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ORIGINAL EMPIRICAL RESEARCH

Effects of switching from channel-specific pricing to uniform pricing in omnichannel retail: Evidence from a quasi-experiment

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Abstract

Many omnichannel firms are switching from channel-specific pricing to uniform pricing, but little is known about the immediate and long-term effects of this change. Relying on a quasi-experiment setting featuring a pricing policy change varying across 4,150 products over 18 months, we empirically investigate the effects of uniform pricing on online and offline channel sales. We show that this transition is beneficial for both online and offline sales, with effects becoming increasingly positive over time. Furthermore, we show that the treatment effects can be decomposed into two contrasting effects: a delayed positive effect and an immediate negative effect on product sales. Last, in consumer-level analysis, we show that the treatment effects are due to two distinct behavioral patterns: a decrease in the shopping frequency of 77.4% of the consumers who can no longer leverage price differences between channels and an increase in the activity of 22.6% of consumers who appreciate the ease of search and the comprehensiveness of the omnichannel experience. This study sheds light on an important pricing decision in omnichannel retail—whether to switch from channel-specific to uniform pricing.

Keywords Omnichannel retailing · Channel-specific pricing · Uniform pricing · Product sales · Quasi-experiment

Introduction

Many omnichannel firms are investing in integrating their online and offline channels to provide a more consistent shopping experience (Cui et al., 2021). By coordinating processes and technologies, a firm can create a consistent, yet unique and contextual brand experience across all consumer touchpoints. However, determining the appropriate pricing strategy for these integrated channels is a significant challenge (Kireyev et al., 2017; Ratchford, 2009; Wolk and Ebling 2010). Under a traditional channel-specific pricing strategy, firms price identical products differently across channels, focusing on the price competitiveness of each channel to meet varying consumer demands (Ofek et al. 2011). This approach can be effective at capturing consumer surplus by segmenting the market (Ratchford, 2009). For instance, firms can charge a price close to the reservation price in one channel while offering a lower price in another (Stahl, 1989). This strategy leverages variations in consumers' willingness to pay and their level of information, using lower prices to attract price-sensitive consumers and higher prices to capture value from those who are less price-sensitive (Varian, 1980).

By contrast, the uniform pricing strategy aims to reduce price discrepancies and corresponding concerns about value inconsistencies across channels for consumers (e.g., Saghiri et al., 2017; Welford, 2023), which in turn improves the utility of consumers who value smooth omnichannel experiences. On the supply side, uniform pricing is gaining popularity among firms, increasing in prevalence from 21% of retailers in 2013 to 50% in 2018 (Cavallo, 2017; Grant,

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2018). On the demand side, consumers express a strong desire for cross-channel integrity in omnichannel retailing. For example, concerns about price discrimination and potential differences in the value proposition of products across channels are prevalent on social media platforms (see Figure WA1 in Web Appendix A for examples). Further, recent surveys suggest that consumers who conduct cross-channel searches are willing to spend more if cross-channel discrepancies can be resolved (Andersen, 2025; Salsify 2025).

Given the trends on both the supply and demand sides, it is surprising that channel-specific pricing remains a significant practice among multichannel retailers. For instance, Walmart was found to have price discrepancies between its online and offline stores (Hetu, 2018), and Detalicznym & Praktyki (2023) documented that, among the twelve multichannel retailers they investigated, two still adopt channel-specific pricing. While some retailers offer a price-matching policy (Srivastava & Lurie 2001), such a policy does not enhance the omnichannel experience by addressing the concerns of channel-specific pricing (see Table WA1 in Web Appendix A for a comparison). Uniform pricing aims to minimize a consumer's intention to search within the same retailer, leading them to focus on the product itself or on cross-seller searches. In contrast, a price-matching policy signals that price discrepancies might exist and that there is a reward for finding them, which encourages consumers to search across the retailer's different channels. Hence, the effectiveness of switching from channel-specific to uniform pricing is a topic of substantial importance for omnichannel retailers, but the current literature does not offer a comprehensive investigation or explanation.

With the discrepancy between business practices and theoretical expectations, this research seeks to answer the following questions: (1) How does a shift from channel-specific to uniform pricing affect product sales? (2) What are the underlying mechanisms that drive these effects? (3) How do these effects manifest in consumer-level behavior patterns? To answer these questions, we empirically investigate an omnichannel retailer's policy change using a quasi-experimental design. Our dataset comprises approximately 2.2 million daily records for 4,150 products over an 18-month period. The quasi-experiment arises from a natural setting in which a consumer electronics retailer implemented a uniform pricing policy for products under one ownership structure (heavy assets), while retaining channel-specific pricing for another (light assets), creating clear treatment and control groups. Our findings indicate that the policy change had an overall positive long-term effect on product sales in both online (+1.31%) and offline (+3.56%) channels.

This aggregate effect, however, masks a critical temporal dynamic: the treatment effect evolved from initially negative in both channels (online: −5.64%; offline: −1.78%) to positive within 12 months (online: +9.20%; offline: +8.00%), with a particularly pronounced trend in the online channel. By examining variations in price adjustments, we identify two mechanisms that explain this pattern: a delayed positive effect from reduced cross-channel search cost and an immediate negative effect from loss of price discrimination. Finally, our consumer-level analysis reveals that this positive aggregate effect is driven by a distinct minority of customers. Specifically, 22.6% of consumers reacted positively to the policy, increasing their purchases by 39.89%, while 77.4% of consumers reacted negatively, decreasing their purchases by 12.54%. The post-hoc analysis further reveals that the positively responding segment is characterized by higher membership levels and greater order frequency.

Our findings offer three main contributions. First, we extend recent research on channel integration (Kim & Chun 2018; Lee et al., 2021), omnichannel practice (Gu & Tayi 2017), cross-channel retailing (Zhang et al., 2019), and price dispersion (Cui et al. 2019). We provide empirical evidence on the performance implications of a uniform pricing policy, an important decision in omnichannel retailing. To the best of our knowledge, our research is the first to empirically compare channel-specific and uniform pricing models. Second, the study uncovers novel dynamics in sales performance across channels, extending the omnichannel literature. Our analysis shows that the effects of adopting a uniform pricing strategy arise from an interplay between the delayed positive effects of reduced cross-channel search cost and the immediate negative effects of loss of price discrimination. We also find that the online channel reacts more strongly to uniform pricing than the offline channel, an insight which implies that the benefits of such policies take time to appear. Third, our consumer-level analysis confirms that adopting uniform pricing leads price-sensitive consumers to reduce their shopping frequency initially. However, the remaining more loyal consumers gradually learn to appreciate the benefits and increase their shopping frequency, ultimately driving the positive sales lift.

Related literature

Our study is related to the literature on omnichannel retail, particularly online-offline channel integration, which investigates the synergy between various channels and consumer journeys and pricing strategy in the omnichannel context. We summarize the related literature in Table WA2 in Web Appendix A.

Channel integration

Given the rapid progression of retailing methods, online-offline channel integration is emerging as a central concern for retailers (Cao & Li 2015; Gu & Tayi 2017). Recent research outlines the core challenges inherent to this landscape, which include data access, consumer privacy, and, pertinent to our investigation, coordination (Cui et al., 2021). Coordination emerges as a pivotal challenge primarily because of the multi-touchpoint nature of online and offline channels and the often nonlinear journey consumers undertake across these platforms.

Historical strategies in this realm have explored several integration avenues. For example, Gallino et al. (2017) delve into strategies such as "buy online, pick up in store," and Bell et al. (2017) and Wang & Goldfarb (2017) investigate the role of offline showrooms in omnichannel retail. Another avenue pertains to the influence of offline store presence on online behaviors. Fisher et al. (2019) demonstrate that the opening of a physical store can have varied effects on online sales and searches based on the existing brand presence in the region. Their findings illuminate the complex interplay between online and offline channels, with factors such as brand awareness, consumer learning, and store presence playing pivotal roles. This intricate dance between channels, as identified by Gu & Tayi (2017), underscores the need for holistic omnichannel strategies that consider the dynamic and often unpredictable behavior of consumers. Prior research has examined several online-offline integration strategies, including buy online, pick up in store (Gallino et al. 2017); offline showrooms in omnichannel retail (Bell et al., 2017; Wang & Goldfarb 2017); a ship-to-store function (Gallino et al. 2017); and product placement (Gu and Tayi 2017). In areas where a retailer has a strong presence, the opening of an offline store is associated with a decrease in online sales and searches; however, in areas where a retailer does not have a strong presence, the opening of an offline store is associated with an increase in online sales and searches. In a quasi-experiment involving the opening of a new distribution center by a US apparel retailer, Fisher et al. (2019) show that online sales have a positive spillover effect on the retailer's offline sales. They identify two main drivers of the observed effect: (1) consumer learning through service interactions with the retailer and (2) an increase in brand presence through the online store penetration rate and offline store presence. In addition, Gu & Tayi (2017) find that consumers frequently switch between online and offline channels as they navigate various stages of the decision journey. This reality suggests that firms should develop omnichannel strategies to optimize their overall profit.

Pricing in omnichannel operations

Despite the importance and complexity of omnichannel coordination, surprisingly the field lacks a clear understanding of the consequences of uniform versus channel-specific pricing, even though pricing is one of the most important decisions in omnichannel operations (e.g., Cavallo, 2017; Kireyev et al., 2017). Such an understanding would be invaluable for companies attempting to integrate online and offline channels to maximize profits. Following price discrimination theory, some studies document that personalized pricing is usually better than uniform pricing when the target markets are well separated (Chen et al. 2020) or have regional and channel price elasticity (Harsha et al. 2019), when a price-matching is guaranteed (Kireyev et al., 2017), and under different targeted markets (Li et al., 2018). By contrast, as cross-channel searching (e.g., showrooming, webrooming; Bell, Gallino & Moreno 2017) becomes prevalent for consumers, other research has shown that uniform pricing can be more beneficial than channel-specific pricing in terms of cost-savings (Cai et al., 2019), pricing fairness (Chen & Cui 2013), and reduced product returns (Chen et al., 2023).

