Common Ownership, Creative Destruction, and Inequality:

Evidence from U.S. Consumers
Abstract

Using over 12 billion observations from the consumer goods industry, we study pricing and non-pricing effects of common ownership at the product level. We find positive elasticity of consumer goods prices to common ownership and that firms pass along price increases to consumers through the marginal cost channel. Our results are robust to using Bartik-like shifters for market shares and common ownership, and variations in common ownership driven by financial mergers. We present the first large-scale systematic evidence on the relationship between common ownership and creative destruction at the product level. Our results show that common owners introduce new products at a faster rate and discontinue existing product items at a similar pace compared to control firms, resulting in greater net variety. Finally, we provide new insights in that the rate of increase in consumer goods prices is higher in product modules catering to lower-income households, whereas consumers in high-income segments benefit more from the increase in product variety due to higher common ownership. These results deliver important parameters for assessing welfare implications to the extent of how different types of consumers benefit from or are hurt by institutional ownership concentration.
1 Introduction

Does the overlapping ownership of same-industry firms by institutional investors—known as common ownership—change the playing field for firms in the industry? How does it affect product innovation? Who benefits from or bears the burden of those changes? The issue of common owners has drawn increasing attention due to the documented increase in common horizontal ownership in the last few decades in the United States (Figure 1).¹ Indeed, economists have long theorized that such common ownership of firms by the same investors reduces incentives to compete vigorously, compared to a situation in which each firm is controlled by separate owners that do not have significant stakes invested in competitors (O’Brien and Salop, 2000; Bresnahan and Salop, 1986). The intuition is straightforward: when product market rivals’ strategic actions impose negative externalities on one another, the benefit of competing aggressively comes at the expense of firms in the same industry and reduces industry profits. Therefore, a common owner could prefer rival firms to make (anticompetitive) choices that maximize the value of its portfolio as opposed to choices that maximize the value of the individual firms.

On the empirical front, the first paper to quantify the anti-competitive incentives arising from common ownership, and to test whether they have a measurable impact on competition, is a study by Azar, Schmalz, and Tecu (2018) of the U.S. airline industry. This paper documents that airfares are higher, and anti-competitive incentives from common ownership are large—an order of magnitude above what would trigger agency concerns in a standard merger investigation. These findings have since ignited a debate on the antitrust risk posed by institutional investors, its legal implications, and potential solutions (Elhauge, 2016, 2019; Posner et al., 2017).² Several other studies have revisited these results and shown that

¹Common ownership pattern occurs when firms across a single industry share the same owners.
²For instance, Posner et al. (2017) advocate a tax solution to the alleged problems of common ownership by institutional investors. They propose that Congress should endorse legislation abolishing the tax advantages available to retirement plans unless those plans “offer only mutual funds that do not own a significant number of shares of more than one firm in a specific industry.” They state that such an approach, under which “mutual funds would be allowed to own shares of only a single firm in any specific industry, but could invest in as many industries as they wanted,” would allow for the sort of inter-industry diversification that protects investors,
the relation that Azar, Schmalz, and Tecu (2018) postulate between common ownership concentration and prices does not hold in the airline industry.

Despite recent scrutiny, whether findings in the airline industry are borne out in other industries remains, surprisingly, an open question and mixed results in the earlier studies highlights the necessity for further empirical work in this area. Our paper contributes to the current debate and provides new evidence on the relevance and importance of common ownership in several dimensions by using a unique product-level data set from the U.S. consumer goods industry. To exemplify the extent of common ownership in the U.S. consumer goods industry, we provide the top five shareholders and their ownership percentages as of the first quarter of 2010 for a sample of consumer goods companies in Table 1. Interestingly, the top shareholders across the major players in our sample are very similar. The mutual fund families Blackrock, Vanguard, and State Street are among the major holders of most of the largest consumer goods companies.

The consumer goods sector is ideal to conduct such a micro-level investigation, because it makes up a significant share (about 30%) of the U.S. gross domestic product, it offers rich data and the concept of a product (i.e., barcode) is unequivocal. The data come directly from the point-of-sale records of over 30,000 pharmacy, grocery, and mass merchandising stores within the United States, obtained through Nielsen Research. The database is massive, in that we have price and consumer information from 85% of the zip codes and counties within the continental United States.

For our empirical tests and similar to the recent work (e.g., Azar, Schmalz, and Tecu, 2018), we use the modified Herfindahl–Hirschman Index (\(MHHI\)). The variable \(MHHI\) is a measure of market power that incorporates the implied levels of market concentration due to common ownership. This is in contrast with the Herfindahl–Hirschman Index (\(HHI\)) traditionally used in assessments of market power concentration, which does not account for while avoiding the intra-industry diversification that may soften competition. Elhauge (2016), instead, has proposed that the federal antitrust agencies should investigate any horizontal stock acquisitions that have created, or would create a modified HHI (or MHHI) over 2500 and change in MHHI (or \(\Delta MHHI\)) over 200, in order to determine whether such acquisitions raised prices or are likely to do so.
common ownership. In the first part of the paper, we show that, in the U.S. consumer goods industry, higher levels of common ownership are correlated with higher consumer goods prices between 2006 and 2016.

However, the positive relation between common ownership and pricing behavior across products does not necessarily have a causal interpretation. In our context, one endogeneity concern is that omitted variables—such as managerial traits, firm-level productivity, latent cost attributes—correlated with both common ownership and the prices set by the firms can make our ordinary least squares (OLS) results spurious. We address the potential bias due to omitted variables by executing fixed effects estimations. We control for latent demand heterogeneity in the cross section through product \( \times \) firm fixed effects and, importantly, for time-varying fluctuations at the firm level through firm \( \times \) time fixed effects. Therefore, our estimation controls for unobservable shocks, such as time-varying demand and input cost fluctuations as well as productivity shocks at the firm level.

This empirical design delivers unbiased estimates if firms and institutional shareholders are matched randomly. However, institutional common owners do not invest randomly; they could opt for investing in companies with particular product market visions. Our empirical strategy to address non-random matching of institutions and firms is based on financial institution mergers as a source of variation for ownership structure in a difference-in-differences (DiD) framework. The reason behind this instrument is that the effect of such mergers should be correlated with common ownership but orthogonal to other parameters that influence consumer goods prices across markets and over time.

Another identification concern is the potential endogeneity of product market shares, which enter into \( MHHI \) calculations and are likely correlated with the error term (because they are a function of price). To eliminate this possibility, we exploit potentially exogenous component of market shares guided by nationwide sociodemographic trends, combined with pre-existing differences in the age–income profiles of expenditure for different product modules. The rational behind this Bartik-like instrumental variable strategy is that sociode-
mographic changes should be exogenous to other determinants of product pricing (Acemoglu and Linn, 2004). We find that a naive empirical approach that refrains from instrumenting for market shares and from accounting for non-ownership shocks at the product-firm level may spark off a bias in our findings that may lead to inaccurate inferences.

By construction, an increase in consumer goods prices must be triggered by either an increase in marginal costs or an increase in markups. In that regard, we next decompose the increase in product-level consumer goods prices into various channels along the market chain. One impediment to such an investigation is that, despite the granular level of detail, we do not observe marginal costs or markups in Nielsen data. Instead, we capture wholesale prices using data from National Promotion Reports’ Price-Trak database (via Promodata). Promodata gathers its information from the largest grocery wholesaler in each market and offers a representative sample of entire market as a result of the (anti-price discrimination) Robinson–Patman Act of 1936.

These data provide the wholesale markups, prices charged by wholesalers to retailers, and wholesale marginal costs, together with relevant product characteristics such as items and pack sizes. After merging Price-Trak’s wholesale data with Nielsen’s retail data, we are able to examine four different channels for changes in consumer goods prices: (1) wholesale markups, (2) retail markups, (3) wholesale marginal costs, and (4) retail marginal costs. Our results reveal that relationship between common ownership and retail prices is channeled largely through marginal cost variation.

