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Abstract

In 1997, a new legislation banning below-invoice retail prices came into force in France. Individually negotiated discounts could no longer be passed on to consumers, which is equivalent to allowing industry-wide price floors. The anti-competitive effects of such practices are well-known. The elimination of intra-brand competition is expected to lead to a sharp increase in the retail prices. Using CPI raw data, we find evidence supporting this claim. The modification or revocation of the existing legislation (as it has been done in Ireland in December 2005) would then be expected to reduce retail prices.

Keywords: retail prices, pricing regulations, resale price maintenance

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Effets des régulations sur les prix du commerce de détail : exemple de la Loi Galland

Résumé

Entrée en application en 1997, la loi Galland interdit la revente en dessous du prix facturé. Les remises négociées individuellement (les « marges arrières ») ne peuvent plus être transmises aux consommateurs. Le seuil de revente à perte découle donc des conditions générales de ventes et s'apparente donc à un prix plancher sectoriel. Les effets anti-concurrentiels de ce type de pratiques sont bien connus. Une augmentation importante des prix de détail est attendue, suite à l'élimination de la concurrence intra-marques. L'analyse les données brute de l'Indices des Prix à la Consommation confirme cet effet. La modification ou l'abrogation de la législation actuelle (comme l'a fait l'Irlande en 2005) devraient donc avoir un effet à la baisse sur les prix de détail.

Mots-clés : prix de détail, réglementation des prix, prix de revente imposés

Classification JEL : L42, L81, K23

Introduction

Vertical relationships between large food retailers and manufacturers have been recently at the center of a fierce debate in France. The starting point of that debate was the observation that the average prices of food products had increased faster than the consumer price index over the 1997-2002 period (11.8% vs. 6.4%), whereas it tended to increase at a slower rate before 1997 (2% vs. 3% over the 1994-1996 period). This inflationary trend also seemed to be specific to France where food prices increased significantly more than in other Euro zone countries. The common feeling was that the enactment of the 1996 Loi Galland banning below-invoice prices had eliminated price competition between the main retail chains and caused this sharp price increase. In 2004, a group of experts (Commission Canivet) was thus commissioned 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 what was expected from the discussions.

Come into force January 1997, the Loi Galland prevents – among other things – retailers from reselling "below-cost".[1] Below-cost prices had been banned in France since 1963 but the definition of the threshold was relatively unclear. The Loi Galland clearly defined the price below which retailers were not allowed to sell as the invoice price, that is, the price paid by the retailer at the time of delivery. The important aspect of this definition is that it does not include any anticipated rebates that are usually paid at the end of the year (e.g. quantity rebates) since they are not included on the invoice. It is therefore impossible for retailers to pass such rebates through to final consumers which thus guarantees a minimum (gross) margin to the retailers.

Parties (manufacturers and retailers) seem to agree that, after the enactment of the Loi Galland, the negotiation shifted from *"upfront margins"* (i.e. rebates that can be included on the invoice) to *"hidden margins"* (i.e. end-of-year rebates and commercial cooperation that cannot be passed through to consumers). According to the producer's association ILEC, the average hidden margin increased from 22% of the net wholesale price in 1998 to 32% in 2003. In some (extreme) cases, rebates that parties knew in advance would be obtained by retailers and could therefore have been included in invoices at the time of delivery, were negotiated as end-of-year rebates. This had the effect of removing effective competition between participating retailers,

[1] The Loi Galland was introduced in order to "level the playing field" between small businesses and the rapidly growing chains of large retailers, mainly in the groceries sector.

thereby leading to higher retail prices. In *Buena Vista Home Entertainment (BVHE)*, the French competition authority considered that some end-of-year rebates were falsely conditional and fined *BVHE* and some of its retailers €14.4 million.[2]

When the shift from upfront to hidden margins is pushed to the extreme (i.e. negotiating hidden rebates only), the new law becomes equivalent to allowing price floors. Furthermore, the *"General Terms of Sales"* published by producers are non-negotiable and cannot be discriminatory. Therefore, the same price floor applies to all retailers. The below-invoice price law de facto eliminated competition on the downstream market, thus leading to higher prices. This is indeed consistent with the results of O'Brien and Shaffer (1992) suggesting that industry-wide price floors can allow a monopolist producer to solve its *opportunism problem* when selling to multiple retailers. When (secretly) negotiating with a retailer, the producer internalizes the whole profit made by this retailer but not the retail margin of the competing retailers. It has incentives to free-ride on the other retailers' downstream margins and thus to lower the negotiated price. This is anticipated by the retailers who are for this reason not willing to accept a high tariff. The equilibrium wholesale price is equal to the marginal production cost, thereby leading to (relatively) competitive retail prices. An industry-wide minimum price floor then prevents the producer from secretly offering a better deal to a retailer since this has no impact on the retailer's sales, thus restoring monopoly prices and profits. Perfectly in line with these arguments, the experts of the Canivet working party proposed to eliminate the separation between upfront and hidden margins, thus allowing retailers to pass all rebates through to consumers. This should restore intrabrand competition, thereby leading to lower retail prices.

The empirical evidence on the effect of the Loi Galland is scarce. Looking at the prices of 1500 (national brands) products sold by large retail chains, *Nielsen* found that retail prices went up by more than 4% during the first two months of 1997. On the contrary, the DGCCRF (Ministry of Finance), considered all types of products – i.e. national brands, private labels and low price products – and found an increase of only 0.5% during the same period. This paper provides a first general empirical test of the claims that the Galland Act, by de facto legalizing (industry-wide) price floors, was indeed responsible for the increase in prices that occurred after 1997.

To our knowledge, the only empirical study of the effects of below-cost pricing regulations was done by Collins, Burt, and Oustapassidis (2001), who evaluate the effect of such a law in Ireland. Ireland indeed introduced an almost identical law in 1987 (*Groceries Order*).[3] Focusing on a

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[2]See *Conseil de la Concurrence*, Decision 05-D-70, December 2005.

[3]The *1987 Groceries Order* has been revoked in December 2005. Very similar arguments to those used in France were mentioned by the Irish Competition Authority in order to justify the decision: the Minister for

specific category of products (processed and preserved fruits and vegetables), Collins, Burt, and Oustapassidis (2001) show that the *1987 Groceries Order* was a significant variable in explaining gross retail margins, and that the average retail margin increased from 15.8% in 1988 to 20.1% in 1993. Our approach is different in that we do not consider retail margins but only retail prices. We also take advantage of the richness of our dataset to focus on individual retail prices (rather than a national index for a basket of products) for a large number of products (more than 100 different categories). Although we cannot directly measure the increase in price due to the Loi Galland, we can indirectly validate the theories of harm presented by the Canivet Commission (among others).[4] Although they are not primarily interested by below-cost pricing regulations, Bonnet and Dubois (2007) analyze vertical contracting between manufacturers and retailers. Using micro-level data on the distribution of bottled water in French supermarkets during the 1998-2001 period (panel of about 11000 French households), they test different hypotheses on the vertical relationships and the pricing strategies. Their empirical analysis suggests that manufacturers use two-part tariffs combined with resale price maintenance. Given that their dataset covers the period 1998-2001, these results are consistent with the theories claiming that the Loi Galland de facto led to minimum resale price maintenance. They also simulate a counter-factual experiment constraining wholesale contracts to two-part tariffs and show that this would lead to a decrease in prices of major national brands of about 7%.

Finally, our paper is related to the growing literature linking market structure and prices. In particular, we test whether the switch to retail prices imposed by the manufacturers has led to a significantly smaller link between retail prices and local market concentration. As some earlier studies focusing on grocery prices, we find that in the absence of resale price maintenance (i.e. before 1997 reform), retail prices are positively correlated with concentration in local grocery markets.[5] However, after the reform, this correlation is no longer significant.

