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The Effects of Retail Regulations on Prices: Evidence from the Loi Galland *

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Abstract

Using a unique dataset merging micro data at product and store level with data on competition within local grocery markets, this paper provides an empirical analysis of a legislation that had the same effect as allowing industry-wide price floors. It shows that, after the introduction of the legislation, the link between retail prices and market concentration has significantly been weakened, especially for branded products. Price dispersion has dropped for branded products more than for store brands and price convergence appears to have taken place across stores. These results are consistent with recent theories on the anticompetitive effects of resale price maintenance in markets with interlocking relationships.

Keywords: retail prices, pricing regulations, resale price maintenance.

JEL: L42, L81, K23.

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1 Introduction

Resale price maintenance (RPM) has recently been at the centre of heated debates among lawyers and economists following the U.S. Supreme Court ruling in the *Leegin* case and the discussions that preceded the revision of the block exemption regulation on vertical restraints by the European Commission. The economic literature on the impact of RPM on consumer and/or total welfare is ambiguous, the effect depending on the context in which RPM is implemented, and there is a clear lack of empirical evidence.[1] Changes to regulations akin to rules de facto legalizing RPM could thus be used as natural experiments to evaluate the competitive effects of RPM. Such changes occurred in the past, at least in Ireland and France. This paper analyses the effects of such a legal change, namely the implementation of the Loi Galland in France in January 1997.

The 1996 Loi Galland modified existing below-cost pricing regulations (in force since 1963) in order to protect small retailers (as well as producers) from the downward pressure exerted by large chain stores on retail prices (as well as wholesale prices). It was not the first attempt to curb the rapid development of chains of supermarkets and hypermarkets. The 1973 Loi Royer, reinforced in 1996 by the Loi Raffarin, implemented restrictive planning regulations for the opening of new large stores and extensions of existing stores. **?** empirically confirmed the negative impact of the Loi Royer on employment and prices. While below-cost pricing regulations were already in place since 1963, they were rather ineffective due to an unclear definition of the relevant cost threshold. The Loi Galland clearly defined the applicable threshold as the invoice price, that is, the price paid by the retailer at the time of delivery. It then became illegal for retailers to pass-on to customers any conditional rebate (e.g., end-of-year rebates).

It was widely believed that conditional rebates (or "hidden rebates") increased substantially after 1997, thus guaranteeing a minimum (gross) margin to retailers. The Loi Galland had therefore similar effects to simply legalizing industry-wide minimum resale price maintenance (RPM) and is likely to have eliminated, or at least substantially limited, intra-brand competition. The inflationary effects of the Loi Galland have been fiercely debated in France. Grocery products' retail prices have indeed increased faster than the consumer price index (CPI) over the 1997-2002 period (11.8% vs. 6.4%), whereas they tended to increase at a slower rate before 1997 (2% vs. 3% over the 1994-1996 period).[2] **?** make a more systematic macro-econometric analysis and show that the Loi Galland might have added about one percentage point to inflation in the period 1997-2004. This effect also seems specific to France.

The empirical analysis of the consequences of the Loi Galland on final prices thus sheds an interesting light on the more general debate about the anticompetitive effects of minimum RPM.[3] Empirical evidence on the consequence of this kind of regulations is scarce. To our knowledge, the only empirical study on the effects of below-cost pricing regulations was done by **?**, who evaluate the effect of the 1987 *Groceries Order* in Ireland, a regulation comparable to the Loi Galland.[4] Focusing on a specific category of

[1] For surveys of the theoretical literature and empirical evidence see respectively **?** and **?**.

[2] See **?**. *AC Nielsen* found that retail prices of 1500 (national brands) products went up by more than 4% during the first two months of 1997 alone.

[3] This is especially the case for markets with "interlocking relationships", i.e., where manufacturers sell through the same retailers. In such settings, **?** and **?** have shown that RPM may be particularly harmful.

[4] The 1987 *Groceries Order* has been revoked in December 2005 and it was then argued that this would *"introduce*

products (processed and preserved fruits and vegetables), **?** show that the *1987 Groceries Order* had a significant impact on gross retail margins.[5]

Using richer data than **?**, this paper is devoted to an empirical investigation of the effects of the Loi Galland on retail prices. We use a unique data set merging CPI micro data at store and product level with local competition data. Individual retail prices are available for about 200 homogeneous products surveyed monthly from 1993 to 2000 in about 2000 stores. Local market concentration is computed using an exhaustive index of French retail stores.

The Loi Galland was enforced on January 1^{st} 1997, for all types of stores and products everywhere in France, with no exception or differentiated implementation delay allowing to construct control groups. Any evaluation of the impact of the Loi Galland on retail prices must therefore rely on testing predictions on the likely impact of industry-wide price floors (minimum RPM) that would not be explained by shocks on costs or demand, nor by other political or legal changes, including changes to planning restrictions such as the 1996 Loi Raffarin. Since both the Loi Galland and the Loi Raffarin are expected to have had inflationary effects, we do not focus on the level of prices. Instead, we exploit the geographical variability of prices, the heterogeneity of store size, location and type, and the heterogeneity of products. The geographical variability of prices is partly a consequence of the regulation restricting the extension of large chain stores that prevented, or largely delayed, adjustment of market structures to market conditions. As far as product heterogeneity is concerned, we exploit the distinction between branded products and store brands. Branded products are produced by large manufacturers with strong bargaining power in their negotiations with retailers, whereas store brands are usually procured through very competitive bidding processes (in which producers - very often SMEs - have very limited bargaining power).

We find evidence consistent with the prediction that the Loi Galland may have played a role in the switch from a regime mostly driven by local competition, to a regime with industry-wide minimum RPM and vertical negotiations that focus essentially on the "hidden rebates" that cannot be passed on to consumers. Indeed, although retail prices have a strong positive correlation with the concentration of local grocery markets before the Loi Galland[6], this correlation strongly decreases after 1997, especially for branded products and hypermarkets. We also find evidence that, after 1997, the price dispersion has decreased more for branded products than for store brands. We finally find some evidence that prices have increased more in initially cheaper stores, so that some price convergence across stores seems to have taken place. Again, this result appears to be stronger for branded products.

The rest of the paper is organized as follows. We start by briefly analyzing the impact of industry-wide price floors and resale price maintenance in the context of vertical negotiations (section 2). We then describe our empirical strategy to identify how the Loi Galland has impacted retail prices through the elimination of intra-brand competition (section 3). We present our data in section 4. In section 5, we test for a break in the correlation between retail prices and local markets concentration after the Loi Galland. In section 6, we focus on the reduction of price dispersion and on the price convergence across stores after the Loi Galland. Section 7 concludes.

greater competition into grocery trade by allowing retailers freedom to determine the prices they charge their customers."

[5]These retail margins increased on average from 15.8% in 1988 to 20.1% in 1993.

[6]For similar results, see **?** for Portugal, **?** for Sweden, **?** for the U.S.

2 Economic Analysis of the Loi Galland

In 2004, a group of experts (Commission Canivet) was commissioned by the Minister of Finance to evaluate the existing legal framework. The title of their report (?) on vertical relationships in the food industry, *"Restoring price competition"*, was a clear indication of what was expected from the discussions. According to this report, the Loi Galland gave producers and retailers the ability to manipulate the below-cost pricing threshold in order to freely set an industry-wide price floor. This (potentially illegal) manipulation might have been a wide-spread practice.[7] Parties (manufacturers and retailers) agree that, after the enactment of the Loi Galland, the negotiation shifted from *"upfront margins"* (i.e., rebates that can be included on the invoice) to *"hidden margins"* (i.e., end-of-year rebates and commercial cooperation that cannot be passed through to consumers).[8]

Manufacturers of national branded products exert some market power towards retailers and thus bargain with them (or with the buying group to which the retailers belong) over contractual terms. Such contracts tend to be extremely complex, especially if they are designed to take advantage of the legal framework. In this section, we describe how the negotiations between manufacturers and retailers tend to take place and how the legal framework – in our case the Loi Galland – may directly affect retail prices formation.

2.1 General Terms of Sale and Minimum Retail Prices

Wholesale contracts are usually negotiated once a year. For each product, the manufacturer announces public and non-discriminatory "*general terms of sales*" (hereafter GTS)[9], before negotiating with each retailer (or more precisely each retail chain) individual rebates or agreements for commercial services. The GTS specify the tariff or "*list price*", i.e., a per-unit wholesale price for the product, as well as two types of rebates including:

- *Unconditional rebates*, i.e., rebates that are granted at the time of delivery (most products are usually delivered to the retail stores or to the logistic platforms of the chain several times a year). These can be rebates for specific store formats (for instance specific rebates for hypermarkets or large supermarkets) independently of the quantity bought, or rebates for early payment.
- *Conditional rebates*, i.e., rebates that cannot be included on the invoice at the time of delivery, for instance because they depend on the total quantity (or turnover) that the store (or the chain) buys during the year. These rebates may for instance be used to induce marketing efforts by the retailers.

[7] For the potential illegality of this manipulation, see for instance the decisions *Childrens Videos* (Conseil de la Concurrence, 05-D-70, December 2005) and *Toys* (Conseil de la Concurrence, 07-D-50, December 2007).

[8] According to the producers' association ILEC, the average hidden margin increased from 22% of the net wholesale price in 1998 to 32% in 2003.

[9] A recent reform that came into force for the negotiation over the 2009 tariffs has profoundly modified the legal framework, allowing manufacturers and retailers to bargain over "individual terms of sale". During the period covered in this paper (01/1993-12/2000), general terms of sales were however non-negotiable.

The GTS have to be non-discriminatory and were, until very recently, non-negotiable. Rebates may be specific to some types of stores but all retailers from a given type must be granted the same tariff. However, producers often negotiate secretly with each retail group additional rebates in exchange for specific services (such as promotional activities, better shelf space, local advertising, ...). These services, also known as "*commercial cooperation*", tend to be billed on a yearly basis.

The existence of conditional rebates and retailer specific commercial cooperation agreements imply that the price that appears on the invoice at the time of delivery, also known as the "*net price*", is not the true cost for the retailer and often differs from it to a great extent. The retailer may indeed ultimately pay much less than the invoice price. Rebates that do not appear on the invoice (i.e., conditional or end-of-year rebates as well as those for "commercial cooperation") are known as the "*hidden rebates*" and constitute the difference between the invoice price and the true price (also known as the "*triple net price*").

Under normal circumstances, the threshold used to identify whether the retailer is selling below cost should likely be the triple net price. In this case, whether rebates are conditional or not does not have any impact on retail price formation: what matters for the retailers when setting their retail prices is their true (expected) cost. It is no longer the case under below-cost pricing regulations such as the Loi Galland which defines the below-cost pricing threshold (i.e., the minimum price below which retailers are not allowed to sell) as the invoice price. Hidden rebates can no longer be passed on to final consumers and thus constitute a guaranteed (gross) margin for the retailer. Most of the economic debate about the Loi Galland revolves around the significance - and relevance - of this particular minimum price.

