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

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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 intrabrand competition is expected to lead to an increase in retail prices. Using a unique dataset merging CPI micro data at store and product level with local competition data, we find evidence supporting this claim. Across the period, prices have increased more where they were initially lower. In the meantime, the link between local competition and retail prices has vanished. Modifying or revoking the existing legislation (as decided in Ireland in December 2005) would then be expected to reduce retail prices.

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

JEL: L42, L81, K23.

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INTRODUCTION

The French retail industry has faced several regulatory changes in the past forty years. As argued by Bertrand and Kramarz (2002), restrictive planning regulations have created large barriers to entry leading to high prices and low employment. In 1963, a regulation banning below-cost prices was passed, but the unclear definition of the threshold meant that this ban proved practically ineffective. This remained the case until the Loi Galland, come into force in January 1997, defined the applicable threshold for below-cost prices as the invoice price, that is, the price paid by the retailer at the time of delivery.¹ The important aspect of this definition is that it cannot 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 retailers.

This issue has recently been at the center of a fierce debate in France. The average prices of food products have indeed 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 (7.7% vs. 16.2% over the 1990-1996 period). 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. This inflationary trend also seemed to be specific to France where food prices increased significantly more than in other Euro zone countries.² The common feeling was that the enactment of the Loi Galland had eliminated price competition between the main retail chains. In 2004, a group of experts (Commission Canivet) was thus commissioned by the Minister of Finance 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.

According to this line of reasoning, the Loi Galland gave producers and retailers the ability to manipulate the threshold in order to freely set an industry-wide price floor. Consistent with the theoretical results of O'Brien and Shaffer (1992), the below-invoice price law de facto eliminated competition on the downstream market, thus leading to higher prices. This (potentially illegal) manipulation (see *Conseil de*

¹The 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 grocery retailing sector.

²See Commission Canivet (2005).

la Concurrence, Décision 05-D-70, December 1995) might have been a wide-spread practice. 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.

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 a comparable law, *the Groceries Order*, passed in 1987 in Ireland.³ Focusing on a specific category of products (processed and preserved fruits and vegetables), Collins, Burt, and Oustapassidis (2001) show that the *1987 Groceries Order* had a significant impact variable on gross retail margins, which, on average, increased from 15.8% in 1988 to 20.1% in 1993.

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 vertical relationships and pricing strategies. Their results are consistent with the theories claiming that the Loi Galland de facto led to minimum resale price maintenance.⁴ 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%.

Our empirical analysis of the Loi Galland is based on a much richer dataset than Collins, Burt, and Oustapassidis (2001). We use a unique dataset merging CPI micro data at store and product level with local competition data. Individual retail prices are available for about 200 homogeneous products surveyed monthly in about 2000 retail stores statistically representative of the French territory. Local market concentration is computed using an exhaustive database of French grocery stores. Various definitions of geographical relevant market are used.

Our empirical strategy consists in testing indirect predictions consistent with the theories of harm presented by the Canivet Commission.⁵ Our paper is related to

³The *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 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.*”

⁴Manufacturers use two-part tariffs (constant unit price combined with a fixed fee) and resale price maintenance (set final price).

⁵See also Allain and Chambolle (2005, 2007) for theoretical papers on this specific issue.

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 the Loi Galland), retail prices are positively correlated with concentration in local grocery markets.⁶ However, this correlation vanishes after 1997. Taking advantage of the panel dimension of our data, we also show that, across the 1994-1999 period spanning the Loi Galland, prices have increased more where they were initially lower.

The rest of the paper is organized as follows. We first briefly review some relevant theoretical literature on minimum resale price maintenance and derive some testable predictions (section 1). We then present our data (section 2). In section 3, we look at the correlation between retail prices and local markets concentration in 1994 and 1999. In section 4, we show that prices have increased more where they were initially lower. Section 5 concludes.

1 THEORETICAL PREDICTIONS

Every year (usually in late autumn), producers announce their “general terms of sale” (hereafter GTS). 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. According to the current laws, the GTS have to be non-discriminatory and are non-negotiable.⁷

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.⁸ 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

⁶See for instance studies on grocery markets by Barros, Brito, and de Lucena (2006) for Portugal, Asplund and Friberg (2002) for Sweden, Marion (1998) for the U.S.