The rest of the paper is organized as follows. Empirical analysis is constrained by the available data. Part of the paper's contribution relies on the originality of our dataset. We thus start by presenting our data (section 1). We then very briefly review some relevant theoretical literature on minimum resale price maintenance and derive some testable predictions (section 2). In section 3, we carry out an indirect test of the theory of harm presented in the introduction and look at the correlation between retail prices and local markets concentration in 1994 and 1999. We then take advantage of the panel dimension of our data to provide a more direct test of our

Enterprise, Trade and Development commented that the revocation would *"introduce greater competition into grocery trade by allowing retailers freedom to determine the prices they charge their customers."*

[4] See also Allain and Chambolle (2005, 2007) for theoretical papers on this specific issue.

[5] See Barros, Brito, and de Lucena (2006) for Portugal, Asplund and Friberg (2002) for Sweden, Marion (1998) for the U.S.

predictions. To achieve this, we construct individual store effects and show that prices increased more in stores that were initially lower (section 4). Section 5 concludes.

1 Data

The empirical analysis in this paper is mainly based on two datasets, one on retail prices and one on the local structure of the grocery retailing sector.

1.1 Retail Prices

We have been granted access to a unique database on individual retail prices, collected by INSEE (the French national institute of statistics) to compute the Consumer Price Index (CPI). This database being the basis for the CPI, it covers the whole of the national territory. Stores and products surveyed are sampled to ensure representativeness.

The products for which prices are collected are coded according to a classification specific to the CPI. We decided to retain only the products that are sufficiently homogeneous across stores and dates (e.g. sugar, milk). We thus exclude intrinsically heterogeneous products such as clothes or furniture. We also further restrict our sample by selecting only products that are widely distributed across all types of retailers. Our dataset includes prices for 141 food items and 45 non-food items in 1994 (147 and 46 in 1999). Almost all products in our sample were surveyed in hypermarkets, supermarkets and *magasins populaires*. More than 88% of these products were surveyed in convenience stores and more than 44% in hard discounts (mainly food items).

Although, characteristics such as brand can change from one store to the other or from one date to another, our data also include a variable for the brand name. Unfortunately, since this information is either missing, or not very informative for most of the observations, it is impossible for us to use it: for instance, we are not able to satisfactorily distinguish between national brands and private labels.

Prices for a given product in a given store are surveyed every month. Whenever a store is shut down, it is replaced in the sample by a similar store within the same area. In practice, the retail price of product i in store j at time t 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 promotion, . . .).

For each store in which at least one price is collected, data include the city administrative code. We will use this code to match the retail price data with data about the local market (see below). The data also include the fascia of the store. Unfortunately, this information is not recorded every month but only when the store is included in the sample for the first time. It is not updated when the fascia changes, therefore the matching of fascias with data on grocery stores is quite imperfect.

General terms of sales are negotiated on a yearly basis. Besides, the Loi Galland induces a fundamental change in the framework of bilateral negotiations. Thus, we do not expect the effects of the Loi Galland to be fully effective just after the enactment. Important mergers take place in year 2000 in the French retailing industry. As a result, we use a monthly panel covering the period 01/1994 to 12/1999, where the statistical unit followed over time is product i in store j.

1.2 Grocery Stores

The local structure of the groceries retail market is provided by a unique dataset constructed by the authors using the *"Atlas de la grande distribution"*, a yearly national index of grocery stores. This index is used as reference by the retailers themselves and is, in principle, exhaustive.

Stores can be classified according to type and size (measured by selling area). All stores within a type category by and large follow the same business model. The various types (*"formes de vente"*) are:

- **Hypermarkets** are large stores (selling area over 2500m^2) generally located at the periphery of large cities.
- **Supermarkets** have selling area between 400 and 2500m^2 and usually located in city centers or at the periphery of smaller cities.
- **Convenience Stores** have selling area smaller than 400m^2 and are located closer to the customers.
- **Hard-discounters** can have sizes comparable to that of supermarkets or convenience stores. However, they do not sell the same range of products (usually do not offer the leading brands) and do not propose the same services.
- ***"Magasins Populaires"*** are the traditional multipurpose stores in city centers. They have sizes comparable to that of supermarkets, but do not primarily focus on food items.

We have collected data for all stores above $120m^2$ for 1994 and 1999 (stocks of stores are evaluated at the start of the year). For each store, information includes variables such as type (as defined above), size (selling area in m^2), fascia and location (administrative city code).[6] We do not have more precise information about the exact location in a city, except for the three largest cities (Paris, Marseille and Lyon) for which the *"Arrondissement"* (i.e. district) is known.[7] Table 1 shows the number of stores (for the various types described above) in both our retail price (*CPI data*) and grocery stores (*store data*) datasets.

TABLE 1 : Number of stores

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

Sources: INSEE (IPC), LSA

1.3 Catchment Areas and Proxies for Local Competition

Downstream competition in the retail industry takes place locally. Delineating geographical relevant markets is however an issue in itself. For the sake of simplicity, we construct catchment areas around each store within our CPI dataset. Our approach is similar to Barros, Brito, and de Lucena (2006) and consistent with assessment of the European Commission in the *Kesko / Tuko* and *Carrefour / Promodès* merger cases. Given that we do not know the exact location of a store but only the city code, a catchment area will be centered around cities rather than stores. All stores within one particular city code will therefore have the same catchment area.

To construct these catchment areas, we use an INSEE dataset providing cartesian coordinates of city barycenters. For any city within the CPI sample, we use these coordinates to compute its distance (as the crow flies) to neighboring cities. We then fix a maximum distance, usually 10km, to identify the cities that will be included in a catchment area.[8]

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

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

[8] Alternative distances of 2.5, 5 and 20km have been used as robustness checks. Distances between cities is to be understood as between city centers. Hence, distances between neighboring cities will be strictly positive. In our sample, the median of the distance to the closest city is 2.5km. When we take a distance of 2.5km as a reference to define our catchment areas, at least half of these areas are reduced to one city.

Using our store dataset, we then list stores (and selling area) within each catchment area.[9] As in Barros, Brito, and de Lucena (2006), our proxies for local competition are then measures of local concentration. These indices (hereafter market concentration, MC) are based on selling area and are built as an Herfindahl-Hirschman index based on selling area rather than turnover or quantities: it is simply the sum of the squared market shares (expressed in terms of selling area).

TABLE 2 : Distribution of surrounding stores

		Hyper		Super		Hard Discount		*Magasin Pop.*	
		1994	1999	1994	1999	1994	1999	1994	1999
0 km	Q1	0	0	0	0	0	0	0	0
	Med.	0	0	1	1	0	0	0	0
	Q3	1	1	2	2	1	1	0	0
	P90	1	1	4	3	1	3	1	1
5 km	Q1	1	1	3	3	0	1	0	0
	Med.	2	2	6	6	1	4	0	0
	Q3	3	3	14	13	3	8	1	1
	P90	7	7	43	21	7	23	9	9
10 km	Q1	2	2	7	7	1	4	0	0
	Med.	4	4	18	18	3	9	1	1
	Q3	7	8	37	34	8	20	2	2
	P90	18	17	74	67	13	42	14	13
20 km	Q1	3	4	19	18	3	6	1	1
	Med.	7	8	41	39	7	20	1	1
	Q3	15	17	75	72	17	44	6	5
	P90	56	55	396	365	57	223	81	85

Sources: INSEE (IPC), LSA, computations by the authors.

Choices have to be made as regards the size of the catchment areas. Increasing the size of the market, either in terms of distance or of scope of stores, allows to better account for all stores that may impose a competitive constraint. However, we also risk to loose some variability, since we are then bound to include stores that are not relevant. Table 2 shows the distribution of the number of stores (excluding convenience stores) surrounding the stores appearing in the CPI dataset (including convenience stores). It shows that these stores are seldom in competition with stores in the same city. Thus, the market should not be too narrowly defined. Besides, although hypermarkets are likely to attract consumers traveling longer distances, convenience stores are

[9] So far, we have excluded convenience stores. We indeed believe that our store dataset is exhaustive for the largest stores and convenience stores that belong to national chains, but in general does not include smaller independent corner stores.

more likely to attract only local consumers. It is thus preferable to select an intermediate distance as a reference. We thus decided to focus on two specifications.[10] First, we build our concentration index including all stores (except convenience stores) within a 10km range. As an alternative, we also build up catchment areas including all *"magasins populaires"* within 5km, all supermarkets and hard discounters within 10km, and all hypermarkets within 20km. Our definition of catchment area is therefore slightly more restrictive than those used in other studies of local competition.[11] However, we believe that this is a more reasonable choice since our sample does not include only large stores (such as hypermarkets or big supermarkets) but also some smaller convenience stores.