The Loi Galland, combined with non-discrimination rules, de facto transformed the net price, which has no real economic meaning, into an industry-wide minimum retail price. The manufacturers thus had the power to directly control retail prices. It is probable that the legislator did not expect that this law would change the repartition of conditional and unconditional rebates. However, this industry-wide threshold provided the industry with a new, potentially very harmful, tool that was easy to manipulate: the producer could simply set a relatively high net price (identical for all comparable retailers) in order to maintain high retail prices and therefore high industry profits. These profits could then be shared with the retailers through individually negotiated hidden rebates. It is often claimed that this manipulation actually occurred, all involved parties (manufacturers and producers) agreeing that, after the enactment of the Loi Galland, the negotiation shifted from *"upfront margins"* (i.e., unconditional rebates that can be included on the invoice) to *"hidden margins"*.

2.2 Minimum Resale Price Maintenance

As shown above, the Loi Galland had the potential to eliminate (or at least substantially soften) intra-brand competition since the manufacturers were able to control their products' retail prices. It is however important to analyse why manufacturers have been willing to use such a mechanism to increase retail prices (since high retail prices reduce the quantities that they can expect to sell) and why they were unable to achieve this result prior to this new law.

In a context of bilateral negotiations between one monopoly manufacturer and retailers, **?** have shown that, when bilateral negotiations are secret (hence the term "hidden rebates"), the joint-profits of the manufacturer and its retailers are not maximized as equilibrium retail prices are too low.[10] When negotiating the (secret) wholesale contract, the manufacturer and the negotiating retailer take the contracts offered to competing distributors as given. If wholesale tariffs can be sophisticated enough, the negotiation will maximize the pair's joint-profits.[11] However, they do not take into account the other retailers' margins, which are positive since retailers are not perfect substitutes (if only due to spatial differentiation). They thus have incentives to favour their own sales thus lowering the negotiated wholesale price in order to "free-ride" on the other retailers' sales. As a result of this so called "producer's opportunism problem", equilibrium wholesale prices - and therefore equilibrium retail prices - are below the industry-profit maximizing prices.[12] Even though the manufacturer is a monopolist, it cannot take advantage of its market power, the opportunism problem dramatically reducing its profit.[13]

? also show that imposing an industry-wide price-floor helps to restore the monopoly profits, as long as the mechanism through which the price floor is set is credible. In this situation, offering better contractual terms to one retailer does not affect its sales and thus only lower the manufacturer's profits: the manufacturer has no longer incentives to free-ride on one retailer's sales when negotiating with another one. In the French grocery retailing market, the Loi Galland (combined with non-discrimination rules) provided this credible industry-wide price-floor mechanism, thus giving high incentives to the manufacturers of well-known brands and/or must-stock items to manipulate their net price.

3 Identifying the Effect of the Loi Galland

3.1 Identification Issue

As explained earlier, one can suspect that the Loi Galland might have allowed manipulations of the below-cost pricing threshold leading de facto to resale price maintenance. This mechanism could ultimately have been (at least partially) responsible for the sharp increase in retail prices that has been observed in France between 1997 and 2000. Even though the unforeseen consequences of the Loi Galland have been much debated, there is a lack of robust empirical evidence. Our goal in this paper is to provide a general and direct analysis, considering the grocery market as a whole and not only one particular product category. However, the causal impact of such a reform is potentially difficult to analyze as it requires separating it from the impact of any other shocks occurring in the industry, would it be classical demand or cost shocks or contemporaneous policy changes.

[10] A similar idea was first proposed by **?** in the context of Cournot competition. See also **?** and **?** who analyse similar situations but using a more traditional game-theoretical approach than the "*contract equilibrium*" concept Ã© la **?** adopted by **?**.

[11] For instance if parties can negotiate over a two-part tariff, the wholesale price can be used to maximize the joint-profits that can then be shared using the fixed fee.

[12] A similar problem would arise in the case sequential public negotiations since, during the first negotiations, the producer cannot commit on the contractual terms that it will negotiate in the subsequent negotiations.

[13] A similar problem exists when retailers have some bargaining power. In this case, whether negotiations are secret or not does not qualitatively affect the results. See for instance **?** or **?**.

3.1.1 Loi Galland or changes induced by demand of cost shocks?

A general retail price increase could also have been explained by increases in production or distribution costs. It is therefore impossible to merely concentrate on price levels and any analysis of the effect of the Loi Galland requires the use of methodologies that can separate all these effects. One way would have been to estimate a structural model. For instance, although they are not primarily interested by below-cost pricing regulations, **?** analyse vertical contracting between manufacturers and retailers using micro-level data on the distribution of bottled water in French supermarkets. Their results are consistent with the use of two-part tariffs and RPM and their simulation of a counter-factual situation without RPM predicts an average 7% drop in prices for the major brands.

It is hardly imaginable to adopt this structural approach to a very large number of different products but it is nevertheless possible to identify more refined predictions than mere price increases of the introduction of industry-wide price floors in the retailing market. Focusing on the price formation mechanism, we argue that cost (or demand) shocks and resale price maintenance would generate very different patterns of price effects, especially once considering their impact on the link between retail prices and concentration on local retail markets. The effect of an industry-wide price floor comes from the elimination of intra-brand competition which is not affected by a change in retailing costs. In the absence of price floors, retail prices are expected to respond to competition between retailers in each local market. Eliminating intra-brand competition should remove this link. Costs shocks may affect the level of equilibrium retail prices, but not link between prices and concentration levels.

3.1.2 Loi Galland or changes induced by other regulatory changes?

Another important regulatory change was contemporaneous with the Loi Galland. As of July 1996, the Loi Raffarin reinforced planning restrictions, reducing the threshold for a mandatory retail permit (obtained through a lengthy administrative procedure) from 300 to 1000 m^2.[14] It has been argued that the Loi Raffarin also led to price inflation. However, the effects of changes to planning restrictions should be very different from the effects of the Loi Galland.

The Loi Raffarin can be viewed as an incremental reinforcement of an already existing restrictive policy, rather than as a structural break: barriers to entry were already in place since the 1973 Loi Royer that introduced the mandatory retail permit for stores over 1000 m^2 (i.e., hypermarkets and large supermarkets). It is well documented that the direct effect of planning restrictions is an increase in market concentration and in the market power of local incumbents. **?** evaluate the effects of the Loi Royer and show that from 1974 to 1998, only 40% of the applications submitted to local commissions granting construction permits were approved. As the process of approval became longer and largely political, this reduced the flow of opening of new large retail stores (differently so across markets depending on the policies of local commissions) eventually leading to significant discrepancies in concentration across markets, for given local demand and cost conditions. As local market power diverged across markets,

[14] Moreover, in 1993, the Finance Minister had given instructions to the local commissions granting these retail permits to slow down the evaluation process. This led to a significant drop in the number of store extensions and openings after 1993. The Loi Raffarin was merely seen as a way to legalize that practice.

one expects the correlation between prices and local concentration to have risen. Therefore, if the Loi Raffarin had an impact on the correlation between retail prices and market concentration, it should have been an increase rather than a drop. The fact that the two regulations were implemented almost simultaneously thus at most implies that we may underestimate the effects of Loi Galland.

3.2 Retail Prices and Local Competition

If the Loi Galland materialized through industry-wide price floors, the correlation between retail prices and competition in local grocery markets should have decreased strongly after 1997. This prediction is unrelated and independent from possible general shocks on costs that might have driven prices up during the same period. As argued before, increased barriers to entry due to the Loi Raffarin, should not have such effect either. On the contrary, in line with what we know from the impact of the Loi Royer, increasing the burden of planning restrictions should further, even though only incrementally, increase the market power of (some) incumbents, which should in turn increase the correlation between local concentration and prices.[15]

3.2.1 Product heterogeneity

Additional predictions can be related to product heterogeneity as well as to store heterogeneity. Economic analysis first suggests that the effect of the loi Galland should be much larger for national brand than for store brand products. The framework of vertical relationships presented in section 2 is relatively well-suited for the well-known brands or "*must-stock*" items sold by all the main retailers and produced by large manufacturers. Only those large manufacturers selling to multiple chains of retailers indeed face the opportunism problem and have an incentive to manipulate the threshold. For the supply of store brands, retailers often rely on SMEs. Moreover, these SMEs (or sometimes large manufacturers) compete annually to supply a particular store brand. The relationship between a retailer and the store brands' suppliers is thus best seen as a bidding market where the retailer tends to have most of the bargaining power. Therefore, the Loi Galland should not affect directly the retail prices for store labels (there are usually no "hidden margins" for store brands). However, since store brand products are to some extent substitutable to branded products, they may have been indirectly affected. We should therefore expect a much higher change in the correlation between prices and local concentration for national brands than for store brands.

[15] It is in theory possible, that the threat of potential entry may lead incumbent firms in highly concentrated markets to set lower prices than their market power would allow, if they feel that excessive prices in the market will eventually favour entry at the expense of future profits. They could lower their price in order to jam the signal to potential entrants regarding the profitability of an entry. This effect is quite unlikely in the present case given that opening a large hypermarket generally involves in depth market studies by large actors of the market. However, would this effect occur, this would only further increase the correlation between local concentration and prices after the enactment of the Loi Raffarin. Indeed, more planning restrictions lower the threat of entry. Incumbents would thus be more free to enjoy their market power after the enactment of the Loi Raffarin than before.

3.2.2 Store heterogeneity

Stores also have different "selling technologies" (proximity to consumers, distribution cost, level of services, etc.). Hypermarkets compete with supermarkets, convenience stores and hard-discount stores, and these different formats do not face the same demand: demand is likely to be less elastic for convenience stores and supermarkets located in city centres, hard-discount stores are more likely to cater for low-income customers. Inner city consumers have to choose between walking to a local retailer (convenience store or supermarket) or driving a large hypermarket at the outskirts of the city. Among local retailers, they are likely to favour the nearest, so that the higher prices they face are somehow compensated by the benefit of proximity. Shopping at hypermarkets, on the contrary, usually requires significant driving times and geographical differentiation is less crucial. We thus expect the sensitivity to local competition to be higher for hypermarkets, and the impact of the Loi Galland, in the form of reduced correlation between prices and concentration, to have been larger for these stores. We should also expect hard-discounters to be less affected, since they usually do not sell branded products: if prices are strategic complements, we should expect a positive reaction from hard-discounters to the increase in prices of rival formats, but only to a reduced extent.[16]

3.3 Price Dispersion and Price Convergence Across Stores

If the Loi Galland has led to possible implementation of (minimum) resale price maintenance, we should expect that, for a given product, price dispersion has been reduced, especially for branded products. The theoretical literature suggests that resale price maintenance should totally eliminate intra-brand competition. In reality, the situation is probably less extreme for several reasons. First, the *"general terms of sales"* and the various rebates are negotiated at the national level by a manufacturer and the buying group of a given chain. Large retail chains usually have a unique buying group - or purchasing unit - but have many stores in different cities and may even sell through different "fascias." However, retail prices are set locally and depend on the local market conditions. Therefore, the minimum retail price implicitly set by the manufacturer (through the net price) is a nationwide-price based which may not be binding everywhere. Markets that initially had relatively high prices, either because of local demand conditions or because they were highly concentrated, are thus unlikely to have been affected by the Loi Galland. On the contrary, markets where prices were initially lower have been affected by the new minimum price and prices thus went up in these markets. Therefore the inflationary impact should have been higher in markets where prices were initially relatively low and price dispersion should have been reduced, prices converging towards the highest pre-Loi Galland levels.

