Cross-Platforms Merger Effects*

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Abstract

Mergers and acquisitions tend to affect the prices and varieties offered by the merging firms. However, most of the existing research considers mergers between firms that interact on the same platform, such as between two online firms, or two firms on the same physical platform. To our knowledge, there is no empirical research on the price effects of integration across different platforms. Such cross-platform mergers likely have substantially different impacts on prices because indirect network effects are much weaker for physical firms than those that interact in low-cost environments having long-tail effects due to lower search costs and fewer constraints on physical inventory. We investigate this problem by analyzing the effects of an acquisition of a national grocery chain by a large online retailer in the United States. Our study differs from prior studies on mergers and acquisitions as the incentives to merge involve not only the usual market power and efficiency arguments, but accessing stronger, indirect network externalities as well. Because the decision to merge is endogenous, identifying merger effects is empirically difficult. We use a doubly-robust causal inference method to address this problem, and we find an evidence of a decrease in price levels in 8 out of 10 treated markets.

Keywords: retail, platforms, mergers and acquisitions, price effects, causal inference

JEL: D43, L410, L5, M2

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1 Introduction

A small number of large grocery chains dominate the U.S. grocery retailing industry. Historically, supermarkets tend to expand in order to either strengthen their market position, to lower prices through efficiencies of scale, to enhance the quality and choices of products available to consumers, or some combination of these motives. Expansions in grocery retailing generally occur through organic growth by physical (that is, bricks-and-mortar) retailers, such as with Walmart's move into food retailing in the 1990s and 2000s (Jia (2008); Holmes (2011); Arcidiacono et al. (2020)), and the growth of hard-discounters more recently (Hokelekli, Lamey, and Verboven (2017); Chenarides et al. (2021)). However, mergers and acquisitions across physical and e-commerce retailers, called *cross-platforms mergers* in this paper, are potentially more important due to the rapid growth of online food retailing during the COVID-19 pandemic. Understanding the welfare effects of such cross-platform integration is therefore critical as online and physical retailers operate in very different ways (Brynjolfsson and Saunders (2010)). In this paper, we examine the impact of a merger between a major online and a physical retailer on grocery price levels.

Ex post analysis of mergers and acquisitions is valuable as there is no theoretical consensus on expected outcomes from any merger or acquisition. On one hand, a merger can increase productive efficiency through better economies of scale, shared assets, and better resource allocation (Perry and Porter (1985); Yan et al. (2019); Asker and Nocke (2021)). As a result, consumers can ultimately benefit if firms pass-through these cost-efficiencies in the form of lower prices and better quality. On the other hand, a combination may also enhance the market power of the integrated firm and subsequently induce higher prices, less variety, and reduced consumer surplus (Hosken, Olson, and Smith (2018); Chen and Gayle (2019); Johnson and Rhodes (2021)). Because of this general lack of *a priori* expectations, empirical analysis is necessary. Despite the theoretical trade-off between market power and merger-induced synergies, there is little empirical evidence on the net impact on consumers which invites more retrospective merger evaluations (Asker and Nocke (2021)).[1] Therefore, *ex post* econometric analysis of mergers and acquisitions is essential, particularly for mergers across different platforms, due to new complexities of modern digital markets.

Physical and online retailers differ in fundamental ways, so there is no reason to expect mergers between firms on different platforms to have the same effects as among own-platform mergers (Brynjolfsson and Saunders (2010); Evans and Schmalensee (2013)). Both physical and online retailers are two-sided platforms, and hence subject to network effects, as consumers demand variety (number of suppliers) and suppliers demand distribution (number of customers) (Richards and Hamilton (2013)). Cross-platform mergers relax restrictions on indirect network externalities placed on physical retailers, as the universe of suppliers and consumers both allow for greater consumer demand and broader distribution for potential suppliers. Because online platforms are therefore likely subject to stronger network externalities than physical platforms, the welfare effects from integration across platforms are likely accentuated, with stronger price and variety effects than would otherwise be the case (Belleflamme and Peitz (2018); Correia-da Silva et al. (2019)).

Economic literature suggests that a cross-platforms merger can generate substantial positive price effects, as the merged entity has the potential to gain market power through various mechanisms. First, if the size of cross-group externalities favors consumers, as they benefit more from an access to a larger group of suppliers than vice versa, the platform tends to set higher equilibrium platform prices for consumers compared to suppliers (Rochet and Tirole (2003); Armstrong (2006); Weyl (2010)). Second, empirical merger studies mostly find limited productivity gains while frequently indicating higher market power, as evidenced by higher markups (Ashenfelter, Hosken, and Weinberg (2013); Blonigen and Pierce (2016); Miller and Weinberg (2017)), despite the existence of a theoretical trade-off between mar-

[1] While there are market concentration measures, such as the Herfindahl-Hirschman Index (HHI) or four-firm concentration ratio (CR4), HHI is found to be a useful guide only to screen mergers if they can harm consumers, and both measures depend critically on how the relevant product market is defined, so it is difficult to predict the general effects of a merger or an acquisition *a priori* (Roberts (2014); Nocke and Whinston (2022)).

ket power and efficiency enhancement (Farrell and Shapiro (1990)). Third, merged entity in the digital space has an increased ability to access more long-tail brands (Brynjolfsson, Hu, and Simester (2011)), thus creating a diverse portfolio of regular and niche products, and cementing its market position. Fourth, cross-platforms merger can result in technological enhancements, more efficient omnichannel operations, and greater inventory capacity by leveraging physical stores as both warehouses and retail spaces (Tang, Chen, and Raghunathan (2023)). The merging firms can attain greater market power through these various channels, potentially resulting in higher prices.

However, a particular characteristic of food retailing can limit the actual exercise of market power in retail mergers: multihoming. First, retail food shoppers tend to "multihome" in the sense that they shop at multiple stores, and not just one, which limits the potential market power of any single merged entity (Zhang, Gangwar, and Seetharaman (2017); Farronato, Fong, and Fradkin (2023); Teh et al. (2023)). The multi-homing behavior and diverse consumer choices in regards to grocery shopping make small and niche products collectively generate substantial sales despite individual low popularity, a long-tail effect witnessed in digital platforms (Brynjolfsson, Hu, and Simester (2011)), so any one online platform is unlikely to gain significant market power from a merger. In our setting, while the merger could provide the acquired chain with enhanced capabilities through access to online retailer's vast customer base and technological advances, the physical chain has a small contribution to market concentration, so one can expect the highly competitive online grocery retail landscape to mitigate any market power effects (Nocke and Whinston (2022)). With these arguments in the backdrop, we argue that empirical estimates of cross-platform mergers should find very different results from mergers between two physical firms.

Others study previous waves of growth, expansion, and dominance in the retailing industry, but there is relatively little on integration across platforms (Hanner et al. (2015); Hosken, Olson, and Smith (2018); Argentesi et al. (2021); Yao (2021); Rickert, Schain, and Stiebale (2021); Stiebale and Szücs (2022)). For example, Hanner et al. (2015) show that expansions or contractions, instead of a new firm's entry or an existing one's exit, are responsible for changing grocery market dynamics. Others provide empirical evidence of a positive association between pre-merger concentration and rivals' (i.e., non-merging firms') markups (Stiebale and Szücs (2022)), and pre-merger concentration and market prices (Hosken, Olson, and Smith (2018); Rickert, Schain, and Stiebale (2021)) following a merger. Besides changes in market power and products, merging firms adjust their product offerings to avoid cannibalization and to mitigate local market competition (Argentesi et al. (2021)). All of this evidence, however, focuses exclusively on brick-and-mortar retailers.

