The Competitive Effects of Store-Format Repositioning: Evidence from a Norwegian Grocery Acquisition*

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July 6, 2026

Abstract

This paper evaluates Coop's 2015 acquisition of ICA, which raised national concentration in Norwegian grocery retail and reshaped local competition through store-format repositioning toward soft-discount. Store-level difference-in-differences estimates for a leading soft-discount incumbent imply that rival conversions to the acquirer's soft-discount format reduced sales by 4.4–12.9%, while closures increased them by 2.4–11.1%. Convenience-discount conversions show no sales response. Local product prices were unchanged, consistent with uniform national pricing, and assortments adjusted little. Nationally, food prices fell 3.8–5.2% relative to neighboring countries. These findings suggest the merger reallocated complementary firm-specific assets—ICA's locations and Coop's soft-discount concept—strengthening competition despite greater concentration.

Keywords: Competition, Horizontal mergers, Acquisitions, Ex post evaluation, Grocery retail, Product repositioning

JEL Codes: L41, L13, L81, L66, L11, D22, D43, C23

*I am grateful to NorgesGruppen and the FOOD research project for data access and support. I thank my supervisors, Frode Steen and Morten Sæthre, as well as colleagues at NHH for valuable guidance and feedback. I also thank participants at the UiB–NHH PhD Workshop, BECCLE, NORIO, CRESSE, and EARIE for helpful comments and discussions.

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1 Introduction

How can mergers between competing firms benefit consumers? Horizontal mergers are conventionally viewed as harming consumers by increasing market power and worsening the terms they face, unless efficiency gains are sufficiently large to offset these effects. Since Williamson (1968), such efficiencies have typically been understood as reductions in marginal production costs that lower prices (Kwoka, 2015).[1] However, mergers may also generate *repositioning efficiencies*: by combining complementary assets, firms may reduce the cost of changing product characteristics.

In this paper, I study the competitive effects of merger-enabled repositioning in the context of a major acquisition in the Norwegian grocery retail market, where the merger's local impact was driven primarily by store-format conversions rather than changes in local ownership concentration. Merger-enabled repositioning can benefit consumers through both direct and indirect channels. Directly, repositioning may improve quality or introduce attributes that better match consumer preferences. Indirectly, repositioning can reshape competitive interactions: as products move closer in characteristic space, substitutability increases, intensifying competitive pressure on prices and non-price attributes (though repositioning can also weaken competition if it instead leads to greater differentiation or product exit; Asker and Nocke, 2021). A central example—and the focus of this paper—is the Norwegian grocery retailer Coop's development of its soft-discount format Extra. The concept was designed to compete with the fast-growing soft discounters Kiwi and Rema 1000, but scaling it required access to suitable store locations. Coop's 2015 acquisition of ICA provided precisely such locations, including stores from the convenience-discount chain Rimi and the supermarket chain ICA Supermarked, making large-scale store-format conversion feasible.

Norway's grocery retail sector is dominated by a few vertically integrated umbrella chains, a structure that has long raised concerns about weak competition, high food prices, and limited quality and variety. These concerns sharpened in 2015 when Coop acquired ICA, increasing national ownership concentration as the market consolidated from four to three umbrella chains that together accounted for 96% of grocery sales. The Norwegian Competition Authority (NCA) cleared the acquisition subject to a divestiture remedy: Coop was required to sell 93 of ICA's roughly 550 stores. As a result, local ownership concentration often changed little. Instead, the most salient post-merger changes in local market structure arose from store-format conversions—particularly into Coop's soft-discount concept Extra. Consequently, the acquisition provides a setting in which the merger's local competitive impact is more likely to operate through changes in stores'

[1] A growing empirical literature seeks to measure such productive efficiency gains directly using granular data on physical inputs and outputs (e.g., Braguinsky et al., 2015; Demirer and Karaduman, 2025).

identities and characteristics than through ownership concentration. The aim of this article is to estimate the competitive effects of these merger-enabled store conversions.

The main research question is: how do rival store rebrandings and closures following Coop's acquisition of ICA affect the prices, product assortments, and sales of NorgesGruppen's soft-discount chain Kiwi? Prices and assortments capture firms' competitive responses to rival repositioning, while sales reflect consumers' purchasing behavior. Together, these outcomes provide evidence on how merger-enabled repositioning alters competitive pressure and consumer choice in local grocery markets. To complement the local analysis, I also examine whether the acquisition coincided with changes in aggregate food prices in Norway relative to neighboring countries. This comparison is informative about national price effects arising from chain-level efficiencies, changes in bargaining power or competitive intensity, or shifts in the composition of store formats following the merger.

The empirical strategy exploits variation in exposure to rival store rebrandings and closures across local grocery markets following Coop's acquisition of ICA. Because the acquired umbrella chain was not present in all local markets prior to the acquisition, some markets experienced no post-acquisition store conversions and thus serve as natural controls. Comparing treated Kiwi stores—whose local markets experienced a rival rebranding or closure—with control stores allows me to estimate the local effects of rival repositioning on prices, product assortments, and sales, while differencing out national- and chain-level effects.

Because store-format conversions occurred at different times between April 2015 and July 2016, I use difference-in-differences estimators that allow for heterogeneous treatment effects across cohorts and over time.[2] The empirical strategy combines nationwide data on the locations, characteristics, and local demographics of most grocery stores in Norway with detailed transaction data from the incumbent soft-discount chain Kiwi, enabling me to track prices, assortments, and sales at the store level.

The national price analysis draws on harmonized consumer price indices from Eurostat and combines a structural break analysis of Norwegian food prices with difference-in-differences and synthetic control methods that compare price developments in Norway with those in neighboring countries around the time of the acquisition.

I find that the estimated competitive effects of store-format conversions depend sharply on the post-merger format. When a nearby rival store rebrands to Coop's soft-discount concept Extra, sales at affected Kiwi stores are estimated to fall by 4.4–12.9%, indicating substantial demand diversion and that Extra is a materially tougher competitor than the predecessor formats.[3] By contrast, rebrandings to convenience-discount formats (Coop's

[2]See de Chaisemartin and D'Haultfœuille (2022) for a survey of difference-in-differences methods accounting for heterogeneous treatment effects.

[3]The reported intervals reflect variation across specifications using different local market definitions, as discussed in Section 4.1.

Prix or the independent chain Bunnpris) generally yield smaller sales responses that are statistically indistinguishable from zero. Rival store closures are associated with 2.4–11.1% higher Kiwi sales, consistent with the removed store having exerted meaningful competitive pressure.

Despite these sizable demand responses, I find no evidence that Kiwi adjusts product-level prices locally in response to either rebrandings or closures, consistent with uniform national pricing in the Norwegian grocery market (Ozhegova, 2025; Friberg et al., 2022; Meile, 2020). However, as shown by Ozhegova (2025), firms can still respond by adjusting the product assortment at the store level toward higher- or lower-priced products. Evidence on *effective store prices*—store-level averages of product prices weighted by expenditure shares—is less clear and varies across specifications, but effective prices tend to increase modestly following rival closures (0.5–2.0%) and rebrandings to Bunnpris (1.2–1.8%), consistent with shifts in basket composition rather than changes in product prices.

On the quality margin, local assortment responses are limited. Kiwi's product variety declines modestly—by 0.6–2.0%—following a rival rebranding to Extra, while there is no clear evidence of systematic variety changes following rebrandings to Prix or Bunnpris. Following rival closures, variety is slightly higher in some specifications. The small magnitude of these local adjustments, together with evidence of average assortment expansion at the national level, is consistent with assortment decisions being negotiated and implemented largely at the chain level, with limited scope for local discretion.[4]

Finally, in the national analysis using harmonized consumer price indices, I show that real food prices in Norway declined by 3.8–5.2% after the acquisition relative to comparable neighboring countries. This decline is consistent with local market conversions that, in the aggregate, shifted the national format mix toward soft discount and intensified within-segment rivalry, but it may also reflect national-level channels such as increased buyer power in negotiations with suppliers or scale efficiencies.

Related Literature. This article contributes to the empirical literature evaluating the competitive effects of horizontal mergers using ex post data (see Asker and Nocke, 2021 for a recent summary of merger retrospectives). Most quasi-experimental merger evaluations operationalize treatment through changes in local ownership concentration, because post-merger incentives to raise prices arise when some of the sales lost to a rival are recaptured by an entity the firm now owns (e.g., Allain et al., 2017; Pires and Trindade, 2018; Rickert et al., 2021). In my setting, however, divestiture remedies limited changes in local ownership concentration in many markets, while the salient post-merger change was extensive store-format rebranding. This creates a setting in which the merger's local competitive impact may operate primarily through changes in stores' identities

[4]Assortment discretion varies across retail formats. Soft-discount chains such as Kiwi typically operate with more standardized national assortments, whereas supermarkets and general stores allow greater store-level flexibility, making local assortment responses more pronounced in those formats (see Ozhegova (2025)).

and product characteristics—i.e., merger-enabled repositioning—rather than through ownership concentration.

A first contribution is therefore to isolate the local competitive impact of store-format rebrandings as a form of product repositioning.[5] A close conceptual antecedent is Hastings (2004), who studies sharp conversions of independent gasoline stations into vertically integrated formats and finds that such changes increased prices by about 5%, illustrating that outlet identity and characteristics can affect competitive pressure even absent changes in the number of competitors. My setting differs in a key institutional dimension: Norwegian grocery retail features chain-level uniform pricing (Friberg et al., 2022), which sharply limits unilateral local price responses. Consistent with this, I find no evidence of systematic local adjustments in product-level prices following rival rebrandings, and only limited evidence of shifts in effective store prices through changes in basket composition. Precisely because local prices are largely rigid, sales responses can be interpreted as demand diversion and as a measure of changes in competitive pressure. They therefore provide suggestive revealed-preference evidence on the direction of consumer welfare effects for marginal shoppers who gain or lose access to preferred alternatives. For the analysis of merger remedies, in particular, this setting allows me to isolate the effect of buyer heterogeneity—showing that preserving the number of competing owners need not preserve competitive pressure when alternative buyers possess materially different assets (see Brown et al., 2023 on the importance of allocation across heterogeneous firms in electricity markets).

A second contribution is to provide suggestive evidence on national price effects in a uniform pricing environment. Store-level difference-in-differences designs are well suited to identifying local competitive effects, but they difference out nationwide changes by construction—including changes in buyer power, chain-level efficiencies, and centralized pricing decisions. Related studies illustrate this limitation in different ways: Allain et al. (2017) identify local price responses among rivals with local pricing, but not national price changes for centrally priced grocery chains, while Aguzzoni et al. (2016) infer national price effects for books using within-country benchmarks—competitors' prices for the same titles and prices for the merging parties' top-selling titles—that may themselves respond to the merger, complicating magnitude interpretation. To shed light on aggregate effects of the ICA acquisition, I therefore compare real food prices in Norway with those in comparable neighboring countries using harmonized consumer price indices. This approach partially

[5] A related literature studies how mergers affect product positioning itself (e.g., Gandhi et al., 2008; Sweeting, 2010; Fan, 2013; Wollmann, 2018; Mazzeo et al., 2018; Herrera-Araujo and Piechucka, 2019; Argentesi et al., 2021; Li et al., 2022; Atalay et al., 2023). Whereas that work treats repositioning as an endogenous outcome of the merger, I treat store-format conversions as quasi-experimental variation in product characteristics and study their downstream effects on a rival. In line with Sweeting (2010)'s finding that merged firms may reposition closer to competitors, Coop frequently replaced ICA's formats with the soft-discount format Extra, increasing head-to-head competition with the soft discounters Kiwi and Rema 1000.

mitigates the absence of a domestic comparison group by netting out shocks common across countries, though causal interpretation remains cautious.

The next section provides an overview of the structure and segmentation of the Norwegian grocery retail market and describes Coop's acquisition of ICA. Section 3 presents the data and descriptive statistics. Section 4 outlines the research design used to analyze the local competitive effects of retail-format rebrandings and closures, while Section 5 reports results for prices, product variety, and sales and presents the aggregate national price analysis based on harmonized consumer price indices. Section 6 discusses identification and presents robustness checks. Finally, Section 7 concludes by summarizing the key findings and discussing their implications for the competitive effects of mergers and divestiture remedies.

2 The Norwegian Grocery Retail Market

2.1 Market Structure

Since 1994, the Norwegian grocery retail market has been dominated by four large umbrella chains accounting for approximately 96% of total sales: NorgesGruppen, Coop, Reitangruppen (Rema 1000), and ICA, until Coop acquired ICA in 2015. Each umbrella chain is vertically integrated with its own wholesaler and distribution system, and all except Reitangruppen operate several chain formats across different segments (see Table B.1 in Appendix B for an overview).[6] The evolution of market shares and ownership concentration over the past two decades is shown in Figure 2.1.[7]

Before 2014, ICA steadily lost market share to the other chains (from 25% in 2001 to 11% in 2014), especially to the market leader NorgesGruppen and Rema 1000, which grew by 7 and 8 percentage points, respectively, over the same period. Coop saw a slight decline before increasing its share by 7 percentage points following the acquisition. Bunnpris has maintained a small but growing presence (from 1.6% in 2001 to 3.4% in 2020), while the hard discounter Lidl exited the market in 2008 after failing to gain a foothold over five years (peaking at 1.7% in 2007). From the early 2000s to 2020, the market transitioned from four to three large umbrella chains, with the remaining firms largely maintaining their relative standings.

[6]Bunnpris is the largest retail chain outside the four umbrella chains and shares distribution with NorgesGruppen.

[7]Ownership concentration is measured using the Herfindahl–Hirschman index: $HHI = \sum_{i=1}^{N}(S_i)^2 \in [0, 1]$, where S_i is the market share of firm i in decimals.

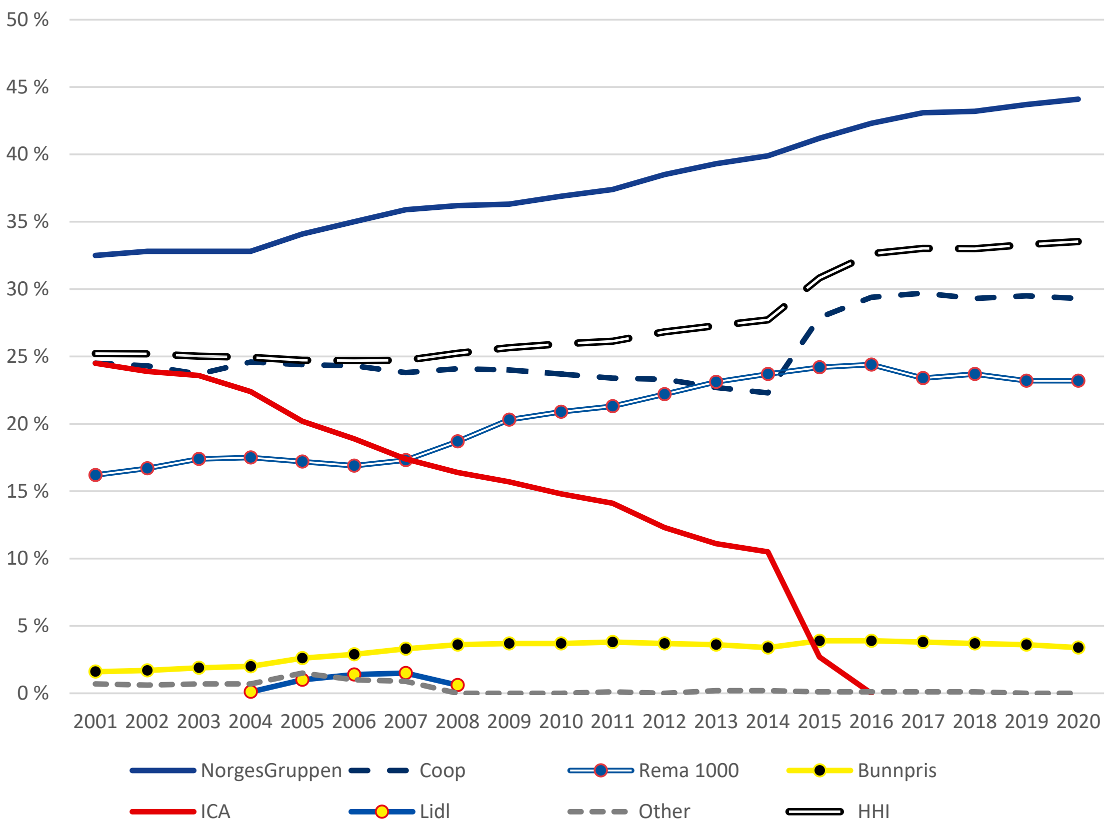

Figure 2.1: Evolution of market shares and the Herfindahl–Hirschman Index (HHI) by umbrella chain, 2001–2020. *Source:* AC Nielsen.

