of predictive power. Section VI.A describes the prediction procedure. Sections VI.B and VI.C discuss different approaches to evaluating predictive power, and Section VI.D comments on some implications for antitrust enforcement.

### VI.A. Defining the Predictive Exercise

In this section, we assess whether the unilateral effect is predictive for a "causal" effect of the merger, which is arguably the object of interest for an antitrust agency. Unlike in the exercise in Section IV, this requires us to take a stance on a counterfactual price that would have occurred but-for the merger. The observed post-merger price is due to not just due to changes in the pricing behavior of the parties in the market but also due to potential changes in demand, product assortment, and costs—both due to observable input costs and unobservable trends. In defining the causal effect, we take the approach of keeping as many of these factors close to the values that would be implied by the observed prices as possible. We take the post-merger product assortment as given. We invert the demand system at the observed post-merger shares and prices to determine the post-merger demand shocks $\boldsymbol{\xi}$ in each market. Finally, we assume that the post-merger costs for non-merging parties are those that would be implied by the Bertrand-Nash equilibrium at the observed prices, and we invert (3) to recover them. We are, however, unwilling to make this assumption for merging parties, as synergies are an often-cited explanation for merging. We instead assume that the pre-period cost function remains fixed for merging parties, and any shocks to costs for merging parties follow the same trend as the non-merging cost shocks do.[23] The counterfactual price $\boldsymbol{p}^{\text{cf}}$ is then the solution to (3) at the post-merger $\boldsymbol{\xi}$, counterfactual cost vector $\boldsymbol{c}^{\text{cf}}$, and pre-merger ownership matrix $\boldsymbol{\Omega}$. Suppressing the dependence on $m$, the causal effect Causal$_{idt}$ is $\log(p_{idt}) - \log(p^{\text{cf}}_{idt})$. We define Unilateral$_{idt}$ as the log difference in prices with the post-merger and pre-merger ownership matrix at these demand and cost schedules. We aggregate across time periods and DMAs in the same manner we aggregate Error$_{idm}$.

We briefly comment on alternate approaches. The first is to estimate the causal effect from a strategy such as a simple difference (Peters, 2006; Björnerstedt and Verboven, 2016) or a difference-in-differences approach (Weinberg, 2011; Weinberg and Hosken, 2013). This approach requires a setting where auxiliary changes to the market were likely minimal relative to the merger itself or where credible controls are available, which is difficult to do at scale. Moreover, these reduced-form approaches are designed to control for trends in the market, which our approach does as well in

[23] Formally, we assume that $h_m(x_{im}, w_{idtm})$ stays the same after the merger, and that for a post-merger time period $t'$ and a product owned by the merged firm $i$, $\omega_{idt'} = \underline{\bar{\omega}_{id,\text{pre}}} + \overline{\Delta\omega}_{nm,dt'}$, where $\omega_{i,\text{pre}}$ is the average $\omega$ for product-DMA $id$ in the last three months before the merger and $\overline{\Delta\omega}_{nm,dt'}$ is the average within-DMA difference in $\omega$ for non-merging parties from the last three months before the merger to period $t'$.




(a) Merging (b) Overall

Figure 7: Scatter of the causal effect versus the unilateral effect, at the merger level, for (a) merging parties, and (b) all parties. The dotted red line indicates the $45°$ line, and the blue line is the line of best fit.

a manner directly consistent with the model. A second approach would be to avoid defining the counterfactual effect entirely and instead compare the total change in price after the merger—from a pre-merger period—to the predicted change in price, using the prediction exercise in Section VI.A. We adopt this approach in Appendix B and our main takeaways are similar; this exercise also illustrates predictive power only using pre-merger information. The caveats are that the overall change from before to after the merger is likely not the object of interest for the enforcement agencies, and in interpreting the results we need to be careful about how much predictive power (or lack thereof) is coming from auxiliary changes to the environment.

## VI.B. Comparing Causal and Unilateral Effects of Mergers

A first question is whether unilateral effects are "calibrated" to the true causal effect of the merger: is an increase in the unilateral effect associated with a increase in the casual effect of the merger in expectation? While a one-for-one relation is strong, a positive association in expectation is in principle a fairly undemanding requirements of merger simulations. However, because it requires a sizable sample of mergers to test, it is not something that has been established before in the literature. Moreover, there are many reasons this could fail even if demand is well-estimated. For instance, mergers that would have large price effects might only be approved (or even proposed) if parties expect large cognizable synergies.[24] A more extreme reason is that the theory that underlies merger simulations—that of unilateral price effects tied to substitution patterns—could be a poor representation of firms' pricing models in reality. Moreover, checking for directional accuracy at different levels is informative: even if parties change prices in a way that is on average consistent with predictions from merger simulations, effects in different geographies and products may be based on rules-of-thumb or uniform pricing rules (Escudero, 2018; DellaVigna and Gentzkow, 2019), mitigating the relation between predictions and realizations.

Figure B.1 scatters $\text{Causal}_m$ against $\text{Unilateral}_m$ both for merging parties and overall. Due the presence of divestitures, unilateral effects need not have been positive, although we find that they are in all mergers. Causal effects often are negative, an observation consistent with merger retrospectives in this industry (Bhattacharya et al., 2025). Both panels show that the predictions are correlated with actual price changes: the slope is 0.77 (s.e. 0.25) for merging price changes and 0.84 (s.e. 0.21) overall. The confidence intervals include the perfect calibration level of 1, although they are wide.

A complementary exercise asks whether merger simulations are calibrated within a given merger: do DMAs and products predicted to have large price increases have large price increases on average? This within-merger test is especially useful because it differences out merger-specific shocks that shift prices broadly (for example, integration-wide cost shocks, aggregate demand shifts, or merger-level implementation disruptions), and it allays concerns about selection into merger proposal. Such results are also relevant for policy, as antitrust enforcement occurs not just at the merger level: agencies often require product divestitures, and they sometimes (but rarely) enforce regional divestitures as well. Relative to the cross-merger analysis, two additional, conflicting forces affect our interpretation of the within-merger results. First, given geography-specific remedies are uncommon, it is unlikely that synergies at the DMA level are negatively correlated with effects from the merger due to selection into merger approval. This force would act to increase the correlation between predicted and actual price changes. On the other hand, there is evidence that price changes are not sensitive to local market structure due to uniform pricing rules at the retailer level (DellaVigna and Gentzkow, 2019), although national firms still seem sensitive to local cost shocks (Butters et al., 2022).

We regress causal effects on unilateral effects at the DMA, product, and product–DMA levels and include merger fixed effects, so identification comes from deviations from each merger's own mean predicted and realized price change. The results are reported in Columns (2)–(4) of Table 7. Our takeaway is that these slopes are generally positive, suggesting that despite the additional forces at play within merger, calibration still remains a feature of these predictions even within-merger. It

[24]The opposite correlation is also possible: firms may only be willing to merge in competitive markets if they expect to realize synergies.

| | | Within-Merger | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| A. Merging | | | | |
| Constant | -0.02 | | | |
| | (0.01) | | | |
| Unilateral Effect | 0.77*** | 0.66*** | 0.07 | 0.28*** |
| | (0.25) | (0.08) | (0.13) | (0.02) |
| B. Overall | | | | |
| Constant | -0.02*** | | | |
| | (0.01) | | | |
| Unilateral Effect | 0.84*** | 0.71*** | 0.33*** | 0.45*** |
| | (0.21) | (0.08) | (0.04) | (0.01) |
| C. Non-Merging | | | | |
| Constant | -0.01** | | | |
| | (0.01) | | | |
| Unilateral Effect | 1.61*** | 0.08 | 0.91*** | 1.24*** |
| | (0.16) | (0.16) | (0.07) | (0.02) |
| Level | Merger | DMA | Product | P-DMA |
| Merger FE | No | Yes | Yes | Yes |

Table 7: Regressions of causal effects on unilateral effects. Each column runs the regression at a different level of observation (with "P-DMA" standing for product-DMA). Panels A and C focus on merging and non-merging price changes only, respectively. Panel B reports results for all price changes.

is worth noting, though, that some levels (e.g. the product level for merging parties and the DMA level for non-merging parties) do have small and statistically insignificant slopes, although we have not been able to identify an economic reason for why.

Despite the positive slope, the scatters in Figure B.1 suggest that the predictive power of simulations for the true price change is low, as the points are not tightly organized around the best-fit line. Table 8 verifies this observation. At the merger-level, we find value of $R^2$ around 8%–14%, depending on whether we look at merging parties, non-merging parties, or all parties. This is not due to extreme outliers driving a low Pearson correlation: Spearman's $\rho$ also indicates a fairly weak correlation. The within-merger analysis in Columns (2)–(4) illustrates that merger fixed effects do have substantial explanatory power, consistent with unmodeled merger-level changes being a source of this low correlation (e.g., nationwide synergies), but the within-merger analysis either yields similar results (in the case of Spearman's $\rho$) or significantly weaker correlations (in the case of $R^2$).

For context, we also compute the $R^2$ of a regression of the causal effect on HHI, DHHI, and its interaction. We find similar levels of predictive power for merging parties (12%) but slightly larger ones for non-merging parties (15%). Thus, the presumptions do have predictive power over

| | (1) | Within-Merger (2) | (3) | (4) |
|---|---|---|---|---|
| A. Merging | | | | |
| $R^2$ | 0.115 | 0.451 | 0.167 | 0.149 |
| Spearman's $\rho$ | 0.126 | 0.194 | 0.121 | 0.104 |
| Within-$R^2$ | – | 0.013 | 0.000 | 0.002 |
| B. Overall | | | | |
| $R^2$ | 0.137 | 0.323 | 0.183 | 0.187 |
| Spearman's $\rho$ | 0.200 | 0.242 | 0.190 | 0.186 |
| Within-$R^2$ | – | 0.014 | 0.003 | 0.005 |
| C. Non-Merging | | | | |
| $R^2$ | 0.082 | 0.306 | 0.376 | 0.329 |
| Spearman's $\rho$ | 0.305 | 0.233 | 0.238 | 0.240 |
| Within-$R^2$ | – | 0.000 | 0.011 | 0.010 |
| Level | Merger | DMA | Product | P-DMA |
| Merger FE | No | Yes | Yes | Yes |

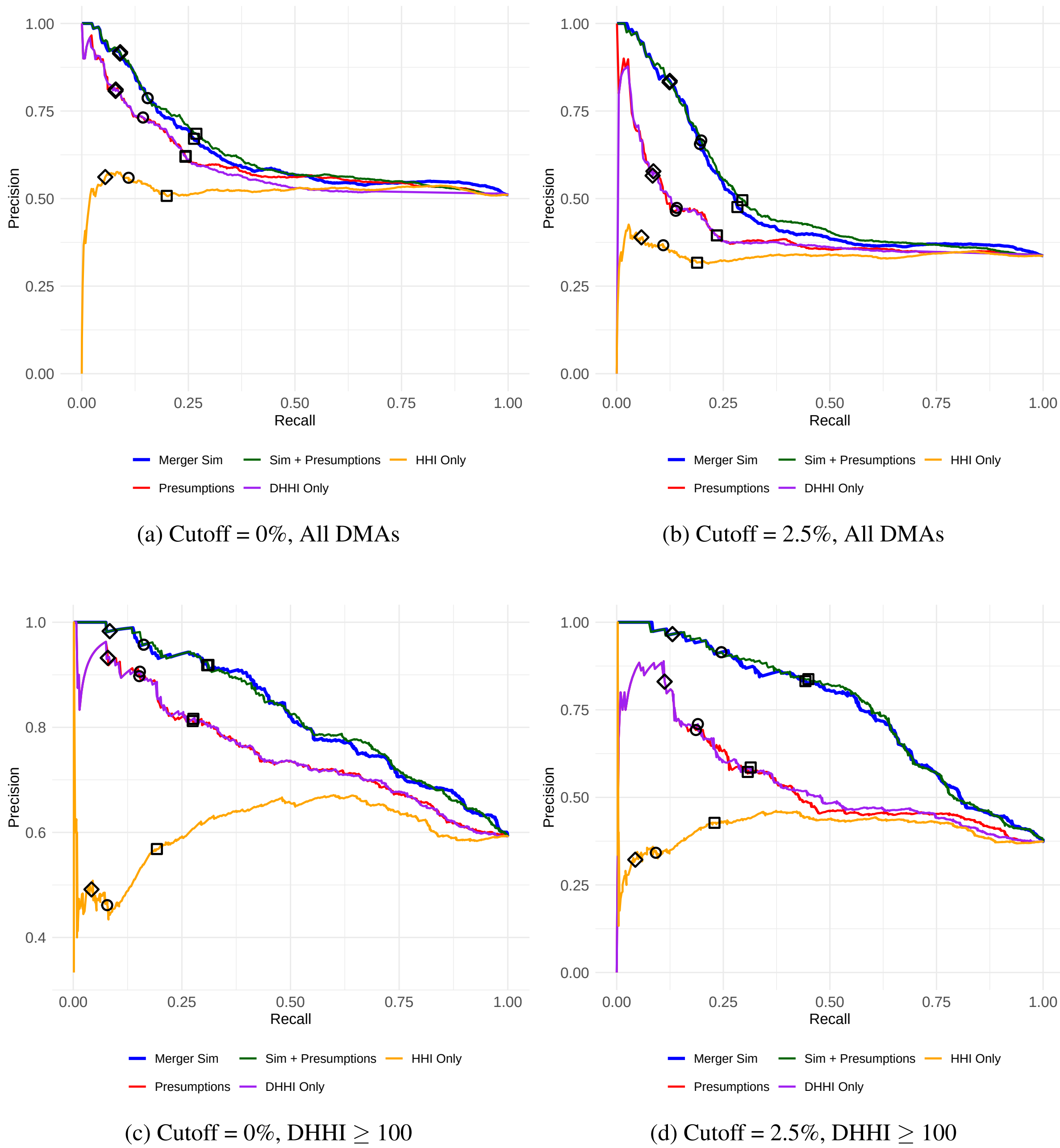
Table 8: Measures of fit for the causal effect on the unilateral effect. In Columns (2)–(4), Within-$R^2$ and Spearman's $\rho$ are computed after partialling out merger fixed effects.

unilateral effects, although the absolute amount is still low.

### VI.C. Merger Simulations as a Screening Tool

The low predictive power documented in Table 8 does not necessarily rule out merger simulations being a useful tool for enforcement. Agencies are concerned with their ability to identify especially harmful mergers, which could be distinct from the ability of a tool to predict the price change of all mergers. We assess this aspect of merger simulations in a statistical decision theory framework. We define a large price change to be situation where the causal effect is larger than a cutoff. We then use the unilateral effect as a predictor for this event: for any $x$, we consider the predicted event to be that the unilateral effect is larger than $x$. For this predicted event, we compute the so-called confusion matrix of true and false positives and negatives, and functions thereof. We then vary $x$.

We focus on precision-recall (PR) curves here: takeaways are similar with other receiver-operator characteristic curves (Appendix A). Precision is the probability of the true positive divided by the probability of a claimed positive: it answers what share of mergers with unilateral effects larger than $x$ actually have large causal effects. Recall answers the complementary question of what share of mergers with large causal effects have unilateral effects larger than $x$. Of course, "unilateral effect" can be replaced by any predictor, and we consider a variety. Points that are northeast dominate those in the southwest. In the extreme case, the oracle predictor has a PR curve




(a) Cutoff = 0%, All DMAs

(b) Cutoff = 2.5%, All DMAs

(c) Cutoff = 0%, DHHI $\geq$ 100

(d) Cutoff = 2.5%, DHHI $\geq$ 100

Figure 8: Precision-recall curves for merging-party price changes, using a variety of predictors. The diamond, circle, and square mark the points that correspond to marking 5%, 10%, and 20% of the mergers as high price changes.

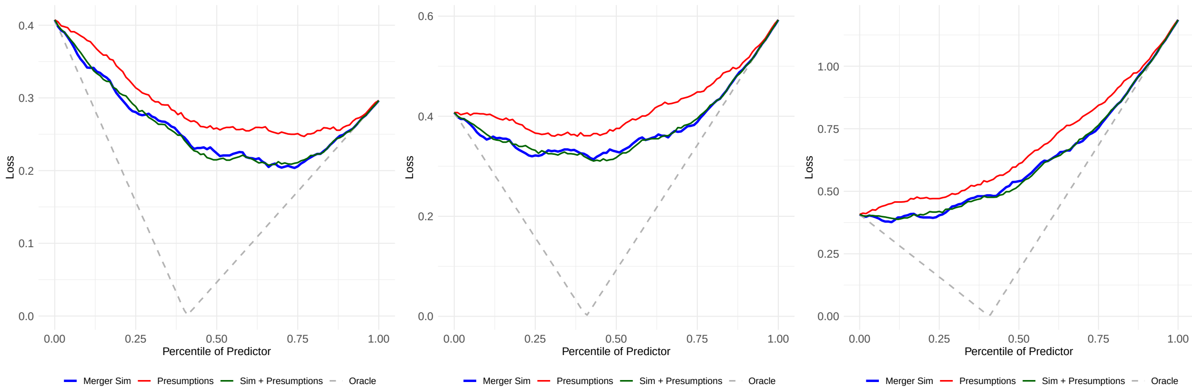
that starts at $(0, 1)$, is horizontal initially, and drops vertically to $(1, 0)$, while a completely random predictor has a PR curve that is flat at the share of observations that are true positives.

Figure 8 plots the associated precision-recall (PR) curves for merging party price changes, where the level of the observation is the DMA-merger. We report merger-level PR and ROC curves, as well as those for overall price changes, in Appendix A. The PR curve for unilateral effects is in blue, and we mark points that correspond to marking 5%, 10%, or 20% of merger as high price changes using a diamond, circle, and square, respectively. We first find that the curves are far from the benchmark of a random predictor, suggesting that there is predictive power. The classification is better for high-DHHI DMAs (those with DHHI $\geq 100$) than for all DMAs: this is sensible, since for markets with especially small DHHI, the merger simulation would predict no price effect. Finally, we do not see a consistent difference in discrimination ability across the two cutoffs.