Due to both advances in technology and the demand for convenience more generally, however, online grocery retailing has attracted a substantial share of consumers, and only promises to grow as younger, more technology-focused consumers grow in importance (Statista (2023a); US-Cenus-Bureau (2023); McKinsey (2023)). Understanding of the welfare effects of consolidation efforts involving online platforms is therefore essential. Among the limited amount of research that considers mergers among online physical firms, Castro (2020) studies cross-platform integration like us but focuses on how delivery cost structures and switching costs impact consumer welfare following the acquisition. Second, Yao (2021) considers online-offline merger of two auction marketplaces for used heavy equipment and evaluates the welfare effects of changes that facilitated search across platforms. While the former leaves open the effect on prices, the latter focuses on different industry than ours and does not estimate price effects. This is the focus of our work.

Firms invest in digital-shopping and delivery capabilities in order to develop large-scale online marketplaces, next-day or even same-day deliveries and expanding online assortments both through direct investment or through mergers. All of these investments aim to enhance firms' online footprints and to meet consumer demand of a seamless omnichannel retailing experience (Grocery-Dive (2019); Food&Power (2020); Timoumi, Gangwar, and Mantrala (2022)). Perhaps through a combination of necessity and opportunity, e-commerce in grocery retailing accelerated during the COVID-19 pandemic. In 2020, online retailing contributed some 84.2% of the growth in the US retail sector. More generally, 2022 e-commerce sales account for 21.2% of all retailing, up from 5.2% in 2012 sales (US-Cenus-Bureau (2012); Conley (2023)).

Because of the rapid adoption of internet-powered technologies over the past decade, many argue that "New Retail", a model that combines online and brick-and-mortar channels and has advantages of the best of both worlds, would be the future of retailing (Forbes (2022a); Shopify (2022); Tang, Chen, and Raghunathan (2023)).[2] The New Retail model represents a fundamental change in retailing as in this new paradigm, physical stores serve not only as sales outlets but also as warehouses for online channels. While traditional retailers have been slow in adopting the New Retail, completely omnichannel model, online retailers have been quicker to move into physical retailing. For instance, e-commerce giants Alibaba and Amazon established physical chains 'Hema' and 'Amazon Go' in 2015 and 2016, respectively. Other examples include the expansion of Digital Native Brands like Bonobos (clothing), Faguo (shoes), Glossier (beauty products), and Allbirds (clothing and shoes) into physical locations (McKinsey (2021)). As the New Retail model comprises the aspects and benefits of both physical and online channels, the sheer scale of the necessary capital investment and steep learning curve means that it is not likely to emerge from organic growth from only one channel but rather by mergers or acquisitions across different retail channels. This latter case is the one we seek to examine, specifically why online retailers would seek to integrate with an "older" retailing technology - one that they seemed to want to leave behind.

The question of whether and how mergers and acquisitions by dominant firms affect economic outcomes (that is, prices and assortments) in local markets is of vital interest to antitrust authorities (Shapiro (2019); White (2022)). In addressing this issue, Shapiro (2019) argues for more stringent regulations on mergers, especially for incumbent technology firms,

[2] In 2016, former Alibaba CEO Jack Ma introduced 'New Retail' as a strategic priority for e-commerce, defined as an integrated retail model combining offline and online channels, logistics, and data to enhance the customer experience (Forbes, 2017).

to prevent them from engaging in anti-competitive behavior that excludes competitors from the market. While such concerns are of paramount importance for safeguarding competition and aggregate welfare, online platforms present a set of new and unique challenges from an analytical perspective. For example, traditional antitrust analysis does not consider strong network effects, the impact of technology on search costs and search behavior, and often requires a precise definition of a market, all of which present a distinct set of challenges to antitrust authorities and call for a possibly different outlook on M&As involving online platforms (White (2022)). Especially in settings with strong network externalities, as in large digital platforms, White (2022) argues against a popular remedy of breaking them up as a means to restore competition as platforms of the same size would emerge after some time due to network effects, and instead argues mandating 'interoperability' within digital platforms in the same industry.[3][4] As the antitrust economics of mergers involving digital platforms continue to evolve, it is particularly important to conduct retrospective analysis of firm-level integration involving digital platforms.

Announcement of the merger inspired a considerable amount of public concern. Because incumbent online firms are now many times larger, both in terms of market capitalization and potential reach than all but the largest physical retailers, the degree of angst reflected in media reports was perhaps to be expected. Reduced competition, slower rates of innovation, monopolization, and higher prices were the primary concerns (FTC (2017); Shephard (2017); Khan (2017); Forbes (2017)). In this paper, we seek to study whether journalistic expectations met with reality, at least in terms of a smaller set of outcomes.

We attempt to study some of these public concerns by examining the price effects of a digital-physical merger. To this end, we merge data from three sources – Council for Community and Economic Research (C2ER) Cost of Living Index, Chain Store Guide (CSG),

[3]Some politicians have called for breaking-up Amazon, Google, Apple, and Facebook as a means to promote competition (New York Times, 2019)

[4]A requirement of interoperability is an idea to mandate a large platform to interconnect with other platforms in the same industry. For instance, requiring Google to interconnect with other search engines can foster market entry of new platforms and enhance innovation despite the strong network effects of Google.

and the American Community Survey (ACS, U.S. Census Bureau) between 2015 and 2019. Our measure of retail prices is from the C2ER Cost of Living Index, which is a price index of products across 6 different categories like food, housing, and transportation "to compare the cost of maintaining a standard of living appropriate for moderately affluent professional and managerial households" for consumers in US urban areas (C2ER-COLI (2017)). Here, we focus on the price index of 26 grocery items that Hosken, Olson, and Smith (2018) use to study the relationship between retail market concentration and the price-effect of mergers between physical grocery retailers. Prices in the C2ER index represent items found in baskets of typical consumers visiting COLI stores. Second, we match this price data with data on grocery market shares from Chain Store Guide, which publishes measures on the sales performance of grocery retailers and their competitors across geographic markets in the US. Our final dataset from ACS allow us to control for MSA-level demographics like income, population size, and the percentage of residents living in poverty. Studying merger effects *ex post* is challenging due to the fact that the mergers themselves are endogenous (Gowrisankaran (1999); Hosken, Olson, and Smith (2018)), and merger effects are likely heterogeneous over affected markets and across time. Intuitively, firms merge in order to achieve the sorts of benefits researchers seek to estimate, so the assignment to treatment and control markets is not random and we need some other exogenous source of variation to identify the true effect of the merger.

We use a *doubly-robust* econometric method, augmented inverse probability weighting (AIPW) estimator, to estimate the effects of the platforms' integration on price index of 26 grocery items across treated markets (Robins, Rotnitzky, and Zhao (1994); Glynn and Quinn (2010); Athey and Wager (2019)). AIPW estimator has several key advantages in our setting over other competing estimators. First, the estimator balances covariate distributions across treatment and comparison groups by weighting each unit's outcome based on its propensity score (that is, its probability of being in a treatment group), addressing bias from endogenous market selection in the merger. In the process, it nests a binary regression model to compute the propensity score followed by a regression model to compute the outcomes. Second, it is doubly-robust in that it gives unbiased estimates even when only one of either propensity score model or the outcome regression model is correctly specified, thus giving researchers an extra degree of freedom for possible model misspecification. Third, we can compute the price effects at each treated *market*, unlike difference-in-differences method which can only estimate the treatment effects at the level of a treatment *group* and does not recover unit-level treatment effects. This is particularly important for us as we are interested in not just group-level effects but also market-level effects as one can expect effects to be heterogeneous given heterogeneity in observable attributes (e.g., market-level HHI, population size, household income, and median housing price). Finally, AIPW produces comparable or lower mean-square-error estimates than standard inverse probability weighting (IPW) methods, making it superior to just IPW or outcome regression models (Glynn and Quinn (2010)). These reasons give a strong econometric credence to our choice of AIPW estimator.

