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THE EFFECTS OF RETAIL REGULATIONS ON PRICES: EVIDENCE FROM THE *LOI GALLAND**

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Using a unique data set merging micro-store level data with grocery markets data, this article 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. 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 anti-competitive effects of resale price maintenance in markets with interlocking relationships.

Resale price maintenance (RPM) has recently been at the centre of heated debates among lawyers and economists following the US Supreme Court ruling in *Leegin* and the recent revision of the European block exemption regulation on vertical restraints (Commission Regulation (EU) No 330/2010 of 20 April 2010 on the application of Article 101(3) of the Treaty on the Functioning of the European Union to categories of vertical agreements and concerted practices). As discussed by Rey and Vergé (2008), 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. Moreover, according to Lafontaine and Slade (2008), there is still a lack of empirical evidence on the effects of RPM. Changes to regulations akin to rules *de facto* legalising RPM can therefore be seen as useful natural experiments to evaluate the competitive effects of RPM. Such changes occurred in Ireland with the 1987 Groceries Order (which was revoked in December 2005) and France with the 1996 *Loi Galland*. This article uses the implementation of the *Loi Galland* in January 1997 to assess the effects of RPM on retail prices.

The *Loi Galland* modified existing below-cost pricing regulations with the goal of protecting small retailers (as well as producers) from the 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 as the 1973 *Loi Royer*, reinforced in 1996 by the *Loi Raffarin*, implemented restrictive planning regulations. Bertrand and Kramarz (2002) confirmed the negative impact of the *Loi Royer* on employment and prices empirically. While below-cost pricing regulations were already in place from 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 price paid

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by the retailer at the time of delivery (i.e. invoice price). It then became illegal for retailers to pass-on to customers any conditional rebate (e.g. end-of-year rebates).

It is widely believed that conditional rebates (or 'hidden rebates') increased substantially after 1997, thereby guaranteeing a minimum (gross) margin to the retailers. The *Loi Galland* thus had the same impact as legalising industry-wide minimum 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, as discussed by the Commission Canivet (2005). While the increase in retail prices for groceries was lower than the consumer price index (CPI) between 1994 and 1996 (2% *versus* 3%), it was substantially higher between 1997 and 2002 (11.8% *versus* 6.4%). AC Nielsen also reported that, for some 1,500 (national brand) products, retail prices went up by more than 4% during the first two months of 1997. Boutin and Guerrero (2008) provide a more systematic macro-econometric analysis and show that the *Loi Galland* might have added about one percentage point to inflation between 1997 and 2004. This effect also seems specific to France.

Empirically assessing the impact of the *Loi Galland* on retail prices sheds an interesting light on the more general debate about the anti-competitive effects of minimum RPM, especially when used in markets with 'interlocking relationships' (i.e., where manufacturers sell their products through the same retailers). Dobson and Waterson (2007) and Rey and Vergé (2010) have shown that RPM may be particularly harmful in such situations. Empirical evidence on the consequence of this kind of regulation is scarce. To our knowledge, Collins *et al.* (2001), who evaluate the effect of the Irish 1987 Groceries Order, provide the only empirical study on the effects of below-cost pricing regulations. Focusing on a specific category of products (processed and preserved fruits and vegetables), they show that the 1987 Groceries Order may have had a significant impact on retail prices, with retail margins increasing on average from 15.8% to 20.1% between 1988 and 1993.

Using a richer data set than Collins *et al.* (2001), this article 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, collected every month in about 2,000 stores, are available for about 200 homogeneous products from 1993 to 2000. Local market concentration is computed using an exhaustive index of French retail stores.

The *Loi Galland* entered into force on 1 January 1997 for all types of stores and products everywhere in France, with no exception or differentiated implementation delay which allows for the construction of control groups. Any evaluation of its impact on retail prices must therefore rely on testing predictions of the likely impact of industry-wide price floors (minimum RPM) that cannot 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. We also exploit the different impact that the *Loi Galland* may have had on branded products and store brands. Branded products are produced by large manufacturers with strong bargaining power in their negotiations with retailers. Conversely store brands are usually procured through a very competitive bidding process which was not directly affected by the new legislation (a process in which producers – very often SMEs – have very limited bargaining power).

Our results confirm 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 essentially focused on the hidden rebates that cannot be passed on to consumers. Indeed, with the enactment of the *Loi Galland*, the correlation between retail prices and the degree of competition in the local grocery market was divided by almost 2.5 (the price difference between more concentrated and less concentrated areas went down from 1.4% to 0.6%) suggesting that intra-brand competition no longer played a significant role. The impact was also stronger for branded products than for store brands. Moreover, price dispersion, that is, variation in retail price for a given product across stores – also went down for branded products and the price gap between more expensive and less expensive stores was also reduced. Even though the reduction in price dispersion is very significant, our analysis suggests that there remains significant differences in the prices of different stores.

The rest of the article is organised as follows. We start by briefly analysing the impact of industry-wide price floors and RPM in the context of vertical negotiations (Section 1). We then describe our empirical strategy to identify how the *Loi Galland* has impacted retail prices through the elimination of intra-brand competition (Section 2) before presenting the data (Section 3). In Section 4, we test for a break in the correlation between retail prices and local markets concentration after the *Loi Galland*. In Section 5, we focus on the reduction of price dispersion for a given product across stores and on the price convergence between stores after the *Loi Galland*. Section 6 concludes.

1. Economic Analysis of the *Loi Galland*

In 2004, a group of experts was commissioned by the Finance Minister to evaluate the existing legal framework. The title of their report (Commission Canivet, 2005) on vertical relationships in the food industry, 'Restoring price competition', was a clear indication of the results of that evaluation. According to this report, the *Loi Galland* had given producers and retailers the ability to manipulate the below-cost pricing threshold in order to set an industry-wide price floor freely. This manipulation might have been a wide-spread practice and some abuses have even been sanctioned by the French competition authority (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)). Manufacturers and retailers agree that, after the enactment of the *Loi Galland*, negotiations 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). According to the producers' association Institut de Liaison et d'Etudes des industries de Consommation (ILEC), the average hidden margin increased from 22% of the net wholesale price in 1998 to 32% in 2003.

Manufacturers of branded products tend to have market power and to bargain with retailers (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 take place and how the legal framework may directly affect price formation.

1.1. *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), before negotiating with each retailer individual rebates or agreements for commercial services.[1] The GTS specify the list price, that is, a per-unit wholesale price for the product, as well as two types of rebates:

(*i*) Unconditional rebates that are granted at the time of delivery (most products are usually delivered to the retail stores or to the retailer's logistic platforms several times a year). These can be, for instance, rebates for specific store formats (hypermarkets or large supermarkets) or for early payment.

(*ii*) Conditional rebates that cannot be included on the invoice at the time of delivery, for instance, because they depend on the annual quantity bought by a retailer. These rebates may be used to increase retailers' marketing efforts.

Rebates may be specific to some types of stores but all retailers of 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 (e.g. promotional activities, better shelf space, local advertising etc.). 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 (i.e. net of unconditional rebates), differs from the retailer's true cost (known as the triple net price, i.e. net of all rebates), sometimes to a great extent. 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 net and the triple-net prices.

Normally, one should expect below-cost pricing regulations to use the triple-net price as the relevant threshold, as this constitutes the retailer's true cost. Whether rebates are conditional or not should not then affect price formation. However, under the *Loi Galland*, the below-cost pricing threshold (i.e. the minimum price below which retailers are not allowed to sell) was the net price, or invoice price. Retailers were no longer able to pass-on hidden rebates to final consumers and hidden rebates thus constituted 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 into an industry-wide minimum retail price. The manufacturers thus had the power to directly control retail prices: the producer simply sets a relatively high net

[1] 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 article (01/1993–12/2000), general terms of sales were, however, non-negotiable.

price (identical for all comparable retailers) in order to maintain high retail prices and therefore high industry profits. These profits can then be shared with the retailers through individually negotiated hidden rebates. As we have already mentioned in the introduction, it is often claimed that this manipulation actually occurred and that hidden rebates significantly increased after 1997.

1.2. *Minimum Resale Price Maintenance*

As we have just discussed, 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.