In 2002, the Herfindahl–Hirschman Index (HHI) in Norway (0.25; $N^e = 4$) and other Nordic countries was already high. For comparison, Germany (0.16; $N^e = 6.25$) and the United Kingdom (0.15; $N^e = 6.67$) exhibited substantially lower concentration levels (Nordic Competition Authorities, 2005).[8] However, it increased by only 0.03 over the 14 years leading up to the acquisition, before rising by another 0.05 by the end of 2016 ($HHI = 0.33$; $N^e = 3$). The acquisition reignited concerns about limited competition and its implications for product variety and food prices (Dagens Næringsliv, 2015; e24, 2014). As of 2026, the NCA continues to list competition in the grocery retail market as one of its priority issues (Ministry of Trade, Industry and Fisheries, 2026).[9]

Although increased ownership concentration at the national level may be cause for concern, transportation costs ensure that competition remains local, as consumers are unlikely to include distant stores in their consideration set. Strategic decisions such as

[8]The effective number of firms is defined as $N^e = \frac{1}{HHI} \leq N$, with equality when firms are of equal size. It can be interpreted as the hypothetical number of symmetric firms that would yield the same HHI as the actual firms of unequal size.

[9]According to the U.S. Department of Justice and FTC's *Horizontal Merger Guidelines*, an HHI above 0.25 indicates high market concentration. In such markets, mergers that increase the HHI by more than 0.02 are presumed likely to increase market power. Similarly, the European Commission cannot rule out competition concerns when the post-merger HHI exceeds 0.2 and the change in HHI is greater than 0.015. For a discussion of how HHI relates to competition intensity and social welfare, see Spiegel (2021).

pricing and base assortment must ultimately reflect local market conditions, even if they are determined at the national level.

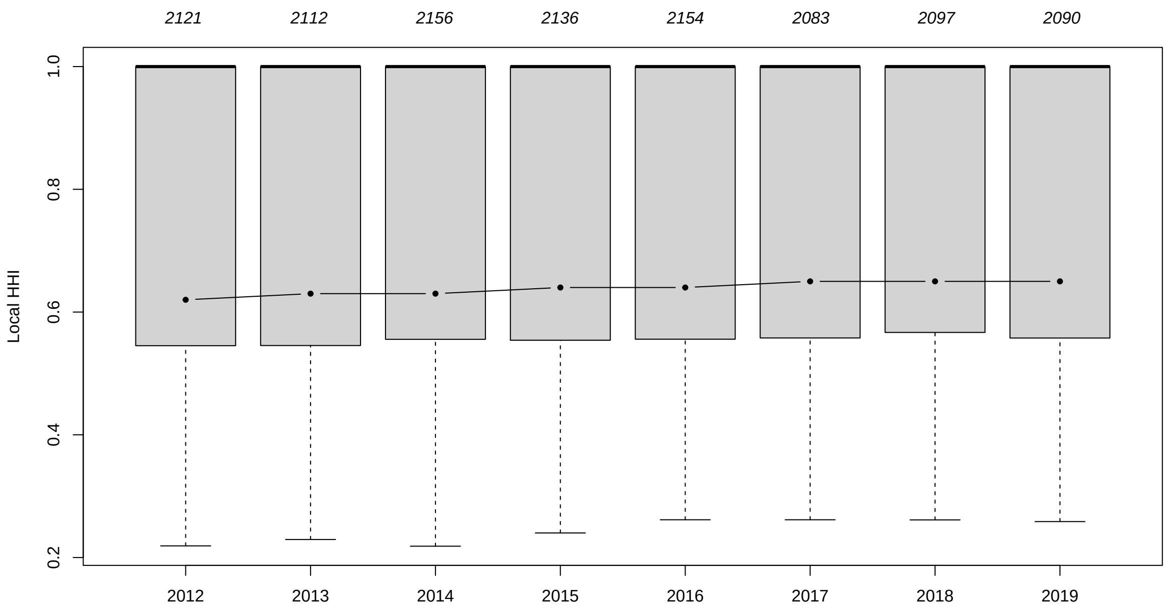

Figure 2.2: Box plots of the Herfindahl–Hirschman Index (HHI) at the *postcode* level, 2012–2019, calculated using store revenues aggregated at the umbrella chain level. The data cover the vast majority of grocery stores in Norway. The weighted mean, based on postcode sales shares, is shown as black dots connected by lines. The number of local markets in the sample is shown above each box plot. *Source:* Geodata.

To examine concentration at the local level, Figure 2.2 displays annual box plots of postcode-level HHI, summarizing the distribution across postcode areas from 2012 to 2019. The size of the postcode area a store resides in tends to vary inversely with the surrounding population and store density and is therefore correlated with consumers' willingness to travel. As a result, postcode areas serve as approximations to true markets (I return to this point in Section 4.1). The distribution of local market concentration clearly displays bunching at the upper endpoint ($HHI = 1$); the median local market is a monopoly. However, several of these markets are rural and served by a single small general store, with sales that likely account for only a minor share of the umbrella chains' national totals. A better measure of the competitive pressure imposed on the umbrella chains is the weighted mean of HHI, using each local market's share of nationwide sales. This is shown as the black dots connected by lines in Figure 2.2. Since national chains are not equally represented in all markets, local ownership concentration tends to be substantially higher than at the national level. The sales-weighted mean HHI across postcode areas increased only marginally, from 0.63 ($N^e = 1.59$) in 2014 to 0.64 ($N^e = 1.56$) in 2016—a change of 0.01 points ($\Delta N^e = -0.03$). This small change reflects that the merging parties did not compete in all local markets, and that the NCA imposed divestment remedies in most of the markets where they did, to limit competition concerns (Konkurransetilsynet, 2015, Clause 1744).

Similar plots of local HHI for the four alternative geographical units—Basic Statistical Units (BSUs), postal areas (larger than postcode areas), municipalities, and counties—are shown in Figures OA.1–OA.4 in the Online Appendix. There are approximately 14 000 BSUs in Norway, but they are too small to capture the relevant market, as the vast majority contain one or no stores. As we consider increasingly large market delineations—namely postal areas, municipalities, and counties—the sales-weighted HHI decreases mechanically toward the national level depicted in Figure 2.1. However, the national increase in HHI following the acquisition is only clearly reflected at the county level. Hence, there is limited scope for the acquisition to have affected competition through changes in local ownership concentration.

2.2 Market Segmentation

The store formats in the Norwegian grocery industry are traditionally categorized into general stores, discount stores, supermarkets, and hypermarkets (AC Nielsen).[10] General stores are typically small and offer a limited selection of goods and services to meet the basic needs of local communities.[11] Supermarkets are large, full-service grocery stores offering a wide variety of products, including fresh produce. Discount chains fall somewhere in between in terms of assortment size and focus on cost efficiency, low prices, and private labels. Hypermarkets are even larger than supermarkets and also stock non-food items such as clothing and electronics. They usually offer lower prices due to economies of scale but are typically located on the outskirts of cities or in large shopping centers. Since the mid-2000s, the market share of discount chains has steadily grown at the expense of the other segments—from 47% in 2005 to 68% in 2020.

[10] In Norwegian: *nærbutikk*, *lavpris*, *supermarked*, and *hypermarked*.

[11] *Nærbutikk* is sometimes translated as "convenience store", but should not be confused with small outlets such as kiosks and petrol stations, which are often located in high-traffic areas. These smaller stores typically offer a narrow range of on-the-go products, such as prepared food, drinks, and tobacco, and have long opening hours.

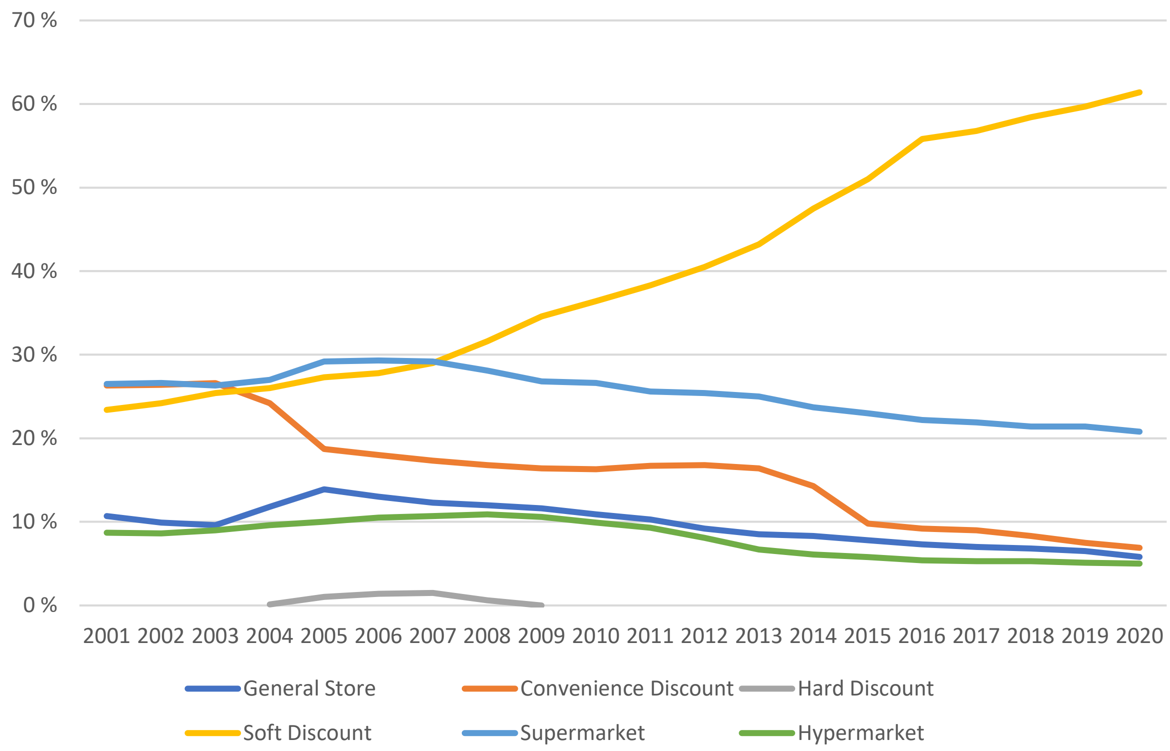

Figure 2.3: The evolution of market shares by retail segment, 2001–2020. *Source:* AC Nielsen.

In the Norwegian context, it is useful to further divide the discount segment into hard, soft, and convenience discount. Hard discount stores are no-frills outlets that carry a narrow selection of essential items, predominantly private labels. This enables high volumes of basic products, efficient operations, and consequently, very low prices.[12] In comparison, soft discounters are characterized by larger assortments and a greater presence of national brand names. Convenience-discount stores are typically smaller and emphasize quick and easy shopping, offering ready-to-eat meals, convenience foods, and on-the-go products such as salad bars, while still providing basic groceries at low cost.

Figure 2.3 shows the evolution of market shares across retail segments. The soft-discount segment has grown substantially over the past two decades, increasing from 23% in 2001 to 61% in 2020, largely at the expense of all other formats. The convenience-discount segment shrank the most, falling from 26% in 2001 to 7% in 2020, with a sharp drop in 2015 when ICA's convenience discounter Rimi was discontinued following the acquisition.

Engelund and Eimind (2018) find that the share of Norwegian consumers reporting discount chains as their preferred store format increased steadily from 2008 to 2016. They

[12]Examples of quintessential hard discount chains include Aldi and Lidl, the latter of which had a brief spell in Norway from 2004 to 2008. Import restrictions, low population density, and the absence of national brands are commonly cited as likely reasons for Lidl's exit (Selmani and Førre, 2023).

also report that accessibility, assortment size, and low prices are the most important store attributes, with the latter growing in importance over time. The success of the soft discounters aligns with their favorable positioning on these characteristics, especially price.[13] Given their growing market share, effective competition in the Norwegian grocery retail market depends heavily on the soft-discount segment.

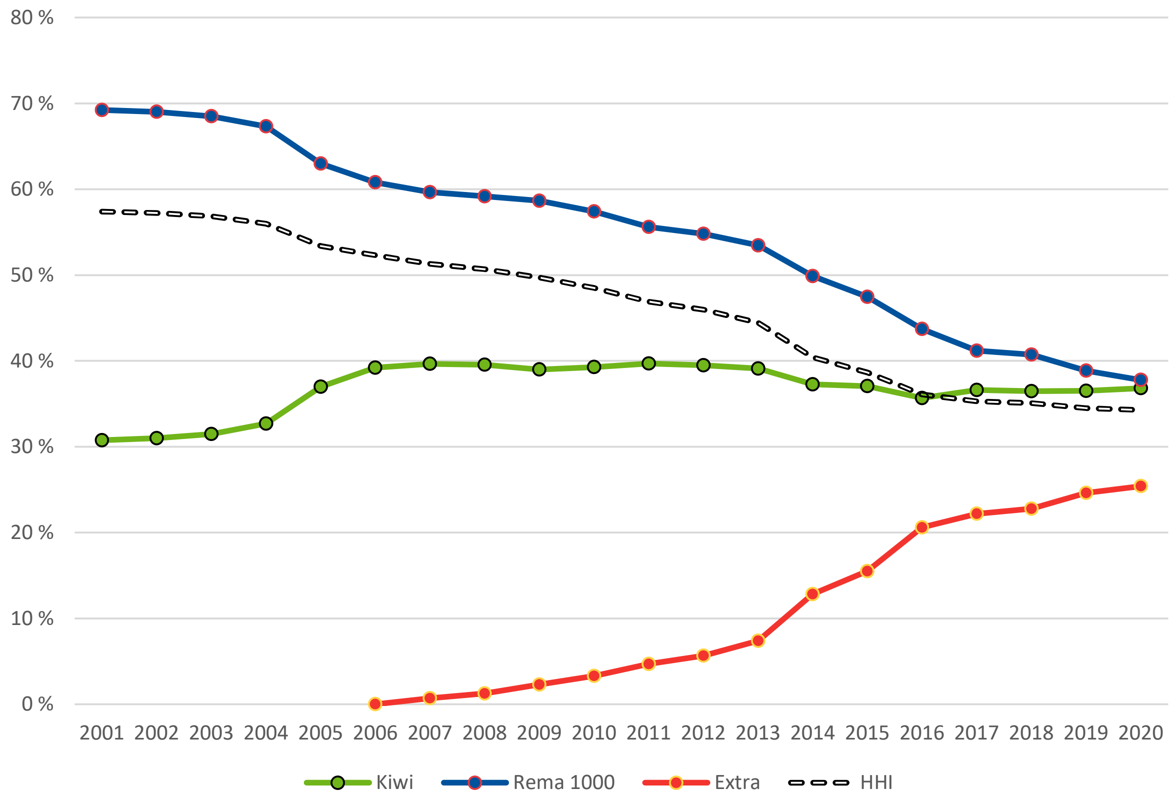

Figure 2.4: The evolution of market shares and the Herfindahl–Hirschman Index (HHI) in the soft-discount segment, 2001–2020. *Source:* AC Nielsen.

Rema 1000, inspired by the German hard discounter Aldi, spearheaded the development of the soft-discount segment in Norway. As shown in Figure 2.4, Rema 1000 dominated the segment throughout the 2000s and early 2010s, but concentration within the soft-discount segment has declined over time. Extra grew steadily after entering the market in 2006—with a noticeable acceleration following the ICA acquisition—and Kiwi has also slightly improved its position. In 2001, Rema 1000 and Kiwi held market shares of 70% and 30%, respectively, yielding a soft-discount HHI of 0.57 ($N^e = 1.75$). By 2020, the segment had evolved into a triopoly of Reitangruppen's Rema 1000, NorgesGruppen's Kiwi, and Coop's Extra, with market shares of 38%, 37%, and 25%, respectively, and an HHI of 0.34 ($N^e = 2.94$). The increase in soft-discount presence and within-segment

[13]The three soft discounters consistently dominate VG's matbørs, a recurring blind test of grocery prices conducted by one of Norway's leading newspapers (see https://www.vg.no/spesial/matborsen/).

rivalry—driven in part by merger-enabled format conversions and expansion—is a central focus of this article.

2.3 Coop Acquires ICA

The ICA group, with its chain concepts Rimi (convenience discount), ICA Supermarked, and Matkroken (general store), struggled with low profits and steadily lost market share throughout the 2000s and early 2010s. In 2014, the NCA blocked a purchasing and distribution agreement with NorgesGruppen that was intended to improve ICA's profitability through lower wholesale prices and more efficient distribution. Consequently, ICA instead sought to sell its 554 Norwegian stores to Coop, a transaction the NCA approved conditional on divestment remedies—43 stores were sold to the independent chain Bunnpris and 50 to NorgesGruppen.

The NCA employed a sequential screening process to select these stores. First, it filtered out 227 ICA stores that, if acquired by Coop, were unlikely to harm competition based solely on the number of competing stores nearby and their market shares. Second, 93 of the remaining stores were identified as problematic based on a high degree of competition between the merging parties (as measured by diversion ratios) and post-acquisition incentives to raise quality-adjusted prices.

