Prices, Quantities, and Cross-Border Shopping: Evidence from Argentina's Gasoline Market

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Abstract

This paper discusses the limitations of a standard test for cross-border shopping (CBS): price convergence at the border. Using a novel station-level panel of prices and quantities for the universe of Argentine gasoline stations, I exploit a 2015 tax reform that expanded a tax-exempt zone and cut the applicable excise from 55.8% to 0%—a roughly 30% reduction in retail prices under full pass-through. Quantities sold rose 22% at exempted stations and fell 7% at non-exempted stations just across the new border, with effects concentrated close to the border. Yet pass-through is spatially uniform within each region, indicating no price convergence despite substantial CBS. Seven years later, firms on the high-tax side of the border experience a 10-percentage-point decline in survival probability relative to exempted stations.

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1 Introduction

Geographic borders create discontinuous changes in tax rates, which generate incentives for consumers to strategically travel across jurisdictions in order to benefit from lower prices. This behavior—commonly referred to as cross-border shopping (CBS)—has long been recognized in the economic literature, given its broad implications for public policy, and in particular for tax policy design. By enabling consumers to avoid higher-tax jurisdictions, CBS distorts tax collection by eroding the tax base, especially for local governments (Agrawal, 2012; Kanbur & Keen, 1993), and weakens the effectiveness of policies aimed at addressing externalities and market power (Christiansen, 1994, 2003). Beyond its fiscal implications, CBS has broader effects on local economies, influencing firms' behavior, employment, and welfare (Baggs et al., 2016; Burstein et al., 2024; Campbell & Lapham, 2004).

Measuring the extent to which cross-border shopping (CBS) takes place is not trivial. Conceptually, CBS is an equilibrium outcome of a game that internalizes consumer, firm, and government responses (Agrawal, 2012; Christiansen, 2003; Kanbur & Keen, 1993). Much of the existing literature therefore exploits exogenous geographic variation or policy discontinuities to study how agents respond to changes in CBS conditions. Empirically, however, direct measures of cross-border consumption are rarely observed, leaving many studies to rely on an indirect test for CBS based on price convergence at jurisdictional borders. Following a tax increase in one region, reallocation of consumer demand toward a neighboring lower-tax jurisdiction should raise prices on the low-tax side and reduce them on the high-tax side. The spatial extent over which such price adjustments occur is informative about the geographic reach of CBS. Conversely, the absence of price convergence is often interpreted as evidence that existing price differentials are insufficient to offset travel costs, and thus that CBS is limited.

Using a novel dataset that combines station-level price and quantity data for the universe of gasoline stations in Argentina, this paper exploits an exogenous regional tax exemption that generated a sharp geographic discontinuity in gasoline excise taxes. The reform, enacted in November 2015 and effective December 1, expanded a tax-exempt area within southern Argentina, reducing the applicable tax rate from 55.8% to 0% in newly covered regions, corresponding to a substantial decline in final prices of approximately 30%. The exogeneity and magnitude of this policy change allow me to study how consumers and firms adjust to changes in cross-border shopping conditions, as well as the longer-run effects on the gasoline market structure.

To empirically investigate the immediate effects of this tax exemption, I construct a unique, geolocated monthly dataset at the gas station level containing average retail prices and total gasoline sales, based on administrative records from the Argentine Ministry of Energy. I divide the analysis into three parts. First, I examine firms' pricing responses using a difference-in-differences approach that exploits variation across locations relative to the tax border. I compare gas stations located south of the border, which benefited from

the tax exemption, and stations located just north of the border, which were not directly affected by the reform, to a control group of stations located far from the border and thus plausibly unaffected by the policy change. I find that prices fell by 21.6% immediately and permanently following the tax reform, while northern stations near the border experienced little to no adjustment in prices. Moreover, I find that price responses remain largely homogeneous within each region, regardless of distance from the border, providing no evidence of price convergence.

Second, I examine quantity responses to assess whether price convergence at the border is a necessary condition for the presence of cross-border shopping. I find that demand in the south increased by 22.5%, while northern stations experienced a decline of 7.4%. Effects diminish as stations are located farther from the border, consistent with consumer reallocation across jurisdictions. These findings suggest that relying solely on price convergence may fail to detect and measure the extent of CBS, even when substantial cross-border reallocation of demand is present.

