The impact of retail mergers on food prices: evidence from France

Marie-Laure Allain*, Claire Chambolle†,
Stephane Turolla‡ and Sofia B. Villas-Boas§

February 1, 2013
VERY PRELIMINARY VERSION

Abstract

Using consumer panel data, we analyze the impact of a merger in the retail sector on food prices in France. In order to capture the local dimension of retail competition, we define local markets as catchment areas around each store. We develop a difference-in-differences analysis to compare price changes in local markets where the merger did modify the ownership structure (treated group) to price changes in local markets where the merger did not affect the ownership structure (control group). We find that prices of competing firms in areas where the merger occurred (treated group) increased significantly relative to the control areas where existing firms were not affected by a merger. In fact, our findings suggest that the merger significantly raised the competitors' prices, while it is not correlated with any significant effect on prices of the merging firms. These results are consistent with a combination of efficiency gains for the merging firms (as their prices did not increase) and possible coordinated effects or a decrease in differentiation (as competing prices increased).

Keywords: Ex-post merger evaluation, Retail grocery sector, Difference-in-differences
JEL Classification: K21; L11; L66

*CNRS, Ecole Polytechnique (Economics Department, Route de Saclay, Palaiseau, France; email: marie-laure.allain@polytechnique.edu) and CREST.

†INRA (UR1303 ALISS, 94205 Ivry-sur-Seine, France; email: claire.chambolle@ivry.inra.fr) and Economics Department, Ecole Polytechnique.

‡INRA (UMR 1302 SMART, 4 Allée Adolphe Bobierre, CS 61103, 35011 Rennes Cedex, France; email: stephane.turolla@rennes.inra.fr)

§ARE University of California, Berkeley (United States; email: sberto@berkeley.edu).

1 Introduction

Over the last thirty years, successive merger waves have dramatically increased the food retail sector concentration in most western economies. In 2000, in the US, the first five retail groups realize close to one third of total food sales. In Europe, the highest concentration ratios are attained in the northern countries, with CR3 -total market share for the first three retailers- up to 90 %, but Western European countries are also characterized by highly concentrated retail sectors.[1] In the US, the number of supermarket mergers has increased from 20 in 1996, to 25 in 1997, to 35 in 1998.[2] In 1999, the Federal Trade Commission reviewed and approved two of the most important supermarket mergers: Albertson's acquisition of American Stores[3] (the second and fourth largest chains in the U.S.) and Kroger's acquisition of Fred Meyer, which created the largest U.S. grocery stores chain the second general retailer in US in terms of revenue behind Wal-Mart. At the same period, the European Commission took two important decisions that have established the basic principles upon which mergers in the retail sector are now assessed in Europe. In 1997, the European Commission prohibited the merger between two leading food retail chains in Finland, Kesko and Tuko,[4] and, in 1999, the merger in Austria between Rewe and Meinl was allowed conditionally to some stores' divestments.[5]

When reviewing retail mergers, Competition authorities have to balance potential efficiency gains with potential anticompetitive effects as in all merger cases. Yet two particular features of the retailing sector, namely the local dimension of retail competition and the buyer power issue, make the analysis more complex. First, as supermarkets compete at the local level, the effects of a merger have to be analyzed for each local relevant market. Having to take the local dimension of competition into account while assessing the effects of a merger creates specific difficulties in the analysis of retail mergers (see Turolla, 2012). Second, competition authorities also have to take into account that, after the merger, the "enlarged" retailer is likely to benefit from an increased buyer power towards manufacturers, that is, a capacity to obtain better terms and conditions from its suppliers. This buyer power effect may be a factor that makes mergers in the retail sector more likely to lower prices than mergers in other industries. Beyond this complexity, supermarket mergers are a particularly important issue for Competition

[1]For instance, the CR3 in the retail sector was 91.2% in Denmark, 79.6% in Finland, 81% in Island, 82% in Norway, and 91.2% in Sweden in 2004 (Source: The Retail Sector in the Nordic Countries A Comparative Analysis, Ágúst Einarsson), while in 2003, the sector's 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]Source: Food Retailing: The two faces of Supermarket Mergers", 1999, A.A. Foer, American Antitrust Institute.

[3]The Federal Trade Commission has approved the proposed merger of Albertson's Inc. and American Stores Co., after the two stores agreed to sell 144 supermarkets. See United States of America before Federal Trade Commission In the Matter of Albertson's, Inc. and American Stores Com.

[4]97/277/EC Kesko/Tuko (OJ L 110/53, 26/4/1997).

[5]1999/674/EC Rewe/Meinl (OJ L 274/1, 23/10/1999).

Authorities because food expenditures represent a large share of household budget, about 12.9% on average in European Countries and thus a retail merger may potentially have a large impact on consumers.[6]

The aim of this paper is to assess the impact of a merger among supermarkets on food prices. This paper fits in a growing economic literature which attempts to evaluate whether approved mergers actually increased prices. According to Ashenfelter et al (2011), some experts consider that U.S. antitrust policy towards horizontal mergers has been too lenient.

Historically, empirical mergers analysis goes into two main directions and there is a lively debate among the two approaches (cf. Weinberg, 2008, Nevo and Whinston, 2010, Angrist and Pischke, 2010). First, some papers in the spirit of Nevo (2000) build structural models of demand and supply and then simulate the merger effect using pre-merger data. In the supermarket industry, Smith (2004) estimates a model of consumer choice and expenditure to assess the effect of cross-elasticities between stores of the same chain on market power on the UK supermarket industry. He shows that a simulated divestiture would reduce the prices of the largest firms by between 2.03 and 3.76% depending on local concentration while mergers between the largest firms would lead prices to increase up to 3.20%. More recently, empirical papers such as Hastings (2004), Hastings and Gilbert (2005), Taylor and Hosken (2007), Ashenfelter and Hosken (2010) or Ashenfelter et al. (2011) have used both pre and post merger data on prices to directly estimate the effects of mergers.[7] 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.

Our analysis is closely related to this second stream of literature, however by contrast with Davis (2010) we compare pre- and post-merger prices at the local level. We benefit from an exceptional database, which records prices over a period where a major retail merger occurred in France. In this paper, we use consumer panel data and a survey of retail outlets in France to assess the effect of a merger on retail prices. We use information on food consumption and food prices from a consumer panel (Quantar TNS-WorldPanel) over the years 1998-2001 and data on the French retail sector (location address and characteristics of the supermarkets) over the same period. In 2000, the merger of two among the main five retailers caused the most important change in market structure over the period: we thus focus on price comparisons before and after 2000. These provide a quasi-experimental setting on which we base our empirical analysis of the impact of the merger on prices. The merger was decided nationally and depending on whether the retailers involved are or are not located in a certain market we are thus able to estimate

[6]Eurostat, 2010.

[7]Recently, Houde (2012) offers to conduct both a retrospective analysis of a vertical merger in the Canadian gasoline sector and a structural econometric approach and reconcile both approaches. See also Weinberg and Hosken (2012), Weinberg (2011), Bjornerstedt and Verboven (2012).

directly the effect of the retail merger on food prices by comparing price changes in markets affected by the merger (treated markets) to price changes in markets unaffected by the merger (control markets).

We use a difference-in-differences approach, by comparing the change in a treatment group to the change in a control group, the latter controlling for what would have happened to prices in the absence of the merger. We assume like in Hastings (2004), Houde (2012) or Focarrelli and Panetta (2003) that in the short term the merger influences prices at the local level only. The definition of treatment and control groups is complex for two main reasons. First, the assignment of stores into affected and unaffected markets is not random but endogenously determined by the merging firms' strategies. Second, the spatial dimension of retail competition makes it particularly difficult to draw the line between affected and unaffected markets. Several recent papers provide methods to improve the definition of the treatment and control group.[8] In this paper, we test several definitions of the treatment and control groups, and our results are robust to most changes in that respect. In our identification strategy and empirical analysis we take advantage of the fact that the merging parties did not decide to merge in certain local markets differently from other local markets, and that the merger was approved at the national level. Local markets thus got affected by the merger to the extent that the merging firms were in business there. In particular, we compare price variations in markets where the merger did imply a change in the ownership (given its pre-determined geographical location) to price variations in markets where the merger decision did not impact the ownership of existing retailers (because stores from both merging parties were not present). In the baseline model, we define the stores that do not compete with a store belonging to the merging firms as belonging to the control group. The treatment group thus consists of the stores located in the same catchment area than a store that belongs to one of the merging groups, while the other stores are in the control group. Inside the treatment group, we break the stores into two types : the merging firms (the insiders) and the stores directly competing with the merging firms (the outsiders).

As the pure difference-in-differences may be affected if the treatment and control groups differ in the pre-period we perform alternative comparisons for the markets affected by the merger through semi-parametric matching estimators. More precisely, we conduct two additional estimation approaches using a propensity score matching estimator where in the first step we estimate the probability of being treated (of a merger occurring) as a function of baseline variables. In a first approach to control for differences in observed confounding factors between treated and control stores, we apply a re-weighting scheme proposed by Hirano *et al* (2003) and Imbens (2004). The basic idea is to use the fitted values of the probability of treatment from the probit analysis (the propensity scores)

[8]For instance, Choné and Linnemer (2010) study a parking merger in Paris and provide a systematic method to construct the groups which applies to any industry with spatial competition.

to re-weight the regression sample, effectively creating a smooth version of a match on propensity score. The second approach uses a nearest neighbor matching estimator. For each treated store, we find the more relevant counterfactual based on the propensity scores, i.e. the control store that matches more closely the treated store.

We find that the merger affected competitors' prices positively and significantly while it was not correlated with the merging firms' prices. By decomposing this effect, we show that competitors have raised their price when the merger brought about a drop in the number of competitors in their local market or a store rebranding.

The rest of the paper proceeds as follows. Section 2 provides the background while section 3 describes the data used. The empirical strategy is outlined in section 4, in section 5 we present and discuss the results as well as perform several robustness checks by varying the definition of treatment and control groups using several matching methods showing that our results appear to be robust to many specifications and placebos. Finally, section 6 concludes.

2 Background

Before giving some facts about the merger, we provide in this section some background on the French food retail sector.

2.1 The French food retail sector

In 2000, the French retail sector is already rather concentrated: the total market share of the five main retail chains (CR5) is close to 73%[9], a rather high concentration compared to the UK or Germany (respectively 64 and 57 %[10]). According to the French Competition authority estimates, on the overall retail market, the joint market share of the two merging groups therefore denoted $M1$ and $M2$ is around 29,4%,[11] while most of the remaining is split between the largest rivals $O1$ (15.4 %), $O2$ (15.1 %), $O3$ (13 %) and $O4$ (9.9 %).[12]

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

[9]Source: French Competition Authority, based on Nielsen data.

[10]Source: Lineaires, based on data by M+M Eurodata

[11]The Conseil de la Concurrence uses Nielsen data to compute these estimates; the report also quotes the -consistent- estimates for the joint market shares of the two groups by format: 31.2% of hypermarket sales, 22.3 % of supermarket sales, 16.1 % of discounter's and 26.9 % for all. 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 $M2$, for discounters $M1$ has 15.1 % and $M2$ 16.4 %.

[12]Due to a confidentiality agreement with TNS Worldpanel who provided us the data, we are not allowed to disclose the retailers' name. Henceforth, we denote by $M1$ and $M2$ the merging groups and by Oi the *ith*-rival group (outside to the merger).

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, hard discount stores are (usually small) supermarkets that carry a limited assortment of products, mostly sold at low prices and under their own brands.[13] 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.[14]

In France, price decisions are made at the store level and thus at least partly reflect local competition (Biscourp et al., 2012; Turolla, 2012). In particular, by contrast with main retail chains in UK, French retail chains have never committed themselves to set uniform pricing at the national level (Dobson and Waterson, 2005).[15] In some retail groups, other strategic decisions (advertising campaigns, negotiations with suppliers, products listings, or private label assortments among others) are taken at a more centralized level.

The evolution of the economic environment of the industry in the late nineties to early two-thousands has constrained our choice of the period around the merger. First, the regulatory environment influenced prices by changing the conditions of competition in the retail sector. Two laws, the Galland law and the Raffarin law 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.[16] Besides, in 2002 the monetary change (French Franc disappeared as Euro was launched on january 1, 2002) is also likely to have had an effect on retail prices.[17] In order to avoid these two sets of shocks that are orthogonal to the merger, we focus on the period 1998-2001. Note that this period is characterized by numerous changes in the retail sector: mergers, opening of new supermarkets, and changes of ownership, to name a few. We will focus on the merger between two of the five main retail groups and will control for all other structural changes in our analysis.

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

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

[15] In 2004 in UK the main retail chains, Tesco, Asda, Sainsbury's, and Morrisons made 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."

[16] See Allain and Chambolle (2011) for a study of the price-floor mechanism involved by the law, and Biscourp et al. (2012) for an empirical investigation of its price effects. For expert reports, see, e.g., Commission Hagelsteen (2008) or Allain et al. (2008) for a review.

[17] 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 eurozone. After the changeover, cheaper goods had higher inflation, and this effect was significant in France.

Finally, an important characteristic of the retail sector is that, irrespective of global concentration ratios, local competitive conditions deeply affect final prices. In its first report on the sector, the French Competition Authority argued that: "The concentration of the retail food industry has little effect on the downstream market because competition is fierce among retail chains" (Conseil de la Concurrence, 1997, p.28). However, the position of the French competition authority has changed over time, and in more recent reports the authority expressed the view that retailers benefit from weak local competitive conditions and exert significant market power in local markets (see Bertrand and Kramarz, 2002; Autorite de la Concurrence, 2007). A 2012 report by the French Competition Authority even calls for the right to impose ex-post remedies to retail groups when they are too highly concentrated in some areas, such as Paris.[18] In this paper, we therefore pay special attention to the local dimension of competition: for each store we will design a "catchment area" (see section 3.2), that is, the local market where the store is active. We define local competitors as the set of competitors located in this catchment area.