To benchmark these PR curves, we also consider the exercise of predicting the high price changes with structural presumptions. For each cutoff, we fit a logit of whether the DMA-merger has a high causal effect on HHI, DHHI, and their interaction, and we use the fitted value as the predictor: the associated PR curve is in red.[25] We find that the PR curve for the simulations dominates that when using just the presumptions—especially for the higher cutoff. For context, we also show PR curves using just DHHI (purple) and just HHI (yellow) as the predictor. These show that the predictive ability of the presumptions comes from DHHI: the PR curve for DHHI almost overlaps with that for the joint predictor, while the PR curve for HHI is close to the one from random noise. Finally, we plot the PR curve (green) for a predictor that uses the unilateral effect, HHI, DHHI, and their interaction: this PR curve almost coincides with that of the simulations themselves, suggesting that while simulations have added value over the presumptions for discriminating large causal effects, the opposite is not true.

Another benchmark is how merger simulations compare to other methods used by practitioners. By modeling enforcement decisions, Bhattacharya et al. (2025) provide an estimate of the accuracy (share of classifications that coincide with the truth) of the agencies' diligence process in determining whether mergers would raise or lower average price. They report an accuracy of 82%. The maximum accuracy merger simulations obtain for predicting the events that mergers raise prices by 0% or 2.5% is 56% and 70%, respectively. This comparison must be viewed with caution, partly since the estimate in Bhattacharya et al. (2025) relies on a specific model of agency enforcement, but it underscores that neither the presumptions nor the simulations by themselves are a substitute for the work done by the agencies. With this caveat, we move to analyzing the implications of these results for enforcement in the next subsection.

[25] An alternative approach would be to regress the causal price change on a function of the presumptions and use that predicted value as the predictor. This approach would weaken the PR curves for the presumptions. Moreover, we have experimented with more flexible functions of HHI and DHHI but show this simpler one on the graph for two reasons. First, out-of-sample performance of a highly flexible function of HHI and DHHI is poor. Second, we anticipate that the lack of interpretability that may come from an especially opaque function of presumptions would limit its use by an enforcement agency.




(a) $\kappa = 1/2$ (b) $\kappa = 1$ (c) $\kappa = 2$

Figure 9: Loss functions at the DMA-merger level, restricting to markets with DHHI $\geq 100$ at a cutoff of 0%

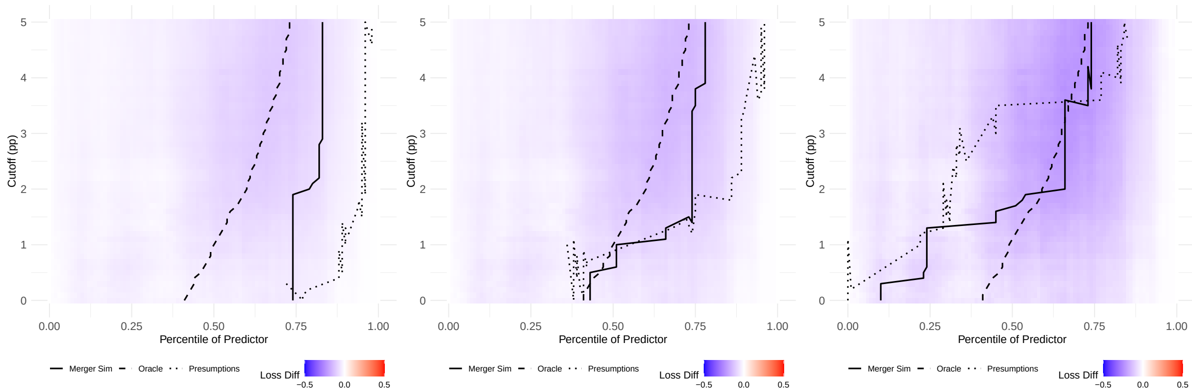
### VI.D. Implications for Enforcement

Another lens through which to interpret the results in Section VI.C is that of an agency deciding on the level of enforcement. Agencies of course weigh a number of factors when deciding whether to challenge a case (or request a remedy): the strength of auxiliary evidence, the probability of winning the case, the possibility of influencing precedent, and non-price effects like innovation. However, to the extent that an agency focuses on price effects, an often-discussed concern in choosing the level of enforcement—even conditional on defining the "cutoff" price change the agency would want to target—is that between "Type I" and "Type II" errors (Hovenkamp, 2021). A Type I error is a false positive (challenging a pro-competitive merger), and a Type II error is a false negative (failing to challenge an anti-competitive merger).

We consider an agency that is minimizing a loss function

$$L(q) \equiv \Pr(\text{Type I Error}|\text{Percentile of Predictor} > q) \cdot \Pr(\text{Causal} \leq 0) \\ + \kappa \cdot \Pr(\text{Type II Error}|\text{Percentile of Predictor} > q) \cdot \Pr(\text{Causal} > 0), \quad (10)$$

which balances Type I and Type II error rates (scaled by the baseline probabilities), with $\kappa$ governing the trade-off. Figure 9 shows this loss function for the unilateral effects from the merger simulation (blue), the presumptions (red), and the combination (green), for three different values of $\kappa$, at a cutoff of 0%, and restricting to mergers with DHHI larger than 100. We also overlay a dashed gray line to illustrate the loss from the oracle. Two observations about these lines mirror that from the PR curves in Section VI.C. First, the loss functions in all three cases are significantly larger than the oracle loss, just as the PR curves were far from the oracle PR curve. Second, we find that the loss function from the unilateral effects dominate that from presumptions, and adding the presumptions




(a) $\kappa = 1/2$ (b) $\kappa = 1$ (c) $\kappa = 2$

Figure 10: Difference in loss functions from simulations and presumptions, plotted against the percentile of the predictor and the cutoff. The solid line is the optimal challenge rate using simulations, the dotted using presumptions, and the dashed the oracle-optimum.

to the unilateral effects does not improve the loss appreciably.

However, viewing the classification exercise through the lens of a loss function also allows us to analyze the optimal behavior of an agency. An agency plans to challenge a fixed number of mergers would only be concerned with the loss functions at one value of $q$. If this were the case, then regardless of $q$, simulations would improve the decisions of the agency. On the other hand, an unconstrained agency would choose $q$ to minimize this loss, and thus the signal in the prediction could affect the challenge rate itself. The three panels in Figure 9 do not show significant differences in optimal challenge rates for this set of parameters. The optimal challenge rates increase as $\kappa$ increases and the agencies become more concerned with avoiding Type II errors, while the oracle-optimal challenge rate naturally stays constant. But the optimal challenge rates do not differ much across predictors at a cutoff of 0%.

Figure 10 plots the difference in the loss functions from simulation and from the presumptions, for cutoffs ranging from 0% to 5%. The difference is always nonpositive, showing that simulations dominate presumptions for a large set of parameter values. The figure also plots the optimal challenge rate using simulations or presumptions, and compares them to the oracle. Here, we do see that for many cutoffs, there are differences in challenge rates depending on the predictor used. However, the simulations typically lead to an optimal challenge rate that is closer to the oracle-optimal.

To interpret the finding that merger simulations lead to optimal challenge rates closer to the oracle, it is worth comparing optimal challenge rates under two extreme predictors. An agency with access to the oracle predictor will optimally challenge a fraction of mergers equal to the probability of a bad merger, while an agency with a purely random predictor will challenge either all mergers or no mergers, depending on how the underlying ratio of good to bad mergers compares to $\kappa$. Structural presumptions behave more similarly to the latter case; they do not provide a strong enough signal to shift the agency far away from its priors, leading to an optimal challenge rate leaning more towards extremes. Meanwhile, the additional predictive power of merger simulations creates a more informative signal, pushing the optimal challenge rate more towards intermediate values. The use of merger sim therefore makes challenge rates more uniform across agencies with different weights on Type I versus Type II errors.

## VII. Conclusion

This paper conducts the largest analysis of merger simulations thus far in the literature. Doing so allows us to find systematic patterns in how simulations depart from realizations of price changes. Merger simulations typically overpredict price changes, by about 4 pp for merging parties. This overprediction could be rationalized with an average synergy of about 2% of initial price. We explore the possibility of imperfect internalization of the newly merged entity's profits, retailer markups, and demand misspecification explaining these results. While we find some evidence of imperfect internalization, we still find a role for (small) synergies on average.

Our finding that merger simulation frequently overpredicts price increases cautions against naively taking the output of merger simulation as unbiased predictions. However, merger simulations could still be predictive even if they are not exactly calibrated to true price changes. While we find limited evidence in favor of their ability to predict price changes of a merger, we find that they are able to screen mergers with high price changes more effectively than measures of market structure by themselves. This has implications for antitrust enforcement, as an unconstrained agency may be willing to challenge more mergers with the improved predictive power.

## References


ADÃO, R., A. COSTINOT, AND D. DONALDSON (2025): "Putting Quantitative Models to the Test: An Application to the US-China Trade War," *Quarterly Journal of Economics*, 140, 1471–1524.

ANGRIST, J. D. AND J.-S. PISCHKE (2010): "The Credibility Revolution in Empirical Economics: How Better Research Design is Taking the Con out of Econometrics," *Journal of Economic Perspectives*, 24, 3–30.

ASHENFELTER, O. C., D. S. HOSKEN, AND M. C. WEINBERG (2015): "Efficiencies Brewed: Pricing and Consolidation in the US Beer Industry," *RAND Journal of Economics*, 46, 328–361.

ASKER, J. (2016): "Diagnosing Foreclosure Due to Exclusive Dealing," *Journal of Industrial Economics*, 64, 375–410.

ASKER, J. AND V. NOCKE (2021): "Collusion, Mergers, and Related Antitrust Issues," in *Handbook of Industrial Organization*, Elsevier, vol. 5, 177–279.

ATALAY, E., E. FROST, A. SORENSEN, C. SULLIVAN, AND W. ZHU (2025): "Scalable Demand and Markups," *Journal of Political Economy*, Forthcoming.

Atalay, E., A. Sorensen, C. Sullivan, and W. Zhu (2024): "Product Repositioning by Merging Firms," *Journal of Industrial Economics*, 72, 868–908.

Athey, S., J. Levin, and E. Seira (2011): "Comparing Open and Sealed Bid Auctions: Evidence from Timber Auctions," *Quarterly Journal of Economics*, 126, 207–257.

Backus, M., C. Conlon, and M. Sinkinson (2021): "Common Ownership and Competition in the Ready-to-Eat Cereal Industry," Tech. rep., National Bureau of Economic Research.

Bajari, P. and A. Hortacsu (2005): "Are Structural Estimates of Auction Models Reasonable? Evidence from Experimental Data," *Journal of Political Economy*, 113, 703–741.

Berry, S., J. Levinsohn, and A. Pakes (1995): "Automobile Prices in Market Equilibrium," *Econometrica*, 63, 841–890.

——— (2004): "Differentiated Products Demand Systems from a Combination of Micro and Macro Data: The New Car Market," *Journal of Political Economy*, 112, 68–105.

Bhattacharya, V., G. Illanes, and D. Stillerman (2025): "Merger Effects and Antitrust Enforcement: Evidence from US Consumer Packaged Goods," Tech. rep., Working Paper.

Björnerstedt, J. and F. Verboven (2016): "Does Merger Simulation Work? Evidence from the Swedish Analgesics Market," *American Economic Journal: Applied Economics*, 8, 125–64.

Bonnet, C. and P. Dubois (2010): "Inference on Vertical Contracts Between Manufacturers and Retailers Allowing for Nonlinear Pricing and Resale Price Maintenance," *RAND Journal of Economics*, 41, 139–164.

Braguinsky, S., A. Ohyama, T. Okazaki, and C. Syverson (2015): "Acquisitions, Productivity, and Profitability: Evidence from the Japanese Cotton Spinning Industry," *American Economic Review*, 105, 2086–2119.

Bray, R. E. and I. Stamatopoulos (2026): "Observational Price Variation in Scanner Data Cannot Reproduce Experimental Price Elasticities," Tech. rep., Northwestern University.

Butters, R. A., D. W. Sacks, and B. Seo (2022): "How Do National Firms Respond to Local Cost Shocks?" *American Economic Review*, 112, 1737–1772.

Chamberlain, G. (1987): "Asymptotic Efficiency in Estimation with Conditional Moment Restrictions," *Journal of Econometrics*, 34, 305–334.

Conlon, C. and J. Gortmaker (2020): "Best Practices for Differentiated Products Demand Estimation with pyblp," *RAND Journal of Economics*, 51, 1108–1161.

——— (2025): "Incorporating Micro Data into Differentiated Products Demand Estimation with PyBLP," *Journal of Econometrics*, 105926.

Crawford, G. S., R. S. Lee, M. D. Whinston, and A. Yurukoglu (2018): "The Welfare Effects of Vertical Integration in Multichannel Television Markets," *Econometrica*, 86, 891–954.

DellaVigna, S. and M. Gentzkow (2019): "Uniform Pricing in US Retail Chains," *Quarterly Journal of Economics*, 134, 2011–2084.

Demirer, M. and O. Karaduman (2025): "Do Mergers and Acquisitions Improve Efficiency: Evidence from Power Plants," *Journal of Political Economy*, Forthcoming.

Dingel, J. I. and F. Tintelnot (2025): "Spatial Economics for Granular Settings," *Econometrica*, Forthcoming.

Döpper, H., A. MacKay, N. H. Miller, and J. Stiebale (2025): "Rising Markups and the Role of Consumer Preferences," *Journal of Political Economy*, 133, 2462–2505.

Dranove, D. and C. Ody (2016): "Evolving Measures of Provider Market Power," *American Journal of Health Economics*, 2, 145–160.

Duarte, M. (2025): "Extending Fixed-Point Methods for Equilibrium Computation in Markets with Differentiated Products," *Economics Letters*, 250, 112275.

Dubois, P., R. Griffith, and M. O'Connell (2018): "The Effects of Banning Advertising in Junk Food Markets," *Review of Economic Studies*, 85, 396–436.

Escudero, M. (2018): "Rule-of-Thumb Pricing: Retail Cannabis in Washington State," Tech. rep., Northwestern University.

Euromonitor International (2022): "Euromonitor Passport," Various data series accessed by hand from interactive website. Last accessed August 2022.

Federal Reserve Bank of St. Louis (2025): "Federal Reserve Economic Data," Various series described in README. Last accessed August 2024.

Focarelli, D. and F. Panetta (2003): "Are Mergers Beneficial to Consumers? Evidence from the Market for Bank Deposits," *American Economic Review*, 93, 1152–1172.

Fournier, G. M. and Y. Gai (2007): "What does Willingness-to-Pay Reveal About Hospital Market Power in Merger Cases?" in *IHEA 2007 6th World Congress*.

Gandhi, A. and J.-F. Houde (2019): "Measuring Substitution Patterns in Differentiated Products Industries," *Working Paper*.

Garmon, C. (2017): "The Accuracy of Hospital Merger Screening Methods," *RAND Journal of Economics*, 48, 1068–1102.

Goldszmidt, A., L. de Frahan, J. List, M. Mogstad, I. Muir, and A. Torgovitsky (2025): "On the Identification and Interpretation of Price Elasticities in a Class of Discrete-Choice Demand Models," Tech. rep., University of Chicago.

Grieco, P., J. Pinkse, and M. Slade (2018): "Brewed in North America: Mergers, Marginal Costs, and Efficiency," *International Journal of Industrial Organization*, 59, 24–65.

Hausman, J., G. Leonard, and J. D. Zona (1994): "Competitive Analysis with Differenciated Products," *Annales d'Economie et de Statistique*, 159–180.

Hendel, I. and A. Nevo (2006): "Measuring the Implications of Sales and Consumer Inventory Behavior," *Econometrica*, 74, 1637–1673.

Houde, J.-F. (2012): "Spatial Differentiation and Vertical Mergers in Retail Markets for Gasoline," *American Economic Review*, 102, 2147–2182.

Hovenkamp, H. (2021): "Antitrust error costs," *U. Pa. J. Bus. L.*, 24, 293.

Jeziorski, P. (2014): "Estimation of Cost Efficiencies from Mergers: Application to US Radio," *RAND Journal of Economics*, 45, 816–846.

Kaboski, J. P. and R. M. Townsend (2011): "A Structural Evaluation of a Large-Scale Quasi-Experimental Microfinance Initiative," *Econometrica*, 79, 1357–1406.

Kehoe, T. J., P. S. Pujolas, and J. Rossbach (2017): "Quantitative Trade Models: Developments and Challenges," *Annual Review of Economics*, 9, 295–325.

Kim, E. H. and V. Singal (1993): "Mergers and Market Power: Evidence from the Airline Industry," *American Economic Review*, 549–569.

Magnolfi, L., J. McClure, and A. Sorensen (2025): "Triplet Embeddings for Demand Estimation," *American Economic Journal: Microeconomics*, 17, 282–307.

May, S. and M. Noether (2014): "Unresolved Questions Relating to Market Definition in Hospital Mergers," *Antitrust Bulletin*, 59, 479–503.

Miller, N. H. and M. C. Weinberg (2017): "Understanding the Price Effects of the MillerCoors Joint Venture," *Econometrica*, 85, 1763–1791.

Mincer, J. A. and V. Zarnowitz (1969): "The Evaluation of Economic Forecasts," in *Economic Forecasts and Expectations: Analysis of Forecasting Behavior and Performance*, NBER, 3–46.

Miravete, E., K. Seim, and J. Thurk (2026): "Elasticity and Curvature of Discrete Choice Models," Tech. rep., University of Texas.

Morrow, W. R. and S. J. Skerlos (2011): "Fixed-Point Approaches to Computing Bertrand-Nash Equilibrium Prices Under Mixed-Logit Demand," *Operations Research*, 59, 328–345.

MOTTA, M. AND E. TARANTINO (2021): "The Effect of Horizontal Mergers, When Firms Compete in Prices and Investments," *International Journal of Industrial Organization*, 78, 102774.

NAKAMURA, E. AND D. ZEROM (2010): "Accounting for Incomplete Pass-Through," *Review of Economic Studies*, 77, 1192–1230.

