The Impact of Retail Mergers on Food Prices: Evidence from France[*]

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Abstract

This paper analyzes the impact of a merger in the French supermarket industry on food prices. We stress the importance of considering whether price decisions are taken at the local or at the national level. Using consumer panel data, we perform a difference-in-differences analysis. We provide a novel approach to define local areas affected by a merger when merging firms set prices nationally while other firms price locally. On average, we find that the merging firms significantly raised their prices after the merger but rather nationally than locally. The merger caused a significant increase of the competitor's prices, which is stronger in local markets in which more merging firms operate, and in which differentiation changed after the merger.

1 Introduction

Over the last thirty years, successive merger waves have dramatically increased food retail sector concentration in most western economies. In 2000, in the US, the largest five retail groups realized close to one third of total food sales. According to the American Antitrust Institute, the number of supermarket mergers in the US has increased from 20 in 1996, to 25 in 1997, and to 35 in 1998 (Foer, 1999). In 1999 alone, the Federal Trade Commission (FTC) reviewed and approved two of the most important supermarket mergers: Albertson's acquisition of American Stores (the second and fourth largest chains in the US) and Kroger's acquisition of Fred Meyer. This second merger created the largest US grocery chain and the second largest retailer in the US in terms of revenue, behind Wal-Mart. Western European countries are also characterized by highly concentrated retail sectors that have become more concentrated, with merger waves happening since the 1980s. The highest concentration ratios are attained in the northern European countries, where the total market share of the largest three retailers (CR3) reaches up to 90%.[1]

Supermarket mergers are a particularly important issue for antitrust authorities because food expenditures represent a large share of household budget - about 13% on average in European countries for 2012, and 7% in the US.[2] Large price variations due to a retail merger may have a large impact on consumer surplus. When reviewing retail mergers, two particular features of the retail sector, namely the local dimension of competition and buyer power, make the antitrust analysis more complex. First, because supermarkets compete at the local level, the effects of a merger have to be analyzed for each local relevant market. Second, antitrust authorities have to balance potential anticompetitive effects against efficiency gains due to synergies, as in all merger cases, but also against gains induced by buyer power. Indeed, the merged retailer is likely to obtain better terms and conditions from its suppliers, and to pass on part of this price reduction to consumers. Increased buyer power can thus lead to a welfare-enhancing reduction in final prices: this effect is specific to the vertical structure of the retail industry and explains why competition authorities may be more prone to clear mergers in the retail industry than in other sectors. For instance, between 1998 and 2007, the FTC approved 134 supermarket mergers for a total of 153 cases under investigation.[3] Among the 100 retail mergers proposed between 1990 and 2012 to the European Com-

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[1] In 2004, the retail CR3 was 91.2% in Denmark, 79.6% in Finland, 81% in Iceland, 82% in Norway, and 91.2% in Sweden (Einarsson, 2007), while in 2003, the CR5 was 72.6% in France, 67.8% in Germany, 69.1% in Spain, 68.5% in Portugal and 63.5% in the UK. Note that in Italy, the retail sector remains rather traditional with a CR5 close to 40%.

[2] Sources: Eurostat (`http://epp.eurostat.ec.europa.eu/statistics_explained/index.php/Household_consumption_expenditure_-_national_accounts`) and USDA (`http://www.ers.usda.gov/data-products/food-expenditures.aspx#.UpMmqhCPglA`).

[3] See Table 4.2 `http://www.ftc.gov/os/2008/12/081201hsrmergerdata.pdf`.

mission (EC), 89 were approved, 8 were approved subject to conditions, and only 2 were denied.[4]

The aim of this paper is to analyze retrospectively the impact of a merger among supermarkets on food prices in France. In 1999, the second largest retail group launched a take-over bid over the fifth largest retail group.[5] This merger was approved by the EC and the French Competition Authority (French CA) in the year 2000. Together, the new group had almost 30% market share. The corporate decision to merge was made at the national level. The merging firms kept almost all their existing store locations, but rebranded two of the pre-existing retail chains. Our research question is twofold: First, we investigate whether this approved merger caused prices to increase. Second, we empirically assess potential economic forces inducing the price changes due to the merger.

We benefit from an exceptional database, which provides a unique setting to define local markets as catchment areas around each store, in order to capture the local dimension of retail competition. The data record food consumption and prices at the store level from a consumer panel (Kantar *TNS Worldpanel*), and data on the French retail sector (location and characteristics of the stores) for the years 1998-2001, i.e., before and after the merger.

In our identification strategy and empirical analysis, we take advantage of the fact that, before the merger, the two merging firms were not operating in all local areas. Because the merger was approved at the national level, it was implemented in all local areas where merging firms were present. In what follows, we refer to the merging firms as "the insiders" and to the other retailers as "the outsiders". We run a Difference-in-Differences analysis (DID), in which we quantify the price effects caused by the merger by comparing price changes in local markets affected by the merger (treated areas) to price changes in markets unaffected by the merger (control areas). We use two definitions of the treatment and control groups. First, we define the treatment group as any local market that experienced a change in local concentration after the merger. This is the standard definition used in the retrospective merger evaluation literature, and applied by competition authorities. We also analyze retailer's pricing strategies in the pre- and post-merger periods. We find that prices are correlated with local concentration only at outsiders' stores. Moreover, we find evidence that, after the merger, insiders raised their prices nationally rather than locally. This leads us to consider a second definition in which the treatment group includes all local markets

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[4]For instance, in 1997, the EC prohibited the merger between two leading food retail chains in Finland, Kesko and Tuko (see, 97/277/EC Kesko/Tuko (OJ L 110/53, 26/4/1997)). In 1999, the merger in Austria between Rewe and Meinl was allowed conditional on divestment of some stores (see, 1999/674/EC Rewe/Meinl (OJ L 274/1, 23/10/1999)).

[5]Due to a confidentiality agreement with *TNS Worldpanel*, which provided us the data, we are not allowed to disclose the retailers' names. The ranking is based on store surface market shares, source: *Panorama Tradedimensions.*

in which at least one insider is active. Indeed, if insiders decide on a national price increase after the merger, any local market including a single insider can be affected by the merger. In that case, there are only outsiders in the control group.

Our results show that the approved merger caused a significant price increase at the outsiders' stores of approximately 2% (between 1.8% and 2.4%). The merger is also correlated with an increase in insiders' prices of about 4 to 5%. By decomposing this effect further, we show that the price increase caused by the merger at outsiders' stores is larger in local markets in which outsiders face a larger number of insiders' stores, or in which, due to the rebranding operations, the merger reduced the number of competing chains.

This paper fits into a growing economic literature which attempts to evaluate whether approved mergers actually increased prices, in a context of some experts stating that the US antitrust policy towards horizontal mergers has been too lenient (Ashenfelter, Hosken and Weinberg, 2014). Historically, empirical mergers analysis goes in two main directions and there is a lively debate between the two approaches (Angrist and Pischke, 2010; Nevo and Whinston, 2010). First, some papers, in the spirit of Nevo (2000), build structural models of demand and supply in order to simulate mergers using pre-merger data. Smith (2004) simulates structural changes in the UK supermarket industry, and finds that retail divestitures reduce prices while mergers increase prices. A second stream of empirical papers uses both pre- and post-merger data on prices to directly estimate the effects of structural changes and mergers (such as Focarelli and Panetta, 2003 for retail banking; Hastings, 2004, Hastings and Gilbert, 2005, Taylor and Hosken, 2007 – all three papers in retail gasoline; Hausman and Liebtag, 2007 and Basker and Noel, 2009 for retail entry; Ashenfelter and Hosken, 2010; Ashenfelter, Hosken and Weinberg, 2015 for food and non-food grocery sectors; and Ashenfelter, Hosken and Weinberg, 2013 for the home appliance sector). Recently, Houde (2012) conducts both a retrospective analysis and a structural econometric simulation of a vertical merger in the Canadian gasoline sector, and reconciles both approaches.[6]

Considering the US supermarket industry, Davis (2010) examines post-merger price changes using store-level scanner data and shows that chains reduce promotions after a merger, both in terms of depth and frequency. The most closely related study to date is by Hosken, Olson and Smith (2012), who examine the price effects of a large set of national US retail chain mergers occurring over a period of time. They find geographically heterogeneous price effects. The implication of these findings is that mergers should be analyzed at the local level, as we do. Choné and Linnemer (2012) analyze the price effect of a merger on the parking market in Paris based on a systematic definition of the control and treatment groups, which accounts for the fact that seemingly distant entities may be affected through indirect channels.

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[6]See also Weinberg and Hosken (2013), Weinberg (2011), or Björnerstedt and Verboven (2015).

Our paper extends this stream of retail literature in three directions by taking advantage of an exceptional database at the store level, which enables us to causally identify localized price effects of a merger. First, we identify that retail pricing strategies are determined at two levels: national and local. Very few papers have analyzed this aspect of retail competition. Dobson and Waterson (2005) develop a model of chain-store pricing where retailers can either price uniformly across the local markets or on a local basis according to market conditions. They compare the profitability of both strategies, and show that both can be optimally chosen, according to market conditions. This dimension of the pricing strategy has implications on the assessment of the price effect of a merger, as shown in Allain, Chambolle and Turolla (2015). Aguzzoni et al. (2015) introduce this issue as they track the price effect of a merger in the book retail sector in the UK. They assume first that all retailers have a local pricing strategy, then that they all have a national pricing strategy. In both cases, they conclude that the merger had no impact on prices. In this paper, we go further by considering the coexistence of firms with a local pricing strategy (outsiders) and firms with a national pricing strategy (insiders). We show how the interaction between firms' pricing strategies drive the effect of the merger on prices: a national price increase at the insiders' may trigger a local price increase by outsiders. Second, we lead this retail merger analysis on a large set of products, rather than just focusing on one product category as often done in the literature. Third, the contribution of our paper is not only to estimate the local causal effect of a merger on prices, like previous related papers, but to test several economic mechanisms at play behind the price responses to a retail merger. In particular, we highlight the role of retail store rebranding on retail prices.

The rest of the paper proceeds as follows. Section 2 provides the background of the French retail sector and an overview of the merger case. Section 3 describes the data. Section 4 provides a general analysis of retailer's pricing strategies. The empirical strategy followed to analyze the retail merger is detailed in Section 5. In Section 6 we present and discuss the results. We perform several robustness checks in Section 7. Finally, Section 8 concludes and discusses the policy implications of our results.

2   The Market and the Merger

At the end of August 1999, the second largest retail group (henceforth M_1) proposed a friendly take-over bid over the fifth largest retail group (henceforth M_2). The EC approved the merger on January 25, 2000, on the condition that $M1$ realizes some divestments. It then delegated the decision to the French and Spanish competition authorities in order to assess the impact of the merger on retail competition at the local level in the two countries where the firms had large market share. The French CA concluded that competition was likely to be affected in 27 local areas. However, the

remedies required were not all enforced by the French Ministry of Economics, and the merger finally received final administrative approval on May 3, 2000.

In what follows, we provide some background on the French food retail market structure and the regulatory environment in Section 2.1, before giving a more detailed overview of the main facts about the merger in Section 2.2.