Empirical research mostly focuses on the prevalence of the two pricing models in business practices. Cavallo (2017) compares online prices with offline prices and finds that they are identical between channels 72% of the time. Most price changes are not synchronized across channels, but the average frequency and size of the changes are similar between online and offline. DellaVigna and Gentzkow (2019) estimate the prevalence of uniform pricing in offline retail chains spanning multiple geographic regions. Tabanakov et al. (2024) find that, although the pricing strategy of a store brand is mixed and complicated, 73% of stores adopt region-specific pricing rather than uniform pricing. However, this is a fundamentally different type of uniform pricing—consumers cannot easily compare prices or switch between stores in different geographic regions, while they can do so between online and offline channels. Our study adds to the existing body of knowledge by empirically comparing the two multichannel pricing models.

Theoretical background

We base our conceptual framework on consumer search and price discrimination literature. We propose that implementing a uniform pricing strategy affects product sales through two contrasting mechanisms: its effect on cross-channel search cost and price discrimination.

Uniform pricing and cross-channel search cost

The first effect associated with the uniform pricing strategy is *reduced cross-channel search cost*. By setting the same price for a product across all channels, retailers can alleviate consumer concerns about discrepancies in prices of the same products across different channels. This reduces the need for consumers to search within the retailer (i.e., compare prices across channels), which can increase their utility and, ultimately, product sales.

A key component of an omnichannel strategy is the alignment of a product's value proposition across all retail channels. Price, as one of the most visible signals in the market, serves as a particularly pertinent indicator of product quality and the associated value proposition of a transaction (Milgrom & Roberts 1986). Implementing channel-specific pricing can cause consumers to question the consistency of value propositions of the same products across different channels, as well as the firm's predictability, trustworthiness, and reliability (e.g., Bolton et al., 2003; Fassnacht & Unterhuber 2016; Xia et al., 2004). Although price comparison across channels can be straightforward with mobile technology, awareness of price discrepancies may lead consumers to question whether they received lower value propositions (e.g., variation in services, accessories, etc.) and whether they are subject to price discrimination (Baye & Morgan 2001). Consequently, consumers may feel compelled to search across channels within the focal retailer, anticipating additional cross-channel costs associated with their purchase. The cross-channel search cost encompasses the time and effort required to compare prices of similar options (Urbany et al., 1996) and to evaluate the product's quality and fit with the consumers' idiosyncratic needs before making the purchase decision (Wang & Sahin 2018). These search costs also encompass the resources expended during the purchase process, including travel and time spent at the point of sale (Huang & Bronnenberg 2023; Narang et al. 2025). Furthermore, if consumers' preferred channel does not align with the channel offering a higher value proposition, they may need to make compromises, resulting in a suboptimal purchase decision (Iyengar & Lepper 2000). The expected cross-channel search costs and suboptimal channel choices diminish the utility consumers expect from transactions with the focal retailer.

By contrast, uniform pricing alleviates concerns about potential discrepancies in price and the perception of value propositions across channels (Chen & Cui 2013). Consumers can confidently expect to receive the same value propositions regardless of the purchasing channel, reducing the necessity for consumers to engage in cross-channel price comparisons (Dickson & Sawyer 1990; Friedman & Resnick 2001). With the guarantee, consumers can expect to save the additional time and effort typically spent on comparing the value proposition of the same product across channels (Gourville & Soman 2002). In addition, the uniform pricing strategy alleviates the dilemma in channel choice. When both channels offer the same value proposition, consumers can select the preferred channel and simplify the decision-making process, thereby maintaining the optimal utility from their purchases (Alter and Oppenheimer 2006). By eliminating the perceived value discrepancies across channels and simplifying cross-channel search, uniform pricing alleviates the expected cognitive, evaluative, and transactional burdens associated with the transaction. In turn, the reduced cross-channel search costs associated with uniform pricing increases product sales.

Uniform pricing and price discrimination

The second effect associated with uniform pricing is *loss of price discrimination*. One key advantage of the channel-specific pricing is that the retailer could charge different product prices across channels in response to channel-specific characteristics and competition (Gerstner et al., 1994). In contrast, uniform pricing is more restrictive because it removes the flexibility to set channel-specific prices. This loss of flexibility forces the retailer to adjust product prices, which can ultimately lead to either reduced competitiveness or lower profit margins.

Channel-specific pricing draws on the principles of price discrimination, serving as a strategic tool for retailers to segment their diverse consumer base across different channels. When retailers have insight into the varying levels of competition and price sensitivity in each channel, they can optimize prices to segment the market effectively (Chen et al. 2024). In channels with high competition or low search costs, for instance, consumers are likely to compare prices across multiple retailers before making a purchase. Channel-specific pricing allows the retailer to offer a discounted price in that channel to target consumers who are price-sensitive or willing to search. At the same time, the retailer can charge a higher price for the same product in another channel, targeting consumers who are less price-sensitive or less likely to search across retailers (e.g., Gerstner et al., 1994). Therefore, a channel-specific pricing strategy gives the retailer the flexibility to differentiate prices based on channel characteristics such as competition and search costs (Hoch et al., 1994).

By contrast, the uniform pricing strategy imposes more restrictions on pricing, resulting in loss of price discrimination for the retailer. Under uniform pricing, retailers cannot leverage channel-based segmentation and charge different prices across channels, but unify the prices to be consistent across all channels. Depending on the directions of price adjustment, the retailers might expect a sales increase (unified price above the higher-priced channel) or sales decrease (unified price below the lower-priced channel). Accordingly, retailers might either reduce prices and lose the retail margins in previously higher-priced channels, or they increase prices and lose attractiveness in previously lower-priced channels.

Furthermore, the sales gains and losses from this price adjustment are often asymmetrical, which can lead to a decrease in overall sales. Channels with lower prices typically have more price-sensitive consumers and stronger competition (Grewal et al., 2010; Khan & Jain 2005). When prices are raised in these channels, the highly sensitive consumers are more likely to seek better deals from competitors. As a result, the decrease in sales from the price increase in the lower-priced channel often outweighs the increase in sales from the price decrease in the higher-priced channel. Faced with this asymmetry, a common practice for retailers is to set the new unified price somewhere between the previous channel-specific prices to balance sales and margins (Kauffman et al., 2009). Therefore, due to the loss of price discrimination, we expect that the sales decreases from raising prices in the more sensitive, lower-priced channel will be greater than the sales gain from lowering prices in the less sensitive, higher-priced channel, resulting in an overall decrease in sales.

The dynamic effects

The overall impact of uniform pricing stems from the interplay between reduced cross-channel search cost and loss of price discrimination, and we expect the net effect to change over time. We argue that the effects of loss of price discrimination are immediate, while the effects of reduced cross-channel search cost are delayed. This difference in timing means that the overall impact of the policy will evolve.

The effect from reduced cross-channel search cost is likely to be delayed because it is subjective and experiential, meaning consumers must experience it firsthand to change their behavior. When a retailer first implements the policy, consumers may not immediately adapt their shopping habits, because the benefits of a service are difficult to evaluate without personal experience (Bolton & Drew 1991). This learning phase allows consumers to verify the same-price guarantee, build trust in the new policy, and eventually appreciate the reduced search costs (Mitra & Golder 2006; Venkatesh & Davis 2000). Therefore, we expect the impact of reduced cross-channel search cost on product sales to emerge gradually, as it takes time for consumers to learn about and trust the benefits of uniform pricing.

In contrast, the effects of loss of price discrimination—which manifest as a direct price change—are objective and immediately apparent to consumers. Unlike the experiential benefits of a consistent policy, a change in price is a concrete number that consumers can easily see and evaluate. Consumers rely heavily on price during their search process because it is one of the most accessible and comparable product attributes (Zhuang et al., 2021). When a retailer unifies its prices, consumers who compare prices across different retailers will notice the adjustment immediately. Consequently, even minor price changes can prompt immediate shifts in consumer decisions (Assuncao & Meyer 1993). Therefore, we expect any price adjustment associated with the loss of price discrimination (i.e., forced price adjustment to unify prices) to have an immediate influence on sales.

Uniform pricing in online and offline channels

We posit that the performance implications of a uniform pricing policy are contingent upon the retail channel due to fundamental differences in two forms of consumer search costs: cross-channel search costs (incurred when searching within the focal retailer) and cross-seller search costs (incurred when searching across different retailers). The potential benefit derived from uniform pricing is a function of cross-channel search cost, whereas consumer sensitivity to the policy's resultant price adjustments is dictated by cross-seller search costs. The asymmetrical nature of these costs across online and offline environments could lead to divergent treatment effects for each channel.

First, the channels differ with respect to cross-channel search cost, which is the cost associated with a consumer obtaining information from a retailer's alternative channel. We argue that these costs are substantially higher for online consumers seeking to validate information offline than for offline consumers searching online. Specifically, online consumers face significant impediments when engaging in offline search, including travel expenditures, time commitments, and geographical constraints (Berman & Katona 2013; Huang & Bronnenberg 2023). Conversely, the ubiquity of mobile technology grants offline consumers greater facility in conducting cross-channel comparisons online with minimal friction (Verhoef et al. 2007). Given that a uniform pricing strategy obviates the need for the more arduous search process, its utility is significantly greater for the online consumer. Accordingly, we expect that the increases in sales attributable to reduced cross-channel search cost will be more pronounced in the online channel than in the offline channel.

Second, the channels exhibit differing levels of cross-seller search cost, which refers to the costs of comparing product offerings across different retailers. It is well-established that such costs are substantially lower in the online environment, where consumers can readily compare offerings from numerous retailers, than in the offline environment, where consumers face the logistical burdens of physical travel (Lynch & Ariely 2000). This lower search cost in the online channel results in heightened price sensitivity among its consumers. Consequently, the effects of loss of price discrimination, which manifest as price adjustments, will have a more significant impact on this channel. Thus, we expect that the impact on sales due to these price adjustments will be stronger in online channels than in offline channels.