⁷Although 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.

⁸Products are usually delivered to retail chains distribution platforms – and thus billed – several times a year.

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. As we have mentioned earlier, this is only true for “conditional rebates”, i.e. rebates that cannot be evaluated at the time of delivery (and invoicing): this is for instance the case for quantity rebates when demand is highly uncertain and the annual quantity cannot be reliably evaluated early in the year. On the contrary, when these rebates can be precisely anticipated, the GTS can be rewritten so that they are included on the invoice as being non-conditional. For instance, in the *Michelin II* case, it has been shown that the company overhauled its commercial policy after the enactment of the Loi Galland and that the bulk of the rebates were from this moment included on the invoice.⁹

However, it seems that in many cases, manufacturers and retailers have agreed on tariffs that included mainly “conditional rebates” even though some of these rebates could have easily been rewritten as unconditional.¹⁰ In some (extreme) cases, rebates that parties knew well in advance that they 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, 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.¹¹ Some toy manufacturers and retailers were recently fined €37 million on similar grounds.¹² If this practice is widespread, the combination of the Galland Act and non-discriminatory laws has the same effect as legalizing industry-wide price floors. However, the law does not generate this effect by itself, but only facilitates such anti-competitive practices.

1.1 MINIMUM RESALE PRICE MAINTENANCE

In the context of vertical relationships between a monopolist producer and competing retailers, it has been shown by O’Brien and Shaffer (1992), that industry-wide price

⁹See case 2002/405/EC, OJEC L143/1, May 2002.

¹⁰If there little uncertainty about demand, the annual quantity sold by a hypermarket is relatively well foreseen. The end-of-year rebates could thus easily be replaced by a reduction of the unit price.

¹¹See *Conseil de la Concurrence*, Decision 05-D-70, December 2005.

¹²See *Conseil de la Concurrence*, Decision 07-D-50, December 2007.

floors can be used to restore the ability of the vertical structure to maintain high prices.¹³ This not only harms the consumers but also reduces total economic welfare. 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 effect of their pricing strategy on the retail margins of those products. In each secret negotiation, the parties have thus incentives to free-ride on the other retailers' sales. This generates a rather competitive equilibrium, wholesale prices being equal to marginal costs. In this context, even without alternative manufacturers, intrabrand competition – i.e. competition between retailers selling the same brand – plays a role and lowers equilibrium prices below their monopoly level. Due to the vertical coordination problem, it is the downstream market structure which drives the retail price.

This “opportunism problem” that prevents the manufacturer from fully exerting its market power can be solved in two different ways. Eliminating retail margins, setting the bilaterally negotiated retail and wholesale prices equal to the monopoly prices, eliminates the incentives to free-ride. However, this requires imposing price ceilings rather price floors, since the objective is to eliminate any retail margin, which would otherwise be positive as soon as retail competition is imperfect. Price floors can nevertheless 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 eliminates the externalities generated by a secret wholesale price cut on the rival retailers. Suppose for instance that the manufacturer sets a price floor equal to the monopoly price and low wholesale prices, for instance equal to its marginal cost, (and uses franchise fees to share the monopoly profit with the retailers). This removes the incentives to agree on a lower wholesale price with a retailer, since it no longer affects the retail prices and thereby the sales of the different retailers. In this context, a publicly announced price floor, that is common to all retailers, acts as a credible commitment device for the producer.¹⁴

Allain and Chambolle (2005) use a very 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

¹³This issue is very similar to that first analyzed by Hart and Tirole (1990) in the context of quantity competition. 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 Crémer and Riordan (1987) used by O'Brien and Shaffer (1992).

¹⁴The opportunism problem faced by the monopolist producer is very similar to the inter-temporal pricing problem faced by a durable good monopolist. See Rey and Tirole (2007) for an overview of this literature.