The NCA considered acquisition-specific efficiency gains but concluded that they were insufficient to offset the costs of adjustment and the loss of competition (Konkurransetilsynet, 2015, Clause 1733).[14] It also deemed the likelihood of disciplining future entry low, citing significant barriers such as import restrictions, irreversible investments, economies of scale, and vertical integration (Konkurransetilsynet, 2015, Clause 156). Ultimately, the NCA approved the acquisition conditional on divestment remedies intended to mitigate its anti-competitive effects (Konkurransetilsynet, 2015, Clause 1753), but emphasized that the remedies would not go beyond what was necessary, in accordance with the principle of proportionality (Konkurransetilsynet, 2015, Clause 1754). The public documents did not consider whether the acquisition could result in a net pro-competitive outcome or examine the potential competitive effects of store-format rebrandings on non-merging stores, motivating an investigation into how such rebrandings may have affected competition.

[14]The details of the proposed efficiency gains are withheld from the public.

3 Data and Descriptive Statistics

3.1 Data Sources and Construction

The analysis draws on several data sources. The first is an annual panel compiled by Geodata, which tracks characteristics such as geographical location, size, retail format, ownership, and opening hours of Norwegian grocery stores.[15] Store-level annual sales figures in the panel are based on data from Nielsen and Iper. The dataset also includes demographic variables—including population, average earnings, and education—at various levels of geographical aggregation, such as basic statistical units (BSUs), municipalities, and counties.

The second source is a schedule of the staggered store rebrandings and closures that followed the acquisition, provided by the acquirer. It details which format replaced each store and at what time. Combined with store location data, this information allows me to determine whether a store experienced a rival's rebranding or closure in its vicinity. In a few cases where the exact reopening date was not provided, the reopening month was obtained from local newspapers.

The third data source consists of high-frequency transaction records provided by NorgesGruppen, covering product-level sales, prices, and assortment sizes for its soft discounter, Kiwi. The data are aggregated to the monthly level, and average monthly product prices are calculated by dividing total sales in NOK by the number of units sold, where units are defined as items, weight, or volume, depending on the product type. The product categories included in the sample are listed in Table B.3 in Appendix B, along with their expenditure shares. Most major categories are represented, but fruits and vegetables are notable exceptions—primarily due to substantial seasonality and variation in their sales and assortment, as well as less reliable recording. As a result, the main product category that typically exhibits non-uniform pricing within chains is excluded from the analysis. This exclusion also limits the occurrence of prices measured by weight or volume (e.g., kilograms or liters), as most remaining products are sold in discrete units or packages.

Combining these sources yields a panel of stores observed at monthly intervals from 2014 to 2016, with a rich set of store and product characteristics. The analysis focuses on the soft-discount chain Kiwi, which accounted for an average of 19% of the Norwegian grocery market during the sample period. These data also allow me to characterize local market structure and to trace store-level format rebrandings before and after the acquisition in detail.

[15] See https://geodata.no/.

3.2 Descriptive Statistics

The following descriptive statistics and visualizations provide an overview of the scope, timing, and geographic distribution of store rebrandings and closures. Table 3.1 reports the frequency of the different format transformations for ICA Supermarked and Rimi stores following the acquisition.[16] The analysis focuses on rebrandings to Coop Extra (129), Coop Prix (83), Bunnpris (32), and store closures (46). Although rebrandings to Bunnpris were less frequent, they are included because they resulted from divestment remedies and belong to the same segment as Prix. Rebrandings to NorgesGruppen are excluded because the analysis focuses on the impact of rival rebrandings on Kiwi stores; a rebranding to NorgesGruppen always involves changes in ownership concentration, not just format. The remaining transformation types are rare or difficult to interpret and are therefore not included in the main analysis. Double change refers to cases where a store underwent two successive changes, such as being rebranded twice, rebranded and then closed, or experiencing a prolonged closure before reopening during 2015 or 2016.

Figure 3.1 illustrates how these store rebrandings and closures accumulated over time. The transformation process took place between April 2015 and July 2016, with most activity concentrated in late 2015 and early 2016; roughly half of the 385 total transformations occurred in September 2015. Except for 27 transformations in the five months leading up to September 2015, the remainder took place gradually until July 2016.

Figure 3.2 provides a geographical overview of the stores included in the analysis. The left panel displays all Kiwi, ICA Supermarked, and Rimi stores operating in Norway in 2014, prior to the onset of the rebranding process. The right panel depicts the outcomes for each ICA store following the rebrandings and closures in 2015–2016, together with the Kiwi stores operating in 2016. Both chains were present in all counties, with Kiwi stores more concentrated in the south and ICA stores relatively more prevalent in the north. The maps also highlight areas of direct competition—where Kiwi and ICA stores are co-located—as well as regions where only one chain operated. In many cases, the spatial separation is such that a broad range of plausible market definitions would yield the same classification of whether stores are exposed to the rebranding.

4 Empirical Strategy

The absence of ICA stores in some local markets enables a difference-in-differences identification strategy: I compare outcomes for stores in markets where a rival rebranded

[16] The general store *Matkroken* is not included here or in the analysis. Coop acquired most Matkroken stores without rebranding, though a few were divested and rebranded. This, together with a few double-change cases, accounts for the discrepancy between the 93 divested stores reported by the competition authority and the 72 Bunnpris and NorgesGruppen stores in Table 3.1.

Table 3.1: Frequency of rebranded ICA stores before and after the acquisition

	ICA Supermarked	Rimi	Sum ICA
Coop Extra	25	104	129
Coop Prix	1	82	83
Closed	6	40	46
NorgesGruppen	8	32	40
Bunnpris	4	28	32
Coop Mega	26	1	27
Double Change	3	18	21
Rema 1000	2	5	7
Sum Rebrandings	75	310	385

Notes: The table summarizes how Rimi and ICA Supermarked stores were rebranded or converted following the acquisition. Rows indicate the new format or status, while columns show the original ICA format. Cell values are the number of stores in each category, with row and column sums reported at the margins.

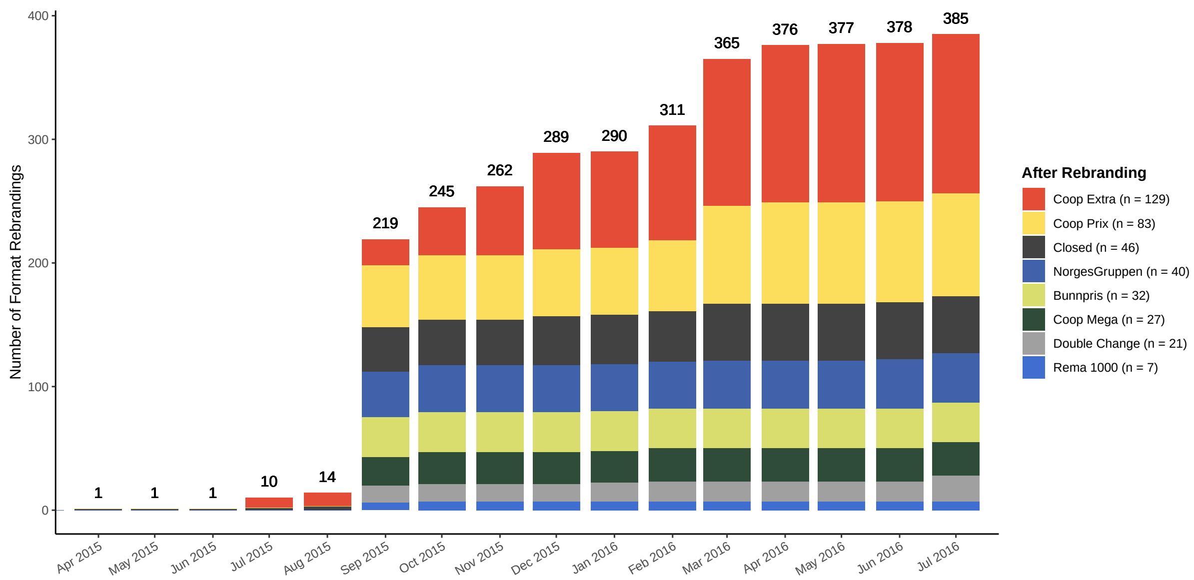

Figure 3.1: The figure displays the cumulative number of store rebrandings from April 2015, when the first rebranding occurred, to July 2016, by which time all stores had been transformed. The colors indicate the share of each type of store transformation.

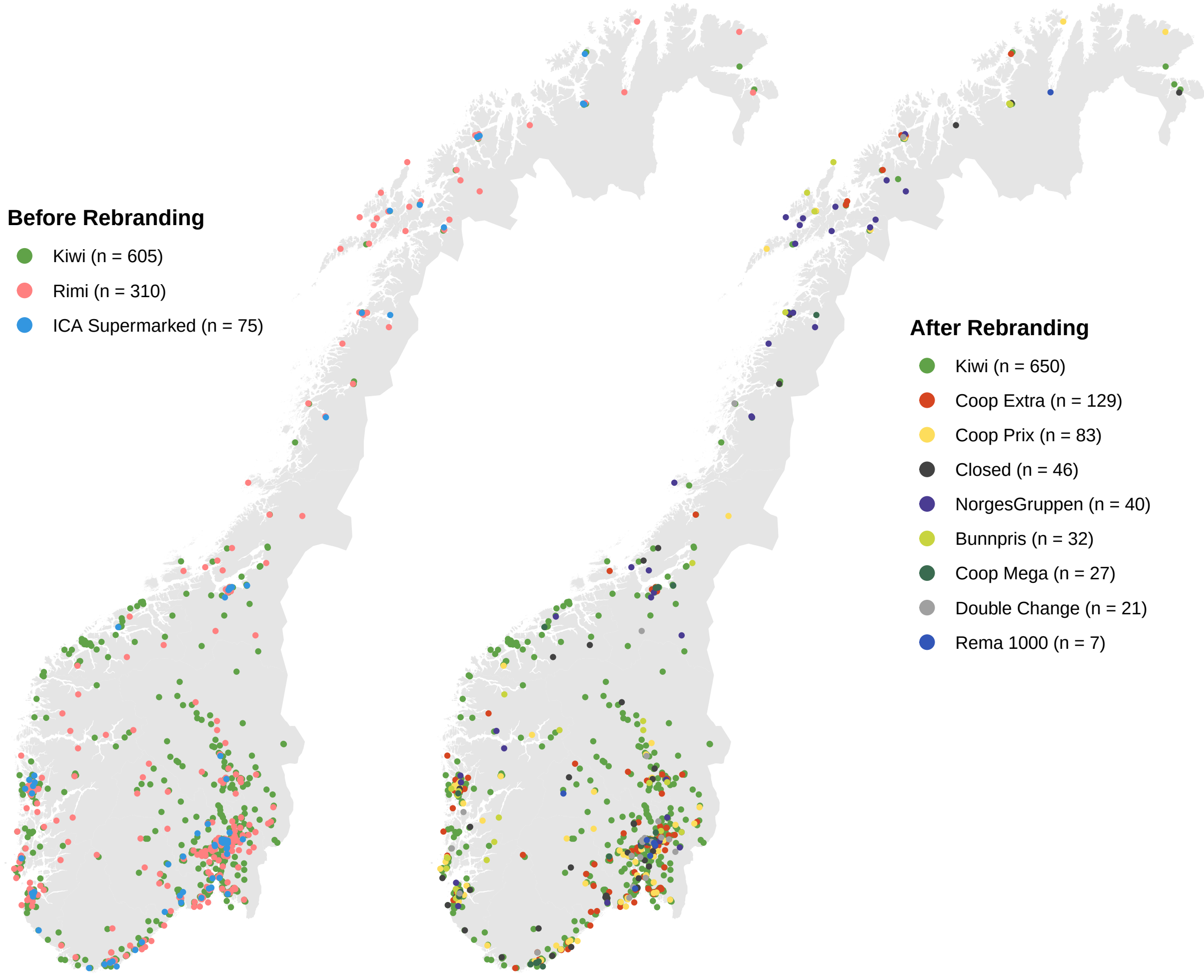

Figure 3.2: The left map shows the locations of Kiwi, Rimi, and ICA Supermarked stores operating in Norway in 2014. The right map shows ICA store outcomes in 2015–2016 and Kiwi stores operating in 2016. During 2014–2016, Kiwi opened 72 stores and closed 27, for a net increase of 45 stores. Dots are layered in decreasing order of category size so that less common formats are more visible.

or closed (treated) with those in control markets, before and after the conversions.[17]

4.1 Local Market Definition

I assume that stores compete if they are located within a given radius of one another, with this radius defining each store's local market. This discretized definition of competition is standard in ex post merger evaluations in retail settings (Hastings, 2004; Allain et al., 2017; Rickert et al., 2021). Nevertheless, because competitive pressure plausibly declines smoothly with distance, such a binary classification may introduce discretization error. This concern is mitigated, however, for stores that are spatially clustered into hubs: in such cases, a range of reasonable market radiuses yields identical treatment assignments. As illustrated in Figure 3.2, many store pairings fall into this category, so moderate variation in the assumed market size does not alter the classification of treated and control stores, limiting sensitivity to the particular discretization rule.

To minimize discretization error, the chosen market size should capture most relevant competitors while excluding as many marginal or non-competing stores as possible. The optimal market size likely varies across settings and geographic contexts. For instance, Hastings (2004) considers gasoline retailers within a one-mile radius (approximately 1.6 km), Choné and Linnemer (2012) analyzes competition among parking lots within 650–1000 meters, and Allain et al. (2017) defines grocery markets as extending 10 km from the city center (20 km for hypermarkets), finding similar results with a 5 km (10 km) definition.

The appropriate market size in Norway is likely smaller than that used by Allain et al. (2017). They base their kilometric market size on the French Competition Authority's assumption that consumers are, on average, willing to drive 10 to 15 minutes to reach a supermarket or discount store. The NCA applies similar driving times in Norway, except in the largest cities and most remote areas, where it uses thresholds of 5 and 20 minutes, respectively (Konkurransetilsynet, 2015, Clause 90). However, it notes that survey evidence suggests Norwegians may be even less willing to spend time traveling, particularly in urban areas (Konkurransetilsynet, 2015, Clause 93), which points to smaller kilometric market sizes in the Norwegian context.[18] Moreover, a given driving time likely corresponds to a shorter distance in Norway due to its topography. I therefore consider smaller market sizes in the range of 0.5–10 km.

Market sizes are also unlikely to be uniform across Norway, particularly due to variation

[17] A store is defined as *treated* if a rival converted within its local market. Although national-level strategic adjustments may indirectly affect all markets, such effects are absorbed by time fixed effects in the difference-in-differences design.

[18] Lower willingness to travel is consistent with Norway's high store density, which reduces the need for long trips to reach a grocery store (Friberg et al., 2020, p. 37). This is especially true in the capital, Oslo, where the average consumer has access to 137 stores within 10 minutes of travel (Friberg et al., 2020, p. 50).

in store density and accessibility. In areas with low store density, consumers must often travel farther to reach a store, leading geographically distant stores to compete for the same customers. In contrast, when store density is high, consumers located between two stores are more likely to be intercepted by intermediate stores, weakening or severing the competitive link between the original pair. A simple way to account for this heterogeneity is to use BSU population density as a proxy for store accessibility, as densely populated BSUs are more likely to have better access to stores in and around them.[19] This approach, for instance, ensures that market sizes are smaller on average in city centers—where many stores are within walking distance—than in rural areas, where only a few stores may be reachable by car. Table 4.1 presents five specifications with varying market sizes across six quantiles of BSU population density, assigning larger market sizes to stores located in sparsely populated BSUs.

Table 4.1: Market sizes across BSU population density quantiles

	Quantiles					
Specifications	1	2	3	4	5	6
10–1.5 km	10 km	7 km	5 km	4 km	2 km	1.5 km
10–1 km	10 km	7 km	5 km	3 km	1.5 km	1 km
10–0.5 km	10 km	7 km	5 km	2 km	1 km	0.5 km
6–1.5 km	6 km	5 km	4 km	3 km	2 km	1.5 km
5–0.5 km	5 km	4 km	3 km	2 km	1 km	0.5 km

Notes: The table reports market radiuses (in kilometers) assigned to stores across six quantiles of BSU population density, under five different specifications. In each specification, stores located in more densely populated BSUs are assigned smaller market radiuses, reflecting higher store accessibility and density.

Some studies (e.g., Aguzzoni et al., 2016; Pires and Trindade, 2018; Argentesi et al., 2021) define local markets using pre-determined geographical units such as cities. In a similar spirit, I include a specification that defines markets based on postcode areas. Postcodes reflect the geographic layout of cities, districts, and townships, with large cities typically divided into several distinct postcode areas. A key advantage of this approach is that the number and size of postcode areas in a given region tend to scale with population and store density. For example, a fixed area in Oslo will typically contain more—and smaller—postcode areas than an equally sized area in rural Norway. As a result, the size of the postcode area in which a store is located is likely correlated with local store density and consumers' willingness to travel, making the postcode specification a flexible complement to the radius-based definitions.

