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Heterogeneity in the tax pass-through to spirit retail prices: Evidence from Belgium☆

Jean Hindriks *, Valerio Serse

CORE, Université catholique de Louvain, Belgium

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ABSTRACT

On 1st November 2015, the Belgian government increased the excise tax on alcoholic beverages. For spirits with 40% of alcohol and bottle size of 70 cl, this tax change is equivalent to an amount of 2,43 € per bottle of spirits. This paper studies the impact of this tax reform at the store level on the (posted) retail price of six major brands of spirits, using a difference-in-differences method. The estimation is based on *a balanced panel of scanner data* from a major supermarket chain (with a 33% market share) and uses the retail prices of the same brands sold in France by the same supermarket chain as a control group. Having information on each store location, we show spatial variations in the tax pass-through for *homogeneous* products. We find that these variations are strongly related to the intensity of local competition and to a lesser extent to the proximity to the borders (mainly with Luxembourg which is the low-price country). We find that the tax was quickly passed through during the first month of tax implementation and that it was mostly over-shifted. However, we also find that both the border and the competition effects are not instantaneous, but arise several months after the tax reform. These findings have important implications for alcohol control policies as they highlight that the incidence of alcohol taxation can vary greatly across space and affect differently households depending on where they live.

1. Introduction

On 1st November 2015, the Belgian government increased the excise tax on alcoholic beverages. This tax reform was not in reaction to market conditions (providing an exogenous change in tax rates). It was part of the general governmental tax shift plan aiming to shift the tax burden from labor to consumption (with higher taxes on electricity, gasoil, cigarettes, alcoholic beverages and sodas). The tax increase was different across alcohol types. For instance, the taxes on beer and wine have increased by 8.5% and 31%, respectively. The strongest tax increase was for the category of spirits, which is also the category that was taxed most heavily before the tax reform. From 2.127,68 €/hl per % alcohol to 2.992,79 €/hl per % alcohol.[1] That is, an increase of 41% in excise tax. Considering a standard bottle of 70 cl with 40 °C, this tax change amounts to an extra tax of 2,43 € per bottle.[2] This tax reform was heavily criticized in the media for inducing sales loss and its failure to bring extra revenues. In fact, the total revenue from excise taxes on alcohol was 318 € million in 2015, 323 € million in 2016 and 319 € million in 2017.[3] One survey of 425 local retailers organized during the spring of 2016 by the SNI (Syndicat neutre des indépendants - Trade union for independent

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* Corresponding author.
E-mail addresses: jean.hindriks@uclouvain.be (J. Hindriks), valerio.serse@uclouvain.be (V. Serse).

[1] In comparison with neighboring countries, excise taxes (and VAT tax levied on the price inclusive of the excise duty) are as follows: Belgium 2.992 €/hl (VAT 21%), France 1.741 €/hl (VAT 20%), the Netherlands 1.686 €/hl (VAT 21%), Germany 1.303 €/hl (VAT 19%), and Luxembourg 1.041 €/hl (VAT 17%) (European Commission 2018, excise duty on alcohol beverages).

[2] The magnitude of the tax change on beer and wine is much lower than on alcohol. The tax change for a standard bottle of wine (75 cl) is 0,13 €, and for a can of beer (33 cl) it is 0,01 €. Interestingly, such differentiation of the tax changes is consistent with Griffith et al. (2019) who estimates the welfare gains from varying tax rates across different types of alcohol depending on the concentration of alcohol externalities among heavy drinkers. It is interesting to concentrate on the spirit market because the planner can target the most socially harmful drinking by taxing more heavily the ethanol in products that are disproportionally consumed by problem drinkers (see Griffith et al., 2019).

[3] Source: SPF Finances Belgium. Available at <https://finances.belgium.be/fr/statistiques_et_analyses/rapport-annuel/chiffres/2018/budget-recettes/recettes-ag-douanes-et-1>.

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workers) suggests that sales have declined by 14% in volume and shop thefts have increased by 11% in a single year. The federation of spirit and wine (Vinum & Spiritus) blames cross-border shopping for the loss of sales, since the tax reform has considerably increased the relative price of Belgian spirits with respect to all the neighboring countries. For example, the price of a bottle of Gin Gordon was after the reform 15 euros in Belgium against 9 euros in Luxembourg. Given that 50% of the Belgian households live within a distance of 50 km from the border we could indeed expect massive cross-border shopping.

The magnitude of this tax increase provides a unique opportunity to estimate the tax pass-through of spirits in the Belgian market and to focus on spatial heterogeneity in tax incidence across geographical areas. Understanding the incidence of alcohol taxation is fundamental to assess the effectiveness of this policy to improve public health and/or generate fiscal revenues. This is also important to identify how the tax burden and health benefits are distributed in the population. Although alcohol is typically taxed homogeneously within a given jurisdiction, the extent to which a tax is passed through to alcohol retail prices can be substantially heterogeneous across geographical areas. Theoretically, the tax pass-through is a function of both the price elasticity of demand and the structure of the supply side of the market. Spatial differences in these two factors can therefore explain the heterogeneity in tax incidence within a tax jurisdiction. The proximity to a lower taxed state can be another important determinant of tax shifting due to tax avoidance by means of cross-border shopping.

This paper contributes to the empirical literature on tax pass-through by analyzing the impact of the recent alcohol tax reform in Belgium on spirit retail prices using *a balanced panel of supermarket scanner data* from a major group of retailers. Unlike conventional scanner average price data used in the literature (e.g. Nielsen measured prices), we use more detailed data on posted prices from a large retail chain. Posted prices are the same as actual transaction prices in general. However, they may differ relative to transaction prices because they are not conditional on purchase and thus less sensitive to local and cyclical shocks (Coibion et al., 2015). Posted prices are not dependent neither on measurement errors due to loyalty cards (Einav et al., 2010). That does not mean that posted prices are always superior to actual transaction prices. Indeed posted prices could in principle not be updated, and this would not be realized if there are no transactions. In our case, posted prices are automatically updated daily for each item. Although posted price data are only observed for all the retailers of the same supermarket chain, this group possesses a significant market share (about one third) and is publicly committed to match prices of local competitors (price matching strategy). Hence, their price can be considered as representative of the general price evolution in the market.[4] Furthermore, as this group is also present in other countries, price data for the exact same products in France (not submitted to the tax change) can be used as a control group. This allows measuring the tax pass-through to spirit retail prices by means of a "*difference-in-differences*" estimator.

In our analysis, posted prices include any taxes. This is different from several studies in the U.S. where posted prices do not include some taxes. The question that arises is about the salience of the tax change. Tax salience matters to assess the tax pass-through since we may expect the retailer to shift more of the tax when consumers do not know whether the tax change has occurred (the tax is less salient). Chetty et al. (2009) provide experimental evidence that consumers are less sensitive to (non-posted) tax changes than they are to changes in the posted price. Interestingly, in our study the tax change was explicitly announced and the posted prices include the tax. Thus, we may expect the tax to be more salient. Nevertheless, our results suggest significant tax over-shifting.

The rich nature of the dataset allows testing for and explaining spatial heterogeneity in tax pass-through over Belgium. Having information on both proximity to the border and the number of competitors for each store, this work provides new evidence on the effects of cross-border shopping and the intensity of competition on the pass-through of alcohol excise taxes. Yet, we cannot make causality claim here because we do not have exogenous variations in the intensity of local competition at the retail level during the tax reform.[5] As price data are collected over several months, this study also checks for the evolution of the tax pass-through over each month after the tax hike and tests whether the observed heterogeneity in price hikes is permanent or just temporary.

The spatial dispersion in posted prices and in the tax pass-through contrasts with the recent empirical study on uniform pricing in U.S. retail chains based on the Nielsen price measure (see Della Vigna and Gentzkow, 2017). The difference may result from the uniform markup rule regulation used in the U.S. (Miravete et al., 2017). These findings highlight that the incidence of alcohol taxation can vary greatly across geographical areas, even within a small country as Belgium. We find that the stores' heterogeneity in tax shifting is strongly related to local differences in the intensity of competition at the retail level. Surprisingly enough, we do not find that differences in tax shifting are significantly related to the proximity to the border in general. Although the tax reform has considerably increased the relative price of Belgian spirits with respect to all the neighboring countries, we find a lower tax shifting only in stores bordering Luxembourg. Which is the neighboring country with lowest spirit prices before the alcohol tax reform. In line with the previous literature, we find that the tax was quickly shifted to spirit retail prices. With a significant tax over-shifting already during the first month of tax hike. Interestingly, we find that both the border and the competition effects are "back-loaded" in the sense that they show up with some lag (few months after the reform). This suggests that it took some time before stores adjusted their prices to both the foreign and domestic competitors.

The rest of the paper is organized as follows. In Section 2, we provide a review of the relevant empirical literature focusing on tax pass-through and identify our contribution. In Section 3, we provide a brief account of the theory on the tax pass-through and how it relates to market structure and the shape of the demand. In Sections 4 and 5, we describe our dataset and perform the empirical analysis. Section 6 provides some summary statistics about the demand response (change in the quantity of bottles sold) to the tax hike. Section 7 concludes.

2. Contribution to the literature

Various empirical studies estimate the tax pass-through to the retail price of sin goods. In particular, recent works focused on tax pass-through in the market of sodas (Cawley and Frisvold, 2017; Berardi et al., 2016; Campos-Vázquez & Medina-Cortina, 2016; Grogger, 2017), cigarettes (Harding et al., 2012; DeCicca et al., 2013; Xu et al., 2014) and alcoholic beverages (Kenkel, 2005; Carbonnier, 2013; Ally et al., 2014; Conlon and Rao, 2016; Shrestha and Markowitz, 2016). These studies mostly consist of reduced-form analysis that use price data collected from different sources during a period of tax policy change. The common strategy is to regress the price variable on a tax indicator plus a set of controls in order to isolate the causal impact of the tax on prices.[6]

Part of this literature, however, identifies tax pass-through by means of a "*difference*" estimator (see DeCicca et al., 2013; Xu et al., 2014;

[4] The local competition analysis would have been more interesting if we could observe the price changes in other stores, to see if the price changes are coordinated. In the same vein, the cross border shopping would also have benefited from the possibility to observe the price changes in stores just next but on the other side of the border.

[5] This would have allowed us to purge the effect of competition from unobserved differences across stores that are specific to their location.

[6] Sources of price data can include, for instance, online price comparison services (Ally et al., 2014; Berardi et al., 2016), self-reported purchases (DeCicca et al., 2013; Xu et al., 2014), scanner data (Harding et al., 2012; Conlon and Rao, 2016), governmental agencies (Campos-Vázquez & Medina-Cortina, 2016; Grogger, 2017) and telephone interviews (Kenkel, 2005).

Kenkel, 2005; Carbonnier, 2013; Ally et al., 2014; Conlon and Rao, 2016). That is, by measuring pre- versus post-tax difference in retail prices. Some of the most recent papers overcome this limitation by introducing control groups that account for the counterfactual price evolution in absence of tax policy change. This allows estimating the tax pass-through by means of a typical "*difference-in-differences*" estimator. Nevertheless, type and quality of control groups for prices tend to vary over different studies. For instance, Berardi et al. (2016), which estimates the impact of the "soda tax" on prices in France, use the price of untaxed beverages as a control group for the taxed products. The same approach is adopted by Campos-Vázquez & Medina-Cortina (2016) and Grogger (2017), which both study the pass-through of the "soda tax" implemented in Mexico in January 2014. Conversely, Harding et al. (2012), who analyze the pass-through of cigarette excise taxes in the United States, use as a control group the same cigarette products sold in those states that did not change their cigarette excise taxes. Similarly, Cawley and Frisvold (2017) use as a control group the price of sugar-sweetened-beverages (SSBs) in the city of San Francisco to estimate the pass-through of the tax on SSBs implemented in the neighboring city of Berkley, California.

This literature generally finds that tax incidence is quite heterogeneous across products and that all three patterns of under-, over- and full shifting are likely to occur after the implementation of a tax on sin goods. In the context of alcohol taxation, existing evidence generally suggests tax over-shifting with a large heterogeneity of tax pass-through across products. Kenkel (2005) find that the pass-through of the alcohol tax hike occurred in Alaska in 2002 ranged between 167% and 213% for 6 major brands of distilled spirit. Ally et al. (2014) estimate the pass-through of excise duties and VAT in UK during the period 2008–2011. They find evidence of tax over-shifting for spirits on average, but they also find a significant tax under-shifting for the cheapest brands. This evidence highlights the complexity in designing sin taxes aimed at improving public health. As price hikes tend to differ even within the same category of taxed products, there should be a rising concern about both the substitution effect towards other taxed goods and the distribution of tax incidence across different types of consumers. Our paper extends this literature by providing evidence of a further dimension of heterogeneity in alcohol tax shifting. That is, the spatial heterogeneity in the tax pass-through for *homogeneous* products. Although such heterogeneity in tax shifting can be theoretically explained by differences in price elasticities and market structure across geographical areas (Hindriks and Myles, 2013), little attention has been given to this phenomenon in the empirical literature. In this paper, we focus on two possible determinants of spatial heterogeneity in tax shifting: the variation in the scope for cross-border shopping and the variation in the local intensity of competition at the retail level.

Prior empirical papers on cross-border shopping have studied the demand side. That is how price differences create incentive to cross the border line (see, for instance, Gopinath et al., 2011; Asplund et al., 2007; Manuszak and Moul, 2009; Chandra et al., 2014 and Chiou and Muehlegger, 2008). This empirical work has shown that consumers do respond to price differences by engaging in cross-border shopping. What is less studied is how retailers in turn respond to that cross-border shopping. Harding et al. (2012) and Cawley and Frisvold (2017), use price data at the store level, respectively for cigarettes and sodas, to find that part of tax pass-through heterogeneity across stores can be explained by their proximity to states with lower tax rates on cigarettes and sodas. In particular, they find lower tax pass-through in stores next to the border, thus suggesting that the scope for cross-border shopping drives down the extent to which stores can rise prices after a tax hike. Doyle and Samphantharak (2008) study the effects of cross-border competition on the gasoline tax shifting to retail prices. They use data of daily prices at the gas station level to estimate the impact of a temporary suspension, and a subsequent reinstatement, of the gasoline sales tax in Illinois and Indiana on the retail price of gasoline, which followed a price spike in the spring of 2000. They adopt a difference-in-differences approach by using the gasoline retail price of neighboring states as control group. Their findings on the border effect are mixed but overall they suggest a smaller tax shift for gas stations close to the border, especially for the reinstatements (tax increase), with some evidence of tax spillover across state borders.