4 Data

Our empirical analysis relies on two datasets, on retail prices and on the local structure of the grocery retailing industry. Merging the two datasets allows us to extend the analysis up to four years before

[16]Given that the period we analyse also corresponds to the development of hard-discounters in France, it might even be the case that prices in hard-discount stores went down during that period.

and after the enactment of the Loi Galland (from January 1993 to December 2000). In this section, we provide a presentation of the data and summary statistics for two years, 1994 (three years before the Loi Galland) and 1999 (third year after its enactment).

4.1 Retail Prices

We exploit a unique database on individual retail prices, collected by INSEE (the French national institute of statistics) to compute the Consumer Price Index (CPI). This database covers the whole French territory, and stores and products surveyed are sampled so as to be representative.

We keep only products that are sufficiently homogeneous across stores and dates (e.g., sugar, milk) and thus exclude intrinsically heterogeneous products such as clothes or furniture. We further restrict our sample by selecting only products that are widely distributed across all types of retailers.[17] Our final dataset contains monthly prices for 141 food items and 45 non-food items in 1994 (147 and 46 in 1999). In practice, the retail price of product i in store j during month m of year y is collected by an INSEE employee visiting the store and recording the price as well as other relevant information (such as brand, whether the product was part of a special offer, etc.).[18]

Stores are classified according to type and size (measured by selling area). All stores within a type category by and large follow the same business model. The various types are described below:

- *Hypermarkets* are the largest stores (selling area over 2500 m^2 and currently up to 25000 m^2) generally located at the outskirts of large urban areas. They sell an extensive range of products including a large share of non-food items.

- *Supermarkets* have a selling area between 400 and 2500 m^2 and usually located in city centres or at the outskirts of smaller cities. They also propose a large range of products but tend to focus on food items, especially for smaller stores.

- *Convenience Stores* have a selling area smaller than 400 m^2 and are located much closer to the customers. They tend to have a limited range of products.

- *Hard-discount stores* have a selling area comparable to that of supermarkets or convenience stores. They usually do not offer the leading brands, focus essentially on food items and do not propose the same services than traditional stores.

- *"Magasins Populaires"* are the traditional multi-purpose stores in city centres. They are comparable to supermarkets in size but do not primarily focus on food items.

Brand is an important issue in the context of the Loi Galland. In our dataset, the products are coded according to a classification specific to the CPI and the brand variable is not as reliable as the core

[17]Almost all products in our sample were sold in the surveyed hypermarkets, supermarkets and *magasins populaires.* More than 88% of these products were sold in the surveyed convenience stores and more than 44% in hard-discount stores (the range of products is more limited in those stores).

[18]Whenever a store is definitively closed, it is replaced in the sample by a similar store within the same area. Prices may continue to be collected in a store even when it changes fascia or size. We do know whether such changes occur either through the information available in the INSEE dataset or through our store dataset.

variables (such as price, store and product category). Although we do not introduce brand controls in our analysis, we use the brand variable as well as a variable indicating whether the product is a store brand to construct our own store brand variable. We thus classify a product as store brand only when it is categorized as such in the INSEE dataset or when the brand corresponds to the store fascia. Our national brand category thus includes the leading branded products but also all "non-store" brands. Due to this imperfect measurement, we should expect the difference in effects between national brand and store brand to be biased downwards.

Finally, for each store in which at least one price has been collected, the city administrative code has been recovered. We use this code to match the retail price data with our store and local market data.

4.2 Grocery Stores

We created a unique dataset of local stores, used to construct local markets, based on the *"Atlas de la grande distribution"*, a yearly index of grocery stores. This index is a reference for the retailers themselves and is, in principle, exhaustive.[19]

The classification used for stores is the same than for the CPI dataset. We have collected exhaustive data for hypermarkets, supermarkets, hard-discount stores and "*magasins populaires*" from 1993 to 2000.[20] For each store, information includes variables such as type (as defined above), size (selling area in m^2), fascia and location (administrative city code).[21] We do not have more precise information about the exact location within a city, except for the three largest cities (Paris, Marseille and Lyon) for which the *"Arrondissement"* (i.e., district) is known.[22] Table 1 shows the number of stores included in the two datasets, the price (CPI data) and the grocery stores (store data) datasets. As shown in this table, the CPI data covers a significant proportion all stores (especially for larger stores).

Table 1: *Number of Stores in CPI and Grocery Stores Datasets*

		Hyper.	Super.	Hard-disc.	Convenience	Magasins Pop.
1994	Store Data	1001	5947	750	n/a	292
	CPI Data	364	780	79	560	130
1999	Store Data	1120	5806	2164	n/a	307
	CPI Data	495	832	173	355	136

Sources: INSEE (CPI), LSA

[19] The types of stores for which information is collected by this source has - to a limited extent - increased over time. For instance, in 1994, the index did not have specific sections for hard-discounters and "*magasins populaires*", which were treated as supermarkets. Although it is straightforward to recover the proper type knowing a store's fascia, it appears that the index was not exhaustive for these types of stores. We also identified instance where supermarkets located in small towns were initially missing but appeared in subsequent issues of the index with opening dates clearly indicating that they should have been present initially. In those instances, we corrected the data using the stores' opening dates.

[20] Whether the stocks of stores are evaluated at the end of each year rather than at the beginning has no impact on our results.

[21] The administrative city code slightly differs from the ZIP-code: it tends to be more precise for small and medium size cities (some ZIP-codes can include several towns or villages that have separate city codes) but is less precise for larger cities (that can have several ZIP-codes but have a unique city code).

[22] Paris, Marseille and Lyon have 20, 16 and 9 *Arrondissements* respectively.

4.3 Catchment Areas and Proxies for Local Competition

Downstream competition in the grocery market takes place locally and delineating geographic markets is an issue in itself. To construct local markets, we adopt an approach that has been commonly used both in the economic literature and by competition authorities: for each store present in the CPI data, we look for all competing stores (as identified by the store data) within a given radius.[23] Given that we do not know a store's exact location in the city but only the city code, a catchment area will be centred on cities rather than stores. All stores within one particular city code will therefore have the same catchment area.

To construct these catchment areas, we use an INSEE dataset providing cartesian coordinates of city geometric mass centres ("barycenters"). For any city within the CPI sample, we use these coordinates to compute its distance (as the crow flies) to neighbouring cities. Using our store data, we are able to list all stores within each of these cities. For any store in the CPI data, we thus compute the number of stores (by type), the total selling area (by type), as well as a concentration index based on selling areas, within a given radius.

As in **?**, our proxy for local competition is a measure of local market concentration, that is, a Herfindahl-Hirschman index (hereafter, HHI). We focus on market shares based on selling areas rather than on turnover for two reasons. Firstly, we do not know the stores' turnover, but we expect turnover to be strongly correlated with size. Secondly, selling area can be seen as an indicator of a store's medium term capacities since it takes time to increase selling area significantly, due to construction lags and planning restrictions. We thus expect our market concentration indices to be less affected by endogeneity problems (in our price equations) than measures based on turnover.

Table 2 shows the distribution across CPI stores of the number of potentially competing stores within various distances. Stores appear to be seldom in competition with other stores located in the same city. Thus, the market should not be too narrowly defined.[24] Besides, although hypermarkets are likely to attract consumers travelling longer distances,[25] convenience stores are more likely to attract only local consumers.[26] We thus decided to focus on smaller catchment areas including stores within a 10km radius. Alternative distances of 2.5, 5 and 20km have been used as robustness checks. Our central results are robust to this specification, as well to many other.[27] Our catchment areas are therefore smaller than

[23]See for instance European Commission's merger decisions *Kesko / Tuko* and *Carrefour / PromodÃ©s*, as well as the UK Competition Commission's decisions in the *Safeway / Morrisons* merger and in their 2000 and 2007 supermarket inquiries.

[24]Distances between cities is to be understood as between city barycenters. Hence, distances between neighbouring cities will always be strictly positive. In our sample, the median distance between a city and its nearest neighbour is 2.5km. Were we to select 2.5km as a reference to define our catchment areas, half of these areas would be reduced to one city.

[25]For instance, for the largest grocery stores (selling area over 1400 m^2), the UK Competition Commission considers that catchment areas include consumers within 15 minutes driving time.

[26]In a recent inquiry focusing on groceries stores in Paris, the French AutoritÃ© de la Concurrence considered that the catchment areas of smaller stores had a 300 to 500m radius.

[27]Different specifications have also been tried regarding the type of stores to be included or the distances to be used. One

Table 2: *Distribution Across CPI Stores of the Number of Potentially Competing Stores*

		Hyper		Super		Hard Disc.		Magasin Pop.	
		1994	1999	1994	1999	1994	1999	1994	1999
0 km	Q_{25}	0	0	0	0	0	0	0	0
	$Med.$	0	0	1	1	0	0	0	0
	Q_{75}	1	1	2	2	1	1	0	0
	Q_{90}	1	1	4	3	1	3	1	1
5 km	Q_{25}	1	1	3	3	0	1	0	0
	$Med.$	2	2	6	6	1	4	0	0
	Q_{75}	3	3	14	13	3	8	1	1
	Q_{90}	7	7	43	21	7	23	9	9
10 km	Q_{25}	2	2	7	7	1	4	0	0
	$Med.$	4	4	18	18	3	9	1	1
	Q_{75}	7	8	37	34	8	20	2	2
	Q_{90}	18	17	74	67	13	42	14	13
20 km	Q_{25}	3	4	19	18	3	6	1	1
	$Med.$	7	8	41	39	7	20	1	1
	Q_{75}	15	17	75	72	17	44	6	5
	Q_{90}	56	55	396	365	57	223	81	85

Sources: INSEE (CPI), LSA, computations by the authors

those constructed in other studies of local competition. For instance **?** use a distance of 30km, but they focus on very large stores only, while we include much smaller stores. Choosing such a long distance would thus have been excessive in our case.

Finally, we use the 1999 Census data to measure the population living within a catchment area. Tax sources also allow us to construct a measure of household income within that area. Population and income data are available respectively for 1998 and 1999 only. To better control for changes in local demand, we additionally use local unemployment rates available every quarter at the level of the *département*.[28] Table 3 summarizes some statistics our catchment areas.

such alternative consisted in including all *"magasins populaires"* within 5km, all supermarkets and hard-discount stores within 10km, and all hypermarkets within 20km. Even though we expect hypermarkets to have larger catchment areas, especially the largest hypermarkets (more than 10 000 m²), there are relatively few of them. Results obtained with such a specification would therefore not be extremely robust. It would also be more intuitive to weight the store sizes by some function of their distance to the reference store (at least beyond a certain distance). However, we would then need to find an ad-hoc way of weighing, which we think is beyond our goal in this paper. Finally, using a single radius for all stores makes the approach more consistent.

[28]There are 94 *dÃ©partements* in continental France (i.e., excluding Corsica and overseas territories). We use this information on local unemployment variations as a robustness check in our within estimations as well as convergence regressions.