NEVO, A. (2000): "Mergers with Differentiated Products: The Case of the Ready-to-Eat Cereal Industry," *RAND Journal of Economics*, 395–421.

——— (2001): "Measuring Market Power in the Ready-to-Eat Cereal Industry," *Econometrica*, 69, 307–342.

NEVO, A. AND M. D. WHINSTON (2010): "Taking the Dogma out of Econometrics: Structural Modeling and Credible Inference," *Journal of Economic Perspectives*, 24, 69–82.

NIELSENIQ (2025): "Consumer Panel and Retail Scanner Data," Multiple electronic files obtained from the Kilts Center at the University of Chicago.

PANHANS, M. T. AND C. TARAGIN (2023): "Consequences of Model Choice in Predicting Horizontal Merger Effects," *International Journal of Industrial Organization*, 89, 102986.

PATHAK, P. A. AND P. SHI (2021): "How Well Do Structural Demand Models Work? Counterfactual Predictions in School Choice," *Journal of Econometrics*, 222, 161–195.

PETERS, C. (2006): "Evaluating the Performance of Merger Simulation: Evidence from the US Airline Industry," *Journal of Law and Economics*, 49, 627–649.

RAVAL, D., T. ROSENBAUM, AND N. E. WILSON (2022): "Using Disaster-Induced Closures to Evaluate Discrete Choice Models of Hospital Demand," *RAND Journal of Economics*, 53, 561–589.

REYNAERT, M. AND F. VERBOVEN (2014): "Improving the Performance of Random Coefficients Demand Models: The Role of Optimal Instruments," *Journal of Econometrics*, 179, 83–98.

SALANIÉ, B. AND F. A. WOLAK (2022): "Fast, Detail-Free, and Approximately Correct: Estimating Mixed Demand Systems," Tech. rep., Working paper.

SCHMITT, M. (2017): "Do Hospital Mergers Reduce Costs?" *Journal of Health Economics*, 52, 74–94.

SHEEN, A. (2014): "The Real Product Market Impact of Mergers," *Journal of Finance*, 69, 2651–2688.

THOMSON REUTERS (2021): "SDC Platinum," Data extracted from electronic database. Last accessed January 2021.

WEINBERG, M. C. (2011): "More Evidence on the Performance of Merger Simulations," *American Economic Review Papers and Proceedings*, 101, 51–55.

WEINBERG, M. C. AND D. HOSKEN (2013): "Evidence on the Accuracy of Merger Simulations," *Review of Economics and Statistics*, 95, 1584–1600.

WERDEN, G. J. AND L. M. FROEB (1994): "The Effects of Mergers in Differentiated Products Industries: Logit Demand and Merger Policy," *Journal of Law, Economics, and Organization*, 10, 407–426.

WERDEN, G. J. AND L. M. FROEB (2006): "Unilateral Competitive Effects of Horizontal Mergers," in *Handbook of Antitrust Economics*, MIT Press.

WHINSTON, M. D. (2007): "Antitrust Policy Toward Horizontal Mergers," in *Handbook of Industrial Organization*, ed. by M. Armstrong and R. Porter, Elsevier, vol. 3, 2369–2440.

## ONLINE APPENDIX

### A. Additional Tables and Figures

This appendix contains additional figures and tables mentioned in the text.

1. Figure A.1 show the distribution of elasticities for low- and high-income customers. Figure A.2 shows diversion ratios between different parties. Figure A.3 shows diversion rations between the target and acquirer, for the estimated model (red) and a logit model (blue).

2. Table A.1 compares estimated elasticities to those from other papers. Keep in mind that the definition of a product and a market are different for different papers (as are the time periods and geographies under study), so these comparisons should be made with caution.

3. Tables A.2 and A.3 show regressions of prediction error on measures of market structure. Figure A.4 shows boxplots of prediction error over time. Figure A.5 overlays prediction errors for placebo mergers: we repeat this simulation assuming a "placebo merger" happened one year prior to the actual merger and predict prices for that year.

4. Figure A.6 shows the distribution of overprediction (at the DMA-level) over mergers.

5. Table A.4 shows a regression of the internalization parameter $\tau$ on measures of market structure and market size.

6. Figure A.7 shows placebo estimates for the internalization parameter.

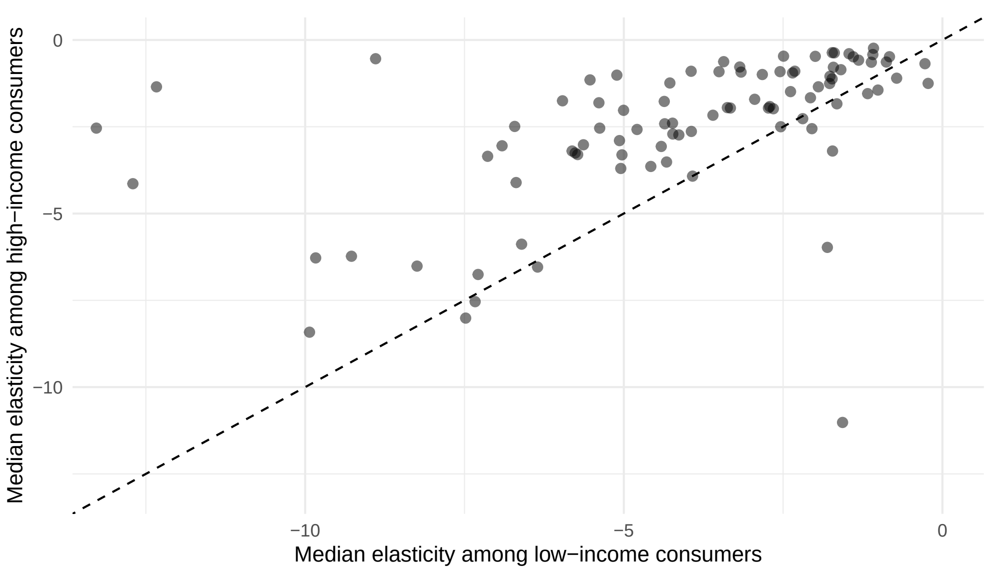
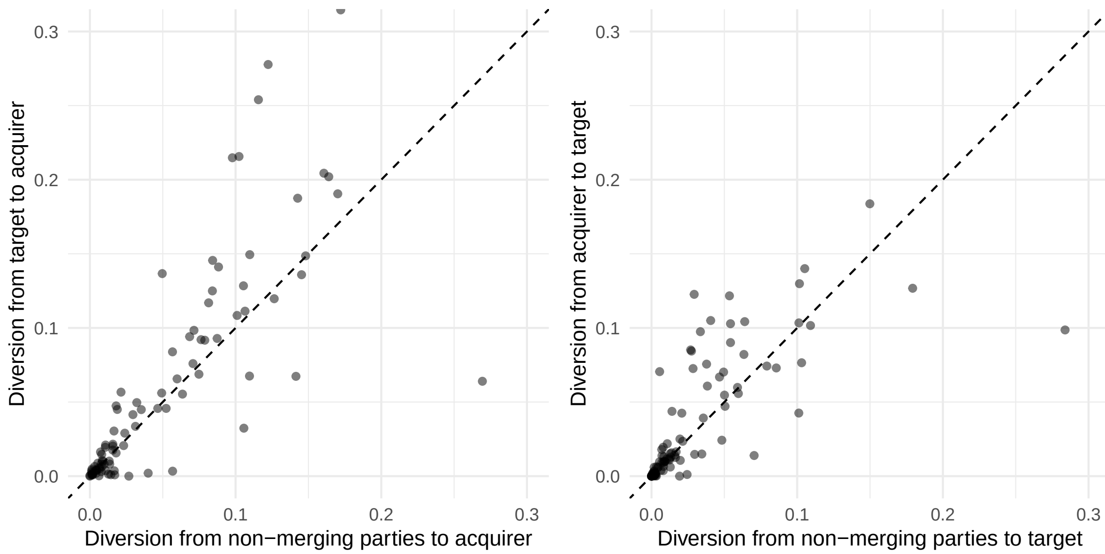



Figure A.1: Median elasticities for low- and high-income consumers




(a) To acquirer

(b) To target

Figure A.2: Comparison of diversion ratios between merging parties to diversion ratios across merging and non-merging parties

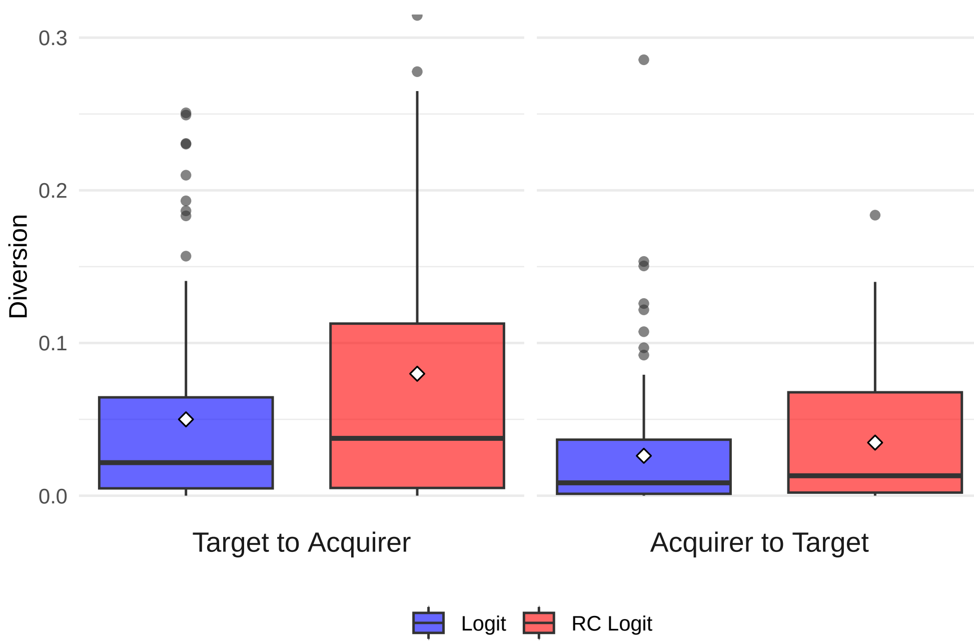



Figure A.3: Comparison of diversion ratios between merging parties under logit and random coefficients logit. The white diamonds are the mean across mergers, while the lower, middle, and upper lines of the boxes are the 25th, 50th, and 75th percentiles.

7. Figure A.8 shows the distribution of synergies assuming partial internalization.

8. Figure A.10 shows the distribution of own-price elasticities across mergers at the optimal

| Product Market | Estimates from Literature | Our Estimates |
|---|---|---|
| Ready-to-eat cereal | -2.3 to -4.3 (Nevo, 2001, Table VIII)<br>-2.4 to -2.7 (Backus et al., 2021, Table 4)<br>-3.9 to -4.2 (Atalay et al., 2025) | -2.3 (RTE cereal)<br>-4.8 (granola) |
| Laundry detergent | -2.4 to -4.3 (Hendel and Nevo, 2006, Table VII)<br>-5.3 to -10.0 (Atalay et al., 2025) | -2.7 (detergent boosters) |
| Beer | -4.3 to -6.3 (Miller and Weinberg, 2017, Section 4.3)<br>-3.4 (Asker, 2016, Table III) | -3.4 to -3.9 |
| Coffee | -3.5 (Nakamura and Zerom, 2010, Table 5)<br>-4.6 to -5.6 (Atalay et al., 2025) | -1.6 |
| Salted Snacks | -1.5 to -3.0 (chips) (Dubois et al., 2018, Table D.5)<br>-4.5 to -8.4 (chips) (Atalay et al., 2025)<br>-3.0 to -5.3 (pretzels) (Atalay et al., 2025) | -12.8 (chips)<br>-3.1 (popcorn)<br>-2.5 to -7.6 (pretzels) |
| Wine | -3.6 to -5.3 (Atalay et al., 2025) | -2.4 to -10.0 |
| Bottled water | -5.0 to -13.0 (Bonnet and Dubois, 2010, Table 6)<br>-2.3 to -6.8 (Atalay et al., 2025) | -3.5 |
| Frozen pizza | -3.5 to -4.3 (Atalay et al., 2025) | -3.0 |
| Frozen breakfasts | -2.6 to -3.7 (Atalay et al., 2025) | -3.9 |
| Sliced lunchmeat | -1.5 to -3.9 (Atalay et al., 2025) | -3.6 |
| Bakery products | -2.1 to -6.5 (Atalay et al., 2025) | -1.4 to -3.3 (-4.4 for fresh bread) |
| Cookies | -3.8 to -4.8 (Atalay et al., 2025) | -1.2 |
| Breakfast sausages | -4.6 to -6.5 (Atalay et al., 2025) | -7.6 |

Table A.1: Comparisons of own-price elasticities from our procedure to those of similar markets in the literature. Ranges for our estimates indicate different mergers (with non-overlapping time periods) and ranges for published papers indicate different specifications, different time periods, or different products (if an average elasticity is not provided).

level of scaling, in the robustness check from Section V.D.

9. Figure A.11 through A.14 show different cuts of ROC and PR curves, for robustness analysis of Figure 8.

| | Merging | | | Overall | | | Non-Merging | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Constant | 0.00 | -0.01 | | 0.02 | 0.00 | | 0.02 | 0.00 | |
| | (0.01) | (0.01) | | (0.01) | (0.01) | | (0.01) | (0.01) | |
| HHI | -0.11** | -0.03 | -0.05*** | -0.13*** | -0.05* | -0.02*** | -0.12** | -0.04* | 0.00 |
| | (0.05) | (0.03) | (0.01) | (0.05) | (0.03) | (0.01) | (0.05) | (0.03) | (0.01) |
| DHHI | -0.67* | -0.47*** | -0.59*** | -0.29 | -0.34*** | -0.53*** | 0.41* | -0.02 | -0.16*** |
| | (0.38) | (0.13) | (0.04) | (0.25) | (0.11) | (0.03) | (0.23) | (0.07) | (0.03) |
| Level | Merger | DMA | DMA | Merger | DMA | DMA | Merger | DMA | DMA |
| Merger FE | No | No | Yes | No | No | Yes | No | No | Yes |

Table A.2: Regression of prediction error on market characteristics, at different levels of observation. Regressions drop any observation where the absolute log prediction error exceeds 0.25.

| | Merging | | | Overall | | | Non-Merging | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Constant | 0.54 | -1.41* | | 0.12 | -0.67 | | -0.27 | -0.91 | |
| | (1.27) | (0.80) | | (1.33) | (0.99) | | (1.41) | (1.09) | |
| HHI $\in$ [1500, 2500] | -2.42* | 0.26 | -0.04 | -0.70 | 0.51 | -0.04 | -0.57 | 0.77 | 0.09 |
| | (1.44) | (0.85) | (0.39) | (1.48) | (0.94) | (0.30) | (1.59) | (1.03) | (0.30) |
| HHI $>$ 2500 | -4.10** | -1.00 | -0.77* | -2.62* | -1.39 | -0.55 | -1.63 | -0.81 | -0.31 |
| | (1.62) | (1.01) | (0.46) | (1.57) | (1.15) | (0.35) | (1.67) | (1.24) | (0.35) |
| DHHI $>$ 100 | -4.07*** | -3.86*** | -2.04*** | -1.85 | -1.86** | -1.62*** | 0.47 | -0.18 | -0.44* |
| | (1.42) | (0.82) | (0.34) | (1.23) | (0.80) | (0.26) | (1.34) | (0.80) | (0.26) |
| Level | Merger | DMA | DMA | Merger | DMA | DMA | Merger | DMA | DMA |
| Merger FE | No | No | Yes | No | No | Yes | No | No | Yes |

Table A.3: Regression of prediction error on market characteristics, at different levels of observation. Regressions drop any observation where the absolute log prediction error exceeds 0.25. Coefficients are multiplied by 100.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Constant | 0.70*** | 0.53*** | 0.57*** | 0.11 | -0.05 |
| | (0.17) | (0.06) | (0.06) | (0.36) | (0.42) |
| HHI $>$ 1500 | -0.22 | | | | -0.12 |
| | (0.18) | | | | (0.17) |
| DHHI $>$ 100 | | -0.10 | | | 0.01 |
| | | (0.10) | | | (0.12) |
| Merging Share $>$ 0.30 | | | -0.30*** | | -0.36*** |
| | | | (0.09) | | (0.12) |
| Log(Merging Revenue) | | | | 0.03 | 0.05** |
| | | | | (0.02) | (0.03) |

Table A.4: Regression of the internalization coefficient on measures of market structure