2.2 The merger

At the end of august 1999, retail groups $M1$ and $M2$ announced their intentions to merge. The two groups were spread across 26 countries, but we focus on the French market, the country of origin of the two groups, where they gathered around 220 hypermarkets and 1100 supermarkets. The two networks were rather complementary. Henceforth, we denote by $M1_H$ and $M2_H$ the hypermarket chains respectively owned by $M1$ and $M2$ and by $M1_S$ and $M2_S$ the main supermarket chains respectively owned by $M1$ and $M2$. According to press releases, only 21% of $M1_H$'s customers also had visited a $M2_H$ store between July 1998 and June 1999, while half of $M2_H$'s customers claim to be occasional $M1_H$'s customers. The EC approved the merger on January 25, 2000, on the condition that $M1$ realized some divestments. It then delegated the decision to national competition authorities in order to assess the impact of the merger on retail competition at the local level in several countries. The French competition authority concluded that competition was likely to be affected in 27 local areas. Finally the merger was approved May 3, 2000, however, the remedies required were not all pressed by the French ministry of Economics and the merger finally received the final administrative approval in May 2000.

The merger was progressively implemented by the two groups over the next 18 months. Before the merger, in France, $M1$ operated stores under eight brands: the hypermarket brand $M1_H$, a main supermarket brand $M1_S$ and $M1'$ which gathers all the other supermarkets, convenience stores and discounters brands. $M2$ operated stores under seven brands: the hypermarket brand $M2_H$, a main supermarket brand $M2_S$, and $M2'$ which

[18] Autorité de la Concurrence (2012), Avis 12-A-01 du 11 janvier 2012.

gathers all the remaining supermarkets and convenience stores brands. The merged firm conducted a large rebranding operation: For hypermarkets, $M2_H$, were rebranded into $M1_H$, while for supermarkets, all $M1_S$ were rebranded into $M2_S$. Figure 1 illustrates the effect of the merger on the supermarket and hypermarket brands operated by the two merging groups.

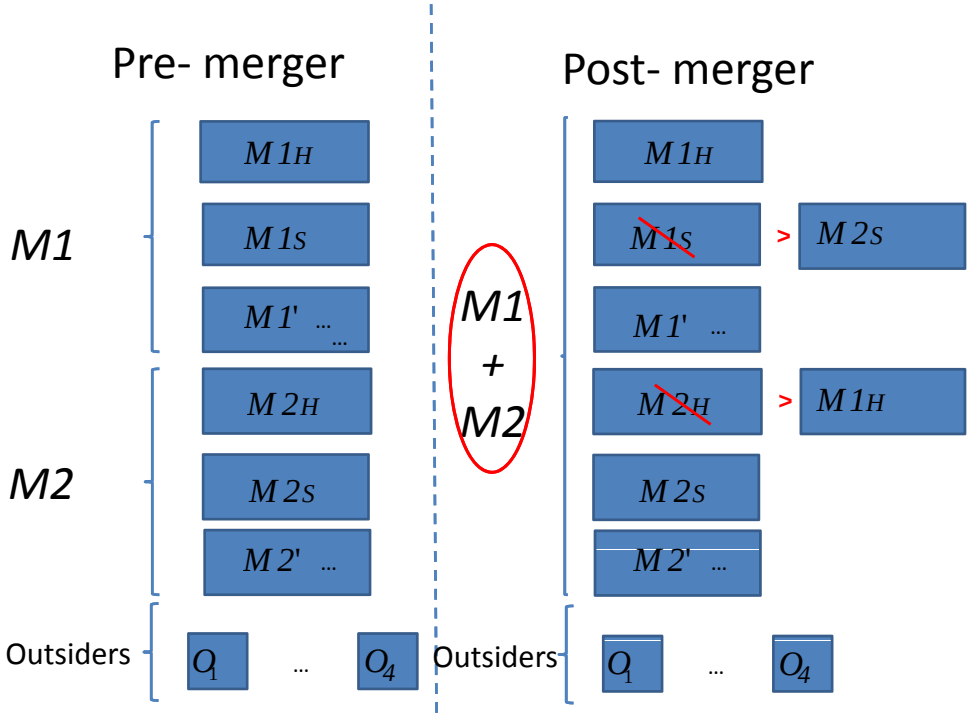

Figure 1: Rebranding

Table 1 indicates the evolution of the rebranding operations for the two main fascias that were suppressed. The first rebranding of a $M2_H$ into $M1_H$ took place in 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). The cost of rebranding a store is rather high, as it involves building work, but also changes in operation systems as well as induced demand shock. In 2000, $M1$ estimated the cost for rebranding a $M2_H$ into $M1_H$ from 75000 to 150000 euros. The reorganization of the logistic system started at the end of 2000.

During the period 1998-2001, the retail concentration clearly increased in France. Panel A of Table 2 displays the evolution of the Herfindhal Hirshman Index (HHI) before and after the merger at the regional and national levels. Note that, as we do not have sufficient data to build the index upon real market shares, but, as it is widely admitted that store sales are highly correlated to their selling area, we base the concentration index

Table 1: A time-line evolution of the $M1$ – $M2$ merger

	1998				1999			
Number of stores	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

	2000				2001			
Number of stores	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 figures reported correspond to the number of stores owned by the merging groups and their rivals over the period 1998 to 2001. $M1_H$ ($M2_H$), $M1_S$ ($M2_S$), and $M1'$ ($M2'$) denote the hypermarket brand, the main supermarket brand, and all the other store brands of the merging group $M1$ ($M2$, respectively). Computed from *Panorama Tradedimensions*; authors' calculation.

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 chain.[19]

According to the European Commission horizontal merger guidelines, a merger is likely to raise competition concern when the post-merger HHI is above 2000 while the variation is above 150. At the regional or national levels, concentration is thus low enough for the merger to be approved without condition. However, the local dimension of the retail market calls for a local assessment of concentration. For each store, we can compute a local concentration index. HHI at the local level is computed using the definition of the local market based on the definition of the catchment area around each store that we will present more in details later on in the paper (that is, including all hypermarkets within a 20 km radius and all supermarkets within a 10 km radius). Panel B of table 2 presents the distribution of HHI across local markets. Local concentration often appears clearly higher than the threshold recommended by the European Commission, and this explains why the Commission referred to the French competition authority for an assessment at the local market level. 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 marked (the third quartile

[19]In order to measure concentration levels, the Commission often applies the Herfindahl-Hirschman Index (HHI), cf. "Guidelines on the assessment of horizontal mergers under the Council Regulation on the control of concentrations between undertakings", 2004, III, §16.

Table 2: HHI before and after merger

Panel A: Regional or 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

	1st Q.	2nd Q.	3rd Q.	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: HHI computed at the retail group level. 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). The three quartiles of the distribution of the local HHIs are reported and the variation between 2000Q1 and 2001Q1 is given by ΔHHI. The mean of the local HHIs is computed and its standard errors is reported in parantheses. For this last case, ΔHHI is computed as the average of the HHI variation observed in each local market.

increased by 187). Note that these data gather the effects of all market changes and not only of the merger we focus on.

In the paper, we will focus on the effect of the merger on prices. A first estimate of the link between local concentration and prices reveals a positive correlation when controlling for local revenue and population. Table 3 estimates the correlation of local HHI index and prices of all products. At first glance, local concentration is negatively correlated with prices (see column 1): however, this effect is reversed if we control for income and population effects. The insight is as follows: In densely populated and richest areas, the retail sector is less concentrated. This effect may be explained by the relatively high radius of 20km we have used to define our catchment areas around each store. When considering big cities (densely populated and on average richer), the density of stores is also high and it is likely that most competing retailers own stores within the limits of the 20 km radius considered. Therefore such areas may have a relatively low retail concentration.

3 The data

3.1 Data Sources

This study uses an original dataset that combines information from three sources. The primary data are scanner data collected by the company TNS Worldpanel. The *TNS Worldpanel* database records food purchases from a panel of French households 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

Table 3: Regression of prices on local market concentration

Dependent variable: (log) of mean price (by semester)			
Variable	(1)	(2)	(3)
Store size (m^2/1000)	0.0016***	0.0011***	0.0008***
	(0.0001)	(0.0001)	(0.0001)
HHI (/10000)	-0.0260***	0.0059***	0.0278***
	(0.0011)	(0.0012)	(0.0013)
log(market income)		0.0851***	0.0597***
		(0.0008)	(0.0010)
log(market population)			0.0037***
			(0.0001)
Constant	7.7514***	6.9346***	7.1304***
	(0.0006)	(0.0078)	(0.0093)
Semester FE	Yes	Yes	No
Store format FE	Yes	Yes	Yes
Product FE	Yes	Yes	Yes
R^2	0.958	0.959	0.959
Observations	2457002	2457002	2457002

Notes: Prices are expressed in centimes (one centime equal 1/100 franc) per measurement unit (i.e. liter, Kg or unit). Promotional prices are excluded from the computation of average prices.

with a home scanner. Information are 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. We have information on prices paid, quantities purchased, and products are described by a rich set of characteristics. Overall, the data cover more than 400 categories of food products. In addition, households provide information about their shopping place, by filling in the store type (e.g., retail stores, convenience stores or specialized shops, and inside retail stores, hypermarket, butcher, or delicatessen, for instance), the store size and, for retail chains, their name. For the purpose of the study, we consider the period that spans from 1998 to 2001 which corresponds nearly to 28 billions of purchases. A more detailed presentation of the home-scan data is provided in the Data Appendix. We complement the data with information on retail store characteristics over the same period, obtained from the *Panorama Tradedimensions* dataset. This dataset lists grocery retail stores that operate in France and gives information on their attributes such as store size (in square meters), format, chain name or store address, for instance. The dataset also reports information on changes of ownership, as well as opening, extension, or closing of stores. Lastly, we collect information in Census surveys at the same period to proxy demand faced by stores at the *commune* level (French administrative unit similar to city): population and average household income.

While 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 exact location of the store where the products are purchased prevents us to match directly

the purchases data with the dataset on store characteristics. We recover the missing information by using the household address, the name of the chain and the size of the store where the purchase was made. We construct an algorithm which (1) defines the set of all candidate stores of the relevant chain around the household residence, and (2) 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 (see the Data Appendix for more details on the procedure). The missing link between the food purchases and the retail stores being recovered, we have now store-product level data.

We observe a large disparity in the frequency of purchases among products. For instance, bottled water represents 6.93% of the recorded purchases whereas chocolate bars amount to 1.81%. 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, making the tracking of products with low sales at the store level difficult. Consequently, we choose to aggregate the data at the semester level to account for a larger part of food products bought in France. Therefore, we compute for each UPC a mean price by semester expressed in centimes of French Franc (1 centime ≈ 0.0015 €). We follow recent studies using retail scanner data (e.g., Nevo, 2000), and calculate price as average revenues (i.e., sales/quantity purchased). Unavoidably, this comes at the cost of introducing a measurement error in price due to price fluctuations during a semester.

Beyond the size of the sample, several factors contribute to explain the low representativity of some products. For instance, the local dimension of some products (e.g., bretzel in the North-East of France) or the introduction (or exit) of new products on the shelves lead to few observations. Therefore, we decide to restrict our attention to UPCs that satisfy some criteria of representativity in order to compare prices over time and across stores affected or not by the merger (see the Data Appendix). According to this selection procedure, we identify 109 UPCs that gather both national brand products and fresh products (i.e. fruits, vegetables, meat and fish).

To sum up, the data used in this study covers 109 UPCs sold in 595 stores over the period 1998 (pre) to 2001 (post) and information are aggregated at the semester level. The unit of observation in our analysis is an average price for a product j computed as a quantity weighted price over a semester in a certain market and retail store.

3.2 Local market definition

Assessing the price effect of the merger requires us to draw the outlines of 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 around the store originate. Hence, the set of competitors for a store will be defined as the set of stores located

Table 4: Market structure of local markets

Fraction of catchment areas with *at least one*:	20/10 km	
	Raw data	Final data
$M1_H$	46.41	50.59
$M2_H$	37.83	38.32
$M1_S$	43.29	47.56
$M2_S$	51.11	50.76
$M1_H$ & $M2_H$	25.90	24.71
$M2_S$ & $M1_S$	25.18	27.23
$M2_H$ & $M1_S$	23.14	24.03
Merging firms $M1 + M2$	84.47	86.72
Total number of catchment areas	9605	595

Notes: Local markets are delimited with the baseline definition (20/10 km). The statistics on market structure are reported for the second semester of 1999 (pre-period). The statistics corresponding to the *Raw data* are computed from Panorama Tradedimensions that compiled global information for all of the grocery stores operating in France. The *Final data* only restrains a subset of those stores for which the recording purchases in the TNS Worldpanel satisfy some criteria of representativity over the period of study. The final dataset corresponds to the data used in the empirical analyses.

inside this area. In the Rewe/Billa and Rewe/Meinl decisions, the European Commission states that, from the direct viewpoint of the consumer, the geographic markets affected by a concentration in food retailing are the local markets in which the firms involved run shops: "These local markets can be defined as a circle with a radius of approximately 20 minutes by car centered on the individual sales outlet". Furthermore, it is generally admitted that hypermarkets have a larger catchment area than supermarkets. The French competition authority considered in the relevant 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 a smaller supermarket or to a discount store.