Sales), before the manufacturer simultaneously negotiates with each retailer individualized rebates. If two-part rebates (a lump-sum as well as a discounted 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. However, this does not affect the determination of retail prices but only the profit sharing rules. In this model, the introduction of the Loi Galland imposes retail prices to be higher than the publicly announced wholesale price. It is thus perfectly equivalent to allowing the manufacturer to impose an industry-wide price floor thus eliminating any opportunism problem and restoring monopoly prices. The situation is slightly different when the negotiated rebates are restricted to be linear (i.e. lump-sum rebates are no longer permitted). In that case, setting a binding wholesale price (i.e. price floor) will only be optimal when the retailers' relative bargaining strength is high. Allain and Chambolle (2007) also allow for interbrand competition and obtain very similar results. Once again, the effect of a price floor common to all retailers is unambiguously positive when parties negotiate over two-part rebates (lump-sum payments and discounted unit price).

Biscourp, Boutin, and Vergé (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 Vergé (2008) introduce an explicit dynamic multilateral framework similar to that proposed by de Fontenay and Gans (2005). It uses 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. However, because 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 will 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

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

some (valuable) products are not available. An industry-wide price floor may then be welfare-enhancing, but this impact tends to be relatively limited since it occurs when brands are highly substitutable (therefore in situations where the missing products do not add much value). Because our data does not include any information on the sets of products that are available on the retailers' shelves, we are not able to estimate the full impact of the Loi Galland but only consider the 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 credible way for the producer to commit not to offer a better (secret) deal (i.e. a lower price floor) to one of the 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 prices. They show that the welfare effects of resale price maintenance (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 (but price floors suffice) 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 interbrand competition does not eliminate the opportunism problem and thus the anticompetitive effects of price floors. 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 (they would otherwise 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, neither are price floors. As a consequence, these firms are unlikely to be affected by the Loi Galland.

At last, price floors may also have pro-competitive effects. They can for instance be used to free-riding problems, in the provision of pre-sales services to consumers, when intrabrand competition is fierce. Price floors might then lead to higher prices but still

increase consumer surplus and total welfare.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.

1.2 PREDICTIONS

1.2.1 CROSS-SECTION

These theoretical models all suggest that the Loi Galland can well 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 scenarios. 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 some measure of concentration, 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 m²) compete with supermarkets (selling area between 400 and 2500 m²), convenience stores (selling area less than 400 m²) 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 when shopping at out-of-town larger stores. We thus expect competition to be fiercer between hypermarkets. As a result, the impact of the Loi Galland is expected to have been larger for these stores.

At last, hard-discount stores do not often sell branded products and thus have probably not faced changes in the terms offered by their suppliers. We therefore expect the price increase to have been very limited for this format since it is only a

¹⁶See Telser (1960). Rey and Vergé (2008) provide a recent survey of the effects (both pro- and anti-competitive) of resale price maintenance.

response (positive if we assume that prices are strategic complements) to the increase in prices of rival formats.¹⁷

1.2.2 LONG-TERM PRICE INCREASE

The theoretical arguments presented above suggest that intrabrand competition has, to a great extent, been eliminated after 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:¹⁸ 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 have been affected by the Loi Galland. On the contrary, markets where prices were initially lower have been affected by the new minimum price and prices thus went up in these markets. We thus expect that the inflationary impact has been higher on markets where prices were initially relatively low.

1.3 BAN ON BELOW-INVOICE PRICES OR PLANNING REGULATIONS?

A second law was passed at almost the same time 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 m². 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.

We have however several reasons to believe that the effects highlighted by our empirical analysis 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

¹⁷Given 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.

¹⁸For instance the central purchasing unit of the *Carrefour* group negotiates with each manufacturer for several “fascias”, which include *Carrefour* hypermarkets but also *Champion* supermarkets, *Ed* and *Dia* hard-discount stores, *Shopi*, *Huit à Huit* and *Proxi* convenience stores.

m², 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), but these effects existed well before 1997. 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 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) do not support contestable market theories.

2 DATA

We do not expect the Loi Galland to have had an immediate impact on retail prices, mainly because negotiations take place on a yearly basis. Using a long enough time period also ensures that we have sufficient price variation, thus reducing the impact of measurement errors. Finally, we want to avoid interference with large mergers taking place in 2000 in the French retailing sector as well as the switch to the euro.¹⁹ In order to use a symmetric time period around the enactment of the Loi Galland (1st January 1997), we focus on the period 01/1994 - 12/1999.