Research context and data

Institutional details

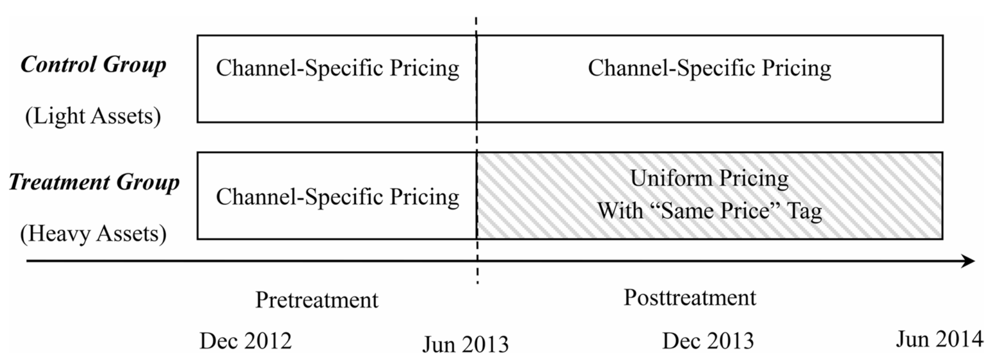
Our study leverages a pricing policy change in mid-2013 at a leading multichannel retailer in China's home appliance and consumer electronics market (similar to Best Buy). Established in 1990 as a brick-and-mortar retailer specializing in home appliances, the firm gradually diversified its product offerings and launched its online channel in 2009, becoming a fully integrated multichannel retailer. Its strategic position is notable, as it was the only major retailer in China at the time offering such a broad product array across both online and offline channels. By 2016, the retailer operated over 4,000 stores across Asia with total revenues of approximately US$56 billion.

The foundation of our quasi-experimental design lies in the retailer's two distinct inventory management models, which refers to as "asset mode". It is important to note that the heavy and light assets are not equivalent to the store-brand and manufacturer-brand examined in prior literature. In our sample, all products in both groups are national manufacturer brands, and the retailer does not carry any of its own store brands. The sole difference between the two groups lies in the contractual ownership of the inventory: heavy-asset inventory is owned by the retailer, while light-asset inventory is owned by the manufacturer and sold on a consignment basis. In the heavy asset mode, the retailer purchases inventory from manufacturers outright, and these products are recorded as assets on its balance sheet. In the light-asset mode, brand manufacturers retain ownership of the products even after they are physically transferred to the retailer; consequently, this inventory does not appear on the retailer's balance sheet. According to the firm, this dual-mode system was maintained for reasons related to financial management and the optimization of its balance sheet structure. Crucially for our research design, this internal operational distinction is entirely unobserved by consumers, who are unaware of a product's asset mode.

Before June 2013, the retailer used a channel-specific pricing strategy for all products. In 2013, online shares reached a meaningful 20% of the retailer's revenue, so the management team recognized the importance of providing a consistent online-offline experience. In the first week of June 2013, the retailer imposed uniform pricing on all products in its "heavy-asset" mode, while the brand manufacturers maintained their channel-specific pricing for products in the "light-asset" mode. Figure 1 illustrates the experiment setting.

Several points are relevant to our research design and identification strategy. First, the selection of heavy-asset products for the new policy was based on pre-existing contractual authority over pricing, which was tied to the ownership structure. The two asset modes are common in the consumer electronics and house appliance industry (Arc Team 2023). Specifically, heavy assets refer to inventory purchased by the retailer, where the brand manufacturer is not involved in the selling process; in contrast, light assets refer to inventory the retailer sells on behalf of the manufacturers, with both parties participating in the sale. The asset mode for each brand was decided long before the adoption of uniform pricing and was determined jointly by the retailer and the brand manufacturer through negotiations. These decisions were generally influenced by operational factors (e.g., commission fees, operational costs, profit sharing), product category, and the manufacturer's influence. As these decisions were made on a case-by-case basis, no single asset mode is generally preferred. Consequently, the retailer could freely adjust prices for heavy assets because

Fig. 1 Experimental design



it owned them, but it could not do the same for light assets. Thus, the differential implementation of the uniform pricing policy was the result of pre-existing contracts rather than a contemporaneous strategic choice by the retailer.

Furthermore, the retailer guaranteed that every participating product would have a consistent price across channels and tagged them with the "same-price guarantee" label. This price guarantee tag was implemented in both online and offline channels for all heavy-asset products, and the new pricing policy was publicly announced and advertised in all stores. This asset mode was therefore pre-determined at the brand level and, crucially, was unobserved by consumers before the policy change, providing a clean context for model identification.

Last, the retailer used the same campaigns, storewide promotions, and advertisements for products in the heavy- and light-asset modes during our sample period. The product assortments and display positions also remained the same before and after the pricing policy change. During the data window, none of the retailer's major competitors announced a similar pricing policy change during the sample period.

Therefore, we can leverage the differential implementation of the pricing policy as a quasi-experiment, free from confounding effects from the retailer's own strategic decisions. That is, we can determine the effect of switching from channel-specific to uniform pricing by measuring how sales changed after the policy changes for products in the heavy-asset mode (treatment group) relative to products in the light-asset mode (control group). Although the asset mode is predetermined and should be considered exogenous to the policy implementation, systematic differences between the products in each group may still exist. This possibility motivates our use of a sample matching algorithm to ensure comparability between the two groups.

Data and measurement

We collected transaction-level data from one major city in China. As we were interested in examining the treatment effects of the new pricing policy, it required a sample of products that were available in both the online and offline channels for a relatively long period. During the time of data collection, 4,150 products were available in both the online and offline stores (i.e., omnichannel products) during the 18-month period, given the limited capacity of the offline stores. Our sample includes products from all eight of the retailer's main categories: air conditioners (4.12%), kitchen appliances (e.g., refrigerators, grills; 7.61%), bath appliances (e.g., heaters, toilets; 6.10%), home theater (5.73%), personal electronics (e.g., cameras, earphones; 10.70%), computers and accessories (19.81%), communications equipment (e.g., mobile phones, tablets; 9.45%), and small appliances (e.g., hair dryers, toasters; 36.48%).

The research dataset includes the daily prices posted online and offline as well as daily transaction records of each product (0 if no transaction was made), yielding 2,249,300 daily records. For each transaction, we gathered the transaction time, online and offline retail prices, number of units sold, transaction channel, product information, and consumer demographics. For each product, we collected the daily posted prices online and offline. We aggregated the individual transactions into a panel data structure at the product-week level because of the sparsity of daily sales.

Figure 2 depicts the pre- and post-treatment cross-channel price differences between the control group (products in the light-asset mode, which retained channel-specific pricing; Panel A) and the treatment group (products in the heavy-asset mode, which switched to uniform pricing; Panel B). The horizontal axis represents the percentage difference between offline and online product prices. The vertical axis represents the percentage of products that fell into the indicated price difference range. For the control group (1,562 of 4,150 products), before the policy change, 39.82% of the products had a higher online price, and 60.18% had a higher offline price; the price difference distribution was similar after the policy change: 46.22% had a higher online price, and 53.78% had a higher offline price. For the treatment group (2,588 of 4,150 products), before the policy change, 44.40% of the products had a higher online price, 12.75% had a higher offline price, and 42.85% had the same online and offline prices; after the policy change, all products had the same price in the two channels. We report the definitions and summary statistics of the key variables in Table WA3 in Web Appendix A.

Research design and methodology

In our quasi-experimental context, we define the baseline condition as the six months before the treatment (i.e., December 2012-May 2013). We estimate the treatment effects over time by comparing weekly sales (units sold, count data) between the treatment and control groups over the subsequent 12-month period (June 2013-June 2014) using a Difference-in-Differences (DiD) design. Because the dependent variables are count data, we adopt the negative binomial model. Given the potential differences in product price and channel characteristics, such as competition, price sensitivity, and ease of search, we separately estimate the DiD model on sales in both online and offline channels to better account for these nuanced differences between the channels.