In line with the position of the French CA, we define the local market around each store as the market area that spans up to 20 km for hypermarkets (and up to 10 km for other formats) around the center of the city where the store is located. Thus, the set of local competitors for a given store consists of all the hypermarkets within 20 km around the city center where the store is located, and all other shops within 10 km.[20] Because 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. The definitions of these alternative scenarios are available in the Data Appendix. Table 4 presents some statistics on the configuration of local markets computed from the whole set of stores operating in France (raw file) and also from the final dataset used hereafter (final data). This table shows that stores belonging to the merging firms are present in more than 84% of the local markets. The hypermarkets of the merging group are very

[20]Note that our definition of the relevant market differs from Barros *et al.* (2006) and is more precise, as we define catchment areas based on the distance between the cities where the shops are located, and not on the distance between the main cities of the departments where the shops are located.

well distributed over the national market, as almost half of the catchment areas contain a store $M1_H$ (46 %), while around 38% have a $M2_H$; furthermore they compete in only 26% of the areas. The supermarket chains belonging to the merging group are also present in around half of the areas, while they compete in around 25% of the areas. Finally, the comparison of the two datasets displayed in Table 4 shows that the final dataset, which corresponds to the purchase data that satisfy some criteria of representativity and that we use hereafter, reflects quite closely the French retail market structure.

4 Empirical strategy

4.1 Methodology and identification

Our goal is to estimate the price changes that result from the merger. A straightforward way to measure those evolutions would consist of comparing the mean changes in prices, i.e. the average differences between pre- and post-merger prices, for stores impacted by the merger with the potential mean changes that those stores will have occured if they were not affected by the merger. Since it is not possible to observe how prices would have evolved "but for" the merger, we construct a counterfactual that must reflect as closely as possible how stores would have reacted in absence of the merger. To that, we exploit the unequal effects of the merger over the French territory by including in the comparison group stores that were not affected by the merger in their market area.

Building on the standard program evaluation literature we postulate that there are two "states of nature" into which a product sold at a given store could have been assigned: The first state is such that a product is sold in a market where no store is affected by the merger and the second state is such that a product is sold in a market where the merger influenced the market structure. In the following we estimate the effect of the merger on prices by comparing the changes of products' prices between the two competitive states. 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 ($T = 1$) and the complementary group, called the *control group*, that includes subjects unexposed to the treatment ($T = 0$). Let $P_{ijt}(0)$ be the price charged by store i for a product j (at a non-treated store) at semester t and let $P_{ijt}(1)$ be the price under treatment, respectively. We are estimating the average treatment effect (ATE), which can be expressed as $\mathbb{E}[P_{jt_1}(1) - P_{jt_0}(1)|T = 1] - \mathbb{E}[P_{jt_1}(0) - P_{jt_0}(0)|T = 0]$ where t_0 and t_1 are the pre and post treatment semesters, respectively. The simple estimate of the average treatment effect is by computing an unconditional difference-in-differences. The key identification assumption is that, absent of the merger, the prices would have evolved

identically between the two groups.

A natural definition of the treatment group is to consider stores affected by the merger either directly (i.e., stores belonging to the merging firms, and hereafter named *insiders*) or indirectly (i.e. rival stores located in the same local market as a store of the new entity and named *outsiders*). Hence, rival stores that do not compete with a store belonging to the merging firms are included in the control group. The treatment group is defined as all stores belonging to a local market where one retail chain of the merging groups is active during the pre-period merger.[21] The control group gathers all the other stores, i.e. the outsiders $O1, O2, ...O4$. Figure 2 illustrates the definition of treatment and control group in a simplified local market with three hypermarkets belonging to the merging groups (M_1 and M_2) and five belonging to outsiders. Note that firm O_2 is in the control group because it faces no local competitor, while firm O_3 is in the control group while it faces a competitor, which in turn competes with one store of a merging firm. Note that, if we use the baseline definition of local markets (d =20/10 km), more than 84% of the markets include one store of the merging groups (see Table 4). The treatment group will therefore be larger than the control group. We will discuss in what follows several methods to correct this potential bias.

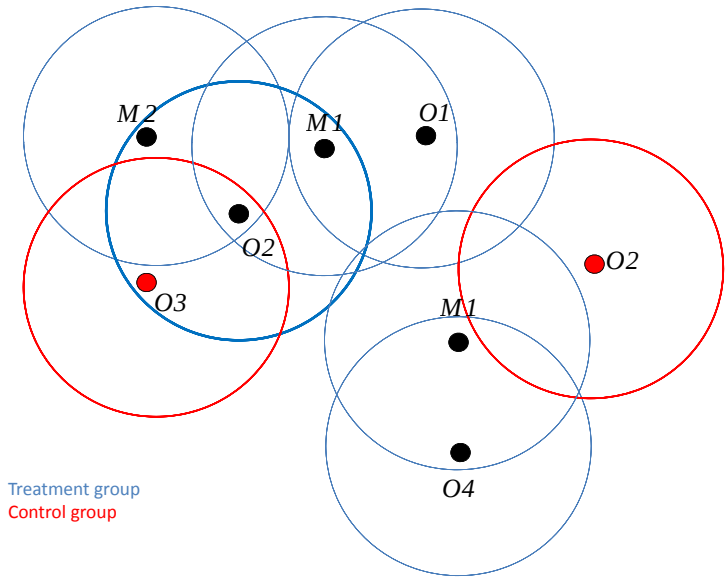

Figure 2: Treatment and control groups: definition

To ensure that the DID estimator identifies and consistently estimates the average effect, one may assume that assignment to treatment is independent of the outcome. Using the natural-experiment terminology, this means that assignment to treatment group is not confounded with the outcome (also known as the *unconfoundness assumption*,

[21] The corresponding retail chains are $M1_H$, $M2_H$, $M1_S$, $M2_S$ and $M1'$, $M2'$

see Rosenbaum and Rubin, 1983). This estimate will be biased if factors that could affect prices vary significantly across treated and comparison markets. Unfortunately, the unconfoundness assumption is hard to sustain in the context of merger evaluation because treatment assignment is not random. This is particularly true for retail mergers where firms decide to settle locally according to markets characteristics. Given that the merger is decided nationally treatment is assigned based on the pre-determined location. A concern then is that locations of firms are endogenous and thus the presence of retailers that merge may be in areas that are very different from the areas where the merging retailers are not located. For instance, firms that offered low quality items are more likely to settle in isolated low-income markets while other firms may prefer to operate in more concentrated and wealthier markets. To account for this selection bias it is usual to only require unconfoundedness conditional on a set of covariates that control for observed disparities between the two groups. According to this standard approach, we estimate the following ordinary least squares (OLS) regression using store-product level prices as dependent variable:

$$\begin{aligned} \ln P_{ijt} &= \alpha_1 + \alpha_2 PostMerger_t + \alpha_3 T_i^d + \beta PostMerger_t \times T_i^d \\ &\quad + \delta' \mathbf{Z}_{it} + \mu_i + \gamma_j + \sum_{n=1}^{N=j\times t} \lambda_n \tau_{jt} + \varepsilon_{ijt} \end{aligned} \tag{1}$$

where P_{ijt} denotes the average price charged by the i-*th* store, for product j during the semester t, $PostMerger_t$ is a dummy variable that identifies the post-merger period, and T_i^d is a dummy variable that characterizes that store i belongs to the treatment group, ie. $T_i^d = 1$ whether store i belongs to the merging firms or competes, in a neighborhood d, with a store belonging to the merging firms. Consequently the average effect of the merger is captured through the coefficient vector β. We note that the vector β is an average of price effects for merging and non merging firms. As it has the merging firms' price effects it cannot be interpreted as causal, as there is really no control group as we do not see the merging firms in markets where they do not operate. The "insiders" effect in β is interpreted as a correlation. However if we just average the effect for the "outsiders", the non merging firms, then it can be interpreted as the causal effect of the merger, as it is indeed a difference-in-differences point estimated effect. In order to clearly separate the type of price reaction that can be interpreted as a causal effect of the merger, we instead estimate the following OLS regression:

$$\begin{aligned} \ln P_{ijt} &= \alpha_1 + \alpha_2 PostMerger_t + \alpha_3 T_i^d \\ &\quad + \beta_1 PostMerger_t \times T_i^d \times O_i + \beta_2 PostMerger_t \times T_i^d \times (1 - O_i) \\ &\quad + \delta' \mathbf{Z}_{it} + \mu_i + \gamma_j + \sum_{n=1}^{N=j\times t} \lambda_n \tau_{jt} + \varepsilon_{ijt} \end{aligned} \tag{2}$$

where O_i takes the value one if store i is considering as an "outsider" at the pre-merger period.[22] The regression also includes a set $\mathbf{X}_{ijt} = \{\mathbf{Z}_{it}, \mu_i, \gamma_j, \tau_{jt}\}$ of observable covariates by store, product and time. The idea is that store fixed effects μ, product fixed effects γ and product semester fixed effects τ control for, respectively, store constant factors affecting price, product specific constant factors affecting price and product semester changing determinants of price, and all these factors are uncorrelated, that is, exogenous, to the merger, the treatment. Further, $\mathbf{Z}_{it}$ are time-variant store's catchment area attributes (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.

To limit the presence of confounding factors in the DID analysis, a particular attention must be paid to the definition of the treated and control groups as well as the selection of the event window surrounding the merger. By selecting a relevant control group we will control for permanent time-varying factors observed in both treated and non-treated markets (like a shock in demand), while choosing an appropriate period of time around the merger will be useful for controlling transitory time-varying factors.

Since we observe only eighteen months of data after the merger approval, we concentrate on the short-term effect of the merger. This will allow us to abstract from long term structural changes, outside the merger, that can affect prices in the long run, like, for instance, the monetary switch from French Franc to Euro (that occurred in 2002), as well as unobserved efficiency gains from reorganization that can reasonably be expected to materialize in a few years. Similarly to previous retrospective merger analyses in retail markets (e.g., Focarrelli and Panetta, 2003; Hastings, 2004 or Houde, 2012), we assume that in the short-term the merger only influences prices locally. In our case, there is no efficiency gain in the short term but cost reductions due to renegotiation of supply contract may be immediate. As we have seen in Table 1, 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 avoid issues related to transitory shocks generated by the rebranding of stores. We also choose to remove data from the first semester of 2000 to leave data uncontaminated by a potential anticipation of the merger by the parties.

4.2 Alternative estimators of the ATE

There are several potential identification issues with the reduced form specification presented above. First, if there is only limited overlap in the distributions of the vector of confounding factors $\mathbf{X}$ across the treatment and control groups and if functional form assumptions are incorrect, missing outcomes will be incorrectly imputed. Estimates of

[22] The above regression also includes the lower order terms of all the higher order interactions associated with the average treatments effects of interest.

average treatment effects can also be biased if control observations are not appropriately reweighted to control for differences in the distribution of the **X** variables over regions common to the control and treatment groups.

To give an overview of the sensitivity of our data to this bias, we present in Table 5 some summary statistics on market structure according to the baseline definition of local markets. The table is organized into four Panels A through D. Panel A reports the mean of population size, household income and concentration measures in treated and control areas, where the several columns in Panel A break up, for the treated areas, the averages of the relevant variables in each row for all stores, for the merging retailers (column referred to as insiders), and for the not merging retailers (column outsiders). Panels B, C, and D are also organized as Panel A in terms of columns. Panel B reports summary statistics in terms of store characteristics while Panel C reports summary statistics for the number of products and the number of purchases recorded. Panel D reports, for the pre- and post-period treatment, the average of the mean prices of the selected products sold in the treated and control stores. In the last four rows panel D reports the pure difference in average prices *Difference*, next the row labeled DD corresponds to the average difference-in-differences in the average prices for the treated and control stores over the selected products, while $D_{Insiders}$ is for the insiders (only a before and after difference), and $DD_{Outsiders}$ is for the outsiders and control stores.

When looking at the top three panels, it appears that some characteristics such as the average population, average HHI and store characteristics are different between the treatment and the control groups. This comes in particular from urban areas such as Paris for which the baseline definition of local markets is rather large due to a high density of stores.[23] Turning now to Panel D, we find that the pure differences in average price changes is positive. Outsider prices, for instance, changed more in treated areas than in control areas, by about 49 centimes although the average change in differences does not appear to be statistically significant. As to the insiders prices, they have increased with the merger by 36 centimes, resulting in an average change in market prices of about 37 centimes. We caution though that while these differences in means are suggestive, we are not controlling for any factors that could be happening at the same time in different markets differently and, to deal with this in the empirical strategy, we will perform a pure difference-in-differences panel data estimation strategy as expressed in Eq.(2).

As we discussed, the pure difference-in-differences may be affected if the treatment and control groups differ in the pre-period. Given the differences observed in the top-three panels of Table 5, we perform alternative comparisons for the markets affected by the

[23]Excluding the Paris area ("Ile de France") the ratio of the average population of the treatment group over that of the control group is 3 instead of 11. Note that inequalities in average income which were already weak further decrease. By contrast the difference in HHI between the two groups remains high. To account for the heterogeneity in store density across cities, we investigate alternative definitions of local markets in the robustness section.