Like these studies, the contribution of our paper is on the cross-border shopping effect on prices. We study how the distance to the border affects the extent of the tax shifting to spirit retail prices. Understanding the tax shifting for alcoholic beverages at the border provides precious insights into how tax avoidance can reduce the effectiveness of the sin tax in curbing the consumption of alcohol or generating tax revenues. Most papers analyzing the effectiveness of alcohol taxes to curb demand get results on volume sales that are only valid conditional on the tax incidence on prices (Wagenaar et al., 2008). With cross-border shopping, affected stores might be less willing to pass on the tax in order to avoid losing consumers to nearby (untaxed) stores. Belgium is a nice candidate for this analysis because it is a small country with high population density and a sizeable population at a short distance to the borders with four neighboring countries using the same currency (Euros). Unlike the previous literature, we also study the timing of the border effect on the tax pass-through. We show that this has to be carefully taken into account in empirical works as it may take time for stores to internalize the cross-border shopping in their price adjustment to the tax reform.

It is important to mention that in this paper, we do not estimate the cross-border spillover effect of the tax change in the neighboring stores on the other side of the border. Bajo-Buenestado and Borrella-Mas (2018) provide interesting estimates (using differences-in-differences) of this "*cross-border pass-through*" from the fuel tax reform in Portugal on the Spanish fuel prices of stations that are close to the Portuguese–Spanish border. Their control group are the Spanish gas stations that are far from the border.[7] In our paper, we only consider the "*domestic pass-through*" since we do not observe price changes in foreign stores near the border (our control group are French stores that are far from the border).

In this paper, we also study how variation in competition at the store level may relate to the spatial variations in tax shifting. Economic theory indicates that the intensity of competition can extensively affect the extent of tax pass-through to retail prices. Yet, this competition effect is not very much studied in the empirical literature. Doyle and Samphantharak (2008) estimate how the tax shifting to gasoline retail prices varies across local markets with different levels of brand concentration. The idea is that the tax change should be reflected upstream in the wholesale price depending on the market power in the wholesale market. They measure the share of gas stations for each (wholesale) brand in a local market and compute a Herfindahl–Hirschman Index of brand concentration. They find some evidence that tax shifting varies with brand concentration at the ZIP code level, with the price hike (after the tax reinstatement) being 2 percentage point lower in the least concentrated markets. Stolper (2016) finds that tax pass-through differences range from 70% to 120% at the specific station level in the Spanish fuel market. Greater market power measured by brand concentration is strongly associated with higher pass-through, even after conditioning on detailed demand-side characteristics. Campos-Vázquez & Medina-Cortina (2016), using price data at the store level, show that the competitive barriers faced by each store generate significant differences in the shifting of the "soda tax" in Mexico. They use as control group the water price that is not subject to the tax increase, but whose price is highly correlated with prices of the taxed product, the soft drinks (treated group). They compute the number of competing retailers within a distance of 8 km from each store and find that the tax pass-through decreases with the number of competitors. Etilé et al. (2018) find similar result for the 2012 soda tax in France. We extend

[7] Doyle and Samphantharak (2008) do a similar analysis for the US and provide evidence of cross-border pass-through.

this literature by providing evidence of the competition effect on the tax shifting to spirit prices using as a control group the same product sold by the same chain in a different country not subject to the tax hike. Although Belgium is a relatively small country, we find a very large store heterogeneity in tax pass-through that can be related to differences in competition intensity at the retail level. We also provide novel evidence about the timing of this effect and show that the competition effect is back-loaded and arises with some lag.

Lastly, evidence on the tax pass-through timing suggests that prices tend to react quickly to the introduction of excise taxes. The "soda tax" in Mexico in January 2014 was already fully shifted into soda prices during the first month of implementation (Campos-Vázquez and Medina-Cortina, 2016; Grogger, 2017). While the "soda tax" in France in January 2012 was gradually passed through to retail prices and fully shifted after six months (Berardi et al., 2016). Carbonnier (2013) reports that the increase in excise taxes on alcohol implemented in France in January 1997 was immediately fully shifted to the price of both beer and aperitif during the first month of tax hike. Conlon and Rao (2016) find that excise taxes on distilled spirits in the U.S are shifted within a month and are often over-shifted. Our paper confirms those findings of a quick tax shifting with frequent over-shifting.

3. Theoretical framework

The basic theory on tax incidence in industrial organization is about estimating the changes in prices and profits resulting from a tax (Fullerton and Metcalf, 2002). Let us denote the excise tax t and the producer price $p(t)$, then the consumer price is $q(t) = p(t) + t$. In our context of supermarket transactions, the producer should be understood as the retailer. Under perfect competition, the tax incidence is very simple. The tax shifts the supply curve vertically upward by the amount of the tax. The incidence of the tax on prices is $q'(t) = p'(t) + 1$ where $q'(t)$ and $p'(t)$ are the tax derivative of the consumer and producer prices. The extent to which consumer price rises is determined by the elasticities of the supply and demand curves. Formally, the pass-through rate is given by

$$dq/dt = \frac{1}{1 + \left(\frac{\varepsilon_D}{\varepsilon_S}\right)}$$

where ε_D is the elasticity of demand (in absolute value) and ε_S is the elasticity of supply (Weyl and Fabinger, 2013). If the demand is inelastic, $q'(t) = 1$ and thus $p'(t) = 0$, that is consumer price will rise by the exact amount of the tax and producer price is unchanged. We have perfect tax shifting. In all other cases the consumer price increases to a lesser extent than the amount of the tax $q'(t) < 1$, and the producer price decreases $p'(t) < 0$. The tax is shifted in part to the consumer and in part to the producer as a function of the elasticities of supply and demand. In this general case we have tax under-shifting $q'(t) < 1$. Hence, with perfect competition, the full amount of the tax may be shifted to consumers but never more, and this is only possible if the demand is perfectly inelastic.

Under imperfect competition, tax incidence is different and tax over-shifting becomes possible. This possibility depends on the shape of the demand function. To illustrate that point we need to trace the effect of the tax on the profit-maximization decisions of the imperfectly competitive firms (here retailers). To see that easily, we follow Hindriks and Myles (2013). Consider a monopoly situation with constant marginal cost. Fig. 1a depicts the profit maximization of a monopoly choosing not shifting all the tax on the consumer. Indeed, the tax is shown to move the intersection between marginal cost and marginal revenue (i.e. the profit-maximization condition) from a to b with a reduction of output from $y°$ to y^t and consumer price rises from p to q. In this case, price rises by less than the tax imposed ($q - p < t$).

In contrast, Fig. 1b depicts the same monopoly facing a demand function with a different shape. The demand has a convex shape: it becomes increasingly flat as quantity increases (whereas, in Fig. 1a the demand has a concave shape: it becomes increasingly steep as quantity increases). In this case, the tax induces a price increase from p to q that is greater than the amount of the tax ($q - p > t$), so we have tax over-shifting.

To extent this result to the case of imperfect competition (Cournot-oligopoly), we can consider an isoelastic demand function $X = q^{\varepsilon}$ where $\varepsilon < 0$ is the price elasticity of demand. With a constant price elasticity, the mark up is constant $\mu^0(n) = \dfrac{n}{n - (\frac{1}{|\varepsilon|})}$ where n is the number of (equal-size) competing firms. When firms have different market shares ($s_i > 0$) we replace the number n by n* (with $n^* < n$) the equal-size equivalent Herfindahl index (with $H(n) = \sum_{i=1}^{n} {s_i}^2 = \frac{1}{n*}$). Since $|\varepsilon| > 0$, we have $\mu^0 > 1$. The equilibrium price is obtained by applying the mark up to the marginal cost-plus tax, to get $q(t) = \mu^0(n)[c + t]$. The tax incidence on price is then $q'(t) = \mu^0 > 1$. Hence, there is always tax over-shifting with isoelastic demand and imperfect competition. This is true for $n = 1$ (monopoly) and $n > 1$ (oligopoly). In addition, from the expression for the markup, we have that $\mu^0(n)$ is decreasing in n, so as the intensity of competition increases (n increases) the markup decreases reducing the extent of over-shifting. At the limit as

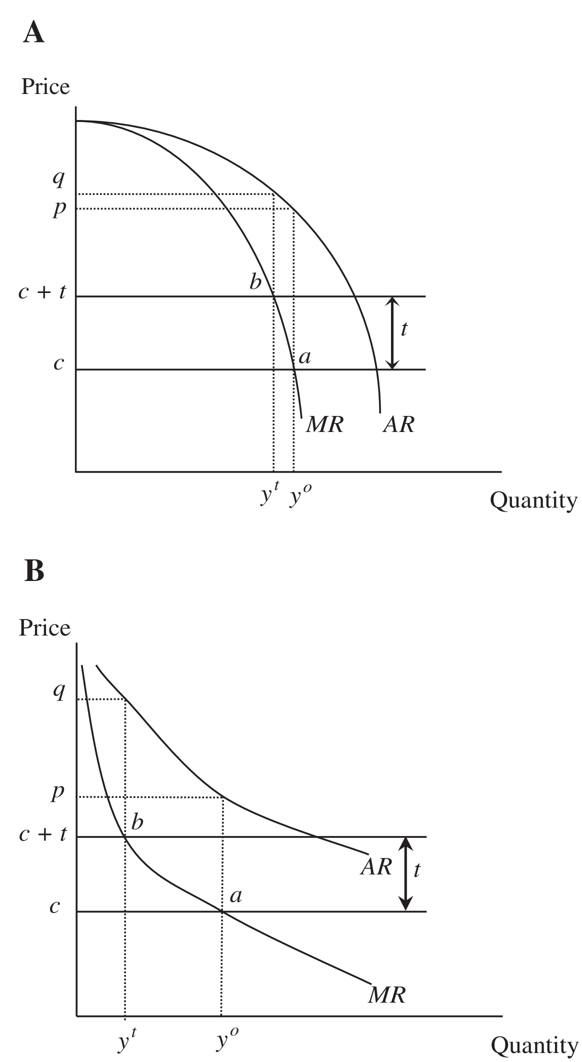


Fig. 1. a: Tax under-shifting under monopoly. b: Tax over-shifting under monopoly.

competition becomes more and more intense $\mu^0(n)$ tends to 1 and the competitive outcome of perfect tax shifting arises $q'(t) = 1$.[8] Given this markup formulae we expect stores facing more competition and stores facing more elastic demand (cross-border shopping) to shift less of the tax on the retail price.

On the effect of cross-border shopping we would expect that the shifting of the tax to the consumers will be lesser the greater the scope for cross-border shopping into another jurisdiction with unchanged tax. Bajo-Buenestado and Borrella-Mas (2018) propose a theoretical model with imperfect competition among differentiated products and cross-border tax spillover to predict that proximity to the border (interpreted as a reduction in product differentiation) reduces the tax pass-through.

4. The data

The data used in this work are provided by a major Belgian supermarket chain with a market share of 33% in Belgium. This retail chain controls >400 local retailers in Belgium, France and Luxembourg. Posted price data are automatically collected by the retailer on a daily basis for every item sold in each store of the group, together with information about any price promotions and rebates. Posted prices differ from the average "measured" price commonly available in scanner data (e.g. Standard Nielsen scanning data price measure in the US). The average "measured" price in a given week is the weekly ratio of sales revenue to the quantity sold. It is a quantity weighted average of posted prices. It can vary across stores and location even though the posted price is uniform. Indeed, stores facing less elastic demand (or higher income) would sell a relatively larger share at higher price, which induces a higher weight on higher prices and thus a higher average price in those stores (see Della Vigna and Gentzkow, 2017).

As stores are located in different areas, posted prices tend to vary considerably both within and across countries. Interestingly, this retail chain acts as local price followers: it is publicly committed to constantly monitor competitors' prices and sell its products at the lowest price in all its local markets. The price monitoring is done either online or manually by a team of its employees. This monitoring occurs on a daily basis and it is on two different levels. First, the chain monitors the price of large retailers operating on a national scale and that price uniformly over the country. If it finds that any of these retailers has a lower price for any of its products, then the chain updates immediately its price on a national scale by setting it just below the price of the competitor. Second, for all remaining small local retailers and the stores not adopting uniform pricing nationally, the chain monitors the local prices of those retailers close to each of its own stores. If the price of a product is lower in any of these local retailers, the chain updates the price of its local store by setting it just below the price of its local competitor. To inform its customers about the effectiveness of this pricing strategy, any time the price of an item has been recently decreased (less than a week) to match the lowest price of a national/local competitor, the store signals the price change on the price tag and displays the new price in red color. Furthermore, the company regularly publishes on its website a prospectus indicating the average price difference between its stores and the main competitors for every geographical area. The effectiveness of this strategy is also confirmed by the independent Belgian consumer association *Test-Achat*, which provides yearly comparative price reports of Belgian retail chains. For every year that the survey was carried out, they find that our retail chain was the cheapest among all its major competitors.

Given this local price matching strategy, any observed price change in these stores actually reflects a change in the lowest price offered by other retailers in their local market. This allows us to extend the study of the tax pass-through from one specific retail chain to each local market, by including the local influence of other retailers. Although this retail chain is committed to match local prices, one concern that can arise is that local competition in prices is meaningless if the majority of the market is supplied by large retail chains that price nationally. However, this does not seem to be the case for Belgium, where still a large part of grocery stores is made up of small independent local retailers that do not belong to larger retail chains. According to a recent report by Nielsen (2017), the market structure of grocery stores in Belgium is as follows: 42% of stores are small traditional local shops; 37% of stores belong to large and medium sized retail chains, and 21% of stores are other small supermarkets operating either at the local or national level. This market structure suggests some possible degree of spatial price dispersion and opens up to the possibility of heterogeneity in tax incidence over the country.

This work focuses on assessing the tax incidence of the tax hike in Belgium on spirits retail prices by selecting six major brands of spirit that have the unique characteristic of being sold both in Belgium (in 337 stores) and in France (in 71 stores of the same supermarket chain). This allows performing a difference-in-differences analysis by considering the price evolution of the same brand sold in France as control group during the period of tax implementation. We therefore assume that, had the tax not been implemented, the Belgian price of each of these products would have followed the same trend as that one of the same product in France. French prices in the same supermarket chain can be considered as a good control group given that these products share the same cost components and are sold by the same retailer in these two neighboring countries. Fig. A.1 in the appendix shows the location of control stores in France. As French stores are located far away from the Belgian border, we should not expect the Belgian tax reform to impact French prices via cross-border shopping. The French store closest to Belgium is about 70 km away from the Belgian border. Cross-border shopping is unlikely because of both a long driving distance (around 1 h) and the fact that French stores in this area (Lorraine region) are much closer to Luxemburg, which is the relevant cross-border shopping destination given its lower spirit prices.