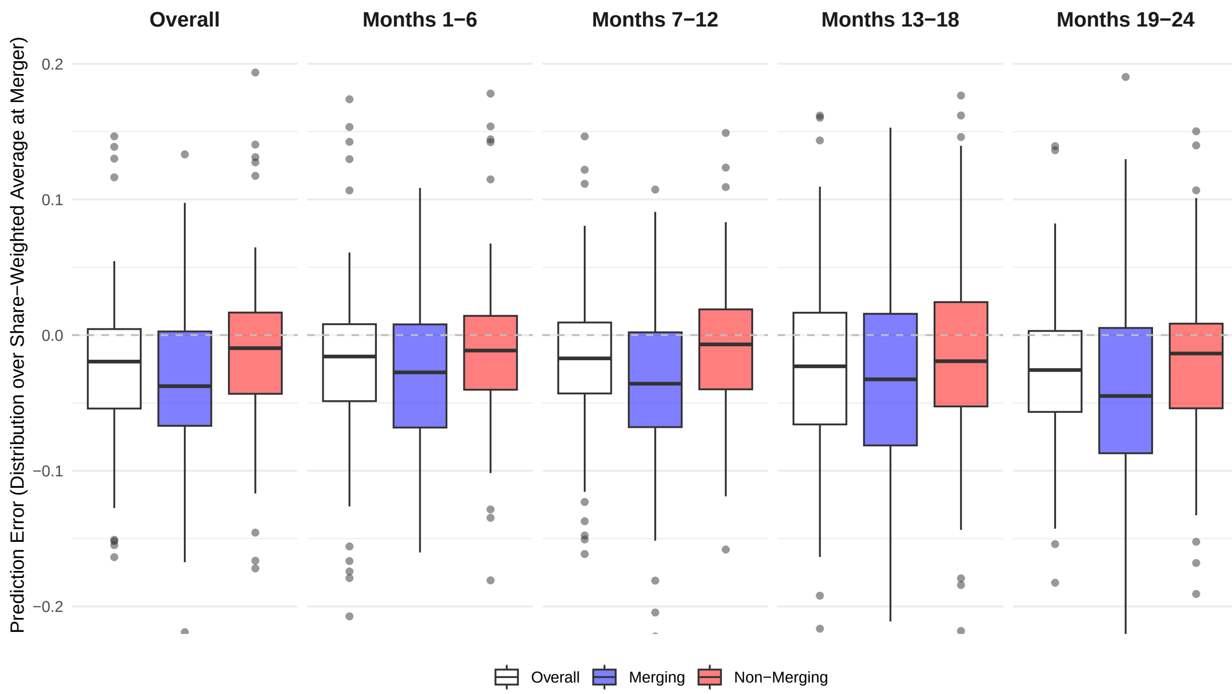



Figure A.4: Distribution of prediction error at the merger level, over time

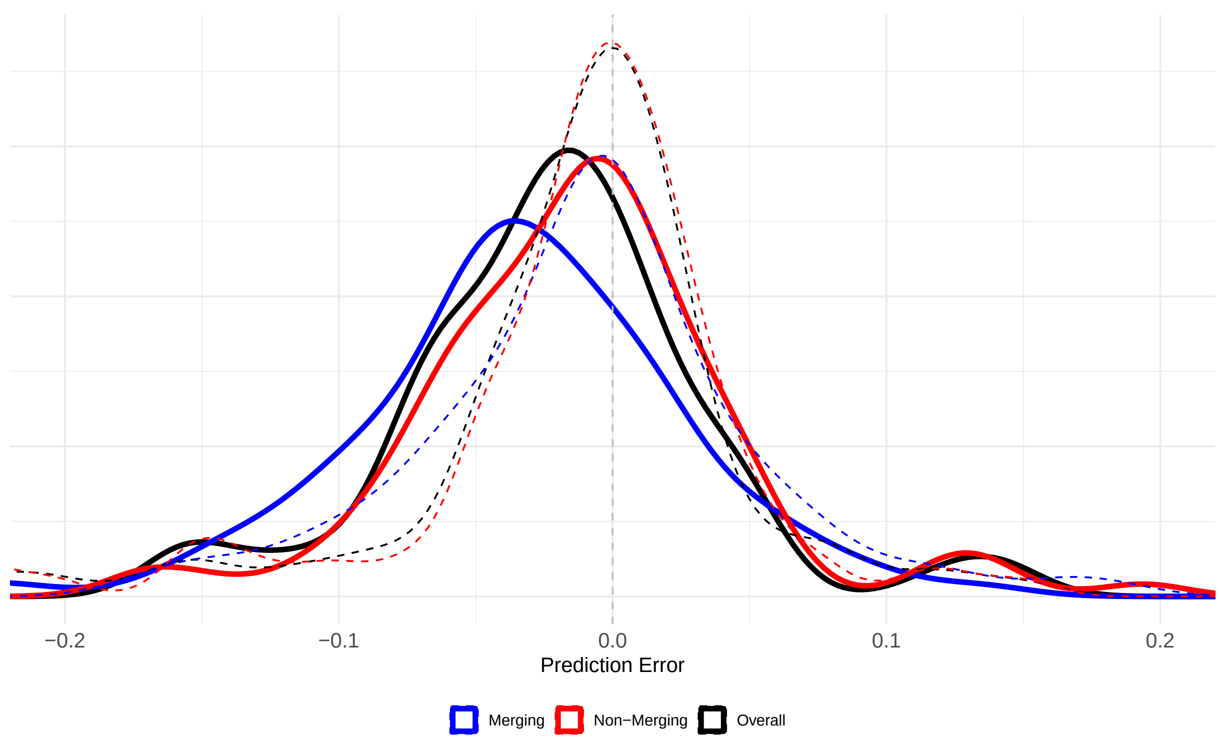
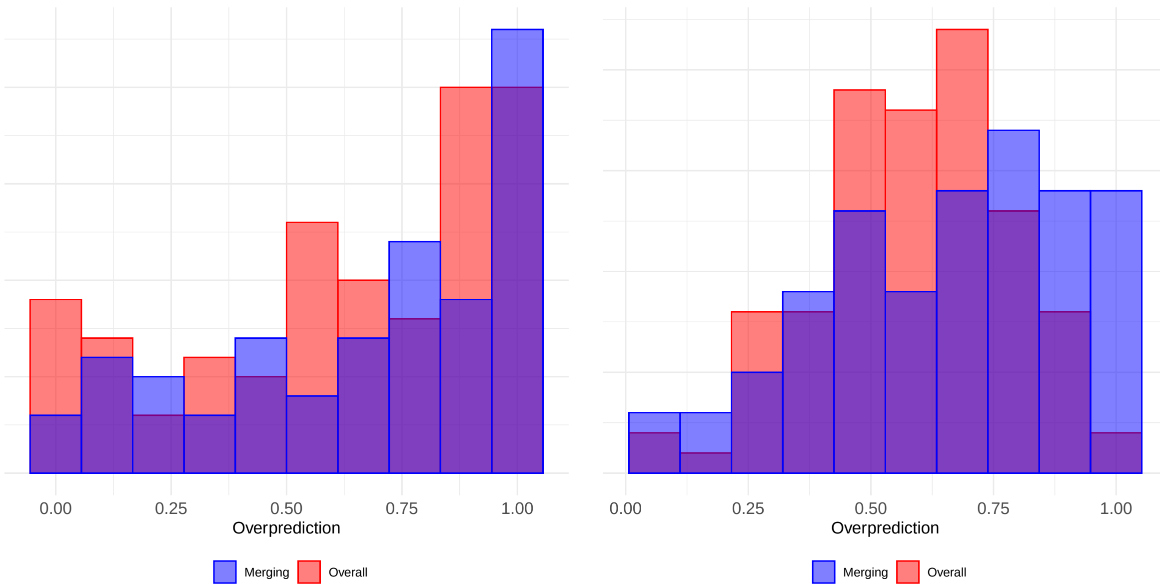



Figure A.5: Distribution of prediction error at the merger level, overlayed with distributions from placebo mergers (dashed)

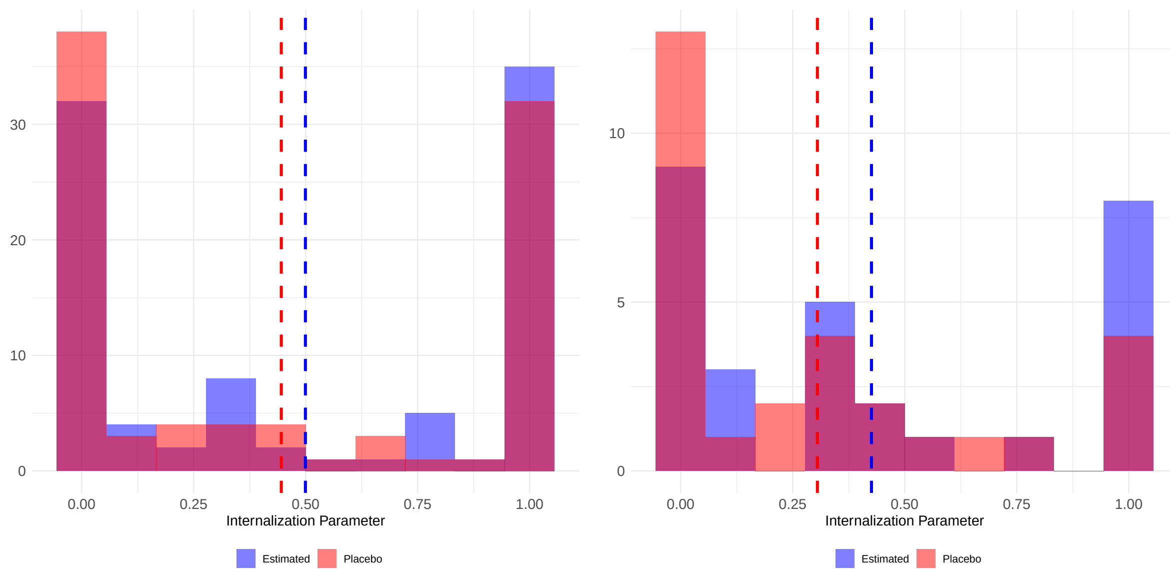



(a) DMA



(b) Product

Figure A.6: Histograms of the share of (a) DMAs or (b) products within a merger in which there is an overprediction of price increases




(a) All Mergers



(b) DHHI $\geq$ 100

Figure A.7: Placebo estimates of imperfect internalization

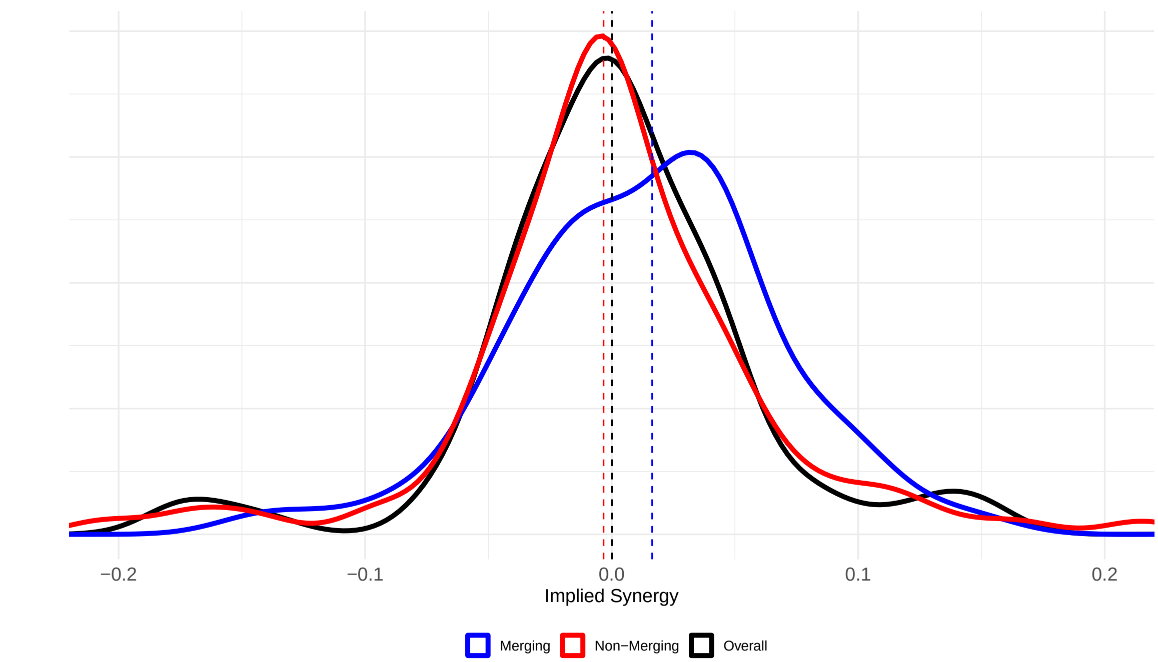
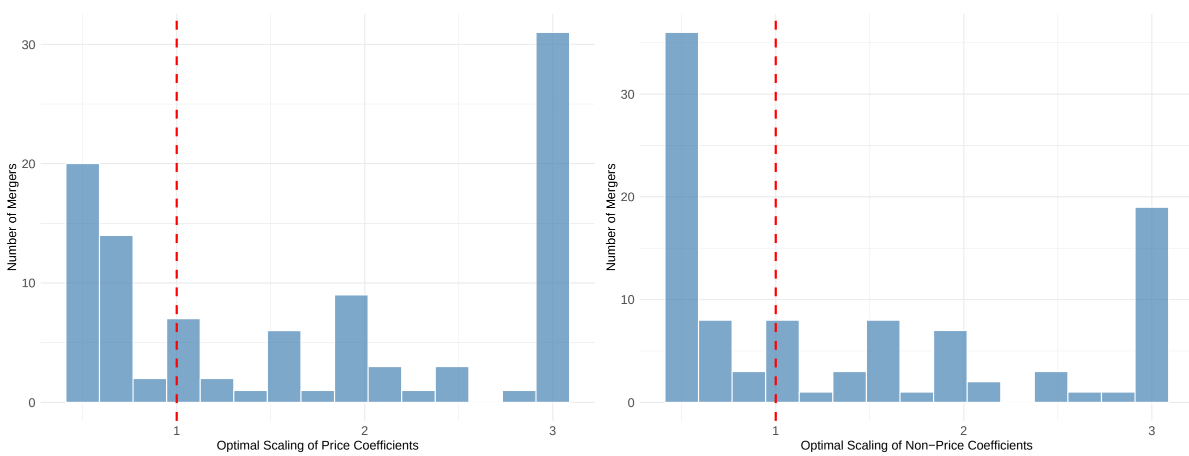



Figure A.8: Distribution of implied synergies at the merger level, assuming partial internalization




(a) Price (b) Non-Price

Figure A.9: Histograms of the optimal scaling of demand coefficients to minimize prediction error

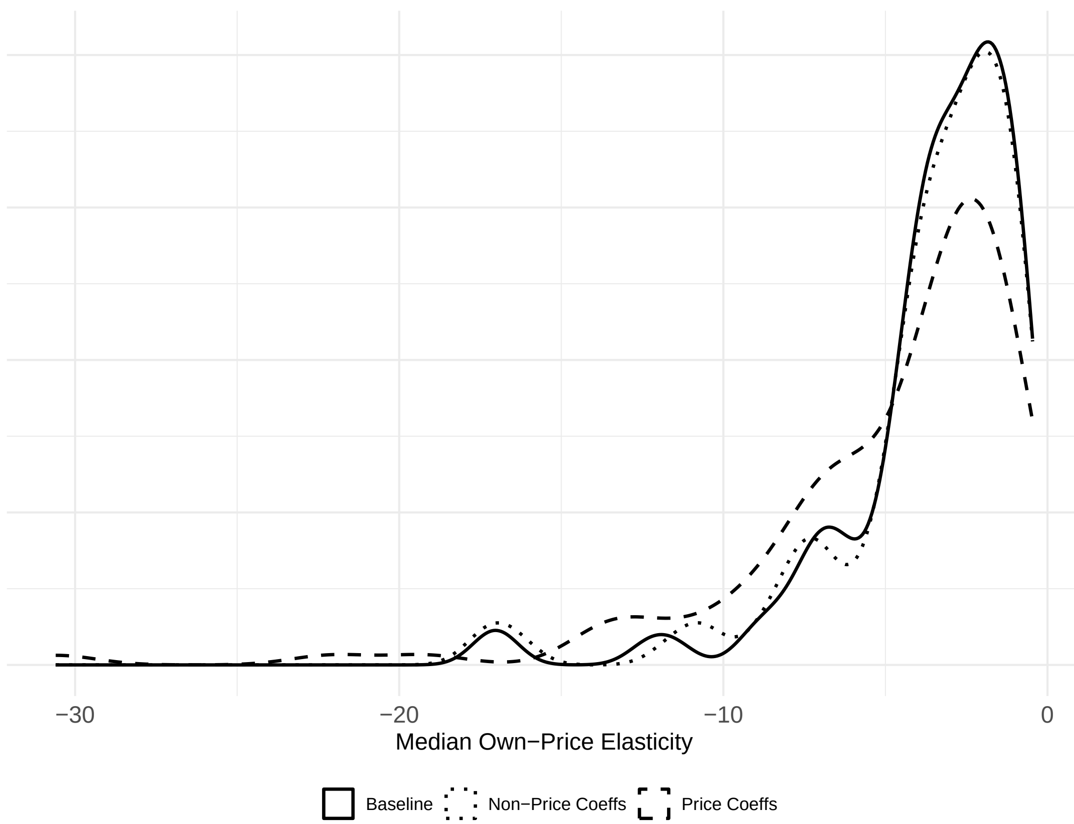
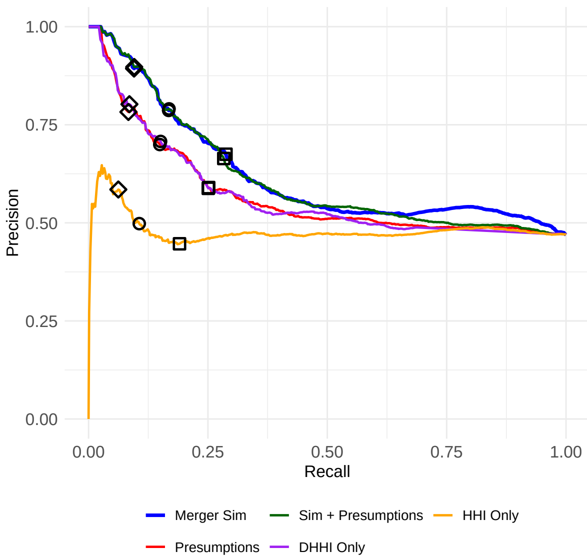
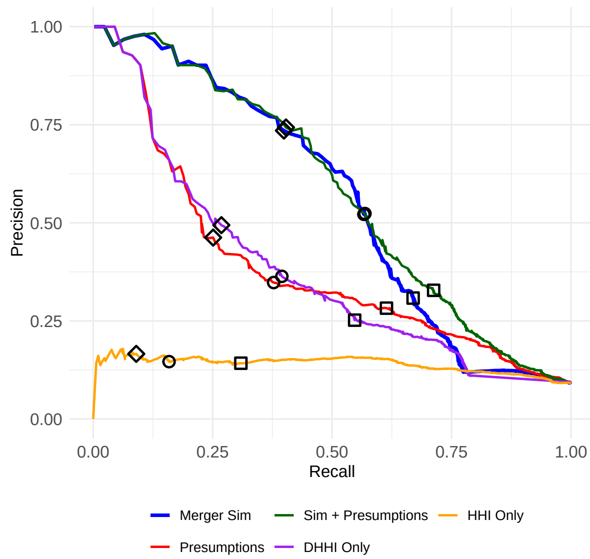