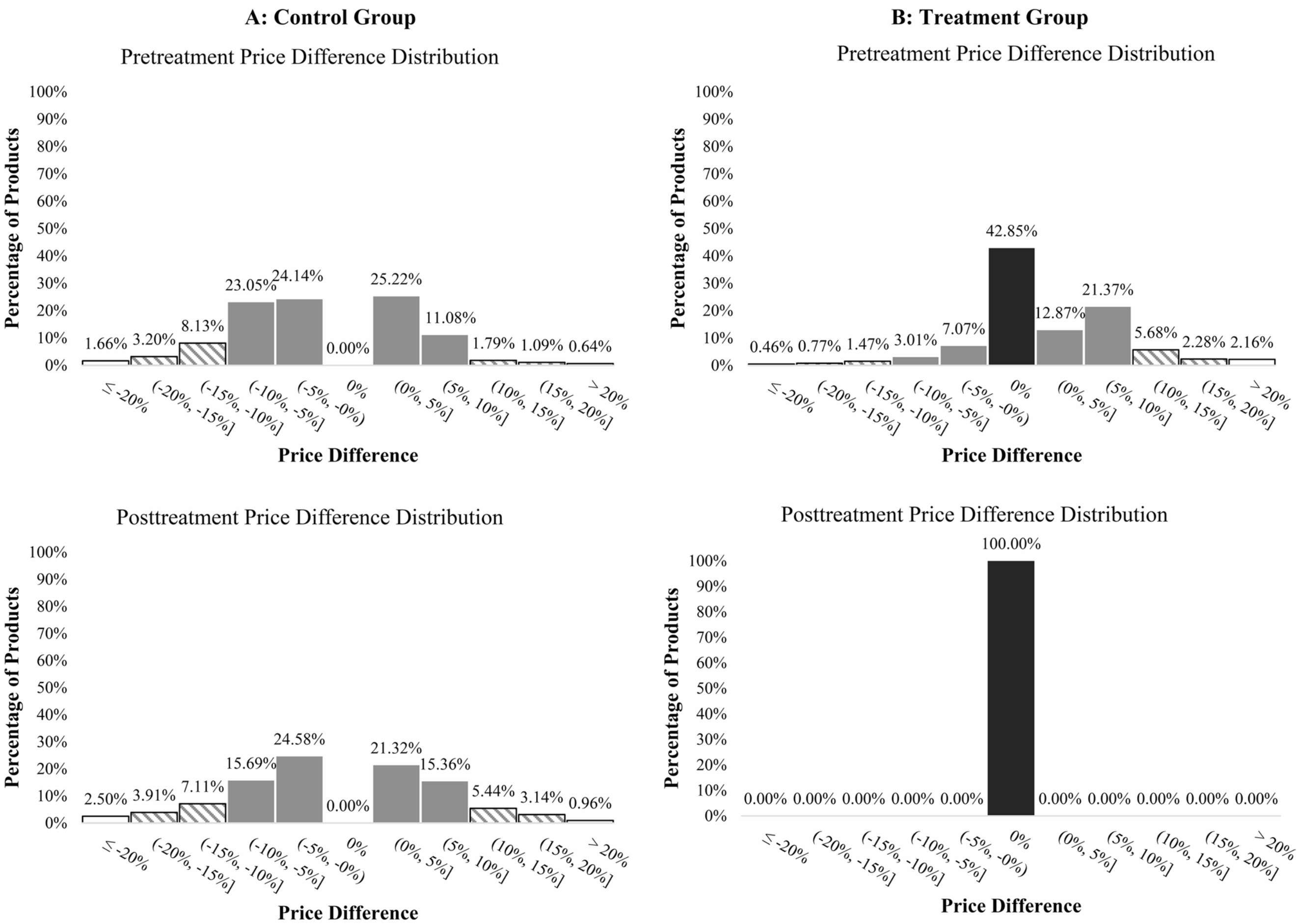


Fig. 2 Pricing distribution in the control group vs treatment group before and after treatment. (**A**) Control group, (**B**) Treatment group. Price difference = 2 x (online price - offline price)/(online price + offline price)

Identification strategy

Due to practical constraints, our setting is a quasi-experiment; assignment to the treatment and control groups was determined by the asset mode (heavy vs. light), which was established *ex ante*, as discussed previously. Our empirical strategy to establish the causal effect of the pricing policy change uses the DiD model, and the identifying assumption of a DiD model is the parallel trends assumption: if the treatment group had not been treated, it would have followed the same trajectory as the control group.

In our empirical setting, the retailer did not randomly assign products into different groups. As such, we might face an omitted variable or simultaneity issue that could lead the error term of the outcome variable to correlate with $Treat_i$ and the DiD estimator $Treat_i \times Post_t$. However, we argue that the nature of the group assignment and our proposed DiD model effectively account for such a bias, for two reasons. First, products were assigned to the light-asset (control) and heavy-asset (treatment) models at the time of brand manufacturers' entry into the retailer's marketplace. For all sampled products, the brand manufacturers' entry was at least one year before the pricing policy change and at least six months before the retailer proposed the change (early 2013). It is highly unlikely that the brand manufacturers had prior knowledge of the upcoming pricing policy change when signing contracts with the retailer. Furthermore, the group assignment is time-invariant, as none of the products switched asset modes during the data window. Therefore, even if some brand manufacturers had prior knowledge, product fixed effects should account for unobserved heterogeneities because the group assignment was a one-time decision. In other words, any self-selection was time-invariant, and any related unobserved heterogeneities should also be time-invariant, at least during the data window. Therefore, a product fixed effect should account for the time-invariant, product-specific unobserved heterogeneities that otherwise might bias our estimation, making the group assignment conditional exogenous to the outcome variables.

Second, the asset modes are not visible to consumers, and the retailer also did not carry out any promotional activities specifically targeting either asset. As we discussed previously, the product assortments and display positions did not change with the pricing policy, and the retailer did not differentiate between the heavy and light assets in store-wide business activities such as advertisements, campaigns, and promotions.[1] In other words, in terms of advertising and promotions, the retailer did not differentiate between heavy and light assets in the data window except for the treatment. To verify our theoretical premises, we compared the characteristics of consumers who purchased heavy and light assets in the pre-treatment period. The results (reported in Table WA4, Web Appendix A) confirm our premise that the two groups of consumers are largely comparable. Thus, the only difference between the two asset modes for consumers was the imposition of uniform pricing, identified by the "same-price guarantee" tag, which is the treatment we study.

Sample matching To account for the observed differences between the treatment and control groups, a commonly adopted approach is to match the two groups to create a sample that is balanced in observed heterogeneities. According to the retailer, a brand's asset mode is influenced by operational costs, product costs, the product category, and the influence of the brand manufacturers. To ensure that the treatment and control groups are comparable based on these factors, we use a sample matching algorithm that considers 10 observed covariates reflecting these key determinants: the average online, offline, and combined sales; average online and offline prices; online-offline price differences; average price fluctuation; product popularity; market competition; and product category. We use one-to-one propensity score matching (PSM) with a caliper size of 0.10 and report the results in Table 1. PSM effectively eliminates the imbalances in the observed covariates between the control and treatment groups. The matched sample contained 2,104 products and 173,628 product-week observations, and we used the matched sample for analysis. We plot the weekly aggregated sales of the treatment and control groups using the matched sample in Figure WA2 in Web Appendix A, showing that the treatment and control groups have visibly similar trends before the treatment.

Common trend test To uncover differential time trends (which would cast doubt on the validity of the parallel trends assumption), we compare the online, offline, and combined sales of the control and treatment groups during the pre-treatment period (week 1-week 25) by estimating the following negative binomial model:

$$Sale_{ijt} = NegativeBinomial(\gamma_0 + \gamma_1 Treat_i + \gamma_2 Treat_i \times Week_t + \sum \delta Week_t) \tag{1}$$

Table 1 Propensity score matching results

	Pre-Matching Sample			Post-Matching Sample		
	Control	Treatment	Difference	Control	Treatment	Difference
Distance	0.286	0.527	−0.241(0.007)***	0.409	0.416	−0.007(0.008)
Avg. Sales	3.644	3.228	0.416(0.028)***	3.470	3.456	0.014(0.040)
Avg. Sales_online	1.102	0.923	0.179(0.013)***	1.034	1.021	0.013(0.018)
Avg. Sales_offline	2.542	2.305	0.236(0.019)***	2.436	2.435	0.001(0.027)
Price_online	967.869	1908.907	−941.039(57.139)***	1298.411	1377.811	−79.400(71.709)
Price_offline	940.231	1973.482	−1033.252(58.192)***	1292.589	1379.152	−86.562(71.233)
Price Difference	0.013	−0.011	0.024(0.001)***	0.004	0.002	0.001(0.001)
Relative Price	−0.119	0.198	−0.317(0.031)***	0.082	0.077	0.004(0.042)
Popularity	591.247	558.790	32.241(53.134)	656.012	570.872	85.140(72.331)
Competition	1494.744	1557.712	−62.615(67.212)	1662.744	1553.100	109.644(97.056)
Category = 1	0.025	0.068	−0.043(0.007)***	0.036	0.036	0.000(0.008)
Category = 2	0.029	0.154	−0.124(0.010)***	0.064	0.072	−0.009(0.011)
Category = 3	0.047	0.075	−0.028(0.008)***	0.044	0.052	−0.009(0.009)
Category = 4	0.127	0.074	0.052(0.009)***	0.085	0.095	−0.010(0.012)
Category = 5	0.265	0.088	0.177(0.011)***	0.101	0.117	−0.016(0.014)
Category = 6	0.101	0.083	0.018(0.009)**	0.106	0.092	0.014(0.013)
Category = 7	0.354	0.383	−0.029(0.015)*	0.494	0.471	0.024(0.022)
Category = 8	0.052	0.076	−0.023(0.008)***	0.070	0.065	0.006(0.011)
Number of products	2,588	1,562		1,052	1,052	

*** $p<.01$, ** $p<.05$, * $p<.10$.

One-to-one nearest neighbor matching with caliper of 0.1 is adopted. Standard errors of differences between treatment and control groups are reported in the parentheses

[1] The flyer usually has footnotes with small font suggesting that some products are not eligible for the "same-price guarantee".

where i is the product, t is the week, j is the channel (online or offline), and $Week_t$ is the weekly dummy since the start of the observation period. The dummy variable $Treat_i$ equals 1 if product i belongs to the treatment group. The coefficient of interest is γ_3, which measures the difference in the sales trends of the treatment and control groups.

Figure 3 depicts the results of our parallel trend test (details reported in Table WA5 in Web Appendix A). The solid line represents the estimated coefficients, and the error bars represent the 95% confidence intervals. We plot the results for the pre-matching sample in panel A and for the post-matching sample in panel B. For the pre-matching sample, we find no statistically significant differences between the treatment and control groups, with the minor exception of a single week in the offline channel (Week 9) at the 95% confidence level. This finding confirms the comparability of the two groups, as they followed parallel trends even before the matching procedure. Furthermore, after PSM, the results for the post-matching sample show no statistically significant differences between the treatment and control groups for either online or offline sales during the entire pre-treatment period. This evidence confirms that the products in the two groups are comparable, and the sample satisfies the parallel trends assumption.

Difference-in-differences design

We anticipate two variations in sales: between the pre- and post-treatment periods and between the treatment and control groups. We estimate the following DiD model:

$$Sale_{ijt} = NegativeBinomial(\beta_0 + \beta_1 Treat_i \times Post_t + \beta_2 Price_{ijt} + \sum Product_i + \sum Time_i) \tag{2}$$

where $Sale_{ijt}$ is the number of units sold of product i during week t in channel j (online or offline), $Treat_i$ is a dummy variable indicating whether product i is in the treatment group, $Post_t$ is a dummy variable indicating whether week t is in the post-treatment period, and $Price_{ijt}$ is the weekly price of product i (normalized by the product's average price) that could capture the influences associated with product price fluctuation, such as product assortment and market competition. Finally, to capture unobserved heterogeneity, we include product and time fixed effects.

Results

Average treatment effects

We report the results in Table 2. Model 1 contains the overall treatment effects; we are most interested in the DiD estimators β_1, which capture the gross effects of the switch from channel-specific to uniform pricing on sales in each of the four quarters following the policy change. Model 2 examines the treatment effects over time, where we replace $Post_t$ with a vector ($Post1_t$, $Post2_t$, $Post3_t$, $Post4_t$) includes four dummy variables indicating the four quarters of the post-treatment period ($Post1_t = 1$ for June-August 2013, $Post2_t = 1$ for September-December 2013, $Post3_t = 1$ for January-March 2014, and $Post4_t = 1$ for April-June 2014).