In the second part of the paper, we examine the role of common ownership as a conduit for innovation. The question of innovation is of central importance because the pace of product innovation is one of the main engines of economic growth, productivity, and income inequality (Grossman and Helpman, 1991; Aghion and Howitt, 1992; Aghion et al., 2018). The nature of our data allows us to investigate the relevance of common ownership for innovation at the product level. Studying at the micro level instead of the firm level circumvents concerns that

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3For instance, Aghion et al. (2019) show that innovativeness accounts for, on average, across U.S. states, around 17% of the total increase in the top 1% income share between 1975 and 2010.
aggregation up to the firm level cause a sizable loss of information about the process of creative destruction, especially if entry and exit mostly occur within existing firms (Holderness, 2016). We show that an increase in common ownership leads to the introduction of new products at a faster pace and to the discontinuation of existing products at a similar pace relative to control firms, which results in greater net product variety.

In the final part of the paper, we examine who might be hurt by or benefit from higher concentrations of cross-ownership by institutional shareholders. More specifically, we turn our attention to households and assess how price changes in product markets, as well as changes in product variety, could differentially impact households along the income distribution. After all, households of different income levels demand very different types of goods and services and are expected to be differently influenced by price changes and product reallocation. Furthermore, focusing only on average effects can mask considerable heterogeneity across the socioeconomic spectrum.

We split sample households into three income brackets (high, medium, low) and build income group–specific product prices using detailed scanner data (at the barcode level), enabling us to examine consumption patterns along different groups of income. This setting also gives us room to control for household demographics and variations in the demand or cost of production at the product level through latent product × firm × time shocks. Remarkably, we find price effects to be more acute in products catering to lower-income households, indicating pricing inequality (i.e., purchasing power inequality) across consumer groups. Moreover, we find that the rate of product creation is elevated for higher income consumers compared to lower income consumers and that the patterns of product destruction are somewhat uniform across products, irrespective of consumer income.

Our work complements emerging literature on common ownership in several respects. First, we provide new evidence on price elasticities to common ownership in the consumer goods industry using a new product-level dataset. The novelty of our empirical analysis comes from the uniqueness of the retail data. Comparing prices within narrowly defined markets
is essential to mitigating concerns that latent product demand shocks could be correlated with ownership shocks. Second, to our knowledge, this is the first study to apportion price changes into changes in markups and marginal costs. Third, our micro-level data also enable us to relate common ownership to a key element of the Schumpeterian creative destruction mechanism: product creation and destruction. This is the major channel through which the creative process has an influence on the productivity, growth, and consumer welfare (Dunne, Roberts, and Samuelson, 1988; Davis and Haltiwanger, 1992). Finally, our study is unique in the literature to highlight the contrasting effects of increased common ownership on consumer segments across income distribution.

We organize the paper as follows. Section 2 discusses the background to the research question and describes the data and sample construction. Section 3 presents the empirical strategy and baseline results on product pricing. Section 4 considers causality and identification and presents the empirical results. Section 5 extends the analysis to product innovation. Section 6 analyzes the heterogeneity of results across the income distribution. Section 7 concludes the paper.

2 Background and data description

2.1 Background

There is a vast theoretical literature on the potential consequences of common ownership. For example, common owners could pressure managers to internalize externalities among their portfolio firms (Easterbrook and Fischel, 1982; Rubin, 2006), which can cause anticompetitive actions (e.g., Bresnahan and Salop, 1986; O’Brien and Salop, 2000; Gilo, Moshe, and Spiegel, 2006; López and Vives, 2019). Gordon (1990) and O’Brien and Salop (2000) predict that common ownership pushes rivals towards monopolistic outcomes.

The O’Brien–Salop (2000) model of partial ownership, which is based on the model of Bresnahan and Salop (1986), assumes a setting of Cournot competitors in which a firm acquires
stock in a competing firm (a partial horizontal merger). In this cross-ownership scenario, the firm that acquired shares in a competitor will consider how its strategic competitive behavior impacts the value of the holdings it owns in the competing firm. Maximizing an objective function that includes not only the firm’s value but also the value of its holdings in the competing firm leads to industry markups that are proportional to a modified Herfindahl–Hirschman Index (\(MHHI\)). This \(MHHI\) measure is calculated as \(MHHI=HHI+MHHI\) \text{} \textit{delta}, where \(MHHI\) \textit{delta} captures the extent to which the competitors are connected by common ownership and control links.

Although the empirical literature on the relation between common ownership and industry competition is still in its early stage, it has provided mixed results. For instance, Azar, Schmalz, and Tecu (2018) quantify the anticompetitive incentives arising from common ownership in the U.S. airline industry and test whether they have a measurable impact on competition. The authors find that anticompetitive incentives from common ownership are strong (that ticket prices are up to 12% higher on the average airline route than would be the case under separate ownership), an order of magnitude above what would trigger agency concerns in a standard merger investigation. In related research, Azar, Raina, and Schmalz (2016) study bank competition in local deposit markets across the United States. They show that changes in the degree to which banks serving a particular U.S. county are commonly owned are positively correlated with changes in account fees and fee thresholds for various deposit products and negatively correlated with deposit rates. Moreover, a recent multi-industry study by Gutiérrez and Philippon (2017) finds that underinvestment relative to investment opportunities in the United States is largely driven by industries with high market concentration and ownership by quasi-indexers, that is, common ownership.

The findings of Azar, Schmalz, and Tecu (2018) have led to proposals to restrict institutional investing through regulatory constraints (Posner et al., 2017) and increased antitrust enforcement (Elhauge, 2016)\textsuperscript{4}. In addition to the implications for financial regulation and

antitrust, some commentators linked common ownership to issues as important as income inequality. López and Vives (2019) show that increases in common ownership increase R&D and output when technological spillovers are large. However, unlike the study of Azar, Schmalz, and Tecu (2018), Kennedy et al. (2017) and Dennis, Gerardi, and Schenone (2018) find no significant evidence that common ownership increases airline prices. Liang (2016) and Antón et al. (2016) find that managers of commonly owned firms are paid more for rival performance, whereas Kong (2016) finds the opposite. He and Huang (2017) argue that common ownership can reduce information asymmetry and possible expropriation among rivals considering collaboration. Lewellen and Lowry (2018) find little evidence that common ownership affects firm behavior. Koch, Panayides, and Thomas (2018) perform an industry-level analysis and show that common ownership does not increase output prices or reduce non-price competition in firms’ advertising or capacity decisions. By investigating brand’s daily stock returns around settlement and the timing to sell the drugs by generic manufacturers who accepted a settlement offer, Xie and Gerakos (2018) conclude that institutional cross-holdings facilitates collusion between incumbent and entrant in the U.S. pharmaceutical industry. Kini, Lee, and Shen (2019) find that anti-competitive effect of common ownership is weaker in less concentrated industries and industries with similar products/technologies. Finally, Boller and Morton (2019) show that product market rivals of firms that experience increased common ownership experience greater abnormal returns, consistent with common owners causing a softening of competition.

Given the diversification and other merits generated by institutional investors, a key question is whether the anticompetitive concerns rationalize regulatory measure that jeopardize the benefits. However, mixed results highlight the need for more empirical work in this space.

antitrust implications of cross-ownership in horizontal competitors by large institutional shareholders. The authors suggest that “the government should enforce the Clayton Act against institutional investors while recognizing a safe harbor for those that either take a small stake in an oligopolistic industry (less than 1 percent of each company) or invest in no more than one company per industry.”
2.2 Data description

2.2.1 Nielsen panel data

Our primary source of consumer goods price data is the Nielsen Research and data consist of two components: (i) Store Panel (“Retail Scanner”) and (ii) Homescan Consumer Panel. As seen in Table A.1 in the Appendix, the size distribution of firms is fat tailed. The top 10 firms alone make up for roughly 28% of the total revenue.