We find an evidence of significant price effects across many treated markets due to the cross-platforms merger. First, our difference-in-difference estimator finds no significant price effects on the treated group in any period beyond the post-acquisition. However, as mentioned earlier, this approach does not pin down heterogeneity across the units in the treatment group, which are treated MSAs in our case. AIPW estimator finds that price level drops in 8 out of 10 merger-affected MSAs, and the result is robust to varying specifications in both propensity score and outcome regression models. Our results suggest that the general concern about market power effects from the acquisition was almost completely unfounded. That is, there was no evident price increases from the acquisition, either immediately or within two years of the announcement and implementation of the event. This is perhaps not surprising: given the acquired chain's small market share and minimal impact on HHI-based market concentration, the merger was unlikely to harm consumers through unilateral effects, despite the acquiring firm's strong online presence, an argument which aligns with Nocke and Whinston (2022).

Our study makes three contributions. First, we provide empirical insights on the price effects of mergers across physical and online platforms. To the best of our knowledge, this is the first study that analyzes price effects following a merger or acquisition between physical and online platforms, and is one of the few that considers mergers among grocery retailers.[5] We are aware of only two empirical studies on cross-platforms mergers – Castro (2020) and Yao (2021) – but neither consider our research question. While the former leaves open the effect on prices, the latter focuses on different industry than ours and does not estimate price effects. Studying price effects of a merger across different platforms is the focus of our work.

Second, our paper contributes to the general literature of platform economics, and particular that of mergers involving multi-sided markets (Rochet and Tirole (2003); Armstrong (2006); Chandra and Collard-Wexler (2009); Weyl (2010); Richards, Hamilton, and Allender (2016); Belleflamme and Peitz (2018); Farronato, Fong, and Fradkin (2023)). By studying a merger between a large online platform with strong network effects and a physical grocery platform with relatively weaker network externalities, we provide empirical evidence on how the merger of the two forces change grocery price levels in the US. Viewing physical retailers as two-sided markets between consumers and suppliers as in Richards and Hamilton (2013), our results align with those in Chandra and Collard-Wexler (2009) which show that mergers in such markets may not always result in increased prices for either side despite a high market concentration pre-merger.

Third, our findings have important implications for the conduct of antitrust policy when physical and digital platforms coexist (Nocke and Whinston (2022); White (2022)). Consistent with Nocke and Whinston (2022), mergers involving firms with a small contribution to HHI, as with the acquired firm in our study, might not have strong unilateral effects on consumers. Although the online retailer in our study is extremely large, the fact that its merger with a much smaller physical chain had no price increases suggests that future cross-platform

[5] A vast majority of prior studies on M&A-analysis focus on estimating only the price effects of mergers in banking (Calomiris and Pornrojnangkool (2005)), hospital (Thompson (2011)), airlines (Kwoka and Shumilkina (2010)), electronics (Ashenfelter, Hosken, and Weinberg (2013)), and petroleum (Kreisle (2013)), mostly in the brick-and-mortar setting.

mergers involving large online firms may be similarly benign, especially in industries where there are a large number of physical firms and the market is competitive. This reasoning also aligns with White (2022) who argues that the size of a firm, and efforts to develop a strong market position through better skills and ingenuity are not automatically a criterion for antitrust scrutiny.

The rest of the paper is organized as follows. We discuss the US grocery industry in Section 2. We describe our three data sources in Section 3. Section 4 discusses our estimation strategy and identification assumptions. In Section 5, we discuss the results and the robustness checks. Section 6 ends with conclusions.

2 Institutional Details: US Grocery Industry

The US grocery industry has grown consistently since 1990 and sold over $800 billion in 2022 across 63,000 heterogeneous stores of varying sizes and sales performance (Statista (2023b)). The grocery retail formats in the US are classified as supermarkets, hypermarkets, discounters, mass merchandisers, convenience stores, and traditional stores.[6] Many retail formats use price and product variety strategically (Richards and Hamilton (2006)) and provide extensive product assortments for convenient one-stop shopping.[7] In recent years, several of these retailers have capitalized their gains on renovating stores, expanding fulfillment centers, and opening new chains. The grocery industry has become more dynamic as new and existing firms are also expanding into the digital grocery space, seeking Stackelberg's first-mover advantages via attractive subscription and rapid delivery options (Aull et al. (2022)).

Building on the rising online grocery demand in the wake of COVID-19 pandemic, the US digital grocery sector is also transforming into a mature industry and has several stylized

[6] Bonfrer, Chintagunta, and Dhar (2022) provide a detailed review on the retail formats and what drives different consumer segments to these formats. Bronnenberg and Ellickson (2015) discuss the role of these different retail formats and their interaction with different agents in the supply chain - upstream and downstream partners - to produce and distribute grocery products.

[7] Hamilton and Richards (2009) study factors behind retailers' decisions to expand their product assortments and find an inverted U-shaped relationship between assortment depth (number of varieties in a category) and product differentiation, with store differentiation influencing the relationship between the two.

facts. First, according to eMarketer (2023b), the ecommerce grocery sales stood at over \$150 billion in 2022, increasing by 20 percent from the year before. During 2020-2022, the largest players in the digital grocery space were Walmart, Amazon, Instacart, and Kroger, with average yearly market shares at 24, 22, 21, and 11 percentages, respectively (eMarketer (2023a)). Another key feature of digital grocery platforms is their use of *big data*, including customer purchase history and competitor prices, to create demand-steering 'dynamic' pricing algorithms (Cavallo (2018)), which ultimately influence both equilibrium prices (Brown and MacKay (2023)) and seller competition (Johnson, Rhodes, and Wildenbeest (2023)). Third, these platforms continue to invest heavily in new micro-fulfillment centers, speedy in-home delivery schemes, and partnerships with third-party logistics to enlarge their footprints and explore new revenue streams. Besides the growth of the big grocers, the last decade also witnessed an increased competition from the entrance and expansion of over 2000 new online platforms like Gopuff, Uber Eats, DoorDash, and Grubhub offering on-demand food and grocery services. On the other hand, the ultrafast grocery-delivery startups like Gorillas and Buyk, born and fostered in the pandemic, seem unable to keep up the momentum as they struggle to profitably fulfill their promise of delivering within 10 or 15 minutes (Forbes (2022b)). Setting their stories aside, the expansion efforts from both small and big players are likely to increase the user penetration rate, thus making the digital grocery sector more robust and likely to have within- and cross-platforms mergers or acquisitions.

The US grocery sector experienced several mergers and acquisitions in the last decade. Why do grocers engage in such practices in the first place? There are several reasons, but they all come down to expanding business footprint, boosting market power, and competing with retailers from alternative formats including hard discounters like Aldi and online retailers like Amazon. The supermarket giant Kroger bought supermarkets Harris Teeter and Roundy's in 2013 and 2015, respectively, giving Kroger new routes to reinforce its market position. Similarly, Albertsons acquired Safeway in 2015, widening its network of stores, distribution centers and manufacturing plants. To withstand the rise of Walmart, Kroger, Albertsons, and Amazon, regional grocers have come together to create regional chains as in Raley's 2021 buyout of Bashas' family of stores in the west coast. Between 2017 and 2019, the total number of registered mergers and acquisitions surpassed 1400, reflecting a rising wave of consolidation in the US grocery sector (Woodall and Shannon (2018)). While existing literature has mostly focused on integration within brick-and-mortar setting, our study centers on how acquisition across platforms affect the grocery prices.