Table 3: *Distribution Across CPI Stores of Market Characteristics*

	HHI (10 km)		Population (log)		Income (log)	
	1994	1999	1994	1999	1994	1999
Q_{25}	0.05	0.04	11.82	11.45	11.35	11.36
$Med.$	0.08	0.08	12.48	12.44	11.46	11.46
$Mean$	0.11	0.11	12.46	12.35	11.46	11.47
Q_{75}	0.14	0.13	13.25	13.25	11.53	11.54
Q_{90}	0.25	0.25	14.38	14.16	11.67	11.71
STD	0.11	0.11	1.37	1.42	0.18	0.17
# Obs.	25994	20243	25994	20243	25994	20243

Sources: INSEE (CPI), LSA, computations by the authors

5 Price and Market Concentration

We now test the prediction that the Loi Galland should have weakened price competition, the effect being larger for national brands than for store brands and for hypermarkets than for other types of stores. Competition in local markets is measured by a concentration index (HHI) based on sales areas and computed over a 10 km radius around each store where prices are observed in the CPI data. Controlling for various other determinants of prices, we expect retail prices to be positively correlated with concentration in local markets and this correlation to have decreased after the Loi Galland.

Let $p_{i,j}^{y,m}$ denote the price (in logarithm) of product i, observed at store j during month m of year y. Let $\theta(i)$ denote the type of product (national brand or store brand), $\varphi(j)$ the type of store (hypermarket, supermarket, etc.) and $c(j)$ the city where store j is located. Our base specification is the following reduced-form price equation:

$$p_{i,j}^{y,m} = \delta^y HHI_{c(j)}^y + \lambda^y X_{c(j)}^y + \alpha_i^{y,m} + \beta_{\theta(i)}^y + \gamma_{\varphi(j)}^y + \varepsilon_{i,j}^{y,m} \qquad (1)$$

where HHI is our Herfindhal-Hirschman index of market concentration and X is a vector of catchment area specifics (e.g., population and income) ; additional covariates are type of store and type of product effects, as well as product×year×month dummies. As catchment areas specifics vary across town×year only, we allow for group effect at the town×year level, so as to recover proper standard errors.[29] In addition to this cross section specification, we also provide a more conservative within estimator obtained by adding fixed store effects (capturing time invariant unobserved store characteristics such as location, quality of management, network effects, market characteristics, etc.) as well as dummies interacting product and store type (capturing the fact that different items may be priced differently in different types of stores).[30] Since our predictions rely on the comparison of the parameters of interest before and after 01/1997, we focus on a balanced panel of stores present across the period 01/1993 to 12/2000, in order to eliminate biases due to different unobserved characteristics of the stores that may have entered or exited the market during the period.[31] The relationship between market concentration and prices

[29] This is done by bootstrapping using clusters by town×year.

[30] The cross-section estimator still has the virtue of keeping the type of store effects apparent.

[31] As robustness check, we also allowed for balanced panels of stores present during shorter periods around 01/1997, that

may have changed over time due to other reasons than regulation changes. For instance, one may expect demand to be more elastic during recessions thus increasing competition intensity. Our analysis amounts to assessing whether the change in the link between market competition and prices after 01/1997 can be considered as a significant break, given the amount of year-to-year variation across the period.

We first estimate the cross-section and within models by restricting the yearly coefficients δ^y to be equal across years before the Loi Galland (δ^0) and equal across years after the Loi Galland (δ^1). Testing our predictions then amounts to comparing the restricted coefficient, i.e., analysing the difference $\triangle = \delta^1 - \delta^0$. This approach simplifies the comparison between the pre- and post-regulation regimes, but it hides the year-to-year variation of δ^y. We therefore also estimate the same models without imposing any restrictions, and carry out post-estimation tests instead.[32] To check the robustness of our results, the post-estimation tests are done for three different periods centred on the Loi Galland (1993-2000, 1994-1999 and 1995-1998).

We proceed in the same way to test the predictions related to store brands and hypermarkets. Compared to equation (1), the only difference is that we now interact the coefficient on concentration (δ^y) alternatively with store brand or hypermarkets dummies (δ^y_θ and δ^y_φ).

The results of the restricted estimations are presented in Table 4. Column (1) provides cross-section estimates for the base specification. Column (2) provides the within estimates for the same specification, whereas columns (3) and (4) provide the within estimates for the specifications related to the store brand and hypermarkets tests respectively (i.e., introduction an interaction between market concentration and a store brand or hypermarket dummy).[33] The complete results of the unconstrained specifications are reported in the Appendix. Table 5 presents a summary of the tests on $\triangle$ derived for the various specifications (constrained / unconstrained, cross-section / within, base / store brand / hypermarkets).

In the cross-section specification, the coefficient of market concentration drops from 0.144 to about 0.063 (see table 4, column (1), HHI). Table 3 shows that the inter-quartile range is about 0.1 for HHI. Comparing local areas from the lower than the upper quartile (Q_{25}/Q_{75}), we thus see that the prices were about 1.4% higher in the more concentrated areas before the Loi Galland and only 0.6% after. The results for the pre-Loi Galland period are in line with previous comparable cross-section estimates for other European countries. For instance, using data on the Portuguese grocery retail industry, **?** find a

is, 1994-1999 and 1995-1998. As our results are robust to these changes, we focus in the presentation on the longest period.

[32] Let δ denote the vector of yearly coefficients δ^y. The restriction on δ takes the form $\delta = g(\delta^0, \triangle)$, where g is defined by:

$$\begin{cases} \delta^y = \delta^0 & \forall y \leq 1996 \\ \delta^y = \delta^0 + \triangle & \forall y \geq 1997 \end{cases}$$

We use asymptotic least squares i.e. minimum distance estimation (**?**, **?**) to recover $\hat{\delta^0}$ and $\hat{\triangle}$ from the unrestricted OLS estimator $\hat{\delta}$ and its estimated covariance matrix $\hat{\Sigma}$, by minimizing the quantity $\left[\hat{\delta} - g(\delta^0, \triangle)\right]' \hat{\Sigma}^{-1} \left[\hat{\delta} - g(\delta^0, \triangle)\right]$ in δ^0 and $\triangle$. Given asymptotic normality of $\hat{\delta}$, the minimized quantity asymptotically follows a χ^2 statistic under the null hypothesis that the restriction is true.

[33] The results for the cross-section estimation equivalent to columns (3) and (4) are presented in the Appendix.

Table 4: *Regression of Retail Prices on Local Market, Store and Product Characteristics*

	Cross Section		Within					
	(1)		(2)		(3)		(4)	
	$\leq$ 1996	$\geq$ 1997	$\leq$ 1996	$\geq$ 1997	$\leq$ 1996	$\geq$ 1997	$\leq$ 1996	$\geq$ 1997
Super	.055	.040	.011	ref.	.011	ref.	.016	ref.
	(.002)	(.002)	(.002)		(.002)		(.003)	
Hard Disc.	-.301	-.332	.034	ref.	.033	ref.	.038	ref.
	(.012)	(.013)	(.008)		(.008)		(.008)	
Mag. Pop.	.081	.089	.000	ref.	.000	ref.	.004	ref.
	(.003)	(.003)	(.003)		(.003)		(.003)	
Convenience	.222	.222	-.003	ref.	-.003	ref.	.002	ref.
	(.004)	(.005)	(.003)		(.003)		(.004)	
Hyper	ref.	ref.	ref.		ref.		ref.	
Store Brand	-.167	-.185	-.167	-.183	-.171	-.191	-.166	-.183
	(.002)	(.002)	(.002)	(.002)	(.002)	(.003)	(.002)	(.002)
Other Brand	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
HHI	.144	.063	.069	.007				
	(.015)	(.015)	(.029)	(.028)				
HHI×StoreB					.097	.047		
					(.030)	(.029)		
HHI×OtherB					.059	-.012		
					(.029)	(.029)		
HHI×Hyper							.104	.004
							(.049)	(.049)
HHI×Other							.066	.010
							(.030)	(.029)
Population	.022	.013	.007	ref.	.007	ref.	.007	ref.
	(.001)	(.001)	(.001)		(.001)		(.001)	
Income	.015	.009	.004	ref.	.004	ref.	.002	ref.
	(.007)	(.009)	(.006)		(.006)		(.006)	
Additional Controls	Prod.×Year×Month		Prod.×Year, Year×Month, Prod.×Store Type, Store effect					
# Obs.	893635	709606	893635	709606	893635	709606	893635	709606

Note: OLS estimator ; standard errors robust to correlation across town×year estimated by bootstrap. Standard errors in parentheses.

Table 5: *Estimated Variation of HHI Coefficients Across LG for Different Estimators and Estimation Periods*

	Constrained		Unconstrained					
	Cr. Sect.	Within	Cr. Sect.			Within		
Est.Period	$93-00$	$93-00$	$93-00$	$94-99$	$95-98$	$93-00$	$94-99$	$95-98$
$\hat{\triangle}$HHI	-.082	-.064	-.085	-.074	-.069	-.060	-.054	-.048
	(.021)	(.012)	(.021)	(.024)	(.029)	(.012)	(.013)	(.015)
$\hat{\triangle}$HHI×Store Brand	-.072	-.051	-.075	-.055	-.049	-.048	-.034	-.027
	(.029)	(.017)	(.029)	(.033)	(.040)	(.017)	(.019)	(.022)
$\hat{\triangle}$HHI×Other Brand	-.087	-.072	-.092	-.083	-.077	-.070	-.065	-.057
	(.021)	(.013)	(.021)	(.024)	(.028)	(.013)	(.014)	(.017)
$\hat{\triangle}$HHI×Hyper	-.142	-.102	-.145	-.122	-.107	-.092	-.079	-.071
	(.032)	(.019)	(.033)	(.037)	(.045)	(.018)	(.019)	(.023)
$\hat{\triangle}$HHI×Other Type	-.074	-.057	-.078	-.069	-.065	-.054	-.047	-.043
	(.022)	(.012)	(.022)	(.026)	(.031)	(.012)	(.013)	(.015)

Note: Post-estimation Asymptotic Least Squares based on Table *** in appendix ; standard errors robust to correlation across town×year estimated by bootstrap.

correlation coefficient of 0.15. **?** find smaller, but comparable, figures for the Swedish market. Looking now at the unconstrained estimations (see Table 9 in the Appendix), we see that there is a clear impact of the Loi Galland in 1997 (the HHI coefficient drops from 0.137 in 1996 to 0.071 in 1997), but that this impact was nevertheless gradual as the coefficient continued to decrease (and is no longer statistically significant) after 1998.[34] Three years after the enactment of the Loi Galland, the correlation between prices and concentration has almost totally vanished. This correlation is relatively stable between 1994 and 1996 and between 1997 and 1999 (following a big drop in 1997). Extreme years are further apart (higher in 1993 and lower in 2000), but both these years are specific, since 1993 corresponds to the end of a long period of recession (one may expect competition intensity to have gone up) and 2000 is characterized by dynamic growth as well as significant mergers (one may except further weakening of competition intensity).

In the within specification reported in columns (2) of Table 4, the coefficient on market concentration before the Loi Galland is much smaller than in the cross section estimation (0.069 instead of 0.144), whereas it is much closer to zero after the Loi Galland (0.007 instead of 0.063).[35] The drop in the coefficient is thus slightly smaller in the within specification (about 0.06 vs 0.08), but of the same order of magnitude and still highly significant. This result holds for every specification (see table 5, △HHI) and seems therefore extremely robust.