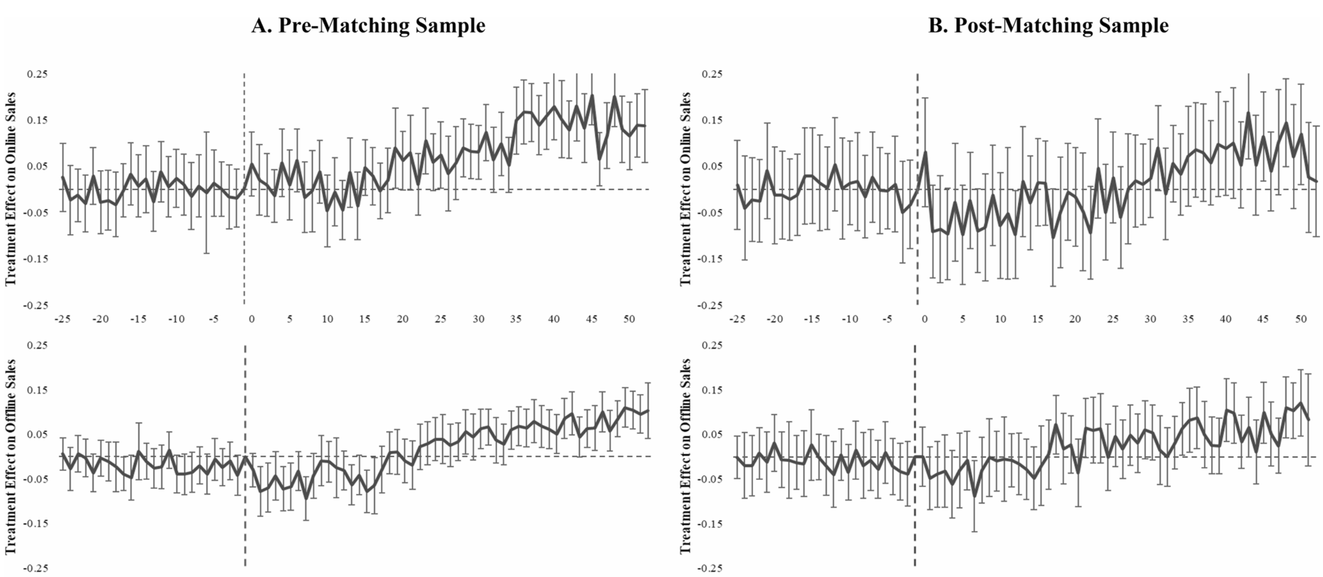


Fig. 3 Parallel trend analysis. (**A**) Pre-matching sample, (**B**) Post-matching sample. The error bars represent 95% confidence intervals. The dashed line represent the last week before the policy implementation

Table 2 Estimation result of the DiD model

Variables	Model 1		Model 2	
	Online Sales	Offline Sales	Online Sales	Offline Sales
Post×Treat	0.013(0.015)	0.035(0.010)***		
Post1×Treat			−0.058(0.019)***	−0.018(0.012)
Post2×Treat			−0.021(0.017)	0.022(0.012)*
Post3×Treat			0.035(0.014)**	0.055(0.011)**
Post4×Treat			0.088(0.021)***	0.077(0.013)***
Product fixed effects	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes
Sample size	164,112	164,112	164,112	164,112
Log-likelihood	−219137.253	−307793.644	−219098.702	−307757.638
AIC	442642.506	619955.288	442571.404	619889.276
BIC	449664.372	626977.154	449602.916	626920.788

*** $p<.01$, ** $p<.05$, * $p<.10$.

Main effects are excluded because of multicollinearity with the fixed effects. Brand-level clustered SEs are reported in parentheses

The results of Model 1 suggest that, in general, the policy change does not significantly affect online sales ($\beta_1=0.013$, $p>.10$) but increases offline sales ($\beta_1=0.035$, $p<.01$). Using the odds ratio, the policy change increases online sales by 1.31% and offline sales by 3.56%, suggesting that the policy change is overall beneficial to the retailer.

The dynamic treatment effects show a clear increasing pattern for both online sales and offline sales. The estimated results of Model 2 suggest that, in general, the switch from channel-specific to uniform pricing has a delayed (but not immediate) positive effect on both online and offline sales. Specifically, in the first quarter, the policy change decreases online sales by 5.64% ($\beta_{11}=-0.058$, $p<.01$). For the other three quarters, online sales are not significantly affected in the second quarter ($\beta_{12}=-0.021$, $p>.10$) but then increase significantly by 3.56% and 9.20% in the third and fourth quarters, respectively ($\beta_{13}=0.035$, $p<.05$; $\beta_{14}=0.088$, $p<.01$). While the policy change does not significantly affect offline sales in the first quarter ($\beta_{11}=-0.018$, $p>.10$), it increases these sales by 2.22%, 5.65%, and 8.00% in the second, third, and fourth quarters, respectively ($\beta_{12}=0.022$, $p<.10$; $\beta_{13}=0.055$, $p<.05$; $\beta_{14}=0.077$, $p<.01$). In other words, the retailer experienced a "warm-up" period after launching the uniform pricing policy; the trend is common for firms adopting new policies (Venkatesh & Davis 2000).

We draw three main conclusions from the mechanism analysis. First, the policy change to uniform pricing generally benefits the retailer, especially by increasing offline sales. This conclusion is consistent with industry evidence that consumers have a strong desire for unified online and offline prices (IBM, 2013). Second, the effect of adopting the uniform pricing strategy turns from negative to positive over time, suggesting strong dynamics in treatment effects. Third, the dynamics in treatment effects are stronger for the online channel than the offline channel.

Product-level spillover effects

Although the treatment in our empirical setting can be considered conditionally exogenous, a potential threat to our identification strategy is the possibility of spillover effects from the treated group to the control group. Because the new pricing policy was publicly announced, manufacturers of products in the control group could have strategically adjusted their own pricing or promotional efforts in response. To explore the potential influence of such spillover effects on our findings, we re-estimate our model with an additional term:

$$Sale_{ijt} = NegativeBinomial(\beta_0 + \beta_1 Post_t + \beta_2 Post_t \times Treat_i + \beta_3 Price_{ijt} + \sum Product_i) \quad (3)$$

where the dummy variable *Post* could capture the difference in sales of the products in the control group between the pre-treatment and post-treatment periods. We report the estimated results in Table WA6 in Web Appendix B. The analysis reveals two key findings. First, in Model 1, the spillover effects for both online and offline sales move in the same direction as the estimated treatment effect. This suggests that the presence of spillover likely causes our main estimate of the average treatment effect to be underestimated, making our findings more conservative. Second, Model 2 shows that for most post-treatment periods, the spillover effects have the same direction as the treatment effects, again suggesting our estimates are conservative. However, we note a few instances where the effects move in opposite directions (e.g., the first and fourth post-treatment periods for the online channel). In these specific cases, our treatment effects may be slightly overestimated, and we interpret them with appropriate caution.

In summary, this analysis suggests that product-level spillover between the treatment and control groups may exist. However, this spillover primarily serves to attenuate our main findings, indicating that our results are robust and likely represent a conservative estimate of the true treatment effect.

Rosenbaum bounds sensitivity test

While our sample matching procedure addresses bias from observed heterogeneity, our results could still be biased by unobserved factors that influence both the group assignment and its sales. Although the presence of such unobserved heterogeneity cannot be directly tested, its potential impact can be assessed using the Rosenbaum bounds sensitivity analysis (Rosenbaum, 2002). This analysis evaluates how strong an unobserved confounding variable would need to be to nullify the estimated treatment effect. The analysis is based on the parameter gamma (γ), where exp(γ) represents the odds ratio of differential treatment assignment due to an unobserved covariate. A gamma of 1 indicates no unobserved selection bias. We systematically increase gamma in increments of 0.10, calculating the *p*-value and confidence interval for the treatment effect at each step. The critical value of gamma is the point at which the effect is no longer statistically significant. A higher critical gamma indicates greater robustness to potential unobserved bias.

We report the results of this sensitivity analysis in Table WA7 in Web Appendix A. In Table 3, we focus on the five treatment effects that were statistically significant in our main analysis. The critical value of gamma at which our findings would be questioned is consistently 1.20 (with one exception). This means that an unobserved factor would need to increase the odds of a product being assigned to the treatment group by 20% to render our findings insignificant. This threshold is comparable to or exceeds those reported in prior marketing and management literature, which often consider values between 1.1 and 1.5 to indicate robustness (Bharath et al. 2011; DiPrete & Gangl 2004; Sun & Zhu 2013). Given that the Rosenbaum bounds analysis provides a conservative, "worst-case" assessment and does not account for the fact that our panel data structure already controls for time-invariant unobserved heterogeneity, we are confident that our results are robust to potential unobserved confounders.

Robustness checks

To ensure the validity of our findings, we conduct the following robustness checks.

Full sample Our primary analysis utilizes a matched sample created via propensity score matching (PSM) to ensure the treatment and control groups are balanced on observable characteristics. A potential limitation of this approach is that by excluding roughly half of the original observations, our findings might not be generalizable to the full product population. To address this concern, we re-estimate our main model using the full, unmatched sample. The results (Model 1 in Table WB1, Web Appendix B) are qualitatively consistent with those from the matched sample, suggesting that our findings are not an artifact of the matching procedure and are broadly representative.

Daily aggregates In our main analysis, we aggregate daily transaction data to the weekly level to mitigate issues arising from excessive zero-sale days. However, such aggregation could potentially obscure important information contained in daily price variations. To test the robustness of the findings, we re-estimate the model using all 2,249,300 product-day observations. The results (Model 2 in Table WB1, Web Appendix B) remain qualitatively consistent with our main findings, confirming that the weekly aggregation does not drive our conclusions.