In the database, the most granular notion of a product is a 12-digit number known as the Universal Product Code (UPC), which uniquely identifies a product. For example, Coca-Cola 12-ounce cans have a different barcode than Coca-Cola two-liter bottles. Nielsen also offers product attributes for each barcode, such as the “brand”, a comprehensive product-type description that Nielsen identifies as a “product module”, and a more aggregated product-type description that Nielsen identifies as a “product group”. For example, Coca-Cola 12-ounce cans are sold under the Coca-Cola brand in the Regular Cola module within the carbonated beverages product group and the dry grocery product department.

Note that a single brand can span multiple modules. Table A.2 in the Appendix displays an example of the organizational hierarchy of the Nielsen data. In the raw sample, there are 10 broad departments (Nielsen product modules that represent the different product categories across departments: alcoholic beverages, frozen food, beauty, packaged meat, dry grocery, dairy, fresh produce, health, non-food, and general merchandise); 125 more comprehensive product groups (oral hygiene, laundry supplies, baking mixes, tools, hosiery, household supplies, appliances, etc.); 1,075 very comprehensive product modules (sugar-free chewing-gum, light beer, laundry treatment aids, etc.), and over 3 million items (UPCs). In addition, a vast majority of firms are well diversified: 27% of firms have a single product, 23% of firms own multiple products in a single module, 15% firms have multiple product modules in a single product group, 20% of firms own multiple product groups in a single department, and 18% are multi-department firms.
Store Panel (Retail Scanner)

The retail scanner panel records store × week × product price and sales data for products sold over 30,000 stores in the United States over the period 2006–2016. The panel has over 12 billion unique store × week × UPC level observations, and covers more than 50% of the total sales of U.S. grocery and drugstores and more than 30% of the sales of U.S. mass merchandisers. Nielsen offers a comprehensive product hierarchy, depending on where the products are sold in stores. The data cover a wide spectrum of retailers, such as grocery stores (e.g., Safeway), supercenters (e.g., Walmart) as well as convenience stores (e.g., 7-Eleven), club stores selling in bulk (e.g., Costco), and drugstores (e.g., Walgreens). Each record involves a store chain identifier and a store identifier so that a given store can be tracked across time and mapped into a specific chain. Although each chain is assigned a unique identifier, there is no direct way to link the chain identifier to the name of the chain. The type of stores in the database range from food stores, drug, mass merchandising stores to liquor, and convenience stores. A total of 97% of the sales in the data come from mass merchandising, food, and drug stores. Each product (in the store panel) has a unique UPC identifier, and each entry in the database involves the number of units sold of a given product barcode and the average quantity-weighted unit price of purchases at a given store and week. The database contains only products with strictly positive sales in a given store–week and precludes certain products since they have no barcode assigned.

The initial sample includes 1.4 million different products for a total average yearly revenue (as recorded in the Nielsen data) of $224 billion. Finally, the geographic coverage of the database is extensive, including stores from all states (and the District of Columbia) except for Alaska and Hawaii. Similarly, the database tracks stores from 371 MSAs (metropolitan statistical areas), containing both the zip codes and Federal Information Processing Standard Publication (FIPS) codes for the store’s county.

Homescan Consumer Panel

The raw data include the purchase decisions of about 60,000 households that made over
500 million transactions in the course of more than 90 million shopping trips between 2006 and 2016. To gather these data, households are instructed to use an optical reader in their homes to scan the barcodes of each product that they purchased during shopping trips to supermarkets, mass merchandisers, convenience stores, drugstores, and club stores. For each purchased product, we observe the transaction date, UPC, identifier for the chain in which the transaction took place, price paid, and several characteristics of the product itself, including the brand name and pack size. The purchases cover over 3.2 million unique UPCs (unique product identifiers) from 1,075 product modules. Using the 2016 Consumer Expenditure Survey, we find that the homescan products cover about 30% of average annual household spending. Homescan households report information on demographic attributes such as race, occupation, income, age, and education of the household head. In addition, the survey provides information about home ownership, household composition, and geographic location of households.

Table A.3 in the Appendix shows the distribution of consumer expenditures along the main product groups that exist in the Nielsen scanner data. While most of the aggregate spending is allocated to food items, an extensive range of product groups are also covered by the dataset. Table A.4 states the fraction of spending for each store type in the Nielsen dataset. Grocery and discount stores account for about 62% of annual spending, while this ratio is only 4% for hardware, home improvement, and electronics stores. Nielsen also provides data on online shopping, which accounts for 3% of the annual retail spending in the dataset.

2.2.2 Ownership and GS1 data

To construct the common ownership network MHHI delta for each product–year–quarter, we retrieve the 13F mandatory institutional holdings reports compiled by the Thomson Reuters Spectrum database (formerly known as the 13F CDA Spectrum 34 database). Under Section 13(f), any institution that exercises investment discretion over accounts of at least $100 million must file Form 13F with the SEC on a quarterly basis, and report the cash flow rights
We aggregate the Nielsen data to the quarterly frequency to match them with the 13F filing intervals. Following Azar, Schmalz and Tecu (2018), we restrict the sample of institutional owners to those holding at least 0.5% of a firm’s shares in any given year-quarter. Cash flow (ownership) rights are computed as all shares owned (voting and non-voting) from Form 13F divided by total shares outstanding (from the Center for Research in Security Prices). We calculate the control share (voting rights) for shareholders as the percentages of sole and shared voting shares of a firm held by an institutional owner.5,6

To merge the Thompson Reuters 13F database with the Nielsen database, we need to assign each UPC to its manufacturer. The UPC standard is overseen by the GS1 organization and manufacturers can buy from GS1 the usage right to a UPC company prefix that corresponds to the first six to nine digits of the UPC codes of its products. Firms are required to disclose their name and address when buying a company prefix. Mapping the Nielsen UPCs to a company prefix using the GS1 U.S. Data Hub yields a match rate of 97% and 37,219 unique firm names.

Next, we link Nielsen-GS1 merged data with Thomson Reuters 13F database through company names. Essentially, we use a text matching code to match the Nielsen and GS1 firm names, and visually inspect every match to ensure accuracy. Later, we apply a fuzzy matching algorithm to the unmatched firms, and we manually check pairs identified in the fuzzy matching with high scores based on their company information to certify that they are true matches- the same firm or a division/subsidiary-parent pair. We also require each

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5There are two types of shared investment discretion: shared-defined and shared-other. The shared-defined types arises with bank holding companies and their subsidiaries, investment companies and their sub-advisers, and insurance companies and their separate accounts. Even though investment discretion is shared in these types of relations, voting authority is still reported as sole. Shared-other investment discretion pertains to any situation that is not shared-defined. Form 13F implies that, for these types of shared investment discretion situations, voting authority is reported as shared.

6Following Dennis et al (2018), we also construct $H_{MHHI} delta$ also by setting control rights equal to the number of shares reported as having sole voting rights. Our results (untabulated) remain qualitatively and economically significant.
firm-product module to have a series of at least four consecutive quarters to assure that our product level common ownership measure is calculated using a meaningful amount of information. Finally, we clean for data recording errors by excluding data entries for which the purchasing price for a UPC is more than three times or less than a third of the median unit-price of any UPC within the same product module.