Let us now turn to columns (3) and (4) of Table 4, i.e., to the predictions related to store brands and hypermarkets. Before the Loi Galland, the effect of market concentration appears to be larger for store brands (about 0.097) than for other brands (0.059), both coefficients being significant. The two coefficients substantially decrease after the Loi Galland (from 0.097 to 0.047 and from 0.059 to -0.012 respectively), the difference between the pre- and post-Loi Galland coefficient always being statistically significant. The impact of the Loi Galland on the relationship between prices and market concentration appears to be more marked for national brands than for store brands. This result is again robust to the choice of specification (see table 5, △HHI×Store Brand and △HHI×Other Brand).[36]

[34] In the above regressions, we have assumed that the effects of the Loi Galland on retail prices were almost instantaneous (i.e., already visible in 1997). However, there are reasons to believe this may not have been the case and the significance of the coefficient in 1997 could be a consequence in the partial lag of the impact of the new regulation on price formation. The Loi Galland was passed in July 1996 but only came into force on January 1^{st}, 1997. Since the negotiations on prices between manufacturers and retailers for year $n+1$ usually start in the last months of year n, we may expect most of the effects to take place in the wake of these negotiations. However, some of these negotiations may have lasted until February, while others may have finished before the end of 1996, so that the change in retail prices over January 1997 may be smoothed out to some extent. There seems to be some substance in this argument, but this effect remains marginal. For instance, removing the last three months of 1996 and the first three months of 1997, in the within store regression we find that the coefficient on HHI goes up from .063 to .070 in 1996, and down from 0.026 to 0.022 in 1997.

[35] The fact that these coefficients are smaller in the within specification may point to measurement errors on market concentration (attenuation bias), or alternatively suggest the existence of a correlation between unobserved time-invariant determinants of prices and market concentration. One possible interpretation is that continuing stores operating in more competitive markets are more efficient (through selection effects), therefore have lower distribution costs and lower prices.

[36] Excluding extreme years (1993 and 2000) i.e. keeping the test more closely centered around the Loi Galland reinforces the conclusion : across the 1994-1999 or 1995-1998 periods, only the change in coefficient of other brands remains significant,

Before the Loi Galland, the relationship between prices and market concentration appeared to be much higher for hypermarkets than for other store types (0.104 compared to 0.066). This correlation decreases substantially after 1997 for all types of stores, but the impact of the Loi Galland is, as predicted, much larger for hypermarkets than for other types of stores, the change in the coefficient being nearly twice as large for hypermarkets in all specifications (see table 5, △HHI×Hyper and △HHI×Other Types).

We have focused so far on the main predictions related to market concentration. We now comment the effect of the other covariates. Not surprisingly store brands are substantially cheaper than other brands (by about 17-18% across the period) and hard-discount stores are by far the cheapest stores, before hypermarkets, supermarkets and convenience stores. It turns out that the price gap between hypermarkets and supermarkets has been reduced after the Loi Galland (from 5.5% to 4.0%), which is consistent with the prediction that prices should have converged upwards (additional evidence is provided in the dedicated analysis of section 6.2). Moreover, the price gap between hypermarkets and hard-discount stores has increased (from 30% to 33%), confirming the expectation that hard-discount stores have not been directly affected by the Loi Galland.

Finally, our results suggest that the effect of market population also decreased during the period. The fact that prices are positively correlated with population density[37], may be the consequence of many unobserved characteristics, such as higher transportation costs for customers due to congestion, higher quality or higher land prices. The decrease in correlation during such a short period is nevertheless consistent with price convergence following the enactment of the Loi Galland.[38]

Overall, our analysis consistently shows that the correlation between prices and market concentration has significantly decreased after the implementation of the new regulation, confirming the view that the new price thresholds might have been manipulated to act as de facto industry-wide minimum RPM.

6 Price Dispersion and Price Convergence Across Stores

Our economic analysis of the Loi Galland leads us to expect that price dispersion should have decreased after the enactment of the Loi Galland, especially for branded products. We now test this prediction. We first provide a straightforward although relatively crude test, before turning to a more general quantile regression approach.

6.1 Price dispersion

We start by computing the dispersion of the logarithm of prices within cells defined by product×year. For each cell, we compute three measures of price dispersion, the standard deviation, the interquartile range ($Q_{75}-Q_{25}$) and the interdecile range ($Q_{90}-Q_{10}$). We then run OLS regressions of these measures of price dispersion on a Loi Galland dummy, clustered by products. While we first keep all observations, we then

with a point estimate twice as large as that of store brands.

[37]The size of the catchment areas being fixed, total population is thus a proxy for population density.

[38]The catchment area characteristics are time invariant and are 1999 values. This can only reinforce our results since we should expect the 1999 coefficient to be more precisely estimated than the 1994 coefficient.

do the same analysis excluding store brands. The results are reported in table 6. In the first regression based on all observations, the Loi Galland dummy (i.e. year$\geq$ 1997) is never significant. In the second regression excluding store brands, however, the Loi Galland Dummy is significant for all three measures of dispersion. This suggests a drop in price dispersion after 01/1997 for branded products. This result is robust to the way cells are constructed (product$\times$year, product$\times$year$\times$month, or product$\times$year$\times$store type) as well as to the controls used. Obviously, however, this approach is crude because it does not allow controlling for other determinants of price dispersion, which may have changed in the sample over time. We therefore turn to the more general method of quantile regression to model the change of price dispersion over time.

Table 6: *Change in Mean Dispersion Across Products*

	All Brands			Excl. Store Brands		
	Std. Dev.	$Q_{75}-Q_{25}$	$Q_{90}-Q_{10}$	Std. Dev.	$Q_{75}-Q_{25}$	$Q_{90}-Q_{10}$
After LG	-.001	.002	-.014	-.009	-.022	-.031
	(.003)	(.006)	(.010)	(.003)	(.007)	(.010)
Before LG	ref.	ref.	ref.	ref.	ref.	ref.
Prod.	Y	Y	Y	Y	Y	Y
# Obs. (cells)	1525	1525	1525	1525	1525	1525

Note: OLS estimator ; clusters by product. Standard errors in parentheses.

Instead of modelling the conditional expectation of price as a function of the covariates on the right hand side of equation (1), we now model the quantiles Q_{25} and Q_{75}, by means of quantile regression. The effect of each covariate on price dispersion is obtained as the difference between its effects on Q_{25} and Q_{75}.[39] Since the pricing regime of branded products should have been affected by the Loi Galland - in the sense of a reduction in price dispersion - whereas store brands should not have been directly affected, we expect the price dispersion of branded products to have decreased after the Loi Galland relative to that of store brand products. Therefore, the coefficient of store brand (relative to national brand) in the interquartile regression should go up after the Loi Galland.

Table 7: *Store Brand Effect on Price Dispersion Across the Loi Galland*

	93-00	94-99	95-98
Store brand before LG $\left(\hat{\beta}^0\right)$	-.006	-.005	-.001
	(.002)	(.002)	(.003)
Store brand change after LG $\left(\hat{\triangle}\right)$	.025	.021	.014
	(.003)	(.004)	(.005)

Note: Post-estimation Asymptotic Least Squares based on Table 15 in appendix; standard errors in parentheses.

Because of the high computational cost of estimation[40], we have to use the unconstrained approach

[39]We use bootstrap to compute standard errors. The bootstrap is done by drawing with replacement from a population of town$\times$year, as we want to account for the fact that stores located within a given market may be subject to common shocks. For each draw, we construct the effect of each covariate of interest on the interquartile range of prices as the difference between the effects on Q_{25} and Q_{75}.

[40]Due to the method of quantile regression itself, as well as the large size of the dataset and the large number of control

(see previous section) rather than the restricted one, as it allows us to run separate regressions for each year (thereby limiting for each estimation the size of the data and the number of dummies). As before, we use a test based on minimum distance post-estimation of mean coefficients before/after the Loi Galland. The full results are presented in the appendix, in table 15. We report below the result of the post-estimation test on the change in store brand coefficient, for three different estimation periods surrounding the Loi Galland. Our regressions show that prior to the Loi Galland, prices for store brand items had, if any, a slightly smaller dispersion. However, controlling for all the factors in equation (1), this difference of dispersions increases by about 0.02 after the Loi Galland, which is highly significant. This confirms the expected relative reduction in price dispersion for branded products, which can be a consequence of minimum RPM but cannot be explained by local or national shocks of costs or demand, or by the Loi Raffarin.[41]

However, this pattern could have been generated by different types of responses to the new regulation. The ban on below cost pricing should first have led firms to give up or at least attenuate the use of loss-leader strategies (i.e., pricing some products aggressively, often below cost, in order to attract customers into the stores, and compensating the loss by higher prices on other products). The limitation of such strategies, mechanically implied by the Loi Galland, should have generated within store decrease in price dispersion: all prices and margins should then be leveled. Alternatively, initial price dispersion may have been generated by the coexistence in the economy of low-price stores (i.e. stores pricing aggressively for all products) as well as high-price stores. Under this scenario, price dispersion occurs due to between stores - or between local markets - heterogeneity. This second mechanism is more directly related to our analysis of the Loi Galland, as it captures the predicted relaxation of intrabrand competition. The quantile regression framework alone does not allow to discriminate between the two mechanisms.[42] It is then necessary to complement the above analysis of price dispersion by an analysis of *price convergence between stores*, across the Loi Galland. One benefit from this move to store level prices is that aggregation of individual prices should smooth out within store price dispersion generated by loss-leader strategies, thus allowing to focus on competition effects.

dummies to be estimated, since they can no longer be absorbed in the quantile regression framework.

[41]To comment the significance of this increase by about 2 percentage points in the relative price dispersion of store brands, we proceed as follows. We first compute the interquartile range of log-prices within cells define by crossing product and store type. The median (the mean) across cells of this interquartile range is about 0.15 (0.17) for branded products before the Loi Galland. Alternatively, we run the cross section equation (1) and compute the interquartile range of its distribution, which is again about 0.16. The 2 point relative increase in price dispersion after the Loi Galland is therefore by no means negligible compared to initial dispersion.

[42]The first reason is data limitation : the quality of the brand variable is poor because it is not part of the main variables of interest used for the computation of the CPI (we only use it to complement the store brand dummy). The second reason is computational : in order to control for brand, one would have to estimate a large number of brand dummies, in addition to the product dummies. Controlling for brand would require different data, focused on fewer products and brands at the cost of sample representativeness.

6.2 Store level analysis: price convergence after the Loi Galland

To aggregate the price data at the store level, we regress (log) prices on a set of dummies of product characteristics[43]. For a given store in a given year, we compute a yearly "store price" as the mean of the residual across products and months.[44] Store prices thus reflect the price component driven by store characteristics and local market conditions.