2.1   Market Structure and Regulatory Framework

In 2000, i.e., before the merger, the French retail sector was already rather concentrated: the total market share of the five main retail groups (CR5) was close to 73%, a rather high concentration compared to the UK or Germany (respectively 64 and 57%). According to the French CA estimates, in the overall retail market, the joint market share of the two merging groups, henceforth called the insiders and denoted $M1$ and $M2$, was around 29.4%, while most of the remaining share was split between the largest rivals, henceforth called outsiders and denoted Oi, with $O1$ (15.4%), $O2$ (15.1%), $O3$ (13%), and $O4$ (9.9%).[7]

According to the standard categorization of stores, there are four main store formats in the French food retail sector. *Hypermarkets* are large grocery stores with a selling surface over 2,500 m^2, which sell both food and non-food products (on average, food accounts for at least one third of their sales). They are generally located outside of the main cities. *Supermarkets* are smaller, but located closer to the city centers: their selling surfaces range from 400 to 2,500 m^2. Compared to hypermarkets, these stores offer a reduced assortment of products, and are more specialized in food products (more than two thirds of their sales). *Convenience stores* have a selling surface below 400 m^2. Finally, *discount stores* are (usually small) supermarkets that carry a limited assortment of products, mostly sold at low prices and under private labels.[8] In 2001, the food expenditure of French households was split as follows: 34.7% in hypermarkets, 29.9% in supermarkets, 8.5% at convenience stores, and 16.3% at specialized shopkeepers, such as butchers, and bakers.[9]

Two laws, the Galland Act and the Raffarin Act enacted in 1996 have had a deep effect on competition and prices, and expert reports, as well as academic papers, point out that these two laws contributed to the reduction of retail competition. First, the Galland Act aimed at preventing below-cost pricing. A side effect of this law was

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[7]The French CA uses Nielsen data to compute these estimates. The report also displays the joint market shares by format provided by the two groups: 31.2% of hypermarket sales, 22.3% of supermarket sales, 16.1% of hard discounts' and overall 26.9% of the whole grocery retailing sales. Computing the market shares in terms of selling surface does not strongly modify these figures: in 1998, $M1$ owns 20.2% and $M2$ 10.3% of total hypermarkets surface, while for supermarkets these figures are 9.8% for $M1$ and 16.4% for $M2$, and for hard discount $M1$ has 15.1%.

[8]In 2000, the market share of private labels in France was around 22.1% in volume and 19.1% in value (source: PLMA / Nielsen/ Allain and Chambolle, 2003).

[9]Source: INSEE, Tableaux de l'Economie Francaise 2002/2003.

to allow for the use of price-floors in the retail sector, which encouraged a raise in retail prices (see Allain and Chambolle, 2011, for a study of the price-floor mechanism involved by the law).[10] Second, the Raffarin Act increased administrative control of the opening of new supermarkets and of the extension of existing supermarkets. Experts also claim that the Raffarin Act had a strong effect on retail competition. By increasing barriers to entry, this law has limited the "organic" growth of retail groups, triggering important merger operations that have led to an increase in the retailers' market power.

Besides, in 2002 the monetary change (French Franc disappeared as the Euro was launched on January 1, 2002) is also likely to have had an effect on retail prices.[11] In order to avoid these two sets of shocks that are orthogonal to the merger, we focus our merger analysis on the period 1998-2001. We concentrate our analysis on the short-term effect of the merger. This will enable us to distinguish competitive effects from the unobserved efficiency gains from reorganization that can reasonably be expected to materialize after a few years (e.g., Focarelli and Panetta, 2003; Hastings, 2004 or Houde, 2012). However, cost reductions due to renegotiation of supply contracts may be immediate.

2.2   The Merger

The merger created the largest retail group in France, where M_1 and M_2 gathered around 220 hypermarkets and 1100 supermarkets, and had a significant impact on concentration measures in the market during the period 1998-2001. According to the EC horizontal merger guidelines, a merger is likely to raise competition concerns if the post-merger Herfindhal-Hirshman Index (HHI) is above 2000, while the variation is above 150.[12] Panel A of Table 1 displays the evolution of the HHI before and after the merger, at both the regional and national levels.[13] At the regional or national levels, concentration is low enough for the merger to be approved without conditions. However, the local dimension of the retail market calls for a local assessment of the merger. For each store, we can compute a local concentration index (HHI) using the definition of local markets explained in more details in Section 3.3. Panel B of Table 1

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[10]For expert reports, see, e.g., Commission Hagelsteen (2008) or Allain, Chambolle and Vergé (2008) for a review.

[11]The introduction of the Euro has led to extensive discussion about its possible effect on inflation, and the economic literature points out ambiguous conclusions. Dziuda and Mastrobuoni (2009), for instance, show that, although the Euro changeover did not significantly increase inflation, it nevertheless had a distortionary effect on prices inside the Euro-zone. After the changeover, cheaper goods had higher inflation, and this effect was significant in France.

[12]See "Guidelines on the assessment of horizontal mergers under the Council Regulation on the control of concentrations between undertakings", 2004, III, § 16.

[13]We do not have sufficient data to build the concentration measure upon real market shares. However, it is widely admitted that store sales are highly correlated to their selling area. Therefore, we base the concentration index on store surface area rather than turnover or quantities sold: the HHI in one market area is then the sum of the squared share of total retail surface for each retail group.

Table 1: Change in Market Concentration Before and After the $M1 - M2$ Merger

Panel A: Regional and National levels

	Paris	East	North	West	Central-W.	Central-E.	South-E.	South-W.	**France**
2000Q1	1599	1171	1261	1510	1430	1325	1498	1551	**1214**
2001Q1	2168	1242	1693	1735	1769	1683	1846	1811	**1534**
ΔHHI	+569	+71	+432	+225	+339	+358	+348	+260	**+320**

Panel B: Local market level

	p_{25}	p_{50}	p_{75}	Mean (S.E.)	Min.	Max.
2000Q1	1939	2424	3310	2939 (16)	1389	10000
2001Q1	2332	2658	3497	3180 (15)	1430	10000
ΔHHI	+393	+234	+187	+241 (5)	–	–

Notes: The table reports the Herfindahl-Hirschman Index (HHI) calculated at the retail group level three months before the merger (2000 Q1) and three months after (2001Q1). In Panel A, regions are defined according to the *TNS Worldpanel* classification. In Panel B, local markets are delimited with the baseline definition (20/10 km) used throughout the paper (see Section 3.3). The 25th, 50th and 75th percentiles of the distribution of the local HHIs are reported. The change in the HHI between 2000Q1 and 2001Q1 is denoted by ΔHHI. The mean of the local HHIs is computed and its standard error is reported in parentheses. For this last case, ΔHHI is computed as the average of the HHI variation observed in each local market.

presents the distribution of HHIs across local markets. Local concentration often appears clearly higher than the threshold recommended by the EC, and this explains why the EC referred to the French CA for an assessment of the merger at the local market level.[14]

Another important feature of this merger is that a substantial rebranding process took place among insiders: several chains were renamed after the merger. Before the merger, $M1$ operated stores under eight chains: the hypermarket chain $M1_H$, a main supermarket chain $M1_S$ and other supermarkets, convenience stores, and hard discount chains that we bring together under a single notation $M1'$. $M2$ operated stores under seven chains: the hypermarket chain $M2_H$, a main supermarket chain $M2_S$, and $M2'$, which gathers all the remaining supermarkets and convenience stores chains.

As illustrated in Figure 1, hypermarkets $M2_H$ were rebranded into $M1_H$, while supermarkets $M1_S$ were rebranded into $M2_S$. Therefore, although $M1$ acquired $M2$, $M2_S$ supermarket chain remained active. This decision was motivated by a desire to keep hypermarket and supermarket chains with the highest brand image, as reported by press releases.[15] In addition, the two chains $M1_H$ and $M2_S$ had a rather higher price positioning than the other chains in the pre-merger period suggesting that the rebranding operations might have a significant impact on prices in the post-merger period (See Figure 1 in the Online Appendix).

Table 2 details the evolution of the rebranding operations. It shows that the merger was progressively implemented by the two groups. The first rebranding of a $M2_H$ into

[14]Note that, overall, concentration seems to have increased mostly in areas with the lowest initial concentration (the first quartile of the HHI distribution increased by 393), while the increase in the most concentrated areas is less pronounced (the third quartile increased by 187). These data gather the effects of all market changes and not only of the merger we focus on.

[15]See *Libre-Service-Actualités*, October 14th, 1999, and *Libre-Service-Actualités*, November 25th, 1999.

Figure 1: Rebranding Operations
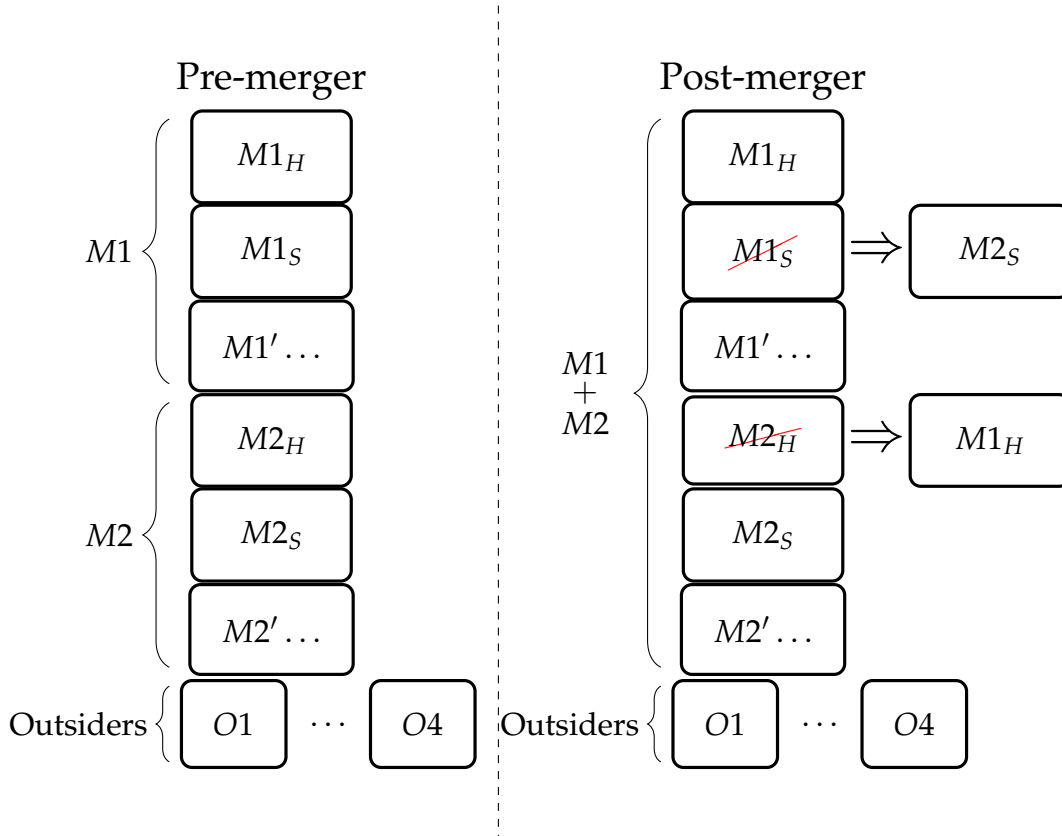
$M1_H$ took place on May 31, 2000 and by August 2000, all the hypermarkets had been rebranded into $M1_H$. The reorganization of the supermarkets took some more time (in August 2000, only half of the rebranding of supermarkets into $M2_S$ had taken place), while the reorganization of the logistics system started at the end of 2000.[16]

3   The data

This study uses a unique dataset that combines information from three sources. We first present our dataset in Section 3.1 before presenting our product and market definitions in Section 3.2 and Section 3.3.

3.1   Data Sources

The primary data are scanner data collected by the company *TNS Worldpanel* (Kantar Worldpanel, 1998-2001). This dataset records food purchases from a panel of house-

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[16] Note that the cost of rebranding a store is rather high, as it involves building work, changes in operation systems, and induced demand shocks. In 2000, $M1$ estimated the cost for rebranding a $M2_H$ into $M1_H$ as 75,000 to 150,000 Euros.