Finally, we use the 1999 Census data as proxies for local demand. Variables used to characterize each catchment area include local population as well as information on household income (percentage of households paying income tax, average reported income). Table 3 summarizes some statistics on our sample.

TABLE 3 : Summary Statistics

	MC (10 km)		MC (5/10/20 km)		Population (log) [a]		Income (log) [a]	
	1994	1999	1994	1999	1994	1999	1994	1999
Q1	0.05	0.04	0.05	0.04	11.82	11.45	11.35	11.36
Median	0.08	0.08	0.07	0.07	12.48	12.44	11.46	11.46
Mean	0.11	0.11	0.10	0.10	12.46	12.35	11.46	11.47
Q3	0.14	0.13	0.13	0.12	13.25	13.25	11.53	11.54
P90	0.25	0.25	0.22	0.23	14.38	14.16	11.67	11.71
STD	0.11	0.11	0.10	0.10	1.37	1.42	0.18	0.17
# Obs.	25994	20243	25994	20243	25994	20243	25994	20243

a: data on population are time invariant and come from the 1999 census. However, the samples for both cross section marginally differ and so do the summary statistics. Sources: INSEE (IPC), LSA, computations by the authors.

2 Theoretical Predictions

In order to understand the inflationary mechanism put forward during the discussions about the effects of the Loi Galland, it is important to understand how actual wholesale and retail prices are formed. Every year (usually in late autumn), producers announce their "general terms of sale" (hereafter GTS). According to the current laws, the GTS have to be non-discriminatory

[10] Our results are however robust to many other specifications. Different specifications as regards to stores that should be included in the "local markets" (i.e. catchment areas) have been tried, including all stores, hypermarkets and supermarkets only or solely hypermarkets, with various distances.

[11] For instance Barros, Brito, and de Lucena use a distance of 30kms but focus on very large stores only.

and are non-negotiable.[12] The GTS usually specify a wholesale price schedule (the "tariff price") and quantity rebates or channel specific rebates (or free units for instance) that are included on the invoice at the time of purchase. This defines the *"net wholesale price"*, which, under the Loi Galland, constitutes the minimum price (the invoice price) below which retailers cannot sell.

The GTS can also include rebates that are not mentioned on the invoice at the time of purchase but are usually paid at the end of the year.[13] These rebates are often linked to the annual quantity ordered by the retailer. The *"double net wholesale price"* includes these rebates. Finally, a producer and a retail chain often negotiate additional rebates for specific services offered by the retailer to the manufacturer (such as promotional activities, better shelf space, local advertising, ...). These services are billed separately and normally on a yearly basis. Once these rebates are included, the wholesale price actually paid by the retailer is referred to as the *"triple net price."* The difference between the net and triple net prices is often called *"hidden"* or *"backward"* margin (or hidden rebates).

Under normal circumstances, the resale below-cost laws should define the threshold as the triple net price. Under the Loi Galland, the hidden rebates cannot be passed on to final consumers and thus constitute a guaranteed (gross) margin for the retailer. Combined with the non-discriminatory laws, this regulation thus has the same effect as legalizing industry-wide price floors.

2.1 Minimum Resale Price Maintenance

In the context of vertical relationships between a monopolist producer and competing retailers, it has been shown that industry-wide price floors can be used to restore the ability of the vertical structure to maintain high prices. This not only harms the consumers but also reduces total economic welfare. This issue was analyzed by O'Brien and Shaffer (1992) who show that, without price floors, a manufacturer is tempted to free-ride on its retailers when vertical contracts are privately negotiated and not publicly observed; as a result, wholesale prices are equal to marginal costs and retail prices are rather competitive. This "opportunism problem" thus prevents the manufacturer from fully exerting its market power. This issue is very similar to that first analyzed by Hart and Tirole (1990) in the context of quantity competition. In this context, even without alternative upstream manufacturer, intrabrand competition, i.e. between retailers who sell the exact same good, is sufficient to generate relatively competitive equilibriums. Due

[12] Although GTS have to be non-discriminatory, they can still differ across distribution channels (e.g. GTS for hypermarkets, GTS for supermarkets, GTS for convenience stores) or be global but include specific terms (e.g. rebates) for a specific channel.

[13] Products are usually delivered to retail chains distribution platforms – and thus billed – several times a year.

to the vertical coordination problem, it is the downstream market structure who drives the retail price.

When negotiating the wholesale contract, a retailer and the manufacturer take the contracts offered to competing distributors as given and therefore do not internalize the retail margins on those products. In each secret negotiation, the manufacturer is then tempted to free-ride on the other retailers' sales. Deviations would induce externalities on the other retailers, who would face a lower demand, as the other retailers would behave more aggressively. Quite intuitively, this "opportunity problem" can be solved in two different ways. First, eliminating retail margins eliminates the incentive to free-ride. It can be done by setting the bilaterally negotiated retail and wholesale prices, as well as a price ceiling (and not price floors), equal to the monopoly price.

Second, price floors can nevertheless be used to solve the opportunism problem as long as the manufacturer is required to set the same price floor for all retailers (industry-wide price floor). This would eliminate the externalities on the other retailers from a deviation in a secret negotiation. Suppose for instance that the manufacturer sets a price floor equal to the monopoly price and wholesale prices equal to its marginal cost (and uses franchise fees to share the monopoly profit with the retailers). In a secret negotiation, the producer can no longer lower the price set by the negotiating retailer and thus the sales of the various retailers. In this context, a credible price floor common to all retailers is a credible commitment device for the producer. [14]

Allain and Chambolle (2005) use a similar framework to specifically analyze the effects of the Loi Galland. In particular, the formation of wholesale prices is assumed to take place in two different stages: the manufacturer first announces a (public) non-discriminatory wholesale price (corresponding to the General Terms of Sales), then the manufacturer and each retailer bilaterally negotiate (individualized) rebates. If two-part rebates (fixed fees and discount on the unit price) can be negotiated, their model is almost identical to that of O'Brien and Shaffer (1992), except for the fact that retailers might have some bargaining power: this does not affect the determination of retail prices but only profit sharing rules. Allowing the manufacturer to impose an industry-wide price floor thus eliminates any opportunism problem and restores monopoly prices. The situation is slightly different when wholesale tariffs can only be linear. In that case, an industry-wide price floor will be used to maintain high retail prices when the retailers' relative bargaining strength is high. Allain and Chambolle (2007) also allow for

[14]The opportunism problem faced by the monopolist producer is very similar to the inter-temporal pricing problem faced by a durable good monopolist. It has also been explored by McAfee and Schwartz (1994) and Rey and Vergé (2004a) using a more standard equilibrium concept (perfect Bayesian equilibrium) rather than the contract equilibrium concept à la Cremer and Riordan (1987) used by O'Brien and Shaffer (1992). See also Rey and Tirole (2007) for an overview of this literature.

interbrand competition and obtain a very similar result. Once again, the effect of a price floor common to all retailers is unambiguously positive when wholesale contracts include fixed fees on the top of the (constant) unit prices.