Our matched sample represents 26,182 stores in 8,282 zip codes, and 55% of the total revenue in the Nielsen database. Tables 2–4 present summary statistics for our merged sample. The HHI values are calculated based on the product module shares of firms and average about 3,626 across markets and over time. Table 2 reveals that the value of products that exist in the current year that were not around in the previous year accounts for 7.2% of expenditures, whereas the value of disappearing UPCs is much smaller, about 3.3%. The finding that the creation is greater than destruction implies that market shares are systematically shifting away from existing products towards new products.

Table 3 shows that, on average, the majority of household heads are over 35 years old, white, middle income, at least a high-school graduate, possibly with some college education, and employed full-time. Note, however, that about 28% of the panel contributors are not employed. The percentage of household income used for spending—expenditure to income—is 26% for low-income households and only 5% for high-income households in our sample. Table 4 presents summary statistics of both the Compustat data and our matched sample for a number of firm attributes. Our merged dataset represents publicly traded firms well in many dimensions.

3 Empirical strategy

3.1 Baseline specification

While raw data are sampled weekly in the Nielsen panel, we construct a quarterly product-level price index, since this allows us to make the time unit comparable to that of ownership
data and eliminates high-frequency noise. Let \( j \) be the firm, \( t \) be the quarter in a given year, \( m \) the product module, and \( u \) an individual UPC. We denote the expenditure share of item \( u \) in product module \( m \) at time \( t \) as \( \omega_{u,m,t} \), which captures the willingness of households to substitute across products. More specifically, products that are highly substitutable for others could receive a much lower weight than their average expenditure share. The weight is very close to the simple average expenditure share for products whose expenditure shares hardly move in reply to a price change. Then, the product-level price index (i.e., price inflation) can be expressed as follows:

\[
\tilde{p}_{m,j,t} = \prod_{u \in \Omega_{mjt}} \left( \frac{P_{u,m,j,t}}{P_{u,m,j,t-1}} \right)^{\omega_{u,m,j,t}}
\]

(1)

where \( \Omega \) is the set of UPCs in product module \( m \) produced by firm \( j \) at time \( t \). Note that, as it is written, this index updates expenditure shares every quarter, accounting for substitution across product modules, and the assumption of a constant elasticity of substitution utility function is theoretically justified (Sato, 1976; Vartia, 1976). Note that across time we track the price of identical items, thus concerns about comparing non-identical products or changes in quality are inconsequential for our results. More specifically, if a low-quality product is switched with a higher-quality product with a higher price, such product replacement does not affect our price index since variations in quality will usually be associated with new UPCs.

Now, consider the following general characterization of the price index for a firm \( j \)’s product \( m \) at time \( t \), \( \tilde{p}_{m,j,t} \):

\[
\tilde{p}_{m,j,t} = \tilde{p}_{m,j,t} (\cdot, \text{ MHHI delta}_{m,t})
\]

(2)

The first argument represents the determinants of product prices other than common ownership and the second argument, \( \text{MHHI delta}_{m,t} \), characterizes the additional concentration triggered by common ownership at the product level, which is defined as:

\[
\text{MHHI delta}_{m,t} = \sum_j \sum_{k \neq j} s_{m,j,t}s_{m,k,t} \frac{\sum_i \gamma_{ij,t} \beta_{ik,t}}{\sum_i \gamma_{ij,t} \beta_{ij,t}}
\]

(3)
where $s_{mj,t}$ is the market share of firm $j$ in product $m$ at time $t$, $\beta_{ij,t}$ is the ownership share of shareholder $i$ in firm $j$ at time $t$, $\gamma_{ij,t}$ is the control share of firm $j$ exercised by shareholder $i$, and $k$ indexes firm $j$’s competitors at time $t$. Note that we compute $MHHI$ at the product level and each quarter in a given year. Figure 2 shows the time-series behavior of the product-level average $MMHI$ and the average $HHI$ index in the consumer goods sector during the period 2006:1–2016:4. By construction, the difference between these two lines is the portion of market concentration that is generated by common ownership ($MHHI$ delta). The average $HHI$ value shows a stable trend over this period, while we observe a clear increasing pattern in $MHHI$ and $MHHI$ delta. The average of $MHHI$ delta is around 500 at the beginning of our sample period, but it increases to 1,700 by the end of 2016.

Returning to equation (2), we are interested in estimating the price elasticity of common ownership: $\eta=\frac{\partial \bar{p}_{m,t}}{\partial MHHI \Delta HHIm,t} \times \frac{MHHI \Delta HHIm,t}{p_{m,j,t}}$. Formally, we estimate ownership–price elasticity, $\eta$, using the following empirical model:

$$\ln \bar{p}_{m,j,t} = Z_{m,t}' \beta + X_t' \phi + \gamma_1 \ln HHI_{m,t} + \eta \ln MHHI \Delta HHIm,t$$

$$+ \nu_{mj} + \tau_{jt} + \varepsilon_{m,j,t}$$

Meanwhile, $\beta$ is a vector of unknown parameters and $\varepsilon_{m,j,t}$ is the error term. $\nu_{mj}$ accounts for unobserved heterogeneity of product $m$ produced by firm $j$, and $\tau_{jt}$ accounts for time-varying firm-specific shocks.

The first component captures, for example, the firm $j$’s knowledge of the market for product $m$. The second component captures time-varying firm-specific shocks, such as latent productivity and demand dynamics, cost attributes, and managerial traits. The vector $Z_{m,t}$ is comprised of demand-driven product-level controls, including a constant, product scope, the product sales growth rate, and market size for product $m$; $X_t$ is a vector of aggregate demand controls such as the GDP per capita, the unemployment rate, changes in the housing price index, wage growth, and population growth. Note that we do not include firm-level controls.
in (4), because they are all absorbed by the firm \times time fixed effects.

3.2 Baseline results

The results of the OLS estimation of equation (4) are presented in Table 5. Throughout the paper, standard errors are double clustered by year-quarter and product to account for correlations across time and products. Column (1) utilizes only HHI and \textit{MHHI delta} measures as the covariates (i.e., \( Z = 0 \) and \( X = 0 \) in (4)), with cross-sectional (product \times firm) fixed effects and time-varying firm fixed effects, while column (2) presents the estimation results of the full specification of (4) by adding time-varying product-level and aggregate demand controls. In column (3), we replace firm \times time fixed effects with time-varying firm controls as listed in Table 4 and year-quarter fixed effects, which absorbs all demand shocks at the aggregate level.

Column (1) of Table 5 shows that the OLS estimate for \textit{MHHI delta} is positive, which is significant at the level of 1 percent. Because the measure of common ownership and product-level price index are both taken in logs, we can interpret the coefficient on innovation as an elasticity. The point estimate of common ownership drops slightly when we include for aggregate and product-level controls in the subsequent specification and mirrors this finding when we replace time-varying firm fixed effects with firm-level controls and time fixed effects in the subsequent column. The finding that the estimated OLS coefficients are similar in magnitude in all three models underscores the stability of the statistical and economic relationships.

Table 5 also shows that other demand proxies have a significant effect on price variation, other things held fixed. We use an expenditure-based measure of market size, constructed using actual prices paid \( (\hat{P}_{m,j,t}^h) \) and quantities purchased \( (q_{m,j,t}^h) \) by consumer \( h \) from the Homescan Consumer Panel. In particular, we calculate the market size for product \( m \) in year-quarter \( t \) as \( \sum_j \sum_h \hat{P}_{m,j,t}^h \times q_{m,j,t}^h \). While the coefficients on \textit{Product market size} and \textit{Product sales growth} are significant, we find \textit{Product scope}—defined as the number of
UPCs within a product module $m$—to be insignificant. Unemployment rate has a negative coefficient and other aggregate demand factors such as population growth, the housing price growth rate, and the GDP per capita have positive coefficients and—except for population growth—they are all statistically significant at different levels.