Table 2: A Time-Line Evolution of the $M1 - M2$ Merger

Number of stores	1998				1999			
	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
# of $M1_H$	116	116	132	132	132	132	132	132
# of $M1_S$	381	436	436	436	467	464	466	469
# of $M1'$	859	858	854	849	808	835	823	809
# of $M2_H$	77	78	83	84	84	84	85	85
# of $M2_S$	484	483	498	496	510	535	541	544
# of $M2'$	547	539	524	521	507	467	460	458
# of Outsiders	7104	7058	7045	7056	7070	7083	7090	7108
Total	9568	9568	9572	9574	9578	9600	9597	9605

Number of stores	2000				2001			
	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
# of $M1_H$	132	132	140	216	216	212	212	211
# of $M1_S$	471	475	351	144	1	0	0	0
# of $M1'$	797	797	794	798	797	799	789	790
# of $M2_H$	85	85	76	0	0	0	0	0
# of $M2_S$	547	543	669	877	1009	988	983	978
# of $M2'$	457	458	461	458	458	454	453	451
# of Outsiders	7123	7123	7122	7123	7139	7164	7177	7184
Total	9612	9613	9613	9616	9620	9617	9614	9614

Notes: The table presents the number of stores for each retail chain of the merging group and for all the outsiders, by quarter, during the pre- and post-merger period (1998-2001). $M1_H$ ($M2_H$), $M1_S$ ($M2_S$), and $M1'$ ($M2'$) denote the hypermarket chain, the main supermarket chain, and all the other store chains of the merging group $M1$ ($M2$, respectively). Computed from *Panorama Tradedimensions*; authors' calculation.

holds that are representative of the geographical and socio-economic group characteristics of the French population. The data contain detailed information on household characteristics, including the postcode of their home address, and all their purchasing activity during the year. Purchase data are collected by the households themselves by recording all their purchases with a home scanner. Information is reported at the level of the individual food product, and for most products these data are directly scanned from the barcode, making information available at the universal product code (UPC) level. Hence a product can be defined by up to 15 descriptive variables (such as flavor, container, and nutritional characteristics, for instance), plus the brand name and the name of the manufacturer. Otherwise, for fresh products without a barcode (often called random weight products), such as fruits, vegetables, meat or fish, information on product characteristics are reported manually into a diary.

In addition, households provide information about the shopping places where the purchases were made, by filling in the store type (e.g., hypermarket, supermarket, convenience store or specialized shop, for instance), the store size and, for retail chains, their name. For the purpose of this study, we consider the period that spans from 1998 to 2001 - which corresponds to nearly 32 million of food product purchases.[17] We complement these data with information on retail store characteristics over the same time period, obtained from the *Panorama Tradedimensions* dataset. This dataset lists

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[17] A more detailed presentation of the home-scan data is given in the Online Appendix.

grocery retail stores that operate in France and gives information on their attributes such as store size (in square meters), format, chain name or the postcode of the city where they operate, for instance. The dataset also reports information on changes in ownership, as well as opening, extension, or closing of stores. Lastly, we collect population and average household income information from census surveys, for the same time period, to proxy for determinants of demand faced by stores at the *commune* level (the French administrative unit similar to city).

Even though the *TNS Worldpanel* home-scan data provide one of the most detailed pictures of the French shopping habits for food products, the lack of information on the precise store where the product is purchased prevents us from directly matching the purchase data with the dataset on store characteristics. We recover the missing information by combining data on the household postcode, the name of the chain and the size of the store where the purchase was made in the following way: we construct an algorithm which (i) defines the set of all candidate stores of the relevant chain around the household residence, (ii) selects the one that matches the store size reported by the household, or if several stores have the same size, selects the closest one among them, and (iii), if no store meets these criteria, we increase the range around the reported surface by 200 square meters and re-run step (ii). Although it is frequent to observe a discrepancy between the surface reported by a household and the one recorded in the store characteristics dataset, the algorithm matches 70.78% of purchases when adopting a measurement error of the store size up to 400 square meters. Overall, 96.78% of purchase observations are matched with a store and we remove the remaining observations from the dataset. We thus obtain a store-product level dataset covering around 27 million purchases.[18]

3.2 Homogenous Product Definition

In the *TNS Worldpanel* database, products are described by a rich set of attributes. However the barcode of branded products is not reported, which complicates their tracking over time. To facilitate the comparison of product prices over time, we therefore create a unique identifying code for each combination of product characteristics using the whole set of attributes, except product capacity and package size.[19] As a result, our definition of a product is close to the universal product code (UPC) definition and

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[18]The Online Appendix gives more detailed information about the matching procedure and also reports a sensitivity analysis that shows that choosing the closest store when several stores are candidates does not alter the results.

[19]We make the choice to aggregate the data across product capacities and package sizes in order to get more observations in the final dataset. When products are delivered in multiple package sizes or capacities the computation of a mean price per unit of weight or volume inevitably introduces a measurement error. However, only a small number of products are subject to this bias in the final dataset.

eliminates aggregation bias which could result from an identifier constructed at the category or brand level, for instance (henceforth, and unless otherwise specified, we use the term UPC to refer both to barcoded products and random weight products). Consequently, the finer level of aggregation is the UPC. Then, UPCs can be clustered into more than 480 categories of food products which can themselves be aggregated into 63 families of products. For instance, in the family "Water", there is a category of product "Plain Water" in which we find the following UPC, "Mineral Water, Plastic bottle, Still, Evian". UPC prices are then reported in centimes of French Franc (1 centime ≈ 0.0015 €) per measurement unit (i.e., per Kg, per Liter or per unit) and are deflated. To give an example of the fine grained-level of our product definition, we track the price of Danone (Dannon) plain yogurt (skimmed cow milk) without bifidus conditioned inside a glass jar, or the price charged for bananas from Ivory Coast (as an example of random weight product).

We observe a large disparity in the frequency of purchases among product categories. For instance, "Plain Water" represents 2.60% of the recorded purchases whereas stock cubes amount to 0.001%. Within product categories, most of the UPCs correspond to a few observations. In fact, as for every home-scan panel data, we only observe a fraction of food sales in the population: the tracking of product prices with low sales at the store level is thus difficult. Consequently, we choose to aggregate the data over a period of six-months in order to account for a larger share of the food products bought in France. For each UPC, we then compute a mean unit price per half-year through the ratio of the French Franc sales to the quantity purchased. Since most of the UPC/store/half-year prices are computed with a few observations–the median number of observations per UPC/store/half-year is 2 and only 10% of them have more than 11 observations, we choose to exclude infrequently sold UPCs by requiring at least 3 purchase observations by store and by time period. Further, we remove from the raw data promotional prices (5.4% of the data) to limit the influence of price cuts in the computation of mean prices. These restrictions aim to limit measurement error into our final measure of price.

3.3 Local Market Definition

Assessing the price effect of the merger requires us to define the relevant market around each store. We base our definition of local competition on the *catchment area* of each store, i.e., the area from which most of the customers originate. Hence, the set of competitors for a store will be defined as the set of stores located inside this *catchment area*.

The French CA assumed in this particular merger case that, on average, consumers are willing to drive from 15 to 30 minutes to reach a hypermarket, while they drive 10

to 15 minutes to reach a supermarket or a discount store.[20] Furthermore, it is generally agreed that hypermarkets have a larger catchment area than supermarkets. In line with the position of the French CA, and converting driving time into kilometric distance, we define around each store a catchment area that spans up to 20 km and includes all the stores (hypermarkets, supermarkets, convenience stores, hard-discounters) within 10 km, and only hypermarkets between 10 and 20 km. More precisely, and given that we only observe the city (through the postcode) where a store operates, we adopt a city-centric definition to delineate the stores' catchment area by assuming that each store is located at the center of its city. A given store i is thus assumed to compete with all the stores contained in its catchment area, that is, all the hypermarkets located in the cities whose center is within a circle of 20 kilometer radius around the center of the city in which store i is located, and all the stores located in the cities whose center is within a circle of 10 kilometer radius around the center of store i's city.[21]

Figure 2 illustrates our city-centric definition of catchment areas for the case of stores located in Rennes (the larger city in Brittany region). The figure plots the borders of the city of Rennes as well as the borders of the surrounding cities. Several retail chains operate in Rennes ($M1_H$, $M1_S$, $M1'$, $O1_S$, $O1_H$, $O2_H$, $O4_S$, $O6_S$, $O7_S$) and are not reported to make the figure clearer. The area within 10 km from the center of Rennes is colored in orange; it includes all the cities whose center is less than 10 km away from the center of Rennes. Similarly, the area between 10 km and 20 km around the center of Rennes is colored in yellow. As observed, only one hypermarket ($O6_H$) is present in the yellow area. I n other words, the set of local competitors of the stores located in Rennes consists of $O6_H$ and all the other stores within the orange area.[22]

Note that our market definition assumes symmetric substitutability between formats: if larger stores (i.e., hypermarkets) are viewed by consumers as credible substitutes to other store formats, smaller formats also exert some competitive pressure (though less intensely) on larger stores. As shown by several studies (see, e.g., Cleeren et al., 2010; Haucap et al., 2013; Turolla, 2015; Maican and Orth, 2015), discounter stores, and to a lesser extent supermarkets, are serious rivals for hypermarkets. This approach contrasts with the definition used by the French CA who considers an asymmetric sub-

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[20]In other retail merger cases, such as Rewe/Billa and Rewe/Meinl decisions, the EC states that: "These local markets can be defined as a circle with a radius of approximately 20 minutes by car centered on the individual sales outlet".

[21]Since the distance traveled for a given driving-time varies according to the geographical features and urbanization, we test other definitions of local markets in the robustness section.

[22]Note that our city-centric approach could be equivalent to a store-centric definition if and only if stores were located in the barycenter of their city. When a store is located in a small city, our city-centric definition of catchment areas is closer to a store-centric definition. Moreover note that the largest cities such as Paris, Lyon, or Marseille are divided into districts each having a proper postcode. As we know postcodes of stores, we have then applied a district-centric approach. However, we agree that having precise information on stores' address and using a store-centric definition would be preferable for stores located on the outskirts of "middle-size" cities (such as Rennes).

Figure 2: An Example of Store's Catchment Area: The Case of Stores Located in Rennes
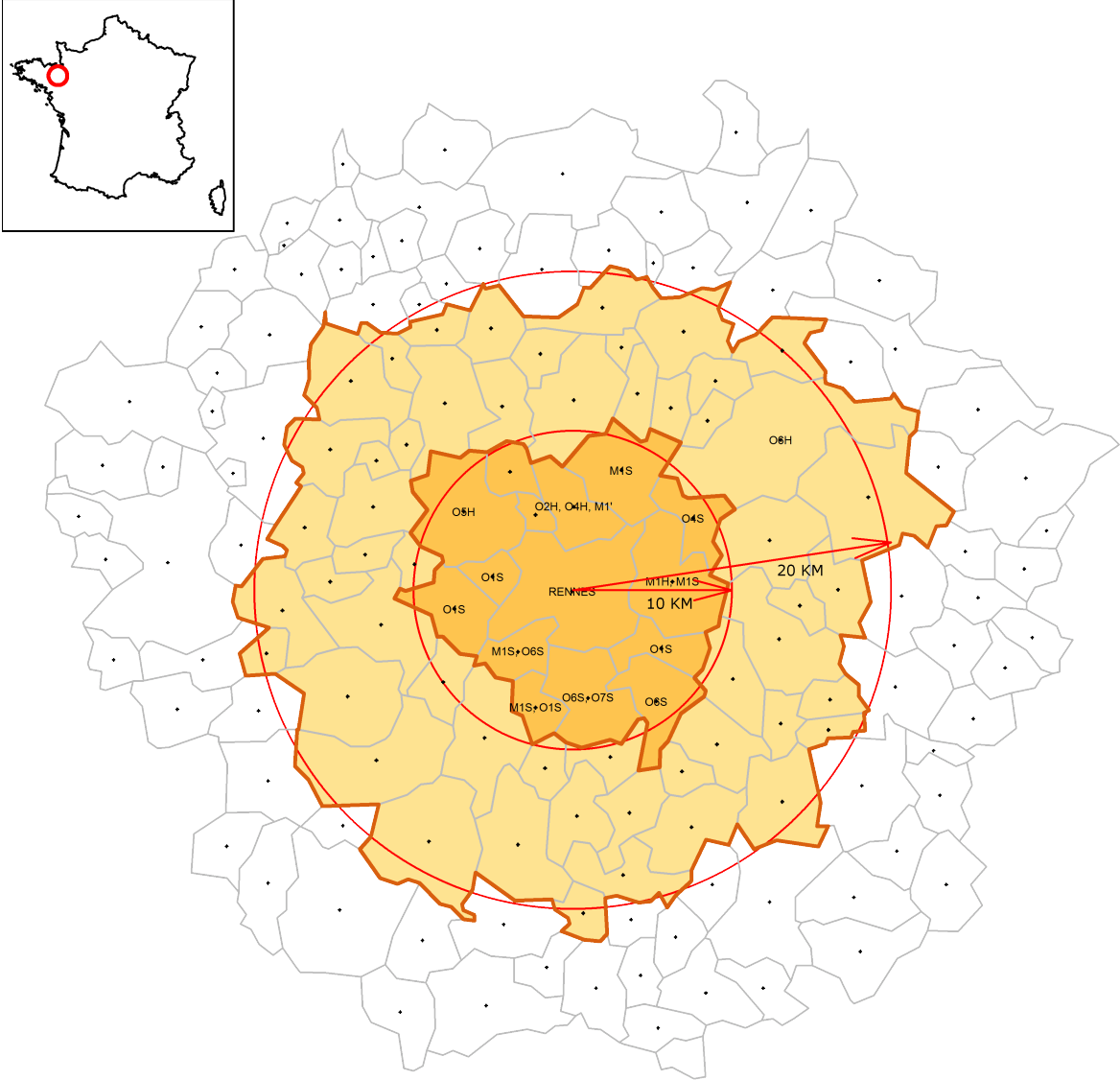
Notes: The figure depicts the delineation of the catchment area of stores located in the city of Rennes. The area is composed of all the cities within a radius of 30 km around Rennes. For each city, we draw the borders of the *commune* and we represent its center with a dot. For cities with a large grocery store, we detail the retail chain and the store format. To make the figure clearer, we do not report the retail chains that operate in Rennes. These retail chains are: $M1_H$, $M1_S$, $M1'$, $O1_S$, $O1_H$, $O2_H$, $O4_S$, $O6_S$, $O7_S$. Stores located in Rennes compete together as well as with all the stores located in cities within a 10 km radius around Rennes (colored in orange) and with all the hypermarkets located in cities up to 20 km (colored in yellow). Note that there is only one hypermarket ($O6_H$) between 10 km and 20 km.

stitutability between hypermarkets and other formats: hypermarkets and other stores are supposed to constitute separate markets, but hypermarkets located within 15 minutes driving-time around household residences might be considered as "local" substitutes for supermarkets and discounter stores. We refer the reader to the robustness section where we discuss in greater details the sensitivity of the results when adopting one or the other of these definitions.