Biscourp, Boutin, and Verge (2008) study a bilateral duopoly with interlocking relationships similar to that of Allain and Chambolle (2007) but allow for endogenous market structure. The main difference between the two approaches relates to the equilibrium concept. To reflect different bargaining powers, Allain and Chambolle assume that hidden margins (rebates) are determined by simultaneous pairwise bargaining, which supposes that a manufacturer has two independent divisions, each of them negotiating with one retailer not taking into account the impact of its own negotiation on the other division. In contrast, Biscourp, Boutin, and Verge (2008) introduce an explicit dynamic multilateral framework: this is similar to the approach of de Fontenay and Gans (2005) who use Stole and Zwiebel's (1996) model of sequential bilateral bargaining, with renegotiation ("from scratch") in a case a relationship breaks-down.[15] As in de Fontenay and Gans (2005) or O'Brien and Shaffer (1992), wholesale contracts are bilaterally efficient. Therefore, introducing an industry-wide price floor will again remove intrabrand competition and, for a given market structure, lead to higher retail prices. Moreover, since it affects the profitability of each product, the price floor may also affect the equilibrium market structure. In the bilateral duopoly model with interlocking relationship, an industry-wide price floor guarantees that both brands with be available on both retailers' shelves. In the absence of such price floor, some products will be missing when intrabrand competition is too fierce. In that case, each retailer only carries one brand. Retail prices for the available product are lower than under the price floor regime, however, consumer surplus may be lower since some products are now missing. An industry-wide price floor may then be welfare-enhancing, however, this impact would tend to be relatively limited since it occurs when brand are highly substitutable. Because our data does not include any information on the sets of products that are available on the retailers' shelves, we will not be able to estimate the full impact of the Loi Galland but will only consider its impact on the prices of available products.

All the models presented above rely on the assumption that the price floor set by a manufacturer is common to all retailers. When contracts are secrets, this assumption is essential since it is the only way to commit not to secretly offer a better deal (i.e. a lower price floor) to the rival retailers. This assumption is no longer necessary when wholesale contracts are observable.

Like Allain and Chambolle (2007), Dobson and Waterson (2007) study bilateral duopoly with interlocking relationships and assume that manufacturers use (observable) linear wholesale

[15] Bedre (2007) considers a similar setting but assuming that wholesale contracts are observable.

prices. They show that the welfare effects of RPM depend on the relative degree of upstream and downstream differentiation as well as on retailers' and manufacturers' bargaining powers; RPM is shown to be socially harmful when retailers are in a strong bargaining position, because the double-marginalization problems generated by the restriction to linear wholesale prices are less severe in such circumstances.

In order to eliminate double marginalization problems and focus instead on the impact of RPM on interbrand and intrabrand competition, Rey and Vergé (2004b) allow manufacturers to use (efficient) two-part wholesale tariffs. RPM is then shown to be unambiguously harmful when (i) either the manufacturers have the bargaining power and there is no retail bottleneck, or (ii) retailers have the bargaining power and there is no supplier bottleneck.

Overall, even if the models are simpler with an upstream monopoly, the previous models with bilateral duopolies show that the existence of interbrand competition does not eliminate the opportunism problem and thus the anticompetitive potentials of RPM. However, these models are likely to be irrelevant if there exist a very large number of small upstream firms. First, these small firms are unlikely to sell to many retailers (otherwise, they would not be small). Second, they are likely to be very substitutable and would make near to zero profits. For these two reasons, they are unlikely to be strategic actors. Then, opportunism is not an issue, nor RPM. As a consequence, these firms would not be affected by the Loi Galland.

At last, fierce intrabrand competition can reduce retailers' incentives to provide pre-sales services to consumers. Price floors used to solve this problem might be beneficial to consumers and total welfare, despite leading to higher retail prices.[16] However, this requires that consumers are uninformed about the product's characteristics and that potential gains (from lower prices) exceed additional transportation costs. This is irrelevant for groceries, where consumers favor one-stop shopping strategies for their repeated purchases.

These theoretical models all suggest that the Loi Galland could have been responsible for the increase in prices that has been observed in France after 1997. This increase could also have been explained by increases in production or distribution costs. The price increase would however be very different in these two possible scenarii. The effect of an industry-wide price floor comes from the elimination of intrabrand competition which is not affected by a change in costs. On the contrary, in the absence of price floors, retail prices are expected to respond to competition between retailers in each local market.

First, we expect some positive correlation between retail prices and our measure of concen-

[16] See Telser (1960). Rey and Vergé (2008) provide a recent survey of the effects of resale price maintenance.

tration (described in the previous section), which acts as a proxy for the degree of competition in local grocery markets. This correlation should disappear (or at least be strongly reduced), after 1997. Note that a change in costs would not affect this correlation.

Moreover, on a given local market, hypermarkets (selling area over 2500 m^2) compete with supermarkets (selling area between 400 and 2500 m^2), convenience stores (selling area less than $400m^2$) and hard-discount stores. However, these different formats do not necessarily face the same demand: less elastic demand for convenience stores and supermarkets located in city centers, low income consumers for hard-discount stores. Inner city consumers face the choice between local retailers, at walkable distance, and large hypermarkets, located at the outskirts of the city. Among the local retailers, they will strongly favor the closest. Inner city retailers are thus very differentiated for consumers. On the contrary, driving times are less important. We thus expect competition to be fiercer between hypermarkets. As a result, the impact of the Loi Galland should have been larger for these stores.

At last, hard-discount stores do not often sell branded products and thus would not have seen changes in the terms offered by their suppliers. Therefore, the price increase should have been very limited for this format since it would only be a response (positive if we assume that prices are strategic complements) to the increase in prices of rival formats.[17]

2.2 National Negotiations and Local Pricing Strategies

The theoretical arguments presented above suggest that intrabrand competition should have been totally eliminated by the enactment of the Loi Galland. In practice, the situation is probably less extreme for several reasons.

First, the *"general terms of sales"* and the various rebates are negotiated at the national level between a manufacturer and the buying group of a given chain. Large retail chains usually have a unique buying group – or purchasing unit – for the whole chain that might include several "fascias" or brands. Retail prices are however set locally and depend on the local market conditions. Therefore, the minimum retail price implicitly set by the manufacturer is a nationwide-price based on average market conditions and might not be binding everywhere. Markets that initially had relatively high prices – either because of local demand conditions or of high concentration – are thus unlikely to be affected by the Galland Act. On the contrary, markets where prices were initially lower have been affected by the new minimum price and

[17] Given that the period we looked at also corresponds to the development of the hard-discount format in France, it might even be the case that hard-discount prices went down during that period.

prices thus went up in these markets. Therefore the inflationary impact should to have been higher on markets where prices were initially relatively low.

2.3 Ban on Below-Invoice Prices or Planning Regulations?

A second law that was passed at almost the same as the Loi Galland. Introduced in July 1996, the Loi Raffarin reinforces planning restrictions, reducing the threshold for a mandatory retail permit (obtained through a lengthy administrative procedure) from 1000 to 300 sq meters. Some observers have argued that the Loi Raffarin was also responsible for the retail price inflation since retailers were no longer threatened by potential entry in their local markets.

However, we have several reasons to believe that the effects that our empirical analysis highlights are not affected by the change in planning regulations.

First, barriers to entry were already in place since the 1973 Loi Royer for large stores: the Loi Royer introduced the mandatory retail permit for stores over 1000 sq. meters, i.e. hypermarkets and most supermarkets. This law has been shown to have had significant effects on prices and job creation in the groceries sector (see Bertrand and Kramarz 2002). Moreover, in 1993, the Finance Minister gave instructions to the local commissions granting these retail permits to slow down the evaluation process (see Askenazy and Weidenfeld 2007). This led to a significant drop in the number of store extensions and openings after 1993. The Loi Raffarin was merely seen as a way to legalize that practice. Since our data covers a period starting in 1994, we thus expect the impact of the Loi Raffarin to have been rather limited.

Second, opening a new store is a complex process that can take years, whereas retail prices can be adjusted daily. It is thus very unlikely that retailers were unable to take advantage of their market power because of the threat of entry before 1996. Moreover, even if this were to be the case, retail prices (pre-1997) would not be correlated with current market concentration since only potential concentration should matter. Thus, our results on this correlation in 1994 (see below) does not support contestable market theories.