Our paper does a retrospective analysis of a buyout of a national grocery chain by a major e-commerce retailer. Before the acquisition, the online retailer primarily focused on several consumer products and had very small presence on grocery. There are a few reasons for its expansion in grocery sector on the ground (Atlantic (2017); Amazon (2017)). First, brick-and-mortar giants were breaking into e-commerce. For instance, Walmart had acquired an online retail site Jet for $3 billion in 2016, and a year later, Kroger, as a part of its 'Restock Kroger' initiative, had spent hundreds of millions of dollars to expand digitally. Second, the acquisition was a part of online-retailer's strategy to grow subscriptions in its existing online membership scheme in exchange of discounts on the groceries. Third and most importantly, the acquisition was a first step to learn the offline grocery sector to become a larger and a mainstream grocery chain. Thus, one of the largest acquisitions in the grocery sector occurred on June 16, 2017 for $12.7 billion, and the FTC approved the acquisition on August 23 in the same year (Amazon (2017); FTC (2017)). After the acquisition, the online retailer let its subscription members and delivery partners use the acquired firm's physical stores, retaining their subscription benefits, which likely boosted multihoming and network externalities for all the sides. Given the relatively modest presence of the physical chain in the overall US grocery industry (see table 1), it is not entirely clear *a priori* the effects of the acquisition on grocery prices. We study this problem using the data described below.

3 Data and Market Taxonomy

3.1 Data

Our analysis combines three data sources. The first dataset, the Cost of Living Index (COLI) from the Council for Community and Economic Research (C2ER), is meant to compare the cost of living for professional and executive US households. The index comprises prices of over 60 goods and services across categories like food, housing, transportation, utilities, and health care. For our purposes, we focus on price indices of food. About 300 researchers visit grocery stores at a specified time in 250-300 urban areas across the county and record the prices on 26 grocery items every quarter. The participants in federally labeled Metropolitan Statistical Areas (MSAs) collect data at the level of urbanized areas, defined by the Census Bureau as areas within a MSA with population density of at least 1000 per square mile. The number of stores visited by the researchers depends on the area's population size and is required to be at least five for metropolitan areas and three for nonmetropolitan areas. But researchers do not have access to the store level prices as COLI provides price indices aggregated at the level of urban areas.[8] The grocery items indices account for 13.24% of the total index and are based on Bureau of Labor Statistics data on the distribution of per-dollar grocery expenditure of a mid-management US household. Because of a lack of a crosswalk of urban areas from COLI data to geographic levels in our other data sources, we aggregate grocery price data at the level of MSAs.

Our second dataset is American Community Survey (ACS) Public Use Microdata Sample (PUMS) from the US Census Bureau which contains information on social, economic, housing, and demographic characteristics of the US population. In this study, we focus on demographic features describing population, income, and regional covariates like unemployment rate and number of stores classified as 'Grocery' under NAICS classification at the

[8] Hosken, Olson, and Smith (2018) and Beresteanu et al. (2023) use the same dataset with sample period pre-2009, and the authors had access to store-product level prices. As the C2ER team communicated to us via email, they no longer publish data at this granularity and only provide aggregated price indices at the level of urban areas.

level of Metropolitan Statistical Areas (MSAs) level to match with the granularity of price data.[9]

Although COLI provides market-level price data in four periods (three quarters (Q1, Q2, Q3), and 'Annual') every year, not every MSA has price data for all periods from 2015-2019, our sample period. In total, COLI has price data for 290 different MSAs between 2015-2019. However, half of them either had missing observations in at least one period or the entire year. Ignoring these MSAs, we consider only 145 MSAs from COLI to ensure we a balanced data structure, that is data for all $5 \times 4 = 20$ periods, required for our empirical methods.

Our final dataset is Chain Store Guide's (CSG) grocery market shares data which helps to track acquired firm's performance and market share over our sample period. The dataset consists of location, banner name, identification of the trade channel, and market shares of grocery chains. Of particular importance to our study is the market share of the acquired chain by store counts at each MSA. Because COLI prices correspond specifically to the grocery stores, CSG data are relevant for our purposes as they also focus on grocery retailers. This is particularly important when we discuss the market classification.

3.2 Market Taxonomy

In this section, we provide more details about our treatment and comparison groups of MSAs based on store-level data from Chain Store Guide and price index data from C2ER COLI. The acquisition in 2017 separated the MSAs into affected and unaffected markets since not every MSA in the CSG data has a store of the acquired chain. In the total 383 unique MSAs in CSG in our study period, we first check if the physical retailer satisfies market share thresholds as discussed in M&A-related papers such as (Hosken, Olson, and Smith (2018)).

If we adapt definition of treated MSAs from Hosken, Olson, and Smith (2018), we are not able to get enough observations. They define the affected market to be the one where a M&A affects at least 5% of its retail stores in the market (also an MSA in their setting).

[9]NAICS stands for North American Industry Classification System and is a taxonomy of business establishments by economic activity. It is maintained by the US Census Bureau. In our case, we focus on stores with NAICS codes 4451, which refers to 'Grocery' stores in the ACS-PUMS data.

If we interpret this as share of the acquired physical retailer by the number of stores, the definition turns out to be very restrictive for our setting as none of the MSAs satisfy that the chain's stores cover at least 5% of the total number of grocery stores. Given a small presence of the physical retailer (see 1), the maximum percentage observed in our data for the retailer is below this threshold. So, we work with a different cut-off value for classifying the MSAs.

We consider a MSA to be in treatment group if the acquired chain has at least 2% market share by number of stores in the MSA. Indeed as the table 1 illustrates, the reason for working with this threshold makes sense as the physical chain occupies a niche of the retail food market that is "ultra high quality" in its own words and targets customers who are willing to pay for high-end foods like organic and GMO-free. We get 30 MSAs based on this new threshold. We need to further restrict to only those MSAs where we have price data for all four periods each year from 2015 to 2019. When we compare 145 such MSAs from COLI data to 30 MSAs from CSG, we arrive at 10 MSAs. This means 20 of the 30 MSAs have a missing price data for at least one period. Our final treatment group thus have the following features: the acquired chain has at least 2% market share in terms of number of stores, and we have price data throughout the sample period for each of the 10 MSAs in the treatment group. The table 1 gives a summary statistics on the MSAs in the treatment group.

We define our comparison or control group to consist of the MSAs that do not have any stores of the acquired chain but have price data in COLI throughout our sample period 2015-2019. The first criterion applied to the CSG data gives 256 MSAs. That is, out of 383 total MSAs found in the data, over two-thirds do not have any presence of the physical chain. When we juxtapose these MSAs against the 145 MSAs that have price data for all periods from 2015-2019, we arrive at the comparison group consisting of 54 MSAs. By construction, none of them has a store of the acquired chain but each has price data in our study period.

Table 1: Market Characteristics in the Treatment Group

Treated MSAs	Acquired Stores	Market Share	Grocery Stores	HHI	Change in HHI
Austin-Round Rock TX	5	4	162	3234.3	-301.7
Boston MA	29	9.1	621	1047.7	-20.3
Denver-Aurora-Lakewood CO	11	5.6	289	2192.7	-59.9
Hilton-Head Island-Bluffton SC	1	2.1	41	1417.4	268.3
Lincoln NE	1	4.5	47	1580	105.8
Manchester-Nashua NH	2	3.2	52	2126.8	-76.7
Portland-Vancouver-Hilsboro OR-WA	8	4.2	333	1404.5	23.5
San Francisco-Oakland-Hayward CA	21	10	800	1525.8	-35.8
South Bend-Mishawaka IN-MI	1	5.8	42	1619.4	-50.6
Tucson AZ	3	3.1	126	1133.2	133.2

Note: The values in the first four columns — number of stores of the acquired chain, revenue market share of the acquired chain, number of grocery stores, and HHI — are from average of the corresponding variable for two years before the treatment, i.e. averages of 2015 and 2016. The final column indicates difference between 'HHI' column and two years average of HHI of 2018 and 2019. The tables illustrates a small presence – in terms of market share by both revenue and store counts – of acquired physical chain across the MSAs.