Table 5: Summary statistics on treatment and control groups

	Treatment group			Control group
	All stores	Insiders	Outsiders	
A. Local market characteristics				
Average Population (in 1999)	640469	954206	504474	55760
Yearly average income per household	13967	14400	13775	12832
Average HHI	2494	2492	2489	4151
B. Stores characteristics				
Number of stores observed	516	158	356	79
Average store size (in sq meters)	5551	7478	4717	3040
C. Products				
Number of homogenous products	109	107	109	109
Number of purchases recorded	278135	96095	181341	36553
D. Average Prices & Average Difference in Mean Prices				
Pre-merger period (1998–1999)	2975	3086	2934	2980
	(286)	(295)	(284)	(293)
Post-merger period (2001)	3110	3219	3081	3077
	(292)	(310)	(288)	(288)
Difference	135	133	147	98
	(32)	(50)	(35)	(50)
DD			**37** (49)	
$D_{Insiders}$			**36** (63)	
$DD_{Outsiders}$			**49** (47)	

Notes: Panel A, B, C, and D report market, store and purchase records summary statistics for treated and control catchment areas. The several columns break up the averages of the variables in each row, for the treated areas, into the averages for all stores, for the merging retailers (insiders) and for the competitors (outsiders). Panel D reports, for the pre- and post-period treatment, the weighted average of the mean prices, measured in centimes of French Francs and weighted by the share of each product in total expenditures in all stores, of the selected products sold in the treated and control stores and in the last four rows reports the pure difference in average prices *Difference*, the row labeled DD corresponds to the average difference-in-differences for the treated and control stores over the selected products, $D_{Insiders}$ is for the insiders and control stores, and $DD_{Outsiders}$ is for the outsiders and control stores.

merger through semi-parametric matching estimators. More precisely, we conduct two additional estimates using a propensity score matching estimator. Therefore we estimate a probit of a merger occurring in a certain market, where as explanatory variables, we include 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 average income in the local area), and regional dummies. We then estimate the probability of being treated (of a merger occurring) as a function of these variables. The propensity score estimates are reported in Appendix (see column (1) in Table 19).

In a first attempt to control for differences in observed confounding factors between treated and control stores, we apply a re-weighting scheme proposed by Hirano *et al* (2003) and Imbens (2004). 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, 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 markets. Intuitively, this technique up-weights data from markets that were not treated, but had a high probability of having been treated (having a merger occur) based on baseline observable data.

The second approach uses a nearest neighbor matching estimator. For each treated store, we find the more relevant counterfactual based on the propensity scores, i.e. the control store that matches more closely the treated store. Owing to the low numbers of control stores, we search for the closest counterfactual, thus applying a one nearest neighbor matching estimator. Given that the stores in our sample do not necessarily sell the same basket of products, we must keep in mind when comparing the ATE estimates with the ones obtained with the previous methods that the price changes are not calculated from the same sample of products.

5 Results

5.1 Assessing outsiders' price reaction to the merger

We first present the main table of results, Table 6 where the dependent variable for all specifications reported in columns (1)–(7) is the log of price (centimes of Franc) of product j sold at a store i during semester t. The basic structure of Table 6 is to present different estimation strategies in different columns. In column (1), we report the estimates of the average treatment effect on the treated by computing an unconditional difference-in-difference. As this estimate will be biased if factors that could affect prices vary significantly across treated markets and comparison markets columns (2) to (7) report point estimates from different strategies: First, we estimate an ordinary least squares specification of the observed log prices on the treatment variables and also include a set X of observable covariates by product, store and time in columns (2) to (4). The idea is that product fixed effects, store fixed effects and product time fixed effects control for, respectively, product specific constant factors affecting price, store constant factors affecting price, and product semester changing determinants of price, and all these factors are exogenous to the merger, that is, uncorrelated to the treatment. In column (5) of this table we repeat the specification in column (4) but weight each price by the share of each product in total expenditures in all stores, where the weights are computed using the pre merger original dataset only. Finally in columns (6) and (7) we turn to semi-parametric matching estimators of propensity score matching and nearest neighbor matching.

In the difference-in-differences specification results presented in Table 6, the parameter of interest is the one associated with the variable in the row "Merger x Outsider", that isolates the purchases made in stores that did not belong to the merging groups. In

Table 6: DID and DID-matching estimates

Dependent variable: (log) price (by product, by store, by semester)							
	No expend. weights				Expend. weights		
Variable	(1)	(2)	(3)	(4)	(5)	(6) (propensity score)	(7) (one-nearest neighbor)
Merger × Outsider	0.0171***	0.0170***	0.0170***	0.0158***	0.0262***	0.0304**	0.0249
	(0.0061)	(0.0062)	(0.0062)	(0.0054)	(0.0083)	(0.0137)	(0.0181)
log(market income)	0.1465	-0.0384	-0.0529	-0.0340	-0.1016	-0.2184**	-0.3231*
	(0.1284)	(0.0488)	(0.0484)	(0.0535)	(0.0683)	(0.1075)	(0.1783)
Merging firms	-0.0602	0.0274	0.0274	-0.0031	-0.0131	0.0016	0.2174***
	(0.3212)	(0.0454)	(0.0455)	(0.0448)	(0.0683)	(0.0691)	(0.0763)
Constant	5.8328***	7.4526***	9.6447***	9.4980***	10.1393***	11.2494***	12.2301***
	(1.2196)	(0.4658)	(0.4621)	(0.5096)	(0.6508)	(1.0238)	(1.6912)
Store	–	Yes	Yes	Yes	Yes	Yes	Yes
Product	–	–	Yes	–	–	–	–
Product-semester	–	–	–	Yes	Yes	Yes	Yes
Clustered errors	store-pr	store-pr	store-pr	store-pr	store-pr	store-pr	store-pr
R^2	0.008	0.187	0.986	0.987	0.988	0.989	0.989
Observations	24888	24888	24888	24888	24888	24888	10728

Notes: Catchment areas are delimited with the 20/10 km boundaries. The treatment group corresponds to catchment areas where $M1_H$, $M2_H$, $M1_S$, $M2_H$, $M1'$ or $M2'$ operate during 1998 and 1999. The control group corresponds to catchment areas where none of the previous retail chains operate during 1998 and 1999. The year 2000 is omitted. The lower order terms of higher order interactions are not reported due to space limitations but are included in all specifications. Clustered standard errors (at store-product level) are reported.

the row associated to "merging firms" we control for the price changes of the merging retailers and this estimate is only to be interpreted as a before-and-after comparison, because all merging firms are by definition absent from the control group. Standard errors are clustered at the product-store level.

According to the pure difference-in-differences, in column (1), we estimate that the merger has a significant effect on prices on average of about 1.7% for outsider firms in affected markets relative to firms in not affected markets. We note also that merging firms have lowered their prices post merger by an average of about 6 %, although not significantly. However, as discussed above, those estimates could be biased and looking at the R^2 we also see that we explain nothing of the variation in prices with this specification in column (1). When we control in column (2) for store fixed effects, those explain 19% of the variation in observed prices; product fixed effects explain an additional 80% of the variation in prices, corresponding to the change in R^2 from column (2) to column (3). We therefore conclude that most of the variation in prices in the data is cross sectional variation (98%). Among the remaining 2% of the variation, semesters explain very little of the variation in prices, as shown in the barely changing R^2 from column (3) to (4).

The specification that is the most ambitious at controlling for any covariates that could affect prices is presented in column (4) and that is the covariate specification that we use henceforth in all other additional tables. Here we control for store fixed effects as well as for product semester specific changing factors that can affect prices and then, the estimated average effect on prices of outsiders in affected markets relative to non affected markets is 1.5 % and significant.

We note that in specification (4) we control for factors that could have changed semester by semester for each product separately, and when compared to specification

(3) while the explanatory power of the specification did not improve substantially given the very similar R^2, the coefficient of interest drops significantly. In column (3) we are not controlling for anything that could have been changing each semester that would be common to all products, for example if the number of manufacturers for a given product drops say post merger at the national level. We are also not controlling for product specific semester changes such as product specific changes in advertising at the national level that coincided with the post merger periods. Anything that could be changing each semester for each product differently is not controlled for in column (3) and that implies that those changes would be captured in the regression in specification (3) by the merger treatment indicator. Indeed, between columns (3) and (4) the estimated treatment effect is lower as the omitted product specific semester changing factors are captured by the product-semester controls. A final note is that, after controlling for confounding factors in column (4), we have found that the merging firms on average did not significantly change their prices post-merger while outsiders significantly increased their prices.

In column (5) when weighting products by how much they get typically purchased, we find again a significant and positive average effect on outsider's prices of about 2.6 %. When compared to the smaller effect of column (4), it appears that price increases at outsiders' stores seem to have affected mostly products with a high turnover (i.e., with high expenditure share).

When using non parametric strategies we find similar results to those in columns (4) and (5). In column (6) we report an average price increase for outsiders of a significant 3% while merging firms do not change their prices. Finally, when focusing on the nearest neighbor estimates, the outsider effects are in the magnitude of the other approaches, namely of about 2.5%, but are no longer significant, probably due to lack of power as we lose a lot of observations when focusing on the nearest neighbor.

While in Table 6 results suggest that the merger did on average have competitive effects, the next analyses aim at finding empirical evidence consistent with different economic forces behind the competitor outsider positive price effects estimated.

5.2 Investigating different sources of price variations

As the next step we take advantage of the pre-merger configuration of the markets to investigate possible sources of price changes of the outsiders.

Market share effect First, the merger affects competition by suppressing a competitor, thus possibly affecting all firms' market shares. For instance, Fig.(2) illustrates the case where a market, represented by a dark blue circle, is affected by the merger and where the number of competitors changes after the merger. Indeed, prior to the merger, stores $M1$ and $M2$ were competitors and constitute distinct rivals for O_2. Once the merger was

Table 7: Market Share Effect estimates

Dependent variable: (log) price (by product, by store, by semester)						
	No expend. weights			Expend. weights		
Variable	Pure DID	DID-Matching (propensity score)	DID-matching (one-nearest neighbor)	Pure DID	DID-Matching (propensity score)	DID-matching (one-nearest neighbor)
Merger × Outsider × MSE	0.0237***	0.0222***	0.0185	0.0338***	0.0372***	0.0335*
	(0.0060)	(0.0084)	(0.0115)	(0.0088)	(0.0135)	(0.0179)
Merger × Outsider × No MSE	0.0079	0.0063	0.0022	0.0176*	0.0225	0.0210
	(0.0061)	(0.0089)	(0.0114)	(0.0093)	(0.0151)	(0.0191)
Merging firms	-0.0036	0.0078	0.0919	-0.0135	0.0013	0.2171***
	(0.0449)	(0.0462)	(0.0824)	(0.0683)	(0.0691)	(0.0765)
log(market income)	-0.0328	-0.0754	-0.0900	-0.0995	-0.2173**	-0.3211*
	(0.0535)	(0.0650)	(0.1021)	(0.0683)	(0.1076)	(0.1786)
Constant	9.4864***	9.8875***	10.2151***	10.1201***	11.2387***	12.5003***
	(0.5101)	(0.6192)	(0.9751)	(0.6507)	(1.0250)	(1.7011)
Store	Yes	Yes	Yes	Yes	Yes	Yes
Product-semester	Yes	Yes	Yes	Yes	Yes	Yes
Clustered errors	store-pr	store-pr	store-pr	store-pr	store-pr	store-pr
R^2	0.987	0.988	0.988	0.988	0.989	0.989
Observations	24888	24888	10728	24888	24888	10728

Notes: Catchment areas are delimited with the 20/10 km boundaries. The treatment group corresponds to catchment areas where $M1_H$, $M2_H$, $M1_S$, $M2_H$, $M1'$ or $M2'$ operate during 1998 and 1999. The control group corresponds to catchment areas where none of the previous retail chains operate during 1998 and 1999. The year 2000 is omitted. The lower order effects of the merger are not reported. Clustered standard errors (at store-product level) are reported.

consummed, the new entity $M1 + M2$ consolidates their market shares and internalize the price decision of each of them. Consequently, this "market share effect", henceforth denoted *MSE*, increases their market power, leading potentially to higher prices. For the outsiders, the increase in the concentration level may also induce to relax the price competition. In order to quantify the price merger effect resulting from an increase in the concentration level in local markets, we focus on the MSE and break the effect of the merger on the outsiders' prices whether being in a treated market with a MSE or without a MSE.

Table 7 displays the OLS regressions conducted with this new specification. According to the pure difference-in-differences estimator (column 1), we show that the merger has a significant effect on outsiders' prices when they operate in markets where a MSE occurs. The estimated coefficient is statistically significant and reveals a price increase from outsiders of about 2.4% in these markets. The results are unchanged in column (2) when using the propensity score matching method. When reweighting observations by expenditures shares, the effect of the merger on outsiders' prices in markets with MSE is still statistically significant and raised up to more than 3% in column (4) and (5). By contrast, we note that the merger does not change significantly the price decision of outsiders locating in markets absent of a MSE compared to outsiders of the control group. This pattern is consistent with the hypothesis that outsiders change their pricing policy mainly due to the increased concentration.