[19]Store density at the BSU level is less informative, as most BSUs contain zero or only one store, even in dense urban areas where consumers routinely shop across BSU boundaries. The next aggregation level at which store density is observed is the municipality, which is too coarse and larger than most local grocery markets.

4.2 Treated and Control Stores

Stores are labeled as *treated* if their local market contained an ICA store prior to the acquisition, and *control* otherwise. Within the treated group, I define separate treatment arms based on the outcome of the nearby ICA store: format rebrandings into Extra, Prix, or Bunnpris, or permanent store closure.[20] To avoid confounding the effects of format rebrandings with changes in local ownership concentration, I restrict attention to local markets where the umbrella group of the rebranded format was not already present. Markets with multiple format transformations are also excluded.

Using the postcode market definition, Table B.2 in Appendix B reports mean values of the outcome and control variables before and after the acquisition, by treatment group, along with their percentage changes. The number of stores in each group is shown in parentheses next to the group label. Sales exhibit an upward trend across all groups, except for the Extra group, which remained flat on average. As expected, the average number of competitors changed only for stores that experienced a rival's exit. Treated stores initially had higher sales and faced more competitors, the latter consistent with the likelihood of experiencing a rival rebranding increasing with the number of competitors in the market. Assortment sizes and price levels rose over the sample period, but by similar magnitudes across groups.[21] Notably, initial price levels and assortment sizes were very similar across groups, despite differences in sales, competitor numbers, and market concentration, suggesting limited scope for stores to adjust these variables locally.

4.3 Staggered Adoption Design

Since the rebrandings occurred at different points in time, the empirical setting follows a *staggered adoption design* (Athey and Imbens, 2021). Under the assumptions of common trends and no anticipation, store outcomes can be modeled using the following two-way fixed effects specification (Borusyak et al., 2024):

$$Y_{it} = \gamma_i + \lambda_t + \beta_{it} T_{it} + \delta' \boldsymbol{Z_{it}} + \epsilon_{it} \tag{4.1}$$

In this specification, Y_{it} denotes the outcome variable for store i at time t, T_{it} is an indicator for whether store i is treated at time t, and $\boldsymbol{Z_{it}}$ is a vector of covariates. The error term ϵ_{it} captures unobserved shocks, which are assumed to be uncorrelated across municipalities but may exhibit serial correlation within stores or contemporaneous correlation within municipalities. The terms γ_i and λ_t are store and time fixed effects, respectively, while

[20] A few irregular cases—such as rebranded stores that later closed or rebranded a second time during the 2014–2016 sample period—are excluded.

[21] The price indices are explained in Section 5.1.

β_{it} and δ' are the parameters to be estimated. Treatment effects β_{it} are allowed to vary arbitrarily across stores and time, and the Average Treatment Effect on the Treated (ATT) is defined as the expectation $\beta = E[\beta_{it} \mid T_{it} = 1]$. The common-trends and no-anticipation assumptions are evaluated in Section 6, and the main conclusions remain robust to specifications that relax these restrictions.

4.4 Accounting for Treatment Effect Heterogeneity

As recently shown in methodological contributions such as Goodman-Bacon (2021), Sun and Abraham (2021), Borusyak et al. (2024), Callaway and Sant'Anna (2021), and De Chaisemartin and d'Haultfoeuille (2020), estimating Equation 4.1 using ordinary least squares does not yield consistent estimates of the ATT when treatment effects are heterogeneous across units or over time and treatment is staggered.[22] Instead, I employ the estimator proposed independently by Sun and Abraham (2021) (S&A) and Callaway and Sant'Anna (2021).[23] Imputation-based estimators, such as those proposed by Gardner et al. (2024) and Borusyak et al. (2024), often yield similar results but are more sensitive to violations of the parallel-trends assumption (see Roth et al., 2023). By contrast, the S&A estimator is more sensitive to anticipation effects; however, these can be diagnosed and mitigated by shifting the event timing and excluding affected pre-event periods (see Section 6). A further advantage of the S&A estimator is that it allows for easily interpretable event-study plots, as its coefficients measure the mean differences between ever-treated and never-treated units relative to a specified reference period—here, the last period before treatment.

5 The Competitive Effects of Retail Format Rebrandings

In this section, I estimate the local effects of rival ICA stores rebranding or closing on Kiwi's prices, product assortment, and sales. Consistent with centralized or chain-level pricing, Kiwi stores do not appear to adjust prices unilaterally in response to local competitive changes. I therefore also examine the evolution of national food prices in Norway relative to neighboring countries.

[22] For example, treatment effects may vary due to differences in market concentration or seasonal variation coinciding with the timing of rebranding.

[23] Implementation is facilitated by the `fixest` package in R (Bergé, 2018).

5.1 Food Prices

Using product-level sales data derived from receipt records, I calculate the average unit price of product j in store i and month t as:

$$P_{ijt} = \frac{\text{Sales}_{ijt}}{\text{Quantity}_{ijt}}$$

where Sales_{ijt} denotes the total monthly turnover in NOK and Quantity_{ijt} the total quantity sold—measured, depending on the product, in number of items, grams, or liters.[24] These unit prices are then aggregated into a store-level price index using weights:

$$w_{ijt} = \frac{\text{Sales}_{ijt}}{\sum_j \text{Sales}_{ijt}}$$

which represent the share of product j in total sales at store i during month t.[25]

I consider two indices to capture both product- and store-level price variation. The first is the sales-weighted average of individual product prices:

$$\hat{P}^1_{it} = \sum_j w_{ijt} P_{ijt}$$

The second is the sales-weighted average of the log-deviations of product prices from their cross-sectional average across stores:

$$\hat{P}^2_{it} = \sum_j w_{ijt} \left[\ln \left(\frac{P_{ijt}}{\overline{P}_{jt}} \right) \right] = \sum_j w_{ijt} \left[\ln(P_{ijt}) - \ln(\overline{P}_{jt}) \right]$$

Each product contributes its approximate percentage deviation from the average price across stores.[26]

The price indices can change either due to changes in individual product prices or in quantities. However, $\hat{P}^2_{it}$ remains fixed at zero and is unaffected by changes in weights (i.e., average basket composition) if product prices are strictly uniform across stores within a chain. In contrast, the effective price $\hat{P}^1_{it}$ can change even in the absence of local price deviations if consumers substitute toward higher- or lower-priced products in response to local market conditions. Stores can influence such shifts by adjusting their product assortment, as noted by Ozhegova (2025). Thus, $\hat{P}^1_{it}$ captures both individual product prices and basket composition, whereas $\hat{P}^2_{it}$ isolates deviations in product-level prices across stores.

[24] Since fruits and vegetables are excluded from the analysis, most product prices refer to items.

[25] The analysis could alternatively be conducted at the store-product level, but this is computationally demanding due to the large number of products.

[26] Because the index is an average of percentage deviations, it is invariant to the units in which products are measured—e.g., grams, liters, or package sizes.

Table 5.1 reports sample means and standard deviations for the price indices under the postcode market definition. The price index $\hat{P}_{it}^2$ is close to zero on average as expected. The very small standard deviation is consistent with previous findings in the literature (Ozhegova, 2025; Friberg et al., 2022; Meile, 2020; DellaVigna and Gentzkow, 2019): product prices are largely uniform across stores, leaving little scope for local product-level price adjustments. In contrast, the effective price index $\hat{P}_{it}^1$ varies across Kiwi stores and may respond to local market conditions—for example, through assortment adjustments.

Table 5.1: Mean and standard deviation of price indices

	$\hat{P}_{it}^1$	$\hat{P}_{it}^2$
Mean	49.98 kr	0.00
Standard deviation	10.88 kr	0.01

Notes: $\hat{P}_{it}^1$ is in NOK and $\hat{P}_{it}^2$ is a percentage deviation from the average product price across stores.

Theoretically, prices can either rise or fall following a change in a competitor's identity, depending on consumer preferences and substitution patterns (Hastings, 2004). Suppose, however, that grocery stores within the same store-format segment are closer substitutes. Formally, this can be modeled using a nested logit framework with nests defined by store-format segments, as in Anderson and de Palma (1992). In such a model, stores in the same segment are closer substitutes than others if there is less taste heterogeneity within segments than across segments (i.e., $\mu_1 \geq \mu_2$ in Anderson and de Palma, 1992). In this case, price competition should be more intense in markets with stores in the same segment, all else equal.[27]

I report the estimated effects for Equation 4.1 using the two price indices as outcome variables in Tables 5.2 and 5.3, across six local market definitions. After taking the natural logarithm, changes in $\hat{P}_{it}^1$ can be interpreted as percentage changes in the price level. I include the natural logarithms of municipality population and median income as controls to account for potentially different demographic trends across treated and control stores.[28]

The results in Table 5.3 are consistent with the limited variation in $\hat{P}_{it}^2$: I do not detect any economically significant local adjustments in product prices in response to rival rebrandings. More interestingly, there is also no clear evidence of substantial changes in effective store prices. One specification shows a 0.7% price increase associated with a rival rebranding to the soft discounter Extra, statistically different from zero at the 5% level, but the other specifications yield smaller and even negative estimates, none of which are statistically significant. The absence of effects for Coop-owned Prix or the independent chain Bunnpris is not surprising, given that both formats are similar

[27] Pricing could also be affected by differences in costs related to the new format or ownership.

[28] The estimators of Sun and Abraham (2021) and Callaway and Sant'Anna (2021) currently do not support multiple treatment variables in a single estimation procedure in either Stata or R. Hence, I run separate regressions for each treatment variable and exclude stores exposed to other treatments.

Table 5.2: Estimated effects of rival rebrandings on effective store prices

	$\ln(\hat{P}^1_{it})$					
	(1)	(2)	(3)	(4)	(5)	(6)
T_{Extra}	-0.002	0.005	0.007*	-0.001	0.006	0.002
	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)
T_{Prix}	0.016	0.006	0.001	0.016	-0.002	0.004
	(0.012)	(0.011)	(0.007)	(0.010)	(0.007)	(0.008)
T_{Bunnpris}	0.012	0.015	0.018	0.015	0.018	0.013
	(0.016)	(0.013)	(0.010)	(0.017)	(0.010)	(0.009)
T_{Closure}	0.016	0.015	0.019*	0.016	0.020*	0.005
	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)	(0.008)
Market Definition	10–1.5 km	10–1 km	10–0.5 km	6–1.5 km	5–0.5 km	Postcode

Notes: Each column reports event-specific ATT estimates for a given local market definition, estimated following Sun and Abraham (2021). The data span all months of 2014–2016, and the standard errors shown in parentheses are clustered by municipality. Each ATT is retrieved from separate regressions reported fully in Tables OA.1–OA.4 in the Online Appendix. Controls for municipality-level population and income are included in each regression. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Table 5.3: Estimated effects of rival rebrandings on local product price deviations

	$\hat{P}^2_{it}$					
	(1)	(2)	(3)	(4)	(5)	(6)
T_{Extra}	0.000	-0.001	-0.001	0.000	-0.001	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
T_{Prix}	0.001	0.000	0.001	0.001	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
T_{Bunnpris}	0.002	0.002	0.001	0.002	0.002	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
T_{Closure}	0.001	0.001	0.000	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Market Definition	10–1.5 km	10–1 km	10–0.5 km	6–1.5 km	5–0.5 km	Postcode

Notes: Each column reports event-specific ATT estimates for a given local market definition, estimated following Sun and Abraham (2021). The data span all months of 2014–2016, and the standard errors shown in parentheses are clustered by municipality. Each ATT is retrieved from separate regressions reported fully in Tables OA.5–OA.8 in the Online Appendix. Controls for municipality-level population and income are included in each regression. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

to ICA's Rimi chain. However, Bunnpris rebrandings are generally associated with higher effective prices in the range of 1.2–1.8%, perhaps because Bunnpris exerts weaker pressure on Kiwi to offer low-priced alternatives. A potential price increase following a Bunnpris rebranding is particularly notable since these rebrandings resulted from divestment remedies. When local market concentration is held constant, there is no evidence that the remedied rebrandings are more pro-competitive than alternatives, which suggests potential limitations of divestment remedies. Still, confidence in the Bunnpris estimates is limited by the small number of ICA stores that rebranded to Bunnpris, as reflected in the large standard errors. Rival closures are associated with effective price increases for Kiwi in the range of 0.5–2%, with two specifications producing estimates statistically different from zero at the 5% level. This finding is consistent with reduced competition following a rival ICA store closure, allowing Kiwi to profitably offer slightly fewer and pricier products, thereby increasing effective store prices, as in Ozhegova (2025).

The fact that rival rebrandings to Extra—a soft discounter like Kiwi—do not induce Kiwi to shift its assortment toward cheaper products is somewhat surprising in light of Ozhegova (2025), who finds that effective store prices are lower in more competitive markets.[29] This, along with the moderate effect of rival closures, could be explained by soft discounters like Kiwi allowing relatively little discretion for individual stores to adjust their assortments. In contrast, convenience stores and supermarkets included in the sample of Ozhegova (2025) may be permitted to cater more to their local customer bases.

5.1.1 National Food Prices After the Acquisition

Overall, I find no evidence of local adjustments in product-level prices in response to the acquisition or subsequent rebrandings. The evidence on effective store prices is less clear, owing to both imprecise estimates and variation across model specifications, although effective prices consistently increase slightly following rival closures and Bunnpris rebrandings. Since product prices are essentially fixed in the cross-section, these changes must reflect shifts in basket composition.

While these findings suggest limited local price effects, an important remaining margin is national pricing: the acquisition may have affected prices at the national level, which would not be captured in the store-level analysis due to the inclusion of time fixed effects. Identifying national price effects is inherently difficult in quasi-experimental settings, as domestic comparison groups are also exposed to national shocks (see Allain et al., 2017; Aguzzoni et al., 2016). To examine this possibility, I use the Harmonized Index of Consumer Prices (HICP), drawing on monthly data for Norway and neighboring countries

[29] Note that effective prices could also change without stores adjusting product prices or assortments if rival rebrandings lead consumers to change their basket composition, for example, by buying some products at the new store while still purchasing others at the old store. However, the scope for such asymmetric substitution is limited if consumers tend to do most of their shopping at the same store.

from 2005 to 2021 provided by Eurostat.[30] The HICP is based on a national basket of representative consumer products selected by national statistical institutes.

The HICP for Food and Non-Alcoholic Beverages (F&B; hereafter food prices) increased by approximately 5.5% from 2014 to 2017, whereas the All-Items HICP rose by about 8% over the same period. To measure the evolution of real food prices, I deflate food prices by overall prices and take logarithms: $\ln(P_t^{Food}) - \ln(P_t^{All})$. This yields a measure of real food prices that can be interpreted as the approximate percentage deviation of food prices from overall prices. Figure 5.1 plots the F&B and All-Items HICP, as well as real food prices in Norway from 2005 to 2021. There appears to be a negative shift in the trend of real food prices following ICA's exit from the market in 2016 (see Appendix A.1 for a formal structural break analysis).

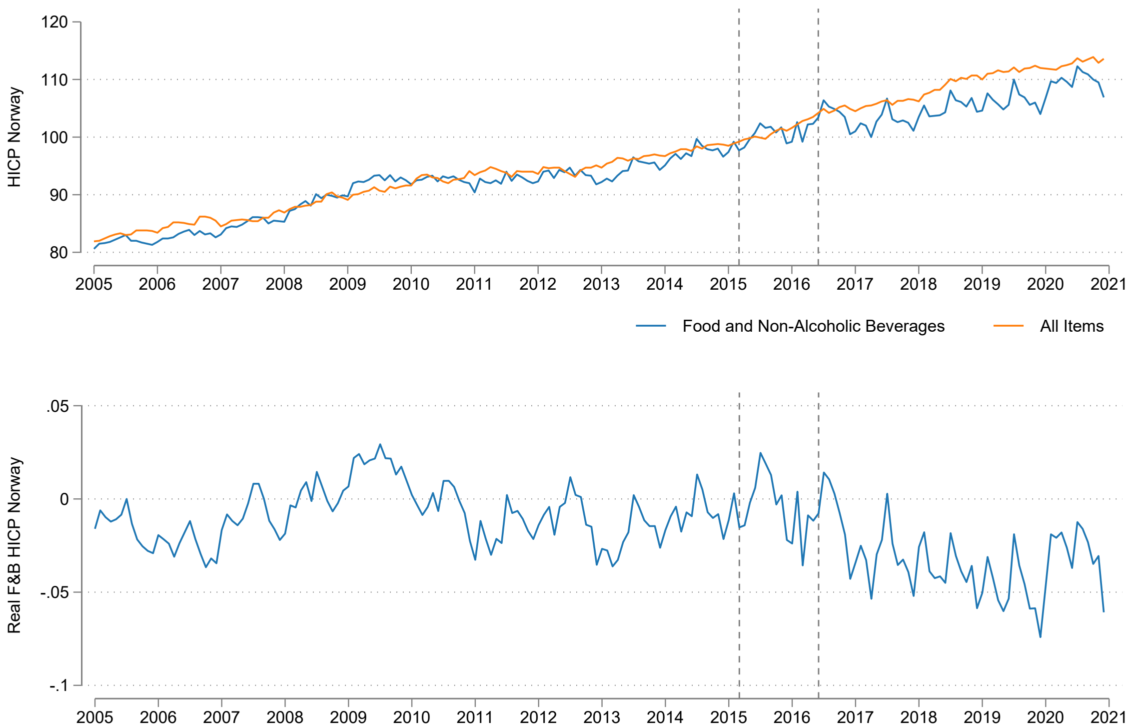

Figure 5.1: The top panel shows the F&B and All-Items HICP in Norway, with mean index values normalized to 2015 = 100. The bottom panel shows the real F&B HICP, calculated as $\ln(P_t^{Food}) - \ln(P_t^{All})$. The two vertical lines indicate the transition period, from just before the first store rebranding (March 2015) to just before the final rebranding (June 2016). *Source:* Eurostat.