We restrict attention to three brands of vodka, one brand of whiskey and two brands of rum. These products are among the leading brands in the market of spirits and have the unique characteristic of being sold in the same format in many stores both in Belgium and in France. This provides the opportunity to compare the price evolution of the exact same product in these two countries. These products differ in their alcohol content, being either 40% or 37,5% and all products considered have the same bottle size of 70 cl. Hence, the tax change is different across these products. For spirits with the 40% of alcohol content the tax increase amounts to 2,43 € per bottle. While for those with the 37,5% this amounts to 2,28 €. As mentioned in the introduction, the tax change on spirits was not in reaction to some pre-existing market conditions, as it was part of a general plan of the Belgian government aiming at shifting the tax burden from labor to consumption. Thus providing us with an exogenous tax reform.

The price data consists of the monthly posted price of each brand of spirit sold in every local store net of any rebate and temporary price promotion. For most products, these discounts are quite frequent during Christmas period, but can also occur in other periods of the year. To control for temporary price promotions, we use the highest daily price of the month (peak price) for each store. This allows controlling for temporary price cuts that are not relevant for the estimation of the tax

[8] The use of price rather than quantity as a strategic variable (Bertrand competition) intensifies competition and reduces profits. This means that the effective elasticity of demand is likely to be larger in magnitude than in the Cournot competition. However, if the cross-price elasticity is limited, the substitutability is limited (differentiated products) then the Cournot markup rule is likely to work. It is also likely to work in markets where competition is stable with no dynamic price wars in general. This kind of stable pricing would arise if firms have been competing for a long time and if there is some kind of price matching strategy in place. Recall that in our case, the supermarket chain under consideration is using an explicit price matching strategy based on local competition.

pass-through to spirit prices.[9] Price records begin three months before the tax reform and end five months after. Panels A–F in Fig. 2 display the evolution of the monthly price for each spirit during this period for both French and Belgian stores. Although a longer price series would be preferred to check for common pre-treatment trend, these figures show that prices in both countries did not diverge over the 3 months prior the tax hike. This gives us a first check of the validity of the control group. As it can be seen from these figures, the tax reform impacted Belgian prices immediately the month of its implementation, while French prices stayed quite stable all over the period. Interestingly, for products A to D the tax reform reversed the price differential between French and Belgian stores. Those products were cheaper in Belgium before the reform and became more expensive after.

Table 1 provides some descriptive statistics about the store locations. We use a set of proxies to control for some supply-side and demand-side factors that could explain spatial heterogeneity in the tax pass-through. To measure the intensity of competition faced by each store, we use a variable indicating the number of competing retailers within a driving distance of 15 min. These data are collected by a private company that provides contact information to suppliers about supermarkets and grocery stores located in Belgium. From their postal address, it is then possible to compute the driving distance from each store to any other retailer in the area. However, this variable is only available for Belgian stores. Therefore, we cannot directly control for competitive pressure in French stores. To check for the robustness of our results, we will use local density of population (in quartile) as a proxy for competitive pressure. Thus, we compare the evolution of prices between Belgian and French stores that are in the same quartile of the population density distribution of their respective country. Using each store geographical location, we can also compute their distance to the nearest border. This enables checking whether those stores close to the border (subject to potential cross-border shopping) responded differently to the tax change. Furthermore, to control for demand-side local heterogeneity, each store is matched with the average GDP per capita at the Local Administrative Unit Level (NUTS 3) and population density data at the municipality level.

5. The empirical models

In order to estimate the tax pass-through to spirits' retail prices, we perform a Difference-in-Differences analysis separately on six distinct products, by considering the retail prices of the same products sold in France as a control group. The use of French prices for the same brand as a counterfactual can potentially control for unobserved factors, common to both France and Belgium, that could have affected the brand retail price over the period of policy implementation. The analysis is organized as follows. Firstly, we estimate for each brand the tax pass-through at the chain level. This gives us a measure of how the tax was shifted across retail stores on average. Secondly, we estimate for each brand the tax pass-through at the store level. This exercise allows assessing the degree of tax pass-through heterogeneity across different geographical locations. We test whether such heterogeneity is associated to differences in local competition and/or proximity to the border. Lastly, we account for time heterogeneity in order to see how the tax shifting evolved during the period. These estimates are also important to check whether the spatial variation in tax pass-through was permanent or just temporary.

All models are estimated using the standard OLS procedure. A main concern in the difference-in-difference literature is that errors can be correlated across different groups of observations. In that case, assuming that errors are independent across observations can lead to an incorrect estimation of the standard errors for the treatment effects (Bertrand et al., 2004). In our context, the potential sources of correlation are (i) serial correlation of errors for each store; and (ii) spatial correlation of errors across stores. The first one is standard when observing the same individual/firm over multiple periods and it can be produced by unobserved characteristics that are constant overtime. The second one can be produced by local shocks that affect stores in the same area similarly. This source of correlation is quite relevant in our case since stores set their prices by matching the lowest price of any competitors within a certain radius. To account for these two possible sources of error correlation, we cluster errors at the arrondissement level. As a result, we use around 60 clusters for each product.[10] This allows us to account for both serial correlation of errors for each store and shocks that could affect stores in the same area equally. Each model is estimated separately for each of the six products analyzed.

5.1. Average tax pass-through

In this section, we estimate the average tax pass-through to the retail price of each spirit considered. We use the standard difference-in-differences procedure. The retail price for each specific brand in store i during month t is expressed as follows:[11]

$$P_{it} = \beta_0 + \beta_1 BE_i + \beta_2 T_t + \beta_3 (BE_i \times T_t) + \varepsilon_{it} \tag{1}$$

β_0 is the pre-reform price level in France. While BE_i is a dummy variable equal to 1 if the store i is located in Belgium and 0 if located in France. Its coefficient β_1 measures the pre-reform difference in prices between Belgium and France. The variable T_t is a dummy variable equal to 1 during the period of tax implementation (post-November 2015) and 0 otherwise. Its coefficient β_2 measures the price difference between the pre-reform and post-reform period in France, which serves as a counterfactual for the price evolution in Belgium. The fourth term is the interaction of the treated group BE_i and the post-reform variable T_t. Its coefficient β_3 captures the price increase in Belgium due to tax change and allows computing the tax pass-through rate as follows:

$$Tax\ Pass\ Through\ Rate = \frac{\beta_3}{\Delta tax} \times 100.$$

This work focusses on the short-run impact of the tax on retail prices, with a narrow time window going from August 2015 until March 2016. In this a way, we actually compute the difference in the average price of the product in Belgium between the three months period before the tax reform (August 2015–October 2015) and the five months period after the tax reform (November 2015–March 2016). This price change in the treated group (stores in Belgium) is then compared with the price change of the same product between the two periods in the control group (stores in France). A fundamental assumption, however, is that nothing else a part from the tax should have affected the retail price for the same spirits' brand in Belgium and France differently in the period after the tax implementation. As the period is quite narrow, it is quite easy to check that there was no major policy change in Belgium and France that should have impacted the product prices in the two countries.

Table 2 shows the estimated coefficients of model (1). The first line of Table 2 shows the intercept of the model for each product, which indicates the average product price in France in the pre-tax period. The line "Treated" shows how prices in Belgium (treated group) differ from France (control group) before the reform. The "Post-reform" line displays the price evolution in France after the reform (November 2015). Most of these coefficients are slightly negative and close to zero, thus suggesting as counterfactual that spirits prices would have

[9] We also estimate the models using the average monthly price to check whether including temporary price discounts affects our results. Yet, this exercise still confirms our findings. These results are available upon request.

[10] We also run the models clustering at either store, province or country level. In every case, we find smaller standard errors. Thus, we are reporting the most conservative estimates (i.e. those with the largest standard errors).

[11] The brand index is dropped in the rest of the analysis to ease notation.

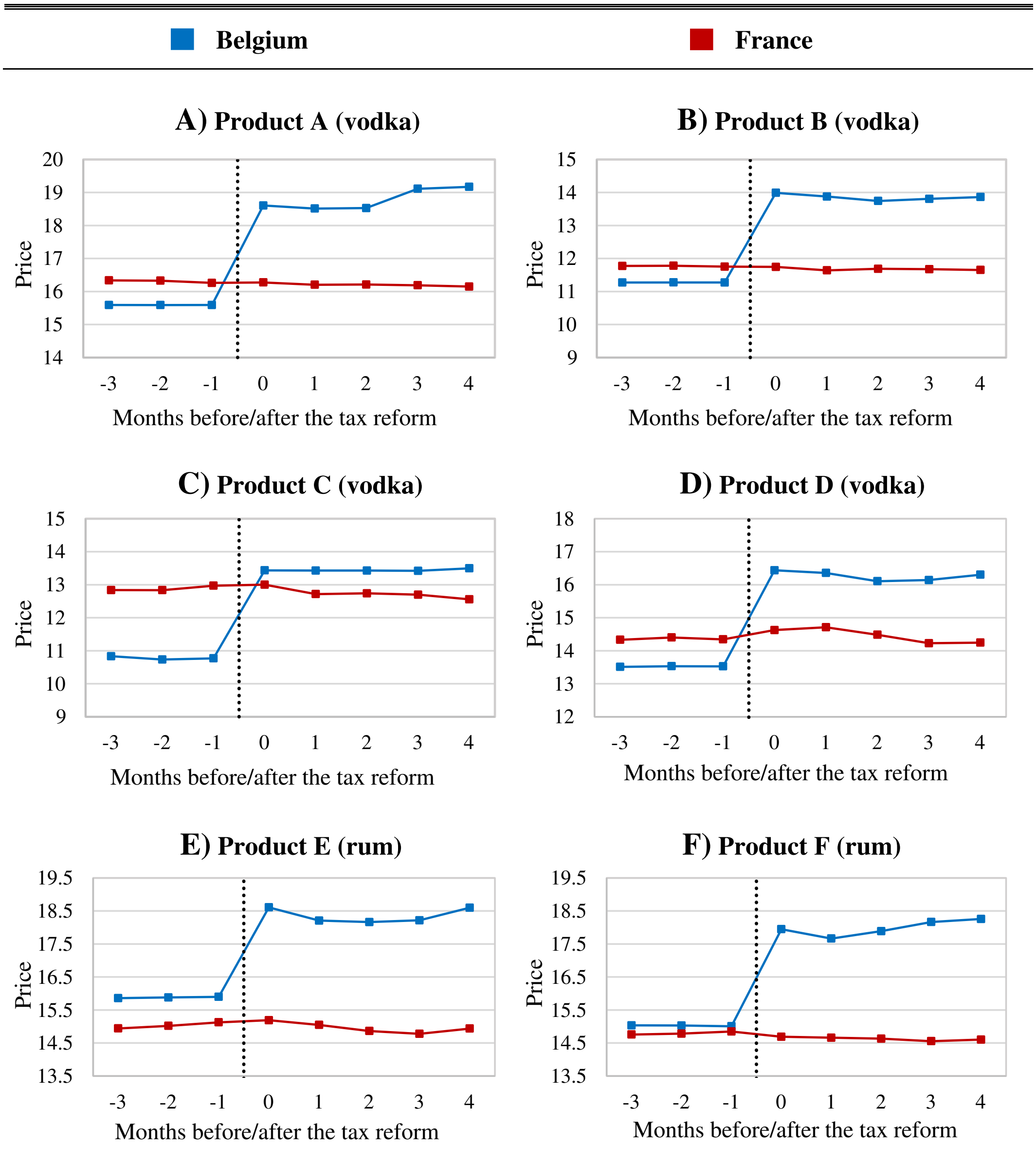


Fig. 2. Evolution of spirit prices.

slightly declined in Belgium without the tax increase. Yet, just three of them are statistically significant at the 5% level. The line "Treatment" shows the impact of the tax reform on the Belgian price for each product. These coefficients can be interpreted as the price change in € induced by the tax reform. As the products considered differ in their alcohol content, the tax change was different across products. From the tax hike specific to each product and its treatment coefficient β_3, it is then possible to calculate the tax pass-through rate. As shown in Table 2, the tax pass-through rate tends to vary across products. The tax was over-shifted to the retail prices of all spirits with a confidence level of 95%. This cross-product variation in pass-through can be related to supply-side and demand-side differences across products. We will not explore further this cross-product variation in the tax pass-through. Instead, we will study the spatial variation in the tax pass-through for each product separately.

Table 1
Characteristics of store locations.

	Average	Std. dev.	Minimum	Maximum
Belgium				
GDP per capita (€)	35.106,58	10.524	15.700	63.330
Population density	1.190,93	2.302,45	36,27	16.393,32
No. of competitors	51,48	43,26	3	225
Next to the border (20 km)	45,40%	49,86	0	1
France				
GDP per capita	28.828,17	5.796	20.400	42.500
Population density	378,18	650,88	9,25	4.635,45

5.2. Spatial heterogeneity in the tax pass-through

In this section, we focus on identifying the spatial variation of tax pass-through for the same product across stores. To get a preliminary measure of heterogeneity in tax shifting, we compare spatial price dispersion in both Belgium and France before and after the tax reform. The spatial price variance of each spirit across Belgian stores has significantly increased after the tax reform, while it stayed constant over the same period in France. A Levene's Test on the homogeneity of spatial price variances between the pre-reform and post-reform period is

Table 2
Average tax pass-through (model (1)).