4 A First Look at Retail Pricing Strategies: Local versus National Pricing

An important pre-requisite before turning to the evaluation of the merger effects is to understand how retailers set prices. Understanding the nature of the retailer's pricing strategies is key to capture the impact of a retail merger on prices. For instance in a country where retailers follow national pricing strategies (e.g., in the UK)[23], a merger is likely to have a different impact than in a country where retailers follow local pricing strategies. In the former case, all markets are likely to be affected by the uniform price increase of the merging firms and by the (national) reactions of their competitors. In the latter case, only the local markets where the two merging firms are active are likely to be affected (see Allain, Chambolle and Turolla, 2015, for a theoretical analysis of this issue). Furthermore, the existence of heterogeneous pricing strategies for competing retailers may further modify the impact of the merger on prices.

Numerous papers devoted to the analysis of the grocery retailing sector have highlighted that irrespective of global concentration ratios, on average, final prices are related to local competitive conditions (e.g., Asplund and Friberg, 2002; Barros, Brito and de Lucena, 2006). In recent years, the French CA expressed the view that retailers benefit from weak local competitive conditions and exert significant market power in local markets (see Competition Authority, 2007).[24] In particular, it has been well documented by consumers' associations and researchers that retailers distort their offers locally, mainly by adopting local pricing policies (Bertrand and Kramarz, 2002; Turolla, 2015).[25]

Using our store-product dataset, we present stylized facts on the pricing strategies implemented by both the insiders and outsiders during the pre- and post-merger

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[23]In 2004, the main retail chains in the UK, Tesco, Asda, Sainsbury's, and Morrisons, made a public commitment to uniform national pricing in the newspapers. For instance, Asda stated that "Asda pricing does not discriminate by geography, store size or level of affluence - we have one Asda price across the entire country". Dobson and Waterson (2005) provide a theoretical framework explaining why, under certain local market conditions, national retail chains are better off setting uniform prices.

[24]A 2012 report by the French CA even calls for the right to impose ex-post remedies on retail groups when they are too highly concentrated in some areas, such as Paris (see Competition Authority, 2012).

[25]Biscourp, Boutin and Vergé (2013) corroborate this view by highlighting a positive correlation between retail prices and the local HHI.

Table 3: Regression of Prices on Local Markets Concentration

Dependent variable: (log) of mean price (by half-year)						
	Pre-merger period					Post-merger period
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Store size (m^2/1000)	0.0002	-0.0000	-0.0001	-0.0002	-0.0002	0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
log(market income)		0.0350***	0.0265***	0.0262***	0.0262***	0.0236***
		(0.0019)	(0.0022)	(0.0022)	(0.0022)	(0.0024)
log(market population)			0.0014***	0.0014***	0.0014***	0.0015***
			(0.0002)	(0.0002)	(0.0002)	(0.0003)
HHI (/10000)	-0.0106***	0.0015	0.0098***	0.0102***		
	(0.0025)	(0.0025)	(0.0028)	(0.0028)		
HHI $\times$ M1					0.0070	
					(0.0051)	
HHI $\times$ M2					0.0134	
					(0.0096)	
HHI $\times$ Insider						0.0073
						(0.0049)
HHI $\times$ Outsider					0.0103***	0.0097***
					(0.0031)	(0.0030)
Constant	7.5351***	7.2021***	7.2643***	7.2668***	7.2667***	7.3352***
	(0.0033)	(0.0181)	(0.0198)	(0.0198)	(0.0198)	(0.0208)
Chain store FE	Yes	Yes	Yes	Yes	Yes	Yes
Half-year FE	Yes	Yes	Yes	No	No	No
Product FE	Yes	Yes	Yes	No	No	No
Product-Half-year FE	No	No	No	Yes	Yes	Yes
R^2	0.978	0.978	0.978	0.981	0.981	0.981
Observations	8909340	8909340	8909340	8909340	8909340	3456648

Notes: Data for the pre-merger period correspond to prices collected between January 1998 and June 2000, and between January 2001 and December 2001 for the post-merger period. Prices are expressed in centimes of French Francs (one centime equals 1/100 French franc) per measurement unit (i.e., liter, Kg or unit). Promotional prices are excluded from the computation of average prices. The *market income* variable corresponds to the mean household income calculated over the set of cities that belong to the catchment area of a given store. The *market population* variable is computed as the sum of inhabitants in 1999 living in cities that belong to the catchment area of a given store. The standard errors, shown in parentheses, are clustered by store. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

periods. In line with recent studies that have analyzed the correlation between local concentration and prices, we relate prices to variables controlling for the level of concentration in local markets. The purpose is to assess to what extent prices are set with regard to the level of local competition encountered.

As detailed previously, we define each store's catchment area according to our baseline definition (20/10 km distance bounds). Concentration in local markets is measured by the HHI computed based on selling surfaces at the retail group level. Note that each retail group is composed of several retail chains, each owning several stores. Controlling for unobserved components at the product and retail chain levels, we relate prices to local market conditions (e.g., income, population, or concentration level). The facts are presented in Table 3.

From Column (1) to (3), we gradually introduce distinct factors of local conditions: concentration (HHI), log of market income, and log of market population, while con-

trolling for store size as well as time, retail chain, and product fixed effects. In line with the aforementioned studies, the point estimate of the HHI variable testifies to a large average impact of local concentration on prices during the pre-merger period. In Column (4), we control for unobserved product-time specific factors that can affect prices without changing the correlation effects. We then investigate in the specification presented in Column (5) whether pricing strategies differ among retailers by interacting the HHI with insiders (decomposed between $M1$ and $M2$) and outsiders. We find that insiders do not adopt a local pricing strategy while outsiders do. Finally, we replicate in Column (6) the analysis conducted in Column (5), but after the merger. The results show that both insiders and outsiders do not change their pricing policy after the merger. In particular, the new merged entity still sets prices irrespective of the degree of local concentration.

These insights will strongly influence our empirical strategy and especially the method we use to define stores affected or not by the merger, that is, the way we define the treatment and control groups.

5   Empirical Strategy

Our goal is to estimate the price changes that result from the merger. A straightforward way to measure these price changes would consist in comparing the mean changes in prices, i.e., the average differences between pre- and post-merger prices, for stores impacted by the merger, to the potential mean changes that those stores would have experienced if they had not been affected by the merger. Since it is not possible to observe how prices would have changed "absent" the merger, we construct a counterfactual that reflects as closely as possible how stores would have reacted in the absence of the merger. As, on average, prices vary according to local competition (see, e.g., Table 3, Column (4)), we take advantage of the following quasi-experimental setting observed at the local level. Before the merger, $M1$ and $M2$ were not operating in all local markets (see Table 2 in the Online Appendix); thus the merger did not have a direct impact on local competition in all markets. We are therefore able to directly estimate the effect of the merger on food prices by comparing price changes in local markets affected by the merger (treated markets) to price changes in local markets unaffected by the merger (control markets).

To quantify the price change that results from the merger, we apply a difference-in-differences (DID hereafter) approach. The principle of a DID analysis is based upon the comparison of the average effect of a treatment (here the merger) on an outcome (here the prices), between two groups: the *treatment group* that includes subjects exposed to the treatment and the *control group*, that includes subjects unexposed to the treatment. Assuming that assignment to treatment is independent of prices charged

by retailers (also known as the *unconfoundness assumption*, see Rosenbaum and Rubin, 1983), the simple estimate of the average treatment effect is performed by computing an unconditional difference-in-differences in the prices where the key identification assumption is that, absent the merger, the prices would have evolved identically between the two groups. In Section 5.1, we define our treatment and control groups. We then select in Section 5.2 our final product sample, before presenting in Section 5.3 our main specification.

5.1 Treatment and Control Groups

The spatial dimension of retail competition makes it particularly difficult to draw the line between affected and unaffected markets. In what follows, we consider two different definitions of treatment and control groups.

- **Treatment/Control 1**: First, and in line with the literature on ex-post evaluation of mergers (e.g., Houde, 2012), we separate the local markets in which the merger affected the market structure, i.e. caused a change in the local concentration, from those in which it did not. A change in local concentration arises in local markets where, before the merger, at least one store of each of the merging firms (M1 and M2) were active. The treatment group is thus defined as all stores belonging to a local market in which at least one store among ($M1_H$, $M1_S$, and $M1'$) *and* one store among ($M2_H$, $M2_S$, and $M2'$) were active before the merger. Therefore, with this first definition, there are both insiders and outsiders in each of the treatment and control groups.

- **Treatment/Control 2**: Second, the treatment group is defined as all stores belonging to a local market in which (at least) one insider is active during the pre-merger period. The treatment group is defined as all stores belonging to a local market in which at least one store among ($M1_H$, $M1_S$, and $M1'$) *or* one store among ($M2_H$ $M2_S$,$M2'$) was active before the merger. Therefore, with this second definition, there are no insiders in the control group.

Treatment/Control 1 represents the usual definition of affected markets. Indeed, Competition Authorities generally consider that markets that are affected by a merger are those in which the HHI varies. However in the present case, as observed in Table 3, the merging firms had a national pricing strategy. In such a case, it is likely that merging firms internalize their competition externality at the national level and that prices uniformly increase at all insiders' stores. All local markets in which at least one store of the insiders is active can thus no longer be considered as unaffected by the merger. Treatment/Control 2 then integrates this effect.

Table 4: Summary Statistics on Treatment and Control Groups

	Treatment group			Control group		
	All stores	Insiders	Outsiders	All stores	Insiders	Outsiders
A. Treatment/Control 1						
Average Population (in 1999)	1228014	1408735	1148189	54519	41313	56518
Yearly average income per household	14788	14877	14749	12756	12418	12795
Average HHI	2130	2184	2106	3526	3905	3470
Number of stores observed	643	197	446	576	76	500
Average store size (in m^2)	4488	6158	3751	2544	2587	2539
B. Treatment/Control 2						
Average Population (in 1999)	729901	952710	647317	–	–	42839
Yearly average income per household	13965	14106	13913	–	–	12785
Average HHI	2564	2699	2514	–	–	4195
Number of stores observed	1102	298	804	–	–	164
Average store size (in m^2)	3846	5214	3339	–	–	2227

Notes: The table reports summary statistics on stores and local market characteristics for the treatment and control groups. We report in Panel A the statistics for Treatment/Control 1 and in Panel B the statistics for Treatment/Control 2.