Our empirical analysis relies on two datasets, one on retail prices and one on the local structure of the grocery retailing industry.

2.1 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 covers the whole of the national territory, and stores and products surveyed are sampled to ensure representativeness.

¹⁹Although the euro only became the official currency in January 2002, the exchange rate was fixed at the end 1999. It has also been argued that retail prices have significantly – because of the switch to the euro increased before 2002.

The products for which prices are collected are coded according to a classification that is specific to the CPI. We retain only the products that are sufficiently homogeneous across stores and dates (e.g., sugar, milk), thus excluding 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.²⁰ Our dataset contains prices for 141 food items and 45 non-food items in 1994 (147 and 46 in 1999).

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 ($P_{p,s}^t$) of product p in store s 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 special offer, ...).

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

- **Hypermarkets** are large stores (selling area over 2500m²) generally located at the periphery of large cities.
- **Supermarkets** have selling area between 400 and 2500m² and usually located in city centers or at the periphery of smaller cities.
- **Convenience Stores** have a selling area smaller than 400m² and are located closer to the customers.
- **Hard-discounters** have a selling area 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.

Our data 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 were not able to satisfactorily distinguish between national brands and private labels. The data also include each store’s fascia. Unfortunately, this information is not recorded every month but only when the store

²⁰Almost 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-discount stores (mainly food items).

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 imperfect.

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

2.2 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 across the whole of the French territory.

The typology of stores is identical to the one used in CPI data. We have collected exhaustive data for hypermarkets, supermarkets, hard-discounters and magasins populaires 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 m²), fascia and location (administrative city code).²¹ 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.²² 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. Our CPI data thus represents a significant proportion of the total number of stores, all the more so for larger stores.

TABLE 1 : Number of stores

		Hyper	Super	Hard Discount	Convenience	<i>Magasin Pop.</i>
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

²¹Administrative 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).

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

2.3 CATCHMENT AREAS AND PROXIES FOR LOCAL COMPETITION

Downstream competition in the retail industry takes place locally. Delineating geographical relevant markets is an issue in itself. For the sake of simplicity, we construct catchment areas around each store present in our CPI dataset. Our approach is similar to that of Barros, Brito, and de Lucena (2006) and consistent with the 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, the catchment areas will be centered around cities rather than stores. All stores within one particular city code have therefore the same catchment area.

To construct these 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. Using our store dataset, we are able to list all stores within each of these cities. For any store belonging to our CPI sample, we are thus able to compute within any given radius (often 10 km) the number of stores by type, the total selling area by type, as well as the concentration in terms of selling area.²³

As in Barros, Brito, and de Lucena (2006), our proxies for local competition are measures of local concentration. Each index (hereafter market concentration, MC) is based on selling areas and built as a Herfindahl-Hirschman index based on selling areas rather than turnover or quantities: it is simply the sum of the squared market shares (expressed in terms of selling areas).

Table 2 shows the distribution across CPI stores of the number of potentially competing stores within various distances. Stores appear to be seldom in competition with stores in the same city. 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. It is thus preferable to select an intermediate distance as a reference. We thus decided to focus on two specifications.²⁴ First, we build our concentration index including stores

²³Alternative distances of 2.5, 5 and 20kms 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.

²⁴Our 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.

TABLE 2 : Distribution of the number of surrounding stores

		Hyper		Super		Hard Discount		<i>Magasin Pop.</i>	
		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

Distribution across CPI stores of the number of potentially competing stores. Sources: INSEE (IPC), LSA, computations by the authors.

within a 10km radius. As an alternative, we also build up catchment areas including all “*magasins populaires*” within 5kms, all supermarkets and hard discounters within 10kms, and all hypermarkets within 20kms. Our constructed catchment areas are therefore smaller than those used in other studies of local competition.²⁵ 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 time-invariant 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.

²⁵For instance Barros, Brito, and de Lucena (2006) use a distance of 30kms but focus on very large stores only.