3 Cross-Section Influence of Market Concentration

We expect the correlation between prices, local concentration and proxies for demand to have decreased after the enactment of the Loi Galland. We now turn to empirical tests of this prediction. Local concentration experiences little variation between 1994 and 1999. Hence, there is no sufficient source of variation to identify the price - concentration correlation using first differences. We then have to rely on cross-sectional estimations, run for 1994 and 1999. As local concentration is available yearly, we aggregate our monthly available price at year level.[18]

For each date, we estimate a reduced-form price equation for product p in store s (of fascia $f(s)$ and type $ty(s)$, in city $c(s)$):

$$\begin{aligned} log(P^t_{p,s}) &= cst^t + \underbrace{\beta^t_{MC}.MC^t_{c(s)}}_{\substack{\texttt{local concen-}\\ \texttt{tration in } c(s)}} + \underbrace{\gamma.Y_{c(s)}}_{\substack{\texttt{other catchment}\\ \texttt{area specifics}}} \\ &+ \underbrace{\sum_{p'} \alpha^t_{p'}.\mathbb{1}_{p=p'}}_{\substack{\texttt{year product}\\ \texttt{dummies}}} + \underbrace{\sum_{ty'} \alpha^t_{ty'}.\mathbb{1}_{ty(s)=ty'}}_{\substack{\texttt{year type}\\ \texttt{dummies}}} + \underbrace{\sum_{f'} \alpha^t_{f'}.\mathbb{1}_{f(s)=f'}}_{\substack{\texttt{year fascia}\\ \texttt{dummies}}} + \varepsilon^t_{p,s} \end{aligned}$$

Data for 1994 and 1999 are pooled so as to run a single regression allowing to test for differences in coefficients across years. Data for catchment areas include our measure of market concentration, as well as overall population and wealth, all constructed at several levels of market aggregation.[19] We also control for the types of stores. Despite the large number of control variables, this regression still omits unobserved determinants of prices that might also impact market concentration. Estimates of β^t_{MC} might thus be biased. As usual, cross section regressions provide valuable insights into variable relationships, but do not lend themselves easily to causal analysis. However, if the bias due to the endogeneity of local concentration is constant over time, the changes in coefficients can be attributed to the Loi Galland, and the difference between the coefficients can be interpreted as the causal impact of Loi Galland. This test will indicate if correlation has decreased during the provided, which provides a first way of checking if the prediction is supported by the data.

Table 4 shows the results of cross-section regressions for 1994 and 1999, for two different constructions of local markets. All stores within a 10 km range are included in the first set of regressions. In the "5/10/20" treatment, hypermarkets up to 20 km were included, a well as supermarkets and hard discounters up to 10 km and all other stores up to 5 km. For each set, the first column provides the coefficient in 1994, while the second shows results for 1999. Regressions for both years are done simultaneously so that it is possible to test for differences in the coefficients across years.

[18]We expect prices to be correlated within markets, given that we imperfectly control for catchment area specifics. Thus, we use robust variances estimates clustered at town level for inference.

[19]Population and wealth come from the 1999 Census, and do not vary over time.

Table 4 : Local Concentration and Prices in 1994 and 1999

	10 km		5/10/20 km	
	1994	1999	1994	1999
Supermarket	0.056***	0.027***	0.056***	0.027***
	(0.004)	(0.004)	(0.004)	(0.004)
Hard discount	-0.363***	-0.435***	-0.362***	-0.435***
	(0.019)	(0.012)	(0.019)	(0.012)
Convenience	0.223***	0.213***	0.223***	0.213***
	(0.007)	(0.010)	(0.007)	(0.010)
Magasin Populaire	0.068***	0.068***	0.068***	0.068***
	(0.007)	(0.006)	(0.007)	(0.006)
Hypermarket	ref.	ref.	ref.	ref.
Market population (log)	0.026***	0.015***	0.026***	0.015***
	(0.003)	(0.003)	(0.003)	(0.002)
Market income (log)	0.027**	0.018	0.027**	0.019*
	(0.013)	(0.016)	(0.014)	(0.016)
Market concentration	0.153***	0.055*	0.172***	0.063*
	(0.032)	(0.028)	(0.035)	(0.037)
Product Dummies	Y	Y	Y	Y
Number of obs.	44051		44051	

Note: Robust OLS estimators clustered by towns. R-squared : 0.997. In parenthesis: standard errors. 3, 2 and 1 stars respectively mean 99, 95 and 90 percent significance for a bilateral test. Not reported : year dummies by product. Sources : INSEE (IPC,CENSUS), LSA. The "market" includes all stores within 10km in the "10 km"' treatment. It includes hypermarkets up to 20km, supermarkets and hard discounters up to 10 km and all other stores up to 5 km in the "5/10/20" treatment.

As far as market concentration is concerned, the results are striking. In 1994, prices are quite intuitively higher when local market concentration is higher. We use an Herfindahl-Hirschman local market concentration indicator.[20] Therefore, for stores located in perfectly concentrated markets, prices are 15% higher than for stores located in densely equipped areas. Stores located in areas more concentrated of one standard deviation of our concentration index were more expensive by about 1.5% in 1999. There results are quite in line with previous comparable cross-section estimates for other European countries. For instance, Barros, Brito, and de Lucena (2006) use data on the Portuguese grocery retail industry and find that prices are 15% higher in extremely concentrated areas than in perfectly competitive areas (a change of a standard error would lead to an increase of 1%). Asplund and Friberg (2002) find smaller, but comparable, figures for the Swedish market. The shortcomings of cross section regressions *when it comes to causal analysis* have been emphasized above. However, if the point estimate

[20]By construction this indicator belongs to the interval]0,1], as it is computed as the sum of squared sales surfaces. It measures the concentration of sales surfaces but may also be interpreted as a proxy for concentrations of sales.

in year 1994 should be interpreted with caution, the comparison with the same estimates for 1999 is striking. Three years after the enactment of Galland Act, there is almost no correlation remaining between prices and concentration, even though the structures of markets have only marginally been altered between 1994 and 1999. This result is not sensitive to the definition of local markets, as shown by table 4.

Our results also confirm the commonly shared opinion that, conditional on product and local market characteristics, hard discounters are by far the cheapest type of stores, before hypermarkets, supermarkets and convenience stores, *magasins populaires* lying somewhere in-between. In terms of evolutions, the average differences in prices between hypermarkets, convenience stores and *magasins populaires* has been stable on the period. Compared to hypermarkets, supermarkets were less expensive in 1999 than in 1994. On the contrary, the spread between prices in hard-discounts and in hypermarkets increased during the period. Even though this assumption is to be confirmed by a dedicated analysis (in section 4), both facts are consistent with our predictions that prices would have converged to the most expensive values: supermarkets are more expensive than hypermarkets, and hard-discounts are not directly affected by the Loi Galland. Finally, our results suggest that the influence of market population, which is a measure for population density, also decreased during the period.[21] The fact that prices are positively correlated with population density, for a given market concentration, 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 this correlation during such a short period is consistent with a uniformization of prices due to Galland Act.[22]

These results are robust to further changes in specification, in particular to the introduction of fascia dummies. Stores sharing the same fascias most of the time also have the same type. Adding fascia dummies to the regression with type dummies raises identifications issues and makes the interpretation of both sets of coefficients difficult. However, controlling for fascias might be important as regards to the consistence of the other coefficients as fascias might also be important determinants of prices. Results are given in table 5. It shows that the previous results on the differences of coefficients are very robust to this specification.

[21] The size of the catchment areas are fixed, and the logarithm of total population is hence a proxy for population density.