Differentiation effect Second, recall that with the merger, two of the chains have changed their names, $M2_H$ rebranded into $M1_H$ and $M1_S$ into $M2_S$ as can be seen in Fig.(3). Rebranding the stores may affect demand. For instance in some areas, due

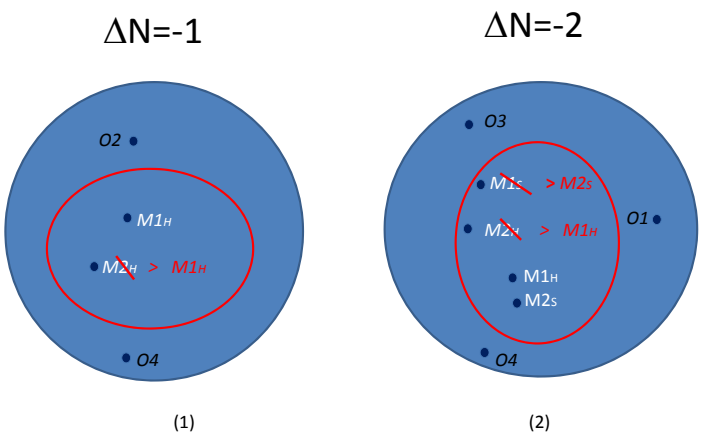

Figure 3: Drop in the number of retail chains

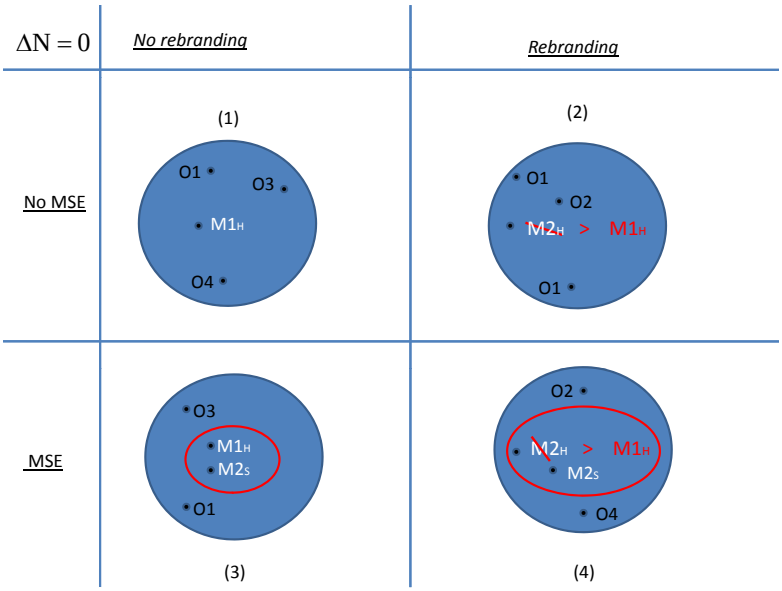

Figure 4: Market share and rebranding effects when $\Delta N = 0$.

Table 8: Differentiation effect estimates

Dependent variable: (log) price (by product, by store, by semester)						
	No expend. weights			Expend. weights		
Variable	Pure DID	DID-Matching (propensity score)	DID-matching (one-nearest neighbor)	Pure DID	DID-Matching (propensity score)	DID-matching (one-nearest neighbor)
Merger × Outsider × $\Delta N = -2$	0.0316***	0.0292***	0.0163	0.0413***	0.0441***	0.0255
	(0.0083)	(0.0100)	(0.0144)	(0.0107)	(0.0143)	(0.0193)
Merger × Outsider × $\Delta N = -1$	0.0146**	0.0140	0.0237	0.0245**	0.0284*	0.0440*
	(0.0073)	(0.0095)	(0.0147)	(0.0107)	(0.0150)	(0.0226)
Merger × Outsider × $\Delta N = 0$	0.0121**	0.0105	0.0044	0.0224**	0.0270*	0.0223
	(0.0058)	(0.0086)	(0.0111)	(0.0089)	(0.0146)	(0.0185)
Merging firms	-0.0032	0.0081	0.0912	-0.0131	0.0016	0.2164***
	(0.0448)	(0.0462)	(0.0825)	(0.0683)	(0.0691)	(0.0766)
log(market income)	-0.0339	-0.0756	-0.0867	-0.1002	-0.2177**	-0.3182*
	(0.0533)	(0.0650)	(0.1021)	(0.0681)	(0.1076)	(0.1786)
Constant	9.4970***	9.8890***	8.1577***	10.1263***	11.2423***	12.4726***
	(0.5082)	(0.6186)	(0.9763)	(0.6492)	(1.0252)	(1.7010)
Store	Yes	Yes	Yes	Yes	Yes	Yes
Product-semester	Yes	Yes	Yes	Yes	Yes	Yes
Clustered errors	store-pr	store-pr	store-pr	store-pr	store-pr	store-pr
R^2	0.987	0.988	0.988	0.988	0.989	0.989
Observations	24888	24888	10728	24888	24888	10728

Notes: Catchment areas are delimited with the 20/10 km boundaries. The treatment group corresponds to catchment areas where $M1_H$, $M2_H$, $M1_S$, $M2_H$, $M1'$ or $M2'$ operate during 1998 and 1999. The control group corresponds to catchment areas where none of the previous retail chains operate during 1998 and 1999. The year 2000 is omitted. The lower order effects of the merger are not reported. The lower order terms of higher order interactions are not reported due to space limitations but are included in all specifications. Clustered standard errors (at store-product level) are reported.

to rebranding, the number of retail brands may have dropped implying a reduction in retail differentiation. We wish to isolate this differentiation effect and we thus investigate evidence consistent with estimating significant different price changes of outsiders that face different changes in the number of retail brands in Table 8. There we use the ΔN sign to characterize such a number of retail brand change, as illustrated in Fig.(3):"$\Delta N = 0$" means that the total number of retail brands is unchanged (even if some stores changed names) while an observed drop of one or (at most) two in the number of chains is denoted by "$\Delta N = -1$" , as in case (1), and "$\Delta N = -2$", as in case (2), respectively.

Table 8 is organized as Table 6. While from Table 6 we estimate in the pure difference-in-differences that outsiders' prices increased on average by about 1.7 %, as we see in column (1) of Table 8 outsiders' prices increased more when their catchment area was affected post merger by a larger drop in the number of competing retailers. When "$\Delta N = -2$" prices increased by 3.2%, when "$\Delta N = -1$" prices increased by 1.5%, and the smallest price increase estimates of 1.2% appears for the case "$\Delta N = 0$". The results are similar when we use semi-parametric estimations, and also similar if we repeat this investigation weighting each observation by the pre period expenditure shares.[24] The estimates reported in Table 8 suggest though that changes in the number of retail brands competing with outsiders due to merger are not the only force at play: Indeed, we do still find outsiders to increase their prices by 1.2% even when "$\Delta N = 0$".

[24]Once again, the nearest neighbor results become insignificant possibly due to lack of power. When investigating heterogeneous effects across catchment areas we found no significant differences by the level of competition in the pre merger period measured by the HHI and also we found no significant heterogeneous price effects depending on whether markets have a nearby hypermarket.

Table 9: MSE, Differentiation and Pure rebranding effects estimates

Dependent variable: (log) price (by product, by store, by semester)						
	No expend. weights			Expend. weights		
Variable	Pure DID	DID-Matching (propensity score)	DID-matching (one-nearest neighbor)	Pure DID	DID-Matching (propensity score)	DID-matching (one-nearest neighbor)
Rebranding						
$\Delta N = -2$	0.0319***	0.0294***	0.0164	0.0415***	0.0442***	0.0254
	(0.0083)	(0.0100)	(0.0144)	(0.0107)	(0.0143)	(0.0193)
$\Delta N = -1$	0.0149**	0.0141	0.0238	0.0247**	0.0285*	0.0440*
	(0.0073)	(0.0095)	(0.0147)	(0.0107)	(0.0150)	(0.0226)
$\Delta N = 0$, MSE	0.0181	0.0165	0.0081	0.0285*	0.0321	0.0231
	(0.0122)	(0.0135)	(0.0132)	(0.0166)	(0.0199)	(0.0202)
$\Delta N = 0$, No MSE	0.0093	0.0075	0.0029	0.0209**	0.0249	0.0202
	(0.0070)	(0.0093)	(0.0117)	(0.0103)	(0.0152)	(0.0191)
No Rebranding						
$\Delta N = 0$, No MSE	0.0065	0.0049	0.0016	0.0139	0.0197	0.0217
	(0.0077)	(0.0103)	(0.0130)	(0.0114)	(0.0172)	(0.0212)
$\Delta N = 0$, MSE	0.0296***	0.0282**	0.0209	0.0406***	0.0442***	0.0331
	(0.0098)	(0.0114)	(0.0183)	(0.0132)	(0.0167)	(0.0245)
Merging firms	-0.0036	0.0078	0.0913	-0.0137	0.0011	0.2166***
	(0.0449)	(0.0462)	(0.0826)	(0.0683)	(0.0691)	(0.0766)
log(market income)	-0.0368	-0.0783	-0.0881	-0.1005	-0.2185**	-0.3201*
	(0.0534)	(0.0650)	(0.1020)	(0.0680)	(0.1080)	(0.1787)
Constant	9.5249***	9.9150***	9.9947***	10.1289***	11.2507***	12.2026***
	(0.5086)	(0.6194)	(0.9676)	(0.6482)	(1.0283)	(1.6949)
Store	Yes	Yes	Yes	Yes	Yes	Yes
Product-semester	Yes	Yes	Yes	Yes	Yes	Yes
Clustered errors	store-pr	store-pr	store-pr	store-pr	store-pr	store-pr
R^2	0.987	0.988	0.988	0.988	0.989	0.989
Observations	24888	24888	10728	24888	24888	10728

Notes: Catchment areas are delimited with the 20/10 km boundaries. The treatment group corresponds to catchment areas where $M1_H$, $M2_H$, $M1_S$, $M2_H$, $M1'$ or $M2'$ operate during 1998 and 1999. The control group corresponds to catchment areas where none of the previous retail chains operate during 1998 and 1999. The year 2000 is omitted. The lower order effects of the merger are not reported. Clustered standard errors (at store-product level) are reported.

Pure rebranding effect However, although treated outsiders who face a drop in the number of retail brands in their catchment area are necessarily competing with a store that rebrands after the merger, the opposite is not true. A catchment area where rebranding occurs but where $\Delta N = 0$ is illustrated in cases (2) and (4) in Fig.(4). Note also that in case (4) in Fig.(4) a market share effect is combined with the rebranding effect. However, "rebranding" in disrupting consumers habits by the change in the fascia, as well as by any slight modification that can follow in a store's organization may affect outsiders who face a rebranded store in their catchment area. Indeed, these outsiders may thus gain new customers disappointed by the changes, or lose some customers wishing to change. We thus wish to isolate a "pure rebranding effect" corresponding to case (2) in Fig.(4). Note that, in the same spirit, the case (3) illustrates a pure market share effect without rebranding. The next Table 9 aims at decomposing further all these effects. The two main factors that appear to explain the outsider's price raise in treated relative to controls are the pure market share effect and a drop in the number of retail brands. In contrast, the pure rebranding effect does not have a significant impact on outsiders' prices in the treated. These effects are robust using the propensity score matching method in column (2) and when reweighting with the expenditure shares in column (4) and (5).

Table 10: Out-of-market effect estimates

Dependent variable: (log) price (by product, by store, by semester)						
	No expend. weights			Expend. weights		
Variable	Pure DID	DID-Matching (propensity score)	DID-matching (one-nearest neighbor)	Pure DID	DID-Matching (propensity score)	DID-matching (one-nearest neighbor)
Merger × Outsider	0.0137	0.0119	0.0104	0.0115	0.0060	0.0062
	(0.0089)	(0.0100)	(0.0105)	(0.0123)	(0.0136)	(0.0138)
Merger firms	-0.0120	-0.0156	0.0231	-0.0045	-0.0117	0.0460
	(0.0172)	(0.0174)	(0.0232)	(0.0204)	(0.0208)	(0.0419)
log(market income)	-0.0358	-0.0302	-0.2619	-0.1049	-0.1374	-0.3560*
	(0.0869)	(0.1099)	(0.1737)	(0.0968)	(0.1213)	(0.2128)
Constant	7.1287***	7.0718***	9.1453***	7.7615***	8.0560***	10.1859***
	(0.8041)	(1.0164)	(1.6033)	(0.8966)	(1.1228)	(2.0244)
Store	Yes	Yes	Yes	Yes	Yes	Yes
Product-semester	Yes	Yes	Yes	Yes	Yes	Yes
Clustered errors	store-pr	store-pr	store-pr	store-pr	store-pr	store-pr
R^2	0.988	0.989	0.989	0.988	0.989	0.990
Observations	4908	4908	3054	4908	4908	3054

Notes: Catchment areas are delimited with the 20/10 Km boundaries. The treatment group corresponds to catchment areas where there is no market share effect and rebranding during 1998 and 1999 (i.e., $M1_H$, $M2_S$, $M1'$ and $M2'$). The control group corresponds to the baseline control group. The year 2000 is omitted. The lower order effects of the merger are not reported. Clustered standard errors (at store-product level) are reported.

Out-of-market effect Previously, we have emphasized that outsiders have reacted to the merger by raising their prices. Precisely, our results demonstrate that outsiders have raised their prices following a change in their market structure generated by one of the two effects (i.e., MSE, differentiation). In what follows, we push our analysis a little further by investigating the possibility that the merger triggered outsiders' price variation absent any of these market structure changes.[25] We adopt another definition of the treated group by limiting the definition of a treated store to stores not confronted to any change in their market structure (like case (1) in Fig.(4)), i.e. with no MSE nor variation in the number of chains of the merging groups nor rebranding. Looking at Table 9, the definition of the treatment group corresponds to the row "No rebranding, $\Delta N = 0$, No MSE". Consequently, if prices rise in the treatment group compared to the control group this suggests that other anti-competitive effects not related to local market structure are at play. For instance, outsiders may have wrong anticipations about the intention of the merging groups to raise their prices and in turn react in the short term to these wrong anticipations by raising their price: we refer to such effect as to a "reputation effect".

The DID estimates are reported in Table 10. For each of the six regressions conducted, we observe that none of them give a coefficient "Merger x Outsider" statistically significant. These results suggest that absent any change in the local competitive environment, the merger has not influenced, in the short term, the price decision of the outsiders. In particular, this result allows to eliminate the hypothesis of a "reputation effect" defined above in the months following the merger.

[25] In their analysis of banking mergers, Focarelli and Panetta (2003) have coined the term "out-of-market effect" that we borrow here. However, we apply a extended definition of this effect by not limiting us to a simple market share effect.