Hart and Tirole (1990) in the context of quantity competition and O'Brien and Shaffer (1992) in the context of price competition have shown that, when a monopolist manufacturer secretly negotiates with competing retailers, the industry profit is not maximised as equilibrium retail prices are too low.[2] When the manufacturer and a retailer (secretly) negotiate the wholesale contract, they take the contracts offered to competing distributors as given. If wholesale tariffs can be sophisticated enough, the negotiation maximises the pair's joint-profit: for instance, if parties can negotiate over a two-part tariff, the wholesale price can be used to maximise the joint-profit that can then be shared through the fixed fee. 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 and lower the bilaterally 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 lower than the industry-profit maximising prices. Despite its monopoly position, the manufacturer cannot take advantage of its market power, the opportunism problem dramatically reducing its profit. As demonstrated by Marx and Shaffer (2007) and Miklòs-Thal *et al.* (2011), a similar problem arises when retailers have some bargaining power.

O'Brien and Shaffer (1992) 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 lowers the manufacturer's profit: the manufacturer no longer has 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 prices.

[2] See also McAfee and Schwartz (1994) and Rey and Vergé (2004) who analyse similar situations using a more traditional game–theoretical approach than the contract equilibrium concept *à la* Crémer and Riordan (1987) adopted by O'Brien and Shaffer (1992).

2. Identifying the Impact of the *Loi Galland*

2.1. *Identification Issue*

As explained earlier, one can suspect that the *Loi Galland* has allowed manipulations of the below-cost pricing threshold leading to *de facto* RPM. This mechanism may ultimately have been responsible (at least partially) for the sharp increase in retail prices that has been observed in France between 1997 and 2000. Even though this unforeseen effect has been much debated, there is a lack of robust empirical evidence. Our goal in this article 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 analyse as it requires separating it from the impact of other shocks occurring in the industry, such as demand or cost shocks and other contemporaneous policy changes.

2.1.1. Loi Galland *or changes induced by demand or cost shocks?*

A general retail price increase can also be 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 these different effects. One way would be to estimate a structural model. For instance, although they are not primarily interested by below-cost pricing regulations, Bonnet and Dubois (2010) 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 with a very large number of different products but it is nevertheless possible to identify more refined predictions than mere price increases. Focusing on the price formation mechanism, we argue that cost (or demand) shocks and RPM generate very different effects, especially when we consider 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 changes 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 removes this link. Costs shocks affect the level of equilibrium retail prices but not the link between prices and concentration levels.

2.1.2. 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 1,000 to 300 m^2. It has been argued that the *Loi Raffarin* also led to price inflation, but the effects of tougher planning restrictions are very different from the effects of the *Loi Galland*.

The *Loi Raffarin* is an incremental reinforcement of an already existing restrictive policy rather than a structural break: barriers to entry had already been in place since the 1973 *Loi Royer* that introduced the mandatory retail permit for stores over

1,000 m^2 (i.e. hypermarkets and large supermarkets). Moreover, in 1993, the Finance Minister had given instructions to the local commissions granting these retail permits to slow down the evaluation process, leading to a significant drop in the number of store extensions and openings after 1993.

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. Bertrand and Kramarz (2002) 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, the number of new large retail stores significantly decreased (although differently across markets depending on the policies of local commissions) eventually leading to substantial discrepancies in concentration across markets, for given local demand and cost conditions. As local market power diverged across markets, 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. Moreover, the effects of the *Loi Raffarin* should have been felt gradually given that market structure evolves only slowly over time. If anything, the existence of this contemporaneous regulation implies that the effects of the *Loi Galland* that we identify in this article are likely to be underestimated.

2.2. *Retail Prices and Local Competition*

If the *Loi Galland* materialised through industry-wide price floors, we expect the correlation between retail prices and competition in local grocery markets to have decreased sharply 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* are also not expected to have such an effect. Additional predictions can be related to product heterogeneity as well as to store heterogeneity.

2.2.1. *Product heterogeneity*

Firstly, economic analysis suggests that a much larger impact of the *Loi Galland* for national brand than for store brand products. The framework of vertical relationships presented in Section 1 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 incentives to manipulate the threshold. For the procurement of store brands, retailers often rely on small and medium sized enterprises (SMEs). Moreover, these SMEs (and sometimes large manufacturers) compete annually to supply a particular store brand. The relationship between a retailer and the potential suppliers is thus best seen as a bidding market where the retailer tends to have most of the bargaining power. Moreover, store brands are sold in one chain only. Therefore, the *Loi Galland* should not directly affect the retail prices for store brands (there are usually no hidden margins for such products). However, since store brand products are to some extent substitutable to branded products, they may have been indirectly affected. We therefore expect a much higher change in the correlation between prices and local concentration for national brands than for store brands.

2.2.2. *Store heterogeneity*

Stores also have different selling technologies (e.g. proximity to consumers, distribution cost, level of services etc.). Hypermarkets compete with supermarkets, convenience stores and hard discount stores but 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 small supermarket) or driving to a large supermarket or a 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 larger stores usually requires significant driving times and choice will thus be mostly driven by prices (and range of products on offer) rather than distance.

Hypermarkets are thus the most likely store format to set low prices and are therefore to be affected by a price floor. They will also be more sensitive to the degree of local competition. We thus expect them to have been more affected by the *Loi Galland* than other formats such as supermarkets or convenience store. We also expect hard discounters to be less affected since they usually do not sell branded products: if prices are strategic complements, we expect a positive reaction from hard discounters to the rival formats' price increase but only to a reduced extent. However, the effect may not be as visible as expected since the period we analyse also corresponds to the development of hard discounters in France.

2.3. *Price Dispersion and Price Convergence*

We expect that before 1997 (and the *Loi Galland*), retail prices were, for stores of a given type, linked to the degree of competition on the local markets as well as to the local demand conditions. For a given product (and in a given year), prices should thus vary a lot across stores. If the *Loi Galland* has led to possible implementation of (minimum) RPM, we should expect this price dispersion to have been reduced after 1997, especially for branded products that have been more directly affected. The theoretical literature suggests that RPM eliminates intra-brand competition and we should therefore expect price dispersion for a given product to have been eliminated. In reality, the effect has probably been less extreme since GTS 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, which may not be binding everywhere. Nevertheless, at least for branded products, price dispersion for a given product should have decreased after 1997. We also expect that stores in which prices were initially high, either because of local demand conditions or because they were highly concentrated, are less likely to have been affected by the *Loi Galland*. On the contrary, stores in which prices were initially lower have been affected by the new minimum price and prices thus went up in these stores. Therefore the inflationary impact should have been higher for stores in which prices were initially relatively low, leading to retail prices converging towards the highest pre-*Loi Galland* levels.

3. Data

Our empirical analysis relies on two data sets on retail prices and on the local structure of the grocery retailing industry covering an eight-year period (from January 1993 to December 2000) centered on the enactment of the *Loi Galland*. 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).

3.1. *Retail Prices*

We exploit a unique data set on individual retail prices. This database is collected by INSEE (the French national institute of statistics) and used to compute the CPI; it covers the whole of France 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.[3] Our final data set contains monthly prices for 141 food items and 45 non-food items in 1994 (respectively 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.).

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

(*i*) Hypermarkets are the largest stores (selling area over 2,500 m^2 and currently up to 25,000 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.
(*ii*) Supermarkets have a selling area between 400 and 2,500 m^2 and are 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 the smaller stores).
(*iii*) 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.
(*iv*) 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 offer the same services as traditional stores.

[3] Almost all the 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).

(*v*) *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.

As mentioned earlier, the effects of the *Loi Galland* are likely to differ for store brands (or lowest price products) and national brands. In our data set, the products are coded according to a classification which is CPI-specific and the brand variable is not as reliable as the core 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 when it is categorised as such in the INSEE data set or when the brand corresponds to the store fascia. Our non-store brand category thus includes products that are not directly affected by the *Loi Galland* (lowest price products) as well as the leading national brands. Therefore, we expect the difference in effects between national brands and store brands to be underestimated in our analysis.

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.