Figure 5.2 illustrates the best linear fits of real food prices in Norway before and after the ICA acquisition, covering the period from 2011 to 2020. The transition period, during which only some ICA stores were rebranded, is excluded. The annualized trend growth rate of real food prices in Norway was 0.24% prior to the acquisition, changing to -1.19% thereafter. By the end of 2019, the trend line for real food prices was 5.85% lower than

[30] https://ec.europa.eu/eurostat/web/hicp/data/database

the trajectory predicted by the pre-acquisition trend.

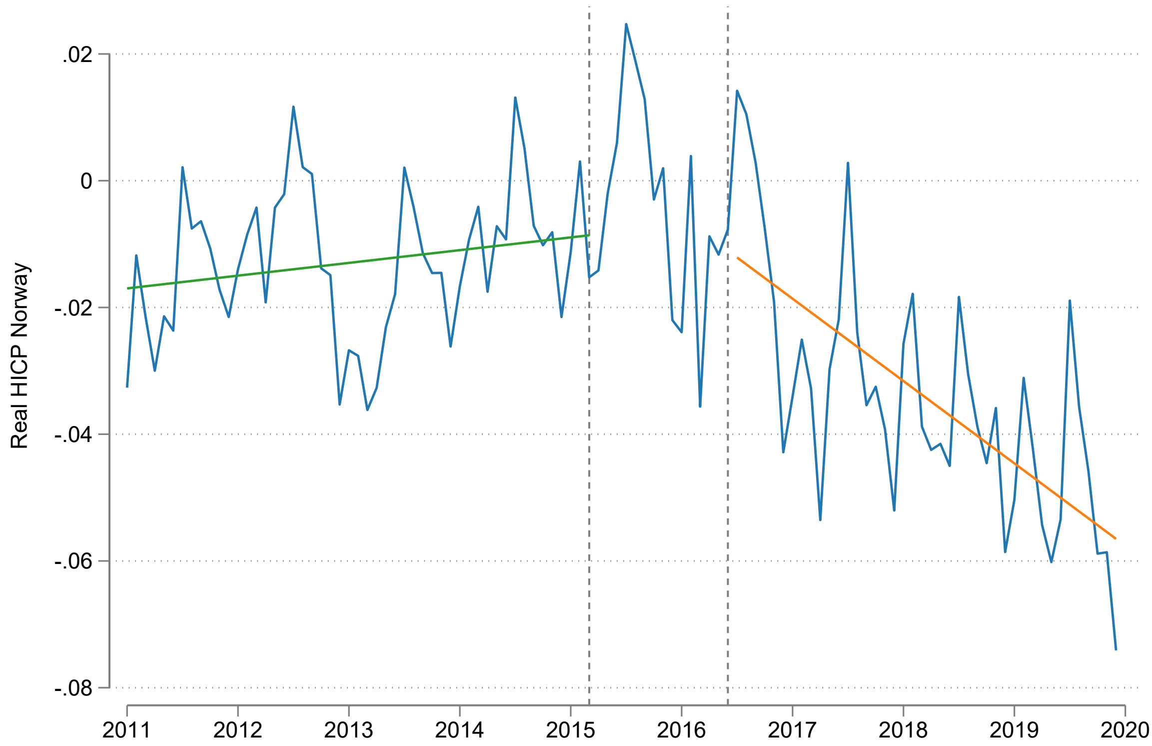

Figure 5.2: Best linear fit of real F&B HICP, calculated as $\ln(P_t^{Food}) - \ln(P_t^{All})$, for 2011–2019, before and after the acquisition. The transition period (April 2015–June 2016) is excluded. *Source:* Eurostat.

The shift in real food prices following the acquisition could be driven fully or partially by changes in confounding variables. However, some of these underlying factors—such as weather, trade and transportation conditions, pandemics, wars, or other supply-chain disruptions—would also affect neighboring countries through global commodity prices (e.g., wheat or coffee beans). These explanations become less plausible if neighboring countries did not experience structural breaks comparable to those observed in Norway.

Figures OA.5 and OA.6 in the Online Appendix plot the F&B and All-Items HICP, as well as real food prices, for a set of candidate comparison countries. By inspection, Sweden, Denmark, Germany, and Iceland appear to follow common linear trends with Norway from the Great Recession until the acquisition. Figure 5.3 shows that real food prices in these countries, including Norway, track similar paths prior to the acquisition, and that Norway experiences a negative trend shift while the others remain on trend. A formal difference-in-differences analysis (Appendix A.2), using Sweden, Denmark, Germany, and Iceland as comparison countries, indicates that real food prices fell by an average of 5.1% in Norway after the acquisition relative to those countries. Given the absence of comparable breaks in the comparison countries, purely common-shock explanations appear less plausible; accounting for Norway's break would require either a Norway-specific shock coinciding with the acquisition or heterogeneous pass-through of a common shock.

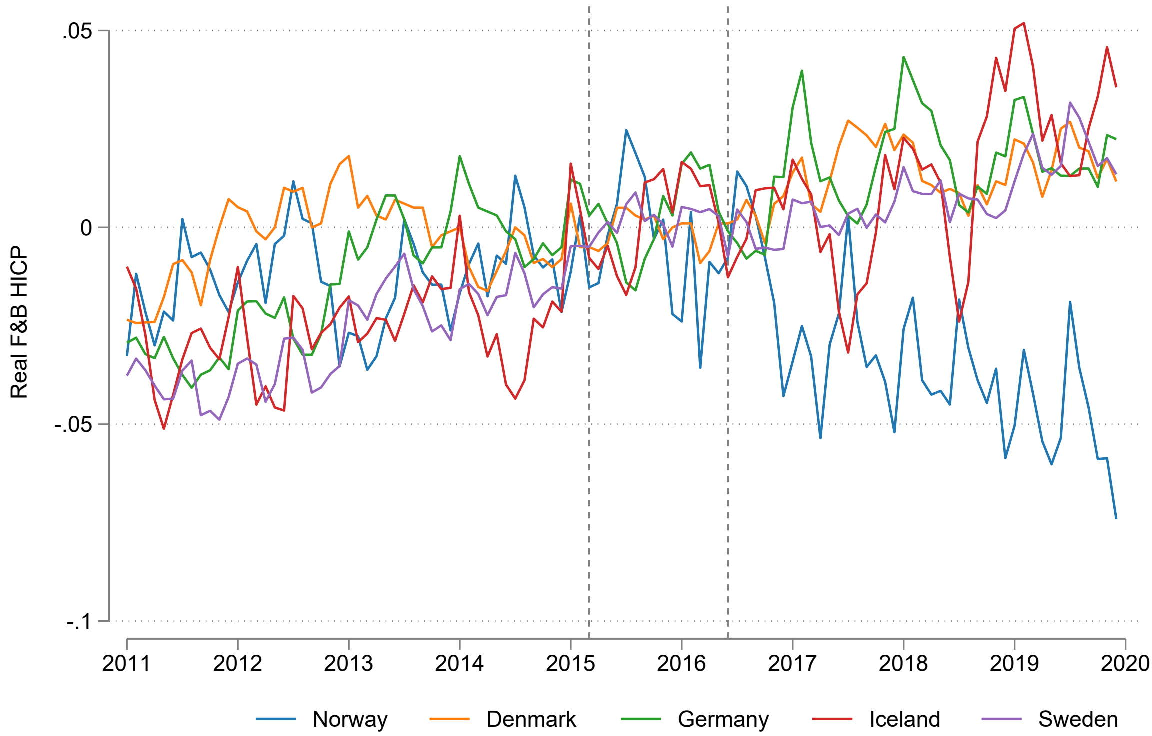

Figure 5.3: Real F&B HICP for Norway and comparison countries, calculated as $\ln(P_t^{Food}) - \ln(P_t^{All})$. The transition period (April 2015–June 2016) is indicated by the vertical dashed lines. *Source:* Eurostat.

To relax the assumptions of common or linear trends, I employ the synthetic control method proposed by Abadie and Gardeazabal (2003). I extend the time series back to 2005 and construct a synthetic comparison country as a weighted average of the Nordic countries—Denmark, Finland, Iceland, and Sweden. The deseasonalized real food prices for Norway, the synthetic control, and their difference are shown in Figure A.1 in Appendix A.3. The synthetic comparison closely tracks Norway until the acquisition, after which a gap emerges as real food prices begin to decline in Norway. Consistent with the difference-in-differences analysis, Norwegian food prices are estimated to fall by an average of 5.2% relative to the synthetic control, suggesting a downward trend shift in Norwegian real food prices not observed in neighboring countries. For additional details and alternative specifications, see Appendix A.3.

Even if the acquisition caused the observed price decline, the underlying mechanism remains unclear. Mergers and acquisitions can alter factors such as bargaining power, ownership concentration, economies of scale, and product positioning. Yet the evidence indicates that the average market experienced a shift in store formats from convenience discount to soft discount without changes in local ownership, as shown in Figures 2.3 and 2.2 in Section 2.2. With the disappearance of ICA's convenience discounter Rimi and the large-scale rebranding of stores to Coop Extra, the market basket tilted toward the soft-discount segment. This compositional shift plausibly reduced average prices, since soft discounters like Extra were consistently cheaper than convenience discounters like Rimi.[31] At the same time, the growing presence of soft discounters increased the frequency of head-to-head competition within the segment (see Figure 2.4), presumably among closer substitutes. With uniform prices determined by competitive pressure across markets, the combination of a lower-price format mix and intensified rivalry aligns with the observed decline in national food prices.

Lower prices could also reflect increased bargaining power or economies of scale. ICA's exit removed one outside option for suppliers, while higher chain-level volumes may have generated efficiencies in distribution, administration, and marketing. However, the NCA did not deem the proposed efficiency gains large enough to offset adjustment costs or the loss of competition in the event of a complete takeover by Coop (Konkurransetilsynet, 2015, Clause 1733).[32]

5.2 Product Variety

Even if stores are unable to change prices in response to shifts in their competitive environment, they may adjust quality. In the grocery retail market, a key component of perceived quality is product variety and accessibility. For example, previous research on shocks to market structure—such as the entry of Walmart—finds significant effects on inventory shortfalls. Matsa (2011) argues that the increased risk of losing customers incentivized supermarkets to reduce stock shortfalls by about one-third. However, when considering assortment size more broadly, greater competitive pressure can also disincentivize the introduction of new products if fixed costs are involved. The business-stealing effect of a fiercer competitor may render a large product assortment unprofitable. These opposing mechanisms suggest that the effect of rival rebrandings on quality and assortment is uncertain and must be determined empirically.

To examine how stores adjust assortment sizes in response to rival rebrandings, we must address the measurement error that arises from not directly observing the full assortment, but rather the number of unique products purchased. In a given month, some offered products may not be purchased and thus remain unobserved. To mitigate this issue, I only count products that sold at least 100 units nationally during every period of the sample. This yields a measure that should change only if the store alters its assortment offering. The trade-off is that we do not capture changes in the assortment of low-volume products.

The estimated percentage effects of a rival rebranding to Extra on assortment size are reported in Table 5.4. Across specifications, Kiwi appears to respond to rivals rebranding to Extra by reducing its product assortment by roughly 1%, although two of the estimates

[31] Rimi was about 5% more expensive than Extra in 2014 (see https://www.vg.no/spesial/matborsen/matborser/22/).

[32] The specific details of those anticipated gains remain confidential.

are not statistically significant at the 5% level. There is no clear evidence that rival rebrandings to Prix or Bunnpris led Kiwi to adjust its assortment size at the local store level. A rival closure, however, is associated with a slightly larger assortment size across specifications, but in only half of the cases is the effect statistically significant at the 5% level.

Table 5.4: Estimated effects of rival rebrandings on assortment size

	ln(Variety)					
	(1)	(2)	(3)	(4)	(5)	(6)
T_{Extra}	-0.009	-0.009*	-0.013***	-0.007	-0.011**	-0.007*
	(0.005)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)
T_{Prix}	-0.001	-0.007	-0.005	-0.002	-0.004	0.000
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
T_{Bunnpris}	0.002	-0.003	-0.003	-0.002	-0.001	0.001
	(0.005)	(0.003)	(0.005)	(0.005)	(0.005)	(0.005)
T_{Closure}	0.006*	0.006	0.007	0.007*	0.004	0.004*
	(0.003)	(0.004)	(0.005)	(0.003)	(0.004)	(0.002)
Market Definition	10–1.5 km	10–1 km	10–0.5 km	6–1.5 km	5–0.5 km	Postcode

Notes: Each column reports event-specific ATT estimates for a given local market definition, estimated following Sun and Abraham (2021). The data span all months of 2014–2016, and the standard errors shown in parentheses are clustered by municipality. Each ATT is retrieved from separate regressions reported fully in Tables OA.9–OA.12 in the Online Appendix. Controls for municipality-level population and income are included in each regression. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Counting the number of unique products offered provides an intuitive measure of product variety, but it does not account for the value to consumers of each individual product. Consider a store offering a large set of marginal products along with a few very popular ones and compare it to a store with a large offering of products with similar sales shares. The latter kind of variety is more valuable to consumers, as revealed by the expenditure shares. Alexander (1997) proposes measuring product variety as entropy (Shannon, 1948):

$$E_{it} = -\sum_{j=1}^{J_{it}} w_{ijt} \ln(w_{ijt})$$

where w_{ijt} is the sales share of product j, and J_{it} is the assortment size of store i at time t. This measure increases with J_{it} and reaches its maximum for a given J_{it}, $E_{it} = \ln(J_{it})$, when $w_{ijt} = 1/J_{it}$. If a single product accounts for all sales, i.e., $w_{1it} = 1$ and $w_{ijt} = 0$ for $2 \leq j \leq J_{it}$, then $E_{it} = 0$. For asymmetric but strictly positive sales shares across multiple products, $0 \leq E_{it} \leq \ln(J_{it})$. Unlike the simple count of unique products, the entropy measure is unaffected by unobserved products that are offered but not sold, since they contribute nothing to the sum.

The estimated effects of rival rebrandings on variety entropy are shown in Table 5.5. All specifications find a decrease in the entropy measure between 0.6% and 2% following

Table 5.5: Estimated effects of rival rebrandings on variety entropy

	E_{it}					
	(1)	(2)	(3)	(4)	(5)	(6)
T_{Extra}	-0.007	-0.006	-0.016**	-0.007	-0.020***	-0.010*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
T_{Prix}	0.003	0.004	0.005	0.003	0.008	0.005
	(0.007)	(0.006)	(0.004)	(0.007)	(0.005)	(0.007)
T_{Bunnpris}	0.020**	-0.002	-0.007	-0.002	-0.009	-0.005
	(0.006)	(0.009)	(0.009)	(0.011)	(0.010)	(0.008)
T_{Closure}	0.002	0.003	0.001	0.002	-0.002	-0.007
	(0.006)	(0.007)	(0.007)	(0.007)	(0.005)	(0.005)
Market Definition	10–1.5 km	10–1 km	10–0.5 km	6–1.5 km	5–0.5 km	Postcode

Notes: Each column reports event-specific ATT estimates for a given local market definition, estimated following Sun and Abraham (2021). The data span all months of 2014–2016, and the standard errors shown in parentheses are clustered by municipality. Each ATT is retrieved from separate regressions reported fully in Tables OA.13–OA.16 in the Online Appendix. Controls for municipality-level population and income are included in each regression. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

a rival rebranding to Extra, but only three specifications are statistically significant at the 5% level. For rivals rebranding to Prix, there is no clear evidence of an effect across specifications, but all estimates are slightly positive. For Bunnpris, five estimates are small and negative, while one is positive and statistically significant at the 1% level. There is no statistically significant effect of rivals closing down.

The most robust finding in Tables 5.4 and 5.5 is that product variety at Kiwi declines modestly following rival rebrandings to Extra. One explanation is that increased competitive pressure renders some products locally unprofitable. An alternative, demand-driven explanation is that consumers who switch from Kiwi to Extra previously purchased niche items, reducing sales of those products or leaving them unsold. Both explanations are consistent with the finding below in Section 5.3 that Extra captures more revenue from Kiwi than other formats. At the same time, Table B.2 in Appendix B shows that both assortment size and entropy-based variety increase over time at the national level. Taken together, these patterns suggest that while competitive pressure increased and modestly reduced variety locally, the primary response occurred at the chain level, where overall assortment variety expanded.