Despite this clear definition of treatment and control groups, a store owned by an outsider may belong simultaneously to the catchment area of a store included in the treatment group and to the catchment area of a store included in the control group. To leave the control group uncontaminated by indirect effects of the merger, we exclude from it all stores whose catchment area includes such an outsider. Indeed, these stores are likely to be indirectly affected through their competitive interaction with the outsider that also belongs to the treated catchment area.[26]

Table 4 presents summary statistics on the treatment and control groups for both definitions. Note first that the insiders tend to be underrepresented in the control group in definition Treatment/Control 1, while they all are in the treatment group with definition Treatment/Control 2. This table shows that the two groups differ with respect to several dimensions. They differ in the number of stores and average store size: both definitions lead to put more stores, that are also larger, in the treatment group than in the control group, though the difference is smaller with definition Treatment/Control 1. The two groups also differ in their local market characteristics (revenue, population). With both definitions, on average, the stores in the control group are located in areas that are less populated and poorer than those in the treatment group. The HHI is also higher on average in the control group: a possible explanation is that density of stores is lower in these less populated areas.

Clearly, stores face different competitive and demand environments between the two groups, and we cannot assume that treatment assignment is orthogonal to all other factors that influence retailer prices. As expected, the unconfoundness assumption is hard to sustain in the context of a retail merger, because treatment assignment is not random: retailers may decide where to acquire stores according to markets character-

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[26]This happens for 68 stores under the Treatment/Control 1 definition and 22 stores under the Treatment/Control 2 definition: we exclude those stores from the control sample.

istics. In our setting, given that the merger is decided nationally, the merger treatment is assigned based on the pre-determined location of the merging firms. Therefore, a concern is that locations of retailers are endogenous and thus retailers that merge may be present in areas that are very different from the areas where the merging retailers are not located. For instance, retailers that offered low quality items are more likely to settle in isolated low-income markets, while other retailers may prefer to operate in more concentrated and wealthier markets. To account for this selection bias, it is usual to require unconfoundness "conditional" on a set of covariates that control for the observed disparities between the two groups.

5.2   Selection of the Product Sample

For the purpose of the merger analysis, we use the purchase data consisting of all UPCs (i) for which we can easily track their prices over time within stores, (ii) that are offered in all the main chain stores and throughout the territory, and (iii) that we can compare across stores affected or not by the merger. Recall also that we have excluded from the sample the UPCs for which there are not a sufficient number of purchase observations by store-half-year pair (at least 3).

Specifically, we impose that a store-UPC pair is observable for every period of six months. This implies that a new product launched after the merger is excluded from the sample, for instance.[27] We also impose that each selected product is sold in all the main retail chains and in every region defined by *TNS Worldpanel* (eight in total). This last condition excludes *de facto* from our analysis private labels that are by definition sold only by one chain. Finally, we only keep UPCs that are sold in both treatment and control groups' stores.

According to this selection procedure, we identify 206 UPCs for Treatment/ Control 1 and 183 UPCs for Treatment/Control 2. Over the 480 product categories present in the raw data, 76 are represented in the product sample for both definitions. All the major product families are included: fruits (6.1% and 6.1% of the observations in Treatment/Control 1 and Treatment/Control 2, respectively), vegetables (1.5% and 1.5%), meat (7.1% and 7.5%), dairy products (25.1% and 25.1%), dry grocery (22.4% and 22.0%), beverages (22.5% and 22.5%), to cite the most important. On average, a UPC is sold in 88 stores with Treatment/Control 1 (*versus* 96 with Treatment/Control 2) and a store offers 12 UPCs regardless of the definition. Overall, our selection of UPCs covers 18.2% (*versus* 17.2%) of household expenditures recorded in the *TNS Worldpanel*

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[27]Though analyzing the effect of the merger on product variety is outside the scope of this paper, we check how much turnover there is in the UPCs offered by the merging firms after the merger. In line with the theoretical literature on this topic (see, e.g., Inderst and Shaffer, 2007), we find that the merging firms shrunk by 10.6% the number of UPCs offered. This figure probably partly reflects the loss of $M2$ private labels after the merger whereas rival firms increased their assortment by 3.5%.

database.[28]

While on the one hand, by limiting the number of products there is a risk to measure only partially the effect of the merger, on the other hand, by comparing the price changes of identical products across stores, we avoid any sample composition effect that would create a bias in our estimates.

To sum up, the dataset used in this study is an unbalanced panel covering 206 (respectively 183) UPCs sold in 1219 (respectively 1266) stores over the period 1998 to 2001, for the definition Treatment/ Control 1 (respectively Treatment/ Control 2). The information is aggregated per six-months period. The unit of observation in our analysis is the mean price of a product, computed as a quantity-weighted price, sold in a given store during a six-months period.

5.3 DID Regression Estimates

We now check that there are no differences in pre-existing price trends for the treated and control groups in our product sample. Figure 3 presents, for both treatment definitions, the time patterns of average (log) prices for insiders and outsiders belonging to the treatment and control groups, where prices are computed as a weighted average over products. Comparing the evolution of prices in the two groups, we first observe no substantial difference in the price trends between the treatment and control groups in the pre-merger period, suggesting that the treatment and control stores share broadly similar price patterns in the pre-merger period. Looking at the post-merger period, it appears that the merger coincides with a larger price increase for the treatment group than for the control group. However, Figure 3 presents raw price trends, and does not control for any factors that could be correlated with prices. In the analysis that follows we control for such factors so as to isolate the effect of the merger "all else being equal".

We carry out inference by estimating how retail prices have evolved before and after the merger between the treatment and control groups, conditional on a set of covariates. We estimate the following regression with OLS using store-product level prices as the dependent variable:

$$\ln P_{ijt} = \alpha_1 + \alpha_2 PostMerger_t + \alpha_3 T_i + \beta_1 PostMerger_t \times T_i \times Outsider_i \qquad (1)$$

$$+ \beta_2 PostMerger_t \times T_i \times Insider_i + \delta' \mathbf{Z}_{it} + \mu_i + \sum_{n=1}^{N=j \times t} \lambda_n \tau_{jt} + \varepsilon_{ijt}$$

where P_{ijt} denotes the average price (in centimes of Franc) charged by the i-*th* store, for product j during the half-year t, $PostMerger_t$ is a dummy variable that identifies the

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[28]We provide in the Online Appendix the lists of the 206 and 183 UPCs, as well as descriptive statistics on the composition of the product samples.

Figure 3: Price Trends by Treatment and Control Groups

(a) Treatment/Control 1   (b) Treatment/Control 2
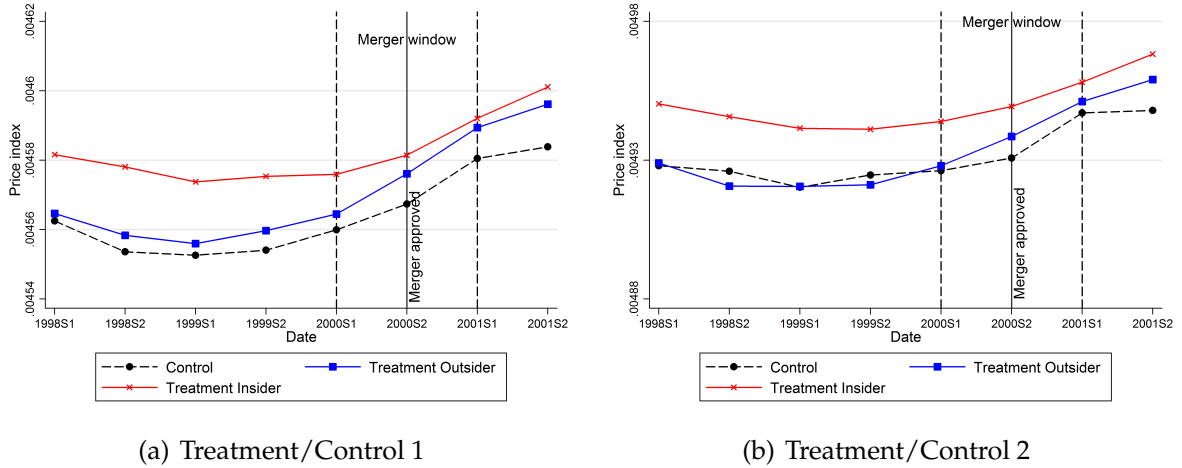
Notes: This figure provides a graphical illustration of the evolution of both insiders' and outsiders' prices in the treatment and control groups for Treatment/Control 1 and Treatment /Control 2 definitions. For each group, the price index is calculated as an average of the weighted mean prices of the UPCs, where a weight corresponds to the share of the UPC in total expenditure (before any product selection).

post-merger period, and T_i is a dummy variable that characterizes store *i* as belonging to the treatment group. Finally, $Outsider_i$ ($Insider_i$, resp.) takes the value of one if store *i* is an outsider (insider). Consequently, the average effect of the merger is captured through the coefficients β_1 and β_2, which can be interpreted as the causal effect of the merger on prices, respectively at the outsiders' stores and at the insiders' stores.

The regression also includes a set $\mathbf{X}_{ijt} = \{\mathbf{Z}_{it}, \mu_i, \tau_{jt}\}$ of observable covariates by store, product, and time.[29] The idea is that store fixed effects μ and product-half-year fixed effects τ control for, respectively, store factors that remain constant and affect price, and product-half-year factors that vary and affect price. The product-half-year fixed effects control for factors that could have changed every six-months for each product separately. The factors that could have changed could be, for example, advertising at the national level for a given product that coincided with the post-merger periods or changes that would be common to all products within a category at a given six-months period, for example, if the number of manufacturers for a given product category drops in a post-merger period at the national level (e.g., milk producers). All these factors are uncorrelated, that is, exogenous, to the merger - the treatment. Further, $\mathbf{Z}_{it}$ are time-variant catchment area attributes of stores (e.g., local market income) that control for time varying market specific effects (e.g., local demand shocks). Despite the introduction of these market level factors, it is worth noting that unobserved shocks are still assumed to affect the outcome identically in both groups.

As we have seen in Table 2, the rebranding of stores took place gradually during the second half of 2000. This leads us to drop the data for the second half of 2000 in order to

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[29]Accordingly, the interaction terms $T \times Outsider$ and $T \times Insider$, as well as the dummy variables *PostMerger*, *T*, *Outsider*, and *Insider* are not included in the regression due to correlation issue.

avoid issues related to transitory shocks generated by the rebranding of stores. We also choose to remove data from the first half-year of 2000 to leave data uncontaminated by a potential anticipation of the merger by the parties.

6 Results

We first present in Section 6.1 a simple before and after analysis of the merger. Then, we outline in Section 6.2 our estimation of eq. (1) using the first definition of treatment and control groups (Treatment/Control 1). According to this definition, the merger may impact prices only through the change in local concentration it triggers. We then derive our results using Treatment/Control 2 in Section 6.3. With this second definition, we analyze the merger effect on prices taking into account that all insiders must have raised their prices after the merger, due to their national pricing strategy. Finally, Section 6.4 complements the analysis by exploring the impact of the merger on household expenditures.

6.1 A Before and After Analysis

Table 5 displays our baseline estimates. In all columns the dependent variable is the log of prices, and we control for market income effects as well as for store and product-half-year fixed effects. Each price is weighted by the share of each product in total expenditures in all stores, where the weights are computed using the pre-merger original dataset only. Standard errors are clustered at the store level to control for correlation across unobserved product-specific factors that co-vary inside a store.[30]

The first two columns of Table 5 display a simple before and after comparison of prices, i.e., a time difference estimate. We find that prices have increased after the merger both for the insiders and for the outsiders. Column (1) shows that prices have increased by 4.76% on average at insiders' stores, while they have increased by 7.37% on average at outsiders' stores. These results corroborate the theory which states that, absent efficiency gains, the primary effect of a merger should be correlated with a price increase. Breaking up the before and after comparison by the type of store, Column (2) points out that the price increase at the insiders is more striking for the hypermarket chains $M1_H$ and $M2_H$ (around 5% each). As for the supermarkets, prices have increased substantially at $M2_S$ but not at $M1_S$. Given the heterogeneous price positioning of the chains in the pre-merger period (see Figure 1 in the Online Appendix), this tends to confirm a catching-up effect for the hypermarket chain $M2_H$.