Table 11: Alternative definition of catchment areas

Dependent variable: (log) price (by product, by store, by semester)						
	No expend. weights			Expend. weights		
Variable	Pure DID	DID-Matching (propensity score)	DID-matching (one-nearest neighbor)	Pure DID	DID-Matching (propensity score)	DID-matching (one-nearest neighbor)
Specification 1: 30km/15km (nb of treated stores=556, nb of control stores=44)						
Merger × Outsider	0.0071	-0.0030	-0.0189**	0.0094	-0.0012	-0.0206*
	(0.0062)	(0.0091)	(0.0087)	(0.0090)	(0.0132)	(0.0122)
Merging firms	-0.0074	0.0027	-0.0353**	-0.0031	0.0106	-0.0333*
	(0.0291)	(0.0300)	(0.0154)	(0.0424)	(0.0427)	(0.0194)
Specification 2: 20km/10km (original, nb of treated stores=516, nb of control stores=79)						
Merger × Outsider	0.0158***	0.0144*	0.0070	0.0262***	0.0304**	0.0249
	(0.0054)	(0.0082)	(0.0108)	(0.0083)	(0.0137)	(0.0181)
Merging firms	-0.0031	0.0083	0.0932	-0.0131	0.0016	0.2174***
	(0.0448)	(0.0462)	(0.0820)	(0.0683)	(0.0691)	(0.0763)
Specification 3: 10km/5km (nb of treated stores=441, nb of control stores=144)						
Merger × Outsider	0.0182***	0.0250***	0.0341***	0.0260***	0.0378***	0.0568***
	(0.0050)	(0.0072)	(0.0125)	(0.0071)	(0.0115)	(0.0218)
Merging firms	-0.0517**	-0.0444**	-0.0015	-0.0448**	-0.0350**	0.0103
	(0.0219)	(0.0210)	(0.0219)	(0.0188)	(0.0152)	(0.0342)
Specification 4: 20km/10km/5km (nb of treated stores=511, nb of control stores=83)						
Merger × Outsider	0.0159***	0.0099	0.0067	0.0260***	0.0242	0.0194
	(0.0053)	(0.0088)	(0.0086)	(0.0081)	(0.0151)	(0.0132)
Merging firms	-0.0031	0.0114	0.0852	-0.0132	0.0091	0.1989***
	(0.0448)	(0.0459)	(0.0850)	(0.0683)	(0.0696)	(0.0768)
Store	Yes	Yes	Yes	Yes	Yes	Yes
Product-semester	Yes	Yes	Yes	Yes	Yes	Yes
Clustered errors	store-pr	store-pr	store-pr	store-pr	store-pr	store-pr

Notes: Specification 1 corresponds to catchment areas delimited with the 30/15 Km boundaries. Specification 2 corresponds to catchment areas delimited with the 20/10 Km boundaries. Specification 3 corresponds to catchment areas delimited with the 10/5 Km boundaries. Specification 4 corresponds to catchment areas delimited with the 20/10/5 Km boundaries. Catchment areas are delimited with the 20/10 km boundaries. The treatment group corresponds to catchment areas where $M1_H$, $M2_H$, $M1_S$, $M2_H$, $M1'$ or $M2'$ operate during 1998 and 1999. The control group corresponds to catchment areas where none of the previous retail chains operate during 1998 and 1999. The year 2000 is omitted. The lower order effects of the merger are not reported. Clustered standard errors (at store-product level) are reported.

5.3 Robustness Checks

We next perform a series of robustness checks for our findings. First we turn to alternative definitions of catchment areas and re-run our main specifications to assess the robustness of results to market definition and results are presented in Table 11. The dependent variable in Table 11 is the log of price (cents of Euros) of product j sold at a store i during semester t. The table has 6 columns, where in columns (1) to (3) we do not weigh observations by the expenditure weights, whereas in columns (4) to (6) we do weigh by the expenditure share. Columns (1) and (4) reports the difference-in-differences estimates controlling for store fixed effects as well as for product semester specific fixed effects. Columns (2) and (5) shows the propensity score estimates and (3) and (6) the nearest neighbor estimates. The results are robust to decreasing the size of the catchment area, as the point estimates yield very similar price effects on average.

One main issue is to whether the merger could have been discussed and anticipated and thus price effects could have occurred even in the pre periods. We assume an announcement of the merger at the end of the second semester in 1998, then first semester in 1999, also second semester in 1999, and finally in the first semester in 2001. Results can be seen in Table 12 that indeed merger effects happened much sooner as the outsider

Table 12: Placebos in Time

Dependent variable: (log) price (by product, by store, by semester)						
	No expend. weights			Expend. weights		
Variable	Pure DID	DID-Matching (propensity score)	DID-matching (one-nearest neighbor)	Pure DID	DID-Matching (propensity score)	DID-matching (one-nearest neighbor)
Specification 1: Merger in 1998 (2nd semester)						
Merger × Outsider	0.0096	0.0015	0.0047	0.0205**	0.0090	0.0138
	(0.0061)	(0.0076)	(0.0090)	(0.0097)	(0.0119)	(0.0152)
Merging firms	-0.0024	0.0060	-0.0047	-0.0100	0.0014	0.0027
	(0.0143)	(0.0159)	(0.0201)	(0.0213)	(0.0241)	(0.0325)
Specification 2: Merger in 1999 (1st semester)						
Merger × Outsider	0.0204***	0.0127**	0.0140*	0.0335***	0.0246**	0.0301**
	(0.0051)	(0.0062)	(0.0078)	(0.0081)	(0.0102)	(0.0138)
Merging firms	-0.0162	-0.0089	0.0063	-0.0330	-0.0223	0.0333
	(0.0243)	(0.0250)	(0.0163)	(0.0349)	(0.0343)	(0.0260)
Specification 3: Merger in 1999 (2nd semester)						
Merger × Outsider	0.0178***	0.0163***	0.0148**	0.0285***	0.0287***	0.0316**
	(0.0047)	(0.0063)	(0.0074)	(0.0076)	(0.0106)	(0.0123)
Merging firms	-0.0095	-0.0049	0.0145	-0.0290	-0.0258	0.0430*
	(0.0329)	(0.0355)	(0.0171)	(0.0437)	(0.0455)	(0.0258)
Specification 4: Merger in 2001 (1st semester)						
Merger × Outsider	0.0158***	0.0170**	0.0042	0.0181**	0.0220*	0.0017
	(0.0061)	(0.0079)	(0.0100)	(0.0091)	(0.0127)	(0.0158)
Merging firms	-0.0345	-0.0296	-0.0163	-0.0559	-0.0514	-0.0118
	(0.0286)	(0.0296)	(0.0200)	(0.0407)	(0.0408)	(0.0321)
Store	Yes	Yes	Yes	Yes	Yes	Yes
Product-semester	Yes	Yes	Yes	Yes	Yes	Yes
Clustered errors	store-pr	store-pr	store-pr	store-pr	store-pr	store-pr

Notes: Catchment areas are delimited with the 20/10 km boundaries. The treatment group corresponds to catchment areas where $M1_H$, $M2_H$, $M1_S$, $M2_H$, $M1'$ or $M2'$ operate during 1998 and 1999. The control group corresponds to catchment areas where none of the previous retail chains operate during 1998 and 1999. The year 2000 is omitted. The lower order effects of the merger are not reported. Clustered standard errors (at store-product level) are reported.

responses are positive and significantly different from zero for the difference-in-differences and matching specifications for all pre period placebos, and the magnitudes are similar to those using the actual merger effective date as treatment period definition. So we were actually underestimating the actual effects of the merger using effective date as the treatment date, due to the significant anticipation effects found in the pre period.

Finally here we test the sensitivity of our results to the cut-offs retained for our selection variables when defining the sample of products. We report in Table 13 the estimates when varying these cut-offs for the same specification of that of colum (4) in Table 6 and we observe that results are sensitive to these constraints but the average price effect is in the same order of magnitude for he outsiders' price changes of between one and two percent.

6 Conclusion

In this paper we take advantage of nationally decided retailer merger that impacted local markets differentially depending on the pre existing set of retail competition to estimate the effect of a merger of two competing retailers on the prices of its competitors. We find that prices of competing firms in areas where the merger occurred (treated

Table 13: DID estimates (Constraints)

Dependent variable: (log) price (by product, by store, by semester)									
	Products sold at least in				Stores that sell at least				
	15 stores	10 stores	5 stores	2 stores	10 prod.	5 prod.	3 products	2 prod.	No const.
Variable		(original)					(original)		
Merger × Outsider	0.0192***	0.0158***	0.0145***	0.0115**	0.0162**	0.0182***	0.0158***	0.0110**	0.0105**
	(0.0058)	(0.0054)	(0.0050)	(0.0048)	(0.0082)	(0.0058)	(0.0054)	(0.0053)	(0.0050)
Merging firms	-0.0074	-0.0031	0.0540	0.0546	-0.0202**	-0.0095	-0.0031	-0.0071	-0.0046
	(0.0066)	(0.0448)	(0.0635)	(0.0640)	(0.0092)	(0.0066)	(0.0448)	(0.0361)	(0.0359)
R^2	0.988	0.988	0.988	0.988	0.987	0.987	0.988	0.988	0.989
Observations	22758	24888	27456	29214	10758	19512	24888	27954	30480

Notes: Catchment areas are delimited with the 20/10 km boundaries. The treatment group corresponds to catchment areas $M1_H$, $M2_H$, $M1_S$, $M2_H$, $M1'$ or $M2'$ operate during 1998 and 1999. The control group corresponds to catchment areas where none of the previous retail chains operate during 1998 and 1999. The year 2000 is omitted. The lower order effects of the merger are not reported. Clustered standard errors (at store-product level) are reported.

group) increased significantly relative to the control areas where existing firms were not affected by a merger. In fact, our findings suggest that the merger significantly raised the competitors' prices, while at the same time not being correlated with any significant effect on prices of the merging firms. These results are consistent with a combination of efficiency gains for the merging firms (as their prices did not increase) and possible coordinated effects or a decrease in differentiation (as competing prices increased). We are not only able to estimate the average price response of retailers when faced with the merger relative to a counterfactual of retailers not facing a merger, but we are also able to investigate possible economic forces behind the price responses we see. We break up the global raise in outsider's price effect into a market share effect, a pure rebranding effect and a differentiation effect. The market share effect appears whenever a treated outsider faces a variation of concentration (HHI) in its market. Our main result is that indeed, a change in local concentration explains a large part of the treated outsider's price raise. Second, we were able to identify a differentiation effect which appears as a result of rebranding and imply a drop in the total number of retail brands in a treated market. In that case, outsiders are facing a decrease in retail differentiation and we show again that this effect explains part of the outsider's price rise in the treated. Finally we have isolated a pure rebranding effect, which appears in markets where one of the merging firm rebrands after the merger, but where no store of the other merging group operates (to avoid any market share effect), and where no store of this new brand was operating before the merger (to avoid a resulting drop in the notal number of chain brands). In contrast, this pure rebranding effect does not explain significantly the treated outsider's price increase.

Theory can bring support to our empirical findings. When taking into account only the market share effect, we may fail to find a convincing theoretical framework where, after a merger between two firms, the merging firms would not raise their prices but their rival would do so. Consider for instance a Salop competition model (e.g Levy and Reitzes (1992)) where retailers are located around a circle and consumers are uniformly located along the circle and incur transportation costs related to their distance to reach one or another store. When two neighbor retailers merge, competition on the segment between them disappears so that they may increase their prices but outsiders reaction function is unchanged; In that model, the price variation of the outsiders is thus only due to their reaction to the insider's price variation. If the insiders decrease (or do not change) their prices, for instance as a result of efficiency gains, the outsiders would also decrease (or not change) their prices. Only a raise in the insiders' prices could trigger an increase in the outsiders' prices.

However, combining the market share effect and the differentiation effect that both show up in our DID analysis, this theoretical framework can now be relevant. To illustrate the drop in the number of retail brands after the merger which we have associated to a

decrease in retail differentiation, we now consider that stores can relocate along the Salop circle. If now the merging firms relocate after the merger by reducing the differentiation between the insiders (not geographically, but in terms of consumer preferences for the brands), then the outsiders will benefit from the ensuing reduction in competition, their reaction function change, and they further increase their prices: in the limit, if the two merging firm relocate on the same place, in the Salop example this may trigger a price increase by all competitors even though the merging firms decrease or do not change their prices due to efficiency gains.[26]

Bibliography

Allain, M.-L., and C. Chambolle (2011) "Anticompetitive Effects of Resale-Below-Cost Laws", *International Journal of Industrial Organization*, 29 (4), 373-385.

Allain, M.-L., C. Chambolle and T. Vergé (2008) "La loi Galland sur les relations commerciales : Jusqu'où la réformer ?", Opuscule du CEPREMAP, ED. de la Rue déUlm.

Angrist, J. and S. Pischke (2010), "The Credibility Revolution in Empirical Economics, How better Research design is Taking the Con out of Econometrics", *Journal of Economic Perspective*, 24(2),3-30.

Ashenfelter, O., D. Ashmore, J. Baker, S. Gleason, and D. Hosken, (2006), "Empirical Methods in Merger Analysis: Econometric Analysis of Pricing in FTC vs. Staples," *International Journal of the Economics of Business*, 13(2), 265-279.

Ashenfelter, O., D. Hosken, and M.C. Weinberg (2011), "The Price Effects of a Large Merger of Manufacturers: A Case Study of Maytag-Whirlpool, *Working Paper 17476*, http://www.nber.org/papers/w17476.

Asplund, M. and R. Friberg (2002) "Food prices and market structure in Sweden", *The Scandinavian Journal of Economics*, 104 (4), 547-566.

Autorité de la Concurrence, Avis 12-A-01 du 11 janvier 2012 relatif à la situation concurrentielle dans le secteur de la distribution alimentaire à Paris.