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-sections marginally differ and so do the summary statistics. Sources: INSEE (IPC), LSA, computations by the authors.

3 CROSS-SECTION IMPACT 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 thus have to rely on cross-sectional estimations, which we run for 1994 and 1999. As local concentration is available yearly, we aggregate our monthly available price at year level.²⁶

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 $c(s)$):

$$\begin{aligned}
 \log(P_{p,s}^t) = & \text{constant}^t + \underbrace{\beta_{MC}^t \cdot MC_{c(s)}^t}_{\text{local concentration in } c(s)} + \underbrace{\gamma \cdot Y_{c(s)}}_{\text{other catchment area specifics}} \\
 & + \underbrace{\sum_{p'} \alpha_{p'}^t \cdot \mathbb{1}_{p=p'}}_{\text{year product dummies}} + \underbrace{\sum_{ty'} \alpha_{ty'}^t \cdot \mathbb{1}_{ty(s)=ty'}}_{\text{year type dummies}} + \underbrace{\sum_{f'} \alpha_{f'}^t \cdot \mathbb{1}_{f(s)=f'}}_{\text{year fascia dummies}} + \varepsilon_{p,s}^t
 \end{aligned}$$

Data for 1994 and 1999 are pooled so as to run a single regression allowing us 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.²⁷ We also control for the types of

²⁶We expect prices to be correlated within markets, given that we imperfectly control for catchment area specifics. We thus use robust variance estimates clustered at town level for inference.

²⁷Population and wealth come from the 1999 Census and do not vary over time.

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_{MC}^t might thus be biased. As usual, cross-section regressions provide valuable insights into variable relationships, but do not easily lend themselves to causal analysis. However, if the bias due to the endogeneity of local concentration is constant over time, the difference between the 1994 and 1999 coefficients can be interpreted as the causal impact of Loi Galland. This test will indicate if the correlation has decreased during the period, which provides a first way of checking if the theoretical 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 kms were included, as well as supermarkets and hard discounters up to 10 kms and all other stores up to 5 kms. 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 between the coefficients.

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.²⁸ Therefore, for stores located in monopoly markets, prices are 15% higher than for stores located in competitive markets, that is, 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 1994. These results are 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 in year 1994 should be interpreted with caution, the comparison with the same estimate for 1999 is striking. Three years after the enactment of Galland Act, there remains almost no correlation between prices and concentration, even though the structure of local markets has changed

²⁸By 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 areas but, since sales tends to be highly correlated with store size, it may also be interpreted as a proxy for concentrations of sales.

TABLE 4 : Local Concentration and Prices in 1994 and 1999

	10 km		5/10/20 km	
	1994	1999	1994	1999
Supermarket	0.056*** (0.004)	0.027*** (0.004)	0.056*** (0.004)	0.027*** (0.004)
Hard discount	-0.363*** (0.019)	-0.435*** (0.012)	-0.362*** (0.019)	-0.435*** (0.012)
Convenience	0.223*** (0.007)	0.213*** (0.010)	0.223*** (0.007)	0.213*** (0.010)
Magasin Populaire	0.068*** (0.007)	0.068*** (0.006)	0.068*** (0.007)	0.068*** (0.006)
Hypermarket	ref.	ref.	ref.	ref.
Market population (log)	0.026*** (0.003)	0.015*** (0.003)	0.026*** (0.003)	0.015*** (0.002)
Market income (log)	0.027** (0.013)	0.018 (0.016)	0.027** (0.014)	0.019* (0.016)
Market concentration	0.153*** (0.032)	0.055* (0.028)	0.172*** (0.035)	0.063* (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 10kms in the “10 km” treatment. It includes hypermarkets up to 20kms, supermarkets and hard discounters up to 10 kms and all other stores up to 5 kms in the “5/10/20” treatment.

only marginally 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, 1 for 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 changes, the average differences in price between hypermarkets, convenience stores and *magasins populaires* has been stable over the period. However, the difference between supermarkets and hypermarkets has decreased between 1994 and 1999. In contrast, the difference between hard-discounters and hypermarkets increased during the period. Even though this assumption is to be confirmed by a dedicated analysis (see section 4), both facts are consistent with our prediction 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.²⁹ 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. Nevertheless, the this correlation reduction during such a short period of time is consistent with a uniformization of prices due to Galland Act.³⁰

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 identification issues and makes the interpretation of both sets of coefficients difficult. However, controlling for fascia might be important as regards to the consistency of the other coefficients, since fascia might also be an important determinant of retail prices. Results are given in table 5. It shows that our results (on the difference between the 1994 and 1999 coefficients) are very robust to this specification.