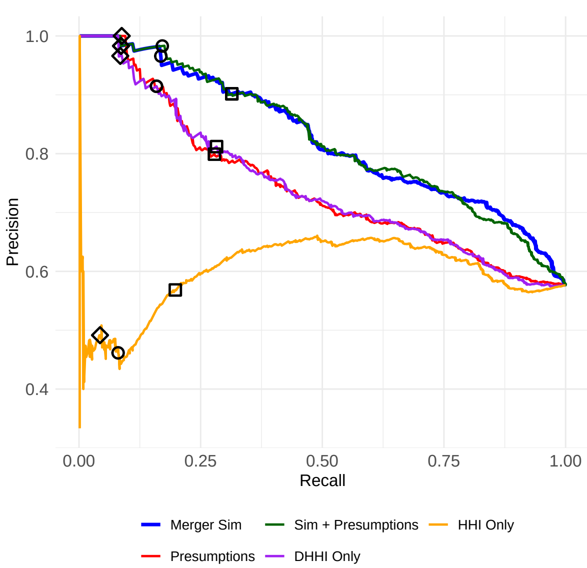

Figure A.10: Elasticities at optimally scaled demand systems



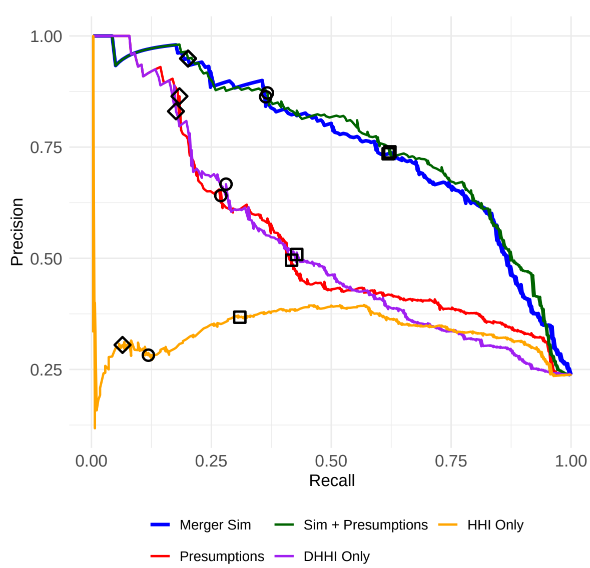

(a) Cutoff = 0%, All DMAs




(b) Cutoff = 2.5%, All DMAs

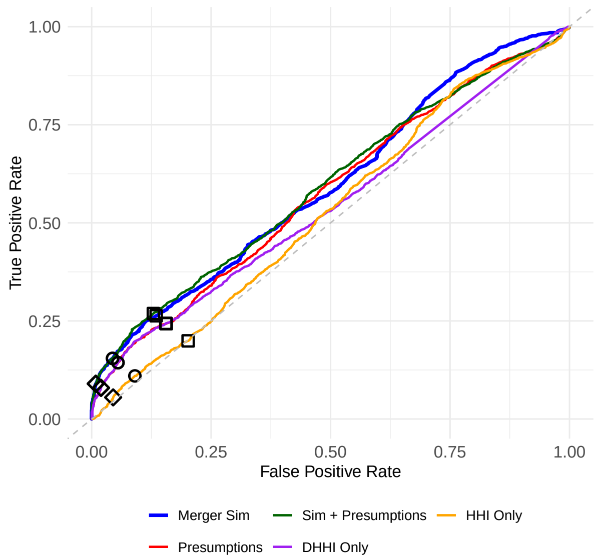



(c) Cutoff = 0%, DHHI $\geq$ 100

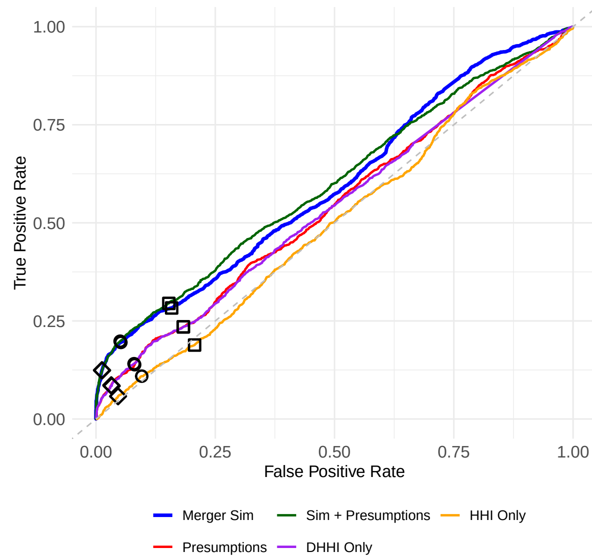



(d) Cutoff = 2.5%, DHHI $\geq$ 100

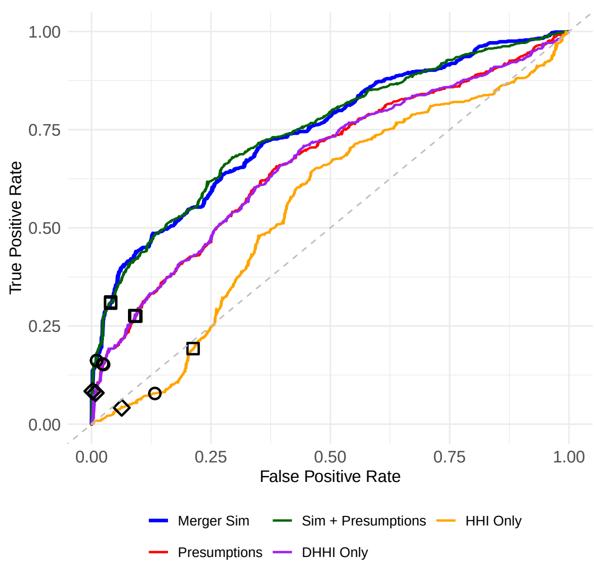
Figure A.11: Precision-recall curves for overall price changes, using a variety of predictors. The diamond, circle, and square mark the points that correspond to marking 5%, 10%, and 20% of the mergers as high price changes.



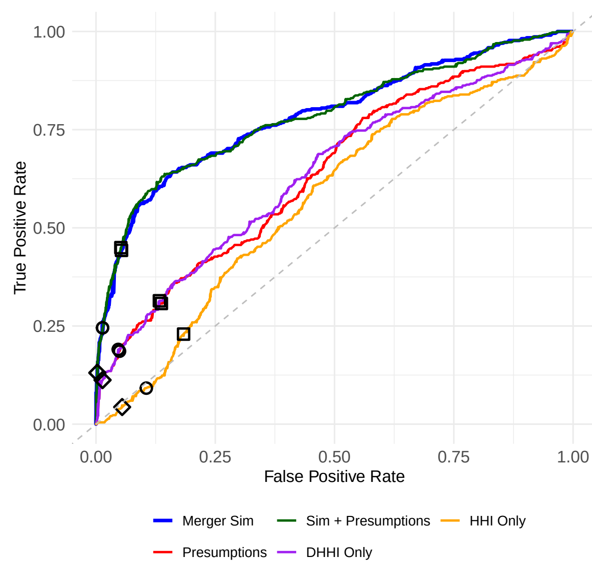

(a) Cutoff = 0%, All DMAs




(b) Cutoff = 2.5%, All DMAs

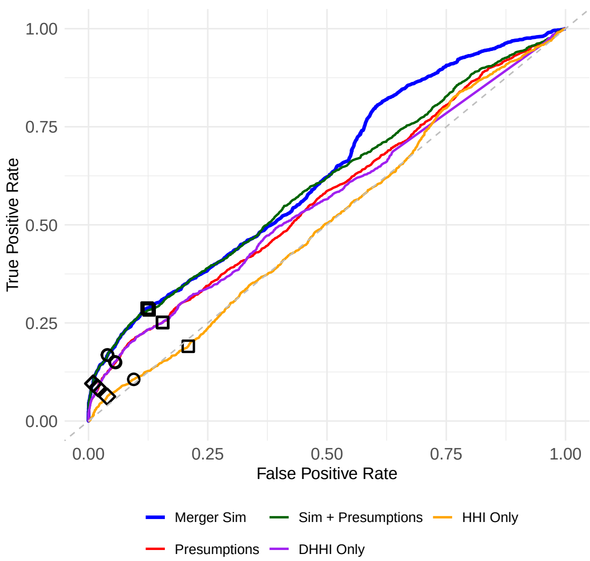



(c) Cutoff = 0%, DHHI $\geq$ 100

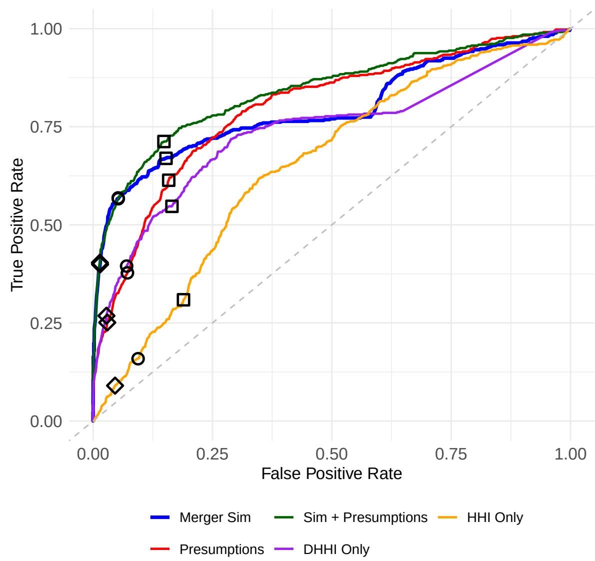



(d) Cutoff = 2.5%, DHHI $\geq$ 100

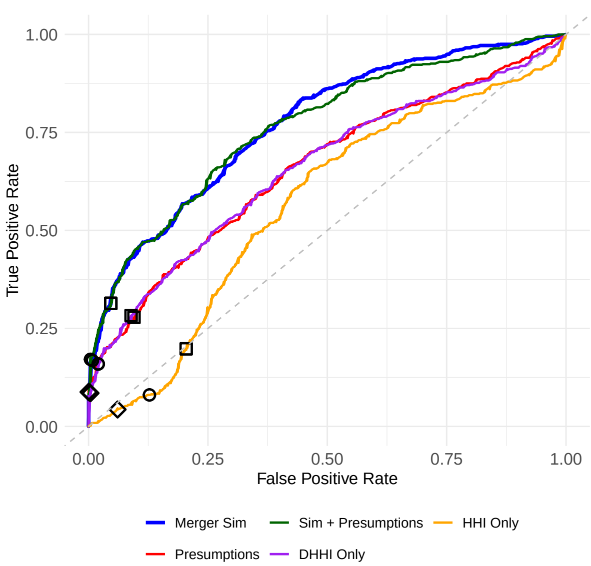
Figure A.12: ROC curves for merging-party price changes, using a variety of predictors. The diamond, circle, and square mark the points that correspond to marking 5%, 10%, and 20% of the mergers as high price changes.



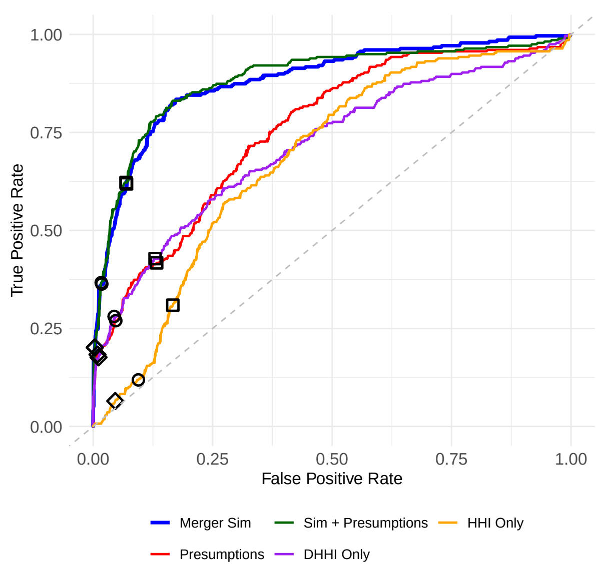

(a) Cutoff = 0%, All DMAs




(b) Cutoff = 2.5%, All DMAs

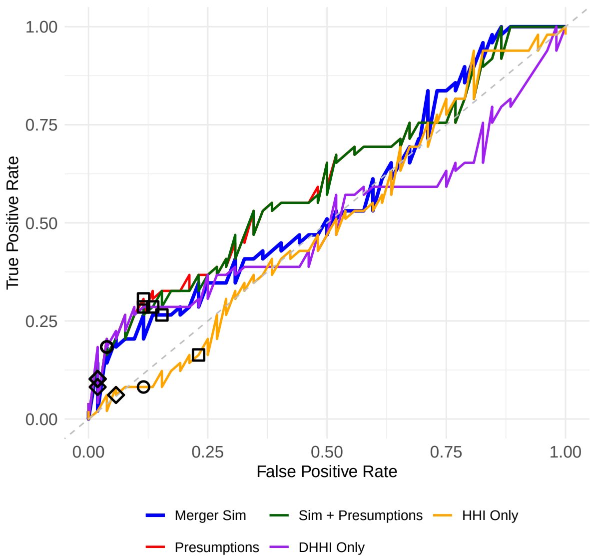



(c) Cutoff = 0%, DHHI $\geq$ 100

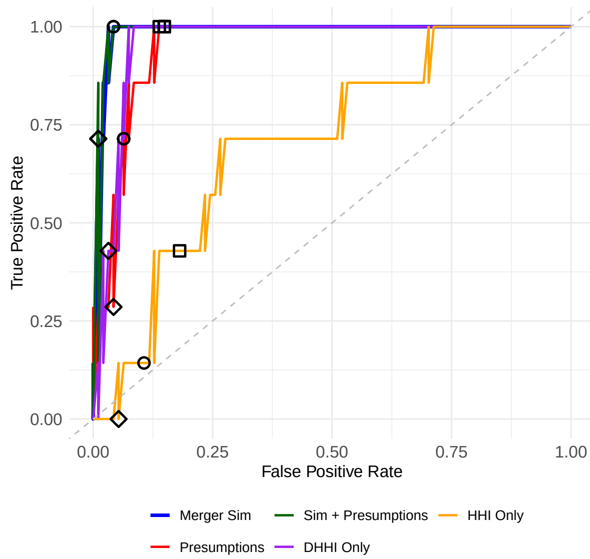



(d) Cutoff = 2.5%, DHHI $\geq$ 100

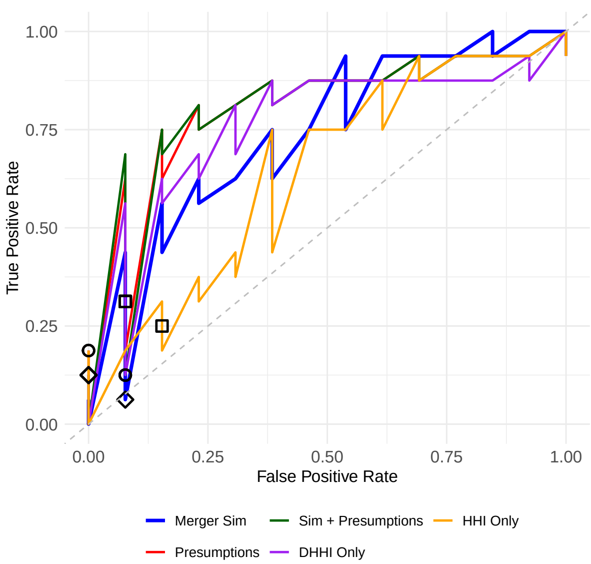
Figure A.13: ROC curves for overall price changes, using a variety of predictors. The diamond, circle, and square mark the points that correspond to marking 5%, 10%, and 20% of the mergers as high price changes.



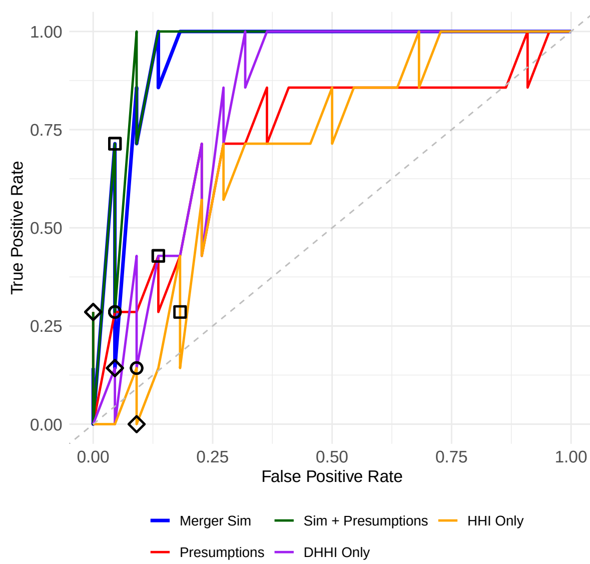

(a) Cutoff = 0%, All DMAs




(b) Cutoff = 2.5%, All DMAs




(c) Cutoff = 0%, DHHI $\geq$ 100




(d) Cutoff = 2.5%, DHHI $\geq$ 100

Figure A.14: ROC curves for overall price changes, using a variety of predictors. The diamond, circle, and square mark the points that correspond to marking 5%, 10%, and 20% of the mergers as high price changes.