In sum, the analysis in Table 5 supports the theoretical prediction (Bersnahan and Salop, 1986; Salop and O’Brien, 2000) and empirical findings of Azar, Schmalz, and Tecu (2018), that common ownership can lead to higher prices. However, the positive relation between common ownership and pricing behavior across products does not necessarily have a causal interpretation. In the next section, we address causality concerns.

4 Identification tests

Identifying the causative effects of common ownership on pricing and non-pricing outcomes is generally challenging. For example, common ownership structure may be spuriously correlated with other factors that influence pricing and non-pricing product market outcomes. In general, two potential endogeneity issues arise from our empirical approach: (i) the endogeneity of market shares and (ii) the endogeneity of ownership structure. In this section, we address the endogeneity of market shares exploiting a Bartik style instrument, and the endogeneity of common ownership in a quasi-natural experiment.

4.1 Endogeneity of market shares: Bartik style instrument

The first potential source of endogeneity stems from the product market shares that we use to compute both the traditional $HHI$ and $MHHI$ _delta_ measures; that is, market shares are a function of price and are likely correlated with the error term. Following Acemoglu and Linn (2004), we overcome this problem by exploiting variations in (expenditure-based) market size driven by _nationwide_ shifts in income and age distributions, which should be potentially exogenous to other determinants of product-level price margins. This approach
is analogous to the classic Bartik research designs, except that industries are replaced with sociodemographic groups and locations are replaced with product categories.\textsuperscript{7}

We use the Nielsen Homescan Consumer Panel and construct the age–income spending profiles of consumers in each product module. Specifically, we consider the interactions of nine age groups and twelve income groups, leaving us with 108 age–income (sociodemographic) groups, denoted by $G$. We define $N_{G,t}$ as the change in the number of U.S. consumers in each age–income group $G$ at time $t$, and $\bar{e}_{m,j,G}$ as the (per-capita) expenditure share of product module $m$ (produced by firm $j$) for each age–income group $G$ in a base year (where we choose base year to be the start of our sample period, 2006). Then, our (sociodemographic-based) Bartik style instrument for market share is:

$$s_{m,j,t}^{\text{Bartik}} = \frac{\sum_G \bar{e}_{m,j,G}^t N_{G,t}}{\sum_j \sum_G \bar{e}_{m,j,G}^t N_{G,t}}$$

(5)

In this setting, $\bar{e}_{m,j,G}$ quantifies the pre-existing exposure of each product module $m$ (manufactured by firm $j$) to the national shifts in $N_{G,t}$ (i.e., change in U.S. population for different age–income consumer groups $G$). It is important to note that the over-time source of variation in this measure is purely from \textit{nationwide} shifts in the number of consumers in each socioeconomic group, captured by $N_{G,t}$ (i.e., $\bar{e}_{m,j,G}$ is not time–varying).\textsuperscript{8} Subsequently, changes in prices and product quality, which may result from innovations and affect consumption patterns, will not cause over-time variation in market shares (Acemoglu and Linn 2004).

The exclusion restriction rests on the fact that $\bar{e}_{m,j,G}$ are predetermined at the time of

\textsuperscript{7}The shift-share instruments are well-accepted in a broad range of literature. For instance, Bartik (1991) interacts local industry structure with national shifts in employment across different industries to identify local labor demand shocks. Shift-share instruments have also been used to identify exogenous variation in local public spending (Wilson 2012; Nakamura and Steinsson, 2012), credit supply (Greenstone, Mas, and Nguyen, 2015), portfolio allocation (Calvet, Campbell, and Sodini, 2009), foreign aid (Nunn and Qian, 2014), and robotization (Acemoglu and Restrepo, 2017). Acemoglu and Linn (2004) use demographic variables to predict the evolution of market size for different drugs, taking into account the usage pattern across age groups in the population.

\textsuperscript{8}Changes in the US age–income distributions during our sample period produce large variation in market size along the product spectrum. Naturally, some products have clear age profiles (for instance, baby formulas), some have distinct income profiles (for instance, farm-raised seafood and wild seafood) and some have strong age-by-income profiles (for instance, beer and high-end liquor).
the demographic shifts which are exogenous to each product. If the $\bar{e}_{m,j,G}$ (initial spending patterns of various age-income groups across product spectrum) are not stationary across the product spectrum, then the empirical model will not have enough power. Left panel of Figure 3 (where each dot denotes five percent of the data) gives a visual representation of the regression which is implemented using the the fraction of firms across 1,075 product modules. It suggests that the initial weights are robust predictors of future weights.

The first-stage of this empirical design is simply to regress actual product level market shares on $s_{m,j,t}^{Bartik}$ in (5) and all other covariates and fixed effects as listed in in column (3) of Table 5. The strength of the relationship between Bartik instrument and common ownership is best seen graphically using the binned scatter plot. Figure 3 (right panel where each dot denotes five percent of the data) shows that the Bartik instrument exhibits strong first-stage results and easily passes weak instrument tests. The estimated coefficient and $R$-squared value indicate the strong predictive power of our sociodemographic measure of market shares. If the explanatory power in the first stage is weak, then this is a cause for concern (Staiger and Stock, 1997; Baum, Schaffer, and Stillman, 2003). Staiger and Stock (1997) suggest a simple rule of thumb where, in the presence of a single endogenous regressor, the instrument is deemed to be weak if the first-stage $F$-statistic is less than 10. For our regressions, the value of the $F$-statistics varies between about 287 and 319.

The results from the second-stage estimations are displayed in Table 6. In columns (2) and (3), we also instrument for the control variable Product market size (used in vector $Z_{m,t}$ in equation (4)) using $\sum_j \sum_G \bar{e}_{m,j,G} N_{G,t}$. Overall, the estimates on the price elasticity to common ownership is economically smaller in magnitude and statistically less significant than those reported in Table 5.

### 4.2 Endogeneity of ownership

Unobservable firm characteristics such as managerial traits, productivity, latent costs, and demand attributes could also be correlated with both common ownership and potential prod-
uct market outcomes, leading to spurious results. So far, we have controlled for all latent heterogeneity in the cross section through product × firm fixed effects and, importantly, for time-varying fluctuations at the firm level through firm × time fixed effects.

However, this empirical setting delivers unbiased estimates if firms and institutional shareholders are randomly matched. Thus, another potential concern is that ownership is not randomly assigned and the institutional holdings of a given firm’s equity could be driven by factors that are correlated with certain product market prospects. Besides utilizing fixed effects, a second pillar of our empirical design comprises instrumenting for common ownership through a difference-in-differences framework (Azar, Schmalz, Tecu, 2018; He and Huang; Lewellen and Lowry, 2018). In this setting, our estimation procedure compares the change in the individual product prices of firms whose ownership structure is affected by a financial merger with product prices of firms whose common ownership structure is not. Table A.5 provides a list of institutional mergers during our sample period. More specifically, we divide products into treatment and control groups, as follows. First, we calculate product-level MHHI delta in the quarter before the merger was announced. We then calculate a counterfactual product-level MHHI delta for the same period t and product m, the only difference being that we treat the holdings of merging institutions as being already held by a single entity. We calculate the difference between the counterfactual and actual MHHI delta values for each product and assign products in the top tercile of this difference to the treated group and products in the bottom tercile to the control group. Now our estimating equation (4) becomes:

\[
\ln p_{m,j,t} = \alpha_1 Treated_m + \alpha_2 Post_t + \eta Treated_m \times Post_t \\
+ Z_{m,t}' \beta + X_{t}' \phi + v_{mj} + \tau_{jt} + \varepsilon_{m,j,t}
\]

where \( v_{mj} \) is the product × firm fixed effect, \( \tau_{jt} \) is the firm × time fixed effects, \( Treated_m \) is an indicator variable if product \( m \) is in the treated group, \( Post_t \) is an indicator for the
post-merger period, \( Z'_{m,t} \) is the vector of product-level controls, and \( X'_t \) is a set of aggregate demand controls as in (4). We continue to instrument for product \( m \)'s market size at time \( t \) (in vector \( Z_{m,t} \)), since market size is a function of price and is likely correlated with the error term (see Section 4.1). As before, we double cluster the standard errors by product and year–quarter to account for the correlations across time and products.