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*** (0.003)	0.009*** (0.002)	0.018*** (0.003)	0.009*** (0.002)
Market income (log)	0.023*** (0.010)	0.025** (0.013)	0.022** (0.011)	0.026** (0.013)
Market concentration	0.103*** (0.028)	0.018 (0.024)	0.138*** (0.032)	0.022 (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 10kms in the “10 km” treatment. It includes hypermarkets up to 20kms, supermarkets and hard discounters up to 10 kms and all other stores up to 5 kms in the “5/10/20” treatment.

²⁹The size of the catchment areas are fixed, and the logarithm of total population is hence a good proxy for population density.

³⁰These catchment area characteristics are time invariant and are 1999 values. This can only reinforce our results since we expect the 1999 coefficient to be more precisely estimated than the 1994 coefficient.

Table 6 shows the results for prices in hypermarkets only.³¹ In 1994, the coefficient for local concentration is larger in both sets of estimations than when we include all types of stores (table 5). The sample size has also been reduced and estimates are thus less precise. For the two definitions of local markets, the difference is larger for hypermarkets than for the whole population of stores, 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, but with a larger magnitude since they were initially more receptive to local competition.

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*** (0.005)	0.008* (0.004)	0.021*** (0.005)	0.005 (0.004)
Market income (log)	0.005 (0.021)	0.016 (0.020)	0.004 (0.020)	0.017 (0.020)
Market concentration	0.160*** (0.070)	0.035 (0.053)	0.222*** (0.068)	-0.006 (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 10kms in the “10 km” treatment. It includes hypermarkets up to 20kms, supermarkets and hard discounters up to 10 kms and all other stores up to 5 kms in the “5/10/20” treatment.

4 LONG-TERM PRICE INCREASE

As mentioned earlier, we expect retail prices to have increased more in stores where they were initially lower. In the cross-sectional approach, we have been running regressions of prices on their determinants, such as market concentration or store types, and the coefficient of correlation between prices and market concentration before and after the enactment of the Loi Galland. In order to obtain a more direct test of our

³¹Several 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.

prediction, we now want to run a regression of price inflation (i.e. the change in price between 1994 and 1999) on initial (1994) price, controlling for various determinants of price increase over the period, such as the change in market concentration.

Implementing this approach requires great care, in order to avoid any bias that might be due to “regression to the mean”. To illustrate this potential problem, 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 easy to see that $cov(P_t - P_{t-1}, P_{t-1}) = -var(\varepsilon_{t-1})$. 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 be an issue. 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 an unusually high demand if a music festival happens to take place in the neighborhood. Aggregating data at store level does not solve the problem 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 the 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 before 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 1994 and 1999 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.³²

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

To estimate store effects, we build an indicator of the relative price for each store in a given year. This means that, for each store, we now have only one observation for each year, thereby solving the problem 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 :³³

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

This specification above captures seasonal changes potentially affecting the price of each product, as well as the type of store.³⁴

We are mainly interested in recovering y_s , the “store effects”, for each year. By definition $\mathbb{E}\{\eta_{p,s,m}|y_s\} = 0$. In order to compute \hat{y}_s , we must first obtain consistent estimates of the $\hat{\alpha}$ parameters.

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

³²We actually use dummies for each *département*, a smaller administrative unit than the region (94 *départements* when we exclude Corsica and overseas territories, as opposed to 21 regions).

³³For expositional simplicity, we omit the superscript for time, but all variables implicitly depend on the year t .