## B. Comparing Predicted and Actual Changes

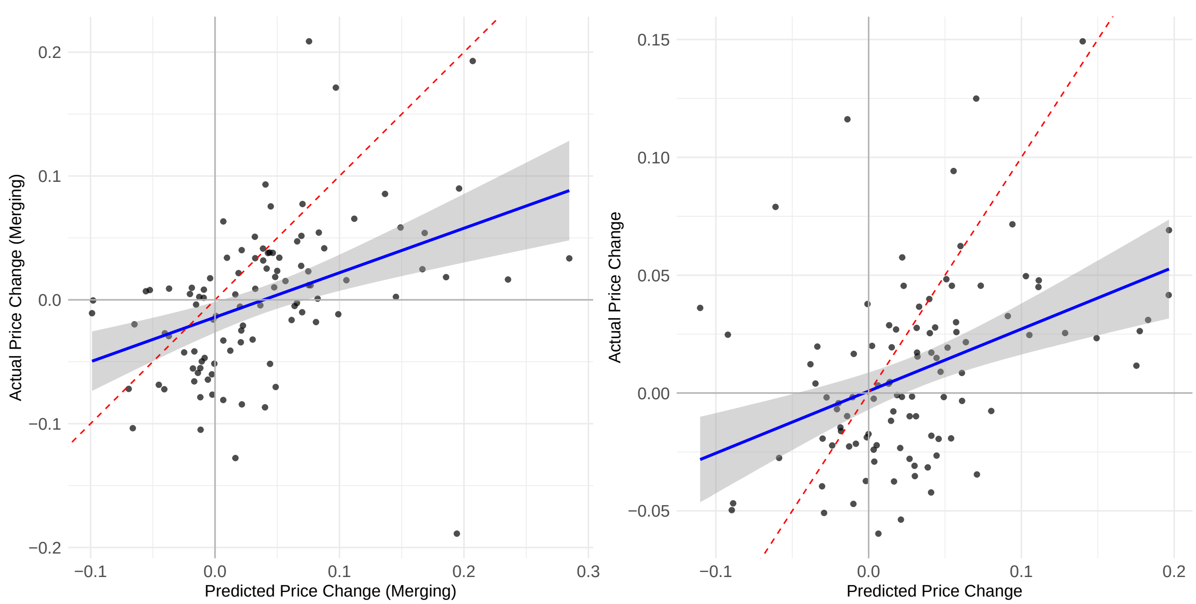
As discussed in Section VI.A, an alternative method to evaluate the predictive power of merger simulations is to define the change as relative to the initial price of the product. Here, we define $p^{\text{Pred}}_{idtm}$ as in Section IV.B, and we define $p^0_{idm}$ as the price of the product in the period before the merger. Define $\Delta\text{Actual}_{idm} \equiv \log(p_{idm}) - \log(p^0_{idm})$ and $\Delta\text{Predicted}_{idm} \equiv \log(p^{\text{Pred}}_{idm}) - \log(p^0_{idm})$. We aggregate these quantities across levels of analysis in the same way we aggregate $\text{Causal}_{idtm}$.

Figure B.1 scatters $\Delta\text{Actual}_m$ against $\Delta\text{Predicted}_m$ both for merging parties and overall. Both panels show that the predictions are correlated with actual price changes, although the predicted price changes do not change one-for-one with actual changes. Since the predicted price changes account for observable changes in cost shifters, they can be negative. However, this also means that the positive correlation is partly due to shifts in costs rather than the structure of the merger simulation itself, unlike in our baseline procedure, which uses the same costs and demand shocks when computing the two prices that determine the change.

To control for such changes, we run an extended version of the Mincer and Zarnowitz (1969) regression:

$$\Delta\text{Actual}_\times = \alpha + \beta \cdot \Delta\text{Predicted}_\times + \text{Controls} + \epsilon_\times, \tag{11}$$

where the $\times$ denotes that the level of the regression changes in different specifications. We include




(a) Merging (b) Overall

Figure B.1: Scatter of actual versus predicted changes in prices, at the merger level, for (a) merging parties, and (b) all parties. The dotted red line indicates the $45°$ line, and the blue line is the line of best fit.

the predicted change in marginal cost, as a proportion of the original price, as a control.[26] When running regressions at the merger or the DMA level, we also control for HHI and DHHI—either averaged across DMAs in the first case, or at the DMA-level in the second. Table B.1 reports results of this analysis, with each column corresponding to a different level of analysis and each panel corresponding to a different set of products.

Columns (1)–(4) show that cross-merger, predicted changes in prices are correlated with actual changes. Unlike in our baseline approach, however, the coefficients do not include 1 in the confidence intervals. This result is not due to changes in marginal costs: we control for predicted changes in marginal costs, which are also typically statistically significant. Finally, in Columns (1) and (2), we also control for market structure, suggesting that the prediction has some relation with the actual changes even controlling for (simple functions of) the market structure. Columns (5)–(7) add merger fixed effects, and the takeaways are similar.[27]

Table B.2 lists partial $R^2$ values for regressions of $\Delta\text{Actual}_{idm}$ on $\Delta\text{Predicted}_{idm}$ at various levels of analysis, controlling for costs and market structure. The first row in each panel shows the unconditional $R^2$: while it is sizeable for merging parties at the merger and DMA levels—even within-merger—it is fairly low at other levels. Part of this predictive power comes from costs, and the partial $R^2$ over costs decreases. The final row shows that while merger simulations have some predictive power over both costs and market structure, this is still low. For comparison, Table B.3 shows analogous quantities for the predictive power of HHI, DHHI, and its interaction, and these are low. Thus, simulations have larger marginal predictive power than measures of market structure do, but it is still low.

Figure B.2 shows ROC curves for actual price changes, both overall and for merging parties, using predicted effects. We restrict to high DHHI DMAs, but the takeaways we discuss in this paragraph are the same for all DMAs. We see that the ROC curves are far from the $45°$ line, suggesting that the prediction has strong predictive power. We also overlay two other ROC curves: one from a prediction using changes in marginal cost, HHI, DHHI, and an interaction; and one from just using marginal cost. The comparisons are somewhat more nuanced than those in the PR and ROC curves using causal and unilateral effects. First, we see that the marginal cost prediction by itself has lower AUC than the merger simulations—but it is not dominated by the merger simulations. In fact, for especially low challenge rates (or high cutoffs), the marginal cost prediction by itself dominates. Second, we see that marginal cost with the structural presumptions is comparable to the predictions themselves: the AUC is typically lower, but this dominates the predictions in certain regions. (Of course, one should remember that this model is estimated exactly on the

[26] When computing this quantity, we winsorize extreme outliers: those where the marginal cost changes by more than 5 times the initial price.

[27] The coefficient on DHHI has an unexpected sign in Column (5), although this could be explained by the fact that it is measuring the residual correlation in actual price changes conditional on the merger simulation itself.

| | Cross-Merger | | | | Within-Merger | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| A. Merging | | | | | | | |
| Constant | -0.01 | -0.01 | -0.02*** | -0.02*** | | | |
| | (0.01) | (0.01) | (0.00) | (0.00) | | | |
| Predicted $\Delta$Price | 0.27 | 0.40*** | 0.34*** | 0.34*** | 0.36*** | 0.29*** | 0.27*** |
| | (0.17) | (0.12) | (0.06) | (0.05) | (0.02) | (0.05) | (0.01) |
| Predicted $\Delta$Marginal Cost | 0.23* | 0.21*** | 0.19** | 0.27*** | 0.34*** | 0.37*** | 0.48*** |
| | (0.12) | (0.08) | (0.08) | (0.06) | (0.03) | (0.06) | (0.01) |
| HHI | -0.02 | -0.02 | | | -0.07*** | | |
| | (0.04) | (0.03) | | | (0.01) | | |
| DHHI | 0.10 | -0.02 | | | -0.26*** | | |
| | (0.31) | (0.13) | | | (0.05) | | |
| B. Overall | | | | | | | |
| Constant | 0.01 | 0.00 | -0.01* | -0.02*** | | | |
| | (0.01) | (0.01) | (0.00) | (0.00) | | | |
| Predicted $\Delta$Price | 0.27*** | 0.27*** | 0.17*** | 0.36*** | 0.27*** | 0.24*** | 0.40*** |
| | (0.07) | (0.05) | (0.05) | (0.03) | (0.02) | (0.02) | (0.00) |
| Predicted $\Delta$Marginal Cost | 0.02 | 0.07* | 0.23*** | 0.14*** | 0.10*** | 0.30*** | 0.17*** |
| | (0.06) | (0.04) | (0.06) | (0.03) | (0.01) | (0.02) | (0.00) |
| HHI | -0.04 | -0.01 | | | 0.01 | | |
| | (0.03) | (0.02) | | | (0.01) | | |
| DHHI | 0.08 | 0.03 | | | -0.09*** | | |
| | (0.12) | (0.06) | | | (0.03) | | |
| C. Non-Merging | | | | | | | |
| Constant | 0.01 | 0.01 | -0.01 | -0.02*** | | | |
| | (0.01) | (0.01) | (0.00) | (0.00) | | | |
| Predicted $\Delta$Price | 0.21*** | 0.24*** | 0.14** | 0.36*** | 0.32*** | 0.24*** | 0.43*** |
| | (0.06) | (0.06) | (0.07) | (0.04) | (0.02) | (0.02) | (0.01) |
| Predicted $\Delta$Marginal Cost | 0.03 | 0.03 | 0.24*** | 0.12*** | 0.03** | 0.30*** | 0.14*** |
| | (0.06) | (0.05) | (0.06) | (0.03) | (0.01) | (0.02) | (0.00) |
| HHI | -0.04* | -0.02 | | | 0.02 | | |
| | (0.02) | (0.02) | | | (0.01) | | |
| DHHI | 0.22 | -0.01 | | | -0.16*** | | |
| | (0.18) | (0.07) | | | (0.03) | | |
| Level | Merger | DMA | Product | P-DMA | DMA | Product | P-DMA |
| Merger FE | No | No | No | No | Yes | Yes | Yes |

Table B.1: Regressions of actual change in (log) price on predicted change in (log) price, at various levels. Each column runs the regression at a different level of observation (with "P-DMA" standing for product-DMA). Panels A and C focus on merging and non-merging price changes only, respectively. Panel B reports results for all price changes. Measures of market structure are not defined at the product level and are thus omitted from Columns (3), (4), (6), and (7).

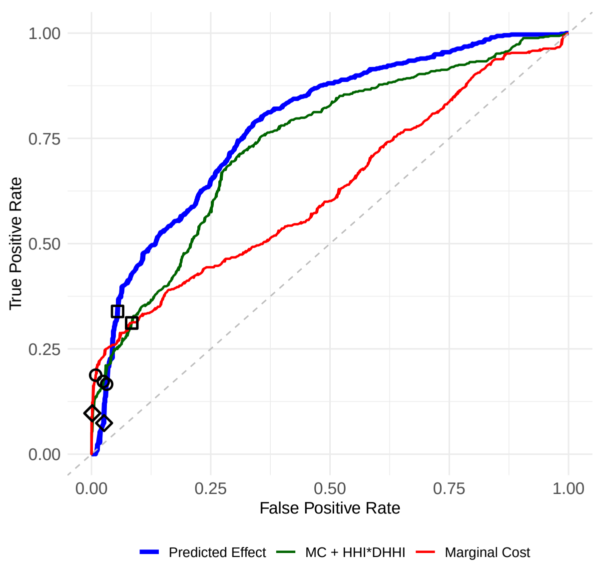
| | Cross-Merger | | | | Within-Merger | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| A. Merging | | | | | | | |
| Individually | 0.173 | 0.169 | 0.071 | 0.086 | 0.155 | 0.064 | 0.083 |
| Over Costs | 0.086 | 0.070 | 0.028 | 0.025 | 0.041 | 0.013 | 0.012 |
| Over Costs and Market Structure | 0.063 | 0.065 | – | – | 0.046 | – | – |
| B. Overall | | | | | | | |
| Individually | 0.163 | 0.121 | 0.028 | 0.051 | 0.077 | 0.038 | 0.054 |
| Over Costs | 0.112 | 0.074 | 0.004 | 0.017 | 0.052 | 0.007 | 0.019 |
| Over Costs and Market Structure | 0.111 | 0.070 | – | – | 0.052 | – | – |
| C. Non-Merging | | | | | | | |
| Individually | 0.101 | 0.052 | 0.025 | 0.046 | 0.039 | 0.036 | 0.052 |
| Over Costs | 0.046 | 0.031 | 0.003 | 0.016 | 0.033 | 0.006 | 0.019 |
| Over Costs and Market Structure | 0.062 | 0.032 | – | – | 0.035 | – | – |
| Level | Merger | DMA | Product | P-DMA | DMA | Product | P-DMA |
| Merger FE | No | No | No | No | Yes | Yes | Yes |

Table B.2: Partial $R^2$ of predicted change in prices over different characteristics of the merger. Columns (5)–(7) report within-$R^2$ values.

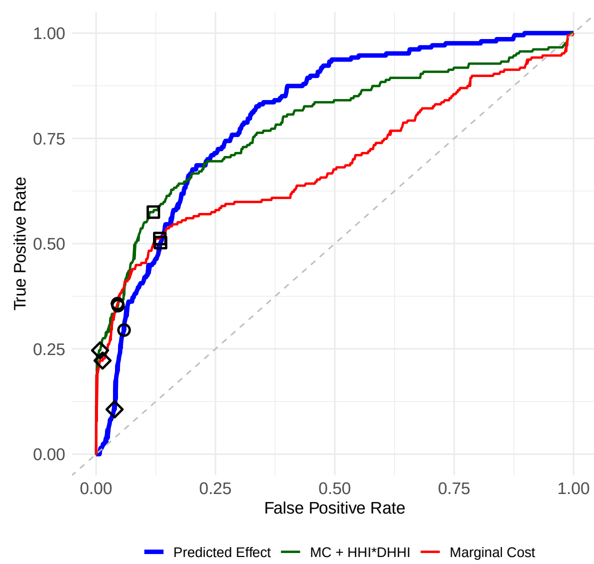
| | Merging | | | Overall | | | Non-Merging | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Individually | 0.016 | 0.005 | 0.005 | 0.012 | 0.005 | 0.002 | 0.012 | 0.001 | 0.003 |
| Over Costs | 0.028 | 0.006 | 0.005 | 0.021 | 0.006 | 0.002 | 0.018 | 0.000 | 0.004 |
| Over Costs and Predictions | 0.003 | 0.001 | 0.011 | 0.019 | 0.001 | 0.002 | 0.034 | 0.001 | 0.006 |
| Level | Merger | DMA | DMA | Merger | DMA | DMA | Merger | DMA | DMA |
| Merger FE | No | No | Yes | No | No | Yes | No | No | Yes |

Table B.3: Partial $R^2$ of market structure in prices over different characteristics of the merger

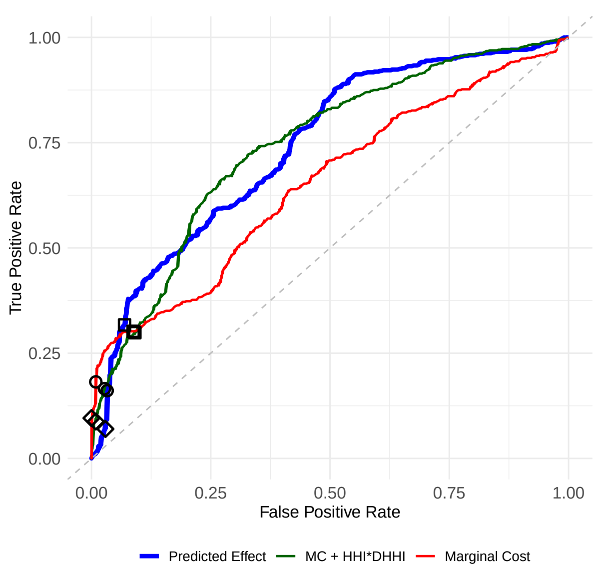
sample on which it is being used to predict, which artificially inflates the ROC curve; however, estimating on a holdout sample does not change the results appreciably.) On net, our takeaway from this exercise is predictions do have significantly predictive power for high price changes, but the marginal contribution of the simulations when analyzing predictions is limited—unless the agencies are challenging a large set of mergers. These results provide another justification for the unilateral/causal analysis done in the main text.



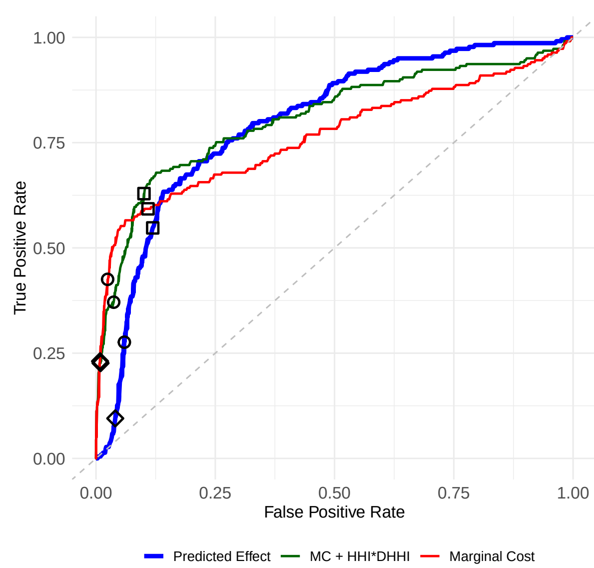

(a) Cutoff = 0%, Merging




(b) Cutoff = 5%, Merging




(c) Cutoff = 0%, Overall




(d) Cutoff = 5%, Overall

Figure B.2: ROC curves for actual price changes, using predicted price changes, marginal costs with market structure, and marginal cost by themselves as predictors. We restrict to DMAs with DHHI $\geq 100$.

## C. List of All Mergers

Table C.1 summarizes all of the mergers included in our sample. For each merger, we list the NielsenIQ department and product group of the relevant product market, and the NielsenIQ product modules that comprise the relevant product market, along with criteria used for dividing the modules into separate markets (if any). We then define all characteristics that we extract from the scanner data, the share of variance of those characteristics (along with a private-label indicator and the package size, which are included in all mergers) that are explained by the first two principal components, the set of candidate cost shifter instruments, and whether the merger is included in our final estimation sample. For mergers that are included, the table also lists the set of instruments (out of Gandhi-Houde, cost shifters, and Hausman) and specification of demographic interactions (out of Price, 1+Price, and 1+PC+Price) used in the demand estimates included in our sample.