3.2. *Grocery Stores*

We created a unique data set of local stores based on the *Atlas de la distribution*, a yearly index of grocery stores. This index is a reference for the retailers themselves and is, in principle, exhaustive.[4]

Types of stores are the same as in the CPI data set. We have collected exhaustive data for hypermarkets, supermarkets, hard discount stores and *magasins populaires* from 1993 to 2000. For each store, information includes variables such as type (as defined above), size (selling area in m^2), fascia and location (administrative city code). 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. Table 1 shows the number of stores included in the price (CPI data) and the grocery stores (store data) data sets. As shown in Table 1, the CPI data covers a significant proportion all stores (especially for larger stores).

3.3. *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 (Barros *et al.*,

[4] The set of stores types for which information is collected by this source has – to a limited extent – widened 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 instances 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.

Table 1
Number of Stores in CPI and Grocery Stores Data Sets

	Hypermarket	Supermarket	Hard discount	Convenience	*Magasins populaires*
1994					
Store data	1,001	5,947	750	n/a	292
CPI data	364	780	79	560	130
1999					
Store data	1,120	5,806	2,164	n/a	307
CPI data	495	832	173	355	136

Sources. INSEE (CPI), LSA.

2006) and by competition authorities (see for instance the European Commission's decisions in *Kesko/Tuko* and *Carrefour/Promodès*): for each store present in the CPI data, we look for all competing stores (as identified by the store data) within a given radius. Given that we do not know the store's exact location but only the city code, a catchment area will be centred on cities rather than stores: all stores within one particular city code thus have the same catchment area.

To construct these catchment areas, we use an INSEE data set providing cartesian coordinates of city geometric mass centres. For any city within the CPI sample, we use these coordinates to compute the distances (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 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 Barros *et al.* (2006), our proxy for local competition is a measure of local market concentration, that is, a Herfindahl–Hirschman index (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 it 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 time 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, for the stores present in the CPI data, the distribution 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 (0 km). Moreover, the median distance between a city and its nearest neighbour is 2.5 km. Thus, the market should not be too narrowly defined. Besides, although hypermarkets are likely to attract consumers travelling longer distances convenience stores are more likely to attract only local consumers.[5] We thus decided to focus on smaller catchment areas including stores within a 10 km radius, testing alternatives modes of define catchment areas (2.5, 5 or 20 km for all stores; or distances differentiated by types of stores for instance) to check that our central results are robust. Our catchment areas are therefore smaller

[5] While competition authorities often use 15 or 30 minute driving-time limits to define catchment areas for large stores, a recent inquiry by the French Autorité de la Concurrence considered that catchment areas for convenience stores in Paris have a radius of less than 500 m.

Table 2

Number of Competing Stores

	Hypermarket		Supermarket		Hard discount		*Magasin populaires*	
	1994	1999	1994	1999	1994	1999	1994	1999
0 km								
Q_{25}	0	0	0	0	0	0	0	0
Median	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
Median	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
Median	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
Median	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.

than those constructed in other studies of local competition. For instance, Barros *et al.* (2006) use a distance of 30 km 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 (for 1998) also allow us to construct a measure of household income within that area. Population and income data are available for one year only and to better control for changes in local demand, we also use local unemployment rates available every quarter at the level of the *département*.[6] Table 3 summarises some statistics for our catchment areas.

4. Price and Market Concentration

We now test the prediction that price competition has been dampened by the *Loi Galland*, the effect being larger for national brands than for store brands and larger 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

[6] There are 94 *départements* 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

Characteristics of Local Grocery Markets

	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
Median	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
No. observations	25,994	20,243	25,994	20,243	25,994	20,243

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

10 km radius around each store where at least one price has been collected 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 1997.

Let $p_{i,j}^{y,m}$ denote the price (in logarithm) of product i, observed in store j during month m of year y. Let $\theta(i)$ denote the type of product (national brand or store brand), $\phi(j)$ 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:

$$p_{i,j}^{y,m} = \delta^y \mathrm{HHI}_{c(j)}^y + \boldsymbol{\lambda}^y \boldsymbol{X}_{c(j)}^y + \alpha_i^{y,m} + \beta_{\theta(i)}^y + \gamma_{\varphi(j)}^y + \varepsilon_{i,j}^{y,m}, \tag{1}$$

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 product-type and store-type effects, as well as product × year × month dummies. 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). 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 01/1993–12/2000, in order to eliminate biases due to differences among the stores that may have entered or exited the market during the period. We also checked that our results are robust by considering shorter time periods (i.e. 01/1994–12/1999 or 01/1995–12/1998). The relationship between market concentration and prices may have changed over time for 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 after the *Loi Galland* (δ^1). Testing our predictions then amounts to comparing the restricted coefficients, that is, analysing the difference $\Delta = \delta^1 - \delta^0$. This approach simplifies the comparison between the pre and post-regulation regimes but 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. Denoting by $\boldsymbol{\delta}$ the vector of yearly coefficients δ^y, we test for the restriction $\boldsymbol{\delta} = g(\delta^0, \Delta)$ given by: $\delta^y = \delta^0$ for any $y \leq 1996$ and $\delta^y = \delta^0 + \Delta$ for any $y \geq 1997$. We use asymptotic least squares (Chamberlain, 1984; Gourieroux *et al.*, 1985) to recover $\hat{\delta}^0$ and $\hat{\Delta}$ from the unrestricted OLS estimator $\hat{\Delta}$ and its estimated covariance matrix $\hat{\boldsymbol{\Sigma}}$, by minimising the quantity $[\hat{\boldsymbol{\delta}} - g(\delta^0, \Delta)]'\hat{\boldsymbol{\Sigma}}^{-1}[\hat{\boldsymbol{\delta}} - g(\delta^0, \Delta)]$ with respect to δ^0 and Δ. To check the robustness of our results, the post-estimation tests are done for three different periods centred on 01/1997 (1993–2000, 1994–9 and 1995–8).

We proceed in the same way to test the predictions related to store brands and hypermarkets. Compared to (1), the only difference is that we now interact our concentration variable alternatively with store brand or hypermarkets dummies.

The results of the restricted estimations are presented in Table 4 and the complete results of the unconstrained specifications are reported in Appendix A. Columns (1) and (2) provide cross-section and within estimates for the base specification, whereas columns (3) and (4) provide the within estimates for the specifications related to the store brand and hypermarkets tests. Table 5 presents a summary of the post-estimation tests for the different specifications.

In the cross-section specification, the market concentration (HHI) coefficient drops from 0.144 to about 0.063 (see column (1) in Table 4). The results for the pre-*Loi Galland* period are in line with previous comparable cross-section estimates for other European countries. Barros *et al.* (2006) find a correlation coefficient of 0.15 for the Portuguese grocery market, while Asplund and Friberg (2002) find smaller, but comparable, figures for the Swedish market. Table 3 shows that the inter-quartile range is about 0.1 for HHI. Therefore, the price difference between the more (i.e. upper quartile – Q_{75}) and the less (i.e. lower quartile – Q_{25}) concentrated areas decreased from 1.4% before 1997 to 0.6% under the *Loi Galland*.