Autorité de la Concurrence, Avis 07-A-12 du 11 octobre 2007 relatif à la législation sur lÕéquipement commercial.

Barros, P. D. Brito, and D. De Lucena. (2006), "Mergers in the Food retailing sector: An empirical investigation", *European Economic Review*, 50,447-468 .

Bertrand, M. and F. Kramartz (2002) "Does Entry Regulation Hinder Job Creation? Evidence From The French Retail Industry," *The Quarterly Journal of Economics*, MIT Press, vol. 117(4), pages 1369-1413, November.

[26]Note that in our analysis, we have ruled out most of the efficiency gains that would probably appear in a longer run after the merger. However, merging firms may have enjoyed in the short run some buyer power gains. A direct illustration are the wedding gifts that were paid by several suppliers to the merging firms in 2000 in order to secure their future relationship with the new group. Several newspapers articles had denounced this practice at that time.

Biscourp, Boutin and Vergé (2012) "The Effects of Retail Regulations on Prices: Evidence from the Loi Galland", Working Paper DESE/INSEE, G2008-02.

Canivet (2004) "Rapport du groupe d'experts constitué sur les rapports entre industrie et commerce", Cour de Cassation.

Choné, P. and L. Linnemer (2010), "A treatment effect method for merger analysis with an application to parking prices in Paris", CREST Working paper 2010-48.

Commission Hagelsteen (2008), "La négociabilité des tarifs et des conditions générales de vente", Report for the French Minister of Economics.

Conseil de la Concurrence (1997), Avis 97-A-04 du 21 janvier 1997 relatif à diverses questions portant sur la concentration de la distribution.

Cotterill, R.W. (1986), " Market Power in the Retail Food Industry : Evidence From Vermont", *Review of Economics and Statistics*, 68, 379-386.

Cotterill and Putsis (2000), "Market Share and Price Setting Behavior for Private Labels and National Brands", *Review of Industrial Organization*, 17:1, August 2000, p. 17-39.

Davis, D. (2010), "Price Effects of Supermarket Mergers", *Journal of Agricultural & Food Industrial Organization*, 8(1), Article 8.

Dobson P. and M. Waterson (1997), "Countervailing Power and Consumer Prices", *Economic Journal*, 07(4), 418-30.

Dobson, P. and M. Waterson (2005), "Chain-Store pricing across local markets", *Journal of Economics and Management Strategy*, 14-1, 93-119)

Dziuda and Mastrobuoni, "The Euro Changeover and Its Effects on Price Transparency and Inflation", *Journal of Money, Credit, and Banking* 41 (2009): 101-129

Einarsson, A.

Focarelli, D. and F. Panetta (2003), "Are mergers beneficial to consumers? Evidence from the market for bank deposits", *American Economic Review*, 93, 4, p 1152-1172.

Galbraith, J. K. (1952), "American Capitalism: the Concept of Countervailing Power", Houghton Mifflin, Boston MA.

Gandhi A., Froeb L., Tschant S. and G.J Werden (2008), "Post-Merger Product Repositioning", *The Journal of Industrial Economics*, LVI, 1, 49-67.

Hastings, J. S.(2004), "Vertical Relationships and Competition in Retail Gasoline Markets: Empirical Evidence from Contract Changes in Southern California", *The American Economic Review*, 94, 1, pp. 317-328.

Hasting, J. S and R. Gilbert (2005), "Market Power, Vertical Integration and the Wholesale price of Gazoline", *The Journal of Industrial Economics*, 53 (4), 469-492.

K. Hirano, G.W. Imbens, G. Ridder, Efficient estimation of average treatment effects using the estimated propensity score, Econometrica, 71 (2003), pp. 1161-1189.

Houde (2011) "Spatial differentiation and vertical mergers in retail markets for gasoline", *The American Economic Review*, forthcoming.

G.W. Imbens, Nonparametric estimation of average treatment effects under exogeneity: a review, *Review of Economics and Statistics*, 86 (2004), pp. 4-29.

D. T. Levy and J. D. Reitzes (1992), "Anticompetitive Effects of Mergers in Markets with Localized Competition", *Journal of Law, Economics, & Organization*, 8, 2, pp. 427-440.

Marion, B. and C. Mazo (1998) "Competition in grocery retailing: The impact of a new strategic group on BLS price increases", *Review of Industrial Organization* 13, 381 – 399.

McAfee P. and Williams M.A. (1992) "Horizontal Mergers and Antitrust Policy", *Journalof Industrial Economics*, 40(2): pp. 1 81-7.

Nevo (2000) "Mergers with Differentiated Products: The Case of the Ready-to-Eat Cereal Industry," *The RAND Journal of Economics*, 31(3), 395-421, 2000.

Nevo, A. and M.D. Whinston (2010), "Taking the Dogma out of Econometrics: Structural Modeling and Credible Inference", *Journal of Economic Perspectives*, 24, 2, 69-82.

Newmark, C.M. (1990) "A new test of the price-concentration relationship in grocery retailing" *EconomicsLetters* 33, 369 – 373.

Perry M. and Porter R.H. (1985) "Oligopoly and the Incentive for Horizontal Merger",*American Economic Review*, 75(1): pp. 219-27.

Salant S.W., Switzer S. and Reynolds R.J. (1983) "Losses from Horizontal Merger: the Effects of an Exogenous Change in Industry Structure on Cournot-Nash Equilibrium",*Quarterly Journal of Economics*, 93: pp. 1 85-99.

Smith, H. (2004), "Supermarket Choice and Supermarket Competition in Market Equilibrium", *Review of Economic Studies*, 71, 235 – 263.

Snyder, C. (1996) "A Dynamic theory of coutervailing power", *RAND Journal of Economics*, 27 (4) 747 – 769.

Taylor, C.T, and D.S. Hosken (2007), "The economic effects of the Marathon-Ashland joint venture: The importance of Industry Supply Shocks and vertical market Structure", *The Journal of Industrial Economics*, 0022-1821, LV, 3.

Turolla (2012) "Spatial competition in the French supermarket industry", Working Paper SMART-LERECO N.12-01.

von Ungern-Sternberg, T. (1996), "Countervailing Power Revisited", *International Journal of Industrial Organization*, 4(4): pp. 507-519.

Weinberg, M. (2008), "The price effects of horizontal mergers", *Journal of Competition Law & Economics*, 4(2), 433-447.

Appendix

A Data

This appendix provides detailed information on the construction of the final dataset.

The databases Three data sources are used in this study. The most important comes from the TNS Worldpanel database available at INRA. The TNS Sofres company (now part of the Kantar group) is recognized as the national leader in consumers panels. The TNS Worldpanel contains detailed information on all the food purchases realized by a panel of French households. For the purpose of the study, we use the data that cover the years 1998, 1999, 2000 and 2001. The panel is constituted of approximately 11,000 households per year representative of the French population. Every year one quarter of the panel is renewed. The data are collected following the home-scan technique, i.e. after each shopping trip, the household records all the food purchases it brings at home by scanning the product's barcode and fill in by hand the quantity purchased, the price paid and the store type where the purchase was made. For packaged products, the attributes of the product are collected from the barcode which made available a rich set of information at the product level. Hence, a product can be defined by up to 15 descriptive variables (such as flavour, container, nutritional characteristics, for instance), plus the brand name and the name of the manufacturer.[27] Otherwise, for products without barcode (often called random weight products) such as fresh fruits and vegetables, or meat and fish, only a subsample of households reports information on product characteristics at hand. Hence, a household either records purchases for packaged products and fruits and vegetables, or for packaged products and meat and fish. Overall, more than 400 product categories are present in the database. The database contains also information on household characteristics collected once a year.

The TNS Worldpanel database is organized as follows. Each observation corresponds to a single purchase of a food item realized by a household (identified by a panelist id) in a given store at a specific date. Each purchase being defined by the characteristics of the product.

There are however two limits to using this database. First, if products are decribed by a rich set of attributes, their barcode (for non-random weight products) are not reported which made complicated their tracking over time. Products are only described by their attributes and are classified by category. To facilitate the comparison of product prices

[27]Part of the appeal of the TNS Worldpanel data lies in the large number of characteristics describing the purchased item. Numerous studies have used this data to investigate various issues such as the role of product nutritional characterictics on pricing (Dubois, Griffith and Nevo, 2012) or resale price maintenance in the botled water market (Bonnet and Dubois, 2010).

over time, we create an unique identifying code for each combination of product characteristics using the whole set of attributes, except product capacity and pack size. It results that our definition of a product is closed to the universal product code (UPC) definition and eliminates aggregation bias which could result from an identifier constructed at the brand level, for instance. Product prices are thus reported in centimes of French Franc (1 centime $\approx$ 0.0015 €) per measurement unit (i.e., per Kg, per Liter or per unit) and are deflated. One second limit concerns the absence of information on the exact location of the store where the purchase was made. Households report only on the channel of sales (e.g., retail stores, convenience stores or specialized shops), the store type (e.g., hypermarket, butcher or delicatessen), and for retail chains their name. To recover this crucial information for our study, we develop an original prodecure presented hereafter.

The second source of data is the Panorama Tradedimensions dataset compiled by Nielsen. This dataset tracks detailed information on the market structure of the French retail sector, over the same period. We are more concerned with data for grocery retail stores. For each store, the dataset provides a multitude of information on store characteristics such as store size (in square meters), format, chain name, store address, number of trolleys, number of parking slots, ownership structure of the group or date of opening. We also have all information on changes of ownership over the period, as well as opening, extension, or closing of stores.

The last source of data comes from the Census (French National Institute of Statistics, INSEE). We supplement the information at the store level by collecting data on population and average household income to proxy demand at the *commune* level.[28]

Matching the purchase and store databases Because our goal is to match the TNS Worldpanel database with the Tradedimensions database, we propose a procedure that exploits information provided by households to determine the location of the store where a product was purchased. Recall that for each purchasing act, we know the residence address of the household (postcode of the commune); and for the store visited, the channel of sales (e.g., retail stores, convenience stores or specialized shops), the store type (e.g., hypermarket, butcher or delicatessen), the store size (but this information is subject to caution as it is not systematically well reported) and for retail chains their name. In order to match each purchasing act to a store, we apply an algorithmic procedure using all the information available. The algorithm can be described as follows.

First of all, we compute the Euclidean distances among all the stores and households'

[28]Newmark (1990) shows that households income explains a large part of retail prices, so that controlling for income disparities is necessary in our study. In a sample of 27 American cities, he tests the impact of local retail concentration on the price of a bundle of 35 products. Using CR4 as the concentration index, he shows that controlling for the income effect strongly reduces the coefficient of concentration in the price regression: in his study, the effect of the concentration ratio is non significant, and slightly negative.

Table 14: Matching process

Step	% of the observations matched
A single item in the 40km area	8.56
1 surface matching in the 10 closest stores	33.97
Closest store with surface matching in +/- 200 sqm	34.90
Closest store with surface matching in +/- 400 sqm	6.15
Closest store with surface matching in +/- 600 sqm	3.36
Closest store with surface matching in +/- 800 sqm	1.89
Closest store with surface matching in +/- 1 000 sqm	1.32
Closest store with surface matching in +/- 1200 sqm	1.24
Closest store with surface matching in +/- 1400 sqm	0.60
Closest store with surface matching in +/-1 600 sqm	0.52
Closest store with surface matching in +/- 1800 sqm	0.36
Closest store with surface matching in +/- 2 000 sqm	0.28
Closest store with no surface matching	3.64
Total matching	**96.78**

residence, assuming that both reside at the center of their respective commune (giving that we know only the postcode of stores and households location). Then, for a given purchasing act, we list all the stores of the relevant chain within a distance of 40 km around the household residence. The distance bound is chosen voluntarily large (i.e., larger than the usual attractiveness of an hypermarket's area) in order to potentially account for any stores reached from the workplace. If there is only one store in that set, we select this one (according to Table 14, we match 8.56% of the observations at this step). Otherwise, we select a subset consisting of the 10 closest stores and we consider the reported size of the store. Several cases are then considered. First, if there exists a unique store of the same surface, we select this one (34% of the observations are matched at this step). Otherwise, we proceed by increasing at each step the range around the reported surface by 200 sqm. We do so in order to account for errors made by the households when reported the store size. Hence, we match an additional 35% of the observation by selecting the store with a surface within 200 sqm of the reported surface, 6% within 400 sqm, and so forth. Finally, we thus match 96% of the observation and remove from the database the remaining observations. Table 14 displays the percentage of observations matched at each setp. Note that more than 83% of the purchasing acts are matched by allowing a measurement error of the store size up to 400 sqm.

Each purchasing act is now linked to a single store, which enables us to work with a store-product level dataset. Before going further, we perform some clearing tasks in the dataset. We drop all purchases made in stores that entered or exit over the period 1998-2001. Further, we drop all stores subject to a change of ownership not related to the merger during the period 1998-2001. This ensures that price comparison within a store is not impacted by a change of ownership not related to the merger. We also remove from the data stores of the merging parties that have been divested to rivals due to the decision of the French ministry of Economics (34 stores in total).

Selection of product-store pair and price computation Because the purchasing acts are recorded daily, we aggregate the data at the semester level. The level of aggregation is chosen sufficiently large to enable a significant number of observations at the store-product level while reflecting as closely the price changes. The mean price of each UPC (or random weight product) is then computed by semester. More precisely, we calculate the mean price of a UPC at a given store as an average revenue (i.e., sales/quantity purchased). Note that we removed promotional prices from the data (5.18% of the data). We present in Table 15 some statistics on the occurence of promotions before and after the merger, to show that the merger does not modify the promotional strategy of retailers.