The "cost shifters" column can be read as follows. Each instrument is separated by a semicolon. Generally, these share a name with the FRED series from which they are taken. Sometimes, the instruments are multiplied by a boolean expression of a characteristic, in which case the instrument is interacted with an indicator for whether the boolean is true. If this column is empty then there are no cost shifter instruments defined for this merger and we rely on the other two types of instruments for estimation (differentiation IVs and Hausman instruments).

| Merger | NielsenIQ department | NielsenIQ product group | Product market definition | Characteristics | % variance explained by two PCs | Cost shifters | In estimation sample | Instruments used | RC on constant | Demographic interactions |
|---|---|---|---|---|---|---|---|---|---|---|
| 1 | Alcoholic beverages | Beer | Ale; Beer; Light Beer (Low Calorie/Alcohol); Stout And Porter | Container; ice; imported; module | 41% | Aluminum x container == “can”; barley; barley x module != “light”; dollar strength x imported == 1; glass x container == “bottle”; wheat | Yes | GH | Yes | 1+PC+Price |
| 2 | Alcoholic beverages | Beer | Ale; Beer; Light Beer (Low Calorie/Alcohol); Stout And Porter | Container; ice; imported; module | 50% | Aluminum x container == “can”; barley; barley x module != “light”; dollar strength x imported == 1; glass x container == “bottle”; wheat | Yes | GH+CS | Yes | 1+PC+Price |
| 3 | Alcoholic beverages | Liquor | Alcoholic Cocktails | Flavor; liquor; party pack; quality | 28% | Barley x liquor in [“whiskey”, “gin”]; corn x liquor == “whiskey”; dollar strength; grapes x liquor == “other”; russet potatoes x liquor == “vodka”; sugar x liquor in [“tequila”, “rum”]; wheat x liquor in [“gin”, “vodka”]; wine grapes x liquor == “other” | Yes | GH | Yes | 1+PC+Price |
| 4 | Alcoholic beverages | Liquor | Bourbon-Blended; Bourbon-Straight/Bonded; Canadian Whiskey; Irish Whiskey; Remaining Whiskey; Scotch | Imported; liquor type; proof; quality | 49% | Barley; corn; dollar strength; dollar strength x imported == 1 | Yes | GH+CS | Yes | 1+PC+Price |
| 5 | Alcoholic beverages | Liquor | Bourbon-Blended; Bourbon-Straight/Bonded; Canadian Whiskey; Irish Whiskey; Remaining Whiskey; Scotch | Imported; liquor type; proof; quality | 49% | Barley; corn; dollar strength; dollar strength x imported == 1 | Yes | GH | Yes | 1+PC+Price |
| 6 | Alcoholic beverages | Liquor | Cordials & Proprietary Liqueurs | Alcohol content; flavor; imported; liquor | 28% | Barley x liquor == “neutral spirit”; corn x liquor == “whiskey or rum”; dollar strength; dollar strength x imported == 1; grapes x liquor == “brandy or cognac”; oranges x liquor == “triple sec”; russet potatoes x liquor == “neutral spirit”; sugar x liquor in [“whiskey or rum”, “triple sec”]; wheat x liquor in [“neutral spirit”, “other”]; wine grapes x liquor == “brandy or cognac” | Yes | GH | Yes | 1+PC+Price |
| 7 | Alcoholic beverages | Liquor | Cordials & Proprietary Liqueurs | Alcohol content; flavor; imported; liquor | 28% | Barley x liquor == “neutral spirit”; corn x liquor == “whiskey or rum”; dollar strength; dollar strength x imported == 1; grapes x liquor == “brandy or cognac”; oranges x liquor == “triple sec”; russet potatoes x liquor == “neutral spirit”; sugar x liquor in [“whiskey or rum”, “triple sec”]; wheat x liquor in [“neutral spirit”, “other”]; wine grapes x liquor == “brandy or cognac” | Yes | GH | Yes | 1+PC+Price |
| 8 | Alcoholic beverages | Liquor | Cordials & Proprietary Liqueurs | Alcohol content; flavor; imported; liquor | 27% | Barley x liquor == “neutral spirit”; corn x liquor == “whiskey or rum”; dollar strength x imported == 1; grapes x liquor == “brandy or cognac”; oranges x liquor == “triple sec”; russet potatoes x liquor == “neutral spirit”; sugar x liquor in [“whiskey or rum”, “triple sec”]; wheat x liquor in [“neutral spirit”, “other”]; wine grapes x liquor == “brandy or cognac” | Yes | GH | Yes | 1+PC+Price |
| 9 | Alcoholic beverages | Liquor | Gin | Flavored; gin type; imported; quality | 46% | Barley; dollar strength; dollar strength x imported == 1; wheat | Yes | GH | Yes | 1+PC+Price |
| 10 | Alcoholic beverages | Liquor | Rum | Flavored; imported; proof; quality; type of rum | 42% | Dollar strength; dollar strength x imported == 1; sugar | Yes | GH | Yes | 1+PC+Price |
| 11 | Alcoholic beverages | Liquor | Tequila | Flavored; proof; quality; type of tequila | 39% | Dollar strength; sugar | Yes | GH+CS | Yes | 1+PC+Price |
| 12 | Alcoholic beverages | Liquor | Vodka | Flavored; imported; proof; quality | 53% | Dollar strength; dollar strength x imported == 1; russet potatoes; wheat | Yes | GH | Yes | 1+PC+Price |