Third, I examine the medium- to long-run effects of CBS on the number of firms in equilibrium, focusing on the exit rates of firms after the shock. I find that, consistent with the short-run results, the probability of remaining in the market rises for firms located in the tax-exempt area and falls for firms in the non-exempted area, relative to the control group. In particular, seven years after the tax change, the probability of survival increases by 5 percentage points for the southern group and decreases by 5 percentage points for the northern group relative to the control area. The results indicate persistent effects of the tax-induced demand reallocation on local market structure.

This paper contributes to the literature on cross-border shopping by studying how changes in CBS conditions affect regional price differentials and induce consumers to reallocate purchases across jurisdictions. Existing studies have typically measured CBS either using aggregated data on consumption (Asplund et al., 2007; Banfi et al., 2005; Burstein et al., 2024; Campbell & Lapham, 2004; Chandra et al., 2014; Jansen & Jonker, 2018; Teixidó et al., 2024) or by relying on price convergence at jurisdictional borders as a test for CBS (Bajo-Buenestado & Borrella-Mas, 2019; Coyne, 2017; Doyle & Samphantharak, 2008; Hindriks & Serse, 2019; Louis & Taylor, 2016; Tsvetanov, 2024). This paper provides direct evidence of CBS by observing both margins at once: prices and quantities for every gas station on both sides of a tax border that did not exist before the reform. This allows me to measure not only the extent of cross-border shopping but also firms' pricing responses to tax-induced demand reallocation. Two related studies are Harding et al. (2012), which uses Nielsen transaction data on cigarettes and finds both lower tax pass-through near state borders and a higher probability of out-of-state purchases after tax increases, and Friberg et al. (2022), which documents a non-monotonic effect of distance on quantities in response to exchange-rate changes in grocery stores along the Norwegian–Swedish border. Importantly, the results show that substantial CBS can arise even in the absence of differential pricing responses by firms across locations. More generally, the paper sheds light on the limitations of relying on heterogeneity in tax pass-through as evidence of the effects of

taxation on firm and market outcomes.[1]

This paper also contributes to the literature on the long-run consequences of cross-border shopping for firm behavior and market structure. A large body of work studies how tax incentives around geographic borders shape firms' location decisions (Bartik, 1985; Holmes, 1998; Peltzman, 2016; Rohlin et al., 2014). Related studies that analyze the long-run equilibrium effects of CBS typically rely on cross-sectional variation that holds arbitrage conditions fixed (Agrawal, 2015; Chiou & Muehlegger, 2008; Manuszak & Moul, 2009) or on temporary shocks, such as exchange-rate fluctuations, to generate changes in cross-border incentives (Baggs et al., 2016; Campbell & Lapham, 2004). By exploiting a large and sustained tax reform that sharply altered CBS conditions, this paper provides the first causal evidence on how tax-induced changes in CBS conditions affect firm survival around jurisdictional borders.

The remainder of the paper is structured as follows. Section 2 provides institutional background on fuel taxation in Argentina, describes the 2015 TTLF tax exemption reform, and introduces the data. Section 3 presents the empirical methodology for the short-run results. Section 4 begins with the difference-in-differences estimates of average price responses on each side of the tax border, before turning to the heterogeneity analysis that examines how these effects vary with distance to the border and discusses the implications for the price-convergence test. Section 5 complements the price results by looking at the quantity data and discussing different models that can explain the results. Section 6 analyzes the long-run effects of the reform on the probability of survival for firms around the border. Section 7 presents a series of robustness checks, including heterogeneous effects by brand, the role of local competition, different distance definitions, and a dynamic event-study analysis that validates the parallel trends assumption and rules out anticipatory behavior. Finally, Section 8 concludes.

2 Institutional Background and Data

This section provides institutional background on fuel taxation in Argentina and the structure of the gasoline retail market. I describe the gas tax and the 2015 reform that expanded tax-exempt areas. I then introduce the datasets used in the analysis and present descriptive statistics.