We exploit a symmetric eight-quarter (two-year) window surrounding the merger event (i.e., four quarters before and four quarters after the institutional merger), although the results (untabulated) based on 10- and 12-quarter windows are broadly similar. The coefficient estimates of \( Treated \times Post \) across columns (1)–(3) of Table 7 indicate that treated firm products, relative to control firm products, experience greater increase in average prices during the period after the institutional merger than during the period before the merger. Comparing this coefficient with those in Table 5, we see that results for the effect of common ownership on product prices are substantially smaller. In column (6), we replace firm \( \times \) time fixed effects with time (year-quarter) fixed effects and time-varying firm characteristics as listed in Table 4. In this case, \( Post \) itself will not be identified—and is excluded from the regression—because its is subsumed by the time fixed effects. We continue to find that the economic and statistical significance for \( Treated \times Post \) is commensurate with the results in column (4) of the same table.

The DiD methodology relies on the validity of parallel trends assumption, which tests for the equality of pre-treatment trends across treatment and comparison groups. Under the null that there are no pre-trends the \( \eta \) in specification (8) should be zero. Similar to Almeida et al. (2012), we, first, directly compare price growth trends between firms in the treatment and control samples during two years before the merger event. Our results show no significant variations. Next, we perform a placebo test, where we use the same array of treatment and control firm-products classified earlier and analyze their average price growth throughout a time window around the pseudo event year–quarter. The model specification is similar to those in Table 7 except that we use pseudo event dates. As seen in Table A.6 in the Appendix,
the coefficient estimates for $Treated \times Post$ are statistically insignificant, confirming parallel trends in product prices across the treatment and control firms before the exogenous merger events.

Alternatively, we estimate another DiD specification where we interact the $Treated$ dummy and control variables with year $\times$ quarter dummy for 10 quarters before and after the announcement of treatment (i.e., announcement of a merger event between financial institutions). In this estimation design, we have

\[
\ln \tilde{p}_{m,j,t} = \sum_{l=-10}^{+10} \eta^{l} Treated_{m} \times Post_{t}^{l} + \sum_{l=-10}^{+10} Z_{m,l}^{l} \beta^{l} \times Post_{t}^{l} + \sum_{l=-10}^{+10} X_{0}^{l} \phi^{l} \times Post_{t}^{l} + v_{mj} + \tau_{jt} + \varepsilon_{m,j,t},
\]

where $Post_{t}^{l}$ marks the periods ($l$ quarters) before or after the merger event quarter. Figure 4 displays the estimated coefficients $\eta$. It is evident that, before the event quarter, the difference between the treated and control groups is, on average, zero, which validates the parallel trends assumption. There is an increasing pattern after the merger announcements and the sign of the coefficients is positive, which supports our earlier findings.

4.3 Markups or the pass-through of marginal costs?

In this section, we consider why increases in common ownership may lead to higher consumer prices. By definition, product prices can be split into markups $\mu$ and marginal costs $c$, ($P_{u,m,j,t} = c_{u,m,j,t} + \mu_{u,m,j,t}$). Although identifying either channel would be appealing, we do not observe marginal costs or markups in the Nielsen data. Instead, we use data from Price-Trak (via Promodata), which provides wholesale price information for packaged consumer goods from grocery wholesalers. The data are representative and sourced from one major wholesaler—whose identity is not disclosed for confidentiality reasons—in each market. Since Promodata includes a less complete array of wholesalers and markets than Nielsen, the wholesale price\footnote{To be clear about terminology, we refer to the price charged by manufacturers—such as Procter & Gamble Company—to wholesalers as the wholesale price, the price charged by retailers to consumers as the retail price.} data coverage is considerably lower. Data are available for 32 of the 50 retail markets and the time period covered differs by market, resulting in an unbalanced panel of...
observations. Furthermore, data are usually available only for the leading products in each market.

Price-Trak includes wholesale list prices, as well as wholesale marginal costs and markups, deal allowances or off-invoice items offered to the retailer by the wholesaler, brand, description, pack, size, and the allowance date between 2006 and 2012 at the UPC level. However, the 2012 data lose a significant amount of coverage. We therefore perform our tests excluding 2012 data from our sample. Of these variables, we are interested in wholesale list prices, markups, marginal costs and deal allowances. We merge the Nielsen and Price-Trak datasets by UPCs based on the weekly date. That is, the Promodata prices are those associated with the week containing the Nielsen week ending Saturday. The periods correspond closely for a Nielsen retailer using a seven-day period ending on Saturday; however, this is not the case for all retailers. For a retailer using a Thursday to Wednesday week, the Nielsen prices would predate the Price-Trak prices by a few days.

We analyze four channels through which the pricing impact of higher common ownership passes from consumer goods companies (manufacturers) to consumers: (1) wholesale markups, (2) retailer markups, (3) wholesale marginal costs, and (4) retailer marginal costs. We follow the previous literature (Hong and Li, 2017, Gopinath et al., 2011) and assume that the wholesale prices net of any allowance paid by retailers constitutes a good proxy for the retailers’ marginal costs. The rationale behind this assumption is simple: the wholesale cost of goods sold comprises the vast majority (about 82%) of a typical retailer’s marginal cost and it is more difficult to break up the remaining 18%. However, the bulk of those costs stand for a fixed overhead (such as corporate salaries and utilities) instead of costs that vary directly with sales. For example, Kroger Company’s 2014 10-Q reports the cost of goods sold at $855.2 million, as opposed to operating and administrative expenses (which involves sales, general, administrative, and all other operating expenses) of $100.3 million.$^{10}$ We calculate

$^{10}$For the typical wholesaler, the share of cost of goods sold in operating and administrative expenses is even higher. For example, Amcon Distributing Company’s 2010 10-Q reports the cost of goods sold at $603.6 million, as opposed to operating and administrative expenses (which involves sales, general, administrative, and all other operating expenses) of $657.4 million.
the retail markup as the difference between the retail price charged to consumers (using Nielsen data) and the retail marginal cost.

Subsequently, we build a marginal cost index $\tilde{c}_{m,j,t}$ and a markup index $\tilde{\mu}_{m,j,t}$ for each product $m$ at both the retailer and wholesaler levels, using the approach described in equation (1):

$$\tilde{c}_{m,j,t} = \prod_{u \in \Omega_{m,j,t}} \left( \frac{c_{u,m,j,t}}{c_{u,m,j,t-1}} \right)^{w_{u,m,j,t}}$$

and

$$\tilde{\mu}_{m,j,t} = \prod_{u \in \Omega_{m,j,t}} \left( \frac{\mu_{u,m,j,t}}{\mu_{u,m,j,t-1}} \right)^{w_{u,m,j,t}}$$

where $\Omega$ is the set of UPCs in product module $m$ produced by firm $j$ at time $t$. We then employ the DiD specification in (6) using the markup index and the marginal cost index as our dependent variables. This approach allows us to test directly whether common ownership shocks are passed on to consumers through changes in markups or marginal costs or both.