5.3 Sales

Having established that (i) product-level prices are uniform across stores within chains and do not adjust locally, and (ii) local assortment responses are limited, I interpret changes in store-level sales following nearby rebrandings or closures primarily as shifts in demand. In this setting, lower sales at Kiwi after a rival's transformation reflect stronger

Table 5.6: Estimated effects of rival rebrandings on sales

	ln(Sales)					
	(1)	(2)	(3)	(4)	(5)	(6)
T_{Extra}	-0.093***	-0.101***	-0.126***	-0.070***	-0.129***	-0.117***
	(0.016)	(0.018)	(0.014)	(0.015)	(0.015)	(0.021)
T_{Prix}	-0.010	-0.008	-0.015	-0.025	-0.025	-0.033
	(0.017)	(0.018)	(0.022)	(0.018)	(0.021)	(0.018)
T_{Bunnpris}	0.081**	0.023	0.025	0.037	-0.001	-0.045
	(0.030)	(0.035)	(0.044)	(0.032)	(0.053)	(0.039)
T_{Closure}	0.078***	0.083***	0.076***	0.082***	0.073**	0.024
	(0.019)	(0.023)	(0.021)	(0.022)	(0.025)	(0.019)
Market Definition	10–1.5 km	10–1 km	10–0.5 km	6–1.5 km	5–0.5 km	Postcode

Notes: Each column reports event-specific ATT estimates for a given local market definition, estimated following Sun and Abraham (2021). The data span all months of 2014–2016, and the standard errors shown in parentheses are clustered by municipality. Each ATT is retrieved from separate regressions reported fully in Tables OA.17–OA.20 in the Online Appendix. Controls for municipality-level population and income are included in each regression. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

demand diversion to that rival—that is, greater competitive pressure—rather than price adjustments.

I estimate Equation 4.1 using the natural logarithm of monthly sales as the outcome, and report results across multiple local market definitions in Table 5.6. As in the price and assortment analyses, I include demographic controls to absorb differential local trends.

Rebrandings of rivals to the soft discounter Extra are consistently associated with 7.0–12.9% declines in Kiwi's sales across market definitions, suggesting that Extra imposed stronger competitive pressure on Kiwi than the predecessor formats. Rebrandings to convenience-discount formats (Coop's Prix or Bunnpris) yield smaller effects that are generally statistically indistinguishable from zero. By contrast, rival store closures are associated with 2.4–8.3% higher Kiwi sales, consistent with reduced competitive pressure. Overall, these results suggest that convenience-discount stores such as Rimi and Prix were weaker competitors to Kiwi than Extra, though they still exerted some pressure. Given that 129 stores rebranded to Extra while only 46 closed, the net effect of rebrandings was an increase in competitive pressure on Kiwi.

The sales results have implications for consumer welfare. Based on revealed preference, consumers who switched away from Kiwi following rival rebrandings were plausibly better off, while those who switched to Kiwi following a rival rebranding or closure plausibly lost access to a preferred alternative. This interpretation is supported by uniform prices and limited assortment changes at Kiwi, as demand responses to national price or assortment changes are differenced out by the responses at stores that did not experience a rival rebranding. The estimated sales response thus approximates the average demand response of a consumer whose choice set still includes the unchanged alternative of Kiwi.[33] That said, the minor reduction in assortment variety at Kiwi following Extra rebrandings could theoretically contribute to the demand diversion, although this adjustment may itself be a consequence of the reduced demand rendering niche items unprofitable or unsold.

The observed demand responses at Kiwi capture only part of the overall welfare implications of the rebrandings, which themselves represent just one component of the broader effects of the acquisition. I make no claim about the total welfare effects of either the acquisition or the rebrandings based solely on the sales results. Nevertheless, the findings highlight a potential synergistic effect of the acquisition that could benefit consumers—namely, the combination of a preferred store-format concept on the acquirer's side and scarce store locations on the acquiree's side. The acquisition may thus generate consumer gains when the acquiree cannot readily replicate the preferred format and the acquirer cannot readily obtain comparable locations elsewhere.

6 Identification and Robustness Checks

6.1 Dynamic Event Study Design

The causal interpretation of the store-level regression results rests on two identifying assumptions: parallel trends and no anticipation. To evaluate their validity, I estimate a variant of Equation 4.1 in which treatment effects vary nonparametrically over time—a dynamic event-study design. This approach serves two purposes. First, it tests for systematic differences in pre-treatment trends between treated and control stores. Second, it detects potential anticipatory effects in the periods preceding rival rebrandings. The dynamic specification is:

$$Y_{it} = \gamma_i + \lambda_t + \sum_{l \neq \{-1,-\infty\}} \mu_l \mathbb{1}\{t - E_i = l\} + \delta^{'} \boldsymbol{Z_{it}} + \epsilon_{it} \tag{6.1}$$

where $\mu_l = E[\mu_{itl}] = ATT_l$ and $\mathbb{1}\{\cdot\}$ denotes an indicator function. E_i is the rebranding month in store i's market, and $t - E_i = l$ measures the number of periods since the rebranding. The coefficients of interest are the average treatment effects μ_l at relative time l after treatment. To avoid perfect collinearity among the relative-time indicators,

[33] The ability to infer welfare changes for both groups of consumers (those switching away from, and those switching to, Kiwi) relies on the characteristics of Kiwi being fixed in the cross-section of stores. However, if Kiwi had become more attractive, say by lowering prices locally, we could still infer that the switchers-away benefited, but the welfare impact on switchers-to would be ambiguous, as the price decrease might compensate for the loss of their preferred alternative. Similarly, increased prices would mean that switchers-to surely suffered, while switchers-away might have chosen to remain absent the price increase.

one period must be omitted as the reference; following convention, I exclude $l = -1$. Markets in the untreated group correspond to $l = -\infty$. Under the parallel-trends and no-anticipation assumptions, trajectories should be indistinguishable across treated and untreated stores for all $l < -1$, implying $\mu_l = 0$.

The paper reports a large number of regressions. Event-study estimates for each store-level regression are presented in Figures OA.8–OA.27 in the Online Appendix. For consistency and readability, all figures display a common event window of $l \in [-18, +10]$ months around the event. Longer horizons are omitted to ensure that most cohorts contribute to each plotted coefficient, since extreme leads and lags are supported by few observations, producing wide confidence intervals and potentially unrepresentative point estimates.

Overall, the estimates support the validity of the parallel-trends and no-anticipation assumptions. In some cases, the linear pre-trends differ slightly or the reference period $l = -1$ appears as an outlier among the pre-periods, but these deviations are small and cannot plausibly rationalize the qualitative implications of the post-event coefficients. Because the data are monthly and somewhat noisy, the event-study plots may appear erratic; in this setting, the overall pattern of the pre-event coefficients is more informative than whether individual leads happen to differ from zero. In the few cases where visible deviations occur, the main conclusions are likely robust to controlling for linear pre-trends or shifting the reference period. This is particularly true for price and variety outcomes, where the estimated effects are small.[34]

[34]Potential exceptions include effective store prices after closures (likely attenuating toward zero), variety after Extra rebrandings (somewhat reduced), variety after Bunnpris rebrandings (possibly turning slightly negative), and variety entropy after Bunnpris rebrandings (where the positive effect likely vanishes).

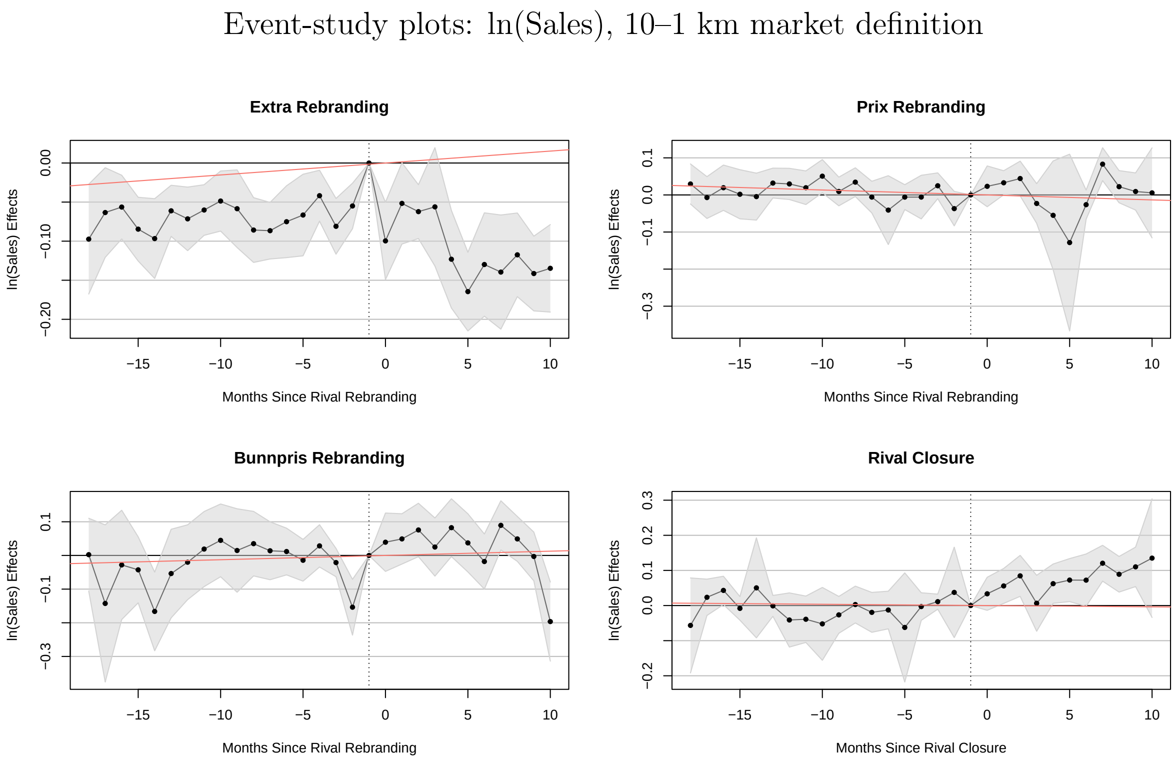

Figure 6.1: Estimated percentage sales effects of rival rebrandings and closures over time. The horizontal axis displays months relative to the event, $l \in [-18, +10]$, with the vertical line marking $l = -1$—the last month before the event. Point estimates are shown with 95% confidence intervals, based on standard errors clustered at the municipality level. A fitted linear pre-trend is displayed and extrapolated into the post-event period for comparison.

The estimated effects on sales are substantially larger than those for price and assortment outcomes, so I examine the corresponding event-study plots in greater detail. Figure 6.1 reports event-study estimates from the sales specification under the 10–1 km market definition, which is broadly representative of the other definitions, separately by rival rebranding and closure type. Sales decline noticeably following a rival rebranding to Extra relative to the control group. However, sales are unusually high in the reference period and exhibit a mild positive pre-trend, which could bias the estimated effect. Sales following Prix and Bunnpris rebrandings appear broadly similar before and after the rebranding, although there are some outliers—particularly for Bunnpris, which had relatively few rebrandings in the sample. Finally, sales are similar across groups prior to rival closures, after which treated Kiwi stores begin to experience higher sales.

6.2 Adjusting for Anticipation and Linear Pre-trends

In this section, I address potential violations of the no-anticipation and parallel-trends assumptions. The former may arise in this context if stores experience an adjustment period during rebranding and thus do not operate normally. To mitigate this concern, I exclude the final month before the event and use the penultimate month as the reference period when estimating Equations 4.1 and 6.1, such that neither the pre- nor post-event periods are measured relative to a potential transition month immediately preceding treatment. The ATT estimates are reported in Table 6.1, and Figure 6.2 presents the corresponding event-study plots for the 10–1 km specification. The complete set of results is shown in Figures OA.28–OA.31 in the Online Appendix.

Table 6.1: Estimated effects of rival rebrandings on sales: last period before event excluded

	ln(Sales)					
	(1)	(2)	(3)	(4)	(5)	(6)
T_{Extra}	-0.072***	-0.051**	-0.096***	-0.053**	-0.091***	-0.094***
	(0.017)	(0.018)	(0.015)	(0.018)	(0.013)	(0.022)
T_{Prix}	0.034	0.019	-0.030	0.007	-0.045	-0.065*
	(0.031)	(0.026)	(0.026)	(0.033)	(0.036)	(0.030)
T_{Bunnpris}	0.255***	0.176***	0.132***	0.223***	0.136**	0.019
	(0.061)	(0.040)	(0.032)	(0.063)	(0.041)	(0.056)
T_{Closure}	0.044	0.046	0.056	0.044	0.051	-0.061
	(0.064)	(0.079)	(0.061)	(0.071)	(0.078)	(0.068)
Market Definition	10–1.5 km	10–1 km	10–0.5 km	6–1.5 km	5–0.5 km	Postcode

Notes: Each column reports event-specific ATT estimates for a given local market definition, estimated following Sun and Abraham (2021). The data span all months of 2014–2016 except the last period before the event, and the standard errors shown in parentheses are clustered by municipality. Each ATT is retrieved from separate regressions reported fully in Tables OA.21–OA.24 in the Online Appendix. Controls for municipality-level population and income are included in each regression. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Event-study plots: ln(Sales), 10–1 km market definition, last period before event excluded

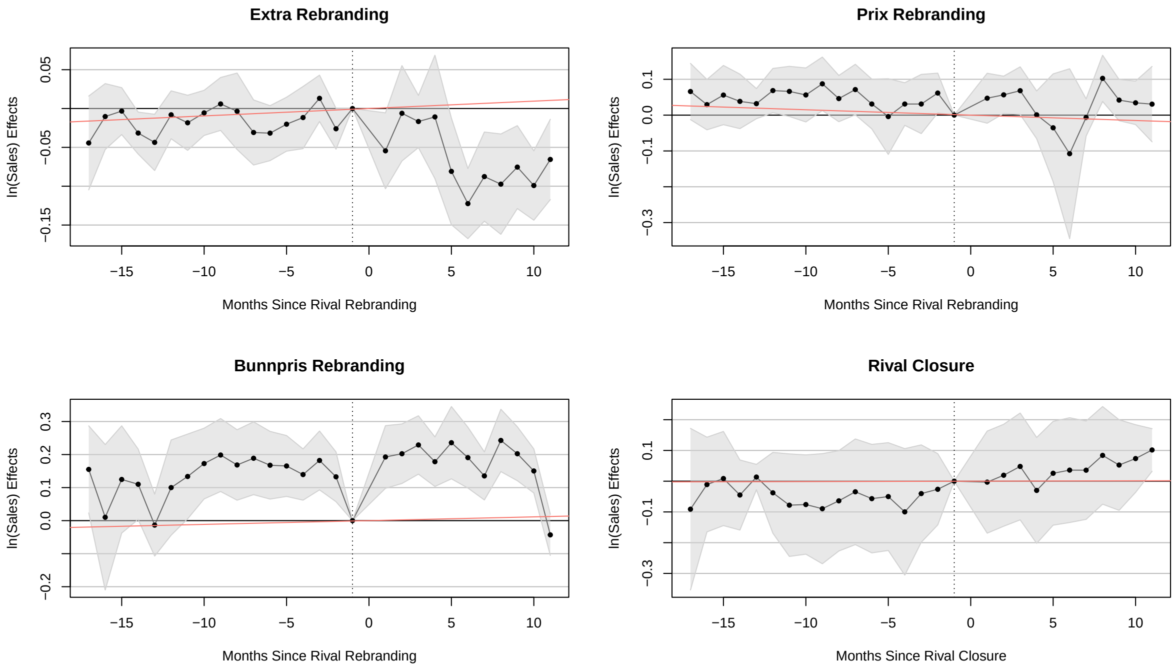

Figure 6.2: Estimated percentage sales effects of rival rebrandings and closures over time, excluding the last period before the event. The horizontal axis displays months relative to the excluded month, $l \in [-17, +11]$, with the vertical line marking $l = -1$—two months before the event. Point estimates are shown with 95% confidence intervals, based on standard errors clustered at the municipality level. A fitted linear pre-trend is displayed and extrapolated into the post-event period for comparison.

The estimated effect of a rival rebranding to Extra now ranges between -5.1% and -9.6% across specifications. As expected, given the high sales immediately before the event, these estimates are smaller than in the unadjusted specification but remain both economically and statistically significant. The ATTs for Prix change little. The estimates for Bunnpris increase substantially due to a negative outlier two periods before the event, while the estimated effect of rival closure becomes smaller and statistically insignificant because of a higher reference point. The S&A estimator is clearly sensitive to the choice of reference point, which the next robustness check addresses.