2.1 Institutional Background

Argentina is a federal country with three levels of government: the national government; the subnational level, comprising 23 provinces and the Autonomous City of Buenos Aires (CABA); and more than 2,300 local governments. Historically, fuels have been primarily

[1] For instance, several papers exploit pass-through variation to infer market power, tax evasion, regulatory constraints, or distributional incidence (Benzarti et al., 2024; Genakos & Pagliero, 2022; Kopczuk et al., 2016; Stolper, 2024).

taxed through a national excise tax known as the Tax on the Transfer of Liquid Fuels (TTLF). The TTLF includes an exemption zone in place since 1998, historically referred to as the *42nd parallel zone*. This zone covered the Patagonian regions in southern Argentina, specifically the provinces of Tierra del Fuego, Santa Cruz, and Chubut, as well as selected municipalities in Río Negro and Neuquén (see Figure 1, original border in blue). The rationale for this exemption was to promote economic activity in remote and sparsely populated regions characterized by higher transportation and logistics costs.

From 2003 until January 2015, the TTLF was levied at 62%; it was then reduced to 55.8%. In addition to the TTLF, gasoline was subject to other taxes during the period analyzed. Specifically, it was subject to two additional national taxes: (i) a value-added tax (VAT) of 21%; and (ii) an additional excise tax of 5% earmarked for financing the Hydric Infrastructure Fund.[2]

2.2 The 2015 TTLF Tax Exemption Reform

On November 25, 2015, the exemption zone was extended to include three entire provinces—Río Negro, Neuquén, and La Pampa—as well as two municipalities in additional provinces: Patagones, located in the south of Buenos Aires Province, and Malargüe, in Mendoza Province (see Figure 1, new border in red). As a result, gasoline stations located south of the newly defined border benefited from a full exemption from the TTLF. This reform reduced the applicable TTLF rate from 55.8% to 0%, which, under full pass-through to consumers, would imply a decline in the final price of -30.7%.[3]

A notable feature of this tax reform is that it was not driven by contemporaneous market conditions. After more than twelve years in power, the incumbent government lost the presidential election on November 22, 2015. In the weeks preceding the transfer of power on December 10, 2015, the outgoing administration enacted a series of laws and regulations aimed at constraining future revenue streams available to the incoming government.[4] The expansion of the TTLF exemption zone was part of this broader set of fiscal measures. Importantly, the reform was implemented immediately: three days after the election, the bill—previously approved by Congress—was enacted by the federal administration and took effect on December 1, 2015. Table A.1 summarizes the legislative timeline of the tax change.

[2]To the best of my knowledge, there were no other excise taxes on gasoline at the state/province level. At the local level, some counties levy a tax on gasoline consumption, but it is small relative to the national tax (Porto et al., 2019).

[3]The change in the tax-inclusive price includes the VAT and the Hydric Infrastructure Fund tax mentioned above and is calculated as follows: $\left(\frac{1+0.21+0+0.05}{1+0.21+0.558+0.05}-1\right)\times 100$.

[4]For related evidence, see contemporaneous reforms to the pension system and changes to the redistribution of federal funds to provinces, like the one published in this *link*.