The results are reported in Table 8 and columns (1) through column (4) mirror the specification in column (5) of Table 7. In the first two columns, we find no evidence that common ownership shocks are correlated with retailer or wholesale markups; although we find a positive relation between wholesale markups for the treated firms, the correlation is not statistically significant. In columns (3) and (4), on the other hand, we find that higher common ownership is associated with higher wholesale and retail marginal costs, which are statistically significant. The magnitude of the DiD estimates is economically greater for wholesale marginal costs than for retail marginal costs. Overall, we find a correlation to marginal costs but not to markups, which indicates that the increase must be in manufacturer prices to wholesalers, which in turn increases wholesale prices to retailers. This only makes economic sense because market shares, $HHI$ and $MHHI$ are defined by manufacturers who make certain product modules. There would thus be no reason to think they would bear any relation to whether wholesalers or retailers have any market power to inflate prices further.

Although the cost of goods sold constitutes the largest portion of marginal costs in consumer goods products, we acknowledge the other cost pass-through channels: commercial rent costs and labor costs. If the shadow cost of labor escalated, for instance due to higher
wage and salaries, consumer goods prices could increase, since wholesalers and consumer goods companies pass on this minor component of marginal cost to customers (Stroebel and Vavra, 2019). However, we control for changes in unemployment rate and average weekly wages and salaries in our estimations and our findings suggest that our results are robust to the pass-through of labor costs. We further control for commercial rent costs at the aggregate level through appreciation in the commercial property price index (CPPI) and the commercial real estate rental index (CRERI) and our tests yield nearly identical results (untabulated but available upon request). Overall, we conclude that firms pass along price increases to consumers through higher marginal costs.

5 Product creation and destruction

Given that incentives to compete are closely connected to incentives to innovate (Bloom et al., 2013; Aghion et al., 2005), we further explore how ownership structure impacts firms’ incentives to innovate. Does an increase in common ownership increase short-termism, undermining innovation efforts? Or does it reassure managers, making them more willing to “strike for the fence”? We study this question by thoroughly investigating a key element of the Schumpeterian creative destruction process: product entry and product exit. Kostovetsky and Manconi (2018) offer evidence that common ownership can be conduits for the diffusion of information about new technologies between firms. However, studying at the product level instead of the firm level circumvents concerns that aggregation up to the firm level cause a loss of critical information about the process of creative destruction, especially if entry and exit mostly occur within existing firms (Holderness, 2016).

The literature on competition and innovation provides conflicting evidence and different mechanisms that might be at play. For example, Aghion et al. (2005) argues that competition leads to increases in innovation through an escape-the-competition effect. This evidence is supportive of theories which predict that firms endogenously determine their R&D to create
barriers to entry. In contrast, Autor et al. (2016) find that competition reduces innovation and argue that a simple mechanism in which competition induces manufacturing firms to contract their operations along multiple margins of operation explains their results. Finally, Gu (2016) argues that R&D projects are more likely to fail in a more competitive environment, because rival firms could win the innovation race. This could mean that the returns to innovation and product market differentiation are higher in a less competitive environment.

A vital part of our empirical analysis is the identification of product creation and destruction. We take advantage of the panel structure of data to identify the creation and destruction periods for each product. Furthermore, we follow earlier studies and use the UPC as the key identifier. We do so because it is rare for a significant quality change to occur without a UPC change. A possible drawback of this approach is that a new UPC might not necessarily characterize a new item. For example, Chevalier, Kashyap, and Rossi (2003) note that some UPCs can be suspended only to have the same item resurface with a new UPC. Fortunately, this is not the case in our dataset, because Nielsen uncovers these UPCs and designates them their previous UPC.

Since one cannot distinguish whether an item new to the store is actually new to the customer—instead of a store panel (retail scanner)—we utilize consumer panel data. In particular, we define the creation and destruction rates for product $m$ by firm $j$ between times $t$ and $s$ as follows:

\[
\text{Destruction rate}_{m,j}(t,s) = \frac{\sum_u \text{Value of disappearing } \iota_{u,m,j}(t,s)}{\text{Total value } \iota_{u,m,j}(s)} \tag{7}
\]

\[
\text{Creation rate}_{m,j}(t,s) = \frac{\sum_u \text{Value of new } \iota_{u,m,j}(t,s)}{\text{Total value } \iota_{u,m,j}(t)} \tag{8}
\]

where $\iota_{u,m,j}$ indexes each item (UPC) $u$ in product module $m$ by firm $j$. The creation rate is the value of new products entered in period $t$ relative to period $s$ as a share of the aggregate value of all products bought in period $t$. Note that a new product arrives in period $t$ but it was not part of the consumption bundle of any consumer in previous period $s$. Analogously,
the destruction rate is the value of products exited in period \( t \) relative to period \( s \) as a share of the aggregate value of all products bought in period \( t \). Instead of a value-weighted creation rate, we also look at the ratio of new items to total number of items at time \( t \) and define the destruction rate similarly, as the ratio of disappearing items to total number of items at time \( s \). Our findings (untabulated) show similar trends. We study the scope of product destruction and creation for the case in which \( t \) and \( s \) are 4 and 8 quarters apart on a rolling basis but report the results only for the former. Finally, we define *Net creation rate* as the difference between the creation and destruction rates and *Turnover rate* as the sum of the creation and destruction rates.

We adopt the same approach as in (6) but, this time, we use product creation, destruction, net creation, and turnover as the dependent variables. Columns (1) and (2) of Table 9 show that the coefficient of \( Treated \times Post \) is positive and significant for *Net creation rate* and *Turnover rate*. The increase in *Net creation rate* can be achieved by introducing new products and/or dropping fewer products. Columns (3) and (4) show that this is done by introducing new products to the market while keeping the same rate of product discontinuation. Overall, these findings suggest innovation benefits offered by common ownership due to information sharing and synchronized R&D strategies among firms with common owners.

In a separate paper, López and Vives (2019) show that in highly concentrated industries the limited number of participants narrows the scope of technological spillover. Thus, the relation between common ownership and product innovation can be weakened by industry concentration. To gauge the validity and economic relevance of this argument, we augment the baseline estimates, in columns (1) through (4), with the HHI interacted with \( Treated \times Post \). The estimates in Table 8 do not seem to support this view.

Items with a strong seasonal cycle do not appear every quarter and, by construction, we treat them as destruction for our sample. To prevent this from affecting our measures of product turnover and net creation, we undertake two more exercises: (1) we exclude items that do not appear every quarter after being introduced, because such exclusion mainly affects
seasonal products, and (2) we redefine the periods $t$ and $s$ as years, since it is extremely unusual for an item to have positive sales in one period and followed by zero sales the next period, and then to exhibit positive sales again the following period, which implies that product destruction and creation are not being caused by products floating in and out of the sample. We find that less than 1.5% of the items are items that resurface after being eliminated. In dollar terms, these items comprise less than 0.2% of the sample. When we run our tests after we exclude products that reappear after a period of leaving our sample from our computation or after we redefine the periods $t$ and $s$ as years, the findings are effectively unchanged.

6 Differences across income groups

So far, we have shown small positive price elasticity and large product innovation effects through common ownership concentration in the consumer goods industry. Our measure of price changes has focused exclusively on variation in product bundles and has assumed that all consumers pay the average price for each product. However, price changes can reflect differently across consumers because they buy different bundles of goods or pay different prices for the same goods. Therefore, the average effect conceals substantial heterogeneity across the socioeconomic spectrum. An important distributional question surrounding common institutional ownership shocks is whether and how the average pricing and innovation effects vary across the income spectrum. Although the emergent literature has focused on the price effects of common ownership, much less attention has been given to if and how the entry of new products and price fluctuations in product markets and could differentially affect households along the income distribution.