The parallel-trends assumption is not directly testable, but a long series of pre-event observations allows me to assess whether pre-trends could plausibly account for the observed post-event dynamics. Moreover, by imposing parametric restrictions on the pre-trends, one can extrapolate them into the post-period to estimate the ATTs under the assumption of common deviations from trends (Angrist and Pischke, 2015). Including linear trends relaxes the identifying assumptions: if trends are truly parallel, this specification remains unbiased, while if there are approximately linear differences in trends, the resulting bias could be reduced. Accordingly, I estimate an augmented version of Equation 6.1:

$$Y_{it} = \gamma_i + \lambda_t + \text{Treated} \cdot \text{Trend} + \sum_{l \geq 0} \mu_l \mathbb{1}\{t - E_i = l\} + \delta' \boldsymbol{Z_{it}} + \epsilon_{it} \tag{6.2}$$

This specification includes a treated-store-specific linear time trend and excludes lead indicators. The linear pre-trend is estimated using all pre-event observations and then extrapolated into the post-period. Consequently, post-event coefficients capture deviations from this extrapolated counterfactual rather than from the final pre-period. This approach reduces sensitivity to idiosyncratic noise in $l = -1$ (or any pre-period) and allows for linear differences in pre-trends between treated and control stores.

Table 6.2: Estimated effects of rival rebrandings on sales: group-specific linear trends and the last period before event excluded

	ln(Sales)					
	(1)	(2)	(3)	(4)	(5)	(6)
Trend – Extra	0.001	0.001	0.002	0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
T_{Extra}	-0.069*	-0.055	-0.108***	-0.044	-0.081***	-0.089**
	(0.029)	(0.030)	(0.022)	(0.032)	(0.023)	(0.030)
Trend – Prix	-0.002	-0.002	-0.001	-0.001	-0.002	0.001
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)
T_{Prix}	-0.003	-0.009	0.015	-0.012	0.028	-0.022
	(0.029)	(0.032)	(0.036)	(0.032)	(0.038)	(0.026)
Trend – Bunnpris	0.002	0.001	0.005	0.005	0.007	0.005
	(0.004)	(0.003)	(0.003)	(0.006)	(0.005)	(0.004)
T_{Bunnpris}	0.058	0.026	-0.015	-0.016	-0.049	-0.062
	(0.032)	(0.036)	(0.035)	(0.047)	(0.049)	(0.040)
Trend – Closure	0.000	0.000	0.001	-0.001	-0.001	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.002)
T_{Closure}	0.073	0.091*	0.083	0.092*	0.111	0.037
	(0.037)	(0.041)	(0.047)	(0.038)	(0.075)	(0.043)
Market Definition	10–1.5 km	10–1 km	10–0.5 km	6–1.5 km	5–0.5 km	Postcode

Notes: Each column reports event-specific ATT estimates and trend coefficients from Equation 6.2 for a given local market definition. The data span all months of 2014–2016 except the last period before the event, and the standard errors shown in parentheses are clustered by municipality. Each ATT and trend is retrieved from separate regressions reported fully in Tables OA.25–OA.28 in the Online Appendix. Controls for municipality-level population and income are included in each regression. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

The ATTs and treatment-group-specific trends estimated from Equation 6.2, with one pre-period excluded, are reported in Table 6.2. None of the treated groups exhibit statistically significant trends; however, some estimates are large enough in magnitude to meaningfully alter the baseline outcome over time, as illustrated in the event-study plots. The ATT estimates for Extra rebrandings are consistently negative, ranging from

−4.4% to −10.8% across specifications, indicating a somewhat smaller magnitude than in the unadjusted specification (Table 5.6). The ATT estimates for Prix remain close to zero, and while the estimates for Bunnpris are more sensitive to specification, they also remain close to zero on average. The estimates for rival closures remain qualitatively similar to the unadjusted specification, though slightly larger on average. Overall, the main conclusions of Section 5 remain robust to the inclusion of treatment-group-specific trends and the exclusion of the final pre-event period.

6.3 Heterogeneous Effects across Treatment Arms

Even when the conditions for identifying average treatment effects *on the treated* (parallel trends and no anticipation) are satisfied, selection into treatment arms may still be a concern. Stores selected for closure, for example, likely underperformed and may have exerted less competitive pressure on Kiwi than a typical rival. In that case, a positive ATT on sales from closures should be interpreted as a lower bound on the average competitive effect (ATE) of a representative rival. Similarly, the effects observed from conversions to Extra, Prix, or Bunnpris may not generalize. Counterfactual conversions could have produced different effects from those observed in markets where such conversions took place. Coop likely chose the target format to match local conditions and store characteristics to maximize profits. This suggests that the observed sales effects may be stronger in the markets where rebrandings occurred than they would be in other treated markets or in the population at large.

One such store characteristic is size, as illustrated in Figure 6.3, which shows the size distributions of Coop's formats Prix, Extra, and Mega. Extra stores tend to be larger than Prix, although there is substantial overlap.[35] If formats were chosen to fit existing facilities, ICA stores rebranded to Extra would on average be larger than those rebranded to Prix. If the stores that became Prix had instead been rebranded to Extra, they would likely not have generated as strong a business-stealing effect as the ATT estimated here, because the Extra format is less suited to smaller premises. Thus, differences in the ATT for Extra and Prix may partly reflect differences in the store sizes of rebranded rivals.

Taken together, these examples highlight that comparing ATTs across treatment arms requires caution. Even if the ATT can be identified separately for each treatment arm, differences across ATTs are not straightforward to interpret. To regard such differences as evidence of variation in the average effect of different conversion types—whether among all treated groups or in the population at large—one must at minimum assume that treatment effects are homogeneous across treatment groups, i.e., that all treated groups would experience the same effect if exposed to a given conversion. Without this assumption, differences in ATTs may instead reflect heterogeneity in how the same conversion affects

[35] Bunnpris resembles Prix more closely than Extra in this regard.

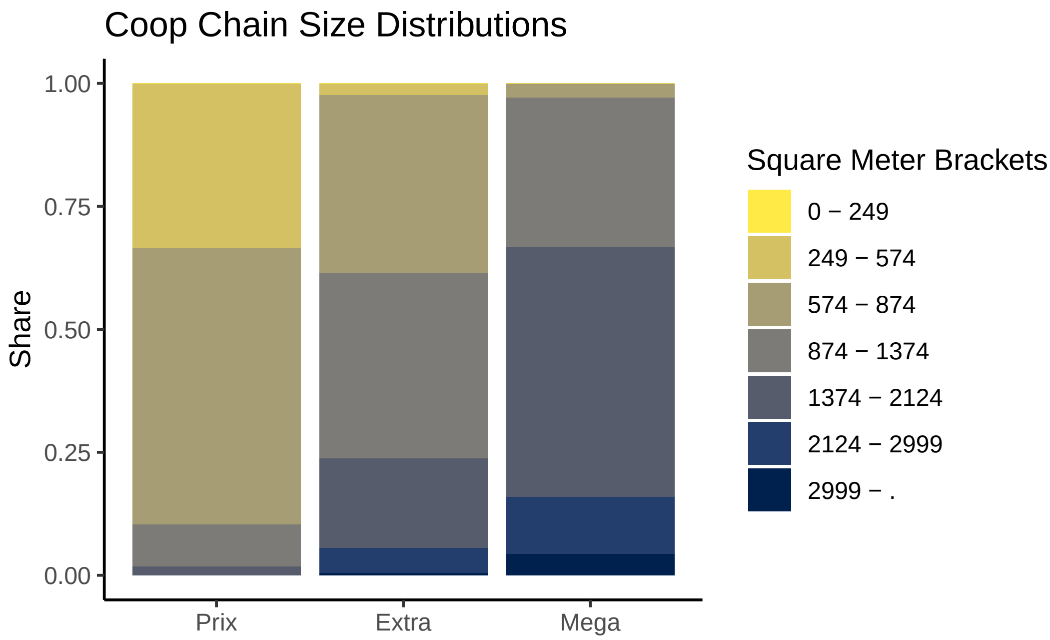

Figure 6.3: Share of stores within each size bracket for Coop's target formats in 2019. While distributions overlap, Coop's Extra stores are routinely larger than their Prix counterparts.

distinct groups of treated stores (Callaway et al., 2025). The cases discussed illustrate why such heterogeneity may arise, but they are not exhaustive.

7 Discussion and Conclusion

This paper provides an ex post evaluation of a major horizontal merger in Norway's concentrated grocery market: Coop's 2015 acquisition of ICA. While the merger increased national ownership concentration, it simultaneously reshaped local competition through extensive store-format repositioning. The evidence is consistent with consumer gains from the reallocation of complementary, firm-specific assets—ICA's scarce store locations and Coop's soft-discount concept—which facilitated extensive format conversions. Overall, the results underscore the role of firm heterogeneity: when complementary, hard-to-replicate assets are combined, mergers can strengthen both the ability and the incentive to compete, intensifying rivalry even as concentration rises.

The analysis yields several findings consistent with consumer gains despite higher concentration. First, although national ownership concentration rose substantially, the change in the sales-weighted local HHI was limited, in part due to divestiture remedies imposed by the Norwegian Competition Authority. Second, I find no evidence of local product-level price responses to rival store conversions, consistent with uniform national pricing in grocery retail (Meile, 2020). Effective store prices and assortment variety exhibit only modest adjustments, consistent with limited local discretion over these margins.

Third, despite the absence of local price reactions, national real food prices declined by 3.8–5.2% relative to a synthetic control constructed from neighboring countries. This decline is consistent with both a composition effect—replacing higher-priced formats with lower-priced soft discounters—and intensified competition as the soft-discount segment became more crowded. Fourth, store-level demand estimates imply that rival conversions to Coop's Extra format reduced sales for NorgesGruppen's soft-discount incumbent, Kiwi, by 4.4–12.9% across specifications, while conversions to convenience-discount formats generally had no significant effect. Rival store closures, by contrast, were associated with 2.4–11.1% higher sales. Taken together, these results suggest stronger competitive pressure despite rising concentration.

The fall in national food prices and the strong consumer response to Extra rebrandings are consistent with consumer gains from merger-related repositioning, especially where ICA stores were converted to Extra. The central mechanism appears to be the large-scale shift of acquired locations into the soft-discount segment, an outcome that may have been more difficult to achieve through stand-alone expansion.

As discussed in Section 2.3, the Norwegian Competition Authority's ex ante assessment emphasized ownership concentration and efficiency claims but did not explicitly consider potential pro-competitive effects arising from store-format conversions. The ex post evidence presented here suggests that mergers combining firm-specific complementarities may generate pro-competitive effects that merit greater attention in merger control. Such effects arise because firms differ in formats, locations, and capabilities. Mergers can realize complementarities among these firm-specific assets that would otherwise require costly investment, facilitating repositioning that benefits consumers directly or strengthening competitive incentives when products become closer substitutes.[36]

The net benefits of divestiture remedies may likewise depend on asymmetries across firms. Divesting stores to Bunnpris mitigated increases in ownership concentration but may also have reduced consumer benefits, given Bunnpris' higher prices, its weaker ability to compete in the large soft-discount segment, and indications that effective store prices at Kiwi tend to rise following Bunnpris rebrandings. For merger reviewers, these findings imply that remedies should be evaluated not only in terms of concentration and scale efficiencies but also in light of whether potential buyers possess assets that are complementary to those being divested. When prospective buyers differ substantially along these dimensions, divestitures may fail to preserve competitive pressure.

While the findings are robust across specifications, several limitations qualify their interpretation. Identification of national price effects relies on comparisons with neighboring countries and cannot fully rule out Norway-specific shocks. The store-level analysis isolates the response of one major rival and may not capture the full

[36] More generally, product repositioning can also harm consumers, either directly through less preferred characteristics or indirectly through its impact on competitive incentives.

industry adjustment, although it is plausible that the other established soft-discount chain was similarly affected by the store conversions. More broadly, causal inference remains challenging because mergers affect markets through multiple channels—ownership concentration, bargaining power, efficiencies, and product positioning—and uniform national pricing complicates the construction of local counterfactuals. Furthermore, the mechanisms highlighted here are most relevant when merging firms control assets that are costly to replicate; where similar complementarities can instead be achieved through contracts, entry, or internal investment, consolidation is unlikely to yield comparable gains.

Future research could extend this analysis in several directions. First, richer industry-wide data would allow tracking substitution patterns and competitive responses across all chains, not only the soft-discount incumbent studied here. Second, alternative quasi-experimental designs could further refine control groups or exploit additional sources of identifying variation to better estimate aggregate effects or isolate specific channels. Finally, the results on Bunnpris rebrandings suggest that the competitive effects of divestiture remedies may depend critically on the identity and characteristics of the buyer. More empirical work is needed to understand how the trade-off between reducing ownership concentration and disrupting potentially efficient asset combinations shapes remedy outcomes.

In sum, mergers can reallocate complementary, firm-specific assets in ways that materially reshape competition and consumer welfare. In this case, the merger enabled rapid repositioning that is consistent with consumer gains despite increased national concentration. While divestiture remedies aim to restore competition, they may fail to replicate the specific complementarities realized by the original buyer. Merger control should therefore weigh the potential gains from reduced concentration against the loss of firm-specific complementarities when designing remedies.

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Appendix

A Analysis of National Real Food Prices

This appendix details the estimation behind the national price results summarized in Section 5.1.1: a structural break analysis of Norwegian real food prices, a comparative difference-in-differences analysis against neighboring countries, and a synthetic control analysis.

A.1 Structural Break Analysis

I test for a structural break in the evolution of real food prices around the time of the acquisition by estimating the following equation:[37]

$$
\begin{aligned}
\ln\left(\frac{P_t^{Food}}{P_t^{All}}\right) = \beta_0 + \beta_1 t + \beta_2 \cdot \text{Post Acquisition} \\
+ \beta_3 t \cdot \text{Post Acquisition} + \beta_4 \cdot \text{Month} + \epsilon_t
\end{aligned}
\tag{A.1}
$$

where *Post Acquisition* is an indicator for the period after the acquisition (April 2015), and *Month* is a vector of monthly dummies. The time trend t is normalized to represent the number of months after the acquisition, so that β_2 captures the shift in real food prices at the time of the acquisition. The sample is restricted to 2011–2019 to avoid confounding effects from the Great Recession and the COVID-19 pandemic, which could each constitute separate structural breaks. The ICA takeover occurred gradually between April 2015 and July 2016, during which only a subset of ICA stores was rebranded.

Specification (1) in Table A.1 excludes the transition period from April 2015 to June 2016, during which a stable trend cannot be expected. Specification (2) replaces *Post Acquisition* with *Rebranding Share*, which measures the cumulative share of store rebrandings completed (as shown in Figure 3.1) and increases from 0 to 1 over the transition period. This allows inclusion of the full sample period and captures the change in real food prices associated with the complete set of 385 format rebrandings, under the assumption of a constant marginal effect.

For specification (1), real food prices trended slightly upward before the acquisition ($\beta_1 = 0.02\%$), after which the trend shifted downward ($\beta_3 = -0.12\%$), with no significant immediate level change ($\beta_2 = 0.87\%$).[38] The annualized growth rate was 0.24% before

[37] The structural break model is also referred to as an Interrupted Time Series design (Reichardt, 2019).

[38] During the study period, Norwegian grocery retailers and wholesalers negotiate prices and terms biannually, in February and July, which typically entails price increases. This may explain why the effects of the acquisition were not instantaneous.

Table A.1: Structural break analysis: real food prices, 2011–2019

$\ln(P_t^{Food}) - \ln(P_t^{All})$	(1)	(2)
Trend	0.0002*	0.0003**
	(0.0001)	(0.0001)
Post Acquisition	0.0087	
	(0.0068)	
Trend × Post Acquisition	-0.0012***	
	(0.0002)	
Rebranding Share		0.0071
		(0.0047)
Trend × Rebranding Share		-0.0013***
		(0.0001)
Constant	-0.0144***	-0.0128***
	(0.0031)	(0.0032)
Monthly Dummies	Yes	Yes
Observations	93	108

Notes: The sample covers January 2011 to December 2019. Driscoll–Kraay standard errors are reported in parentheses. The transition period (April 2015–June 2016) is excluded in model (1). *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

the acquisition and declined by 1.43% afterward.[39] Specification (2) yields similar results, with an annualized growth rate of 0.36% before and a reduction of 1.55% after. Both specifications imply a post-acquisition annualized growth rate of -1.19%, and by the end of 2019, the trajectory of real food prices was 5.85% lower relative to the pre-acquisition trend. Specification (1) is illustrated in Figure 5.2 in Section 5.1.1.