Anticipation and grace period Our analysis defines the pre- and post-treatment periods using the official policy implementation date. However, consumer or firm behavior might have shifted immediately before or after this date. We consider two possible influences: (1) an anticipation period, where consumers adjusted their behavior in advance of the policy change, and (2) a grace period, where the retailer might have adjusted its communications or honored previous prices. We re-estimate our model twice: first, excluding the four weeks immediately preceding the policy change (weeks 22–25; Model 3 in Table WB1), and second, excluding the four weeks immediately following it (weeks 26–29; Model 4 in Table WB1). In both cases, the results are qualitatively consistent with our main analysis, suggesting that our findings are not driven by behavioral anomalies around the time of the policy implementation.

Revenue as the outcome Our main analysis focuses on product sales (units sold). Because the policy change directly involves price, it is crucial to assess its impact on revenue. We conduct another robustness check using the natural logarithm of revenue ($Revenue_{ijt} = Sales_{ijt} \times Price_{ijt}$) as the dependent variable. The results (Table WB2, Web Appendix B) are consistent with our primary findings. This confirms that the positive effect on sales translates into a positive effect on revenue and that our conclusions are robust to the choice of the outcome variable.

Pooled model Our primary analysis estimates separate models for the online and offline channels to account for channel-specific nuances. However, this approach does not

Table 3 Mechanism testing

Variables	Model 1			Model 2		
	Sales	Online Sales	Offline Sales	Sales	Online Sales	Offline Sales
Post×Treat	0.053(0.013)***	0.072(0.018)***	0.052(0.013)***			
Post×Treat×Online	0.016(0.016)					
Post×PriceChange	−0.024(0.016)	−0.089(0.021)***	−0.025(0.016)			
Post×PriceChange×Online	−0.061(0.018)***					
Post1×Treat				0.018(0.016)	0.030(0.025)	0.017(0.016)
Post2×Treat				0.058(0.016)***	0.060(0.023)***	0.056(0.016)***
Post3×Treat				0.076(0.017)***	0.075(0.019)***	0.074(0.017)***
Post4×Treat				0.060(0.014)***	0.123(0.026)***	0.058(0.014)***
Post1×Treat×Online				0.009(0.024)		
Post2×Treat×Online				−0.000(0.023)		
Post3×Treat×Online				−0.007(0.019)		
Post4×Treat×Online				0.062(0.024)***		
Post1×PriceChange				−0.051(0.021)**	−0.134(0.029)***	−0.052(0.021)**
Post2×PriceChange				−0.048(0.018)**	−0.123(0.027)***	−0.050(0.019)**
Post3×PriceChange				−0.028(0.020)	−0.059(0.025)**	−0.029(0.020)
Post4×PriceChange				0.026(0.020)	−0.051(0.027)*	0.027(0.020)
Post1×PriceChange×Online				−0.081(0.029)**		
Post2×PriceChange×Online				−0.070(0.029)**		
Post3×PriceChange×Online				−0.028(0.024)		
Post4×PriceChange×Online				−0.074(0.026)***		
Product fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	328,224	164,112	164,112	328,224	164,112	164,112
Log-likelihood	−531749.346	−219123.347	−307791.237	−531556.942	−219077.238	−307742.265
AIC	1067878.692	442616.694	619952.474	1067521.884	442536.476	619866.530
BIC	1075579.105	449641.776	626977.556	1075271.523	449580.848	626910.902

*** p<.01, ** p<.05, * p<.10.

Main effects are excluded because of multicollinearity with the fixed effects. Brand-level clustered SEs are reported in parentheses permit a formal statistical test of the difference between the treatment effects in each channel. To address this, we estimate a pooled model combining data from both channels. We include an interaction term between our DiD estimator and a dummy variable for the online channel. In this specification (Table WB3, Web Appendix B), the coefficient on the DiD estimator represents the treatment effect for the offline channel, while the sum of that coefficient and the interaction term's coefficient represents the effect for the online channel. The results confirm a statistically significant difference between the channels, consistent with the conclusions drawn from the main analysis.

Linear model Our main analysis employs a negative binomial model, which is appropriate for count data. However, the marginal effects of interaction terms in nonlinear models can be complex to interpret and potentially inconsistent with coefficient signs (Ai & Norton 2003). To ensure our conclusions are not an artifact of this specification, we re-estimate our model using both a standard linear model (OLS) and a log-linear model. The results, reported in Table WB4 (Web Appendix B), are quantitatively consistent with those from the negative binomial model. The results verify that the marginal effects are similar to the results of nonlinear models.

Within-category unobserved heterogeneity In the original analysis, we conducted sample matching and included two-way fixed effects to account for unobserved heterogeneity. However, unobserved heterogeneity (e.g., new entry, market fluctuations) may affect all products within a certain category, and our current model specification cannot effectively account for them. To address this issue, we adopt two approaches. First, we refine our matching procedure, performing PSM within each of the eight product categories separately before re-estimating the model (Model 1 of Table WB5, Web Appendix B). Second, we add category-week interaction fixed effects to our main specification to absorb any category-specific time trends (Model 2 of Table WB5, Web Appendix B). In both cases, the results are largely consistent with our primary findings, suggesting our conclusions are robust to unobserved heterogeneity at the product-category level.

Consumer-level spillover effects A potential threat to our identification is consumer-level spillover, wherein consumers may compare products in the treatment and control groups, thereby biasing the estimated treatment effect. To mitigate this concern, we employ a cross-category matching approach. This method is designed to reduce the likelihood of direct comparison by operating on the assumption that consumers are less likely to compare products across different categories (e.g., a television for a microwave) than within the same category. Specifically, for each product in the treatment group, we constructed a new control group by matching it with products from the other seven product categories, and the intensity of treatment differs across categories (from 24% to 84% of products were treated). This procedure resulted in a matched sample of 1,565 treated products and 971 control products. The results from this analysis (Table WB6, Web Appendix B) are qualitatively consistent with our main findings. While this approach cannot eliminate all potential for substitution, the consistency of the results substantially mitigates concerns that our main findings are driven by consumer-level spillover effects.

Advertising spillover effects Brand manufacturers and the retailer might also have different marketing strategies, which will further affect the identification of the uniform pricing strategy. To rule this out, we segment all products into three tiers (top, medium, bottom) based on their pre-treatment sales volume. We then interact these tier dummies with our DiD estimator. The results (Table WB7, Web Appendix B) show that the vast majority of these interaction terms are not statistically significant. This indicates that our findings are not driven by systematic differences in marketing efforts between high- and low-performing products.

Mechanism testing

As we discussed in the theoretical section, the effect of the pricing policy change is driven by two contrasting mechanisms: cross-channel search cost and price discrimination. Reduced cross-channel search cost corresponds to the effect of the same-price guarantee, which reduces consumers' need to search within the retailer.[2] loss of price discrimination refers to consumers' reactions to the price adjustment required by the uniform pricing policy.

To disentangle these two effects, we leverage the natural variation within our treatment group. Specifically, 1,109 products (26.72% of the sample) already had the same online and offline prices before the policy change. For these products, the policy change did not involve an actual price adjustment and only consisted of adding the "same-price guarantee" label. The remaining 1,479 products (35.64%) required an actual price adjustment to comply with the uniform pricing policy. To identify the effect of this adjustment, we create the dummy variable *PriceChange*, which identifies products that required a change to their online and/or offline prices. For treated products that receive only the guarantee (no price adjustment), *Treat* equals 1 and

[2] In Web Appendix C, we conducted a complementary lab experiment to show that consumers indeed associate the same-price guarantee with greater ease of search.

PriceChange equals 0. For treated products that received both the guarantee and a price adjustment, Treat equals 1 and *PriceChange* is 1. For the control group, both variables are 0. Accordingly, we can identify the effect of reduced c through the coefficients on *Post* × *Treat* and the effect of forced price adjustment (due to loss of price discrimination) through the coefficients on *Post* × *PriceChange*.

Moreover, two types of price adjustments occurred in our sample. The first is the forced price adjustment due to loss of price discrimination, where a product's online and offline prices were unified to follow the uniform pricing policy. The second is active price adjustment, where a product's price changed due to other factors, such as market competition, that are unrelated to the policy change. To account for the effects of these active price adjustments, we include the unit price, *Price*, as a covariate in the model. For each product, we first average its daily prices at the week level. Then, to avoid multicollinearity with the product fixed effects, we normalize this weekly price by dividing it by the average price of that product over the entire sample period. Therefore, our normalized Price variable captures the effects of routine price adjustments due to factors other than the policy change. We specify the augmented research model as follows:

$$\begin{gathered} Sale_{ijt} = NegativeBinomial(\beta_0 + \beta_1 Post_t \times \\ Treat_i + \beta_2 Post_t \times Treat_i \times Online_j + \\ \beta_3 Post_t \times PriceChange_i + \beta_4 Post_t \times \\ PriceChange_i \times Online_j + \beta_5 Treat_i \times \\ Online_j + \beta_6 PriceChange_i \times Online_j + \\ \beta_7 Online_j + \beta_8 Price_{ijt} + \sum Product_i + \sum Time_i) \end{gathered} \tag{4}$$

where $Sale_{ijt}$ is the number of units sold of product i during week t in channel j (online or offline), $Post_t$ is a dummy variable indicating whether week t is in the post-treatment period, $Treat_i$ is a dummy variable indicating whether product i receives the "same-price guarantee" label, the dummy variable $PriceChange_i$ indicates whether product i underwent price adjustment due to the policy change, the dummy variable $Online_j$ indicates the retail channel to which the observation belongs, and $Price_{ijt}$ is the weekly price of product i (normalized by the product's average price), which controls for active price adjustments.

Results Table 3 reports the results. The coefficients of interest are $Post_t \times Treat_i$, which captures the effect of reduced cross-channel search cost, and $Post_t \times PriceChange_i$, which captures the effect of forced price adjustment. The three-way interaction terms $Post_t \times Treat_i \times Online_j$ and $Post_t \times PriceChange_i \times Online_j$ capture the differential effects between the online and offline channels.