4 Empirical Approach

In this section, we provide details on our two reduced-form methods to estimate the effects of the acquisition on price levels.

4.1 Difference-in-Differences Estimator

Our first approach is difference-in-differences (DiD) method which uses a combination of before-after and treatment-control comparisons. To this end, we first estimate the price effects using the standard two-way fixed effects model in 1:

$$\ln(p_{jt}) = \alpha_j + \beta_t + \delta D_{jt} + \varepsilon_{jt} \tag{1}$$

where p_{jt} is the price index of MSA $j \in \{1, 2, \ldots, 64\}$ in time period $t \in \{1, 2, \ldots, 20\}$; α_j and β_t are MSA and time fixed effects that control for unobserved heterogeneity specific to MSA and time, respectively; D_{jt} is the treatment indicator which equals 1 if MSA j is treated at time t and 0 otherwise; and ε_{jt} is the error term. In our case, treatment starts on June, 2017 if we go by announcement date of the acquisition, and on August, 2017 if we take the implementation date or date when FTC approved the acquisition. In the COLI price data, these dates fall on second and third quarters of 2017, hence at $t = 10$ and $t = 11$, respectively. Our main parameter of interest is δ, which captures the average price effect of the acquisition on the treatment group of 10 MSAs.

Equation 1 is simple and similar to the DiD model used in Hosken, Olson, and Smith (2018) but is uninformative of how treatment effects evolve over periods post-acquisition. So, we also consider the standard event study approach using model 2 to understand the price effects at each period after the announcement and implementation of the acquisition:

$$\ln(p_{jt}) = \alpha_j + \beta_t + \sum_{\tau=1}^{k-1} \underbrace{\delta_\tau}_{\text{leads}} D_{j\tau} + \sum_{\tau=k+1}^{20} \underbrace{\delta_\tau}_{\text{lags}} D_{j\tau} + \varepsilon_{jt} \tag{2}$$

where lead and lag terms indicate anticipatory effects and post-treatment effects, respectively. Another advantage of event study approach over the static model 1 is anticipatory effects also provide a way to test an identifying assumption of parallel trends as we discuss in section 4.1.1. We take two versions of model 2 for $k = 10$ (announcement of merger) and $k = 11$ (implementation of merger). In both models 1 and 2, we do not add control variables (MSA-specific covariates) in the regression because, as we will see in 4.1.1, the key identifying assumption about parallel trends is *unconditional*, that is independent of the covariates. We cluster standard errors at the level of MSAs in both specifications.[10]

4.1.1 Identification in DiD

Here, we verify that our data satisfy the identifying assumptions of the DiD framework (Bertrand, Duflo, and Mullainathan (2004)). First, the acquisition must be an exogenous event and not be subject to the lobbying efforts of the retailers in the treatment group to

[10]Recent DiD literature has shown that canonical two-way fixed effects models as in 1 and 2 can give biased estimates when treatment is staggered or rolled-out across multiple time periods (Goodman-Bacon (2021)). These results do not hold in our setting as treatment (acquisition) affects the markets at a single time period, either $t = 10$ (merger announcement) or $t = 11$ (merger implementation).

have the online retailer acquire the physical chain. This is a reasonable assumption as the event took place only in the interests of the two firms and for reasons mentioned in section 2. Moreover, our next empirical method deals with the possibility of merger endogeneity by considering the propensity score approach.

Second, the treatment and control groups need to satisfy the stable unit treatment value assumption (SUTVA) (Bertrand, Duflo, and Mullainathan (2004)). This says that there are no spillover effects in the treatment. In simpler words, this means that the potential prices in the treated MSAs do not vary with the treatment assigned to other MSAs, and there are no two different versions of treatment status for each MSA. This is obvious for our setting as MSA affected by the acquisition continues to be so and its prices are independent of the treatment status of other MSAs.

Third and the most important assumption is that of the parallel trends which states that the prices in the treatment group, without the acquisition, would have followed the same trend as that in control group. While several factors might cause the price index to differ between the two groups, we require that this difference is static throughout the sample period. However, researchers cannot fundamentally test this assumption in the post-treatment period due to absence of the counterfactual. So, one resorts to testing the assumption only for pre-treatment data. A visual supporting evidence for the parallel-trends before the treatment is in 2, which shows all estimates before the treatment (both in terms of acquisition announcement and implementation) are statistically different from zero. A supplementary evidence is that none of the leads in model 2 are statistically significant, as shown by the estimates in the table 3. These three assumptions guarantee that DiD estimates give a reliable measure of the price effects following the acquisition. We discuss the results of models 1 and 2 in section 5.1. Next, we describe our main empirical method.

4.2 Augmented Inverse Probability Weighting Estimator

In this section, we discuss our main estimator to get treatment effects for each of the 10 treated MSAs. Augmented inverse probability weighting (AIPW) estimator is an approach within a larger family of conditional average treatment effects (CATE) estimators. We base the discussion on Robins, Rotnitzky, and Zhao (1994), Glynn and Quinn (2010), and Athey and Wager (2019).

AIPW estimator nests two estimation approaches: inverse propensity weighting (IPW) model and outcome regression model. In the first approach, one starts by estimating each unit's propensity score, that is its probability of receiving the treatment based on observed covariates. Here, the idea is to model the treatment assignment mechanism and create a balance in covariates between the treated and comparison groups. After computing propensity scores, one calculates the *weights* for each unit using the inverse of the estimated propensity scores. Here, treated units with low propensity scores and control units with high propensity scores both exert a significant influence on the inverse propensity weighting estimates. This completes IPW approach inside AIPW estimator.

In AIPW's second approach which is outcome regression model (an example is OLS), one fits a regression model for the outcome variable using the both treated and control groups, and models the expected outcome as a function of covariates, thus capturing the relationship between covariates and the outcome variable.

Using only IPW or outcome regression method can lead to biasness, especially due to possible model misspecifications. AIPW estimator is comparable or superior to both due to its double robustness-property: one only requires either the outcome model or the propensity score model to be correctly specified to get an unbiased estimate of the average treatment effect.[11] How does it achieve this property? It 'augments' the outcome regression model by having the estimated propensity scores as additional covariates. The final AIPW estimates thus represent the difference in expected outcomes between the treated and comparison

[11]See Glynn and Quinn (2010) for a formal proof of the doubly-robust property of AIPW estimator.

groups, considering the covariate imbalances and potential model misspecifications.

Formally, AIPW combines an outcome regression $\mu^D(X) = E[Y|D, X]$ with propensity scores $e(X) = E[D|X]$, where Y represents the outcome variable, D the treatment, and X the covariates. For each of the 10 treated MSAs, we estimate the AIPW model in 3:

$$\hat{\tau}_{AIPW} =$$
$$\frac{1}{n}\sum\nolimits_{i=1}^{n}\left(\underbrace{\hat{\mu}^{(1)}(X_i) - \hat{\mu}^{(0)}(X_i)}_{\text{Outcome regression model}} + \underbrace{\frac{D_i}{\hat{e}(X_i)}\left(Y_i - \hat{\mu}^{(1)}(X_i)\right) - \frac{(1-D_i)}{1-\hat{e}(X_i)}\left(Y_i - \hat{\mu}^{(0)}(X_i)\right)}_{\text{IPW model}}\right) \tag{3}$$

where $\mu^{(d)}(x) = \mathbb{E}\left[Y_i \mid X_i = x, D_i = d\right]$ is the expected value of the outcome, conditional on observable characteristics x and treatment status d, and $e(x) = \mathbb{E}\left[D_i = 1 \mid X_i = x\right]$ is the propensity score or the probability of treatment given the covariates.