A.2 Comparative Structural Break Analysis

The difference-in-differences approach typically takes the form of a two-way fixed effects regression:

$$\ln\left(\frac{P_{it}^{Food}}{P_{it}^{All}}\right) = \alpha_i + \lambda_t + \alpha_5 \cdot \text{Post Acquisition} \cdot \text{Norway} + \epsilon_{it} \tag{A.2}$$

where α_i and λ_t are fixed effects, and *Norway* is an indicator variable. Column 3 of Table A.2 reports the results of this regression, indicating that Norwegian food prices declined by 5.1% after the acquisition relative to the comparison countries. The time fixed effects capture trends of any nonparametric form common across countries. However, with a small cross-section, each fixed effect is estimated from relatively few observations. To address this concern, the specification in column 2 of Table A.2 replaces the time fixed effects with a before-after indicator, *Post Acquisition*, and monthly dummies. This alternative

[39] Calculated as $(1 + \text{monthly growth rate})^{12} - 1$.

specification yields a similar estimated decline in food prices of 5.23%, but assumes no time trend, other than discrete shifts in food prices at the time of the acquisition, which may differ between Norway and the comparison countries.

An alternative approach is to include linear time trends, which can be consistently estimated given the longer time dimension, under the assumption that these trends are linear. By introducing country-specific linear trends, we can dispense with the assumption of common trends and instead assume common deviations from trends (Angrist and Pischke, 2015).[40] I estimate the following equation to examine how real food prices evolved in Norway relative to neighboring countries:

$$\begin{aligned}
\ln\left(\frac{P_{it}^{Food}}{P_{it}^{All}}\right) = {} & \alpha_i + \alpha_1 t + \alpha_2 \cdot \text{Post Acquisition} + \alpha_3 t \cdot \text{Post Acquisition} \\
& + \alpha_4 t \cdot \text{Norway} + \alpha_5 \cdot \text{Post Acquisition} \cdot \text{Norway} \\
& + \alpha_6 t \cdot \text{Post Acquisition} \cdot \text{Norway} + \alpha_7 \cdot \text{Month} \cdot \text{Country} + \epsilon_{it}
\end{aligned} \tag{A.3}$$

where α_i are country-specific intercepts and *Country* denotes the vector of country indicators. The specification allows for separate trends and levels for Norway and the control group, both before and after the acquisition. Allowing for country-specific trends and structural breaks within the control group and then comparing Norway with the average control country yields nearly identical results. For simplicity, common trends and breaks are therefore assumed within the control group, while seasonal effects remain country specific. The results, reported in column 1 of Table A.2, include tests for structural breaks in both groups to validate the findings in Table A.1.

[40]See Nilsen et al. (2016) for an application of this approach to evaluating an upstream merger in the Norwegian egg market.

Table A.2: Difference-in-differences analysis: real food prices, 2011–2019

$\ln(P_t^{Food}) - \ln(P_t^{All})$	(1)	(2)	(3)	(4)	(5)	(6)
Trend	0.0005***			0.0006***		
	(0.0001)			(0.0000)		
Trend × Norway	-0.0004**			-0.0003**		
	(0.0001)			(0.0001)		
Post Acquisition	-0.0022	0.0301***				
	(0.0030)	(0.0034)				
Post Acquisition × Norway	0.0109	-0.0523***	-0.0510***			
	(0.0081)	(0.0065)	(0.0084)			
Post Acquisition × Trend	-0.0000					
	(0.0001)					
Post Acquisition × Trend × Norway	-0.0012***					
	(0.0002)					
Rebranding Share				-0.0008	0.0279***	
				(0.0026)	(0.0037)	
Rebranding Share × Norway				0.0079	-0.0481***	-0.0483***
				(0.0058)	(0.0074)	(0.0088)
Rebranding Share × Trend				-0.0001		
				(0.0001)		
Rebranding Share × Trend × Norway				-0.0012***		
				(0.0001)		
Constant	-0.0144***	-0.0181***	-0.0234***	-0.0128***	-0.0177***	-0.0224***
	(0.0031)	(0.0038)	(0.0032)	(0.0031)	(0.0038)	(0.0032)
Monthly Dummies	Yes	Yes	No	Yes	Yes	No
Time Fixed Effects	No	No	Yes	No	No	Yes
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	465	465	465	540	540	540

Notes: The panel includes Norway, Sweden, Denmark, Germany, and Iceland, spanning January 2011 to December 2019. Driscoll–Kraay standard errors are reported in parentheses. The transition period (April 2015–June 2016) is excluded from models (1)–(3). *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

The results in column 1 of Table A.1 can be derived from the structural break regression in Table A.2.[41] The difference is that the difference-in-differences specification moderates changes in Norway's trend and level by those observed in the comparison countries. If there had been a structural break common among the comparison countries, regardless of the acquisition, it would be captured by the control group. Coefficient α_5 measures the difference in level shifts after the acquisition between Norway and the control group. Neither Norway nor the control countries exhibit level shifts significantly different from zero or from each other. Coefficient α_6 measures the difference in trend shifts after the acquisition. The absence of a trend change among the control countries indicates that

[41] Coefficients $\beta_1 = 0.0002 \approx 0.0005 - 0.0004 = \alpha_1 + \alpha_4$, $\beta_2 = 0.0087 \approx (-0.0022) + 0.0109 = \alpha_2 + \alpha_5$, and $\beta_3 = -0.0012 \approx (-0.0000) - 0.0012 = \alpha_3 + \alpha_6$.

the observed shift in Norwegian real food prices was not driven by common regional factors ($\beta_3 = \alpha_6 = -0.0012$). However, coefficient $\alpha_4 = -0.0004$ shows that Norway and the control countries had slightly different average pre-acquisition trends (approximately 0.5% annualized growth), violating the common-trends assumption and underscoring the importance of accounting for differing pre-trends.

Columns 4–6 replicate the preceding analysis but replace the *Post Acquisition* indicator with the variable *Rebranding Share*. The results are similar, though the estimated effects in columns 5 and 6 are slightly smaller than their counterparts in columns 2 and 3, at approximately -4.8%.

A.3 Synthetic Control Analysis

The preceding analysis assumes either that real food prices follow a linear trend or that the comparison countries share a common trend on average. In practice, real food prices occasionally deviate from linear trends, and the unweighted average of comparison countries does not perfectly parallel Norway's pre-acquisition trajectory. The synthetic control method of Abadie and Gardeazabal (2003) accommodates both non-linear and non-parallel trends by constructing a weighted average of comparison countries that better reproduces Norway's path. The underlying idea is that while outcomes are driven by common factors, differences in factor loadings can generate non-parallel trends. These loadings are unobserved, but only a synthetic control that closely matches Norway's loadings is likely to replicate its pre-acquisition history—especially when the pre-period is long and the data relatively stable (see Abadie (2021) for an in-depth discussion).

The goal is to identify the potentially time-varying effects of the intervention, defined as $\tau_{1t} = Y_{1t}^I - Y_{1t}^N$, where unit 1 is the treated unit and Y_{1t}^I and Y_{1t}^N denote the potential outcomes with and without the intervention. The counterfactual outcome is unobserved but imputed as a weighted combination of control units, yielding the estimator

$$\hat{\tau}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j Y_{jt}$$

where the weights w_j are non-negative, sum to one, and are chosen to best reproduce the pre-acquisition history of Norwegian food prices.

The credibility of the synthetic control estimate increases when the model closely reproduces Norway's pre-acquisition outcomes over a long horizon, so I extend the pre-acquisition period to 2005. As emphasized by Abadie (2021), filtering volatile series helps prevent overfitting. I therefore consider two specifications: one that controls for country-specific monthly effects, and another that applies the Hodrick–Prescott filter with $\lambda = 14400$ (Hodrick and Prescott, 1997).[42] All series are normalized to have mean zero in

[42]This is a standard choice for monthly data and performs well here, smoothing cyclical fluctuations

the pre-acquisition period, thereby controlling for level differences analogous to unit fixed effects in a difference-in-differences framework. To reduce interpolation bias arising from large cross-country discrepancies, I restrict the donor pool to Nordic countries.[43]

Figure A.1a plots deseasonalized real food prices for Norway and its synthetic control over 2005–2019, along with their difference. The average post-acquisition decline in real food prices is estimated at −5.2%. The synthetic control tracks Norway's price dynamics reasonably well, although the pre-acquisition fit is imperfect due to substantial volatility. Applying the HP filter ($\lambda = 14400$) improves the pre-acquisition fit, as shown in Figure A.1b, and yields an estimated decline of −4.7%. Synthetic control weights are reported in Table A.3. Restricting the sample to 2011–2019—the window used in the structural break and difference-in-differences analyses—produces an almost perfect pre-acquisition fit under both transformations (Figures OA.7a and OA.7b in the Online Appendix), reflecting the stable linear trends in this period, and implies a post-acquisition decline of −3.8%. Across all specifications, the estimated effects are broadly consistent with the difference-in-differences estimates.

in food prices while preserving larger shifts.

[43]Including Ireland, whose trend differs from Norway's, marginally improves the pre-acquisition fit by offsetting countries with steeper trends. However, Ireland also exhibits a pronounced downward trend relative to Norway, which can introduce interpolation bias (Abadie, 2021).

Table A.3: Synthetic control weights

	Deseasonalized 2005–2019	HP-filter 2005–2019	Deseasonalized 2011–2019	HP-filter 2011–2019
Country	Weight	Weight	Weight	Weight
Denmark	0.279	0.313	0.477	0.538
Finland	0	0	0	0
Iceland	0.511	0.687	0.143	0.462
Sweden	0.21	0	0.38	0

B Tables and Figures

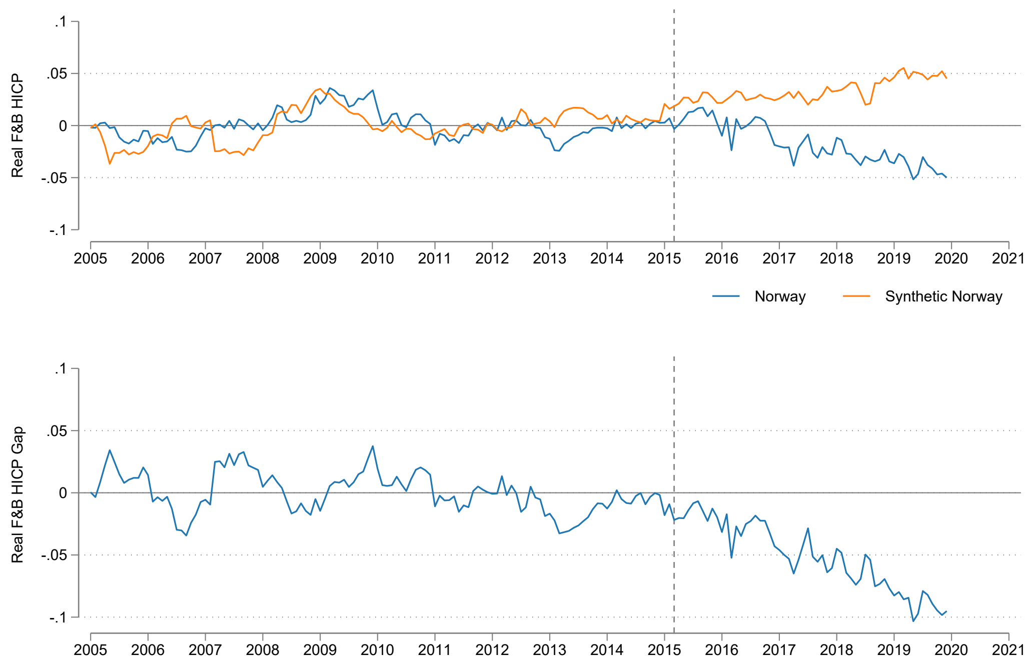
Table B.1: Retail chain segmentation

Retail Chain	Umbrella Group	Market Segment
Joker	NorgesGruppen	General Store
Coop Marked	Coop	General Store
Nærbutikken	NorgesGruppen	General Store
Matkroken	ICA (then Coop)	General Store
Prix	Coop	Convenience Discount
Bunnpris	Bunnpris	Convenience Discount
Rimi	ICA	Convenience Discount
Lidl	Lidl	Hard Discount
Kiwi	NorgesGruppen	Soft Discount
Rema 1000	Reitangruppen	Soft Discount
Extra	Coop	Soft Discount
Eurospar	NorgesGruppen	Supermarket
Meny	NorgesGruppen	Supermarket
Coop Mega	Coop	Supermarket
ICA Supermarked	ICA	Supermarket
Spar	NorgesGruppen	Local Supermarket
obs!	Coop	Hypermarket

Notes: This table shows Norwegian retail chains, their ownership, and market segmentation.

Synthetic control analysis: real food prices, 2005–2019

(a) Deseasonalized (monthly fixed effects)



(b) HP-filtered ($\lambda = 14400$)

Figure A.1: Percentage deviations in food prices relative to overall prices for Norway and its synthetic control, computed as $\ln(P_t^{Food}) - \ln(P_t^{All})$. Panel (a) removes monthly fixed effects (deseasonalization), while Panel (b) applies an HP filter ($\lambda = 14400$). The lower part of each panel shows the gap between Norway and its synthetic control. The vertical line marks March 2015, preceding the first store rebranding. Synthetic control weights (Table A.3) minimize pre-acquisition differences. The donor pool is restricted to Nordic countries. *Source:* Eurostat.

Table B.2: Descriptive statistics: treatment and control groups

	Control (484)			Extra (38)			Prix (20)			Bunnpris (12)			Closed (26)		
	Before	After	%	Before	After	%	Before	After	%	Before	After	%	Before	After	%
Sales (millions NOK)	2.68	2.97	10.82%	3.19	3.19	0.00%	2.81	3.27	16.37%	2.63	3.16	20.15%	2.76	3.28	18.84%
Assortment Size	2166	2266	4.62%	2228	2304	3.41%	2130	2264	6.29%	2157	2300	6.63%	2226	2329	4.63%
Assortment Size (Min. 100 units)	1427	1944	36.23%	1427	1965	37.70%	1383	1945	40.64%	1388	1971	42.00%	1465	1990	35.84%
Assortment Entropy	6.818	6.835	0.25%	6.861	6.864	0.04%	6.805	6.825	0.29%	6.828	6.851	0.34%	6.806	6.816	0.15%
Effective Store Price	48.6	51.9	6.79%	47.6	51.2	7.56%	47.8	51.3	7.32%	48.9	52.5	7.36%	49.6	53.3	7.46%
Local Product Price Deviation	0.0	0.0	0.00%	0.0	0.0	0.00%	0.0	0.0	0.00%	0.0	0.0	0.00%	0.0	0.0	0.00%
Number of Competitors	1.5	1.4	-6.67%	2.6	2.6	0.00%	2.2	2.1	-4.55%	2.8	2.8	0.00%	4.3	3.2	-25.58%
Herfindahl–Hirschman Index	0.62	0.62	0.00%	0.36	0.37	2.78%	0.39	0.41	5.13%	0.34	0.36	5.88%	0.28	0.33	17.86%
Population (in thousands)	124.9	128.2	2.64%	119.6	122.2	2.17%	151.2	130.5	-13.69%	114.1	142.9	25.24%	44	43.2	-1.82%
Population Density (per square km)	406.1	417.7	2.86%	453.7	443.7	-2.20%	390.4	358	-8.30%	334.9	401.1	19.77%	169.5	170.3	0.47%
Median Income (thousands NOK)	488	500.6	2.58%	494.5	503.5	1.82%	466.2	483.8	3.78%	474.2	488.2	2.95%	509.8	518.5	1.71%
Higher Education Share	0.31	0.32	3.23%	0.35	0.35	0.00%	0.29	0.31	6.90%	0.28	0.31	10.71%	0.27	0.28	3.70%
Employees	20.8	19.7	-5.29%	27.8	22.7	-18.35%	24.7	20.6	-16.60%	22.4	21.4	-4.46%	20.4	20.2	-0.98%
Postal Office Present Share	0.28	0.28	0.00%	0.16	0.15	-6.25%	0.27	0.31	14.81%	0	0	–	0.21	0.19	-9.52%

Notes: This table reports mean values of outcome and control variables measured at the store-month level. Values are shown for treatment and control stores before the first (June 2015) and after the last (July 2016) store rebranding in the vicinity of Kiwi. Stores are defined as treated if they shared a postcode with ICA in 2014 and are categorized by the rival format rebranding they experienced. I exclude 32 stores that either experienced multiple format rebrandings or a change in local market concentration. The number of stores is indicated in parentheses next to each group label. Assortment size is measured as the number of unique products sold. Assortment Size (Min. 100 units) is calculated in the same way but restricted to products with at least 100 units sold in every period. Assortment entropy measures variety while accounting for sales shares (see Section 5.2 for details). Effective store price is the expenditure-weighted average product price. Local product price deviation is the expenditure-weighted average deviation from the mean product price across stores. The Herfindahl–Hirschman Index is calculated using accounting sales. Demographic variables are measured at the municipality level.

Table B.3: Product category expenditure shares

Product Categories	Expenditure Share
Beverages	23.5%
Frozen Food	5.3%
Fresh Pastries	5.1%
Ready-to-eat Meals	11.7%
Fresh Fish and Shellfish	1.4%
Fresh Meat	7.2%
Home Supplies	4.0%
Kiosk Goods	10.9%
Dairy Products	19.9%
Cosmetics	3.8%
Other	7.3%

Notes: This table shows the product categories included in the analysis and their expenditure shares.