We model store prices as the sum of an observed component - explained by store and local market characteristics - and a stochastic shock reflecting unobserved supply and demand characteristics as well as measurement errors. Store prices changes between any two dates *prior to the Loi Galland* can thus be written as:

$$\triangle^0 p = \beta \times \triangle^0 x + \triangle^0 \varepsilon \tag{2}$$

where $\triangle^0$ denotes a difference taken across a "normal" period, i.e., where no change in the regime of price determination occurs, x denotes observable covariates such as supply and demand determinants of prices and ε unobserved shocks (on supply and demand). By contrast, store price changes between two dates *spanning the Loi Galland* are modelled as:

$$\triangle^1 p = \delta \times p^0 + \beta \times \triangle^1 x + \triangle^1 \varepsilon \tag{3}$$

Where $\triangle^1$ denotes a difference taken across the Loi Galland, i.e., across a period where a change in the regime of price determination takes place (in the sense of a relaxation of competition for a given market structure). The presence of p^0 captures this change of regime, which takes the form of a convergence between initially low-price and high-price stores. δ is expected to be negative, so that higher (positive) values of initial price induce a lower price change, whereas lower (negative) values induce a higher price change.[45] δ is the main parameter of interest. One can think of p^0 as the mean store price computed over all dates prior to the Loi Galland. However, depending on the stochastic properties of ε, p^0 may be endogenous in equation (3), generating spurious price convergence irrespective of any change of regime due to the Loi Galland. This is obviously the case if p^0 includes the price at the beginning of the $\triangle^1$ period (for instance the 1994 store price if the difference is taken over the 1994 to 1998 period, see appendix). But even if we exclude, in this case, the 1994 price from the computation of p^0, the endogeneity issue may survive under serial correlation of ε. p^0 must therefore be instrumented in an appropriate way depending on the serial correlation properties of unobserved price shocks.[46] If ε is serially correlated without having a trend, we instrument p^0 in equation (3) by the price at the middle point of the interval corresponding to $\triangle^1 p$ (1996 if the difference is taken between 1994 and 1998, see appendix). We call this estimator "difference" estimator. If ε has a trend, this method no longer works and we differentiate the model by

[43] Product×year×month dummies as well as product type dummy.

[44] We either compute this mean across all products, or across branded products only.

[45] Recall that store prices are computed as mean residuals.

[46] For instance, such serial correlation may occur if stores do not instantly know the magnitude of demand or supply shocks, or the responses of competitors and consumers.

substracting equation (2) from equation (3), which provides a "difference-in-differences" estimator.[47] As a final robustness check, we propose a third estimator allowing us to evaluate the magnitude of shocks' serial correlation. To capture such serial correlation in a tractable way, we assume a simple $AR(1)$ process for ε (with yearly autoregressive coefficient ρ and innovation ν). We define $\triangle^0$ and $\triangle^1$ over two-year periods. Substracting $\rho^2 \times (2)$ from (3) we obtain a dynamic model of price change where only the white noise ν appears in the residual. However, both the lagged price change $\triangle^0 p$ and p^0 are endogenous in this equation. Our best candidates to instrument both variables are the same appropriately lagged price levels, which results in poor identification. To circumvent this problem, we use a two-step estimation strategy. We first estimate the dynamic model of price change over a period set entirely prior to the Loi Galland. Since the p^0 term does not appear in this equation, ρ^2 can be estimated. We then use this estimate of ρ^2 (assumed to be constant over time) to compute a second stage estimating equation, obtained by substracting $\hat{\rho^2} \times (2)$ from (3). The δ parameter can then be estimated by IV, in a way similar to the difference-in-differences approach (see appendix for details).

The three tests of convergence are shown in table 8, first using the whole sample of observations to compute store prices, then excluding store brand products. The first estimate, noted Diff., corresponds to the difference estimator, the second, Diff.Diff., to the difference-in-differences, and the last one, noted $AR(1)$, to the two-step procedure.

Table 8: *Test of Price Convergence Across Stores*

	All Brands			Excl. Store Brands		
	Diff.	Diff.Diff.	AR(1)	Diff.	Diff.Diff.	AR(1)
AR coef. ρ^2			.282			.178
			(.092)			(.071)
Initial Price	-.105	-.115	-.145	-.133	-.132	-.175
	(.044)	(.038)	(.042)	(.045)	(.047)	(.039)
Super.	-.005	-.005	-.001	-.009	-.005	-.003
	(.004)	(.004)	(.004)	(.005)	(.005)	(.004)
Hard Disc.	-.022	-.089	-.148	-.045	-.135	-.241
	(.026)	(.031)	(.060)	(.047)	(.042)	(.057)
Mag. Pop.	.000	-.010	.001	-.010	.011	.013
	(.009)	(.011)	(.008)	(.010)	(.012)	(.008)
Convenience	.042	.003	.022	.048	.012	.027
	(.011)	(.011)	(.011)	(.013)	(.013)	(.012)
Hyper.	ref.	ref.	ref.	ref.	ref.	ref.
HHI	-.015	-.079	-.082	.014	-.070	-.024
	(.079)	(.092)	(.124)	(.096)	(.123)	(.187)
Unempl.	-.003	-.006	.001	-.006	-.005	-.007
	(.004)	(.005)	(.005)	(.004)	(.006)	(.006)
# Obs.	1327	1391	1273	1257	1322	1224

Note: The Diff. and Diff.Diff. columns are IV estimators ; the AR(1) column corresponds to the two-step estimator (see Appendix B.2). Standard errors in parentheses. Standard errors are robust to correlation across town (estimated by bootstrap).

The results of all three estimations are consistent with the prediction that relative prices have in-

[47] Such a trend might occur due to any unobserved factor making markets gradually more competitive, or affecting local demand.

creased more in initially cheaper stores (i.e., negative coefficient for the variable *Initial price*). The coefficient of convergence coefficient lies between 0.10 and 0.15 then all brands are used to compute store effects. The individual trend hypothesis does not seem to be supported by the data, since the estimated AR coefficient is much lower than 1. However, in all regressions presented in table 8, store type dummies are included in order to control for different trends across store types, possibly correlated with initial prices. Indeed, hard discounters appear to be less affected by the Loi Galland than other types, a result that should be expected since they mainly distribute store brands. Omitting this control would bias our estimates, since hard discounters are also by far the cheapest stores.

Restricting the estimation of store effects to national brands (i.e., excluding store brands) yields higher convergence coefficients, lying between 0.13 and 0.18. This is again consistent with the fact that the magnitude of convergence should be higher for branded products than for store products. These predictions are not only extremely robust, they are also consistent with the idea that the Loi Galland induced the manufacturers to impose de facto industry-wide price floors. Moreover, they would not be explained by national or local cost or demand shocks, nor by planning restrictions.

7 Conclusion

Using a unique dataset on retail prices for a large number of products, collected in a large and representative sample of grocery stores, this paper provides an empirical evaluation of the effects of the 1996 below-cost pricing regulation (Loi Galland). We find evidence supporting the claim that the Loi Galland effectively led a substantial reduction in (if not the elimination of) intra-brand competition and is likely to have been partially responsible for the sharp increase in prices of groceries observed after 1997. We provide three different empirical tests. Firstly, we look at the correlation between retail prices and the level of concentration on the various local markets. We find that retail prices were initially significantly lower in less concentrated markets. Before the implementation of the Loi Galland, the magnitude of the correlation is also consistent with previous analysis conducted on the same sector in other European countries. The new legislation led to a sharp drop in this correlation confirming that retail chains were no longer competing fiercely. This attenuation in the link between retail prices and local competition is also larger for branded products than for store brands, as predicted by the theoretical analysis of the legislation. Secondly, we find a reduction in the price dispersion of branded products relative to store brands, consistent with the fact that only branded products were directly affected by the Loi Galland, store brands being only indirectly affected through strategic complementarity between prices. Finally, we find evidence that some price convergence has been taking place across stores after the Loi Galland. Although this kind of evidence is always difficult to establish, our results are robust to various specifications.

However, this paper does not provide a complete evaluation of the effects of the Loi Galland. It explains the mechanism through which this regulation has relaxed intra-brand competition and led to higher prices, and finds strong empirical support to this argument. However, the legislation was driven by a will to level the playing field between small businesses and large retail chains. The Loi Galland might have partially achieved this objective by eliminating part of the price disadvantages faced by small village shops. Anecdotal evidence suggests that this had a rather limited impact and it has been made possible by inducing large stores to increase their prices. Many convenience stores that were previously independent also became part of retail chains (either because they have been taken over by these chains or because they joined them as franchisees).[48] Moreover there is no strong evidence that the rate of closure of independent specialized shops has slowed down after 1997. Fiscal data suggests that the number of small traditional retail businesses such as bakeries, butchers and fishmongers has steadily decreased over the period, although perhaps at a decreasing rate after 2000. There also seems to have been during the same period a renewed interest from consumers for shopping locally and for quality products. This has led large chains to invest more in convenience stores located in city centers and may also have supported the survival of local high quality craftsmen or specialized retailers. It is therefore very difficult to assess the particular role of the Loi Galland in achieving its initial aim. This would in any case constitute a research question on its own. Nevertheless, one should remember that a problem is more efficiently solved by addressing directly the issue. Thus, even if they did have an impact in this direction, price regulations are unlikely to constitute the best policy to help the few remaining rural shops or the independent specialized shops.

Finally, this paper confirms that resale price maintenance or industry-wide price floors can be detrimental to final customers. However, this paper does not intend to take a position in the debate on whether resale price maintenance is overall more often pro- or anti-competitive. Even though there is no evidence that the general quality of services offered by grocery stores to customers has increased in such way that it offsets the detrimental impact of the price increase, we cannot exclude that it could be the case in a different context.

[48]Less than 15% of small grocery retailers are truly independent (i.e., are not subsidiaries or franchisees of the main national chains).

References

A Unconstrained Cross-Sections Regressions

Table 9: *Cross-Section Regression of Retail Prices on Local Market, Store and Product Characteristics*

	1993	1994	1995	1996	1997	1998	1999	2000
Super	.057	.055	.054	.051	.046	.039	.037	.035
	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)
Hard Disc.	-.263	-.312	-.302	-.302	-.322	-.312	-.327	-.367
	(.044)	(.027)	(.020)	(.017)	(.021)	(.024)	(.026)	(.029)
Mag. Pop.	.080	.082	.081	.080	.088	.088	.089	.092
	(.007)	(.006)	(.007)	(.006)	(.007)	(.006)	(.006)	(.005)
Convenience	.224	.216	.223	.223	.220	.222	.228	.219
	(.009)	(.008)	(.009)	(.009)	(.009)	(.010)	(.010)	(.010)
Hyper	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
Store Brand	-.171	-.171	-.166	-.160	-.175	-.185	-.189	-.195
	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)
Other Brand	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
HHI	.160	.144	.133	.137	.071	.059	.058	.038
	(.032)	(.029)	(.032)	(.027)	(.025)	(.032)	(.037)	(.032)
Population	.024	.023	.022	.021	.015	.013	.014	.010
	(.003)	(.003)	(.003)	(.003)	(.002)	(.002)	(.003)	(.003)
Income	.022	.021	.007	.010	.005	.018	.004	.009
	(.015)	(.013)	(.014)	(.017)	(.017)	(.015)	(.016)	(.017)
Add. Controls	Prod.×Year×Month							
# Obs.	227711	234450	231335	200139	214963	166487	165399	162757

Note: OLS estimator ; standard errors robust to correlation across town×year estimated by bootstrap. Standard errors in parentheses

Table 10: *Cross-Section Regression of Retail Prices on Local Market, Store and Product Characteristics*