We estimate $\hat{\tau}_{AIPW}$ using a variety of machine learning algorithms for both propensity score model and outcome regression model. Specifically, in our baseline results, we choose the random forest algorithm for propensity scores and linear regression for outcome model. We consider other specifications in our robustness analysis in 5.3. Following the practices in machine learning literature, we estimate both propensity scores and outcome regression models using *cross-fitting* procedure, where one divides data, estimates treatment effects separately in all subsets, and aggregates results for robust causal effect estimation. We estimate the price effects at each MSA by considering treatment to be either announcement date or implementation date of the acquisition.

4.2.1 Identification in AIPW

This section discusses the three identifying assumptions required for AIPW estimator (Glynn and Quinn (2010); Imbens and Rubin (2015)). First, we assume that the data generating process satisfy *unconfoundedness* or *selection on observables*. Here, we assume that the treatment status is not randomly assigned, that is some MSAs can *select themselves* into the treatment based on observed covariates. However, we require that the treatment status is as good as randomly assigned based after conditioning on the observed covariates. Formally, it

Table 2: Balance of covariates across treatment and control groups

Variable	Control	Treatment	SMD
HHI	2475.08	1734.91	-1.2113
Household Mean Income (USD)	68716	90686	1.4055
Household with Children Percent	0.18	0.19	0.1592
Median House Prices (USD)	170389	317126	1.1178
Number of Grocery Stores	36	252	1.1903
Population	253474	1907330	1.3793
Hispanic Population Percent	0.16	0.17	0.0309
Black Population Percent	0.09	0.07	-0.1981
Population Percent with Poverty	0.16	0.12	-0.9736
Observations	540	100	

Notes: The results in the table are based on pre-treatment, that is 2016, and are averages for the corresponding covariate within control and treatment groups. The final column reports standardized mean difference defined in terms of means and variances of treatment and control groups: $\frac{\bar{T}-\bar{C}}{\sqrt{(T^2_{var}+C^2_{var})/2}}$*, where* T, C *refer to treatment and control groups, respectively. SMD* > 0.1 *indicates that corresponding covariate is unbalanced across the two groups.*

states $\{Y(1), Y(0)\} \perp\!\!\!\perp D|X$, where $Y(1)$ and $Y(0)$ are potential outcomes under treatment and control, D is the treatment status, and X refers to observed covariates. Intuitively, this means we assume that there are no unobserved covariates that might affect both the firms' decision to merge and the MSA price levels. One cannot directly test this assumption, but we report a balance of several covariates across treatment and control groups in table 2, which shows that the two groups differ along several covariates.

Second, one assumes *overlap* or *common support* of the estimated propensity scores $e(X)$. This says that along certain range of covariate values, there could be some MSAs that are in treatment group and some in control group. Figure 1 provide an evidence that propensity scores, based on logistic regression and all covariates from table 2, do overlap across treatment and control groups.

Third, we assume stable unit treatment value assumption (SUTVA), which states price levels at a MSA does not depend on the treatment status of other MSAs. This is reasonable considering we don't expect spillover effects from the cross-platforms acquisition. Based on

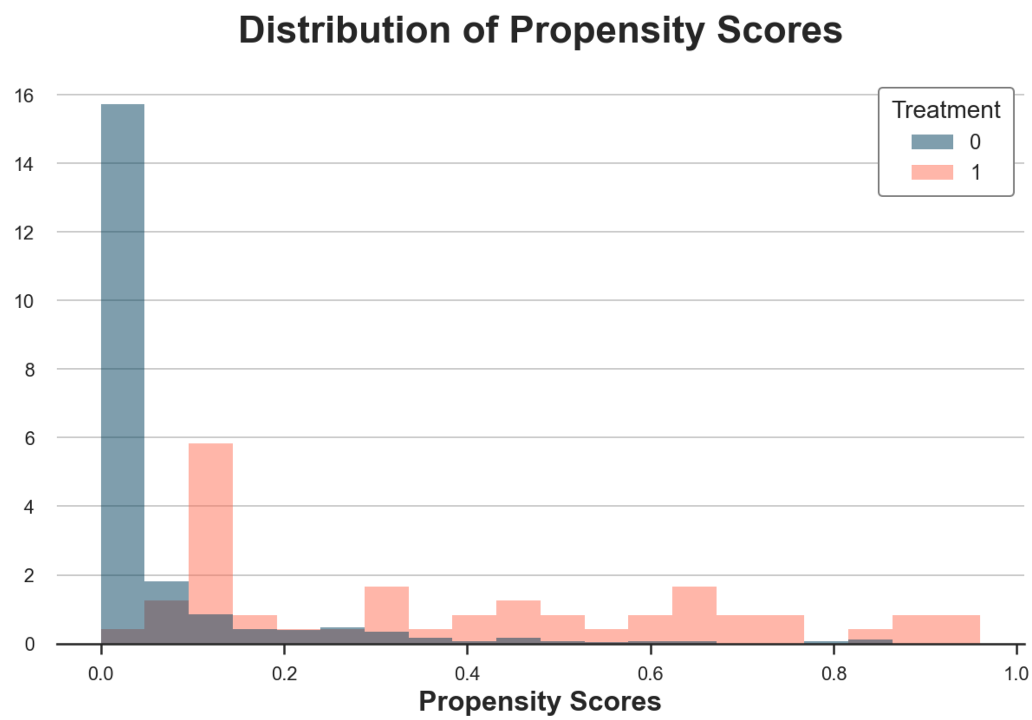

Figure 1: *The graph shows an overlap of propensity scores across treatment and control groups based on covariates from table 2 and logistic regression. Here, we take the treatment time to be merger announcement, and we get similar distribution when we take treatment time to be merger implementation.*

these three assumptions, we implement AIPW estimator and report the results in section 5.2.

5 Results and Robustness Checks

5.1 DiD Results

We describe our DiD results. Graph 2 and table 4 based on models 1 and 2 show the insignificant effects on all periods post-treatment. The average treatment effect on the treated (ATT) post acquisition, that is the estimate for the parameter δ in model 1, is negative but statistically insignificant. This suggests that the acquisition had no effects on the *treated group* of 10 MSAs in periods after treatment, by both announcement and implementation dates. However, because the parallel trends is at the level of treatment group, DiD does not tell about the effects for each MSA in the group. It is possible that effects can also be heterogeneous across treated units, especially given that the MSAs are heterogeneous in terms of income, HHI, unemployment rate, population, and other covariates. We explore this in section 5.2.

5.2 AIPW Results

We discuss results from AIPW estimator in the baseline specification. We perform random forest algorithm for the propensity score model and linear regression for the outcome regression.

In table 5, we observe that price effects of the acquisition are negative for 8 of the 10 treated MSAs, and positive for 1 treated MSA (Tucson AZ). Since the dependent variable is in log, we interpret the estimates as percentage changes due following the acquisition. For instance, we see Austin-Round Rock TX witnessed an average drop of 5.2 percent in its grocery price level in periods following the acquisition. We also note that price effects for all but one MSA (Manchester-Nashua NH) are robust and statistically significant across both columns, that is when considering the treatment to be acquisition announcement and

Table 3: Estimates of leads in model 2

Time period	(1)	(2)
$t = 1$	-0.0020	-0.0012
	(0.0017)	(0.0021)
$t = 2$	-0.0019	-0.0011
	(0.0017)	(0.0019)
$t = 3$	-0.0011	-0.0004
	(0.0016)	(0.0017)
$t = 4$	-0.0011	-0.0004
	(0.0012)	(0.0017)
$t = 5$	0.0006	0.0012
	(0.0015)	(0.0019)
$t = 6$	0.0006	0.0012
	(0.0018)	(0.0017)
$t = 7$	0.0014	0.0019
	(0.0015)	(0.0016)
$t = 8$	0.0007	0.0012
	(0.0013)	(0.0015)
$t = 9$	0.0001	0.0007
	(0.0015)	(0.0012)
$t = 10$		0.0006
		(0.0012)
Num. Obs.	1280	1280
R2	0.913	0.913

The std. errors are in parentheses and clustered at the MSA level. Columns (1) and (2) refer to treatment by merger announcement and implementation, respectively.