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[30]It is worth noting that the number of clusters defined at the store level (1219 clusters) is large enough to correct any potential serial correlation issues in the computation of the DID estimates (see Bertrand, Duflo and Mullainathan, 2004, for a discussion).

Table 5: Before and After Price Comparisons and DID Estimates

Dependent variable: (log) price (P_{ijt})						
Variable	Before and After		DID			
	All stores	All stores	All stores	All stores	Outsiders only	Insiders only
	(1)	(2)	(3)	(4)	(5)	(6)
PostMerger × Outsider	0.0737***	0.0731***				
	(0.0048)	(0.0049)				
PostMerger × Insider	0.0476***					
	(0.0056)					
PostMerger × $M1_H$		0.0495***				
		(0.0069)				
PostMerger × $M1_S$		0.0164				
		(0.0160)				
PostMerger × $M1'$		0.0750				
		(0.0497)				
PostMerger × $M2_H$		0.0563***				
		(0.0076)				
PostMerger × $M2_S$		0.0321**				
		(0.0125)				
PostMerger × $M2'$		0.0677**				
		(0.0295)				
PostMerger × T			0.0114**			
			(0.0051)			
PostMerger × T × Outsider				0.0236***	0.0223***	
				(0.0056)	(0.0059)	
PostMerger × T × Insider				-0.0077		-0.0014
				(0.0059)		(0.0099)
log(market income)	-0.0925	-0.0826	-0.0302	-0.0350	-0.0086	-0.0837
	(0.0572)	(0.0593)	(0.0657)	(0.0647)	(0.0798)	(0.1265)
Constant	10.0331***	9.9384***	7.5008***	7.5468***	7.4074***	7.5878***
	(0.5461)	(0.5660)	(0.6281)	(0.6184)	(0.7630)	(1.2460)
Store FE	Yes	Yes	Yes	Yes	Yes	Yes
Product FE	Yes	Yes	–	–	–	–
Product-time FE	–	–	Yes	Yes	Yes	Yes
R^2	0.988	0.988	0.989	0.989	0.989	0.991
Observations	33714	33714	33714	33714	23994	9720

Notes: Stores catchment areas are delimited using the baseline definition (20/10 km), and treatment and control groups are defined according to Treatment/Control 1. The observations are weighted by the expenditure shares of food products calculated at the national level. Data for the year 2000 are removed (i.e., event windows). The standard errors, shown in parentheses, are clustered by store. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

6.2 The Merger Effect due to a Change in Local Concentration

We now estimate eq. (1) using the standard definition of treatment and control groups, that is, Treatment/Control 1. Columns (3) to (6) of Table 5 present the difference-in-differences estimates with this treatment definition.

Columns (3) to (6) present the DID analysis that enables us to estimate the causal effect of the merger: on all firms in Column (3), while Columns (4) to (6) split the effect between outsiders and insiders. The average causal effect of the merger is statistically significant and about 1.14% (see Column (3)). However, Column (4) shows that the only statistically significant causal effect comes from the outsiders: the merger has caused a 2.36% significant increase for outsiders, whereas there is no increase for

insiders. In Column (5), the sample is reduced to outsiders only. This enables us to better isolate the effect of the merger on outsiders (compared to Column (4)), as the control group now consists only of outsiders. As switching from the specification of Column (4) to that of Column (5) leads to remove the insiders that were in the control group, and as these insiders tend to increase prices less, this contributes to explain why the coefficient is lower in Column (5). In Column (6), the sample is reduced to insiders only and no statistically significant effect arises.

Though Column (1) of Table 5 shows that the prices have increased after the merger both at the outsiders and at the insiders' stores, the DID estimates highlight a striking difference between the causal effect of the merger on prices at insiders and outsiders. In theory, if all firms had a local pricing strategy, the DID estimates should highlight a causal effect of the merger on insiders prices as well as on outsiders'. Indeed, the merging firms $M1$ and $M2$ internalize the competition effect in all treated markets, and therefore increase their prices. In reaction, their competitors also increase their prices. However, we do not observe the causal effect on insiders' prices: this is consistent with the observation made in Table 3 that insiders do not adapt their prices locally, whereas outsiders tend to have local pricing strategies. That the merged entity has a national pricing strategy explains well the absence of significant causal effect of the merger at insiders. Following the acquisition of $M2$ by $M1$, the new entity seems to have internalized the competition externality at the national level. As a consequence, local markets in the control group in which only one insider was active must have been affected by the merger. In contrast, the causal effect of the merger on outsiders is consistent with the hypothesis that outsiders change their prices differently in all local markets. Again Table 3 shows that both pre-merger and post-merger, outsiders prices are correlated with local concentration.

6.3 The Merger Effect when Insiders Raised their Prices Nationally

In the Treatment/Control 1 definition, some local markets of the control group include insiders. Yet, as we have shown that insiders increased their prices at the national level, these local markets can no longer be considered as unaffected by the merger. In what follows, we thus adopt the Treatment/Control 2 definition: the treatment group is now defined as outsiders' stores belonging to a local market where (at least) one insider is active during the pre-merger period. As there are no insiders in the control group, we cannot explore the causal effect of the merger on insiders with this second definition. However, this new definition will enable us to better analyze the local reaction of outsiders to the merger. To this end, we estimate the following equation while removing

Table 6: Local Effects on Outsiders

Dependent variable: (log) price (P_{ijt})		
Variable	DID	DID
	(1)	(2)
PostMerger $\times$ T	0.0181**	
	(0.0076)	
PostMerger $\times$ # of Insiders		0.0004*
		(0.0002)
log(market income)	-0.0216	-0.0445
	(0.0769)	(0.0796)
Constant	7.5523***	7.7770***
	(0.7348)	(0.7609)
Store FE	Yes	Yes
Product-time FE	Yes	Yes
R^2	0.989	0.989
Observations	25164	25164

Notes: Insiders are removed from the sample. Stores catchment areas are delimited using the baseline definition (20/10 km), and treatment and control groups are defined according to Treatment/Control 2. The observations are weighted by the expenditure shares of food products calculated at the national level. Data for the year 2000 are removed (i.e., event windows). The standard errors, shown in parentheses, are clustered by store. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

insiders from the sample:

$$\ln P_{ijt} \;=\; \alpha_1 + \alpha_2 PostMerger_t + \alpha_3 T_i + \beta \left(PostMerger_t \times T_i \right) \tag{2}$$
$$+\delta' \mathbf{Z}_{it} + \mu_i + \sum_{n=1}^{N=j \times t} \lambda_n \tau_{jt} + \varepsilon_{ijt}$$

Table 6 presents the DID estimates with this new definition. Column (1) shows that the merger caused a significant increase in outsiders' prices by 1.81%. It is interesting to compare this figure with the 2.23% price increase obtained in Column (5) of Table 5. In each of these two columns, there are only outsiders in the control group but the sample of stores differs. In column (5) of Table 5, some outsiders in the control group compete with stores belonging to one of the merging firms. Such outsiders now switch to the treatment group when considering Column (1) of Table 6. The outsiders that have switched are likely to be those who compete on average with a smaller number of insiders: This may tend to decrease the average price change in the treatment group more than it reduces the average price change in the control group, which contributes to explain why the point estimate obtained is lower with the Treatment/Control 2 definition than with the Treatment/Control 1 definition.

Heterogeneous Treatment Effect To further investigate the mechanism behind this result, we explore the effect of the number of insiders competing in a local market on the outsider's price increase. In theory, if insiders have a national pricing strategy

while outsiders have a local pricing strategy, then after the merger, the reaction function of each outsider is unchanged, so that outsiders modify their prices only to react to the insiders' price increase. After the merger, they should increase their prices more in areas where (for a given number of competitors), they compete with a larger number of insiders. Column (2) of Table 6 then corroborates this insight and shows a positive and significant effect of the number of insiders on outsider's price increase caused by the merger. Note that these results are also in line with those obtained in Section 6.2 as there is a direct correlation between the number of insiders in a market and the change in local concentration a market experienced after the merger.

Differentiation Effect There are other potential sources behind the local price reaction of outsiders due to this particular merger. Recall that, with the merger, two of the chains have changed their names: $M2_H$ was rebranded into $M1_H$ and $M1_S$ into $M2_S$. Therefore, at the national level, two chain names have disappeared. In local markets, this is not always the case since it depends on the geographical distribution of the stores in the pre-merger period. In the post-merger period we can have one of the three situations: a drop in two chain names, labelled as "$\Delta N = -2$"; a drop in only one chain name, labelled as "$\Delta N = -1$"; or, finally, no drop at all, labelled "$\Delta N = 0$". The last situation covers two cases: either no chain at all changed name in the local market (no rebranding), or there is rebranding as a chain name has been suppressed and replaced by another which did not exist before in this market. In this last situation, referred to as "pure rebranding" hereafter, the net change in names is zero.

The reduction in the variety of stores available to consumers may simply impact competition. Indeed, it results in an increase in the distance to the preferred variety of each consumer, and therefore relaxes competition between stores. In theory, this differentiation effect is well illustrated in a Salop (1979) competition framework, where retail chains are located around a circle and consumers are uniformly located along the circle and incur transportation costs related to their distance to reach a store. In this model, the distance between stores is related to the differentiation among chains. When two neighboring retailers merge, a drop in the number of chains could be modeled as a relocation of two previous stores into the same unique location. By relocating symmetrically around the circle, all firms would then obtain a higher market share because their two nearest neighbors are more distant. In equilibrium, the merger would then result in a price increase for all stores (e.g., Levy and Reitzes, 1992).

Table 7 presents the impact of a drop in N on treated outsiders' prices. We show in Column (1) that in all areas where the number of chain names dropped by 2 ($\Delta N = -2$), the outsider's price increase caused by the merger is statistically significant and about 3.22%, which is higher than the average causal effect on outsiders' prices.

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Table 7: Differentiation and Rebranding Effects on Outsiders

Dependent variable: (log) price (P_{ijt})

Variable	DID (1)	DID (2)
PostMerger × T × $\Delta N = -2$	0.0322***	0.0322***
	(0.0094)	(0.0094)
PostMerger × T × $\Delta N = -1$	0.0120	0.0120
	(0.0098)	(0.0098)
PostMerger × T × $\Delta N = 0$	0.0158*	
	(0.0081)	
PostMerger × T × $\Delta N = 0$ × Rebranding		0.0171*
		(0.0091)
PostMerger × T × $\Delta N = 0$ × No Rebranding		0.0148
		(0.0091)
log(market income)	-0.0260	-0.0237
	(0.0771)	(0.0771)
Constant	7.5956***	7.5735***
	(0.7367)	(0.7367)
Store FE	Yes	Yes
Product-time FE	Yes	Yes
R^2	0.989	0.989
Observations	25164	25164

Notes: Insiders are removed from the sample. Stores catchment areas are delimited using the baseline definition (20/10 km), and treatment and control groups are defined according to Treatment/Control 2. The observations are weighted by the expenditure shares of food products calculated at the national level. Data for the year 2000 are removed (i.e., event windows). The standard errors, shown in parentheses, are clustered by store. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

Rebranding Effect A "pure rebranding" may have consequences in itself for both insiders and outsiders. Indeed, the rebranding of stores may negatively affect the local demand of the merging firms. By adopting the chain name of a previous competitor, a risk exists of disrupting the established connection between consumers and stores of the removed chain. For instance, inconveniences due to revamping stores (e.g., store layout) or the replacement of private labels by another brand may induce consumers to visit rival stores. It may thus affect outsiders who face a rebranded store in their catchment area. These outsiders may indeed gain new customers disappointed by the changes, or lose some customers wishing to change. We can interpret resulting changes in prices at outsiders when there is no drop in the number of names and rebranding as due to the pure rebranding effect. Column (2) of Table 7 shows a positive (though barely statistically significant) effect of the pure rebranding on outsiders' prices.

6.4   The Merger Effect on Household Expenditures

To complement the analysis on the price effect of the merger, we attempt now to measure to what extent the merger has changed household expenditures. Basically, if the merger has raised the prices at outsiders' stores that compete with one of the merging

firm (i.e., the treated outsiders in Treatment/Control 2), one would expect to observe a rise in the expenditures of households living in affected markets, compared to households living in unaffected markets, *ceteris paribus*. Obviously, this corollary relies on the assumption that households still visit the same stores after the merger and buy the same quantities (i.e., that demand is inelastic).