	1993	1994	1995	1996	1997	1998	1999	2000
Super	.057	.055	.054	.051	.046	.039	.037	.035
	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)
Hard Disc.	-.263	-.312	-.302	-.301	-.321	-.311	-.326	-.367
	(.044)	(.027)	(.020)	(.018)	(.021)	(.024)	(.026)	(.029)
Mag. Pop.	.080	.082	.081	.080	.088	.088	.089	.092
	(.007)	(.006)	(.007)	(.006)	(.007)	(.006)	(.006)	(.005)
Convenience	.224	.216	.223	.223	.220	.221	.227	.219
	(.009)	(.008)	(.009)	(.009)	(.009)	(.010)	(.010)	(.010)
Hyper	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
Store Brand	-.174	-.172	-.166	-.163	-.179	-.191	-.193	-.197
	(.005)	(.005)	(.005)	(.006)	(.005)	(.006)	(.006)	(.006)
Other Brand	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
HHI×StoreB	.183	.150	.137	.151	.096	.092	.080	.050
	(.040)	(.041)	(.037)	(.039)	(.037)	(.045)	(.046)	(.043)
HHI×OtherB	.154	.142	.131	.132	.062	.044	.047	.032
	(.032)	(.028)	(.033)	(.026)	(.024)	(.031)	(.038)	(.034)
Population	.024	.023	.022	.021	.015	.013	.013	.010
	(.003)	(.003)	(.003)	(.003)	(.002)	(.003)	(.003)	(.003)
Income	.021	.021	.007	.010	.005	.018	.004	.009
	(.014)	(.013)	(.014)	(.017)	(.017)	(.015)	(.016)	(.017)
Add. Controls				Prod.×Year×Month				
# Obs.	227711	234450	231335	200139	214963	166487	165399	162757

Note: OLS estimator ; standard errors robust to correlation across town×year estimated by bootstrap. Standard errors in parentheses

Table 11: *Cross-Section Regression of Retail Prices on Local Market, Store and Product Characteristics*

	1993	1994	1995	1996	1997	1998	1999	2000
Super	.067	.062	.061	.060	.052	.040	.035	.033
	(.007)	(.006)	(.006)	(.006)	(.006)	(.005)	(.005)	(.005)
Hard Disc.	-.253	-.306	-.296	-.294	-.317	-.311	-.329	-.369
	(.044)	(.027)	(.020)	(.018)	(.022)	(.024)	(.026)	(.029)
Mag. Pop.	.089	.087	.086	.087	.093	.089	.087	.091
	(.008)	(.007)	(.008)	(.007)	(.007)	(.007)	(.007)	(.006)
Convenience	.235	.222	.230	.232	.227	.223	.225	.218
	(.011)	(.010)	(.010)	(.010)	(.010)	(.011)	(.011)	(.011)
Hyper	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
Store Brand	-.170	-.171	-.166	-.160	-.175	-.185	-.189	-.195
	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)
Other Brand	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
HHI×Hyper	.253	.200	.195	.207	.119	.068	.046	.025
	(.057)	(.049)	(.048)	(.045)	(.044)	(.047)	(.049)	(.047)
HHI×Other	.149	.137	.124	.127	.063	.057	.063	.041
	(.032)	(.031)	(.033)	(.028)	(.026)	(.033)	(.037)	(.033)
Population	.025	.023	.023	.022	.015	.013	.013	.010
	(.003)	(.003)	(.003)	(.003)	(.002)	(.003)	(.003)	(.003)
Income	.019	.019	.005	.008	.004	.018	.005	.010
	(.014)	(.013)	(.014)	(.017)	(.017)	(.015)	(.016)	(.018)
Add. Controls				Prod.×Year×Month				
# Obs.	227711	234450	231335	200139	214963	166487	165399	162757

Note: OLS estimator ; standard errors robust to correlation across town×year estimated by bootstrap. Standard errors in parentheses

Table 12: *Within Store Regression of Retail Prices on Local Market, Store and Product Characteristics*

	1993	1994	1995	1996	1997	1998	1999	2000
Super	.016	.016	.015	.014	.010	.003	.001	ref.
	(.004)	(.004)	(.004)	(.003)	(.003)	(.003)	(.004)	
Hard Disc.	.062	.056	.075	.066	.046	.053	.031	ref.
	(.019)	(.014)	(.012)	(.012)	(.012)	(.013)	(.011)	
Mag. Pop.	-.008	-.001	-.003	-.004	-.003	-.006	-.006	ref.
	(.007)	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)	
Convenience	-.007	-.006	.004	.002	.000	.000	.005	ref.
	(.007)	(.006)	(.006)	(.006)	(.006)	(.006)	(.007)	
Hyper	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
Store Brand	-.170	-.170	-.165	-.160	-.173	-.183	-.187	-.193
	(.003)	(.003)	(.003)	(.003)	(.003)	(.004)	(.004)	(.004)
Other Brand	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
HHI	.086	.078	.067	.070	.028	.011	.011	-.016
	(.031)	(.029)	(.031)	(.030)	(.028)	(.029)	(.031)	(.032)
Population	.011	.010	.010	.009	.005	.003	.003	ref.
	(.003)	(.002)	(.002)	(.002)	(.002)	(.002)	(.003)	
Income	.013	.011	-.004	-.003	-.004	.008	-.001	ref.
	(.014)	(.013)	(.013)	(.013)	(.013)	(.013)	(.014)	
Add. Controls	Prod.×Year, Year×Month, Prod.×Store Type, Store effect							
# Obs.	227711	234450	231335	200139	214963	166487	165399	162757

Note: OLS estimator ; standard errors robust to correlation across town×year estimated by bootstrap. Standard errors in parentheses

Table 13: *Within Store Regression of Retail Prices on Local Market, Store and Product Characteristics*

	1993	1994	1995	1996	1997	1998	1999	2000
Super	.016	.016	.015	.014	.010	.003	.001	ref.
	(.004)	(.004)	(.004)	(.003)	(.003)	(.003)	(.004)	
Hard Disc.	.061	.055	.074	.066	.046	.054	.031	ref.
	(.019)	(.014)	(.012)	(.012)	(.012)	(.013)	(.012)	
Mag. Pop.	-.008	-.001	-.003	-.004	-.003	-.006	-.006	ref.
	(.007)	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)	
Convenience	-.007	-.006	.003	.002	-.001	.000	.005	ref.
	(.007)	(.006)	(.006)	(.006)	(.006)	(.006)	(.007)	
Hyper	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
Store Brand	-.175	-.174	-.169	-.164	-.180	-.193	-.195	-.199
	(.004)	(.004)	(.004)	(.005)	(.004)	(.005)	(.005)	(.006)
Other Brand	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
HHI×StoreB	.123	.102	.089	.098	.069	.063	.053	.015
	(.035)	(.034)	(.034)	(.035)	(.032)	(.035)	(.036)	(.038)
HHI×OtherB	.074	.070	.058	.058	.012	-.013	-.011	-.033
	(.031)	(.030)	(.031)	(.031)	(.029)	(.030)	(.033)	(.035)
Population	.011	.010	.010	.009	.005	.003	.003	ref.
	(.003)	(.002)	(.002)	(.002)	(.002)	(.002)	(.003)	
Income	.012	.011	-.004	-.003	-.004	.008	-.001	ref.
	(.013)	(.013)	(.013)	(.013)	(.013)	(.013)	(.014)	
Add. Controls	Prod.×Year, Year×Month, Prod.×Store Type, Store effect							
# Obs.	227711	234450	231335	200139	214963	166487	165399	162757

Note: OLS estimator ; standard errors robust to correlation across town×year estimated by bootstrap. Standard errors in parentheses

Table 14: *Within Store Regression of Retail Prices on Local Market, Store and Product Characteristics*

	1993	1994	1995	1996	1997	1998	1999	2000
Super	.024	.021	.021	.021	.015	.005	.000	ref.
	(.006)	(.006)	(.005)	(.005)	(.005)	(.005)	(.006)	
Hard Disc.	.069	.060	.080	.072	.051	.055	.030	ref.
	(.019)	(.014)	(.013)	(.013)	(.013)	(.013)	(.012)	
Mag. Pop.	-.001	.003	.002	.002	.001	-.005	-.007	ref.
	(.007)	(.007)	(.007)	(.007)	(.006)	(.007)	(.007)	
Convenience	.002	-.001	.010	.009	.005	.002	.004	ref.
	(.008)	(.008)	(.007)	(.008)	(.008)	(.008)	(.008)	
Hyper	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
Store Brand	-.170	-.170	-.165	-.160	-.173	-.184	-.187	-.193
	(.003)	(.003)	(.003)	(.003)	(.003)	(.004)	(.004)	(.004)
Other Brand	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
HHI×Hyper	.131	.095	.094	.105	.046	.005	-.015	-.036
	(.054)	(.054)	(.055)	(.058)	(.054)	(.054)	(.055)	(.064)
HHI×Other	.081	.078	.065	.066	.027	.016	.021	-.009
	(.031)	(.030)	(.031)	(.030)	(.029)	(.030)	(.031)	(.032)
Population	.012	.011	.010	.010	.005	.003	.003	ref.
	(.003)	(.002)	(.002)	(.002)	(.002)	(.002)	(.003)	
Income	.010	.009	-.006	-.005	-.006	.008	-.001	ref.
	(.014)	(.013)	(.013)	(.013)	(.013)	(.013)	(.014)	
Add. Controls	Prod.×Year, Year×Month, Prod.×Store Type, Store effect							
# Obs.	227711	234450	231335	200139	214963	166487	165399	162757

Note: OLS estimator ; standard errors robust to correlation across town×year estimated by bootstrap. Standard errors in parentheses

B Price Dispersion

B.1 Price Dispersion

Table 15: *Interquartile Regression of Retail Prices on Local Market, Store and Product Characteristics*

	1993	1994	1995	1996	1997	1998	1999	2000
Super	.000	-.003	-.002	-.001	-.003	-.005	-.002	.002
	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)
Hard Disc.	.200	.166	.148	.150	.154	.133	.131	.204
	(.110)	(.035)	(.025)	(.022)	(.026)	(.037)	(.035)	(.038)
Mag. Pop.	.002	-.014	-.008	-.004	-.003	-.005	-.009	-.010
	(.006)	(.004)	(.005)	(.006)	(.005)	(.005)	(.005)	(.005)
Convenience	.065	.057	.063	.080	.079	.080	.081	.079
	(.010)	(.010)	(.010)	(.011)	(.011)	(.012)	(.012)	(.011)
Hyper	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
Store Brand	-.010	-.009	-.001	-.001	.010	.017	.022	.027
	(.004)	(.004)	(.004)	(.005)	(.005)	(.005)	(.005)	(.004)
Other Brand	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
HHI	.030	-.010	-.018	.019	-.009	.010	.013	.011
	(.029)	(.024)	(.023)	(.025)	(.024)	(.026)	(.023)	(.027)
Population	.001	-.001	.000	.002	.000	.000	.001	.000
	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)
Income	.027	.023	.020	.021	.021	.023	.020	.017
	(.011)	(.010)	(.009)	(.011)	(.009)	(.011)	(.010)	(.009)
Add. Controls	Prod.×Year, Year×Month							
# Obs.	227711	234450	231335	200139	214963	166487	165399	162757

Note: OLS estimator ; standard errors robust to correlation across town×year estimated by bootstrap. Standard errors in parentheses