The estimation results are consistent with our theorization. The results in Model 1 suggest that reduced cross-channel search cost significantly increased sales in both the online (β_1=0.072, p<.01) and offline channels (β_1=0.052, p<.01), although the difference between the channels was not statistically significant (β_2=0.016, p>.10). In contrast, forced price adjustment significantly decreased online sales (β_3=−0.089, p<.01) but not offline sales (β_3=−0.025, p>.10). This negative effect was significantly stronger for the online channel (β_4=−0.061, p<.01). We also use the odds ratio ([exp(β) −1] × 100%) to interpret the results. Overall, the reduced cross-channel search cost increased online sales by 7.47% and offline sales by 5.34%, while loss of price discrimination decreased online sales by 8.52% and offline sales by 2.47%.

We now turn to Model 2 to investigate the dynamics of the two effects. The results reveal that the positive effect of the reduced cross-channel search cost grew over time. In the first quarter, the effect was not statistically significant in either online (β_{11}=0.030, p>.10) or offline channels (β_{11}=0.017, p>.10). However, from the second quarter onward, the effect became positive and significant, with the magnitude increasing over the subsequent periods for both online (online: β_{12}=0.060, p<.01; β_{13}=0.075, p<.01; β_{14}=0.123, p<.01) and offline sales (β_{12}=0.056, p<.01; β_{13}=0.074, p<.01; β_{14}=0.058, p<.01). Furthermore, this positive effect was significantly stronger for the online channel in the second, third, and fourth quarters (β_{21}=0.018, p>.10; β_{22}=0.058, p<.01; β_{23}=0.076, p<.01; β_{24}=0.060, p<.01).

In contrast, loss of price discrimination had an immediate negative effect that diminished over time. In the first quarter, this mechanism had its largest impact, decreasing online sales by 12.54% (β_{31}=−0.134, p<.01) and offline sales by 5.07% (β_{31}=−0.052, p<.05). For the online channel, this negative effect persisted but weakened over the year, becoming marginal by the fourth quarter (β_{32}=−0.123, p<.01; β_{33}=−0.059, p<.05; β_{34}=−0.051, p<.10). For the offline channel, the negative effect became statistically insignificant after the second quarter (β_{32}=−0.050, p<.05; β_{33}=−0.029, p>.10; β_{34}=0.027, p>.10). The negative impact was also significantly stronger for the online channel than the offline channel in nearly all periods. (β_{41}=−0.081, p<.05; β_{42}=−0.070, p<.01; β_{43}=−0.028, p>.10; β_{44}=−0.074, p<.05).

This mechanism test explains the delayed positive effect of the policy change observed in our main model. For products that only received the "same-price guarantee" label, the policy led to a steadily growing increase in sales, driven by the positive effect of reduced cross-channel search cost. For products that also underwent a forced price adjustment, the policy initially triggered a negative market reaction, particularly online, due to loss of price discrimination. Over time, as consumers learned to value the benefits of the policy, the effect of reduced cross-channel search cost began to outweigh the effect of loss of price discrimination, causing the overall treatment effect to turn positive. This dynamic was more pronounced in the online channel, which experienced both a stronger negative immediate effect and a stronger delayed positive effect.

Intensity of price adjustment Previously, we included the dummy variable *PriceChange* to account for the effects of price adjustment due to loss of price discrimination. However, this approach treats all price adjustments equally, regardless of their magnitude or direction. To further explore the effects of forced price adjustment, we introduce the intensity of price adjustment of product i in channel j; that is, $\Delta Price_{ij} = (Price_{ij,\,t0} - Price_{ij,\,t0-1})/Price_{ij,\,t0-1}$, where $t0$ is the first week after treatment (week 26). We re-estimate the model and include the interaction term between the intensity of price adjustment and the four post-treatment period dummies (we exclude the main effect because it is captured by the product fixed effects). The results, reported in Table WA8 of Web Appendix A, support our framework. As expected, after including *ΔPrice*, the coefficients on the original *PriceChange* dummy are attenuated, as the new variable captures the direct effect of the price change more precisely. The significant negative coefficients of the interaction between *ΔPrice* and the period dummies confirm that sales were negatively impacted in channels where prices increased and positively impacted where prices decreased. Importantly, the estimated effects of the "same-price guarantee" (*Period* × *Treat*) remain quantitatively consistent with our main findings, indicating the robustness of our findings.

Price increases versus decreases Next, we test whether consumers react differently to price increases versus price decreases. We decomposed the forced price adjustment into price increases and price decreases to determine whether consumers react to these price adjustments in the same way. We decomposed $\Delta Price_{ij}$ into $\Delta PriceIncrease_{ij}=\Delta Price_{ij}$ if $\Delta Price_{ij} > 0$ and $\Delta PriceDecrease_{ij}=\Delta Price_{ij}$ if $\Delta Price_{ij} < 0$. Re-estimating the model with these separate variables yields several insights (reported in Table WA9, Web Appendix A). First, online consumers are significantly more sensitive to price decreases than price increases, suggesting an asymmetric response. Second, offline consumers exhibit a more symmetric response, reacting relatively similarly to both increases and decreases. Third, consistent with our main findings, offline consumers are generally less sensitive to price adjustments overall than online consumers. In summary, these additional findings lend further support to our theorization. They highlight the significant, immediate impact of price adjustments—particularly in the online channel—and reinforce our conclusion that the overall positive effect of the uniform pricing policy is driven by the delayed, but ultimately powerful, benefits of reduced cross-channel search cost.

Consumer-level analysis

Our product-level analysis revealed that the treatment effect evolved from negative to positive over the 12-month post-treatment period. This aggregate finding raises a critical question: how do individual consumers react to the pricing policy change? A policy shift could either cause existing consumers to adapt their behavior or alter the composition of the retailer's customer base altogether. Our theoretical framework suggests a tension between two distinct consumer responses: an immediate negative reaction to loss of price discrimination (driving some consumers away) and a delayed positive reaction to reduced cross-channel search cost (as other consumers learn to value the consistent experience). Distinguishing between these behavioral patterns requires an analysis at the consumer level. Therefore, we conduct a consumer-level analysis to investigate the effects of the pricing policy change on purchasing behavior. (We report details of the data in Web Appendix D and summary statistics in Table WD1.) In our sample, 56.53% of consumers bought products from both treated and control groups. On average, a consumer bought 1.144 orders per quarter for the products in both treated and control groups; therefore, the consumer-level analysis was estimated based on 6.864 orders, which should be considered substantial for the consumer electronics and home appliance industry.

Model specification

The dependent variable (i.e., units of product sold) is a discrete variable, so we adopt a finite mixture model with a negative binomial framework to analyze the individual consumer-level data. Conditional on a finite mixture of K consumer segments, we specify the likelihood function of observing consumer i's units purchased in product group j (treated or control) at time t as

$$f\left(Sales_{ijt}\right) = \sum_{k=1}^{k} \lambda_{ik} \prod_{t=1}^{T} \prod_{j=1}^{2} f\left(Sales_{ijt} \middle| \lambda_{i} = k\right) \tag{5}$$

where $Sales_{ijt}$ are the units of products affected by uniform pricing ($j=1$) or units of products not affected by uniform pricing ($j=0$) purchased by consumer i at time t; k is the latent consumer class, where $k=1, \ldots, K$; t is the time of observation, where $t=0, 1, \ldots, T$; and $\lambda_i = (\lambda_{i1}, \lambda_{i2}, \ldots \lambda_{iK})$ is the vector of the K independent probability that consumer i belongs to latent-class k, defined as:

Table 4. Estimation result for consumer-level analysis

	Segment 1	Segment 2	Segment 3	Segment 4	Segment 5
Constant	1.155(0.005)***	0.001(0.016)	0.000(0.012)	0.207(0.002)***	0.000(0.009)
Group	−0.270(0.007)***	−0.000(0.020)	−0.000(0.014)	0.536(0.002)***	0.693(0.010)***
Group×Post1	0.229(0.011)***	0.693(0.023)***	−0.000(0.025)	−0.139(0.004)***	−0.695(0.013)***
Group×Post2	0.399(0.011)***	0.000(0.033)	0.693(0.015)***	−0.086(0.004)***	−0.693(0.015)***
Prob(λ)	0.181(0.001)***	0.030(0.154)	0.015(0.090)	0.736(0.002)***	0.038(0.153)
Consumer Characteristics					
Tenure	982.191	597.838	595.119	812.207	603.805
Age	43.604	35.580	35.230	39.432	35.431
Member	1.671	0.470	0.512	1.082	0.495
Pre-treatment orders	5.426	2.247	2.535	3.675	3.545

*** p<.01, ** p<.05, * p<.10.

In the upper panel, other covariances are not reported for parsimony. Classification in the lower panel is based on the largest posterior probability of the consumers

$$\lambda_{ik} = \frac{\prod_{t=1}^{T_i}\prod_{j=1}^{2} NB\left(u_{ijtk}\right)}{\sum_{q=1}^{K}\prod_{t=1}^{T_i}\prod_{j=1}^{2} NB\left(u_{ijtq}\right)} \quad and \quad \sum_{k=1}^{K}\lambda_{ik} = 1 \tag{6}$$

Given the individual probability λ_{ik}, we can calculate the mixing proportions of latent segments in the population as $\lambda_k = \sum_{i=1}^{N} \lambda_{ik}/N$, where N is the total number of consumers. We specify the likelihood function of dependent variable $Sales_{ijt}$ as

$$f\left(Sales_{ijt}|\lambda_i = k\right) = exp(\beta_{0k} + \beta_{k,1}Group_j + \beta_{k,2-3} Period_j \times Group_j + \beta_{4-5}Period_t + \beta X_i + \epsilon_{k,ijt}) \tag{7}$$

where $Group_j$ is a dummy variable indicating whether consumers in product group j are affected by the policy change; $\boldsymbol{Post_t} = (Post1_t, Post2_t)$ is a vector of two dummy variables indicating the two post-treatment periods ($Post1_t$=1 for June-December 2013 and $Post2_t$=1 for January-June 2014); $\boldsymbol{X_i}$ is a vector of variables containing consumer i's characteristics, including age, gender, membership level, and tenure; and $\varepsilon_{k,\,ijt}$ is the error term for latent-class k and follows a normal distribution.