To answer this question, we use all the household purchases recorded in the *TNS Worldpanel* database. Compared to the price analysis run previously, we are no longer constrained here to perform price comparisons on a sample of identical UPCs since we are now working on a higher level of aggregation (the total expenditure of a household), which extends the scope of our analysis. Furthermore, it is no longer necessary to determine where the purchases were made, which means that this approach depends less on the allocation of the households to particular stores (i.e., our matching procedure between purchase data and stores).

We first delineate the set of stores that a household can visit assuming that the household is living at the barycenter of its city, and then applying the baseline definition of a stores' catchment area (i.e., 20/10 km). We then define a treated household according to the treatment status of the stores this household can visit, following the Treatment/Control 2 definition. Specifically, a household is defined as treated when at least one of the stores he can visit is an insider, or, if he can visit only outsiders' stores, when at least one of these outsiders' stores is treated. As previously, we aggregate the household expenditures by six-months period. To limit measurement error in household expenditures, we exclude from the sample the households that do not shop every month, as well as those who change their place of residence between 1998 and 2001. Overall, we deal with 2783 households living in 1554 cities. The treatment group is composed of 2244 households, while 539 households are in the control group. The average household expenditures during a six-months period is 803667 centimes of French Francs, with a standard deviation of 415453.

To formally quantify the merger effect on household expenditures, we estimate the following regression using the log of household expenditures as the dependent variable:

$$\ln EXP_{hmt} = \alpha_1 + \alpha_2 PostMerger_t + \alpha_3 T_h + \beta \left(PostMerger_t \times T_h \right) + \phi' \mathbf{H}_{ht} \quad (3)$$
$$+ \delta' \mathbf{M}_{mt} + \mu_h + \tau_t + \varepsilon_{hmt}$$

where EXP_{hmt} denotes the average expenditure (in centimes of Franc) of household h living in the catchment area m during the half-year t, $PostMerger_t$ is a dummy variable that identifies the post-merger period, and T_h is a dummy variable that characterizes the treated households. The average causal effect of the merger on household expenditures is captured through the coefficient β. The regression also includes a set $\mathbf{H}_{ht}$ of

Table 8: Merger Effect on Household Expenditures

Dependent variable: (log) of household expenditures (by half-year)				
Variable	(1)	(2)	(3)	(4)
			Primary shopping destination	
	Household expenditures	Household expenditures in outsiders stores	Household expenditures	Household expenditures if outsider store
PostMerger × T	-0.0022	-0.0189	0.0026	0.0102
	(0.0142)	(0.0235)	(0.0197)	(0.0205)
# of stores visited			-0.0165***	-0.0153***
			(0.0044)	(0.0047)
Constant	12.3906***	11.2515***	12.0038***	11.8693***
	(0.2399)	(0.3896)	(0.3212)	(0.3544)
Household characteristics FE	Yes	Yes	Yes	Yes
Market characteristics FE	Yes	Yes	Yes	Yes
Half-year FE	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes
Chain store FE	Yes	Yes	Yes	Yes
Treated households	2244	2065	1829	1383
Control households	539	528	494	472
R^2	0.900	0.867	0.886	0.884
Observations	16698	15558	13938	11130

Notes: Data for the year 2000 are removed (i.e., event windows). Treatment and control groups are defined according to Treatment/Control 2. The standard errors, shown in parentheses, are clustered by city. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

time-varying household characteristics (i.e., the size-adjusted household income, the number of persons per household, the age of the youngest infant less than three years old, the number of infants less than three years old, the age of the youngest child, the presence of a cat and a dog, and a set of dummy variables for the chain stores visited) and time-varying catchment area characteristics $\mathbf{M}_{mt}$ (i.e., a set of dummy variables for outsiders chains), as well as household and time fixed-effects (μ and τ).

The results of the estimation are presented in Column (1) of Table 8. We obtain a non-statistically significant point estimate for β, which shows that the merger has not modified the household expenditures. However, as we have seen in Section 6.2, since the merging firms have raised their prices nationally, we are not able to identify a *local* effect of the merger on households whose expenditures mainly consist in purchases made in insiders' stores: the *local* effect of the merger only affects outsiders' stores. What we can measure locally is whether the change in the expenditures made in outsiders' stores is different for the households living in a treated area and for those living in a control area. As the merger caused a price increase at the treated outsider' stores, we could expect to observe a larger increase in the expenditures made at outsiders' stores by the households of the treatment group than by those of the control group. We now report in Column (2) the point estimates when household expenditures only include purchases made at outsiders' stores. The parameter is estimated with more precision but it is still statistically non-significant. To push the analysis a little fur-

ther, we consider only the expenditures made in the primary shopping destination. In order to define the store in which a household spends the most (i.e., its primary shopping destination), we use our matching between purchases and stores data. Using the aggregate household expenditures made in their primary shopping destination as dependent variable, we replicate in Columns (3)-(4) the estimates conducted in Columns (1)-(2). Even if the point estimates have now the expected sign, we do not find a statistically significant effect of the merger on household expenditures made at the primary shopping destinations.[31]

Altogether, our findings suggest that households exposed to the outsiders' price increase have reacted by allocating differently across stores the quantities they purchase. As emphasized before, we expected the expenditures of treated households to increase if they did not modify the quantities they purchased. A plausible explanation for our difficulties in identifying any merger effect on household expenditures could be that households have reacted to the merger by allocating differently their budget between insiders and outsiders, or within outsiders' stores (e.g., visit a different store format), or even between the food retail sector and the outside option (e.g., specialist stores, farmer markets).

7 Robustness Checks

Our results demonstrate that, regardless of the definition of the treatment and control groups, the merger caused a price increase at outsiders' stores. We now assess the robustness of our findings with respect to several central hypotheses used in the baseline specification: namely, the definition of stores' catchment area, the hypothesis that the treatment and control groups have similar characteristics, and the absence of an anticipation of the merger beyond the merger window we define. Finally, we investigate whether the price effect of the merger varies across product types. All along this section, we keep the definition Treatment/Control 2 and we focus on the merger effect on outsiders.

Robustness to the Definition of Catchment Areas   Panel A of Table 9 repeats the estimate of eq. (2) for four additional definitions of a catchment area, resulting in five columns. In the first column (labelled 30/15 km), we consider larger catchment areas, and delimit local markets around city centers where stores are located using a 30 km (15 km) radius for hypermarkets (all stores), respectively. The second column (labelled 20/10 km) corresponds to the baseline definition (20/10 km) and the results are reported for ease of comparison. In Column (3), we adopt a tighter definition of the

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[31]Note that the results change marginally when we consider only the selected sample of products.

catchment areas by using a 10/5 km radius: this may be more appropriate for densely populated areas where traffic congestion significantly reduces the distances that people can travel. In Column (4), we adopt a more flexible definition by using the baseline 20/10 km definition, except for stores located in the most populated areas, where we adopt the 10/5 km definition.[32]

Finally, in the last column we use the market definition used by the French CA. In contrast with our approach, the French CA keeps the same distance bounds around a store for hypermarkets as for other stores, but considers an asymmetric substitutability between hypermarkets and other formats. Specifically, the French CA assumes that the catchment area of a hypermarket is composed uniquely of hypermarkets located within 20 km. In other words, no other store formats can exert a competitive pressure on hypermarkets. By contrast, the French CA assumes that hypermarkets are valid substitutes for other formats, and we define the catchment area of all other stores (i.e., supermarkets, discount stores, convenience stores) as all the stores located within 10 km, including hypermarkets.

Except for the case of large markets (30/15 km), the results appear robust to alternative market definitions. When we use a narrower definition of local markets (10/5 km and 20/10/5 km), the size of the control group increases mechanically, since fewer stores are affected by the merger, and yet the results appear very similar to those of the baseline definition. This shows that our results are not driven by outsiders' stores located far away from insiders. In the case of large markets (30/15 km), the absence of a merger effect on outsiders is not surprising. Indeed, with this wide definition of the catchment areas, the treatment group includes outsiders' stores located far away from insiders: those stores are presumably less (or not) affected by the merger. Mechanically, we obtain a control group composed of few stores (only 66 stores for 1152 treated stores), located in remote areas with market characteristics that differ significantly from those of the treatment group. The lower price responsiveness of the treated stores, combined with a poor definition of the comparison group, explain why we cannot measure any price effect of the merger. Finally, note that when we adopt the market definition used by the French CA, we obtain results that are very similar to those obtained with the baseline definition. Whether one considers a symmetric or an asymmetric substitutability between hypermarkets and other formats does not substantially change the result.

An Alternative Estimator of the Average Treatment Effect One of the key identifying assumption of the DID approach is that the treatment and control groups must

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[32]The most populated areas are defined at the *département* (French administrative unit) level and correspond to stores located in one of the following *département*: Bouches-du-Rhône (13), Rhône (69), Paris (75), Seine-et-Marne (77), Yvelines (78), Essonne (91), Hauts-de-Seine (92), Seine-Saint-Denis (93), Val-de-Marne (94), and Val-d'Oise (95).

Table 9: Alternative Definitions of Catchment Areas

Dependent variable: (log) price (by product, by store, by half-year)					
					French CA
Variable	30/15 km	20/10 km	10/5 km	20/10/5 km	20/10 km
Panel A: DID estimates					
PostMerger × T	0.0024	0.0181**	0.0189***	0.0190***	0.0187***
	(0.0099)	(0.0076)	(0.0061)	(0.0071)	(0.0068)
Stores FE	Yes	Yes	Yes	Yes	Yes
Product-Period FE	Yes	Yes	Yes	Yes	Yes
R^2	0.989	0.989	0.989	0.989	0.989
Observations	21450	25164	26196	25596	18768
Panel B: DID-Matching estimates					
PostMerger × T	0.0398***	0.0273***	0.0283***	0.0272***	0.0239**
	(0.0133)	(0.0106)	(0.0087)	(0.0092)	(0.0107)
Stores FE	Yes	Yes	Yes	Yes	Yes
Product-Period FE	Yes	Yes	Yes	Yes	Yes
R^2	0.990	0.990	0.991	0.991	0.990
Observations	21450	25164	26196	25596	18768

Notes: Insiders are removed from the sample. Treatment and control groups are defined according to Treatment/Control 2. Column labelled *30/15 km* reports the point estimates of the merger effect on outsiders with catchment areas including all the hypermarkets within a 30 km distance bound around a store and all the other stores within a distance bound of 15 km. Column labelled *20/10 km* reports the results of the baseline definition previously available in Column (1) of Table 6. Column labelled *10/5 km* reports the results with the 10/5 km boundaries. Column labelled *20/10/5 km* reports the results with the 20/10/5 km boundaries. Column labelled *French CA 20/10 km* reports the results when adopting the asymmetric definition of the French CA and the 20/10 km boundaries. The results show in Panel A are obtained using the DID estimator, whereas a DID-matching estimator is used in Panel B. The observations are weighted by the expenditure shares of food products calculated at the national level. Data for the year 2000 are removed (i.e., event windows). The standard errors, shown in parentheses, are clustered by store. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

share similar pre-merger characteristics: For instance that, absent the merger, the average prices for the treated and control groups would have followed parallel paths over time. If there is only limited overlap in the distributions of the confounding factors across the treatment and control groups, missing outcomes will be incorrectly imputed. Estimates of average treatment effects can also be biased if control observations are not appropriately re-weighted to control for differences in the distribution of the set of variables over regions common to the control and treatment groups. This problem is particularly highlighted when using a large market definition (30/15 km) in Table 9 because the characteristics of the control stores differ substantially from those of the treated stores.