	Product					
	A	B	C	D	E	F
Intercept (β_0)	16,31***	11,77***	12,88***	14,36***	15,03***	14,80***
	(0,08)	(0,04)	(0,08)	(0,06)	(0,09)	(0,11)
Treated (β_1)	−0,71***	−0,50***	−2,10***	−0,84***	−0,85***	0,22**
	(0,08)	(0,05)	(0,09)	(0,06)	(0,09)	(0,11)
Post-reform (β_2)	−0,10**	−0,09***	−0,14	0,10	−0,06	−0,17**
	(0,05)	(0,03)	(0,10)	(0,07)	(0,07)	(0,07)
Treatment (β_3)	**3,30*** **	**2,67*** **	**2,80*** **	**2,64*** **	**2,54*** **	**3,13*** **
	(0,05)	(0,05)	(0,11)	(0,08)	(0,09)	(0,07)
No. observations	2960	3096	3248	3256	3240	3208
Product type	Vodka	Vodka	Vodka	Whiskey	Rum	Rum
% alcohol	40%	37,5%	37,5%	40%	37,5%	40%
Excise tax increase	2,43 €	2,28 €	2,28 €	2,43 €	2,28 €	2,43 €
% pass-through	**135,80**	**117,11**	**122,81**	**108,64**	**111,40**	**128,81**
Confidence interval	131,68–139,91	112,28–121,49	113,60–132,02	102,06–115,64	103,51–119,74	122,63–134,98

Notes: *, ** and *** indicate statistical significance at the 0.10, 0.05 and 0.01 level respectively. Standard errors, clustered at the arrondissement level, are in parenthesis.

rejected for all products in the treated group with the 99% confidence level (except for F). While it is accepted for all products in the control group (except F, for which it has slightly declined).[12]

To provide more compelling evidence about the evolution of spatial differences in spirit prices, we estimate the same model as above (model (1)) by including both store fixed effects and a store specific treatment effect. This will deliver a store specific tax pass-through. Store fixed effects are fundamental in order to capture tax pass-through heterogeneity. This is because they can account for possible pre-reform (time invariant) unobserved factors that affect the store's pricing. These can include differences in the cost of selling the products (such as transportation costs, rents or local wages) and in price elasticity of demand. If we do not correctly control for these pre-reform differences in prices across stores, there is a risk of confounding them with heterogeneity in tax shifting. From now on, every model we present includes store fixed effects. Formally, we estimate the following regression for each product:

$$P_{it} = \delta_i + \beta_2 T_t + \beta_{3i}(BE_i \times T_t \times \delta_i) + \varepsilon_{it}. \tag{2}$$

where δ_i are the fixed effects store i located in either Belgium or France. These are captured by store-specific dummy variables and give the average price level of each store i before the tax reform. The coefficient β_2 is capturing the evolution of the average price in French stores after the tax reform. Which is seen as the counterfactual scenario. While β_{3i} is the store i's specific tax pass-through if this store is located in Belgium. The results of these estimations are shown in Fig. 3A–F. Results are aggregated at the municipality level. Every color represents a certain degree of tax pass-through in a given municipality. Interestingly, since these stores are local price followers, their tax shifting should be indicative of the general trend in spirit price changes for each geographical location. These figures display heterogeneous tax shifting across space after the tax reform. Although the tax was over-shifted to different extents in most municipalities, there are also some areas where the tax was instead under-shifted (blue areas in the figures).

Variation in the tax pass-through is related to variation in market structure and price elasticity of demand. Thus, accounting for spatial differences in these two factors can enable us to understand such heterogeneity in tax shifting. In order to do so, we proceed as follows. First, we test for the effect of local competition on the tax pass-through at the store level. To account for local differences in market structure, the model contains information about the intensity of competition at the store level. Intuitively, one would expect lower tax pass-through when there are many competitors nearby. Second, we focus on the proximity to the border. The scope for cross-border shopping may be quite important in Belgium, a relatively small country, because a large part of the population lives in proximity to the border (and there are many cross-border workers). This is also relevant because Belgium shares borders with several different countries which set different alcohol taxes. For this reason, we also estimate a model that includes information about the proximity to the border of each store. That model allows us to test for differences in price setting for stores close to the border. If cross-border shopping is an effective threat for those stores, tax shifting in border areas should be lower as the demand elasticity would be higher. Third, as demand-side factors may distort our results, we also estimate a model that includes information about spatial heterogeneity in some supply-side and demand-side factors.

5.2.1. Intensity of competition

Having information about the number of competing retailers for each store allows us to test for the effect of competition on the tax pass-through.[13] As we are comparing the tax shifting of the same product across different geographical locations, it is clear that we restrict our focus to the intensity of competition among retailers and not among producers. Each product analyzed is among the world's most popular brands in their respective category and none of their producers is vertically integrated with any Belgian or French retailer. To test whether the local intensity of competition at the store level can be related to the observed spatial heterogeneity in tax pass-through, we compare the tax shifting among areas exhibiting a low, medium or high intensity of competition.

We define the intensity of competition in terms of number of local competitors for each store within a driving distance of 15 min. The competitors are from different supermarket chains than the chain under study. A store is considered in a low-competition cluster if it falls in the first quartile of this distribution with at most 26 local competitors. A store is in a medium-competition cluster if it falls in the 2nd or 3rd quartile of the distribution with between 27 and 59 local competitors. While it is in a high-competition cluster if it is in the last quartile of the distribution with >60 competitors. Formally, we estimate the following regression:

$$\begin{aligned} P_{it} = {} & \delta_i + \beta_2 T_t \\ & + \beta_L (BE_i \times T_t \times Low_{Comp_i}) \\ & + \beta_M (BE_i \times T_t \times Med_{Comp_i}) + \beta_H \left(BE_i \times T_t \times High_{Comp_i}\right) + \varepsilon_{it}. \end{aligned} \tag{3}$$

where Low_{Compi}, Med_{Compi} and $High_{Compi}$ are dummy variables equal to one if store i is in either a low, medium or high-competition cluster.

[12] The results of this test can be found in Table A.1 in the appendix.

[13] As mentioned earlier, we cannot claim any causality here because we do not have exogenous variation in competition to identify the possible causal effect of competition on tax pass through.

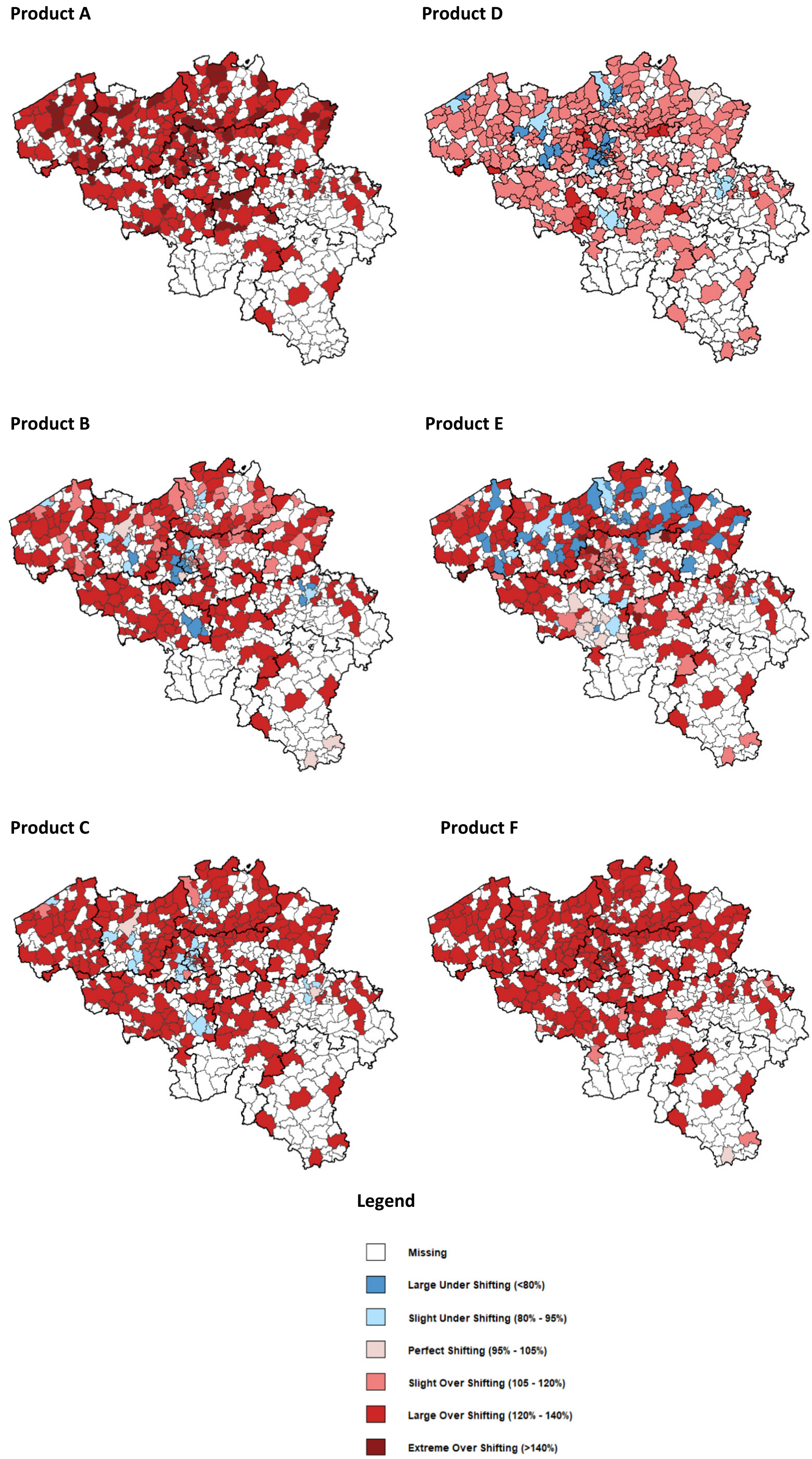


Fig. 3. Tax shifting at the arrondissement level. Dark blue is large under-shifting (<80%), Light blue is slight under-shifting (80%–95%), Light pink is perfect shifting (95%–100%), Dark pink is slight over-shifting (105%–120%), red is large over-shifting (120–130%) and brown is extreme over-shifting (>140%).

Table 3
Tax pass-through and intensity of competition (model (3)).

	Product					
	A	B	C	D	E	F
Low competition	**3,36**	**2,82**	**2,92**	**2,76**	**2,79**	**3,11**
($\boldsymbol{\beta_L}$)	(0,06)	(0,04)	(0,11)	(0,08)	(0,09)	(0,08)
Medium competition	**3,29**	**2,78**	**2,91**	**2,78**	**2,48**	**3,15**
($\boldsymbol{\beta_M}$)	(0,05)	(0,04)	(0,11)	(0,08)	(0,12)	(0,08)
High competition	**3,25**	**2,32**	**2,47**	**2,27**	**2,41**	**3,11**
($\boldsymbol{\beta_H}$)	(0,06)	(0,08)	(0,08)	(0,12)	(0,14)	(0,08)
Test on the equality of coefficients ($H_0 : \beta_L = \beta_H$)						
F value	13,98	46,78	39,05	26,12	8,76	0,04
p-value	<0,01	<0,01	<0,01	<0,01	<0,01	0,84

Notes: All coefficients are statistically significant at the 0,01 level. Standard errors, clustered at the arrondissement level, are in parenthesis. The last two rows show the results of the Wald test on the equality of the coefficients for low and high competition, where the null hypothesis is $H_0 : \beta_L = \beta_H$.

Table 4
Tax pass-through rate and intensity of competition.

	Product					
	A	B	C	D	E	F
Low competition	**138%**	**124%**	**128%**	**114%**	**122%**	**128%**
(C.I.)	133–142	120–127	119–137	107–121	117–131	121–135
Medium competition	**135%**	**122%**	**128%**	**114%**	**109%**	**130%**
(C.I.)	130–140	118–125	118–138	108–121	98–119	123–136
High competition	**134%**	**102%**	**108%**	**93%**	**105%**	**128%**
(C.I.)	129–138	94–108	97–119	84–103	94–117	121–135

We want to estimate the coefficients β_L, β_M and β_H, reflecting the tax pass-through specific to each of these three competition clusters. We expect these coefficients to be statistically different from each other and, in particular, to decrease with the intensity of competition. That is, we expect to find that $\beta_L > \beta_M > \beta_H$. The results of this estimation are displayed in Table 3. The last two rows of this table also show the results of the Wald test on the equality of coefficients for low and high competition. Where the null hypothesis is that there is no difference in tax shifting between low and high competition. That is, $H_0 : \beta_L = \beta_H$.

The results of Table 3 tend to confirm our theoretical prediction. The price increase was smaller in high-competition areas. The magnitude of this effect, however, can vary across products. For most products, the difference in tax shifting between low and high competition is between 0,40 € and 0,50 €. The magnitude of such effect is much smaller for product A, for which this difference is equal to 0,11 €. While it is absent for product F. The test on the equality of coefficients for high and low competition indicates that, except for product F, these differences in tax shifting are statistically significant at the 99% confidence level. Therefore, the results of *model* (3) suggest that the tax shifting decreased with the intensity of competition at the local level.

To retrieve the tax pass-through rate for each competition level, we divide the treatment coefficients presented in Table 3 by the product specific increase in the excise tax. The results are displayed in Table 4. As already suggested in Table 3, the tax pass-through rate varies with the intensity of competition. The tax was largely over-shifted with low competition. Whereas, it was shifted to a lesser extent or even under-shifted with high competition. This indicates that the extent of tax shifting and the intensity of competition are indeed negatively correlated.

5.2.2. Cross-border shopping

Another possible source of the tax pass-through heterogeneity is the proximity to the border. Cross-border shopping can be quite important in Belgium since a large part of the population lives close to the border. In our sample, 45,4% of Belgian stores are within a distance of 20 km to the border. Moreover, Belgium shares borders with four different countries (France, Luxembourg, Germany and The Netherlands), which have different levels of alcohol taxation and spirit prices. The alcohol tax reform in Belgium has considerably increased the price gap in spirit prices between Belgian and foreign stores. Luxembourg and to a lesser extent Germany, had lower spirit prices before the reform. Whereas the Netherlands and to a lesser extent France, had higher spirit prices before the reform. In order to investigate the relationship between tax pass-through and the scope for cross-border shopping, we estimate a model that includes information about the proximity to the border of each store. This allows testing for differences in tax shifting according to whether or not stores are close to the border. For each specific product, we estimate the following model:

$$P_{it} = \delta_i + \beta_2 T_t + \beta_3 (BE_i \times T_t) + \beta_{BR} \left(BE_i \times T_t \times BR_{km_i}\right) + \varepsilon_{it}. \tag{4.1}$$

The only difference here is the inclusion of the last interaction term ($BE_i \times T_t \times BR_{km_i}$). Where BR_{km_i} is a dummy variable indicating whether store *i* is within a certain km distance to the border. The coefficient β_{BR} therefore measures the difference in the treatment effect (tax shifting) for those stores that are within that certain distance to the border. In particular, we use three different distances. Namely 10 km, 15 km or 20 km. As long as cross-border shopping is really binding price decisions, we expect β_{BR} to be negative and significantly different from zero.