Looking now at the unconstrained estimations (Table A1), we see a clear impact of the *Loi Galland* in 1997 (the HHI coefficient drops from 0.137 in 1996 to 0.071 in 1997), but this impact was nevertheless gradual as the coefficient continued to decrease (and is no longer statistically significant) after 1998. Three years after the enactment of the *Loi Galland*, the correlation between prices and concentration had almost totally vanished. This correlation was relatively stable between 1994 and 1996 and between 1997 and 1999 (following the big drop in 1997). Extreme years are further apart (higher in 1993 and lower in 2000) but these years are peculiar since 1993 corresponds to the end of a long period of recession (one may expect competition intensity to have gone up) and 2000 is characterised by dynamic growth as well as significant mergers (one may except further weakening of competition intensity). The drop in the coefficient is of the same order of magnitude in all specifications (see Table 5, ΔHHI) and is always significant, which suggests that our results are extremely robust. The results for the within specification reported in column (2) of Table 4 show a much smaller coefficient for market concentration than in the cross-section estimation (0.069 *versus* 0.144 before the *Loi Galland* and 0.007 *versus* 0.063 after 1997). This suggests a possible correlation between unobserved

Table 4

Cross-section and Within Estimates

	Cross-section				Within			
	(1)		(2)		(3)		(4)	
	$\leq$ 1996	$\geq$ 1997	$\leq$ 1996	$\geq$ 1997	$\leq$ 1996	$\geq$ 1997	$\leq$ 1996	$\geq$ 1997
Super	0.055	0.040	0.011	Ref.	0.011	Ref.	0.016	Ref.
	(0.002)	(0.002)	(0.002)		(0.002)		(0.003)	
Hard discount	−0.301	−0.332	0.034	Ref.	0.033	Ref.	0.038	Ref.
	(0.012)	(0.013)	(0.008)		(0.008)		(0.008)	
Magasin populaires	0.081	0.089	0.000	Ref.	0.000	Ref.	0.004	Ref.
	(0.003)	(0.003)	(0.003)		(0.003)		(0.003)	
Convenience	0.222	0.222	−0.003	Ref.	−0.003	Ref.	0.002	Ref.
	(0.004)	(0.005)	(0.003)		(0.003)		(0.004)	
Hypermarket	Ref.	Ref.	Ref.		Ref.		Ref.	
Store brand	−0.167	−0.185	−0.167	−0.183	−0.171	−0.191	−0.166	−0.183
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
HHI	0.144	0.063	0.069	0.007				
	(0.015)	(0.015)	(0.029)	(0.028)				
HHI × store B					0.097	0.047		
					(0.030)	(0.029)		
HHI × other B					0.059	−0.012		
					(0.029)	(0.029)		
HHI × hypermarket							0.104	0.004
							(0.049)	(0.049)
HHI × other							0.066	0.010
							(0.030)	(0.029)
Population	0.022	0.013	0.007	Ref.	0.007	Ref.	0.007	Ref.
	(0.001)	(0.001)	(0.001)		(0.001)		(0.001)	
Income	0.015	0.009	0.004	Ref.	0.004	Ref.	0.002	Ref.
	(0.007)	(0.009)	(0.006)		(0.006)		(0.006)	
Additional controls	(Product × year × month)		(Product × year, year × month, product × store type, store effect)					
No. observations	893,635	709,606	893,635	709,606	893,635	709,606	893,635	709,606

Notes. OLS estimator, standard errors in parentheses. Catchment areas specifics vary across town × year only. The residuals of observations at this level might then be correlated. In order to recover robust standard errors, we allow in all our regressions for group effects at the town × year level. This is done by bootstrapping using clusters by town × year.

Table 5

Estimated Variation of HHI Coefficients

	Constrained			Unconstrained				
	Cross-section	Within		Cross-section			Within	
Estimated period	1993–2000	1993–2000	1993–2000	1994–99	1995–98	1993–2000	1994–99	1995–98
$\hat{\Delta}$HHI	−0.082	−0.064	−0.085	−0.074	−0.069	−0.060	−0.054	−0.048
	(0.021)	(0.012)	(0.021)	(0.024)	(0.029)	(0.012)	(0.013)	(0.015)
$\hat{\Delta}$HHI × store brand	−0.072	−0.051	−0.075	−0.055	−0.049	−0.048	−0.034	−0.027
	(0.029)	(0.017)	(0.029)	(0.033)	(0.040)	(0.017)	(0.019)	(0.022)
$\hat{\Delta}$HHI × other brand	−0.087	−0.072	−0.092	−0.083	−0.077	−0.070	−0.065	−0.057
	(0.021)	(0.013)	(0.021)	(0.024)	(0.028)	(0.013)	(0.014)	(0.017)
$\hat{\Delta}$HHI × hyper	−0.142	−0.102	−0.145	−0.122	−0.107	−0.092	−0.079	−0.071
	(0.032)	(0.019)	(0.033)	(0.037)	(0.045)	(0.018)	(0.019)	(0.023)
$\hat{\Delta}$HHI × other type	−0.074	−0.057	−0.078	−0.069	−0.065	−0.054	−0.047	−0.043
	(0.022)	(0.012)	(0.022)	(0.026)	(0.031)	(0.012)	(0.013)	(0.015)

Notes. Post-estimation asymptotic least squares based on Table 4 as well as on Tables A1–A6 presented in Appendix A. Standard errors clustered by town × year.

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) and therefore have lower distribution costs and lower prices.

Let us now turn to the predictions related to store brands (column (3)) and hypermarkets (column (4)). Before the *Loi Galland*, market concentration had a significant impact on both types of products, this effect being larger for store brands (0.097) than for other brands (0.059). The two coefficients substantially decreased after the *Loi Galland* (from 0.097 to 0.047 and from 0.059 to −0.012 respectively) and the difference between the pre and post-*Loi Galland* coefficients is statistically significant. For all specifications, we also observe that, as predicted, the *Loi Galland* had a smaller impact on store brands (see Table 5, ΔHHI × store brand and ΔHHI × other brand). Before the *Loi Galland*, market concentration had a higher impact on hypermarkets' prices than on other stores' prices (0.104 *versus* 0.066). The correlation coefficient decreases substantially after 1997 all types of stores (from 0.104 to 0.004 and from 0.066 to 0.010 respectively). The impact of the *Loi Galland* is however, as predicted, much larger for hypermarkets than for other types of stores, the change in coefficient being nearly twice as large for hypermarkets in all specifications (see Table 5, ΔHHI × hyper and ΔHHI × other types).

We have focused so far on the main predictions related to market concentration and now briefly discuss the effect of the other covariates. Not surprisingly store brands are substantially cheaper than other brands (by about 17–18%) and hard discount stores are by far the cheapest stores. It also turns out that the price gap between hypermarkets and supermarkets has been reduced after the *Loi Galland* (from about 5.5% to 4.0%). This is consistent with the prediction that prices should have converged upwards, but we discuss convergence in depth in subsection 5.2). 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 (which can be seen as a proxy for population density as the size of catchment areas is fixed) 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*.

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 threshold may have been manipulated to act as *de facto* industry-wide minimum RPM.

5. Price Dispersion and Price Convergence

To provide additional evidence that intra-brand competition was weakened by the new regulation, we now consider price dispersion directly. Conditional on market structure, the price dispersion of branded products should have decreased across stores, while that of store brands should not have been affected. In order to test this prediction, we must somehow condition on brand. However, since the brand variable is not used directly in the computation of the CPI, it is not subject to the same quality controls as the CPI's core variables. As a result, brand is poorly measured in our data. We therefore derive a test based on contrasting two groups of products, namely store brands and branded products.[7]

We use two different approaches to investigate the effect of the *Loi Galland* on price dispersion. We first look at the dispersion of prices in the original product × store × date dimension of the data. In order to focus more closely on the reduction of price dispersion across stores, we then aggregate the data at the store × year level and ask whether price convergence has taken place across stores after the *Loi Galland*.

5.1. *Product Level Analysis: Price Dispersion*

We start the price dispersion analysis by a straightforward (although relatively crude) test. We then turn to a more general quantile regression approach.

For every product × year cell, we first compute three measures of the dispersion of prices (in logarithm) across stores: the standard deviation, the inter-quartile range (P_{75}–P_{25}) and the inter-decile range (P_{90}–P_{10}). We compute these measures first for all products in the sample, then for branded products only. We then run an OLS regression of these measures of price dispersion on a *Loi Galland* dummy as well as product dummies. The results are reported in Table 6.

[7] As already mentioned, we use the information on brand, in addition to the store brand dummy available in the data, to construct the store brand group. Branded products are then defined as the complement of this set.

Table 6

Change in Mean Dispersion Across Products

	All brands			Excluding store brands		
	SD	P_{75}–P_{25}	P_{90}–P_{10}	SD	P_{75}–P_{25}	P_{90}–P_{10}
LG dummy	−0.001	0.002	−0.014	−0.009	−0.022	−0.031
	(0.003)	(0.006)	(0.010)	(0.003)	(0.007)	(0.010)
No. product	Y	Y	Y	Y	Y	Y
No. observations (cells)	1,525	1,525	1,525	1,525	1,525	1,525

Notes. OLS estimator, standard errors in parentheses; clusters by product.