In addition, we restrict our attention on UPCs that satisfy some criteria of representativity to ensure comparison of prices over time and within stores (affected or not by the merger). In order to limit selection bias, especially in the spreading out of observations between the control group and the treatment group, we select a subsample of UPCs by applying the following criteria sequentially:

- The sales of a UPC in a given store (UPC-store, hereafter) must be observed four semesters before and two semesters after the merger (that is in years 1998, 1999 and 2001), which will allow for before-and-after comparisons;
- A UPC-store must have at least three observations per semester;
- A UPC-store must be sold at least in ten different stores;
- Only stores that sell at least three products are considered;
- Only UPC for which we have at least one observation in a store of the control group and one observation in the treatment group are considered.

We report in Table 16 some descriptive statistics on the selection criteria used to select our sample of UPC. In the robustness section, we provide some sensitivy analyzes with

Table 15: Summary statistics on promotions

Retail chain	Pre-treatment (1998-99)		Treatment (2001)	
	Mean	S.D.	Mean	S.D.
O_3	0.0695	(0.0026)	0.0625	(0.0015)
$M1_H$	0.0656	(0.0061)	0.0521	(0.0020)
O_4	0.0454	(0.0050)	0.0345	(0.0044)
$M2_S$	0.0536	(0.0040)	0.0396	(0.0069)
O_6	0.0762	(0.0048)	0.0578	(0.0046)
O_1	0.0412	(0.0046)	0.0390	(0.0031)
O_2	0.0543	(0.0036)	0.0541	(0.0001)
O_5	0.0617	(0.0036)	0.0499	(0.0004)
# semesters	4		2	

Notes: The averages reported correspond to the average number of sales by retail chain and by semester for the pre- and post-period merger. S. D. corresponds to standard deviation. The year 2000 is ommited.

Table 16: Descriptive statistics on selection criteria

Panel A: Without the constraints

Selection criterion	Obs	Mean	S.D.	P25	Median	P75	Min	Max	Cutoff
Nb. of semesters filled in by store-product	926174	1.88	1.30	1.00	1.00	2.00	1	6	6
Nb. of observations by store-product-semester	1737744	2.22	3.12	1.00	1.00	2.00	1	330	3
Nb. of stores by product	1070	865.58	520.52	506.00	720.50	1056.00	125	3833	10
Nb. of products by store	7501	123.47	165.30	14.00	50.00	170.00	1	1005	3

Panel B: With sequential implementation of the constraints

Selection criterion	Obs	Mean	S.D.	P25	Median	P75	Min	Max	Cutoff
Nb. of semesters filled in by store-product	926174	1.88	1.30	1.00	1.00	2.00	1	6	6
Nb. of observations by store-product-semester	166578	5.75	6.74	2.00	4.00	7.00	1	330	3
Nb. of stores by product	658	10.71	27.59	1.00	3.00	8.00	1	281	10
Nb. of products by store	1305	4.19	5.08	1.00	2.00	5.00	1	51	3

Notes: The figures are reported for the baseline definition of the catchment areas (i.e., 20/10 km). The year 2000 is removed. The column "Obs" reports the number of distinct observations at the level defining the criterion. It thus differs from the total number of recorded purchases equal to 3847615. In Panel A, for instance, the figure 926174 corresponds to the number of semesters observed in the dataset for store-product pair. On average, we have 1.88 semesters by store-product pair. Panel A displays the descriptive statistics, for each criterion considered, computed without applying the criteria. It is thus interesting to compare the avegare of observations by criteria with the cutoff considered. The descriptive statistics of Panel A are thus computed from the "raw file". By contrast, Panel B displays the descriptive statistics computed from the data where observations that do not satisfy the criterion are removed (the constraints are applying sequentially). Hence, the statistics of the second row of Panel B are computed once removed the store-product observations for which we do not have a record for the six semesters, i.e. 1998S1, 1998S2, 1999S1, 1999S2, 2001S1, and 2001S2.

Table 17: Homogeneous product purchases by product category

Product category	# obs.	%	cumulative %
10. EAUX NATURE	65082	20.70	20.70
269. PAIN INDUSTRIEL (SANS EAN)	23141	7.36	28.06
12. COLAS	22421	7.13	35.19
5111. CHARCUTERIE HORS JAMBON (SANS EAN)	21748	6.92	42.11
103. LAIT LONGUE CONSERVATION STERILISE	20196	6.42	48.53
5127. PORC A ROTIR/GRILLER/POELER (SANS EAN)	19564	6.22	54.75
310. SUCRE EN MORCEAUX	17357	5.52	60.27
311. SUCRE AUTRES PRESENTATIONS	9626	3.06	63.34
109. MARGARINE ET ASSIMILES	9551	3.04	66.37
67. ORANGES FRAICHES	9282	2.95	69.33
5188. TOUS FROMAGES SANS EAN 4P97	8938	2.84	72.17
202. CAFE TORREFIE AVEC EAN	8332	2.65	74.82
172. YAOURTS FRAIS AVEC EAN	8111	2.58	77.40
5039. JAMBON BLANC DETAIL (SANS EAN)	7106	2.26	79.66
63. CITRON	6204	1.97	81.63
66. BANANES	4181	1.33	82.96
271. PATE A TARTINER CHOCOLATEE	4161	1.32	84.28
72. CREME FRAICHE CONDITIONNEE	3779	1.20	85.49
592. PAIN INDUSTRIEL (AV EAN)	3528	1.12	86.61
90. AUTRES FROMAGES (AVEC EAN)	3496	1.11	87.72
268. OLIVES CONDITIONNEES	2839	0.90	88.62
8. BIERE	2834	0.90	89.52
91. FROMAGES FONDUS (AVEC EAN)	2749	0.87	90.40
591. VIENNOISERIE INDUSTRIELLE (AV EAN)	2483	0.79	91.19
32. BEURRE CONDITIONNE	2154	0.69	91.87
5032. CREVETTES (SANS EAN)	2096	0.67	92.54
205. CHICOREE CAFE SOLUBLE ET AUTRE MELANGE	1771	0.56	93.10
79. ENDIVES	1572	0.50	93.60
93. FROMAGES BLANCS/AROMATISES (AVEC EAN)	1442	0.46	94.06
23. V.Q.P.R.D (AVEC EAN)	1437	0.46	94.52
255. HUILE AUTRE CONDIT.A CONSOMMER	1423	0.45	94.97
565. PAIN FRAIS POUB. ALIMENTS AUTRES ANIMAUX	1421	0.45	95.42
92. PETITS SUISSES (AVEC EAN JUIN 95)	1078	0.34	95.77
206. CEREALES POUR PETIT DEJEUNER	1073	0.34	96.11
212. CHOCOLATS DE FETES & PERMANENTS EMBALLES	1070	0.34	96.45
5117. VEAU A ROTIR/GRILLER/POELER (SANS EAN)	1044	0.33	96.78
37. RADIS ROSES FRAIS	910	0.29	97.07
5189. JAMBON CRU (SANS EAN) FEVRIER 97	857	0.27	97.34
209. CHICOREE ET MALT	753	0.24	97.58
501. NAVET FRAIS	738	0.23	97.82
7. APERITIFS	732	0.23	98.05
42. MACHE FRAICHE	728	0.23	98.28
188. BISCUITS A GRIGNOTER POUR L'APERITIF	720	0.23	98.51
112. OEUFS EN VRAC	716	0.23	98.74
211. CHOCOLAT EN TABLETTE	611	0.19	98.93
5142. VIANDE HACHEE DEVANT VOUS (SANS EAN)	581	0.18	99.12
509. POIVRON FRAIS	533	0.17	99.29
5141. VIANDE HACHEE PREEMBALLEE (SANS EAN)	528	0.17	99.45
586. PATES ET RILLETTES FRAIS (AVEC EAN)	474	0.15	99.61
187. AIDE A LA CUISINE AVEC EAN	428	0.14	99.74
316. THON EN CONSERVE	419	0.13	99.87
626. PLATS CUISINES APPERTISES (AVEC EAN)	394	0.13	100.00
Total	314412	100.00	

Notes:

Table 18: Market structure of local markets for the alternative scenarios

Fraction of catchment areas with *at least one*:	30/15 km		20/10 km	
	Raw data	Final data	Raw data	Final data
$M1_H$	56.92	58.10	46.41	50.59
$M2_H$	49.42	48.25	37.83	38.32
$M1_S$	54.02	58.43	43.29	47.56
$M2_S$	61.97	60.93	51.11	50.76
$M1_H$ & $M2_H$	34.29	31.89	25.90	24.71
$M2_S$ & $M1_S$	36.94	39.23	25.18	27.23
$M2_H$ & $M1_S$	32.98	34.22	23.14	24.03
Merging firms $M1 + M2$	91.87	92.65	84.47	86.72
Total number of catchment areas	9605	599	9605	595

Fraction of catchment areas with *at least one*:	10/5 km		20/10/5 km	
	Raw data	Final data	Raw data	Final data
$M1_H$	35.19	41.30	44.38	49.41
$M2_H$	26.16	29.18	34.38	35.46
$M1_S$	26.15	29.86	34.90	40.50
$M2_S$	38.31	35.49	48.52	46.72
$M1_H$ & $M2_H$	17.12	17.75	22.24	21.85
$M2_S$ & $M1_S$	9.39	11.09	15.63	18.49
$M2_H$ & $M1_S$	8.11	10.24	13.28	15.97
Merging firms $M1 + M2$	74.54	75.43	83.72	86.05
Total number of catchment areas	9605	586	9605	595

Notes: The statistics on market structure are reported for the second semester of 1999 (pre-period). The statistics corresponding to the *Raw data* are computed from Panorama Tradedimensions that compiled global information for all of the grocery stores operating in France. The *Final data* only restrains a subset of those stores for which the recording purchases in the TNS Worldpanel satisfy some criteria of representativity over the period of study.

respect to these criteria. Finally, according to this selection procedure, we identify 109 UPCs that gather both national brand products and fresh products (i.e. fruits, vegetables, meat and fish). Table 17 reports the number of observations and the percentage associated of the UPCs retained in our study gathered by product category.

Local market definition The baseline definition of local markets is grounded upon the approach followed by the French CA. We define the local market around each store as the market area that spans up to 20 km for hypermarkets (and up to 10 km for other formats) around the city center where the store is located. Thus, the set of local competitors for a given store consists of all the hypermarkets within 20 km around the city center where the store is located, and all other shops within 10 km. Alongside, we consider the alternative cases where local markets are defined according to the following boundaries: "30/15 km" (i.e., up to 30 km for a hypermarket and 15 km for another format), "10/5 km" and "20/10/5 km". The last cases corresponds to a mix between the "20/10 km" and "10/5 km" definitions in order to reflect the more limited travel in densely populated urban areas. Precisely, we adopt the "20/10 km" definition, except for the three largest French metropolitan areas (i.e., Paris, Lyon, Marseille) for which we adopt the "10/5 km" definition. We report in Table 18 the market structure arising from

applying these definitions on the French market (raw file) and on the final dataset (final data).

B Additional tables

Table 19: Propensity score estimate (probit model)

Dependent variable: Binary variable of merger occurence)		
Variable	Table 6 to 9	Table 10
	(1)	(2)
HHI	-3.5765***	-4.0171***
	(0.5910)	(1.0056)
log(market income)	1.2523	-1.4672
	(0.7767)	(1.2373)
Store size	0.1351***	0.0944
	(0.0454)	(0.0628)
Constant	-8.8664	15.6849
	(7.3682)	(11.7734)
Region FE	Yes	Yes
Retail chain FE	Yes	Yes
Pseudo-R^2	0.3420	0.2071
Observations	595	138

Notes: This Table presents the point estimates of a a probit of a merger occurring in a certain local market, as a function of baseline HHI concentration, baseline income, regional dummies, and rival retail chains dummies. We use the fitted values of the probability of treatment from the probit analysis (the propensity scores) to re-weight the regression sample.

Table 20: Before and After like Table 6

Dependent variable: (log) price (by product, by store, by semester)							
	No expend. weights				Expend. weights		
Variable	(1)	(2)	(3)	(4)	(5)	(6) (propensity score)	(7) (one-nearest neighbor)
Post merger × Outsider	0.1844	0.0494	0.0494	0.0204	0.0129	0.0190	0.2233***
	(0.3197)	(0.0453)	(0.0454)	(0.0447)	(0.0682)	(0.0690)	(0.0727)
Post merger × Insider	-0.0566	0.0274	0.0274	-0.0032	-0.0129	0.0005	0.2138***
	(0.3211)	(0.0454)	(0.0455)	(0.0449)	(0.0683)	(0.0691)	(0.0751)
log(market income)	0.0592	-0.0392	-0.0537	-0.0357	-0.0995	-0.2369**	-0.3700*
	(0.1262)	(0.0489)	(0.0486)	(0.0538)	(0.0687)	(0.1165)	(0.1999)
Constant	6.5219***	7.4609***	9.6530***	9.5137***	10.1190***	11.4239***	12.9670***
	(1.2039)	(0.4677)	(0.4642)	(0.5126)	(0.6548)	(1.1094)	(1.9061)
Store	–	Yes	Yes	Yes	Yes	Yes	Yes
Product	–	–	Yes	–	–	–	–
Product-semester	–	–	–	Yes	Yes	Yes	Yes
Clustered errors	store-pr	store-pr	store-pr	store-pr	store-pr	store-pr	store-pr
R^2	0.003	0.187	0.986	0.987	0.988	0.989	0.989
Observations	24888	24888	24888	24888	24888	24888	10728

Notes: Catchment areas are delimited with the 20/10 km boundaries. The year 2000 is omitted. Clustered standard errors (at store-product level) are reported.