As mentioned earlier, one distinct feature of the Nielsen Homescan Consumer Panel is the availability of actual prices paid, the quantities purchased by the households, and the demographic characteristics of those consumers. Exploiting this unique aspect of our dataset,
we track consumer spending patterns and revise (1) for different income groups as follows:

\[ \tilde{p}_{m,j,t}^h = \prod_{u \in \Omega_{m,j}^h} \left( \frac{P_{u,m,j,t}^h}{P_{u,m,j,t-1}^h} \right)^{\omega_{u,m,j,t}^h} \]

where \( \Omega_{m,j}^h \) is the set of UPCs in product module \( m \) produced by firm \( j \) and purchased by consumers in income group \( h \) at time \( t \). Note that we now let the prices paid for a product, \( P_{u,m,j,t}^h \), and the expenditure share weights of our price index, \( \omega_{u,m,t}^h \), be income specific. By tracking households’ actual expenditures, we move away from the standard assumption that each income group faces the same price increases for any particular product at each point in time. Moreover, this approach enables us to estimate empirical specifications with a full array of product \( \times \) firm \( \times \) time indicators and account for non-ownership shocks—for instance time-varying productivity and product demand shocks. We can also incorporate \( \psi_m^h \) fixed effects for demand heterogeneity across consumers in different income groups. We argue that this feature of our estimation approach is crucial: a naive estimation—without controlling for time-varying non-ownership shocks at the product–income level—could result in biased estimates that can lead to misleading inferences, both quantitatively and qualitatively. Accordingly, we revise the DiD model (6) as follows:

\[
\ln \tilde{p}_{m,j,t}^h = \alpha_1 Treated_m + \alpha_2 Post_t + \eta Treated_m \times Post_t \\
+ \eta^h Income_t^h \times Treated_m \times Post_t \\
+ Z_{m,j,t}' \beta + X_{t}' \phi + W_t^{hp} \gamma + \tau_{mjt} + \psi_m^h + \varepsilon_{m,j,t}
\]

The vectors \( X_t' \) and \( Z_{m,t} \) include the same set of aggregate- and product-level controls, respectively, as in equation (4). Now, we also control for a number of consumer characteristics in each income group through \( W_t^{hp} \), such as age, income, marital status, gender, education, labor market status, and household size. We split households in our sample into three income brackets: the high-income bracket includes households income over $100K a year, whereas the
The presence of age effects is essential, since it separates experience effects from the link between age and consumption through life cycle effects, such as precautionary motives and increasing risk aversion with age (e.g., Caballero, 1990; Carroll, 1994), or diminishing liquidity constraints after retirement (e.g., Gourinchas and Parker, 2002). The controls for demographics and labor market status allow for the effect of these factors on the intertemporal allocation of expenditures (Attanasio and Browning, 1995; Blundell, Browning, and Meghir, 1994).

The setting in (9) facilitates the aforementioned unobservable product × firm × time shocks, \( \tau_{mjt} \), which absorb any fluctuation in the demand for product \( m \) produced by firm \( j \). Here, \( \eta^h \) is the coefficient of interest, capturing price sensitivity to common ownership across different income groups. It is evident from Table 10 (column (1)) that the estimated ownership elasticities of price for households in different income brackets, \( \eta^h \), differ markedly and the price impact of common ownership resides heavily in the low-income bracket. More specifically, holding product demand heterogeneity within each income group constant, through \( \psi^h_m \), the estimated elasticity coefficient is higher for low-income households, which earn less than $25K per year, relative to households earning more than $100K a year, significant at the 5 percent level.

Next, we revise the measures of product innovation—turnover, creation, and destruction—across income groups by separating out the consumption bundles among high- and low-income households. The idea is simple: a household can benefit from or be hurt by the entry of new products or the exit of existing products only if these products are in the household’s consumption bundle. We identify a product as entering the market if it was not part of low- or high-income consumption bundles in period \( s \) but entered in period \( t \) (where \( t > s \)) and, similarly, we characterize a product as exiting the market if it was a part of low- or high-income consumption bundles in period \( t \) but disappears in period \( t \) (where \( t > s \)). Accordingly, the product creation rate is the expenditure share of consumers in the high- and low-income bracket comprises those earning less than $25K, and the middle-income bracket includes the remainder of the household sample.
groups on new products (see equation (8)) and the destruction rate is the expenditure share of consumers in the high- or low-income consumer groups on existing products (see equation (7)). Now, we let \( \eta_{u,m,j}^h \), product creation and destruction rates be income specific:

\[
\text{Destruction rate}_{m,j}^h(t, s) = \sum_{u \in \Omega_{mjt}^h} \frac{\text{Value of disappearing } t_{u,m,j}^h(t, s)}{\text{Total value } t_{u,m,j}^h(t)} \tag{10}
\]

\[
\text{Creation rate}_{m,j}^h(t, s) = \sum_{u \in \Omega_{mjt}^h} \frac{\text{Value of new } t_{u,m,j}^h(t, s)}{\text{Total value } t_{u,m,j}^h(t)} \tag{11}
\]

where \( \Omega_{mjt}^h \) is the set of UPCs in product module \( m \) produced by firm \( j \) and purchased by consumers in income group \( h \) at time \( t \). All the estimates in columns (1)–(4) of Table 10 come from models comparable to Table 7 (column (5)) for product price index, and Table 9 (columns (2)–(4)) for product innovation. We observe that consumers in high-income segments benefit more from increases in product variety due to higher common ownership and the difference is statistically significant. Overall, the findings are suggestive of a large amount of heterogeneity across the economic spectrum in the effects of increasing common ownership, which has not been emphasized in earlier work.

7 Conclusion

The conceptual foundation in the literature is that horizontal shareholdings can reduce the incentives of horizontal competitors to compete with each other. Empirically, while a recent study has shown how common ownership contributes to anticompetitive firm behavior in the airline industry (e.g., Azar, Schmalz, and Tecu, 2018), a few others suggest the opposite (Kennedy et al., 2017). Given the diversification and other merits offered by institutional investors, a key question is whether anticompetitive concerns rationalize regulatory measures that jeopardize the benefits. However, mixed results urge the need for more empirical work in this space.

Utilizing a unique micro panel dataset of U.S. consumer goods companies between 2006
and 2016, we analyze the pricing and non-pricing effects of common ownership at the product level. Two potential endogeneity issues arise from our empirical approach: (i) the endogeneity of market shares and (ii) the endogeneity of ownership structure. Our data allow us to control for product and time-varying latent demand effects at the firm level, which facilitates the identification of the effects of common ownership through several fixed effects. Regardless, to account for possible endogeneity in market shares, we implement a Bartik-style estimator with instruments obtained from the sociodemographic characteristics of consumers. We find that a common ownership is associated with a small increase in average consumer goods prices.

To eliminate the idea that ownership structure is endogenous—that is, some investors correctly anticipate demand and price changes in certain products and buy stakes in view of such anticipated demand—we implement a quasi-natural experiment of mergers by large asset managers. Results from the DiD estimates indicate that price elasticity to common ownership remains positive but its magnitude implies that common ownership–induced shocks account for a small portion of price changes in our sample. We decompose price changes into markups and marginal costs and show that the pricing impact of common ownership travels through the marginal cost channel.

Apart from pricing effects, this paper is the first in-depth study of common ownership in the consumer goods industry and its implications for product creation and destruction. Using a DiD analysis—and in comparison with a counterfactual world in which firms are separately owned—we find that common owners introduce product items (UPCs) at a faster rate and discontinue existing product items at a similar pace compared to control firms, resulting in greater net variety. Finally, we find strong evidence that the rate of increase in consumer goods prices is greater in product groups catering to lower-income consumers, whereas consumers in high-income segments benefit more from the increase in product variety due to higher common ownership. These results provide important parameters for assessing welfare implications in terms of how different types of consumers benefit from or are hurt by
institutional ownership concentration.
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