In the first set of regressions based on all observations, the *Loi Galland* dummy (i.e. year $\geq$ 1997) is never significant. However, once we exclude store brands, the *Loi Galland* dummy is significant for all three measures of dispersion suggesting a drop in price dispersion after 01/1997 for branded products. This result is robust to the way we define observations (product × year, product × year × month or product × year × store type) as well as to the set of controls that we use. This approach is however crude since it does not allow us to control for other determinants of price dispersion which may have changed over time.

We therefore turn to the more general method of quantile regression. Instead of modelling the conditional expectation of price as a function of the covariates as we did in (1), we now model the quantiles P_{25} and P_{75}. The effect of each covariate on price dispersion is obtained as the difference between its effects on P_{75} and P_{25} (hence the denomination of inter-quartile regression).[8]

Since only branded products should have been directly affected by the *Loi Galland*, we expect price dispersion across stores (for a given product in a given year) to have decreased more for branded products than for store brands. Therefore, we expect the coefficient for store brand (relative to that for non-store brand) to have gone up after the *Loi Galland*, conditional on the type of store, the product, the date, as well as market level characteristics (size, income, concentration).

Due to the high computational cost of estimation,[9] we can no longer use the data pooled across years to impose *a priori* restrictions on the yearly coefficients. Instead, we run separate regressions for each year, thereby limiting in each estimation the size of the data and the number of coefficients to estimate. As in the previous Section, we use a post-estimation test based on the minimum distance between mean coefficients before and after the *Loi Galland*. The full results are presented in Table B1 in Appendix B. We report in Table 7 the result of the post-estimation test for three different estimation periods. Controlling for all the factors in (1) (i.e. type of store, HHI, population, income etc.), it appears that, prior to the *Loi Galland*, prices for

[8] We use bootstrapping to compute standard errors by drawing with replacement from a population of town × 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 inter-quartile range of prices as the difference between the effects on P_{75} and P_{25}.

[9] In the quantile regression, control dummies can no longer be absorbed. Given the large number of controls and the large size of the data set, this substantially increases the computational cost of the estimation.

Table 7

Store Brand Effect on Price Dispersion

	1993–2000	1994–9	1995–8
Store brand effect before LG ($\hat{\beta}^0$)	−0.006	−0.005	−0.001
	(0.002)	(0.002)	(0.003)
Change of store brand effect after LG ($\hat{\Delta}$)	0.025	0.021	0.014
	(0.003)	(0.004)	(0.005)

Notes. Post-estimation asymptotic least squares based on Table B1; standard errors in parentheses.

store brands were less dispersed than prices for other products, although the difference is not always significant. However, the difference in price dispersion significantly increased by about 0.02 after the *Loi Galland*, which is perfectly consistent with our prediction. To help interpret the magnitude of this effect, we compute the inter-quartile range of the (log)price of branded products, within product × store type cells. Before the *Loi Galland*, the median of the inter-quartile range across those cells is 0.15. The 0.02 (i.e. two percentage points) change in relative price dispersion attributed to the *Loi Galland*, although strongly significant, appears relatively small. We should first note that this estimate is likely to be biased towards zero due to measurement errors on brand. On the other hand, we may be concerned that this result might not solely capture the relaxation of intra-brand competition. Indeed, as the *Loi Galland* imposes a ban on below-cost pricing, it is likely to have constrained the use of loss-leader strategies (i.e. pricing some products aggressively, often below cost, in order to attract customers into the stores and setting higher prices on other products). If loss-leading takes the form of random price cuts on subsets of branded products – as is typically the case – the ban on below-cost pricing could have reduced the price variation of these products, without necessarily affecting the average price over a representative business period. Yet, it is the dispersion across stores of the latter that should best capture the intensity of intra-brand competition. Since our economic framework of vertical relations cannot be extended easily to include a multi-product–multi-brand dimension, we proceed empirically and complement the quantile regression analysis by explicitly testing for price convergence between stores.

5.2. *Store Level Analysis: Price Convergence*

The cross-sectional dispersion of prices results from the potentially complex pricing strategies of multi-product stores but also from the coexistence of low-price and high-price stores (for instance due to differences in the intensity of competition in local markets). We now focus on the latter source of variation, more directly related to intra-brand competition. We do so by asking whether prices – especially the prices of branded products – have converged between initially low-price and high-price stores.

To aggregate the price data at the store level, we first regress (log) prices on a set of dummies of product characteristics (product × year × month as well as product type dummies). For a given store in a given year, we then compute a yearly store price as the mean of the residuals of these regressions across products and months. A store price of -0.01 in 1996 thus means that the store was, on average across products and months, approximately 1% cheaper than the market average. This relative store price reflects the component of retail prices affected by store and local market characteristics. We compute two different store prices: the first one including all products, the second one focusing on branded products (i.e. non-store brands) only.

Our empirical strategy is the following: we model store prices as the sum of an observed component which can be explained by store and local market characteristics and of a stochastic shock reflecting unobserved supply and demand characteristics as well as measurement errors.[10] The change in store price between any two dates prior to the *Loi Galland* can thus be written as:

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

where Δ^0 denotes a difference taken across a normal period (i.e. with no exogenous change in the price formation mechanism), x denotes observable covariates such as store and local market characteristics and ε is an unobserved shock. By contrast, store price changes between a date after 1997 and a date prior to 1997 are modelled as:

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

where Δ^1 denotes a difference taken between two dates, one before and one after the *Loi Galland* (i.e. during a period in which a change in the price formation mechanism occurred). Including p^0 in (3) captures the idea of price convergence. Given that we expect prices to have increased less after the *Loi Galland* in initially more expensive stores (i.e. higher store price p^0), our main parameter of interest δ is expected to be negative.

One can think of p^0 as the mean store price computed over all dates prior to the *Loi Galland*, that is, the mean store price of the years 1993–6. However, depending on the stochastic properties of the unobserved shock ε, p^0 may be endogenous in (3), thus generating spurious price convergence. This is obviously the case when $\Delta^1 p = p^{1998} - p^{1994}$ and the initial price p^0 includes p^{1994}. Even if we exclude p^{1994} from the computation of the initial price p^0, endogeneity may still be an issue when the unobserved shocks are serially correlated.[11] We must therefore find appropriate instruments, whose validity depends on the properties of ε.

If ε has no trend, we can instrument p^0 in (3) by the price at the middle point of the interval corresponding to $\Delta^1 p$ (i.e. p^{1996} for $\Delta^1 p = p^{1998} - p^{1994}$). This will generate a valid instrument as long as the process followed by ε is covariance stationary (i.e. the covariance of ε at any two dates only depends on the length of time between these dates) since, in that case, the covariances of ε^{1996} with ε^{1998} and ε^{1994} cancel each other out. We call this estimator difference estimator. If the shocks ε have a trend,[12] we differentiate the model by substracting (2) from (3), which provides a difference-in-differences estimator.

[10] The full details are presented in Appendix C.

[11] For instance, serial correlation may be due to imperfect knowledge for the stores of the magnitude of demand or supply shocks, or of the competitors' and consumers' reactions.

[12] Such a trend may come unobserved factors making markets gradually more competitive or gradually modifying local demand.

Table 8

Test of Price Convergence Across Stores

	All brands			Excluding store brands		
	Diff.	Diff.Diff.	AR(1)	Diff.	Diff.Diff.	AR(1)
AR coefficient ρ^2			0.282			0.178
			(0.092)			(0.071)
Initial price	−0.105	−0.115	−0.145	−0.133	−0.132	−0.175
	(0.044)	(0.038)	(0.042)	(0.045)	(0.047)	(0.039)
Supermarket	−0.005	−0.005	−0.001	−0.009	−0.005	−0.003
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)
Hard discount	−0.022	−0.089	−0.148	−0.045	−0.135	−0.241
	(0.026)	(0.031)	(0.060)	(0.047)	(0.042)	(0.057)
Magasins populaire	0.000	−0.010	0.001	−0.010	0.011	0.013
	(0.009)	(0.011)	(0.008)	(0.010)	(0.012)	(0.008)
Convenience	0.042	0.003	0.022	0.048	0.012	0.027
	(0.011)	(0.011)	(0.011)	(0.013)	(0.013)	(0.012)
Hypermarket	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
HHI	−0.015	−0.079	−0.082	0.014	−0.070	−0.024
	(0.079)	(0.092)	(0.124)	(0.096)	(0.123)	(0.187)
Unemployed	−0.003	−0.006	0.001	−0.006	−0.005	−0.007
	(0.004)	(0.005)	(0.005)	(0.004)	(0.006)	(0.006)
No. Observations	1,327	1,391	1,273	1,257	1,322	1,224

Notes. The Diff. and Diff.Diff. columns are IV estimators; the AR(1) column corresponds to the two-step estimator. Standard errors in parentheses, robust to correlation within town (estimated by bootstrap).