The results of *model* (4.1) are displayed in Table 5. Table 5 shows that tax shifting did not change with the proximity to any border.[14] At any distance considered, those stores close to the border did not shift differently the tax to the retail price compared to other stores. We obtain the same results even when controlling for the intensity of competition as in *model* (3). This suggests that the threat of cross-border shopping does not seem to play a significant role in the shifting of the tax on spirit prices, even though the price gap with several neighboring countries increased substantially after the reform. A possible explanation for this can be the fact that the price gap with neighboring countries was not high enough to justify a price adjustment at the border or that Belgian stores are poorly informed about foreign prices near the border. Another possible option could be the market segmentation between mobile and immobile shoppers. The stores locate close to the border only retain the non-cross-border shoppers (immobile shoppers) who are likely to exhibit more inelastic demand than the cross-border shoppers

[14] Although we find a slightly positive difference for stores within 10 km distance from the border for two products, this disappears once controlling for the number of competitors.

Table 5
Tax pass-through and proximity to any border (model (4.1)).

β_{BR}	Product					
	A	B	C	D	E	F
Border at 20 km	−0,01 (0,03)	0,07 (0,11)	0,09 (0,09)	0,07 (0,09)	0,03 (0,11)	−0,01 (0,01)
Border at 15 km	0,03 (0,03)	0,07 (0,09)	0,12 (0,08)	0,12 (0,09)	0,14 (0,11)	−0,03 (0,01)
Border at 10 km	0,06** (0,03)	0,06 (0,09)	0,11 (0,09)	0,13 (0,10)	0,22** (0,10)	−0,02 (0,02)

Notes: *, ** and *** indicate statistical significance at the 0.10, 0.05 and 0.01 level respectively. Standard errors, clustered at the arrondissement level, are in parenthesis. Each row shows the estimated coefficient β_{BR} for every product considering stores within a 10 km, 15 km or 20 km distance to any border.

Table 6
Tax pass-through and proximity to Luxembourg (model (4.2)).

	Product					
	A	B	C	D	E	F
Low competition and no proximity to Luxembourg ($\boldsymbol{\beta_L}$)	**3,36** (0,06)	**2,83** (0,04)	**2,92** (0,11)	**2,76** (0,08)	**2,80** (0,10)	**3,12** (0,08)
Low competition and proximity to Luxembourg ($\boldsymbol{\beta_{LUX}}$)	**3,30** (0,05)	**2,45** (0,18)	**3,00** (0,11)	**2,70** (0,09)	**2,63** (0,11)	**2,73** (0,19)
Medium competition ($\boldsymbol{\beta_M}$)	3,29 (0,06)	2,78 (0,04)	2,91 (0,11)	2,78 (0,08)	2,48 (0,12)	3,15 (0,08)
High competition ($\boldsymbol{\beta_H}$)	3,25 (0,06)	2,32 (0,08)	2,47 (0,13)	2,27 (0,12)	2,41 (0,14)	3,11 (0,08)
Test on the equality of coefficients $(\boldsymbol{H_0 : \beta_L = \beta_{LUX}})$						
F value	15,49	4,42	3,10	2,09	3,10	4,90
p-value	<0,01	0,04	0,08	0,15	0,08	0,03

Notes: All coefficients are statistically significant at the 0,01 level. Standard errors, clustered at the arrondissement level, are in parenthesis. The last two rows show the results of the Wald test on the equality of the coefficients for low-competition areas either close to (β_{LUX}) or far away (β_L) from Luxembourg, where the null hypothesis is $H_0 : \beta_L = \beta_{LUX}$.

(mobile shoppers). This effect could offset the downward pressing effect of cross-border shopping on prices.

The absence of border effect on tax shifting may also be due to the averaging out of various border effects among the four different neighboring countries. Indeed, if the border effect depends on the size and the sign of the price gap, we may expect different border effects for the four different countries, notably for Luxembourg with the lowest spirit price. We test for this hypothesis by re-estimating *model* (4.1) differently. That is, we now consider each border separately to estimate how tax shifting varies when a store is close to a specific border. In doing so, we did not find any significant impact when considering just those stores at the border with either France, Netherlands or Germany. Where prices were respectively comparable, higher or slightly lower than in Belgium before the reform.[15] However, we did find some interesting results for those stores close to Luxembourg (where spirits were on average 4 € cheaper before the tax reform).

In our sample, we only have three stores that are located within 10 km distance from the Luxembourg border and no other store is located within 20 km. These stores are all located in remote areas with a small number of competitors (less than nine) and hence they face a quite low competition. As we have learned from the results of *model* (3), this means that the tax shifting of these stores should be significantly higher than the one of stores facing more competition. Yet, if competition at the Luxembourg border matters, this effect can be ambiguous. This is because the lower domestic competition could be offset by the higher foreign competition from Luxembourg. In order to limit cross-border shopping, these stores could have shifted the tax on spirit prices to a lesser extent compared to those stores facing a similar domestic competition but no proximity to the border. Formally, to measure the tax pass-through of stores at the border of Luxembourg we estimate the following regression for each product separately:

$$P_{it} = \delta_i + \beta_2 T_t + \beta_L \left(BE_i \times T_t \times Low_{Comp_i} \times NoLUX_{B_i}\right) + \beta_{LUX}\left(BE_i \times T_t \times Low_{Comp_i} \times LUX_{B_i}\right) + \beta_M\left(BE_i \times T_t \times Med_{Comp_i}\right) + \beta_H\left(BE_i \times T_t \times High_{Comp_i}\right) + \varepsilon_{it}. \tag{4.2}$$

[15] As for *model 4.1*, no effect was found when considering stores within either 20 km, 15 km or 10 km from the border.

where Low_{Compi}, Med_{Compi} and $High_{Compi}$ are the same variables as in *model* (3). However, the first interaction term includes the dummy variable $NoLUX_{Bi}$, which is equal to 1 if store i is not at the border of Luxembourg (within 10 km). The coefficient β_L therefore measures the tax pass-through of stores facing low competition and not at the border of Luxembourg. The dummy variable LUX_{Bi} is instead equal to 1 if a store is close to Luxembourg (within 10 km). Hence, the coefficient β_{LUX} measures the tax pass-through of these stores, which are also all facing low domestic competition. The other variables are the same as in *model* (3). The objective of this regression is to estimate β_{LUX} and test whether $\beta_{LUX} < \beta_L$. That is, we would like to know whether for the same level of (domestic) competition, tax shifting decreases with the proximity to the border of Luxembourg.

The results of *model* (4.2) are displayed in Table 6. From this table we can compare the tax pass-through of store close to Luxembourg (β_{LUX}) with other stores located in low-competition areas (β_L). Interestingly, the tax pass-through of stores close to Luxembourg seems to be lower than the one of other stores in low-competition areas. This is true for most product. Yet, the Wald test on the equality of coefficient suggests that only three of these differences in tax pass-through are significant at the 0,05 level. These are the products A, B and F. For product B and F, such difference is quite large, being close to 0,40 €, while it is small for product A, being only 0,06 €. The difference is 0,17 € for product E, but it is only significant at the 0,10 level. This heterogeneity in the "border effect" across products might depend on many factors, such as different tastes for different products to make it worth doing cross-border shopping or the effective supply of those same products on the other side of the border. This cross-product heterogeneity of the "border effect" also suggests that it is important to analyze the tax pass-through at the product level. Since we could not have found this effect when averaging out the border effect over different products.

The results of *model* (4.1) and *model* (4.2) suggest that only a significant price gap with a neighboring country can reduce tax shifting for some products (but not for all). This is confirming the standard view that the scope for cross-border shopping increases with the price gap between neighboring countries. Yet, the absence of "border effect" for stores close to either France (where spirit prices were only 0,5 € higher before the tax) or Germany (where spirit prices were around 1 € lower before the tax) could also suggest a lack of information/attention about foreign prices.

Table 7
Controlling for demand-side characteristics (model (5)).

	Product					
	A	B	C	D	E	F
"Gross" treatment ($\boldsymbol{\beta_3}$)	−2,83 (1,72)	3,65 (2,23)	2,12 (5,04)	−2,49 (3,46)	−7,05** (3,50)	−3,71* (3,16)
GDP per capita FR ($\boldsymbol{\beta_{Y_F}}$)	−0,54*** (0,17)	0,04 (0,19)	−0,10 (0,55)	−0,54* (0,33)	−0,85*** (0,28)	−0,65** (0,31)
GDP per capita BE ($\boldsymbol{\beta_{Y_B}}$)	0,62*** (0,17)	0,01 (0,23)	0,18 (0,51)	0,62* (0,34)	0,98*** (0,35)	0,68** (0,31)
Rural areas FR ($\boldsymbol{\beta_{R_F}}$)	0,09 (0,10)	0,04 (0,05)	0,26 (0,16)	0,24* (0,12)	0,00 (0,08)	0,05 (0,04)
Rural areas BE ($\boldsymbol{\beta_{R_B}}$)	0,00 (0,11)	−0,08 (0,07)	−0,33* (0,16)	−0,32** (0,14)	0,27** (0,11)	−0,10 (0,05)
No. of competitors ($\boldsymbol{\beta_C}$)	**−0,06***** (0,02)	**−0,30***** (0,06)	**−0,27***** (0,07)	**−0,30***** (0,08)	**−0,15*** (0,08)	−0,02 (0,01)
Proximity to Luxembourg ($\boldsymbol{\beta_{LUX}}$)	**−0,16***** (0,04)	**−0,72***** (0,18)	**−0,21**** (0,09)	**−0,43***** (0,12)	**−0,27*** (0,14)	**−0,40**** (0,18)

Notes: *, ** and *** indicate statistical significance at the 0.10, 0.05 and 0.01 level respectively. Standard errors, clustered at the arrondissement level, are in parenthesis.

5.2.3. Demand-side heterogeneity

All models estimated so far provide a supply-side explanation on the spatial heterogeneity in the tax pass-through based on the idea that domestic and foreign competition circumstances vary across space. Yet, tax incidence can also depend on the demand circumstances that may also vary across space. Therefore, we estimate another model of tax pass-through heterogeneity that controls for some differences in demand-side characteristics. We do that by including information about local population density (whether the store is in a rural area or not) and the GDP per capita at the arrondissement level. Intensity of competition is now measured by the log of competing stores within a driving distance of 15 min from the store. We account for proximity to the border as in *model* (4.1). The treatment coefficient estimates of this model will tell us whether the heterogeneity in the tax pass-through is still correlated to the intensity of competition and proximity to Luxembourg after controlling for differences in some observable demand-side characteristics (such as rural/urban status and GDP per capita). For each specific product, we estimate the following regression:

$$\begin{aligned} P_{it} = \delta_i &+ \beta_2 T_t + \beta_3 (BE_i \times T_t) + \beta_{Y_F} (T_t \times \ln(Y)_i) \\ &+ \beta_{Y_B} (BE_i \times T_t \times \ln(Y)_i) + \beta_{R_F} (T_t \times Rural_i) \\ &+ \beta_{R_B} (BE_i \times T_t \times Rural_i) + \beta_C (BE_i \times T_t \times \ln(COMP)_i) \\ &+ \beta_{LUX} (BE_i \times T_t \times LUX_{Bi}) + \varepsilon_{it}. \end{aligned} \tag{5}$$

As for every other specification, δ_i is the store-specific fixed effect, which captures all those pre-reform unobserved factors that are store-specific and time-invariant. The coefficients β_2 and β_3 measure the baseline of the counterfactual and the treatment effect respectively. The variable $\log(Y)_i$ is the log of the GDP per capita in the arrondissement in which store *i* is located. While $Rural_i$ is a dummy variable equal to one when the store is in a rural area (with <200 inhabitants per km^2). Each of these variables is interacted with the post-reform dummy (T_t) and the treatment interaction term ($BE_i \times T_t$). Their respective coefficients measure how prices evolved after the reform in the control (France) and in the treated group (Belgium). In particular, β_{Y_B} and β_{R_B} measure the additional effect of treated stores relative to control stores in areas with higher GDP and rural areas, respectively. $\ln(COMP)_i$ is the log of the number of competing retailers for store *i*. The coefficient β_C measures how tax shifting varies with the number of competing retailers. If results of *model* (3) are confirmed, we expect to find $\beta_C < 0$. That is, tax pass-through should decrease with competition. LUX_{Bi} is a dummy variable indicating if a store is close to the border with Luxembourg (within 10 km). Because in this model we include the baseline treatment effect, the interpretation of β_{LUX} is slightly different from the one of *model* (4.2). Here β_{LUX} estimates directly by how much tax shifting differs in these areas with respect to the average store, once controlling for some spatial differences in demand-side characteristics and the number of competing retailers.

The estimates of *model* (5) are reported in Table 7. The coefficients β_3 is the "gross" treatment effect. Although it is negative for most products, that does not mean a net negative treatment effect. Indeed, one must take into account the other treatment interaction effects, notably the coefficient β_{Y_B} for the GDP interaction that is positive for every product, although not always significant. Consider for instance product E. Its "gross" treatment effect β_3 is equal to $-7{,}05$, while β_{Y_B} amounts to 0,98. Considering that the lowest GDP per capita amounts to 15.700 €, taking the log and multiplying by the β_{Y_B} we obtain $\ln(15.700) \times 0,98 = 9,47$. The net treatment effect after controlling for the GDP is then equal to $9{,}47 - 7{,}05 = 2{,}42$. This indicates that in areas with the lowest GDP per capita, prices in Belgium after the tax reform increased by 2,42 € more than similar areas (in terms of GDP) in France. As all other stores have a higher GDP per capita, the treatment effect after controlling for GDP must be greater than this figure.[16] The fact that β_{Y_B} tend to be positive for most products suggests that spirit prices in Belgium increased by more in richer areas compared to France. Yet, the results in Table 7 also show that this effect is mostly driven by a decline of spirit prices for stores located in richer areas in France. Furthermore, stores in rural areas do not seem to follow any particular trend after the reform.

Interestingly, the results of *model* (5) seem to confirm our previous findings on the correlation between the tax pass-through and the local competition. The extent of the tax pass-through is negatively correlated to the number of local competitors for all products except F. This effect is more prevalent and it is similar in magnitude for products B, C and D. It is smaller but still significantly different from zero for product A, while it is only significant at the 10% level for product E. To get an idea on the magnitude of the competition effect on tax shifting, we compute how the tax pass-through changes when increasing the number of competitors from 20 to 100. We consider the case of a store located in an area with the average GDP per capita and ignore the rural area and border effect. Considering product D, the treatment effect for a store with only 20 competitors would be equal to the following:

$$\begin{aligned} \tau_{20} &= \beta_3 + (\ln(Y_i) \times B_{Y_B}) + (\ln(COMP_i) \times B_C) \\ &= -2,49 + (\ln(35.100) \times 0,62) - (\ln(20) \times 0,30) = 3,03. \end{aligned}$$

While if the number of competitors rises to 100 we get:

$$\tau_{100} = -2,49 + (\ln(35.100) \times 0,62) - (\ln(100) \times 0,30) = 2,55.$$

Which means that increasing the number of competitors from 20 to 100 decreases the tax shifting by 0,48 €. These results are in line with those of *model* (3), in which the difference in tax shifting between low and high-competition areas for product D was 0,49 € on average.