Table 4: Estimates of lags in model 2

Time period	(1)	(2)
$t = 11$	-0.0007	
	(0.0013)	
$t = 12$	-0.0003	0.0004
	(0.0008)	(0.0007)
$t = 13$	-0.0009	-0.0002
	(0.0011)	(0.0010)
$t = 14$	-0.0010	-0.0003
	(0.0014)	(0.0015)
$t = 15$	-0.0013	-0.0005
	(0.0015)	(0.0016)
$t = 16$	-0.0011	-0.0004
	(0.0011)	(0.0012)
$t = 17$	-0.0023	-0.0015
	(0.0016)	(0.0017)
$t = 18$	-0.0007	0.0000
	(0.0015)	(0.0014)
$t = 19$	-0.0022	-0.0013
	(0.0013)	(0.0015)
$t = 20$	-0.0018	-0.0010
	(0.0013)	(0.0014)
ATT	-0.0090	-0.0100
	(0.0130)	0.0130
Num. Obs.	1280	1280
R2	0.913	0.913

The std. errors are in parentheses and clustered at the MSA level. Columns (1) and (2) refer to treatment by merger announcement and implementation, respectively.

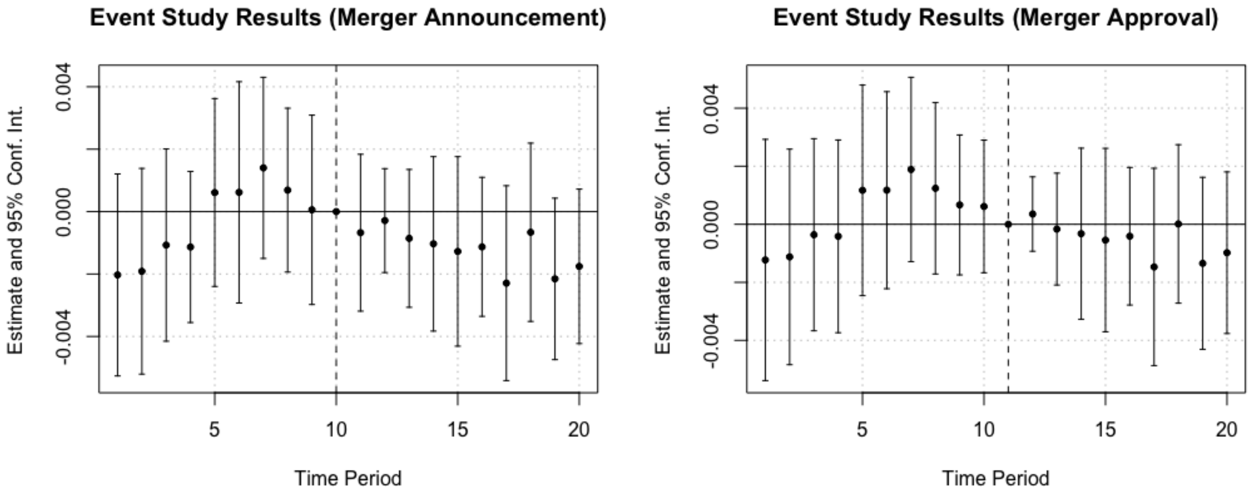

Figure 2: *The two graphs event-study plots based on model 2. The vertical lines depict the time of treatment. The left graph corresponds to treatment being the announcement of acquisition, namely June 2017 or* $t = 10$ *in the COLI data. The right graph corresponds to treatment at date of acquisition approval, August 2017 or* $t = 11$*. Both provide a supporting evidence for parallel pre-trends as none of the leads are significant at 5% significance level.*

implementation dates.

In section 5.3, we check whether these results hold under varying specifications in AIPW estimator.

5.3 Robustness Checks for AIPW Results

We carry out robustness analysis of the AIPW results in two ways. First, we change the specification of propensity score model from random forest algorithm to logistic regression, which is a standard model to compute propensity scores. We report the results in table 6. The estimates are mostly similar to our baseline results in table 5. For 4 MSAs (Denver, Portland, San Francisco), we observe that price effects are not significant when we consider treatment to be acquisition announcement, unlike the same treatment in our baseline results. Qualitatively, our results in tables 5 and 6 do not differ much as we have almost identical price effects when we consider treatment in terms of merger announcement and very similar when treatment is set to be merger implementation.

Second, we model propensity scores using logistic regression with cross validation. We

Table 5: AIPW Baseline Estimates: Prop. Scores by Random Forest

Treated MSAs	(1)	(2)
Austin-Round Rock TX	-0.052***	-0.057***
	(0.007)	(0.008)
Boston MA	-0.018**	-0.017***
	(0.009)	(0.007)
Denver-Aurora-Lakewood CO	-0.125***	-0.119***
	(0.016)	(0.008)
Hilton Head Island-Bluffton SC	-0.039***	-0.036***
	(0.007)	(0.009)
Lincoln NE	-0.033***	-0.032***
	(0.007)	(0.007)
Manchester-Nashua NH	-0.041***	-0.040
	(0.007)	(0.029)
Portland-Vancouver-Hillsboro OR-WA	-0.074***	-0.069***
	(0.007)	(0.012)
San Francisco-Oakland-Hayward CA	-0.115***	-0.116***
	(0.007)	(0.015)
South Bend-Mishawaka IN-MI	-0.010	-0.011
	(0.007)	(0.008)
Tucson AZ	0.033***	0.027***
	(0.010)	(0.008)
Model specifications:		
Propensity Scores = Random Forest	✓	✓
Outcome Regression = Linear Regression	✓	✓
Num. Obs.	1100	1100

Signif. Codes: ***$p < 0.01$, ** $p < 0.05$, * $p < 0.1$. *The standard errors are in parentheses. Columns (1) and (2) refer to treatment by merger announcement (*$t = 10$*) and implementation (*$t = 11$*), respectively.*

report the tables in 7. The results are qualitatively, often even quantitatively, identical to the baseline results for both treatments (acquisition announcement and implementation dates), with one addition that estimates for South Bend-Mishawaka IN-MI are marginally negative at 10 percent level, unlike in the baseline results.

Table 6: AIPW Robustness Estimates: Prop. Scores by Logistic Reg.