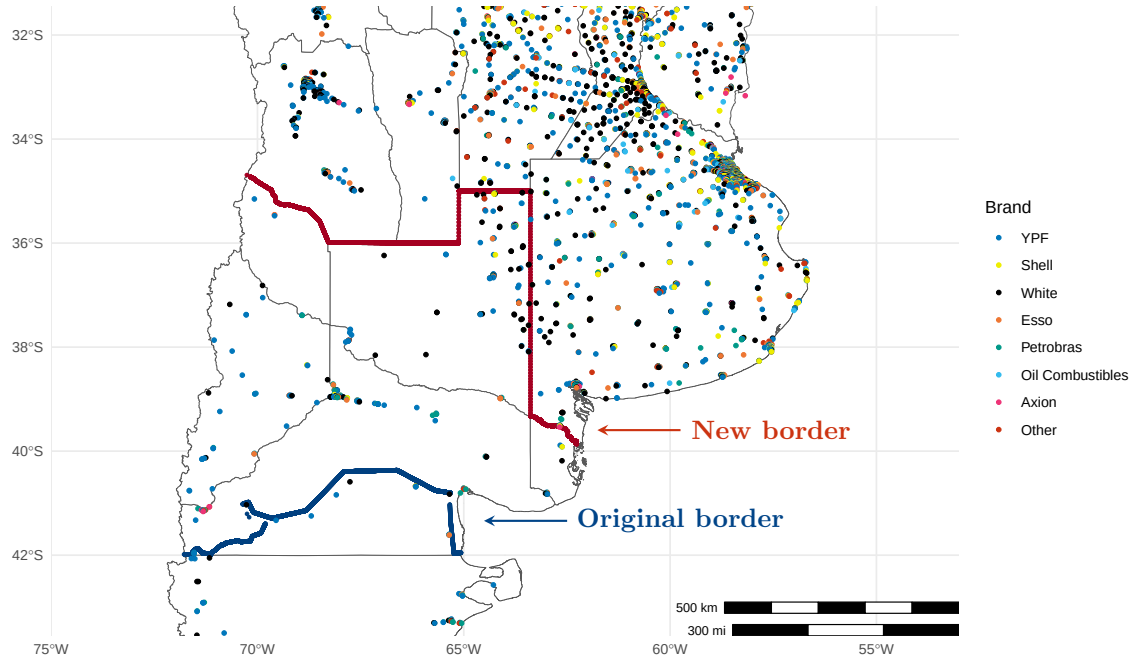


Figure 1: The 2015 TTLF Tax Exemption Reform

Note: Dots represent the locations of gasoline stations. Colors indicate station brand. The blue line denotes the original TTLF exemption boundary ("42nd parallel zone"), while the red line denotes the expanded exemption boundary implemented in December 2015.

2.3 Market Structure and Pricing

The oil industry value chain can be broadly divided into two main segments: upstream and downstream. The upstream segment comprises exploration and production activities, while the downstream segment includes refining—the conversion of crude oil into fuels for energy consumption—distribution, and retail sales at the gas station level.[5] In Argentina, the refining sector is highly concentrated, with five firms accounting for more than 95% of total refining capacity. Among these, the state-owned company YPF controls over 50% of national refinery capacity.[6] After refining, fuel is transported to dispatch terminals, where it is stored before being delivered to gas stations by tanker trucks. Each major refinery operates its own delivery fleet, which constitutes the primary mode of fuel transportation in the country (Scheimberg, 2018).

At the retail level, Argentina has approximately 5,600 gas stations, over 85% of which

[5]Figure A.1 illustrates the structure of the oil value chain.

[6]The remaining capacity is held by Shell (16%), Axion (14%), Oil Combustibles (8%), and Petrobras (5%).

operate under a branded network. Major refining companies maintain extensive retail networks, either through direct ownership or through majority ownership stakes. Although regulation limits ownership to no more than 40% of stations operating under a given brand, contractual arrangements are asymmetric, allowing refineries to exert substantial influence over pricing policies and operational decisions (Scheimberg, 2018). The remaining stations operate under a *White Flags* arrangement, analogous to unbranded gas stations in the United States.

2.4 Data and Descriptive Statistics

The empirical analysis relies on a main dataset constructed using open administrative records from the Argentine Ministry of Energy. I am able to build a geolocated monthly panel containing retail fuel prices and total fuel sales for the universe of gasoline stations in Argentina for the period 2008–2022. The dataset reports the total quantity sold and the average monthly retail price for each fuel type, the brand affiliation of each gasoline station, and its geographic coordinates.[7]

I focus on regular and premium gasoline, as these are the fuels most consumed by final consumers, representing more than 60% of total quantities sold. As in the United States, the distinction between regular and premium gasoline primarily reflects differences in octane content, with premium gasoline having higher octane levels and a higher retail price. Other fuel types include diesel, kerosene, and liquefied petroleum gas (LPG). Diesel is widely used by trucks and agricultural machinery and is not subject to the TTLF when consumed as an intermediate input. Although the tax is collected at the pump, firms can later deduct it as an advance payment for other taxes. Kerosene and LPG represent a small share of total fuel retail sales.