³⁴Different specifications are possible, for instance omitting type dummies. Our results are robust to such changes.

that there is no selection bias in our sample. We therefore use pooled OLS under the assumption of strict exogeneity of $y_s + \eta_{p,s,m}$.³⁵

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

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

Our variable of interest, y_s , is thus measured with error ζ_s , which might generate spurious correlation between the price differences and the initial price. 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.³⁶

4.2 LONG-TERM DIFFERENTIAL PRICE INCREASE

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 .

In order to test our prediction on long term price increase, we run the following regression over the period 1994-1999:

$$\begin{aligned} \Delta \widehat{y}_s = & \text{constant} + \alpha \widehat{y}_s + \underbrace{\beta_{MC} \cdot \Delta MC_{c(s)}}_{\text{variation of local concentration in } c(s)} + \underbrace{\beta_{HD} \cdot \Delta HD_{c(s)}}_{\text{variation of the share of Hard Discount in } c(s)} \\ & + \underbrace{\sum_{dep'} \alpha_{dep'} \cdot \mathbb{1}_{dep(s)=dep'}}_{\substack{\text{d\u00e9partement} \\ \text{dummies}}} + \underbrace{\sum_{ty'} \alpha_{ty'} \cdot \mathbb{1}_{ty(s)=ty'}}_{\substack{\text{type} \\ \text{dummies}}} + \varepsilon_s \end{aligned}$$

where Δ denotes differences taken between 1994 and 1999.

In order to deal with endogeneity issues (regression to the mean), we first use the mean \widehat{y}_s^t of store effects computed over the three years, 1994, 1995 and 1996, to capture initial price. However, our simulations tend to show that the bias may still be large when random shocks exhibit little persistence.³⁷ We therefore turn to instrumental variables, using initial local market concentration MC_s^t as well as local population size and income, to instrument the initial store price \widehat{y}_s^t . The validity of

³⁵The 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.

³⁶Besides, averaging the initial price over three dates in itself also mitigates endogeneity problems.

³⁷If random shocks follow the process $\varepsilon_s^{t+1} = \rho \varepsilon_s^t + \xi_s^{t+1}$, with $0 \leq \rho < 1$, we 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 . Using the average on the first three years instead, we have $cov(y_s^{t+5} - y_s^t, \widehat{y}_s^t) = -\frac{1}{3}(1 - \rho^5 + (\rho + \rho^2)(1 - \rho^2))var(\varepsilon_s)$. The bias is reduced by a third in the worst-case scenario where $\rho = 0$.

these instruments relies on the assumption that these variables are strongly correlated with initial price \widehat{y}_s^t , but uncorrelated with the equation residual. Our cross-section regressions have confirmed the strong correlation.

Local population and income are “structural” characteristics and can therefore be considered as unaffected by short term shocks on prices. Similarly, the main argument in favor of the validity of initial market concentration as an instrument for initial price, is that retailing groups 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 the market structure should react to short-term positive demand shocks affecting the local markets. The time span for store closure is typically smaller than for openings. However, it generates heavy opportunity 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.

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 initially run an OLS regression using a robust variance matrix clustered by towns for inference. We then run an OLS regression of \widehat{y}_s on all exogenous variables, as well as our instruments (market concentration in 1994, log of population and household income in 1999). Recovering the residual of this first stage regression, we then run the initial OLS regression, augmented by the first stage residual. This provides a convenient endogeneity test, asymptotically equivalent to the Hausman test. This also provides point estimates of the two-stage least-squares regression and allows us to compare the magnitudes of the OLS and TSLS estimates.³⁸

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 instrumental variables. Besides, as the coefficient of the first step residual in the augmented regression is not significant, we cannot reject the exogeneity of initial price under

³⁸If exogeneity is rejected, it is however important to perform the TSLS regression in order to get appropriate standard errors.

TABLE 7 : Long term price variations between 1994 and 1999

	OLS	Aug. Reg.
Initial Average Individual Effects	-0.275*** (0.043)	-0.547*** (0.209)
First Stage Residual	-	0,276 (0.209)
Δ Market Concentration	-0.139 (0.094)	-0.152 (0.101)
Δ Share of Hard Discounts	-0.131 (0.100)	-0.167 (0.107)
Regional Dummies	Y	Y
Type Dummies	Y	Y
R^2	0.11	0.11
Number of obs.	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 : dummies by *département* and dummies by type. Sources : INSEE (IPC,CENSUS), LSA. In the augmented regression, initial average individual effects are instrumented by “5/10/20” market concentration indices as well as logs of population and income.

the assumption that our instruments are valid. Note that these tests are asymptotically valid. Given our number of observations, one should interpret the result of this test of exogeneity cautiously. Nonetheless, it remains that, even using instrumental variables, the coefficient of initial prices is highly significant.