Model (5) also confirms that stores close to Luxembourg tend to set lower spirit prices after the tax reform as β_{LUX} is negative and significant for most products. This effect seems more pronounced than the one found in *model* (4.2). Although the two coefficients have a different interpretation and cannot be directly compared. This is probably because *model* (5) controls for the number of competing retailers through a continuous variable (i.e. the natural log of the number of competitors), which is extremely low at the Luxembourg border (less than nine). Hence, the Luxembourg border dummy could also capture some non-

[16] However, in order to compute the overall net treatment effect all other treatment interaction terms must also be taken into account.

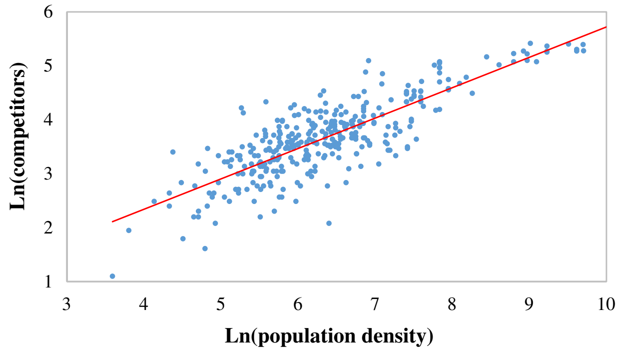


Fig. 4. Population density versus number of competitors (Belgian stores).

linearity in the relationship between the number of competitors and the tax shifting. Overall, these results indicate that, after controlling for some observable heterogeneity in demand-side characteristics, the number of competing retailers and proximity to Luxembourg (the lowest price country) are still significantly correlated with the heterogeneity in the tax shifting.

5.2.4. Robustness checks

A possible concern in estimating the relationship between competition and tax pass-through can be the lack of a proper counterfactual for stores facing a similar degree of competition in France (our control group). As we do not observe the number of competitors for the French stores, we did not formally check whether spirit prices in France have changed differently after the reform in high-competition and low-competition areas. The validity of the control group requires to compare stores in France and in Belgium facing the same level of competition.

The results of the Levene's test presented at the beginning of this section show that the spatial price dispersion was mostly stable in France after the tax reform, while it increased substantially in Belgium. This suggests that the spirit prices in the control group did not diverge much across stores facing different competition after the tax reform. However, it is still possible that this "average effect" conceals heterogeneous changes between high-competition and low-competition stores in France.

To address this issue, we run another model using population density at the local level (municipality) as a substitute to proxy for the intensity of competition. In such a way, we can compare stores facing different intensity of competition (proxied by the population density) both in France (control group) and in Belgium (treated group). The assumption here is that French stores face more competition in high population density areas. The use of population density to measure the intensity of competition at the local level is not a bad proxy. As shown in Fig. 4, the number of stores in a local area is highly correlated with the population density in Belgium.

The idea is to re-estimate *model* (4.2) by using the population density at the municipality level instead of the number of competitors. To control for the difference in population density among Belgian and French municipalities we will express the population density in quartiles in the regression. In such a way, we compare the price changes between Belgian and French stores that are in the same quartile of the population density distribution of their respective country. For instance, we consider in the low-competition areas, those stores that are in the first quartile of the population density distribution of either Belgium or France. Formally, for each product we estimate the following model:

$$\begin{aligned} P_{it} = {} & \delta_i + \beta_{L_F}(T_t \times Low_{den_i}) + \beta_{L_B}(BE_i \times T_t \times Low_{den_i} \times NoLUX_{B_i}) \\ & + \beta_{LUX}(BE_i \times T_t \times Low_{den_i} \times LUX_{B_i}) + \beta_{M_F}(T_t \times Med_{den_i}) \\ & + \beta_{M_B}(BE_i \times T_t \times Med_{den_i}) + \beta_{H_F}\left(T_t \times High_{den_i}\right) \\ & + \beta_{H_B}\left(BE_i \times T_t \times High_{den_i}\right) + \varepsilon_{it}. \end{aligned} \tag{6.1}$$

The structure of *model* (6.1) is similar to *model* (4.2). Here the difference is that we use population density as a proxy for competition so that we can control for the price changes of French stores facing different level of competition. The counterfactual scenarios for different levels of competition are captured by the coefficients β_{L_F}, β_{M_F} and β_{H_F}. Which correspond to the post-reform price changes in France for stores that are in low, medium or high-competition areas, respectively. The coefficients β_{L_B} and β_{LUX} measure the tax pass-through for Belgian stores in low-competition areas not close and close to Luxembourg, respectively. Note that their counterfactual scenario is not the same as in *model* (4.2), where we use the average price change in France β_2. Here the counterfactual scenario is β_{L_F}, which is the specific price change in France in low competitive areas (less densely populated). Similarly, the coefficients β_{M_B} and β_{H_B} measure Belgian stores' tax pass-through in medium and high-competition areas compared to their respective counterfactual in France. The results of this estimation are displayed in Table 8.

Interestingly, the results of *model* (6.1) are similar to those of *model* (3) and *model* (4.2). Tax shifting decreases with population density (used as proxy for competition). The magnitude of the "competition effect" is also quite similar to the one we find in the previous models. The Wald test on the equality of coefficients indicates that for most products this difference is statistically significant at the 0,01 level (except for product C, where it is significant at the 0,07 level and product F where the competition effect is not significant as in the previous models).

As for the "border effect", we find very similar results to *model* (4.2) when comparing tax shifting in low-competition areas in the proximity

Table 8
Population density as a proxy for competition (model (6.1)).

	Product					
	A	B	C	D	E	F
Low pop. density and no proximity to Luxembourg (β_{L_B})	**3,48** (0,07)	**2,83** (0,05)	**2,90** (0,08)	**2,89** (0,10)	**2,95** (0,14)	**3,21** (0,14)
Low pop. density and proximity to Luxembourg (β_{LUX})	**3,37**(0,07)	**2,45** (0,18)	**2,98** (0,08)	**2,79** (0,10)	**2,78** (0,16)	**2,83** (0,22)
Medium pop. density (β_{M_B})	**3,27** (0,08)	**2,71** (0,05)	**2,84** (0,14)	**2,65** (0,12)	**2,45** (0,12)	**3,11** (0,07)
High pop. density (β_{H_B})	**3,17** (0,06)	**2,47** (0,09)	**2,61** (0,15)	**2,37** (0,12)	**2,28** (0,14)	**3,07** (0,06)
Test on the equality of coefficients $(H_0: \beta_{L_B} = \beta_{H_B})$						
F value	30,55	11,91	3,50	11,05	12,62	1,81
p-value	<0,01	<0,01	0,07	<0,01	<0,01	0,18
Test on the equality of coefficients $(H_0: \beta_{L_B} = \beta_{LUX})$						
F value	12,59	4,63	3,65	3,43	3,57	4,76
p-value	<0,01	0,04	0,06	0,07	0,06	0,03

Notes: All coefficients are statistically significant at the 0,01 level. Standard errors, clustered at the arrondissement level, are in parenthesis. The table displays only the treatment coefficients for Belgium. The 5th and 6th rows show the results of the Wald test on the equality of the coefficients for low and high population density, where the null hypothesis is $H_0: \beta_{L_B} = \beta_{H_B}$. The last two rows show the results of the Wald test on the equality of the coefficients for low density areas close (β_{LUX}) or not close (β_{L_B}) to Luxembourg, where the null hypothesis is $H_0: \beta_{L_B} = \beta_{LUX}$.

Table 9
Population density as a proxy for competition with controls for demand-side characteristics (model (6.2)).

	Product					
	A	B	C	D	E	F
"Gross" treatment ($\boldsymbol{\beta_3}$)	−1,85 (1,57)	4,09* (2,18)	3,83 (5,14)	−1,33 (3,22)	−6,77* (3,62)	−2,85* (2,58)
GDP per capita (FR) ($\boldsymbol{\beta_{Y_F}}$)	−0,49*** (0,15)	0,04 (0,18)	−0,06 (0,49)	−0,55* (0,30)	−0,85*** (0,28)	−0,62** (0,28)
GDP per capita (BE) ($\boldsymbol{\beta_{Y_B}}$)	0,60*** (0,16)	−0,01 (0,22)	0,09 (0,51)	0,55* (0,33)	1,00*** (0,35)	0,64** (0,29)
Rural areas (FR) ($\boldsymbol{\beta_{R_F}}$)	0,23 (0,20)	0,14 (0,16)	0,44* (0,23)	0,33* (0,20)	0,04 (0,15)	0,17 (0,16)
Rural areas (BE) ($\boldsymbol{\beta_{R_B}}$)	−0,19 (0,11)	−0,16 (0,16)	−0,58** (0,25)	−0,47** (0,22)	0,14 (0,20)	−0,22 (0,16)
Pop. density (FR) ($\boldsymbol{\beta_{D_F}}$)	−0,09 (0,07)	−0,02 (0,07)	0,11 (0,07)	0,05 (0,08)	0,02 (0,07)	0,07 (0,08)
Pop. density (BE) ($\boldsymbol{\beta_{D_B}}$)	**−0,16**** (0,07)	**−0,20**** (0,08)	**−0,29***** (0,09)	**−0,24**** (0,10)	−0,16 (0,10)	−0,09 (0,08)
Proximity to Luxembourg ($\boldsymbol{\beta_{LUX}}$)	**−0,12***** (0,03)	**−0,44***** (0,16)	0,03 (0,07)	**−0,15***** (0,06)	−0,13 (0,10)	**−0,38**** (0,17)

Notes: *, ** and *** indicate statistical significance at the 0.10, 0.05 and 0.01 level respectively. Standard errors, clustered at the arrondissement level, are in parenthesis.

or not to Luxembourg. The tax pass-through of stores close to Luxembourg is smaller for most products. The magnitude of these differences is quite similar to the one found in model (4.2), with just three of them being significant at the 0.05 level. (i.e., product A, B and F). These results suggest that, after controlling for possible differences in price changes in differently competitive areas in France (by means of population density), the competition effect and the border effect with Luxembourg remain significant.

However, stores in municipalities with different population density might not only differ in terms of the number of local competitors. They could also differ in terms of other demand and supply-side characteristics. For this reason, we re-estimate a different version of *model* (5) in which we substitute the number of competing retailers by population density at the municipality level. In this way, we will have a counterfactual for areas with different level of competition (proxied by population density), while also controlling for possible differences in local demand characteristics (proxied by GDP per capita and rural status). The model is as follows:

$$\begin{aligned} P_{it} = {} & \delta_i + \beta_2 T_t + \beta_3 (BE_i \times T_t) + \beta_{Y_F}(T_t \times \ln(Y)_i) \\ & + \beta_{Y_B}(BE_i \times T_t \times \ln(Y)_i) \\ & + \beta_{R_F}(T_t \times Rural_i) + \beta_{R_B}(BE_i \times T_t \times Rural_i) + \beta_{D_F}(T_t \times \ln(D)_i) \\ & + \beta_{D_B}(BE_i \times T_t \times \ln(D)_i) + \beta_{LUX}(BE_i \times T_t \times LUX_{Bi}) + \varepsilon_{it}. \end{aligned} \tag{6.2}$$

where $\ln(D)_i$ is the natural logarithm of population density at the municipality level. The β_{D_F} coefficient measures how price changes in France vary with population density after the reform. This can be interpreted as the counterfactual scenario for Belgian stores facing an increasing competitive pressure at the retail level. The β_{D_B} coefficient measures how the treatment varies with population density, once accounting for this counterfactual scenario. All other variables have the same interpretation as in *model* (5) and are needed to control for some possible differences in local demand characteristics that can influence tax shifting.

The results of *model* (6.2) are displayed in Table 9. For every product, the β_{D_F} coefficient is not significantly different from zero. This indicates that French prices did not change with population density after the reform. The β_{D_B} coefficients are instead all negatives and significant for most products (except for E and F). This suggests a lower tax shifting in more competitive areas (measured by population density). We also re-run the same model by including province-specific treatment effects in order to control for some potential unobservable factors at the province level. However, this does not affect our results.

We run another robustness check in order to verify our results about the border effect with Luxembourg. Although we recognize that this effect is not significant for every product, we would like to verify that the lower tax pass-through for some products in stores close to Luxembourg can be related to cross-border shopping motives. In order to do that, we re-estimate a different version of *model* (4.2) where we compute the tax.

pass-through of all stores that are within 50 km distance from the Luxembourg border (instead of those within a distance of 10 km).[17] The rationale behind this test is to check whether we still find a lower tax pass-through when increasing the distance to the border. If that is the case, then this is somehow concerning as the scope for cross-border shopping should decline with the distance from the border, suggesting that perhaps we are probably capturing some other regional effect. The result is that extending the distance to the border to 50 km eliminates the border effect in the sense that we do not find any significant difference in tax shifting between those stores within 50 km from the Luxembourg border and the other stores.

5.3. Timing of the tax pass-through

So far, we focused on the spatial dimension of the tax pass-through heterogeneity. We have implicitly assumed that the tax shift was uniform over the months after the tax reform. Yet, a tax reform could take some time before being shifted into retail prices and this shift could also vary overtime. Hence, we estimate a model that allows for leads and lags of the treatment effect. On the one hand, this strategy allows us to see how tax pass-through evolved overtime. On the other hand, the leads of the treatment allow testing formally the parallel trend assumption during the months before the tax hike. In particular, these need to be equal to zero, meaning that the spirit price in Belgium and France did not diverge before the tax reform. For each product, we estimate the following model:

$$P_{it} = \delta_i + \sum_{t=-3}^{4} \beta_{F_t} M_t + \sum_{t=-3}^{4} \beta_{Bt}(BE_i \times M_t) + \varepsilon_{it}. \tag{7}$$

The variable M_t is a dummy variable indicating the month t in which the price is observed. In total, there are eight months in our sample. From August until March. Three months before the tax reform and four months after, plus the month in which the reform is implemented. The month t is indexed such that the month in which the tax reform takes place, which is November, is equal to $t = 0$. In this way, we can refer to t as the number of months before or after the tax reform. We use the month before the tax reform $t = -1$ (October) as the reference month. The coefficients β_{F_t} measure price changes in France over the month before and after the reform with respect to the reference month. All the β_{F_t} with $t \geq 0$ represent the counterfactual scenarios for Belgian stores for each month after tax reform.