Table 1 presents descriptive statistics for the universe of gas stations over the period 2008–2022. On average, each gas station appears for about 141 months ($\approx$ 12 years). Panel A shows that each gas station sells around 117.5 thousand liters per month of regular gas and 48.6 thousand liters of premium gas (equivalent to 31.0 and 12.8 thousand gallons, respectively). For reference, the California Annual Retail Fuel Outlet Report (CEC-A15) estimates that, on average, a gas station sold around 88 thousand gallons per month in 2023. Because of the inflationary context, prices are presented in real pesos of December 2015. On average, regular gas costs about 14.8 pesos per liter, equivalent to 4.9 dollars per gallon as of December 2015. Panel B in Table 1 describes the distribution of stations for the period. As noted in Section 2, the state-owned brand stands out both in the number of gas stations operating under its name and in market share, for both regular and premium gas, followed by *Shell*. The second-largest group is *White Flags*.

Station-level geographical coordinates are used in two ways. First, they allow me to measure proximity to the new tax border, by computing, for each station, the distance to the nearest competitor located on the opposite side of the border. Throughout the

[7] The price and quantity data are available at http://datos.energia.gob.ar.

Table 1: Descriptive Statistics: Complete Database

Panel A: Price and quantity

	Regular Gas		Premium Gas	
VARIABLES	Mean	(SD)	Mean	(SD)
Quantity (thousands liters)	117.5	(99.1)	48.6	(52.5)
Price (pesos/liter)	14.8	(1.3)	16.6	(1.2)

Panel B: Distribution Stations and market shares

BRAND	N	%	% Regular	% Premium
Axion	152	2.7	1.8	1.7
Esso	484	8.6	8.9	6.4
Oil Com.	313	5.6	3.8	2.0
Other	330	5.9	2.9	1.0
Petrobr	334	6.0	5.6	3.8
Shell	775	13.8	17.2	22.4
White	1,438	25.7	5.1	2.5
YPF	1,775	31.7	54.7	60.3
Total	5,601	100.0	100.0	100.0

Panel C: Number of nearby stations around

DISTANCE	Radius	Mean	(SD)	Radius	Mean	(SD)
Euclidean	1 km	4.92	(3.94)	5 km	29.36	(36.70)
Driving	1 km	2.12	(1.26)	5 km	17.43	(19.51)
Time	1 min	1.46	(0.76)	5 min	9.58	(8.31)

Descriptive statistics for the whole universe of gas stations. Period: 2008 –2022 . Panel A shows total quantity in thousands of liters. Price is the average retail price (tax-included) reported by the gas stations each month. Prices are in December 2015 pesos and converted into dollars using the December 2015 exchange rate (11.43 pesos per dollar). Quantities and prices are residualized from seasonal patterns. Panel B shows the distribution of stations and market shares for each brand (as of November 2015) and type of gas. The number of stations is the total number of stations in the period. The market share is the average percentage of quantities per brand over the whole period. Panel C shows the average number of competitors around each gas station. Distances are reported as Euclidean distance (computed with the *geodist* package—developed in Picard, 2010) and as driving distance (computed with the *osrmtime* package—developed in Huber and Rust, 2016—using the free OpenStreetMap OSRM software). 1 mile is equivalent to 1.6 km. Author's calculations based on Argentine Ministry of Energy data.

paper, each such distance is computed in two ways: (i) linear Euclidean distance using the *geodist* package—developed in Picard (2010); and (ii) route-based driving distance (kilometers along the road network and travel time in minutes) using the *osrmtime* package—developed in Huber and Rust (2016)—with the free OpenStreetMap OSRM software.[8] Figure A.2 summarizes how the distance to the nearest station on the other side of the border is distributed when computed as straight-line (Euclidean) separation versus route-based driving distance along the road network. Second, coordinates allow me to measure distance between stations in order to describe the local competitive environment. Panel C of Table 1 reports the average number of competitors within a given radius around each station, using the same Euclidean and route-based definitions.