As a robustness check, we run the same regressions over the period 1994-1996, using the average of the first two years as the initial price. Prior to the Loi Galland, we do not expect prices to have significantly increased more where they were initially lower. The OLS regression might still be affected by regression to the mean. However, we expect our corrected estimator to allow us to reject convergence of prices over this period. Results are presented in table 9 in appendix. Using OLS, the coefficient on initial price is indeed negative and significant, although much closer to zero. However, when using instrumental variables, we can reject convergence of prices, thereby confirming the validity of our approach.

As stated before, we expect hypermarkets to be more substitutable than smaller stores, and hence the effect of the Loi Galland to be stronger for this type of stores. We thus run the same regression as above for hypermarkets only. Results are presented in table 8. It confirms that the effect is slightly more important, even though less precisely estimated, especially in the case of instrumental variables, since the number of observations is significantly reduced.

TABLE 8 : Long term price variations between 1994 and 1999
Hypermarkets only

	OLS	Aug. Reg.
Initial Average Individual Effects	-0.391*** (0.075)	-0.618* (0.353)
First Stage Residual	-	0,232 (0.349)
Δ Market Concentration	-0.350** (0.161)	-0.370** (0.164)
Δ Share of Hard Discounts	-0.224* (0.117)	-0.214* (0.113)
Regional Dummies	Y	Y
Type Dummies	Y	Y
R^2	0.30	0.30
Number of obs.	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 : dummies by *département* and dummies by type. Sources : INSEE (IPC,CENSUS), LSA. In the augmented regression, initial average individual effects are instrumented by "5/10/20" market concentration indices as well as logs of population and income.

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 for 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 part of the price disadvantage faced by small village shops. However, anecdotal evidence suggests that this effect was probably rather limited (see Commission Canivet 2005). For instance, 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).³⁹ 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.

³⁹Less than 15% of small grocery retailers are really 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:

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

The first part of the error term is the consequence of the estimation of β . The variance of the estimator $\hat{\beta}$ being inversely proportional to the sample's size, β is very precisely estimated given the size of our dataset. We thus neglect this 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 , \hat{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 $y_s^{99} - y_s^{94}$ on \bar{y}_s^{94-96} , we do the regression of the estimated counterparts.⁴⁰ Assuming that ζ_s are uncorrelated and homoscedastic, we have:

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

and

$$Var \left[\bar{\hat{y}}_s^{94-96} \right] = Var \left[\bar{y}_s^{94-96} \right] + \frac{1}{3} Var \left[\zeta_s \right]$$

Therefore:

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

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 the 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 $\hat{\xi}_{p,s,m}$ is the residual of the within store estimator, an estimator of $Var(\eta)$ is:

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

⁴⁰For 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.

Besides:

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

This in turn implies that:

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

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

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

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 negative correlation). Instruments are generally a good 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, as well as for 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.

B LONG TERM PRICE INCREASE: ROBUSTNESS CHECK

TABLE 9 : Long term price variations between 1994 and 1996

	OLS	Aug. Reg.
Initial Average Individual Effects	-0.183*** (0.026)	-0.169 (0.176)
First Stage Residual	-	-0,014 (0.176)
Δ Market Concentration	0.114 (0.074)	0.111 (0.079)
Δ Share of Hard Discounts	0.049 (0.054)	0.051 (0.066)
Regional Dummies	Y	Y
Type Dummies	Y	Y
R^2	0.12	0.12
Number of obs.	1398	1398

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 : dummies by *département* and dummies by type. Sources : INSEE (IPC,CENSUS), LSA. In the augmented regression, initial average individual effects are instrumented by “5/10/20” market concentration indices as well as logs of population and income.