The main coefficients of interest in this model are the β_{Bt} coefficients, which measure the price change for each month before or after the tax reform with respect to the reference month (November). Each β_{B_t} with $t < 0$ are the leads of the treatment. In order to see whether the parallel trend assumption holds, these coefficients must be equal to zero. If not, this means that Belgian prices before the tax reform diverged

[17] All stores in this area have very few competitors. Therefore, their tax pass-through should tend to be on average larger than in areas with more competing stores.

Table 10
Time heterogeneity in the tax pass-through (model (7)).

	Product					
	A	B	C	D	E	F
3 months before ($\boldsymbol{\beta_{B_{-3}}}$)	**−0,08** (0,06)	**−0,02** (0,07)	**0,20*** (0,11)	**0,00** (0,06)	**0,14**** (0,07)	**0,12** (0,14)
2 months before ($\boldsymbol{\beta_{B_{-2}}}$)	**0,07** (0,07)	**−0,02** (0,07)	**0,10** (0,09)	**−0,06** (0,06)	**0,09** (0,07)	**0,09** (0,14)
Month of the reform ($\boldsymbol{\beta_{B_0}}$)	**3,00***** (0,04)	**2,72***** (0,04)	**2,63***** (0,05)	**2,63***** (0,09)	**2,64***** (0,10)	**3,10***** (0,08)
1 month after ($\boldsymbol{\beta_1}$)	**2,98***** (0,05)	**2,72***** (0,04)	**2,91***** (0,09)	**2,46***** (0,12)	**2,39***** (0,14)	**2,84***** (0,09)
2 months after ($\boldsymbol{\beta_{B_2}}$)	**2,98***** (0,06)	**2,53***** (0,09)	**2,89***** (0,11)	**2,44***** (0,13)	**2,53***** (0,12)	**3,10***** (0,09)
3 months after ($\boldsymbol{\beta_{B_3}}$)	**3,59***** (0,09)	**2,61***** (0,10)	**2,92***** (0,10)	**2,73***** (0,12)	**2,66***** (0,13)	**3,45***** (0,11)
4 months after ($\boldsymbol{\beta_{B_4}}$)	**3,69***** (0,08)	**2,69***** (0,10)	**3,14***** (0,20)	**2,87***** (0,10)	**2,89***** (0,13)	**3,50***** (0,11)
Test on the equality of coefficients ($\boldsymbol{H_0: \beta_{B_0} = \beta_{B_4}}$)						
F value	49,13	0,12	6,03	4,06	3,58	91,17
p-value	<0,01	0,73	0,02	0,05	0,06	<0,01

Notes: *, ** and *** indicate statistical significance at the 0.10, 0.05 and 0.01 level respectively. Standard errors, clustered at the arrondissement level, are in parenthesis. The table displays only the betas coefficients for Belgium. The last two rows show the results of the Wald test on the equality of the coefficients for the month of tax reform (β_{B_0}) and 4 months after (β_{B_4}), where the null hypothesis is $H_0 : \beta_{B_0} = \beta_{B_4}$. The month before the tax reform $t = -1$ (October) is used as the reference month.

from the French prices and hence we would reject France as being a good control group for Belgium. Yet, our time window before the tax reform is quite narrow, since we can just observe three months before the reform. Each β_{B_t} with $t \geq 0$ measure instead the tax pass-through in the treated group for every month after the tax reform. For instance, β_{B_0} is the tax pass-through during the month of the reform, while β_{B_2} is the tax pass-through two months after the reform. Our empirical test consists in checking whether these effects are statistically different overtime. Table 9 shows the results of this estimation.

Although we have already checked for the pre-treatment trend graphically in Section 3, the results of *model* (7) can be quite useful to test the hypothesis of parallel trend before the tax reform. This is shown in Table 10. The coefficients measuring the leads of the treatment are not statistically different from zero (with the exception of lead $\beta_{B_{-3}}$ for product E). This indicates that spirit prices in French stores did not diverge from those in Belgium in the three months before the tax reform. The coefficients for the treatment lags indicate that the tax pass-through did generally increase over time after the tax reform. The test on the equality of the tax pass-through one month later and four month later indicates significant difference for four products out of six. Yet, during the first month of tax reform, the tax hike was overshifted with a confidence level of 95%. This is shown in Table 11, which displays the tax pass-through for the month of November and March. Which are the first and last month of price observations after the tax reform. We refer to them as short-run and long-run tax pass-through, respectively.

Accounting for timing in tax pass-through can also provide more insights on the competition and the border effects. So far, the analysis of the border and competition effects was carried out by averaging price changes at the store level over the months following the tax reform. The risk is to confound a lower tax shift in more competitive areas with a simple delay in the tax shift needed for those stores to see how competitors react to the reform. The same argument could apply for the border effect, with the stores close to the border waiting to see the effect of the tax reform on cross-border shopping. To test for different timing in the competition and border effect, we estimate a model that accounts for both spatial and time variations in tax shifting. Following *model* (4.2), we specify this model as follows:

$$\begin{aligned} P_{it} = \delta_i &+ \sum_{t=-3}^{4} \beta_{F_t} M_t + \sum_{t=-3}^{4} \beta_{L_t} (BE_i \times M_t \times Low_{Comp_i} \times NoLUX_{B_i}) \\ &+ \sum_{t=-3}^{4} \beta_{LUX_t} (BE_i \times M_t \times Low_{Comp_i} \times LUX_{B_i}) \\ &+ \sum_{t=-3}^{4} \beta_{M_t} (BE_i \times M_t \times Med_{Comp_i}) \\ &+ \sum_{t=-3}^{4} \beta_{H_t} \left(BE_i \times M_t \times High_{Comp_i} \right) + \varepsilon_{it}. \end{aligned} \tag{8}$$

Model (8) is a combination of *model* (4.2) and *model* (7). Each beta coefficient with $t \geq 0$ provides a measure of how tax shifting evolved in areas with different level of competition. This allows us to check whether the "competition effect" on the tax shift is temporary or persistent over the first five months of tax reform. Table 12 shows the change in the tax shifting difference between high- and low-competition areas for each month after the tax reform. The tax shift difference is computed as the difference between the treatment coefficient in high-competition areas β_{H_t} and the treatment coefficient in low-competition areas β_{L_t}.

As shown in Table 12, the tax shifting difference between high- and low-competition areas becomes statistically significant for all products (except F) two months after the tax reform and it is persistent four months later. The tax shift in high- and low-competition areas was initially comparable for product B, D and E. Then they start diverging two months later, with the tax shifting in high-competition areas being around 0,70 € lower than in low-competition areas. This suggests that it took two months before stores adjusted prices in order to account for the competition. For product A and C instead, such difference is already significant during the first month of tax reform. Thus indicating

Table 11
Short-run vs long-run tax pass-through rate.

	Product					
	A	B	C	D	E	F
November	**123%**	**119%**	**115%**	**108%**	**116%**	**128%**
C.I.	121–126	116–123	111–120	101–116	107–125	121–134
March	**152%**	**118%**	**138%**	**118%**	**127%**	**144%**
C.I.	145–159	110–126	120–155	109–127	115–138	135–153

Notes: C.I. is the 95% confidence interval of the tax pass-through for each product. The tax pass-though is computed with the estimates of *model* (7). November is the first month of tax reform. This row shows the tax pass-through in the short-run. March is the last month of price observation. This row shows the tax pass-through in the long-run.

Table 12
Timing of the competition effect (model (8)).

Product	Competition effect: $(\beta_{H_t} - \beta_{L_t})$				
	November ($t = 0$)	December ($t = 1$)	January ($t = 2$)	February ($t = 3$)	March ($t = 4$)
A	**−0,05**** (0,02)	**−0,12**** (0,05)	**−0,15**** (0,06)	**−0,18***** (0,07)	**−0,06**** (0,03)
B	−0,03 (0,03)	−0,30* (0,15)	**−0,76***** (0,11)	**−0,70***** (0,13)	**−0,74***** (0,14)
C	**−0,31***** (0,05)	**−0,31***** (0,05)	**−0,31***** (0,05)	**−0,30***** (0,05)	**−0,34***** (0,06)
D	−0,10* (0,06)	−0,10* (0,06)	**−0,73***** (0,14)	**−0,72***** (0,14)	**−0,76***** (0,16)
E	−0,04 (0,07)	−0,37* (0,20)	**−0,65***** (0,18)	**−0,37**** (0,18)	**−0,62**** (0,20)
F	0,00 (0,00)	−0,17* (0,10)	−0,02 (0,03)	−0,02 (0,03)	0,01 (0,02)

Notes: The table shows the results of $\beta_{H_t} - \beta_{L_t}$ for each month after the tax reform as estimated in *model* (8). The standard errors of this difference are in parenthesis. *, ** and *** indicate statistical significance at the 0.10, 0.05 and 0.01 level respectively.

Table 13
Timing of the border effect (model (8)).

Product	Border effect: $(\beta_{L_t} - \beta_{LUX_t})$				
	November ($t = 0$)	December ($t = 1$)	January ($t = 2$)	February ($t = 3$)	March ($t = 4$)
A	**−0,09***** (0,02)	**−0,07***** (0,02)	**−0,10***** (0,03)	−0,02 (0,02)	**−0,04***** (0,01)
B	−0,03 (0,03)	−0,02 (0,01)	−0,01 (0,03)	**−0,93**** (0,42)	**−0,97**** (0,45)
C	0,02 (0,01)	0,02 (0,01)	0,18*** (0,06)	0,17** (0,07)	0,14* (0,07)
D	0,01 (0,01)	−0,01 (0,01)	−0,36* (0,19)	0,05 (0,05)	−0,02 (0,06)
E	0,04 (0,02)	0,13* (0,07)	0,12* (0,07)	−0,28 (0,21)	**−0,78***** (0,21)
F	0,00 (0,00)	0,27*** (0,07)	**−0,42**** (0,20)	**−0,74**** (0,35)	**−0,81**** (0,39)

Notes: The table shows the results of $\beta_{L_t} - \beta_{LUX_t}$ for each month after the tax reform as estimated in *model* (8). The standard errors of this difference are in parenthesis. *, ** and *** indicate statistical significance at the 0.10, 0.05 and 0.01 level respectively.

that prices in low and high-competition areas diverged immediately after the tax reform. The results reject the hypothesis that stores facing more competitors tend to delay the tax shift waiting to see how competitors react. Indeed, if that was true we would observe a "front loaded" tax shift difference. Conversely, we find a "back-loaded" tax shift difference with the stores in both low and highly competition areas reacting first similarly to the reform and then progressively the competitive pressure introduced some differential adjustment in the tax shifting.

The estimates of *model* (8) also allow exploring the time dynamics of the tax shifting for stores at the border of Luxembourg. Table 13 displays the timing of the border effect. Interestingly, the table reveals that the border effect on the tax shift appears with some lag (three months after the reform). The tax shift of product B and F was considerably lower in stores close to Luxembourg inducing a price difference between 0,70 € and 1 €. The same timing arises for product E but only four months after the reform, with a price difference of 0,78 €. Conversely, for product A we find a persistent but negligible difference in tax shifting overtime. These results highlight that it took some time before stores close to Luxembourg adjusted prices differently.[18] A possible explanation could be some demand smoothing during the reform with consumers anticipating the reform by stockpiling spirits just before the tax hike. That is, the demand response to the tax hike was postponed for a few months, once the consumers' inventories were over. We confirm the existence of stockpiling in the next section where we study the impact of the tax reform on the quantity of spirits sold in these stores. To check for the robustness of these results, we also estimated *model* (8) using population density as a proxy for competition (as in model (6.1)). The results are consistent with the findings of *model* (8).

We also use *model* (8) to test for the parallel pre-treatment trend at the competition subgroup level. The reference month in *model* (8) is the month before the tax reform ($t = -1$). Each β_{F_t} with $t < -1$ measures the difference in French prices between the reference month and each of its previous month. All other betas with $t < -1$ are the leads of the treatment effect. They indicate how Belgian prices of different competition subgroups differ from this average French price for every month prior the tax reform. To check whether the assumption of parallel pre-treatment trend at the subgroup level holds, it suffices to check whether these leads are not significantly different from zero for every subgroup of stores. This means that the price evolution of these subgroups is parallel to the control group (average French price) and hence parallel to each other. Table A.2 in the appendix shows the leads of the treatment for different degrees of local competition and proximity to Luxembourg. As shown in Table A.2, spirit prices did not diverge in the pre-reform period across different subgroups, with treatment leads being close to zero and not significant.[19]

6. The impact on the quantity of spirits sold

In this section, we study the effect of the tax reform on the quantity of spirits sold in the retail chain under consideration. As the tax shifting was substantially heterogeneous over the country, the quantity response to such policy may also vary across store locations. Furthermore, the significant tax shift in areas close to the border also suggests that a great part of domestic sales could have been lost by cross-border shopping. In order to test for these hypotheses, we analyze the number of bottles of spirits that were sold in stores of our retail chain during the period of tax reform. The products we consider are the same six brands analyzed for the tax pass-through estimation. Interestingly, as this retail chain also controls some stores located in the Grand Duchy of Luxembourg, we also have quantity data for stores located on the other side of the border. This allows us to test directly for cross-border sales spillover.