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

Table 5 : Local Concentration and Prices in 1994 and 1999

	10 km		5/10/20 km	
	1994	1999	1994	1999
Market population (log)	0.016***	0.009***	0.018***	0.009***
	(0.003)	(0.002)	(0.003)	(0.002)
Market income (log)	0.023***	0.025**	0.022**	0.026**
	(0.010)	(0.013)	(0.011)	(0.013)
Market concentration	0.103***	0.018	0.138***	0.022
	(0.028)	(0.024)	(0.032)	(0.031)
Product Dummies	Y	Y	Y	Y
Type Dummies	Y	Y	Y	Y
Fascia Dummies	Y	Y	Y	Y
Number of obs.	41877		41877	

Note: Robust OLS estimators clustered by towns. R squared : 0.997. In parenthesis: standard errors. 3, 2 and 1 stars respectively mean 99, 95 and 90 percent significance for a bilateral test. Not reported : year dummies by product, year dummies by type and fascia. Sources : INSEE (IPC,CENSUS), LSA. The "market" includes all stores within 10km in the "10 km"' treatment. It includes hypermarkets up to 20km, supermarkets and hard discounters up to 10 km and all other stores up to 5 km in the "5/10/20" treatment.

Table 6 shows the results for prices in hypermarkets only.[23] In 1994, coefficients for local concentration are larger for both sets of estimates, compared to these for the whole population in table 5. The sample has also been dramatically reduced and estimates are less precise. For the two definitions of local markets, the difference is larger for hypermarkets than for the whole population, confirming that hypermarkets have an influence on, and are influenced by, hypermarkets located further apart. Overall, our results suggest that the enactment of Loi Galland had the same influence on the larger stores of our sample, with a larger magnitude since they were initially more receptive to local competition.

4 Long-Term Price Increase

The cross-sectional approach, although providing valuable insights into the effects of the Loi Galland, must be complemented by a more direct test of our predictions, taking advantage of the panel dimension of our data.

As mentioned earlier, we expect retail prices to have increased more in stores where they

[23]Several other robustness checks were implemented, using prices of hypermarkets and supermarkets only or alternative indicators of local concentration, including some types of stores only - e.g. supermarkets and hypermarkets only. All the results are consistent with those presented in this paper.

TABLE 6 : Local Concentration and Prices in 1994 and 1999
Hypermarkets only

	10 km		5/10/20 km	
	1994	1999	1994	1999
Market population (log)	0.019***	0.008*	0.021***	0.005
	(0.005)	(0.004)	(0.005)	(0.004)
Market income (log)	0.005	0.016	0.004	0.017
	(0.021)	(0.020)	(0.020)	(0.020)
Market concentration	0.160***	0.035	0.222***	-0.006
	(0.070)	(0.053)	(0.068)	(0.063)
Product Dummies	Y	Y	Y	Y
Fascia Dummies	Y	Y	Y	Y
Number of obs.	15570		15570	

Note: Robust OLS estimators clustered by towns. R squared : 0.997. In parenthesis: standard errors. 3, 2 and 1 stars respectively mean 99, 95 and 90 percent significance for a bilateral test. Not reported : year dummies by product, year dummies by fascia. Sources : INSEE (IPC,CENSUS), LSA. The "market" includes all stores within 10km in the "10 km"' treatment. It includes hypermarkets up to 20km, supermarkets and hard discounters up to 10 km and all other stores up to 5 km in the "5/10/20" treatment.

were initially lower. In the cross-sectional approach, we have been running regressions of prices on their determinants, such as market concentration, and comparing the correlation of cross-sectional price with market concentration before and after the implementation of the Loi Galland. In order to obtain a more direct test, we now want to run a regression of price growth on initial price, controlling for various determinants of price growth over the period, such as the change in market concentration.

Implementing this approach requires great care, in order to avoid biases due to "regression to the mean". To illustrate this, assume for simplicity that prices are determined as the sum of a stationary value and some stochastic shock, $P_t = P^* + \varepsilon_t$. The shock may be the results of measurement error or idiosyncratic supply and/or demand shocks. If ε_t is white noise, it is easily seen that $cov(P_t - P_{t-1}, P_{t-1}) = -var(\varepsilon_t)$. Because of the correlation between unobserved determinants of price growth and initial price, we expect a negative correlation between price growth and initial price. In other words, initial price may be endogenous in the regression we want to run, and the parameter of interest may thus be biased toward negative values.

Before presenting our strategy to deal with this potential endogeneity issue, we first discuss the nature of shocks that can arise in the context of our empirical analysis. Unobserved shocks affecting prices may arise at different levels.

- **At product level:** Random price variations for a given product in a given store can arise due to special offers. Special offers can be determined on an inventory rotation basis, when demand is itself random. If so, items on special offer in a given month of 1994 are likely to be cheaper than the same product in another store at the same moment, but unlikely to be on special offer again during the same month of 1999. This kind of random variation generates regression to the mean. Special offers on popular items or brands can also correspond to a strategy of stores aiming to attract customers. If special offers are determined on a rotation basis within a set of popular items, regression to the mean may arise. Aggregating data at year and store level provides a simple way of eliminating or, at least mitigating, this problem.

- **At store level:** Store level prices may experience random short term variation due to idiosyncratic shocks of supply and/or demand. A store may for instance face unusually high demand if a music festival happens to take place in the neighborhood. Aggregating data at store level does not help in this case, and endogeneity must therefore be dealt with in a different way. A first way of alleviating the problem of regression to the mean consists in replacing the initial price P_{t-1} by an average of past prices, computed over as many dates as possible in order to smooth out shocks. This may not be sufficient as our sample only allows us to use three dates previous to the Loi Galland. We thus complement this approach by an instrumental variable strategy, whereby we instrument the averaged out initial price by initial market concentration.

- **At regional level:** Some regions (e.g., the more industrial ones) may be more sensitive to macroeconomic cycles. The year 1994 corresponds to the end of a recession, whereas 1999 corresponds to the top of a cycle. More sensitive regions will thus have larger aggregate variations in demand between 1999 and 1994 than the less sensitive ones. We already control for income; however, this may not be sufficient. We thus include regional dummies in our regressions to control for this source of regression to the mean.[24]

Before turning to the empirical test, we describe and discuss the aggregation process of prices at the store level, which allows us to construct our dependent variable.

4.1 Estimating store effects

Because investigating precise pricing strategies of retailers is beyond the scope of this paper, we build an indicator of the relative price for each store in a given year. Our data will include

[24] We actually use dummies for each *départements*, a smaller administrative unit (94 *départements* when we exclude Corsica and overseas territories) than the region (21).

one observation for each store each year, which solves the issues of dealing with data at product level. More precisely, *for each year t*, we consider the following model for the price of a product p in store s of type ty, during month m:[25]

$$log(P_{p,s,m}) \quad = \quad const + \underbrace{\sum_{ty'} \alpha_{ty'} \times \mathbb{1}_{ty(s)=ty'}}_{\substack{\texttt{type}\\\texttt{dummies}}} + \underbrace{\sum_{p'} \alpha_{p',m'} \times \mathbb{1}_{p=p'} \times \mathbb{1}_{m=m'}}_{\substack{\texttt{month product}\\\texttt{dummies}}} + y_s + \quad \eta_{p,s,m}$$

This specification above captures seasonal changes potentially affecting the price of each product, as well as the type of store.[26]

We are mainly interested in recovering y_s, the "store effects", for each year. By definition $\mathbb{E}\left\{\eta_{p,s,m}|y_s\right\} = 0$. In order to compute $\widehat{y}_s$, we must first estimate the $\widehat{\alpha}$ parameters consistently.

Our sample of products consists of items commonly sold in all types of stores. Since the data used in our study are used by INSEE to compute inflation in France, we believe that products are surveyed using a proper sampling scheme and thus assume that there is no selection bias in our sample. We therefore use OLS under the assumption of strict exogeneity of $y_s + \eta_{p,s,m}$. The composite structure of the error would normally require robust variance matrix estimators. However, since we are not interested in inference on α, this is unnecessary here.