B.2 Price Convergence

At any date prior to the Loi Galland (i.e. before 1997), the store price is modelled as a linear function of a set of observed covariates x and an unobserved shock ξ, classically decomposed into a time-invariant store level shock α and a time-varying idiosyncratic shock ε (which may be serially correlated). The price change between any two dates before the Loi Galland is then (omitting store subscripts):

$$\triangle^0 p = \beta \times \triangle^0 x + \triangle^0 \varepsilon \tag{4}$$

where $\triangle^0$ denotes a difference taken before the Loi Galland. At any date following the Loi Galland, we capture the price increase (decrease) in initially cheaper (more expensive) stores through an additional term $\delta \times p^0$, where p^0 is the mean store price across all dates before the Loi Galland. The price change equation between any two dates around the date of enforcement of the Loi Galland therefore takes the form of a convergence equation :

$$\triangle^1 p = \delta \times p^0 + \beta \times \triangle^1 x + \triangle^1 \varepsilon \tag{5}$$

where $\triangle^1$ denotes a difference taken across the implementation of the Loi Galland. Recall that store prices are computed as mean residuals (across products for a given store) from a regression of log-prices on product characteristics. Thus, p^0 can be positive (initially high price store) or negative (initially low price store), so that the convergence coefficient δ should be negative. Other explanatory variables include the change in market concentration, the change in the local rate of unemployment (a proxy for local demand), as well as store type dummies capturing specific price trends. We treat these variables as exogenous in the short run.[49] Indeed, store types are approximately time-invariant for continuing stores. Although they may be correlated with the individual effects α, they should not be correlated with ε. Due to construction lags and planning regulation procedures (reinforced from 1996 on by the Loi Raffarin), market concentration (in terms of selling area) cannot rapidly adjust downwards through entry. It could increase following a large enough shock to drive some stores out of the market, but this seems unlikely in the short run. Similarly, we do not expect significant short run feedback from price shocks to the local unemployment rate (computed across all industries, at the level of the *département*). For robustness, however, we relax the strong exogeneity assumption (x uncorrelated with ε at any date) by instrumenting the change in market concentration, the change in unemployment rate and the store type, by lagged levels of these variables under the weaker assumption that x can be correlated with past shocks only (i.e. x predetermined as opposed to strongly exogenous). The estimate of our main parameter of interest δ is then unchanged. In fact, apart from store types, the x have little explanatory power in the price change equations. We therefore focus on the endogeneity of p^0, the most likely source of bias. We discuss instrumentation strategies in relation with the serial correlation of ε.

B.2.1 Difference estimator

Assume $\triangle^1 p = p^{1998} - p^{1994}$. If ε is not serially correlated, p^0 is endogenous in the convergence equation (through p^{1994}) because ε^{1994} appears both in p^{1994} and in the equation residual $\triangle^1\varepsilon = \varepsilon^{1998} - \varepsilon^{1994}$. p^0 can then be instrumented by all store prices available at any date strictly before the Loi Galland, excluding p^{1994}. If ε is serially correlated, p^0 can be instrumented by the store price at the midpoint of the $\triangle^1$ interval (i.e. p^{1996}) provided the process from which the store level shocks ε are drawn is covariance stationary (i.e. the covariance between ε at any two dates only depends on the length of time between these dates). Under this assumption, $E\left[\varepsilon^{y+n} \times \varepsilon^y\right] = E\left[\varepsilon^{y-n} \times \varepsilon^y\right]$ for any n, so that ε^{1996} has the same covariance with ε^{1994} and ε^{1998}, and p^{1996} is uncorrelated with $\triangle^1\varepsilon = \varepsilon^{1998} - \varepsilon^{1994}$.[50] In our estimation, we use the following "difference" estimator (denoted Diff.) :

$$\triangle^1 p = Mean\left(p^{1998}, p^{1999}\right) - Mean\left(p^{1993}, p^{1994}\right)$$

where we average out store prices (as well as x) at both ends of the interval in order to smooth out measurement errors (it turns out that using the simple difference $\triangle^1 p = p^{1998} - p^{1994}$ yields essentially

[49] Conditional on the individual effect α, which has been removed by differencing the price equation.

[50] We also classically assume that ε is uncorrelated with the time invariant shock α. It is indeed sufficient that their covariance be constant over time.

the same result). We instrument p^0 by p^{1996}. This has a strong impact on the estimated δ, which drops from -0.36 to -0.10 (with standard error of 0.04). As a "placebo" test, we run a similar regression over a period taken prior to the Loi Galland ($\triangle^0 p = p^{1996} - p^{1992}$). Under our stationarity assumption, we can instrument p^0 either by itself (this is valid because p^0 is computed precisely over the period 1992 to 1996), or by the midpoint p^{1994}. The estimated coefficient of p^0 in these regressions is never significantly different from zero (respectively 0.00 with standard error 0.04, and 0.05 with standard error 0.05).

B.2.2 Difference-in-differences estimator

Consider the case where ε captures heterogenous trends across stores : $\varepsilon^y = \gamma \times y + \nu^y$ (where γ is store specific and ν^y is white noise). Unobserved store level price trends may bias the estimate of δ if they are correlated with p^0. To eliminate store trends, we differentiate the model again by substracting (4) from (5), where both periods have the same length. In this difference-in-differences estimator (denoted Diff. Diff.), we use $\triangle^1 p = p^{1998} - p^{1996}$ and $\triangle^0 p = p^{1995} - p^{1993}$, so that the equation residual is $\nu^{1998} - \nu^{1996} - (\nu^{1995} - \nu^{1993})$. We instrument p^0 by $Mean\left(p^{1995}, p^{1993}\right)$.

B.2.3 Modelling the residual as an AR(1)

In order to investigate empirically the serial correlation of ε, we run regression (4) in first-differences for each year from 1993 to 1996 (i.e., for all first-differences available in the pre-Loi Galland period). We compute the correlations between all pairs of the corresponding residuals, focusing first on correlations such as $corr\left[(\varepsilon^{1996} - \varepsilon^{1995}) \times (\varepsilon^{1995} - \varepsilon^{1994})\right]$, which should be equal to -0.5 under the null hypothesis of absence of serial correlation of ε (this the base of Wooldridge's test of serial correlation). For all such pairs, we find a small negative and weakly significant correlation (about -0.05). The absence of serial correlation is therefore strongly rejected by the data. Considering pairs of residuals such as $corr\left[(\varepsilon^{1996} - \varepsilon^{1995}) \times (\varepsilon^{1994} - \varepsilon^{1993})\right]$ further leads to reject the hypotheses of random walk and idiosyncratic trend (the correlation is about -0.2 and strongly significant). The pattern of correlation in first-differences is therefore consistent with $MA(1)$, $AR(1)$, or more complex processes exhibiting persistence for at least one period. We then run regression (4) in second-differences, and proceed as before. The correlation $corr\left[(\varepsilon^{1996} - \varepsilon^{1994}) \times (\varepsilon^{1994} - \varepsilon^{1992})\right]$ is about -0.2, strongly significant yet still much smaller than -0.5. This leads to reject not only the absence of serial correlation but also the $MA(1)$ process. Finally, correlations such as $corr\left[(\varepsilon^{1996} - \varepsilon^{1994}) \times (\varepsilon^{1995} - \varepsilon^{1993})\right]$ are positive and strongly significant (ranging from 0.2 to 0.4). Overall, the correlation pattern of the differenced residuals is consistent with positive serial correlation of ε, decreasing over time, but more gradually than a $MA(1)$. To go further, we explicitly model serial correlation by assuming that ε follows an $AR(1)$ process, which captures the essential features of the process while being tractable (the persistence of the data is captured by a single parameter):

$$\varepsilon^y = \rho \times \varepsilon^{y-1} + \nu^y$$

Where ν^y is white noise. Based on this model, we proceed to estimate ρ and the parameter of interest

δ. We use the $AR(1)$ representation to eliminate the serially correlated ε from the estimating equation (i.e., to "*whiten*" the residual). To illustrate this, consider the case where both $\triangle^1$ and $\triangle^0$ have a length of one year. Substracting $\rho\times(4)$ from (5) and rearranging terms yields the dynamic representation[51]:

$$\triangle^1 p = \rho \times \triangle^0 p + \beta \times \left[\triangle^1 x - \rho \triangle^0 x\right] + \delta \times p^0 + \triangle^1 \nu \tag{6}$$

Where only the white noise ν now appears in the residual. In our estimation, however, we want $\triangle^1$ and $\triangle^0$ to have a length of two years, i.e., we use $\triangle^1 p = p^{1998} - p^{1996}$ and $\triangle^0 p = p^{1996} - p^{1994}$.[52] This is a minor change to the previous equation : the coefficients of $\triangle^0$ become ρ^2, and the residual becomes $\nu^{1998} + \rho\nu^{1997} - (\nu^{1996} + \rho\nu^{1995})$. The lagged term $\triangle^0 p$ is correlated with the residual through ν^{1996}. A natural estimation strategy consists in instrumenting $\triangle^0 p = p^{1996} - p^{1994}$ by p^{1994}, which is uncorrelated with the equation residual while being correlated with $\triangle^0 p$. However, p^{1994} is also a natural candidate to instrument p^0 (the initial price should ideally be as close as possible to 1996, the year just preceding the Loi Galland, but p^{1996} and p^{1995} are ruled out). Therefore, ρ^2 and δ are poorly identified. If we knew the true value of ρ, however, we could avoid this difficulty by estimating (6) in the following form (again given for one year $\triangle^1$ and $\triangle^0$ periods):

$$\triangle^1 p - \rho \times \triangle^0 p = \beta \times \left[\triangle^1 x - \rho \times \triangle^0 x\right] + \delta \times p^0 + \triangle^1 \nu \tag{7}$$

We therefore proceed as follows:

- We estimate ρ^2 from a dynamic equation analogous to (6), taking both periods $\triangle^1$ and $\triangle^0$ prior to the Loi Galland, so that p^0 does not appear in the equation. Specifically, we regress $p^{1996} - p^{1994}$ on $p^{1994} - p^{1992}$ (as well as the corresponding differences in x), instrumenting $p^{1994} - p^{1992}$ by p^{1992}.
- We make the identifying assumption that ρ^2 has not been affected by the Loi Galland.
- We use the estimated value $\hat{\rho^2}$ to compute the left-hand side of (7), as well as the term in brackets on the right-end side. We estimate δ from the resulting equation by $2SLS$, instrumenting p^0 by p^{1994}, p^{1993} and p^{1992}.
- We estimate standard errors of this two-step approach by bootstrap, repeating the entire procedure a large number of times.

[51] This method of dealing with an $ar(1)$ residual is often called "*quasi-differentiation*" in the time series literature.

[52] The first reason is that we want to capture the full effect of the Loi Galland, which is unlikely to be complete after just one year. The second reason is that second-differences are less sensitive to measurement errors than first-differences. Finally, we rely on internal instruments (lagged levels of explanatory variables to instrument current changes). For the robustness of our estimates, it is important to check that current differences are indeed strongly correlated with lagged levels. We do so by regressing price changes on lagged levels (as well as regressors x). It turns out that lagged levels tend to be weak instruments for first-differences, but not for second-differences.