As a final robustness check, we propose a third estimator allowing us to evaluate the magnitude of the shocks' serial correlation. To capture such serial correlation in a tractable way, we model ε as a simple $AR(1)$ process (with yearly autoregressive coefficient ρ and innovation v). We define Δ^0 and Δ^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 v appears in the residual. However, both the lagged price change $\Delta^0 p$ and p^0 are endogenous. Our best candidates to instrument both variables are the same appropriately lagged price levels, but this 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 p^0 does not appear in this model, 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 parameter δ can then be estimated by IV, as in the difference-in-differences approach.[13]

The results of the three convergence tests are shown in Table 8. The first three columns show the results obtained using all observations, the last three show the results for branded products (i.e. non-store brands) only. For each set of results, the first estimate, noted Diff., corresponds to the difference estimator (we use $\Delta^1 p = \text{mean}(p^{1998}, p^{1999}) - \text{mean}(p^{1993}, p^{1994})$), the second, Diff.Diff., to the difference-in-differences (with $\Delta^1 p = p^{1998} - p^{1996}$ and $\Delta^0 p = p^{1995} - p^{1993}$) and the last one, noted AR(1), to the two-step procedure (where the first step is based on the differences $p^{1996} - p^{1994}$ and $p^{1994} - p^{1992}$ and we use $\Delta^1 p = p^{1998} - p^{1996}$ and $\Delta^0 p = p^{1996} - p^{1994}$ in the second step).

[13] The technical details are presented in Appendix C.

The results of all three estimations methods are consistent with the prediction that prices have increased more in initially cheaper stores (i.e. the coefficient for the variable Initial price is negative). As expected, the price convergence effect is stronger for branded products than for store brands. The coefficient is indeed substantially larger (between -0.13 and -0.18 *versus* between -0.10 to -0.15) when we restrict our attention to branded products only (more precisely to non-store brands).

Note also that the data does not seem to support the existence of an individual trend, the estimated AR coefficient being lower than 1. In all regressions presented in Table 8, store type dummies have been included in order to control for different trends across store types, possibly correlated with initial prices. Hard discounters appear to be less affected by the *Loi Galland* than other types of stores, a result that was expected since they mainly distribute store brands. Omitting this control would bias our estimates since hard discounters are the cheapest stores.

To get an idea of the magnitude of the convergence effect, consider a store that was initially 10% cheaper than the market average for branded products (i.e. this is about one standard deviation below average). According to our estimates, the relative price of this store (expressed in percent deviation from market average) would have gone up by about 1.5 points after the *Loi Galland*. As a polar case benchmark, the imposition of a unique price across stores for branded products due to full RPM, would have resulted in a 10-point increase (corresponding to a convergence coefficient of -1). The magnitude of price convergence may thus appear relatively small. However, measurement errors on the brand variable (remember that it also include low-price products) as well as on the estimated store prices are likely to bias the magnitude of the coefficients towards zero. It may also be the case that only some manufacturers representing a limited share of total sales succeeded in imposing *de facto* industry-wide price floors. Finally, the implementation of full scale RPM may have been limited by the possibility for consumers to substitute across brands, from branded products to store brands and away from the types of stores and products present in our sample.

6. Conclusion

Using a unique data set on retail prices for a large number of products, collected in a large and representative sample of grocery stores, this article 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 of grocery retail prices 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, prices in more concentrated areas (in the upper quartile) being about 1.4% higher than in less concentrated areas (lower quartile). The new legislation led to a sharp drop in this correlation – price in more concentrated areas being only 0.6% higher – 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 significant 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.

Overall, our results appear to be qualitatively extremely robust to various approaches and specifications. We argued that the outcomes tested empirically could not have been generated by cost shocks, demand shocks, or by the change in planning regulations. Therefore, our empirical tests support the idea that the *Loi Galland* has allowed some manufacturers to impose *de facto* industry-wide price floors.

This article does not provide a complete evaluation of the effects of the *Loi Galland* but does find strong empirical support for theoretical arguments explaining the mechanism through which this regulation has relaxed intra-brand competition and led to higher prices. However, one of the main objectives of the *Loi Galland* was to level the playing field between small businesses and large retail chains. This objective might have been – at least partially – achieved by eliminating part of the price disadvantages faced by small village shops. Our analysis has shown this has been made possible by inducing large stores to increase their prices. However, anecdotal evidence suggests that this had a rather limited impact on the price disadvantage of small stores. 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).[14] Moreover there is no strong evidence that the rate of closure of independent specialised 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 centres and may also have supported the survival of local high quality craftsmen or specialised 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 specialised shops.

Finally, this article confirms that RPM or industry-wide price floors can be detrimental to final customers. However, this article does not intend to take a position in the debate on whether RPM 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.

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

Appendix A. Unconstrained Regressions

Table A1

Unconstrained Cross-section Regressions

	1993	1994	1995	1996	1997	1998	1999	2000
Supermarket	0.057	0.055	0.054	0.051	0.046	0.039	0.037	0.035
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Hard discount	−0.263	−0.312	−0.302	−0.302	−0.322	−0.312	−0.327	−0.367
	(0.044)	(0.027)	(0.020)	(0.017)	(0.021)	(0.024)	(0.026)	(0.029)
Magasins populaires	0.080	0.082	0.081	0.080	0.088	0.088	0.089	0.092
	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)	(0.005)
Convenience	0.224	0.216	0.223	0.223	0.220	0.222	0.228	0.219
	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)
Hypermarket	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Store brand	−0.171	−0.171	−0.166	−0.160	−0.175	−0.185	−0.189	−0.195
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Other brand	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
HHI	0.160	0.144	0.133	0.137	0.071	0.059	0.058	0.038
	(0.032)	(0.029)	(0.032)	(0.027)	(0.025)	(0.032)	(0.037)	(0.032)
Population	0.024	0.023	0.022	0.021	0.015	0.013	0.014	0.010
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Income	0.022	0.021	0.007	0.010	0.005	0.018	0.004	0.009
	(0.015)	(0.013)	(0.014)	(0.017)	(0.017)	(0.015)	(0.016)	(0.017)
Additional controls	Prod. × year × month							
No. observations	227,711	234,450	231,335	200,139	214,963	166,487	165,399	162,757

Notes. OLS estimator, standard errors in parentheses. Standard errors robust to correlation within town × year estimated by bootstrap.

Table A2

Unconstrained Cross-section Regressions (Store Brand Effect)

	1993	1994	1995	1996	1997	1998	1999	2000
Supermarket	0.057	0.055	0.054	0.051	0.046	0.039	0.037	0.035
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Hard discount	−0.263	−0.312	−0.302	−0.301	−0.321	−0.311	−0.326	−0.367
	(0.044)	(0.027)	(0.020)	(0.018)	(0.021)	(0.024)	(0.026)	(0.029)
Magasins populaires	0.080	0.082	0.081	0.080	0.088	0.088	0.089	0.092
	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)	(0.005)
Convenience	0.224	0.216	0.223	0.223	0.220	0.221	0.227	0.219
	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)
Hypermarket	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Store brand	−0.174	−0.172	−0.166	−0.163	−0.179	−0.191	−0.193	−0.197
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)
Other brand	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
HHI × store B	0.183	0.150	0.137	0.151	0.096	0.092	0.080	0.050
	(0.040)	(0.041)	(0.037)	(0.039)	(0.037)	(0.045)	(0.046)	(0.043)
HHI × other B	0.154	0.142	0.131	0.132	0.062	0.044	0.047	0.032
	(0.032)	(0.028)	(0.033)	(0.026)	(0.024)	(0.031)	(0.038)	(0.034)
Population	0.024	0.023	0.022	0.021	0.015	0.013	0.013	0.010
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
Income	0.021	0.021	0.007	0.010	0.005	0.018	0.004	0.009
	(0.014)	(0.013)	(0.014)	(0.017)	(0.017)	(0.015)	(0.016)	(0.017)
Additional controls				Prod. × year × month				
No. observations	227,711	234,450	231,335	200,139	214,963	166,487	165,399	162,757

Notes. OLS estimator, standard errors in parentheses. Standard errors robust to correlation within town × year estimated by bootstrap.