Let $\widehat{log(P_{p,s,m})}$ denote the price predicted value (actually log(price)). Each year and for each store, we are able to compute an estimate of the store effect by averaging out residuals over products and months:

$$\widehat{y}_s \quad = \quad \overline{log(P_{p,s,m}) - \widehat{log(P_{p,s,m})}} \quad = \quad y_s + \zeta_s$$

Our variable of interest y_s is thus measured with error ζ_s, which might create spurious correlation between the price differences and initial prices. We provide a detailed evaluation of the magnitude of this bias in Appendix A, and show that store effects seem to be sufficiently precisely estimated to consider the bias as negligible.[27]

[25]For expositional simplicity, we omit the subscript for time, but, all variables implicitly depend on the year t.
[26]Different specifications are possible, for instance omitting type dummies. Our results are robust to such changes.
[27]Besides, averaging the initial price over three dates in itself also mitigates endogeneity problems.

4.2 Long-term differential price increase

Estimating "store effects" allows us to deal with the problems created by unobserved random shocks at product level. From now on, the store effect is denoted y_s^t to emphasize time variation; it aggregates all the information about the (idiosyncratic) pricing strategy of store s during year t.

Let us assume that random shocks follow the process $\varepsilon_s^{t+1} = \rho\varepsilon_s^t + \xi_s^{t+1}$, with $0 \leq \rho < 1$. Ignoring other time varying determinants of store effects, it appears that the variations of store effects are always correlated with initial levels, and this correlation is relatively larger when the process is weakly persistent.[28] Even in the absence of any structural break generated by the enactment of the Loi Galland, our regression would conclude to a negative link. In order to deal with this endogeneity issue, we first use the mean $\widetilde{\overline{y_s^t}}$ of store effects computed over the three years, 1994, 1995 and 1996, to reduce the potential bias. Our simulations tend to show that the bias may nevertheless still be large when random shocks exhibit little persistence.[29]

We therefore turn to instrumental variables, using initial local market concentration MC_s^t to instrument initial store price $\widetilde{\overline{y_s^t}}$. The validity of such an instrument relies on the assumption that market concentration is strongly correlated with initial price $\widetilde{\overline{y_s^t}}$ but uncorrelated with the equation residual $\varepsilon_s^{t+1} - \varepsilon_s^t$. In other words, we assume that instantaneous unobserved shocks may translate into price changes but do not affect local market concentration.

Market concentration is computed over a relatively large area surrounding the store, according to our definition of local markets. A demand shock affecting a single store within this market is thus expected to have a much smaller impact on the whole local market.

Agents determine their development strategies according to long-term prospects, rather than in response to short-term events. Store construction lags is a first obvious reason. Furthermore, barriers to entry are rather important in the French grocery sector (see for instance Bertrand and Kramarz 2002). Opening a new store or extending an already existing one needs to be approved by a local commission. Even in case of success, the overall process for opening a new store generally takes several years. It is thus highly unlikely that market structures should react to short-term positive demand shocks affecting the local markets. The time span of store closure is typically smaller than for store openings. However, it generates heavy opportunity

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[28]We regress the difference between 1999 and 1994 (5 years) on the initial level. We thus have $cov(y_s^{t+5} - y_s^t, y_s^t) = -(1-\rho^5)var(\varepsilon_s)$. If $\rho = 1$, the process is not stationary, but $y_s^{t+1} - y_s^t$ is uncorrelated with y_s^t.

[29]We then have $cov(y_s^{t+5} - y_s^t, \widetilde{\overline{y_s^t}}) = -\frac{1}{3}(1-\rho^5 + (\rho+\rho^2)(1-\rho^2))var(\varepsilon_s)$. The bias is reduced by a third in the worst-case scenario where $\rho = 0$.

costs as the ability to open a new store is questionable, given the restrictive regulations on openings. Besides, the existence of large retail chains is also likely to smooth the consequences of short-term adverse local shocks. From a practical standpoint, market concentration appears to be extremely inert in our data. Overall, we thus expect our identification condition to hold, especially in the very case where endogeneity is most likely to be an issue.[30]

Finally, in order to deal with the potentially differential impact of macroeconomic or regional shocks, we always include dummies for each *département* in our regressions.

We run the following regression over the period 1994-1999:

$$\begin{aligned}\Delta\widehat{y}_s \;=\; & cst + \alpha\overline{\widehat{y}_s} + \underbrace{\beta_{MC}.\Delta MC_{c(s)}}_{\substack{\texttt{variation of local}\\ \texttt{concentration in } c(s)}} + \underbrace{\beta_{HD}.\Delta HD_{c(s)}}_{\substack{\texttt{variation of the share}\\ \texttt{of Hard Discount in } c(s)}} \\ & + \underbrace{\sum_{dep'} \alpha_{dep'}.\mathbb{1}_{dep(s)=dep'}}_{\substack{\textit{département}\\ \texttt{dummies}}} + \underbrace{\gamma.Y_{c(s)}}_{\substack{\texttt{other catchment}\\ \texttt{area specifics}}} + \underbrace{\sum_{ty'} \alpha_{ty'}.\mathbb{1}_{ty(s)=ty'}}_{\substack{\texttt{type}\\ \texttt{dummies}}} + \varepsilon_s\end{aligned}$$

where Δ denotes differences taken between 1994 and 1999 and $\overline{\widehat{y}_s}$ is an average of store effects computed over the three years, 1994, 1995 and 1996. We first run an OLS regression using a robust variance matrix clustered by towns for inference. We then run an OLS regression of $\overline{\widehat{y}_s}$ on all exogenous variables, as well as our instrument $MC_{c(s)}$ (market concentration in 1994). Recovering the residual of this first stage regression, we then run the first OLS regression, augmented by the residual of the first stage regression. This provides a convenient test of endogeneity, asymptotically equivalent to the Hausman test. This also provides us with the point estimates of the two-stage least squares and allows us to compare the magnitudes of the OLS and TSLS estimates.[31]

Results for the balanced sample of 1348 stores (of all types) across 1994-1999 are presented in table 7. The results support the prediction that prices have increased more where they were initially lower. This is the case using both OLS and TSLS. Besides, there is no evidence of endogeneity of the initial price in the regression, as the coefficient of the first step residual in the augmented regression is not significant. The effect is slightly more important, even though less precisely estimated, if we run the same regressions keeping hypermarkets only (see table 8).

[30]It is in the absence of persistence of shocks ($\rho = 0$) that the issue of endogeneity is the most troublesome.

[31]If exogeneity is rejected, it is however important to perform the TSLS in order to get appropriate standard errors.

Table 7 : Long term price variations

	OLS	TSLS	Aug. Reg.
Initial Average Individual Effects	-0.273***	-0.579***	-0.579**
	(0.043)	(0.225)	(0.232)
First Stage Residual	-	-	0,301
	-	-	(0.240)
Δ Market Concentration	-0.117	-0.136	-0.136
	(0.094)	(0.108)	(0.095)
Δ Share of Hard Discounts	-0.125	-0.175	-0.175
	(0.098)	(0.108)	(0.107)
Catchment Area Specifics	Y	Y	Y
Regional Dummies	Y	Y	Y
Type Dummies	Y	Y	Y
R^2	0.11	0.04	0.11
Number of obs.	1348	1348	1348

Note: Robust variance estimators clustered by towns. In parenthesis: standard errors. 3, 2 and 1 stars respectively mean 99, 95 and 90 percent significance for a bilateral test. Not reported : log(population), log(income), dummies by *département* and dummies by type. Sources : INSEE (IPC,CENSUS), LSA. In the TSLS and in the augmented regressions, initial average individual effects are instrumented by "5/10/20" market concentration indices.

Table 8 : Long term price variations
Hypermarkets only

	OLS	TSLS	Aug. Reg.
Initial Average Individual Effects	-0.390***	-0.605	-0.605*
	(0.075)	(0.397)	(0.352)
First Stage Residual	-	-	0,219
	-	-	(0.351)
Δ Market Concentration	-0.372**	-0.384*	-0.384**
	(0.170)	(0.200)	(0.168)
Δ Share of Hard Discounts	-0.228**	-0.217*	-0.217*
	(0.116)	(0.114)	(0.113)
Catchment Area Specifics	Y	Y	Y
Regional Dummies	Y	Y	Y
R^2	0.30	0.27	0.30
Number of obs.	322	322	322

Note: Robust variance estimators clustered by towns. In parenthesis: standard errors. 3, 2 and 1 stars respectively mean 99, 95 and 90 percent significance for a bilateral test. Not reported : log(population), log(income), dummies by *département* and dummies by type. Sources : INSEE (IPC,CENSUS), LSA. In the TSLS and in the augmented regressions, initial average individual effects are instrumented by "5/10/20" market concentration indices.