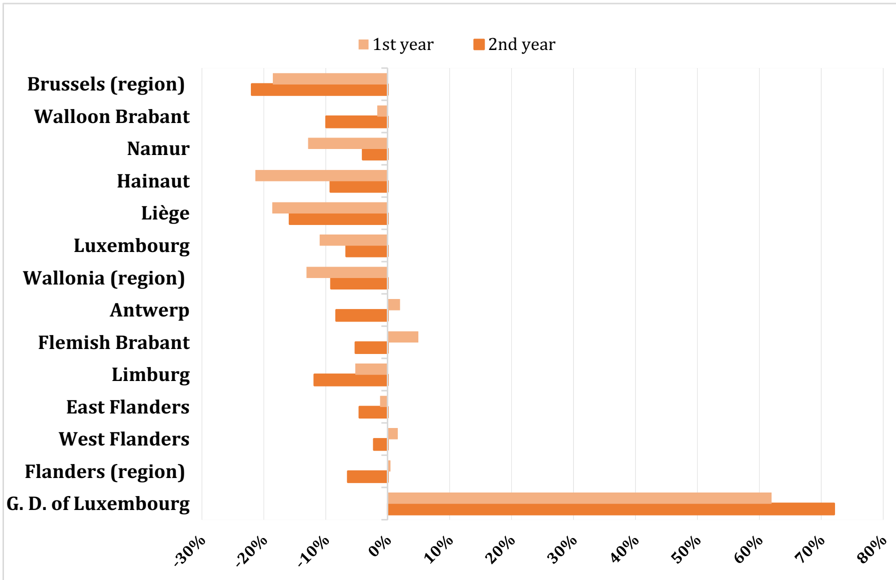
Table 14 shows the yearly percentage change in the quantity of bottles sold in each Belgian province (including the Luxembourg province) and in the Grand Duchy of Luxembourg (the country). Overall in Belgium, during the first year of the tax reform (November 2015–September 2016), spirit sales have declined by 8,51% with respect to the same period in the previous year. Interestingly, sales have continued to drop the year afterwards by 9,25% with respect to the first year of tax reform.[20] The reduction in sales seems quite heterogeneous across provinces. One year after the reform, the sales of spirits in stores located

[18] We also estimated a time-varying version of *model 4.1* in order to study the possible timing-varying "border effect" for all the neighboring countries. Yet, we did not find any significant "border effect" apart for Luxembourg.

[19] Few leads are positive for product C and E, but only for the month of August. This can be due to some temporary shock for some stores during that month.

[20] As the tax change was announced in October 2015 (one month before the tax reform), this month is excluded from the computation to remove the possible effect of stockpiling during that period.

Table 14
Yearly % change in the quantity of spirits sold after tax reform.



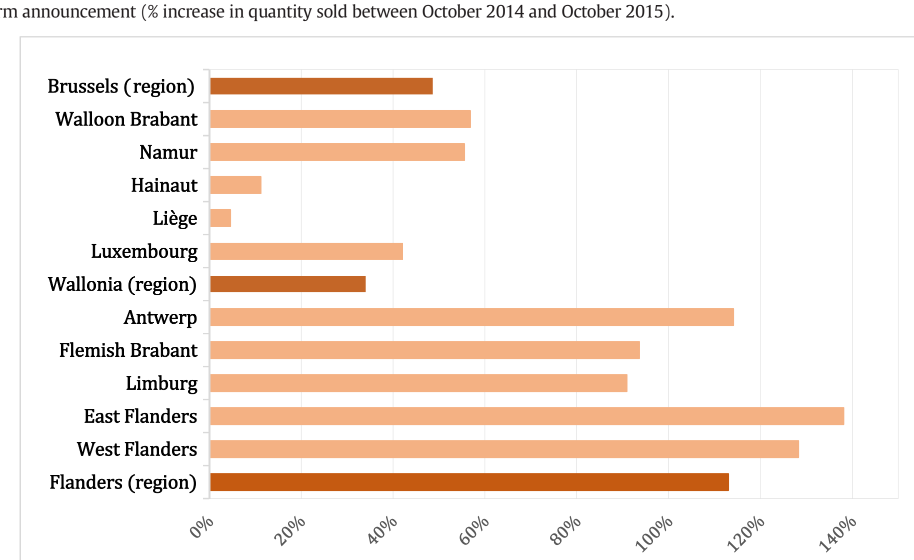
in G.D Luxembourg (the country) have increased by nearly 62% with respect to the previous year. The second year after the reform those sales have continued rising by 72% as compared to the first year of tax reform. These figures suggest massive cross-border shopping of Belgian households in this neighboring country.

To test whether Belgian consumers have anticipated the tax hike by stockpiling spirits, we compare the number of bottles of spirit sold in October 2015 with that of October 2014. The results are shown in Table 15. Interestingly, we find an increase of nearly 80% in the quantity of spirits sold, which suggests stockpiling in response to the tax announcement in October 2015. If such stockpiling is not properly taken into account when evaluating ex-ante the impact of a tax policy, this can lead to overestimating the tax effect on consumer demand (Wang, 2015).

As these figures are limited to only one retail chain, it is not sure whether the tax reform has led some consumer to switch from one retail chain to another. Some evidence of this can be found by looking at the evolution of spirit sales in the provinces of Flemish Brabant, Antwerp and West Flanders during the first year of the reform. In these provinces, stockpiling was greater than average and demand had slightly increased compared to the previous year. Suggesting a possible shift of consumers from other chains and thus an increase in the market share of the chain under consideration. Another possible reason is the lack of alternative as compared to the rest of the country. Indeed, all these provinces are located in the north of the country and share a border with the Netherlands, which is the only neighboring country with similar spirit prices after the tax reform. Conversely, provinces located more in the south (Region of Wallonia), which share borders with countries having lower spirit prices (notably Luxembourg), experienced both a greater drop in demand and a lower spirit stockpiling compared to the average. This can suggest that consumers that have access to cross-border option started purchasing spirits in Luxembourg after the tax reform. Evidence on the evolution of sales in Luxembourg clearly supports this hypothesis.

Since we do not control for any confounding factors that might have occurred during the years after the reform and uses data from just one retail chain of retailers, these figures cannot be interpreted as the causal impact of this tax reform on the volume of sales. Yet, this analysis clearly suggests the presence of stockpiling and the heterogeneous changes in sales across provinces after the tax reform. Moreover, the quantity

Table 15
Stockpiling after the tax reform announcement (% increase in quantity sold between October 2014 and October 2015).



analysis also reveals a strong positive spillover effect of the tax increase on sales in the neighboring country with the lowest spirit prices (Luxembourg), making the case for cross-border shopping.

7. Conclusions

The results of this analysis have shown that the alcohol tax reform implemented in Belgium in November 2015 was mostly over-shifted to the retail price of six major brands of spirit. These products reacted very quickly to the tax reform by adapting their retail prices already during the first month of its implementation. Results also indicate that the tax incidence was substantially heterogeneous both across spirits and over the country. In particular, the intensity of competition is found to be significantly correlated to the extent of tax shifting. The higher the number of retailers in the area, the lower the tax shift. Conversely, proximity to the French, Dutch and German border does not seem to affect the tax shifting even though the tax reform has considerably increased the relative price of Belgian spirits with respect to these countries. Yet, we do find a quite smaller tax shift for some products in stores close to Luxembourg which is the country having the lowest spirit prices both before and after the tax reform. We have also shown that the tax pass-through varies over time, and that the border and the competition effects are back-loaded in the sense that they progressively show up several months after the reform.

In a public health perspective, our findings suggest that the health benefits associated with the tax reform will have a differential impact on Belgian households according to where they live. To support this hypothesis further, we analyze the evolution of spirit sales in the stores considered before and after the reform and provide evidence of a heterogeneous variation of spirit sales over Belgian provinces. We also find evidence of spirit stockpiling before the tax reform and a substantial rise of spirit sales in Luxembourg, which suggests effective cross-border shopping of spirits by Belgian consumers.

Appendix A. Appendix

Table A.1
Spatial price dispersion.

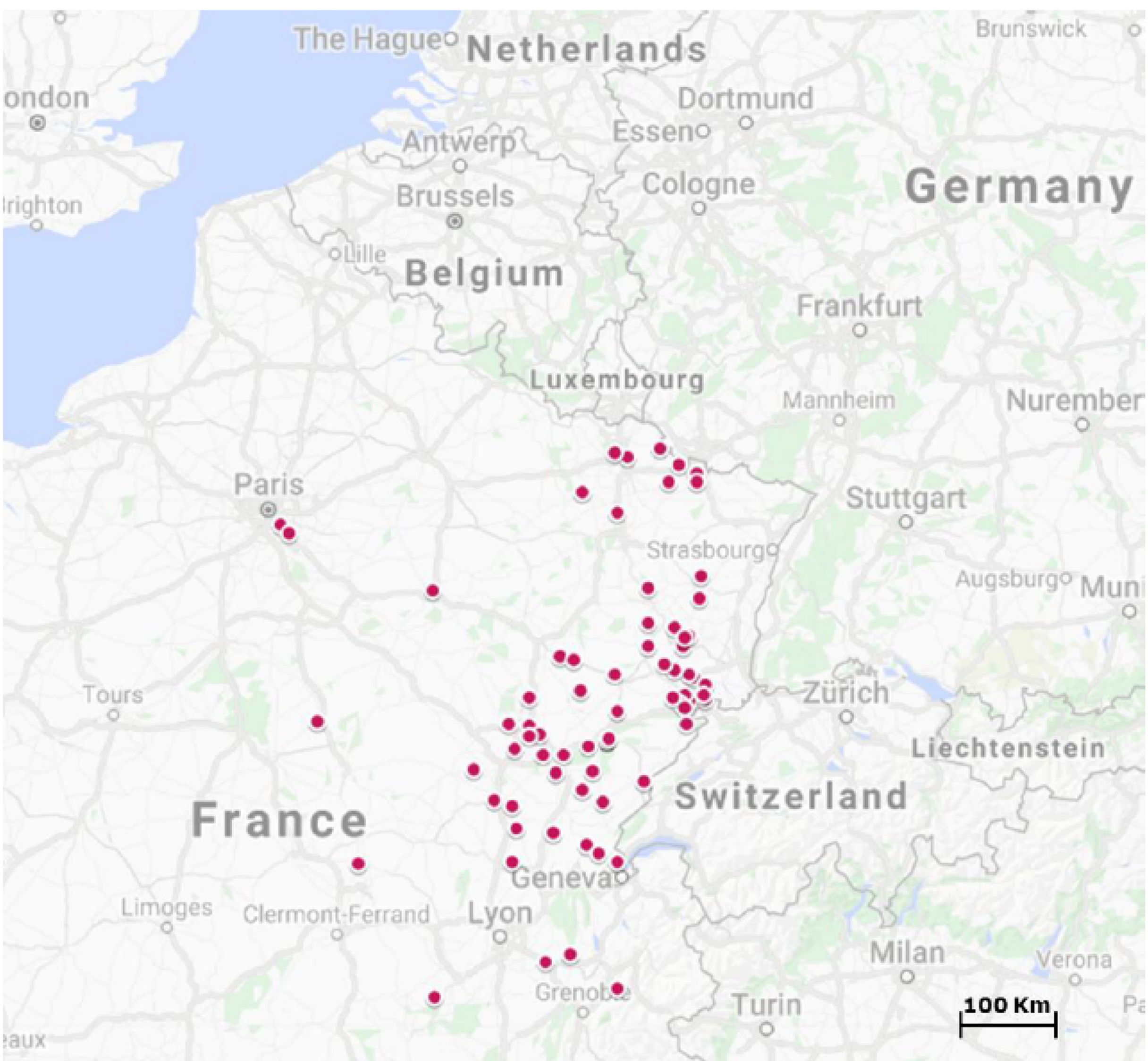


Fig. A.1. Location of French stores (control group).

Product	Average price		Standard deviation		Levene's test (homogeneity of σ^2)	
	Pre-reform	Post-reform	Pre-reform	Post-reform	F value	p-value
Belgium						
A	15,59	18,79	0,04	0,21	272,06	<0,01
B	11,27	13,86	0,13	0,46	177,12	<0,01
C	10,78	13,44	0,34	0,68	67,45	<0,01
D	13,52	16,27	0,18	0,51	131,42	<0,01
E	15,88	18,36	0,20	0,68	385,33	<0,01
F	15,02	17,98	0,12	0,14	0,70	0,40
France						
A	16,31	16,21	0,56	0,50	2,90	0,09
B	11,77	11,68	0,28	0,33	0,01	0,93
C	12,88	12,74	0,51	0,42	1,55	0,22
D	14,36	14,46	0,55	0,48	0,01	0,93
E	15,03	14,97	0,59	0,57	0,53	0,47
F	14,80	14,63	0,64	0,52	4,96	0,03

Notes: The sample is divided in two groups: Belgium (treated) and France (control). The second column shows the average product price for both groups before and after the tax reform. The third column displays the standard deviation of store prices from the average price before and after the tax reform. The last column shows the results of the Levene's test on the homogeneity of price variance between the pre- and post-reform period. The null hypothesis of equal variances between the two periods ($H_0 : \sigma^2_{PRE} = \sigma^2_{POST}$) is rejected for all products in the treated group (except for F), while it is accepted for all products in the control group (except for F).

Table A.2
Pre-treatment trend by subgroups of stores (model (8)).

		Product					
		A	B	C	D	E	F
3 months before reform	Low competition ($\beta_{L_{-3}}$)	−0,08 (0,06)	−0,03 (0,07)	0,06 (0,10)	−0,01 (0,06)	0,14* (0,07)	0,15 (0,14)
	Medium competition ($\beta_{M_{-3}}$)	−0,07 (0,06)	−0,02 (0,07)	0,07 (0,10)	−0,01 (0,06)	0,17** (0,07)	0,12 (0,14)
	High competition ($\beta_{H_{-3}}$)	−0,08 (0,06)	−0,01 (0,07)	0,57*** (0,13)	−0,01 (0,06)	0,10* (0,09)	0,09 (0,14)
	Proximity to Luxembourg ($\beta_{LUX_{-3}}$)	−0,08 (0,06)	−0,02 (0,07)	0,13 (0,09)	−0,01 (0,06)	0,18*** (0,09)	0,22 (0,15)
2 months before reform	Low competition ($\beta_{L_{-2}}$)	0,07 (0,07)	−0,03 (0,07)	0,13 (0,09)	−0,06 (0,06)	0,09 (0,07)	0,12 (0,14)
	Medium competition ($\beta_{M_{-2}}$)	0,07 (0,07)	−0,03 (0,07)	0,12 (0,09)	−0,06 (0,06)	0,09 (0,07)	0,08 (0,14)
	High competition ($\beta_{H_{-2}}$)	0,07 (0,07)	−0,02 (0,07)	0,04 (0,09)	−0,04 (0,06)	0,07 (0,08)	0,06 (0,14)
	Proximity to Luxembourg ($\beta_{LUX_{-2}}$)	0,07 (0,07)	−0,03 (0,07)	0,13 (0,09)	−0,06 (0,06)	0,10 (0,07)	0,19 (0,15)

Notes: *, ** and *** indicate statistical significance at the 0.10, 0.05 and 0.01 level respectively. Standard errors, clustered at the arrondissement level, are in parenthesis.

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