Treated MSAs	(1)	(2)
Austin-Round Rock TX	-0.051***	-0.056***
	(0.007)	(0.008)
Boston MA	-0.019**	-0.017***
	(0.007)	(0.007)
Denver-Aurora-Lakewood CO	-0.127***	1.673
	(0.007)	(12.106)
Hilton Head Island-Bluffton SC	-0.039***	-0.039***
	(0.01)	(0.008)
Lincoln NE	-0.034***	-0.031***
	(0.007)	(0.007)
Manchester-Nashua NH	-0.04***	-0.046***
	(0.008)	(0.011)
Portland-Vancouver-Hillsboro OR-WA	-0.075***	-0.018
	(0.007)	(0.591)
San Francisco-Oakland-Hayward CA	-0.116***	0.407
	(0.007)	(3.693)
South Bend-Mishawaka IN-MI	0.047	0.012
	(0.093)	(0.055)
Tucson AZ	0.032***	0.032
	(0.007)	(0.036)
Model specifications:		
Propensity Scores = Logistic Regression	✓	✓
Outcome Regression = Linear Regression	✓	✓
Num. Obs.	1100	1100

Signif. Codes: ***$p < 0.01$, ** $p < 0.05$, * $p < 0.1$. *The standard errors are in parentheses. Columns (1) and (2) refer to treatment by merger announcement ($t = 10$) and implementation ($t = 11$), respectively.*

Table 7: AIPW Robustness: Prop. Scores by Logistic Reg. CV

Treated MSAs	(1)	(2)
Austin-Round Rock TX	-0.045***	-0.057***
	(0.007)	(0.008)
Boston MA	-0.024**	-0.018***
	(0.007)	(0.007)
Denver-Aurora-Lakewood CO	-0.125***	-0.122***
	(0.007)	(0.009)
Hilton Head Island-Bluffton SC	-0.040***	-0.035***
	(0.009)	(0.011)
Lincoln NE	-0.034***	-0.034***
	(0.007)	(0.007)
Manchester-Nashua NH	-0.034**	-0.050***
	(0.016)	(0.009)
Portland-Vancouver-Hillsboro OR-WA	-0.077***	-0.071***
	(0.007)	(0.007)
San Francisco-Oakland-Hayward CA	-0.117***	-0.108***
	(0.008)	(0.007)
South Bend-Mishawaka IN-MI	-0.012*	-0.012*
	(0.007)	(0.007)
Tucson AZ	0.034***	0.025***
	(0.007)	(0.007)
Model specifications:		
Propensity Scores = Logistic Regression CV	✓	✓
Outcome Regression = Linear Regression	✓	✓
Num. Obs.	1100	1100

*Signif. Codes: ***$p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The standard errors are in parentheses. Columns (1) and (2) refer to treatment by merger announcement ($t = 10$) and implementation ($t = 11$), respectively.*

5.4 Discussion

We find negative price effects on 8 MSAs, and positive price effect in one MSA. In the robustness, we see these results are generally valid for both types of treatment – merger announcement on June, 2017 and merger implementation on August, 2017. We argue that these estimates align with a discussion in the FTC's Horizontal Merger Guidelines (FTC (2010)).

Figure 3 illustrates how AIPW estimates can be interpreted in the context of the merger guidelines. The vertical lines at 1500 and 2500 denote FTC's thresholds: market is unconcentrated market if HHI < 1500 post-merger, moderately concentrated if HHI $\in [1500, 2500]$, and highly concentrated if HHI > 2500. The Guidelines assert that the mergers leading to an increase in HHI < 100 are unlikely to have anti-competitive effects. The blue dots in figure 3 belong to this category. In this context, it makes sense that price effects are negative, and not positive for each of the blue dots. Similarly, the Guidelines also maintain that in the moderately concentrated region, if post-mergers lead to HHI increase of greater than 100 points, that might call for merger scrutiny. In our case, the green and red dots in the moderately concentrated region belong to this category, but we see that price levels fall in those markets post-merger. Another related Guideline is that mergers in unconcentrated markets might not lead to anti-competitive effects. However, in our case, the green dot in the unconcentrated region is associated with an increase in price levels. Overall, we see our results mostly align with FTC Merger Guidelines, and our results also suggest that concentrated thresholds and changes in HHI alone would not be enough for predicting which mergers are benign. This is particular true of the digital-physical mergers where several unique factors can determine how mergers affect competition and price levels.

6 Conclusion

The grocery industry in the US continues to experience expansion from incumbent firms, mergers and acquisitions, and evolution in the digital space. Understanding the price effects

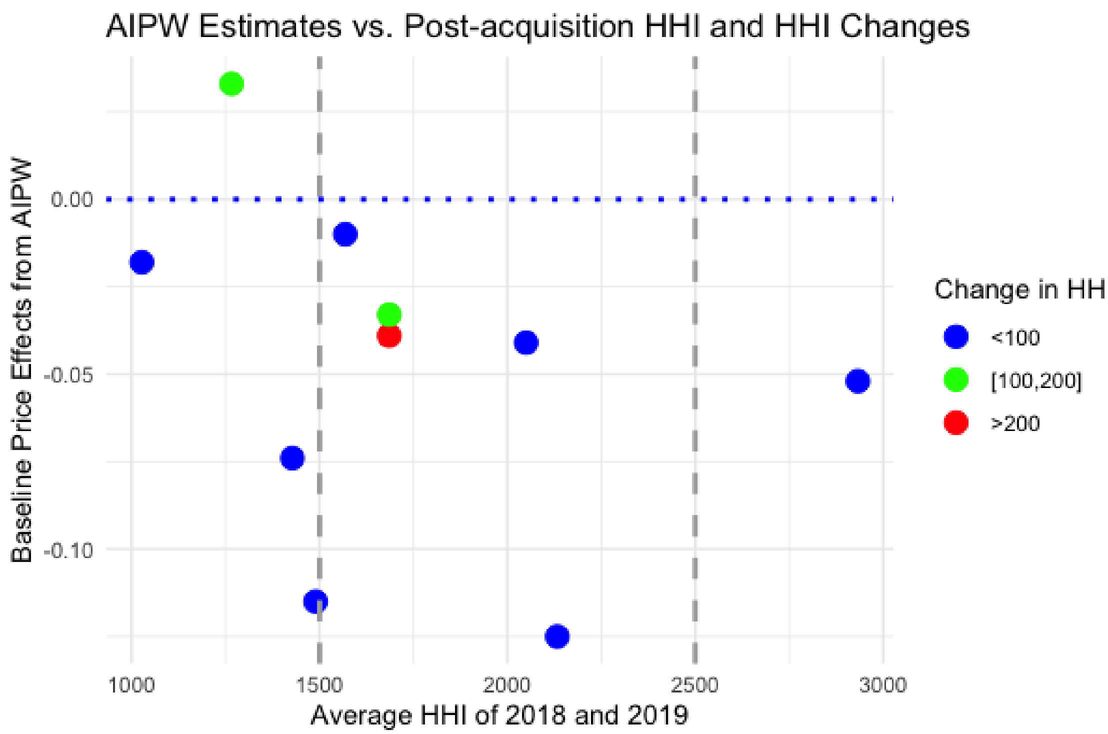

Figure 3: *The graph shows a relationship between post-acquisition HHI, change in HHI, and baseline AIPW estimates when we take the treatment to be in terms of merger announcement.*

of these changes is important for consumers as well as antitrust authorities. The literature on mergers and acquisitions does not converge in regards to the direction of price effects. Moreover, the existence of network externalities, as two-sided markets possess, in our setting presents unique challenges and importance of studying the effects of integration across different platforms.

This study examines the price effects from an acquisition of a national grocery chain by a large online retailer in the US. Although initially hailed as an event that would disrupt the competition and increase the prices, an *ex post* analysis of the acquisition does not validate these claims. We find price levels fall in at least 8 of the 10 treated MSAs, and the results are stable under various specifications of our estimation strategy.

While further methods and more granular datasets on prices would be necessary to fully attribute the effects to the acquisition, we can already see that the price estimates from our reduced-form approach are largely consistent with the FTC's 2010 Horizontal Merger Guidelines. The MSAs where price index decreased after the acquisition are those whose HHI is moderately concentrated or very close to being so (that is, $HHI > 1500$), so any merger involving the stores with relatively small market shares is unlikely to increase the prices at such markets. Future work with firm- and product-level data can shed more light on the precise mechanisms behind our results, and on the merger's effects on other dimensions like consumer and producer welfare and retail competition.

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