Table A3

Unconstrained Cross-section Regressions (Type of Store Effect)

	1993	1994	1995	1996	1997	1998	1999	2000
Supermarket	0.067	0.062	0.061	0.060	0.052	0.040	0.035	0.033
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)
Hard discount	−0.253	−0.306	−0.296	−0.294	−0.317	−0.311	−0.329	−0.369
	(0.044)	(0.027)	(0.020)	(0.018)	(0.022)	(0.024)	(0.026)	(0.029)
Magasins populaires	0.089	0.087	0.086	0.087	0.093	0.089	0.087	0.091
	(0.008)	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
Convenience	0.235	0.222	0.230	0.232	0.227	0.223	0.225	0.218
	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)
Hypermarket	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Store brand	−0.170	−0.171	−0.166	−0.160	−0.175	−0.185	−0.189	−0.195
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Other brand	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
HHI × hypermarket	0.253	0.200	0.195	0.207	0.119	0.068	0.046	0.025
	(0.057)	(0.049)	(0.048)	(0.045)	(0.044)	(0.047)	(0.049)	(0.047)
HHI × other	0.149	0.137	0.124	0.127	0.063	0.057	0.063	0.041
	(0.032)	(0.031)	(0.033)	(0.028)	(0.026)	(0.033)	(0.037)	(0.033)
Population	0.025	0.023	0.023	0.022	0.015	0.013	0.013	0.010
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
Income	0.019	0.019	0.005	0.008	0.004	0.018	0.005	0.010
	(0.014)	(0.013)	(0.014)	(0.017)	(0.017)	(0.015)	(0.016)	(0.018)
Additional controls				Prod. × year × month				
No. observations	227,711	234,450	231,335	200,139	214,963	166,487	165,399	162,757

Notes. OLS estimator, standard errors in parentheses. Standard errors robust to correlation within town × year estimated by bootstrap.

Table A4

Unconstrained Within Store Regressions

	1993	1994	1995	1996	1997	1998	1999	2000
Supermarket	0.016	0.016	0.015	0.014	0.010	0.003	0.001	Ref.
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	
Hard discount	0.062	0.056	0.075	0.066	0.046	0.053	0.031	Ref.
	(0.019)	(0.014)	(0.012)	(0.012)	(0.012)	(0.013)	(0.011)	
Magasins populaires	−0.008	−0.001	−0.003	−0.004	−0.003	−0.006	−0.006	Ref.
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	
Convenience	−0.007	−0.006	0.004	0.002	0.000	0.000	0.005	Ref.
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	
Hypermarket	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Store brand	−0.170	−0.170	−0.165	−0.160	−0.173	−0.183	−0.187	−0.193
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Other brand	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
HHI	0.086	0.078	0.067	0.070	0.028	0.011	0.011	−0.016
	(0.031)	(0.029)	(0.031)	(0.030)	(0.028)	(0.029)	(0.031)	(0.032)
Population	0.011	0.010	0.010	0.009	0.005	0.003	0.003	Ref.
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	
Income	0.013	0.011	−0.004	−0.003	−0.004	0.008	−0.001	Ref.
	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)	
Additional controls	Prod. × year, year × month, prod. × store type, store effect							
No. observations	227,711	234,450	231,335	200,139	214,963	166,487	165,399	162,757

Notes. OLS estimator, standard errors in parentheses. Standard errors robust to correlation within town × year estimated by bootstrap.

Table A5

Unconstrained Within Store Regression (Type of Store Effect)

	1993	1994	1995	1996	1997	1998	1999	2000
Supermarket	0.016	0.016	0.015	0.014	0.010	0.003	0.001	Ref.
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	
Hard discount	0.061	0.055	0.074	0.066	0.046	0.054	0.031	Ref.
	(0.019)	(0.014)	(0.012)	(0.012)	(0.012)	(0.013)	(0.012)	
Magasins populaires	−0.008	−0.001	−0.003	−0.004	−0.003	−0.006	−0.006	Ref.
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	
Convenience	−0.007	−0.006	0.003	0.002	−0.001	0.000	0.005	Ref.
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	
Hypermarket	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Store brand	−0.175	−0.174	−0.169	−0.164	−0.180	−0.193	−0.195	−0.199
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.005)	(0.006)
Other brand	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
HHI × store B	0.123	0.102	0.089	0.098	0.069	0.063	0.053	0.015
	(0.035)	(0.034)	(0.034)	(0.035)	(0.032)	(0.035)	(0.036)	(0.038)
HHI × other B	0.074	0.070	0.058	0.058	0.012	−0.013	−0.011	−0.033
	(0.031)	(0.030)	(0.031)	(0.031)	(0.029)	(0.030)	(0.033)	(0.035)
Population	0.011	0.010	0.010	0.009	0.005	0.003	0.003	Ref.
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	
Income	0.012	0.011	−0.004	−0.003	−0.004	0.008	−0.001	Ref.
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)	
Additional controls	Prod. × year, year × month, prod. × store type, store effect							
No. observations	227,711	234,450	231,335	200,139	214,963	166,487	165,399	162,757

Notes. OLS estimator, standard errors in parentheses. Standard errors robust to correlation within town × year estimated by bootstrap.

Table A6

Unconstrained Within Store Regression (Type of Store Effect)

	1993	1994	1995	1996	1997	1998	1999	2000
Supermarket	0.024	0.021	0.021	0.021	0.015	0.005	0.000	Ref.
	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	
Hard discount	0.069	0.060	0.080	0.072	0.051	0.055	0.030	Ref.
	(0.019)	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)	
Magasins populaires	−0.001	0.003	0.002	0.002	0.001	−0.005	−0.007	Ref.
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)	
Convenience	0.002	−0.001	0.010	0.009	0.005	0.002	0.004	Ref.
	(0.008)	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	
Hypermarket	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Store brand	−0.170	−0.170	−0.165	−0.160	−0.173	−0.184	−0.187	−0.193
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Other brand	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
HHI × hypermarket	0.131	0.095	0.094	0.105	0.046	0.005	−0.015	−0.036
	(0.054)	(0.054)	(0.055)	(0.058)	(0.054)	(0.054)	(0.055)	(0.064)
HHI × other	0.081	0.078	0.065	0.066	0.027	0.016	0.021	−0.009
	(0.031)	(0.030)	(0.031)	(0.030)	(0.029)	(0.030)	(0.031)	(0.032)
Population	0.012	0.011	0.010	0.010	0.005	0.003	0.003	Ref.
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	
Income	0.010	0.009	−0.006	−0.005	−0.006	0.008	−0.001	Ref.
	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)	
Additional controls	Prod. × year, year × month, prod. × store type, store effect							
No. observations	227,711	234,450	231,335	200,139	214,963	166,487	165,399	162,757

Notes. OLS estimator, standard errors in parentheses. Standard errors robust to correlation within town × year estimated by bootstrap.