To assess the robustness of the results to this particular concern, we perform alternative comparisons for the stores affected by the merger through a semi-parametric matching estimator. More specifically, we use a propensity score matching estimator. As a first step, we estimate a probit of the merger occurring in a local market where we include, as explanatory variables, store characteristics (such as store size), baseline factors that affect price trends (such as baseline concentration and competitors operating in the market), baseline factors that affect demand (such as the market population

and the average income in the local area), and regional dummies. We then estimate the probability that a store is affected by the merger as a function of these variables.[33] In a second step, we apply a re-weighting scheme, as proposed by Hirano, Imbens and Ridder (2003) and Imbens (2004), to control for differences in observed confounding factors between treated and control stores. The basic idea is to use the fitted values of the probability of treatment from the probit analysis (the propensity scores) to re-weight the regression sample, thereby effectively creating a smooth version of a match on propensity score. Let the propensity score S be the probability that a market in the data is impacted by the merger as a function of baseline characteristics. We re-weight observations in the non-affected sample by $S/(1-S)$. This balances the distribution of baseline characteristics across the treated and non-treated stores. Intuitively, this technique up-weights data from stores that were not treated, but had a high probability of having been affected by the merger based on observable data.

The DID-matching estimates are performed for each definition of the catchment areas and are reported in Panel B of Table 9. Overall, the point estimate of the merger effect appears substantially higher and remains highly statistically significant. The higher point estimate suggests that stores unaffected by the merger but whose characteristics are closer to those of the treated stores have moderately increased their prices. With our baseline definition of catchment areas (20/10 km), outsiders have reacted to the merger by increasing their prices by 2.73 % compared to non-affected stores. When using a narrower definition of catchment areas (10/5 km and 20/10/5 km), we obtain lower point estimates: this simply reflects the fact that the control group is now enlarged as it includes new outsiders' stores with a high probability to be affected by the merger, and that those stores have raised their prices substantially after the merger. Interestingly, with the largest market definition (30/10 km), we obtain now a relatively high point estimate, that is statistically significant; however this result relies on a small control group in which a few stores have a high probability to be affected by the merger.

Taken together, these results stress the importance of controlling for unbalanced covariates between the treatment and control groups as well as choosing a relevant definition of local markets when conducting retrospective merger analyses in retail markets.

Robustness to Anticipation Concerns The baseline specification has been estimated removing six months before and after the merger, in order to prevent a short term

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[33]The propensity score probit estimates are reported in the Online Appendix. We also estimate the price effect of the merger using the more standard nearest neighbor DID matching estimators. However, due to the common support assumption, we lose almost half of the treated stores, which reduces considerably the sample size: we then can no longer guarantee the balance between the panels of products in the treatment and in the control groups. Subject to this caveat, and except for the case of one-nearest neighbor, we obtain significant point estimates with 2, 3, 4 or 5 nearest neighbors. These results are available upon request.

Table 10: Long-Difference Estimates

Dependent variable: $\Delta_{01S2-98S2}$ (log) price (by product, by store)		
Variable	DID	DID-Matching
	(1)	(2)
PostMerger $\times$ T	0.0345***	0.0414***
	(0.0098)	(0.0134)
Δ log(market income)	0.0003	-0.0613
	(0.1362)	(0.1670)
Constant	0.0494***	0.0390*
	(0.0139)	(0.0232)
Product FE	Yes	Yes
R^2	0.221	0.234
Observations	4194	4194

Notes: Insiders are removed from the sample. Stores catchment areas are delimited using the baseline definition (20/10 km), and treatment and control groups are defined according to Treatment/Control 2. The observations are weighted by the expenditure shares of food products calculated at the national level. The standard errors, shown in parentheses, are clustered by store. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

anticipation effect due to the merger. In our case, the merger was announced in the press nearly one year before the approval, suggesting that the parties could have coordinated their actions well before May 2000. In an attempt to evaluate whether our results are sensitive to a longer anticipation, we consider an alternative econometric specification that compares the level of prices on the long-difference between 1998 and 2001. Basically, the purpose of the long-difference specification is to confront the long-run equilibrium outcomes before and after the merger, which eliminates all possible biases yielded by an anticipation of the merger, and more generally by any transitory shock occurring during the period. By contrast, the baseline analysis conducted with the full panel may suffer from understated estimates if the merging groups anticipate the operation and raise their prices before the event window.

In order to control for the seasonality of sales in the food retail sector, we regress the difference in (log)prices between the second half-year of 2001 (2001S2) and the second half-year of 1998 (1998S2) for each product j sold in store i:

$$\Delta P_{ij} = \alpha + \beta_1 T_i \times Outsider_i + \delta' \mathbf{\Delta Z}_i + \gamma_j + \varepsilon_{ij} \tag{4}$$

where $\Delta P_{ij} = lnP_{ij}^{01S2} - lnP_{ij}^{98S2}$, T_i is the dummy variable equal to one for stores belonging to the treatment group, and β_1 is the coefficient measuring the price merger effect for outsiders. The long-difference regression also controls for the change of market characteristics $\mathbf{\Delta Z}_i$ during the period and for product-specific fixed effects γ_j.

Table 10 presents the DID and DID-matching estimates for the long-difference specification. We obtain a noticeably higher point estimate of the price merger effect compared to the full panel specification, but we control at a different level for unobserved product heterogeneity. The estimated merger effect in Column (1) shows that prices

Table 11: Heterogeneous Effects by Product Category

Dependent variable: (log) price (P_{ijt})						
Variable	Product families				Types of products	
	Fruits & Vegetables	Meat & Fish	Alcoholic Beverages	Other Products	Random Weight Products	Branded Products
	(1)	(2)	(3)	(4)	(5)	(6)
PostMerger × T	0.0162	0.0119	0.0401	0.0183**	0.0208	0.0163***
	(0.0362)	(0.0300)	(0.0558)	(0.0076)	(0.0173)	(0.0057)
log(market income)	0.4070	-0.2573	-0.1508	-0.0172	-0.4701	-0.0353
	(0.4454)	(0.4411)	(0.2981)	(0.0720)	(0.3511)	(0.0881)
Constant	3.0480	10.8678**	9.0339***	7.3551***	8.7359***	6.7871***
	(4.1280)	(4.4099)	(2.8313)	(0.6874)	(2.0390)	(0.4701)
Store FE	Yes	Yes	Yes	Yes	Yes	Yes
Product-time FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.848	0.854	0.983	0.991	0.927	0.996
Observations	1980	1932	588	20664	7896	17268

Notes: Insiders are removed from the sample. Stores catchment areas are delimited using the baseline definition (20/10 km), and treatment and control groups are defined according to Treatment/Control 2. The observations are weighted by the expenditure shares of food products calculated at the national level. Data for the year 2000 are removed (i.e., event windows). The standard errors, shown in parentheses, are clustered by store. *, **, *** indicate significance at the 10%, 5%, 1% level, respectively.

have increased on average about 3.45% for outsiders' stores affected by the merger compared to non-affected outsiders' stores. These results reinforce our previous findings and demonstrate their robustness regarding potential temporary confounding factors and/or an anticipation of the merger. When using the DID-matching estimator (see Column (2)), the point estimate is even more important and still statistically significant.

Heterogeneous Effects by Product Category   One difficulty when dealing with price variations in the food retail sector is that households' shopping basket are composed of very different items, and pricing policies may vary substantially from one product category to another, as well as across retailers. Some retailers adopt aggressive price positioning on top-selling products of the non-fresh aisles (e.g., dry grocery or beverages) whereas others choose to differentiate themselves through the sale of high-quality (expensive) products, such as fruits, vegetables or meat. Reductions in competition may enable retailers to raise prices on certain product categories. Therefore, it is important to detect whether the price raising effect of the merger is due to a price increase of all goods, or whether it is mainly driven by some product categories.[34]

We present in Table 11 the DID estimates decomposed by product categories. In Columns (1) to (4), we estimate the merger effect separately for four product groups: Fruits & vegetables, meat & fish, alcoholic beverages, and other products. We observe that the prices of fresh products and alcoholic beverages do not vary differently between outsiders' stores affected by the merger and those unaffected. By contrast,

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[34]Note that, when defining our product sample, we have not kept private label products in our data analysis.

the prices of grocery items and non-alcoholic beverages (i.e., other products) have increased significantly more in outsiders' stores exposed to the merger. In Columns (5) and (6), we test more directly whether our results suffer from a measurement error in prices by decomposing products between random weight products and branded products. Because there is no barcode for random weight products, we cannot guarantee that this type of products are homogeneous at the UPC-level. The computation of their average prices may thus suffer from a measurement error and it is important to check that our results are not subject to this bias. As observed in Column (5), the non-statistical significance of the point estimate attests that the potential measurement error in prices for random weight products does not cause the observed outcome.[35] By contrast, and as observed in Column (4), the prices of branded products have significantly raised by 1.63% after the merger at outsiders' stores exposed to the merger (see Column (6)).

8   Conclusion

In this paper, we take advantage of a national merger between two French retailers, which impacted market structure differentially across local markets, to estimate the causal effect of this retail merger on retail prices. Our findings are along the lines of the related literature on retrospective merger analyses, which often concludes that mergers cause price increases.[36] In the supermarket industry, previous research finds that mergers cause price increases especially when they occur in already concentrated markets (Hosken, Olson and Smith, 2012). Our empirical evidence supports this. Moreover, we are able to separate the price effects for: (i) the merging firms, for which we point out a significant and national price increase after the merger of about 7%; (ii) the outsiders, for which we identify a significant and causal local effect of the merger, that translates into a price increase of about 2% in the outsiders' stores that are affected by the merger, compared to the outsiders' stores that are not affected by the merger. Furthermore, we point out that the price effect on outsiders is larger in markets in which the merger changed the chains' differentiation (the price increase at outsiders' stores is larger in local markets where the variety of stores available to consumers has decreased more).

The estimated price increase has important implications for consumer welfare. As food expenditures amount to approximately 12.9% in the European Union (on average, as of 1999), and as supermarket chains represent around 70% of total food sales in France (74% in 2011, INSEE), a back-of-the-envelope calculation shows that a 1.14% increase in supermarket food prices roughly represents a 0.1% drop in consumer pur-

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[35]Those findings are robust when using a DID-matching estimator (see the Online Appendix).

[36]Many retrospectives have examined mergers in airlines, banking, oil, consumer goods, and in the hospital industry (see Hunter, Leonard and Olley, 2008, for a review).

chasing power. Obviously such a simple calculation has to be taken with caution, as we do not take into account the effect on non-food prices and other services, but it gives an idea of the possible impact of such a merger on welfare.

Our findings are also important for retrospective merger analysis in a methodological sense. In terms of policy implications, we believe the main lesson from this analysis for competition authorities is that the way the relevant local markets are usually defined in the retail industry may lead to an underestimation of the price effect of the merger. We show that, when reviewing retail mergers, one needs to pay particular attention to the pre-merger pricing strategies of the parties under review. Given the local dimension of competition in the retail sector, any merger analysis should focus on the effects of the proposed merger on local markets. We actually show that, with the usual definition of the relevant market (for instance, the one used by the EC or by the French CA in the present case), some local markets would be considered as unaffected by the merger, because the merger does not induce a change in their local concentration, even though we observe that they are affected by price changes. This occurs when some retailers follow a national pricing strategy: by increasing their prices uniformly on a national scale, they increase prices in areas supposedly "unaffected". In the present case, we observe that the two merging firms follow such a national pricing strategy, while the outsiders have a local pricing strategy. Of course, the final impact of a merger on prices depends on which firm follows which type of pricing strategies. We thus conclude that, before defining the relevant markets for the analysis of the merger effect on final prices, competition authorities should first analyze whether the retailers have local or national pricing strategies.

Finally, one of the major challenges of competition policy is to predict the potential price effects at the time when antitrust authorities are notified of a merger, in order to impose relevant remedies and to better protect consumers. In this setting, a retrospective merger analysis is not possible. Using our detailed data, we can perform a first step in that direction, by providing a simple prediction of how the local concentration changes induced by the merger would affect local market retail prices. Using the estimation results of Table 3 (Column 4), we perform an out-of-sample price prediction, given the post-merger local HHI levels. We find a predicted price increase of 1.19% with the new HHI, with a standard error of 0.01%. We conclude that these simple predictions based on the variation in the local HHI are rather close to the 1.14% price increase obtained with our DID approach (see Table 5 column 3). However, as our DID estimates, these predictions do not account for price changes at the national level: using the HHI as a preliminary screen for merger analysis is an attractive tool (a finding consistent with Hosken, Olson and Smith (2012)), but it should be complemented with an analysis of the pricing strategies of insiders and outsiders in order to account for national price changes. This calls for a more complete structural approach

(as in Houde (2012)), which we leave for future work.

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