Estimation results

We use the expectation-maximization algorithm to estimate the proposed models with two to eight latent segments, and the model with five latent segments has the best fit (for details, see Table WD2, Web Appendix D). The estimation results are presented in the upper panel of Table 4.

The results suggest that adopting uniform pricing leads to two distinct patterns of reactions: one group of consumers who dislike uniform pricing and one group that prefers the new uniform pricing. Our analysis identifies five segments. The most positive response to the adoption of uniform pricing occurs among the first three segments of consumers (segment 1: 18.1% of the population; segment 2: 3.0%; segment 3: 1.5%). This is reflected in a 39.89% weighted-average increase in product sales (segment 1: β_{13}=0.229, p<.01; β_{14}=0.399, p<.01; segment 2: β_{23}=0.693, p<.01; β_{24}=0.000, p>.10; segment 3: β_{33}=−0.000, p>.10; β_{34}=0.693, p<.01). The most negative response occurred among the fourth and fifth segments of consumers (segment 4: 73.6% of the population; segment 5: 3.8%). This is reflected in a 12.54% weighted-average decrease in product sales (β_{43}=−0.139, p<.01; β_{44}=−0.086, p<.01; β_{53}=−0.695, p<.01; β_{54}=−0.693, p<.01). In summary, the consumer-level analysis suggests that the findings of the product-level analysis are due to the two distinct behaviors of consumers—an increasing boost in the shopping frequency of 22.6% consumers and an immediate decrease in the shopping frequency of 77.4% consumers. The results lend support to our theorization that the former consumers appreciate the ease of shopping and the comprehensiveness of the omnichannel experience while the latter consumers stop buying from the focal retailer due to loss of price discrimination.[3]

To further explain the varying effects of the uniform pricing strategy across consumer segments, we compared four consumer characteristics (lower panel of Table 4.): tenure, age, membership level, and pre-treatment orders. We find a significant difference between the two largest segments representing the two types of consumers (segments 1 and 4). Specifically, consumers who prefer uniform pricing are those who have longer tenure (segment 1: 982.191 days vs.

[3] Consumers with decreasing sales might not necessarily mean the consumers left the retailer, but it might indicate a reduced purchase frequency of products in the treatment condition. The consumers might redirect their spending toward the products in control conditions, purchasing from competitors, or forgoing a purchase entirely. Due to data limitations, we cannot empirically distinguish between these alternative behaviors.

segment 4: 812.207 days; $t=32.734$, $p<.01$), are older in age (segment 1: 43.604 days vs. segment 4: 39.432 days; $t=56.077$, $p<.01$), have a higher membership level (segment 1: 1.671 vs. segment 4: 1.081; $t=54.990$, $p<.01$), and purchased (nonsignificant) more before the treatment (segment 1: 5.426 orders vs. segment 4: 3.675 orders; $t=0.945$, $p>.10$). The between-segment comparison further supports our theorization that consumers who prefer uniform pricing are more loyal and have a higher value than those who dislike uniform pricing.

Discussion

Theoretical contributions

We contribute to the channel integration literature, now one of the top managerial challenges faced by retailers (Gu & Tayi 2017). First, online-offline channel integration is quickly becoming one of the top managerial challenges for retailers (Cao & Li 2015; Gu & Tayi 2017), and existing research has examined several online-offline integration strategies, including showroom effects (Bell et al., 2017; Wang & Goldfarb 2017), product placement (Gu & Tayi 2017), and offline-to-online targeting (Luo et al., 2020). Our study is among the first to empirically examine the effect of switching from channel-specific pricing to uniform pricing on a firm's sales performance. Second, our study uncovers novel temporal and cross-channel dynamics in omnichannel pricing. We demonstrate that the online channel reacts more strongly to the policy change than the offline channel, a finding that holds for both the immediate effects of loss of price discrimination and the delayed effects of reduced cross-channel search cost. This provides a more granular understanding of how and why the benefits of such policies vary across time and across channels. Third, we offer a nuanced, micro-level explanation for these aggregate sales dynamics. Our consumer-level analysis reveals that the policy's success is not driven by a uniform positive response. Instead, it is the net result of two opposing behaviors: a significant decrease in purchasing from a large segment of price-sensitive consumers, and a substantial increase in purchasing from a smaller, more loyal, and higher-value consumer segment.

Furthermore, we also contribute to the price discrimination literature by providing a more nuanced understanding of the trade-offs involved in channel-based pricing. Existing research has extensively examined channel-specific pricing as a form of third-degree price discrimination, focusing on how firms can maximize profit by segmenting consumers across channels with varying price sensitivities (Cavallo, 2017; Gerstner et al., 1994; Hinz et al., 2011). This traditional perspective, however, largely overlooks the potential negative externalities of such a strategy in an integrated omnichannel environment. Our research identifies and quantifies a significant countervailing mechanism: the positive effect of reduced cross-channel search cost. We empirically disentangle these two forces—the well-documented effect of loss of price discrimination and the previously unexamined positive effect of reduced cross-channel search cost. In doing so, we demonstrate that the long-term gains from providing a seamless omnichannel experience can outweigh the short-term benefits of price discrimination, challenging the conventional wisdom on optimal pricing strategies in a multichannel world.

It is important to acknowledge that the relative strength of the two mechanisms we identify—reduced cross-channel search cost and loss of price discrimination—is likely moderated by the broader technological and competitive context. For example, retailers that adopt a price-matching strategy (matching competitors' prices) may experience a weaker negative impact from loss of price discrimination. This is because uniform pricing can remain competitive for price-sensitive consumers by matching competitors' prices. In addition, with the advancement of mobile connections and omnichannel retailing in recent years, consumers are more likely to notice potential price discrepancies across different channels and become more motivated to compare prices across retailers (Gevelber, 2016). This situation increases both the price competition among similar retailers and the need to alleviate concerns about price discrimination across channels. Consequently, the impact of a uniform pricing strategy might not be universal. It is highly contingent upon the specific empirical context, including the level of market competition and the portfolio of other omnichannel policies the retailer has implemented.

Managerial implications

A firm's pricing strategy has critical ramifications for firm performance, market competition, and consumer relationships. Our research has implications for firms that want to synergize their operations across online and offline channels but fear the loss of price discrimination and product competitiveness (Kireyev et al., 2017). We summarize our study's implications in the following three key takeaways.

Should multichannel retailers switch to the uniform pricing strategy and advertise a "same price guarantee"? The short answer is "yes," but with an important and cautious consideration. Our findings show a clear trade-off in time: switching to uniform pricing leads to an immediate sales drop followed by a delayed but significant sales increase. This effect is stronger for the online channel, which suffers a larger initial sales loss and takes longer to become profitable under the uniform pricing strategy. To provide more detailed implications regarding the timing of the transition period, we conducted a post-hoc analysis (post-hoc analysis 1 in Web Appendix E). The results show a guide for this "breakeven" point (Table WE1, Web Appendix E), showing that it arrives sooner for retailers with a higher proportion of offline sales—retailers with only online sales should expect the cumulative sales to increase after 43 weeks of implementing the uniform pricing strategy, while retailers with only offline sales should expect the cumulative sales to be positive only after 24 weeks of uniform pricing implementation. Managers should consider if they can withstand the initial period of negative returns before the benefits of uniform pricing take hold.

How should the omnichannel retailers determine the unified prices if they adopt the uniform pricing strategy? Our analysis suggests the transition to uniform pricing is most successful when the original price differences between online and offline channels are small. Adopting a uniform price forces a retailer to make adjustments to obtain the unified prices, and our research shows that large price increases in any channel can significantly harm sales. To illustrate the expected outcome of adopting uniform pricing and subsequent price adjustment, we conducted another post-hoc analysis and simulation (post-hoc analysis 2 in Web Appendix E) regarding different directions and levels of price adjustments. The results (Table WE2, Web Appendix E) suggest that small price increases (e.g., under 10%) have a manageable negative effect on sales (−2.0% for offline sales and + 0.6% for online sales). However, larger increases (e.g., over 20%) lead to substantial sales drops (more than 6.2% of sales decreases) that the benefits of consistency cannot compensate for in the 12-month period. Thus, the adoption of uniform pricing is overall beneficial for retailers that have close channel-specific optimal prices. If the initial price adjustment is severe, managers might need to balance the impact on margin rates and product sales to optimize overall profits.

How does the uniform pricing strategy change consumer behavior? Our consumer-level analysis suggests that the shift to uniform pricing results in two distinct types of reactions. One group of consumers is less likely to choose the products adopting uniform pricing, which might be due to the associated price adjustment as other retailers might offer lower prices too. The other group of consumers, by contrast, is more likely to select the products adopting uniform pricing, as they gradually recognize the value of the consistent omnichannel shopping experience. Our post-hoc descriptive statistics suggest that the latter group generally shops more, is older, and has a higher level of membership than the former group. Retailers that wish to implement the uniform pricing strategy should consider the current composition of consumers and determine the price adjustment strategy accordingly.

Limitations and future research directions

Several limitations of this study suggest worthwhile future research opportunities. First, a key limitation of this research is that we do not analyze how consumer heterogeneity interacts with the firm-level policy change, given the individual-level data sparsity issue. Future research could try to further unearth the heterogeneous effect of the policy change at the consumer level. Second, data limitations also prevented us from examining the impact of the pricing model transition on product returns and operational costs (e.g., inventory, delivery methods, associated costs). As all these factors significantly affect the profitability of the retailer, future research could further investigate the effects of the policy change on operational costs and product returns. Third, due to the restrictions of the empirical context, the potential bias introduced by SUTVA violations cannot be perfectly resolved. Consequently, the causal effects that we identified could be biased, and readers should interpret the results with caution. Finally, omnichannel platforms are inherently different. For example, a mobile shopping app might offer location-based deals, while a desktop website might offer detailed product comparisons. The way consumers interact with and perceive prices can vary widely across these platforms. Future studies could explore how platform heterogeneity affects the performance of uniform pricing relative to channel-specific pricing.

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Declarations

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