Appendix B. Price Dispersion

Table B1

Inter-quartile Regression of Retail Prices

	1993	1994	1995	1996	1997	1998	1999	2000
Supermarket	0.000	−0.003	−0.002	−0.001	−0.003	−0.005	−0.002	0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Hard discount	0.200	0.166	0.148	0.150	0.154	0.133	0.131	0.204
	(0.110)	(0.035)	(0.025)	(0.022)	(0.026)	(0.037)	(0.035)	(0.038)
Magasins populaires	0.002	−0.014	−0.008	−0.004	−0.003	−0.005	−0.009	−0.010
	(0.006)	(0.004)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Convenience	0.065	0.057	0.063	0.080	0.079	0.080	0.081	0.079
	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.012)	(0.012)	(0.011)
Hypermarket	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
Store brand	−0.010	−0.009	−0.001	−0.001	0.010	0.017	0.022	0.027
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
Other brand	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
HHI	0.030	−0.010	−0.018	0.019	−0.009	0.010	0.013	0.011
	(0.029)	(0.024)	(0.023)	(0.025)	(0.024)	(0.026)	(0.023)	(0.027)
Population	0.001	−0.001	0.000	0.002	0.000	0.000	0.001	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Income	0.027	0.023	0.020	0.021	0.021	0.023	0.020	0.017
	(0.011)	(0.010)	(0.009)	(0.011)	(0.009)	(0.011)	(0.010)	(0.009)
Additional controls	Prod. × year, year × month							
No. observations	227,711	234,450	231,335	200,139	214,963	166,487	165,399	162,757

Notes. OLS estimator. Standard errors robust to correlation within town × year estimated by bootstrap, standard errors in parentheses.

Appendix C. Price Convergence

At any date prior to the enactment of 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 ε, 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):

$$\Delta^0 p = \beta \times \Delta^0 x + \Delta^0 \varepsilon, \tag{C.1}$$

where Δ^0 denotes a difference taken between two dates 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 enactment of the *Loi Galland* therefore takes the form of a convergence equation.

$$\Delta^1 p = \delta \times p^0 + \beta \times \Delta^1 x + \Delta^1 \varepsilon, \tag{C.2}$$

where Δ^1 denotes a difference taken between a date after and a date before the enactment 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 is expected to 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.[15] 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 in 1996 with 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*). Nevertheless, as a further robustness check, 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 pre-determined as opposed to strongly exogenous). The estimate of our main parameter of interest δ is then unchanged. In fact, apart from store types, the covariates x have little explanatory power in the price difference 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.

C.1. *Difference Estimator*

Assume $\Delta^1 p = p^{1998} - p^{1994}$. If ε is not serially correlated, p^0 is endogenous in the convergence equation (through to p^{1994}) because ε^{1994} appears both in p^{1994} and in the equation residual $\Delta^1 \varepsilon = \varepsilon^{1998} - \varepsilon^{1994}$. p^0 can then be instrumented by p^{1993}, p^{1995} and/or p^{1996}. If ε is serially correlated, p^0 can be instrumented by the store price at the midpoint of the Δ^1 interval (i.e. p^{1996}) provided the process from which the store level shocks ε are drawn is covariance stationary

[15] The individual effect α does not appear in the equation as it cancels out once we take the difference between two dates.

(i.e. the covariance between ε at any two dates only depends on the length of time between these dates). Under this assumption, $E(\varepsilon^{y+n} \times \varepsilon^y) = E(\varepsilon^{y-n} \times \varepsilon^y)$ for any n, so that ε^{1996} has the same covariance with ε^{1994} and ε^{1998} and p^{1996} is thus uncorrelated with $\Delta^1\varepsilon = \varepsilon^{1998} - \varepsilon^{1994}$.[16] In our estimation, we use the following difference estimator (denoted Diff.):

$$\Delta^1 p = \text{mean}(p^{1998}, p^{1999}) - \text{mean}(p^{1993}, p^{1994}),$$

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 $\Delta^1 p = p^{1998} - p^{1994}$ yields essentially 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* ($\Delta^0 p = p^{1996} - p^{1992}$).[17] Under our stationarity assumption, we can instrument p^0 either by itself (this is valid because p^0 is computed precisely over the period 1992–6), 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).

C.2. *Difference-in-differences Estimator*

Consider the case where ε captures heterogenous trends across stores: $\varepsilon^y = \gamma \times y + v^y$ (where γ is store specific and v^y is a 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 (C.1) from (C.2), where both periods have the same length. In this difference-in-differences estimator (denoted Diff.Diff.), we use $\Delta^1 p = p^{1998} - p^{1996}$ and $\Delta^0 p = p^{1995} - p^{1993}$, so that the equation residual is $v^{1998} - v^{1996} - (v^{1995} - v^{1993})$. We instrument p^0 by $\text{mean}(p^{1995}, p^{1993})$.

C.3. *Modelling the Residual as an AR(1)*

In order to investigate empirically the serial correlation of ε, we run regression (C.1) in first-differences for each year from 1993 to 1996 (i.e. for $p^{1994} - p^{1993}$, $p^{1995} - p^{1994}$ and $p^{1996} - p^{1995}$). We then compute the correlations between all pairs of the corresponding residuals, focusing first on correlations such as $\text{corr}[(\varepsilon^{1996} - \varepsilon^{1995}) \times (\varepsilon^{1995} - \varepsilon^{1994})]$, 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 $\text{corr}[(\varepsilon^{1996} - \varepsilon^{1995}) \times (\varepsilon^{1994} - \varepsilon^{1993})]$ 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 (C.1) in second-differences and proceed as before. The correlation $\text{corr}[(\varepsilon^{1996} - \varepsilon^{1994}) \times (\varepsilon^{1994} - \varepsilon^{1992})]$ 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 $\text{corr}[(\varepsilon^{1996} - \varepsilon^{1994}) \times (\varepsilon^{1995} - \varepsilon^{1993})]$ 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 get a better understanding of convergence, we explicitly model serial

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

[17] Although we mostly focus in the article on the period 01/1993–12/2000, we also have data prior to 1993 which we use for some robustness checks in the price convergence analysis.

correlation by assuming that ε follows an AR(1) process, which captures the essential features of the process while being tractable (i.e. the persistence of the data is captured by a single parameter). We thus assume that $\varepsilon^y = \rho \times \varepsilon^{y-1} + v^y$, where v^y is a 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. To illustrate this, consider the case where both Δ^1 and Δ^0 have a length of one year. Substracting $\rho\times$(C.1) from (C.2) yields the dynamic representation:[18]

$$\Delta^1 p = \rho \times \Delta^0 p + \beta \times (\Delta^1 x - \rho \Delta^0 x) + \delta \times p^0 + \Delta^1 v, \tag{C.3}$$

where only the white noise v now appears in the residual. In our estimation, however, we want Δ^1 and Δ^0 to have a length of two years, that is, we use $\Delta^1 p = p^{1998} - p^{1996}$ and $\Delta^0 p = p^{1996} - p^{1994}$.[19] This is a minor change to the previous equation: the coefficients of Δ^0 become ρ^2 and the residual becomes $v^{1998} + \rho v^{1997} - (v^{1996} + \rho v^{1995})$. The lagged term $\Delta^0 p$ is correlated with the residual through v^{1996}. A natural estimation strategy consists in instrumenting $\Delta^0 p = p^{1996} - p^{1994}$ by p^{1994}, which is uncorrelated with the equation residual while being correlated with $\Delta^0 p$. 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 (C.3) in the following form (again given for one-year periods for the differences Δ^1 and Δ^0):

$$\Delta^1 p - \rho \times \Delta^0 p = \beta \times (\Delta^1 x - \rho \times \Delta^0 x) + \delta \times p^0 + \Delta^1 v. \tag{C.4}$$

We therefore proceed as follows:

(*i*) We estimate ρ^2 from a dynamic equation analogous to (C.3), taking both periods Δ^1 and Δ^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}.
(*ii*) We make the identifying assumption that ρ^2 has not been affected by the *Loi Galland.*
(*iii*) We use the estimated value $\hat{\rho}^2$ to compute the left-hand side of (C.4), as well as the term in brackets on the right-end side. We estimate δ from the resulting equation by two stage least squares (2SLS), instrumenting p^0 by p^{1994}, p^{1993} and p^{1992}.
(*iv*) We estimate standard errors of this two-step approach by bootstrap, repeating the entire procedure a large number of times.

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[18] This method of dealing with an AR(1) residual is often called quasi-differentiation in the time-series literature (Wooldridge 2009, ch. 12).

[19] 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.

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