5 Conclusion

Using a unique dataset on retail prices of a large number of products, collected in a large and representative sample of grocery stores, this paper provides a first empirical evaluation of the effects of the 1996 below-cost pricing regulations (Loi Galland). More precisely, we show that there is strong evidence to support the claim that the Loi Galland effectively led to the elimination of (or at least an important reduction in) intrabrand competition. This could, at least partially, explain the sharp increase in prices of groceries that occurred after 1997. We provide two different tests of our theoretical predictions. Firstly, in the spirit of the empirical literature linking prices to market structure, 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. The magnitude of the correlation is also consistent with previous analysis conducted on the same sector in other European countries. Two years after the enactment of the new legislation, the correlation has however vanished confirming that retail chains are no longer competing fiercely. We then provide a second (more direct) test, estimating individual store effects and observing that prices were increasing more in stores that were initially lower.

It should however be noted that this paper does not provide a complete evaluation of the effects of the Loi Galland. For instance, although our results support the claims that it was responsible for (at least part) of the price increase that occurred after 1997, there might have been other effects on the lines of products carried, or on the quality of services provided by retailers. Moreover, one of the reasons to introduce the legislation was to level the playing field between small businesses and large retail chains. The Galland Act might have partly achieved this role, by filling a part of the price disadvantages of small village shops. However, anecdotal evidence suggests that this effect was probably rather limited (see Commission Canivet 2005). Many convenience stores that were previously independent are now part of retail chains (either because they have been taken over by these chains or because they joined them as franchisees).[32] Moreover, it does not seem that the rate of closure of independent specialized shops such as butchers, fishmongers or bakeries has slowed down after 1997. One lesson from economic theory is that a problem is more efficiently solved by addressing directly the issue. Price regulations are thus unlikely to constitute the best policy to help the few remaining rural shops or the independent specialized retailers.

[32] Less than 15% of small grocery retailers are independent.

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A Estimating the impact of the first step in section 4

As stated in section 4, we use estimated values of y_s in our regression. We thus measure y_s with error, this error being:

$$\zeta_s = \frac{1}{N_s} \sum_{p \in \mathcal{P}_s, m \in \mathcal{M}_{p,s}} \left[x_{p,s,m}(\beta - \widehat{\beta}) + \eta_{p,s,m} \right]$$

The first part in the error term is the consequence of the estimation of β. The variance of the estimator $\widehat{\beta}$ being proportional to the inverse of the size of the overall sample, β is very precisely estimated given the size of our dataset. We thus neglect the first part of the error term.

The second part is the average of all the error terms, which are all zero-mean conditional on $x_{p,s,m}$ and y_s. Our estimator of y_s, $\widehat{y}_s$, is therefore unbiased.

However, since we compute differences of the estimated effects , this second error term still generates a bias in our regressions. Instead of regressing $y_s^{1999} - y_s^{1994}$ on $\overline{y}_s^{1994-96}$, we do the regression of the estimated counterparts.[33] Assuming that ζ_s are uncorrelated and homoscedastic, we have:

$$\begin{aligned} Cov\left[\Delta^{99-94}\widehat{y}_s, \overline{\widehat{y}_s}^{94-96}\right] &= Cov\left[\Delta^{99-94}y_s, \overline{y}_s^{94-96}\right] + Cov\left[\Delta^{99-94}\zeta_s, \overline{\zeta}_s^{94-96}\right] \\ &= Cov\left[\Delta^{99-94}y_s, \overline{y}_s^{94-96}\right] + \tfrac{1}{3}Var\left[\zeta_s\right] \end{aligned}$$

and

$$Var\left[\overline{\widehat{y}_s}^{94-96}\right] = Var\left[\overline{y}_s^{94-96}\right] + \tfrac{1}{3}Var\left[\zeta_s\right]$$

Then:

$$\frac{Cov\left[\Delta^{99-94}y_s, \overline{y}_s^{94-96}\right]}{Var\left[\overline{y}_s^{94-96}\right]} = \frac{\frac{Cov\left[\Delta^{99-94}\widehat{y}_s, \overline{\widehat{y}_s}^{94-96}\right]}{Var\left[\overline{\widehat{y}_s}^{94-96}\right]} - \frac{Var[\zeta_s]}{3Var\left[\overline{\widehat{y}_s}^{94-96}\right]}}{1 - \frac{Var[\zeta_s]}{3Var\left[\overline{\widehat{y}_s}^{94-96}\right]}}$$

Calculating the bias created by the error requires estimating the variance of ζ_s. Assuming that $\eta_{p,s,m}$ are uncorrelated and homoscedastic, it is possible to get an estimator of $Var(\eta)$ using the within store estimator corresponding to fixed effect method. Given that our sample is unbalanced, we need to correct for the heteroscedasticity created by the difference in the number of observations for each store. If $\widehat{\xi}_{p,s,m}$ is the residual of the within store estimator, an estimator

[33] For the sake of simplicity, we compute the bias in the absence of other explanatory variables. In the complete case, it would be necessary to apply the Frisch-Waugh theorem and to project all variables on the orthogonal of the other variables.

of $Var(\eta)$ is:

$$\widehat{Var}(\eta) = \frac{1}{N} \sum_{s} \sum_{p \in \mathcal{P}_s, m \in \mathcal{M}_{p,s}} \left(\frac{\widehat{\xi}_{p,s,m}}{1 - \frac{1}{N_s}} \right)^2$$

Besides:

$$Var(\epsilon_s|s) = \frac{Var(\eta)}{N_s} \implies Var(\epsilon_s) = \mathbb{E}\left\{Var(\epsilon_s|s)\right\} = Var(\eta)\mathbb{E}\left\{\frac{1}{N_s}\right\}.$$

This in turn implies that:

$$\widehat{Var}(\epsilon_s) = \overline{\frac{1}{N_s}} \widehat{Var}(\eta).$$

It is now possible to correct our estimator. This correction is linked to:

$$\frac{Var\left[\zeta_s\right]}{3Var\left[\widehat{\overline{y}}_s^{94-96}\right]}, \text{ which in our sample is: } \frac{\widehat{Var}\left[\zeta_s\right]}{3\widehat{Var}\left[\widehat{\overline{y}}_s^{94-96}\right]} < 1\%.$$

The bias created by the first step is thus negligible when $\eta_{p,s,m}$ are uncorrelated and homoscedastic.

This last assumption may however fail to hold. For instance, due to product complementarity, for a given store in a given month, $\eta_{p,s,m}$ may be negatively correlated. Besides, for a given product in a store, $\eta_{p,s,m}$ may also be correlated, either positively if the pricing strategy is stable over time (e.g., *"every day low price"* policy for some products), or negatively when this pricing strategy evolves regularly (e.g., *"high-low"* or sales strategies). The variance matrix of $\eta_{p,s,m}$ may then be very hard to specify. Our estimator of the variance of ζ_s may then underestimate the noise (if there is important positive correlation), or alternatively overestimate it (if there is massive negative correlation). Instruments are a general method to deal with such measurement errors. The instrument we use in our estimation (initial concentration) is assumed to be uncorrelated with short-term demand shocks. We assume that the residuals of prices, $\eta_{p,s,m}$, once we control for types of products and stores and time invariant heterogeneity (by time invariant, we hereby mean within a year), are not correlated with initial concentration. This assumption would be violated if, for instance, some types of products were only surveyed in very competitive stores, or if some products were more often used as loss leaders in very competitive stores. However, if it holds, the instrumentation of our main regression also solves the issues created by the use of estimated values from the first step.

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