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| 13 | Alcoholic beverages | Liquor | Vodka | Flavored; imported; proof; quality | 53% | Dollar strength; dollar strength x imported == 1; russet potatoes; wheat | Yes | GH+CS | Yes | 1+PC+Price |
| 14 | Alcoholic beverages | Wine | Wine-Domestic Dry Table; Wine-Imported Dry Table; is red = 0 | Grape type; imported; is red; quality | 37% | Dollar strength x imported == 1; wine grapes | Yes | GH+H | Yes | 1+PC+Price |
| 15 | Alcoholic beverages | Wine | Wine-Domestic Dry Table; Wine-Imported Dry Table; is red = 1 | Grape type; imported; is red; quality | 32% | Dollar strength x imported == 1; wine grapes | Yes | GH+H | Yes | 1+PC+Price |
| 16 | Dry grocery | Baby Food | Baby Milk And Milk Flavoring; infant and toddler = 0 | Form; infant and toddler; organic | 47% | Corn sweeteners; starch vegetable fats oils; vitamin nutrient hematinic human | Yes | GH | No | Price |
| 17 | Dry grocery | Baby Food | Baby Milk And Milk Flavoring; infant and toddler = 1 | Form; infant and toddler; organic | 77% | Corn sweeteners; starch vegetable fats oils; vitamin nutrient hematinic human | Yes | GH+H | Yes | 1+Price |
| 18 | Dry grocery | Bread And Baked Goods | Bakery - Bread - Fresh | Bread type; bread usage; is herb garlic; is potato; is sourdough; is sweet; is wheat; low cholesterol; low fat; regular health | 32% | Russet potatoes x is potato == 1; wheat | Yes | GH | No | Price |
| 19 | Dry grocery | Bread And Baked Goods | Bakery - Bread - Fresh | Bread type; bread usage; is herb garlic; is potato; is sourdough; is sweet; is wheat; low cholesterol; low fat; regular health | 33% | Russet potatoes x is potato == 1; wheat | Yes | GH | Yes | 1+PC+Price |
| 20 | Dry grocery | Bread And Baked Goods | Bakery-Bagels-Fresh | Bagel size; is low fat; primary flavor; quality tier; secondary flavor | 8% | Wheat; wheat flour | Yes | GH | Yes | 1+PC+Price |
| 21 | Dry grocery | Bread And Baked Goods | Bakery-Bagels-Fresh | Bagel size; has no salt; is low fat; is sliced; is wheat; primary flavor; quality tier; secondary flavor | 9% | Sugar x primary flavor == “sweet”; wheat; wheat flour | Yes | GH | Yes | Price |
| 22 | Dry grocery | Bread And Baked Goods | Bakery-Breakfast Cakes/Sweet Rolls-Fresh | Has nuts; is filled; primary flavor; primary type; quality tier; secondary flavor; secondary type | 6% | Cocoa beans x primary flavor == “chocolate”; corn sweeteners; sugar; wheat; wheat flour | Yes | GH | Yes | 1+Price |
| 23 | Dry grocery | Bread And Baked Goods | Bakery-Buns-Fresh | Ingredient; is low cholesterol; is low fat; is potato; is specialty; is wheat; primary flavor; primary type; secondary flavor; secondary type | 10% | Russet potatoes x is potato == 1; wheat | Yes | GH+CS | Yes | 1+PC+Price |
| 24 | Dry grocery | Bread And Baked Goods | Bakery-Buns-Fresh | Ingredient; is low cholesterol; is low fat; is potato; is specialty; is wheat; primary flavor; primary type; secondary flavor; secondary type | 9% | Russet potatoes x is potato == 1; wheat | Yes | GH+H | Yes | 1+PC+Price |
| 25 | Dry grocery | Bread And Baked Goods | Bakery-Doughnuts-Fresh | Donut style; has chocolate; is frosted; is glazed; primary flavor; secondary flavor | 11% | Cocoa beans x has chocolate == 1; corn sweeteners; sugar; vegetable oil; wheat | Yes | GH | Yes | 1+PC+Price |
| 26 | Dry grocery | Bread And Baked Goods | Bakery-Doughnuts-Fresh | Donut style; has chocolate; is frosted; is glazed; primary flavor; secondary flavor | 10% | Cocoa beans x has chocolate == 1; corn sweeteners; sugar; vegetable oil; wheat | Yes | GH | Yes | 1+PC+Price |
| 27 | Dry grocery | Bread And Baked Goods | Bakery-Muffins-Fresh | Is english muffin; is mini; is wheat; low cholesterol; low fat; primary flavor; quality tier; secondary flavor | 8% | Cocoa beans x primary flavor == “chocolate dessert”; other grains x primary flavor == “breakfast grains”; sugar x is english muffin == 0; wheat; wheat flour | Yes | GH+H | Yes | 1+PC+Price |
| 28 | Dry grocery | Bread And Baked Goods | Bakery-Pies-Fresh | Is tiny; primary flavor; secondary flavor | 21% | | Yes | GH | Yes | 1+Price |
| 29 | Dry grocery | Bread And Baked Goods | Bakery-Rolls-Fresh | Bread region; is low cholesterol; is low fat; is potato; is sourdough; is wheat; primary flavor; primary type; secondary flavor; secondary type | 3% | Russet potatoes x is potato == 1; wheat | Yes | GH+H | Yes | 1+PC+Price |
| 30 | Dry grocery | Bread And Baked Goods | Bakery-Rolls-Fresh | Bread region; is low cholesterol; is low fat; is potato; is sourdough; is wheat; primary flavor; primary type; secondary flavor; secondary type | 4% | Russet potatoes x is potato == 1; wheat | Yes | GH+H | Yes | 1+PC+Price |
| 31 | Dry grocery | Candy | Candy-Chocolate; Candy-Chocolate-Miniatures; Candy-Chocolate-Special | Chocolate shape; chocolate type; flavor; high quality; holiday; module info | 21% | Almonds x flavor == “almond”; cocoa beans; cocoa beans x chocolate type != “white”; corn sweeteners x flavor == “caramel”; peanuts x flavor == “peanut butter”; sugar; sugar x chocolate type != “bittersweet” | No | | | |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| 32 | Dry grocery | Candy | Candy-Hard Rolled; Candy-Lollipops; Candy-Non-Chocolate; Candy-Non-Chocolate-Miniatures | Candy type | 52% | Corn sweeteners; sugar | No | | | |
| 33 | Dry grocery | Candy | Candy-Hard Rolled; Candy-Lollipops; Candy-Non-Chocolate; Candy-Non-Chocolate-Miniatures | Candy type; flavor; holiday | 28% | Corn sweeteners; sugar | No | | | |
| 34 | Dry grocery | Cereal | Cereal - Granola & Natural Types | Has chocolate; high fiber; high protein; low sugar; nut type; primary flavor; secondary flavor | 11% | Almonds x nut type == “almond”; other grains; peanuts x nut type == “peanut”; pecans x nut type == “pecan”; sugar x low sugar == 0 | Yes | GH | Yes | 1+PC+Price |
| 35 | Dry grocery | Cereal | Cereal - Ready To Eat | Additions; audience; high fiber; ingredient; is sweet; low cholesterol; low sugar | 38% | Corn; corn sweeteners; corn sweeteners x is sweet; corn x ingredient == “corn”; oats; oats x ingredient == “oats”; rough rice; rough rice x ingredient == “rice”; sugar; sugar x is sweet; wheat; wheat x ingredient in [“wheat”, “multigrain/other”] | Yes | GH+H | Yes | 1+PC+Price |
| 36 | Dry grocery | Coffee | Coffee - Soluble; Coffee - Soluble Flavored | Decaf; drying method; flavor; is flavored; milk beverage; roast | 37% | Cocoa beans x flavor == “chocolate”; coffee beans; dairy x milk beverage == 1 | Yes | GH+H | Yes | 1+PC+Price |
| 37 | Dry grocery | Coffee | Ground And Whole Bean Coffee; cup = 0 | Cup; decaf; flavored | 59% | Coffee beans | Yes | GH+H | Yes | Price |
| 38 | Dry grocery | Coffee | Ground And Whole Bean Coffee; cup = 1 | Cup; decaf; flavored | 58% | Coffee beans | Yes | GH+H | Yes | Price |
| 39 | Dry grocery | Condiments, Gravies, And Sauces | Fish & Seafood & Cocktail Sauce | Sauce type | 46% | Mayonnaise and dressing x sauce type == “tartar”; pickles and horseradish x sauce type == “tartar”; shrimp; tomatos x sauce type == “cocktail”; unprocessed finfish | Yes | GH+CS | Yes | 1+PC+Price |
| 40 | Dry grocery | Condiments, Gravies, And Sauces | Meat Sauce; Worcestershire Sauce | Flavor; sauce type | 41% | Salt pepper spices; tomatos x sauce type == “steak”; vinegar | No | | | |
| 41 | Dry grocery | Condiments, Gravies, And Sauces | Sauce & Seasoning Mix-Remaining | Cuisine; flavor; preparation | 31% | Dry onions; salt pepper spices; spices; tomatos; vinegar | Yes | GH | Yes | 1+PC+Price |
| 42 | Dry grocery | Condiments, Gravies, And Sauces | Sauce & Seasoning Mix-Remaining | Bbq; beef; burger; curry; pork; sausage; turkey; wing | 32% | Dry onions; salt pepper spices; spices; tomatos; vinegar | Yes | GH | No | Price |
| 43 | Dry grocery | Condiments, Gravies, And Sauces | Sauce & Seasoning Mix-Remaining Mexican; Sauce Mix - Taco | Hot; other flavor; type of sauce | 28% | Dry onions; salt pepper spices; spices; tomatos x type of sauce == “salsa”; vinegar | Yes | GH | Yes | 1+Price |
| 44 | Dry grocery | Condiments, Gravies, And Sauces | Sauce Mix - Spaghetti | Mushroom; pesto or alfredo | 61% | Salt pepper spices; spices; tomatos | Yes | GH | Yes | 1+PC+Price |
| 45 | Dry grocery | Condiments, Gravies, And Sauces | Seasoning Mix - Chili | Hot; regular; texas | 65% | Dry onions; salt pepper spices; spices; tomatos | Yes | GH+CS | Yes | 1+PC+Price |
| 46 | Dry grocery | Condiments, Gravies, And Sauces | Seasoning Mix - Chili | Hot; regular; texas | 63% | Dry onions; salt pepper spices; spices; tomatos; vinegar | Yes | GH | Yes | 1+PC+Price |
| 47 | Dry grocery | Condiments, Gravies, And Sauces | Seasoning Mix - Sloppy Joe | No msg | 87% | Dry onions; salt pepper spices; spices; tomatos | Yes | GH+H | Yes | 1+PC+Price |
| 48 | Dry grocery | Cookies | Cookies | Bite size; coated; cookie type; flavor; holiday; low fat | 22% | | Yes | GH | Yes | 1+PC+Price |
| 49 | Dry grocery | Gum | Gum-Bubble; Gum-Bubble-Sugarfree; Gum-Chewing; Gum-Chewing-Sugarfree | Bubblegum; flavor; presentation; sugarfree | 38% | Corn sweeteners x sugarfree == 0; resin and synthetic rubber; sugar x sugarfree == 0 | Yes | GH | Yes | Price |
| 50 | Dry grocery | Gum | Gum-Bubble; Gum-Bubble-Sugarfree; Gum-Chewing; Gum-Chewing-Sugarfree | Flavor; sugarfree | 42% | Corn sweeteners x sugarfree == 0; resin and synthetic rubber; sugar x sugarfree == 0 | No | | | |
| 51 | Dry grocery | Jams, Jellies, Spreads | Garlic Spreads | Herbs | 93% | Soybean oil; vegetable oil | No | | | |
| 52 | Dry grocery | Pet Food | Cat Food - Dry Type | Bulk; flavor; kitten; quality | 47% | Corn; other grains; rough rice; slaughter cattle x flavor == “beef”; slaughter poultry x flavor in [“chicken”, “turkey”]; unprocessed finfish x flavor == “fish”; wheat | Yes | GH | Yes | 1+Price |
| 53 | Dry grocery | Pet Food | Dog & Cat Treats; animal = cat | Animal; flavor | 53% | Corn; other grains; rough rice; slaughter cattle x flavor == “meat poultry”; slaughter poultry x flavor == “meat poultry”; unprocessed finfish x flavor == “seafood” | Yes | GH+H | Yes | 1+PC+Price |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| 54 | Dry grocery | Pet Food | Dog & Cat Treats; animal = dog | Animal; flavor | 44% | Corn; other grains; rough rice; slaughter cattle x flavor == “meat poultry”; slaughter poultry x flavor == “meat poultry”; unprocessed finfish x flavor == “seafood” | Yes | GH+CS | Yes | 1+PC+Price |
| 55 | Dry grocery | Pet Food | Dog Food - Dry Type | Bulk; flavor; puppy; quality | 36% | Corn; other grains; rough rice; slaughter cattle x flavor == “beef”; slaughter poultry x flavor in [“chicken”, “turkey”]; unprocessed finfish x flavor == “fish”; wheat | Yes | GH | Yes | 1+Price |
| 56 | Dry grocery | Pet Food | Dog Food - Moist Type; Dog Food - Wet Type; puppy = 0 | Flavor; puppy | 46% | Corn; other grains; rough rice; slaughter cattle x flavor == “beef”; slaughter poultry x flavor in [“chicken”, “turkey”]; unprocessed finfish x flavor == “fish”; wheat | No | | | |
| 57 | Dry grocery | Pet Food | Dog Food - Moist Type; Dog Food - Wet Type; puppy = 1 | Flavor; puppy | 75% | Corn; other grains; rough rice; slaughter cattle x flavor == “beef”; slaughter poultry x flavor in [“chicken”, “turkey”]; unprocessed finfish x flavor == “fish”; wheat | Yes | GH+H | Yes | Price |
| 58 | Dry grocery | Pickles, Olives, And Relish | Pickles - Sweet | Cut; spicy; style | 28% | Cucumbers; sugar; vinegar | Yes | GH | Yes | 1+PC+Price |
| 59 | Dry grocery | Pickles, Olives, And Relish | Relishes | Condiment type; dill; fruity; hot; sweet | 52% | Cucumbers x condiment type == “pickle relish”; sugar x sweet == 1; vinegar | Yes | GH+CS | Yes | 1+PC+Price |
| 60 | Dry grocery | Pickles, Olives, And Relish | Relishes | Condiment type; dill; fruity; hot; sweet | 49% | Cucumbers x condiment type == “pickle relish”; sugar x sweet == 1; vinegar | Yes | GH | Yes | 1+PC+Price |
| 61 | Dry grocery | Prepared Food-Ready-To-Serve | Chicken - Shelf Stable | Chicken type; has broth; white only | 40% | Aluminum; poultry processing; slaughter poultry; wheat x chicken type in [“noodle”, “pot pie”, “dumplings”] | Yes | GH+CS | Yes | 1+PC+Price |
| 62 | Dry grocery | Prepared Food-Ready-To-Serve | Chili-Shelf Stable | Con carne; has beans; hot; meat | 32% | Aluminum; beans city average x has beans == 1; poultry processing x meat in [“chicken”, “turkey”]; slaughter cattle x meat == “regular”; slaughter poultry x meat in [“chicken”, “turkey”] | Yes | GH | Yes | 1+PC+Price |
| 63 | Dry grocery | Prepared Food-Ready-To-Serve | Stew - Beef - Shelf Stable; Stew - Chicken - Shelf Stable; Stew - Remaining - Shelf Stable | Brunswick; contains beef; contains chicken; microwave; regular | 57% | Beans city average x brunswick == 1; poultry processing x contains chicken == 1; sauces; slaughter cattle x contains beef == 1; slaughter poultry x contains chicken == 1; tomatos x brunswick == 1 | Yes | GH+H | Yes | 1+PC+Price |
| 64 | Dry grocery | Shortening, Oil | Cooking Sprays | Butter; oil type; pump | 44% | Dairy x butter == 1; olive oil x oil type == “olive”; soybean oil x oil type == “soybean”; vegetable oil x oil type == “vegetable” | Yes | GH | No | Price |
| 65 | Dry grocery | Snacks | Dip - Mixes | Type of mix | 34% | Dry onions; salt pepper spices | Yes | GH | Yes | 1+PC+Price |
| 66 | Dry grocery | Snacks | Popcorn - Popped; Snacks - Caramel Corn | Flavor; is caramel corn; low fat; low salt | 31% | Cheese x flavor == “cheese”; corn; corn sweeteners x is caramel corn == 1; sugar x flavor == “sweet” | Yes | GH | Yes | 1+PC+Price |
| 67 | Dry grocery | Snacks | Popcorn - Popped; Snacks - Caramel Corn | Flavor; is caramel corn; low fat; low salt | 34% | Cheese x flavor == “cheese”; corn; corn sweeteners x is caramel corn == 1; sugar x flavor == “sweet” | Yes | GH | Yes | Price |
| 68 | Dry grocery | Snacks | Snacks - Potato Chips; Snacks - Potato Sticks | Low fat; low salt | 57% | Russet potatoes; salt pepper spices; vegetable oil; vegetable oil x low fat == 0 | Yes | GH | Yes | 1+PC+Price |
| 69 | Dry grocery | Snacks | Snacks - Potato Sticks | Cut; low fat; low salt; style | 31% | Russet potatoes; soybean oil x style != “baked”; vegetable oil x style != “baked” | Yes | GH | Yes | 1+PC+Price |
| 70 | Dry grocery | Snacks | Snacks - Pretzel | Flavor; is coated; is filled; low fat; low salt; pretzel size | 32% | Peanuts x flavor == “nutty”; sugar x flavor == “sweet”; wheat; wheat flour | Yes | GH | Yes | 1+PC+Price |
| 71 | Dry grocery | Snacks | Snacks - Pretzel | Flavor; is coated; is filled; low fat; low salt; pretzel size | 32% | Peanuts x flavor == “nutty”; sugar x flavor == “sweet”; wheat; wheat flour | Yes | GH | Yes | Price |
| 72 | Dry grocery | Snacks | Snacks - Remaining | Flavor; ingredient; low fat; low salt; snack preparation; snack type | 19% | Beans city average x ingredient == “legume”; corn x ingredient == “corn”; rough rice x ingredient == “rice”; russet potatoes x ingredient == “potato”; wheat flour x ingredient == “wheat grain”; wheat x ingredient == “wheat grain” | No | | | |
| 73 | Dry grocery | Snacks | Snacks - Remaining | Flavor; ingredient; low fat; low salt; snack preparation; snack type | 18% | | Yes | GH | Yes | Price |
| 74 | Dry grocery | Soft Drinks-Non-Carbonated | Water-Bottled | Flavored; single bottle; type of water | 46% | Diesel; plastic; vitamins x type of water == “enhanced” | Yes | GH | Yes | 1+PC+Price |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| 75 | Dry grocery | Spices, Seasoning, Extracts | Meat Marinades & Tenderizers | Asian; citrus or mojo; herb and garlic; mesquite bbq; steakhouse; tenderizer; teriyaki | 32% | Lemons x citrus or mojo == 1; salt pepper spices; spices | Yes | GH | Yes | 1+PC+Price |
| 76 | Dry grocery | Spices, Seasoning, Extracts | Pepper | Black; garlic; ground; lemon; red; whole | 42% | Lemons x lemon == 1; salt pepper spices; spices | Yes | GH | Yes | 1+PC+Price |
| 77 | Dry grocery | Spices, Seasoning, Extracts | Salt - Cooking/Edible/Seasoned | Garlic; kosher; seasoned | 63% | | Yes | GH | Yes | 1+PC+Price |
| 78 | Dry grocery | Spices, Seasoning, Extracts | Seasoning-Dry; spice = All purpose seasoning or blend | Spice | 100% | | Yes | GH | No | Price |
| 79 | Dry grocery | Spices, Seasoning, Extracts | Seasoning-Dry; spice = Chili powder | Spice | 100% | | Yes | GH+H | Yes | 1+Price |
| 80 | Dry grocery | Spices, Seasoning, Extracts | Seasoning-Dry; spice = Cinnamon powder | Spice | 100% | | Yes | GH+H | Yes | 1+Price |
| 81 | Dry grocery | Spices, Seasoning, Extracts | Seasoning-Dry; spice = Garlic powder/granules | Spice | 100% | | Yes | GH+H | Yes | Price |
| 82 | Dry grocery | Spices, Seasoning, Extracts | Seasoning-Dry; spice = Meat seasoning | Spice | 100% | | Yes | GH+H | Yes | 1+Price |
| 83 | Dry grocery | Spices, Seasoning, Extracts | Vegetables - Onions - Instant | Chopped; minced | 75% | Dry onions | Yes | GH+CS | Yes | 1+PC+Price |
| 84 | Dry grocery | Vegetables - Canned | Vegetables-Mixed-Canned | Brand tier; low salt; preparation style; vegetable type | 28% | Beans city average x vegetable type == “bean”; tomatos x vegetable type contains “tomato” | Yes | GH | Yes | 1+PC+Price |
| 85 | Dry grocery | Vegetables - Canned | Vegetables-Peas & Carrots-Canned; Vegetables-Peas-Canned; Vegetables-Peas-Remaining-Canned | Has carrots; has meat; salt; type of peas | 49% | Aluminum; carrots x has carrots; green peas x type of peas != “blackeye”; salt pepper spices x salt | Yes | GH | Yes | 1+PC+Price |
| 86 | Dry grocery | Vegetables And Grains - Dried | Rice - Instant | Instant | 79% | Fertilizer; rough rice | Yes | GH | Yes | Price |
| 87 | Fresh produce | Fresh Produce | Fresh Fruit-Remaining | Baby; banana; gold; organic; pineapple | 63% | | Yes | GH+H | Yes | 1+Price |
| 88 | Frozen foods | Baked Goods-Frozen | Bakery-Bagels-Frozen | Bagel size; has no salt; is low fat; is sliced; is wheat; primary flavor; quality tier; secondary flavor | 32% | Sugar x primary flavor == “sweet”; wheat; wheat flour | Yes | GH | Yes | 1+PC+Price |
| 89 | Frozen foods | Breakfast Foods-Frozen | Frozen/Refrigerated Breakfasts | Breakfast type; carb; has cheese; healthy; is veg; protein | 26% | Eggs x protein == “egg”; poultry processing x protein in [“chicken”, “turkey”]; slaughter cattle x protein == “beef”; slaughter hogs x protein in [“ham/bacon”, “sausage”]; wheat x carb != “other” | Yes | GH+CS | Yes | 1+PC+Price |
| 90 | Frozen foods | Pizza/Snacks/Hors Doeurves-Frzn | Pizza-Frozen | Crust; healthy; microwave; quality; topping | 26% | Cheese; slaughter cattle x topping in [“pepperoni”, “other meats”]; slaughter hogs x topping in [“pepperoni”, “other meats”]; tomatos; wheat; wheat flour | Yes | GH | Yes | 1+PC+Price |
| 91 | Frozen foods | Prepared Foods-Frozen | Entrees - Meat - 1 Food - Frozen | Cheesy; cuisine; has sides; protein; saucy | 24% | Beef and veal x protein == “beef”; cheese x cheesy == 1; poultry processing x protein in [“turkey”, “chicken”]; processed meat; salt pepper spices; slaughter cattle x protein == “beef”; slaughter hogs x protein in [“pork”, “meatball sausage”]; slaughter poultry x protein in [“turkey”, “chicken”] | Yes | GH | Yes | 1+PC+Price |
| 92 | Frozen foods | Unprep Meat/Poultry/Seafood-Frzn | Frozen Poultry | Boneless; chicken; cut; meat | 34% | Poultry processing; processed foods and feeds; slaughter poultry | Yes | GH | Yes | 1+PC+Price |
| 93 | General merchandise | Kitchen Gadgets | Beverage Storage Container | Bottle type; quality; size description; volume oz | 34% | Plastic; stainless steel | Yes | GH | Yes | 1+Price |
| 94 | General merchandise | Stationery, School Supplies | Dry Erase Bulletin Board And Accesory | Board type; has calendar; has cork; is magnetic | 47% | | Yes | GH | Yes | 1+PC+Price |
| 95 | General merchandise | Stationery, School Supplies | Personal Planners Binders And Folders | Product type | 35% | Plastic; pulp paper | Yes | GH+CS | Yes | 1+Price |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| 96 | Health & beauty care | Cosmetics | Cosmetic Kits | Kit type | 42% | | Yes | GH | No | 1+PC+Price |
| 97 | Health & beauty care | Cosmetics | Cosmetics-Eyebrow & Eye Liner | Product type | 71% | | Yes | GH | No | 1+PC+Price |
| 98 | Health & beauty care | Cosmetics | Cosmetics-Foundation-Cream And Powder; Cosmetics-Foundation-Liquid | Foundation type | 55% | | No | | | |
| 99 | Health & beauty care | Cosmetics | Cosmetics-Lipsticks | Product purpose | 45% | Fatty acids; starch vegetable fats oils | Yes | GH | No | Price |
| 100 | Health & beauty care | Cosmetics | Talcum & Dusting Powder | Scent | 66% | Corn starch; talc | Yes | GH+H | Yes | 1+Price |
| 101 | Health & beauty care | Fragrances - Women | Cologne & Perfume-Women’s | Common consumer name; value | 46% | Coal; coal x common consumer name == “body spray”; ethanol; ethanol x common consumer name == “body spray”; other grains; other grains x common consumer name == “body spray”; soybeans; soybeans x common consumer name == “body spray” | No | | | |
| 102 | Health & beauty care | Grooming Aids | Cosmetic And Nail Grooming Accessory | Primary type | 53% | | Yes | GH | No | Price |
| 103 | Health & beauty care | Hair Care | Shampoo-Aerosol/ Liquid/ Lotion/ Powder; Shampoo-Combinations | Dandruff; liquid; regular or other; two in one | 43% | Fatty acids; surfactants | No | | | |
| 104 | Health & beauty care | Medications/Remedies/H Aids | Foot Preparations-Athlete’s Foot | Cream; deodorant; extra strength; liquid; liquid spray; ointment; powder; powder spray | 41% | Basic organic compounds; basic organic compounds x extra strength == 1 | Yes | GH+CS | Yes | Price |
| 105 | Health & beauty care | Men’s Toiletries | Cologne/Lotion-Men’s | Common consumer name | 53% | Coal; coal x common consumer name == “body spray”; ethanol; ethanol x common consumer name == “body spray”; other grains; other grains x common consumer name == “body spray”; soybeans; soybeans x common consumer name == “body spray” | No | | | |
| 106 | Health & beauty care | Men’s Toiletries | Cologne/Lotion-Men’s | Common consumer name; form; value | 41% | Coal; coal x common consumer name == “body spray”; ethanol; ethanol x common consumer name == “body spray”; other grains; other grains x common consumer name == “body spray”; soybeans; soybeans x common consumer name == “body spray” | Yes | GH+H | Yes | 1+Price |
| 107 | Health & beauty care | Skin Care Preparations | Skin Cream-All Purpose | High price tier; scent; target condition; vitamin e | 44% | Fatty acids; starch vegetable fats oils; vitamins x vitamin e == 1 | Yes | GH+CS | Yes | 1+PC+Price |
| 108 | Non-food grocery | Detergents | Detergents - Heavy Duty - Liquid; Detergents - Light Duty; Detergents-Packaged | Scent none | 70% | Surfactants | No | | | |
| 109 | Non-food grocery | Laundry Supplies | Detergent Boosters | Liquid; powder | 67% | Surfactants | Yes | GH+CS | Yes | 1+PC+Price |
| 110 | Non-food grocery | Tobacco & Accessories | Cigarettes | Low tar; menthol | 74% | Pulp paper; tobacco | Yes | GH | Yes | 1+PC+Price |
| 111 | Packaged meat | Packaged Meats-Deli | Bratwurst & Knockwurst; Frankfurters-Refrigerated; Sausage-Dinner | Form; frozen; hot; meat; sausage type; skinless | 14% | Beef and veal x meat contains “(beef\|veal)”; poultry processing x meat contains “(chicken\|turkey)”; processed meat; salt pepper spices; slaughter cattle x meat contains “beef”; slaughter hogs x meat contains “pork”; slaughter poultry x meat contains “(chicken\|turkey)” | Yes | GH+CS | Yes | 1+PC+Price |
| 112 | Packaged meat | Packaged Meats-Deli | Bratwurst & Knockwurst; Frankfurters-Refrigerated; Sausage-Dinner | Form; frozen; hot; meat; sausage type; skinless | 16% | Beef and veal x meat contains “(beef\|veal)”; poultry processing x meat contains “(chicken\|turkey)”; processed meat; slaughter cattle x meat contains “beef”; slaughter hogs x meat contains “pork”; slaughter poultry x meat contains “(chicken\|turkey)” | Yes | GH | Yes | 1+PC+Price |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| 113 | Packaged meat | Packaged Meats-Deli | Lunchmeat-Deli Pouches-Refrigerated | Flavor; form; low fat; low salt; meat type | 27% | Beef and veal x meat type contains “(beef\|veal)”; poultry processing x meat type in [“turkey”, “chicken”]; processed meat; salt pepper spices; slaughter cattle x meat type in [“beef”, “salami pepperoni”]; slaughter hogs x meat type in [“ham”, “salami pepperoni”]; slaughter poultry x meat type in [“turkey”, “chicken”] | No | | | |
| 114 | Packaged meat | Packaged Meats-Deli | Lunchmeat-Sliced-Refrigerated | Flavor; form; low fat; low salt; meat type | 25% | Beef and veal x meat type contains “(beef\|veal)”; poultry processing x meat type in [“turkey”, “chicken”]; processed meat; salt pepper spices; slaughter cattle x meat type in [“beef”, “salami pepperoni”]; slaughter hogs x meat type in [“ham”, “salami pepperoni”]; slaughter poultry x meat type in [“turkey”, “chicken”] | Yes | GH | Yes | Price |
| 115 | Packaged meat | Packaged Meats-Deli | Sausage-Breakfast | Flavor; form; low fat; meat; precooked | 21% | Beef and veal x meat contains “(beef\|veal)”; poultry processing x meat contains “(chicken\|turkey)”; processed meat; salt pepper spices; slaughter cattle x meat contains “beef”; slaughter hogs x meat contains “pork”; slaughter poultry x meat contains “(chicken\|turkey)” | Yes | GH | Yes | 1+PC+Price |

Table C.1: All mergers in our sample. Characteristics explained by principal components always include an indicator for whether the product is a store brand, and the package size. The possible values for “Instruments used” are “GH” (only differentiation instruments), “GH+CS” (differentiation and cost shifter instruments), and “GH+H” (differentiation and Hausman instruments). The possible values for “Demographic interactions” are “Price” (high income indicator interacted with only price), “1+Price” (interacted with price and the constant), and “1+PC+Price” (interacted with price, the constant, and principal components of characteristics).