The dataset is novel in several respects. First, it contains quantity information at the firm level. Papers that analyze CBS with quantity data generally rely on aggregated regional data (Asplund et al., 2007; Bajo-Buenestado & Borrella-Mas, 2019; Banfi et al., 2005; Teixidó et al., 2024) or consumer expenditure surveys (Burstein et al., 2024; Chandra et al., 2014). Second, the precise geolocation of each station allows for the calculation of the true distances to the nearest competitor, complementing previous papers using other proximity measures such as zip code centroids or population centers (Agrawal, 2015; Harding et al., 2012). Third, the dataset covers the universe of gasoline stations, complementing previous studies that rely on a subsample of firms—often collected manually (Manuszak & Moul, 2009) or through credit card data sources such as OPIS or GasBuddy (Coyne, 2017; Doyle & Samphantharak, 2008; Louis & Taylor, 2016; Tsvetanov, 2024). Finally, the dataset spans a long period of time, making it possible to observe firm entry and exit directly.

3 Methodology

This section presents the identification strategy to analyze the short-run responses to the policy. I first present the average difference-in-differences approach and the identifying assumption. Second, I present the heterogeneity analysis. Finally, I discuss further potential validity concerns.

3.1 Average Effects

The strategy to study the effects of the tax change in the area around the border consists of employing a difference-in-differences approach. However, instead of using two groups (treatment and control), I will define three: a control group composed of gasoline stations located far from the border—assumed to be unaffected by the tax exemption—and two

[8] The package has advantages over those that use online route distance services. First, georeferenced data cannot always be used online due to sensitivity issues. Additionally, offline routines using free software are not at risk of becoming obsolete due to online software updates. Finally, since the results do not depend on time-specific real-time data, they can be replicated at any time.

treatment groups. The first treatment group includes stations located south of the border, which benefited from the tax exemption, while the second group includes stations located just north of the border, which were not directly affected by the tax change. The main model to estimate is Equation 1:

$$Y_{i,t} = \alpha + \beta_S \times South_i \times Post_t + \beta_N \times North_i \times Post_t + \eta_t + \psi_i + \epsilon_{i,t} \quad (1)$$

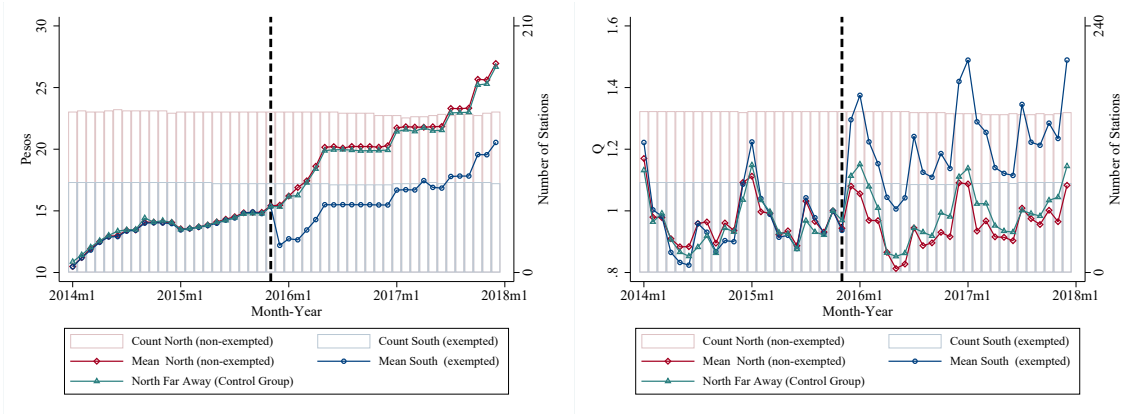
where $Y_{i,t}$ is the log of price or quantity in gas station i during month t. $South_i$ ($North_i$) is a binary variable equal to 1 if the station i is located within 180 km ($\approx$ 111.8 miles) south (north) of the border. The control group consists of firms located far north of the border $550 - 750$ km ($342 - 466$ miles). A time fixed effect η_t is included to control for seasonal patterns common to every gas station. Fixed effects ψ_i for gas stations are included to control for any time-invariant characteristics. Errors are clustered at the county level.[9] Because the outcome is in logs and the treatment indicator is binary, the transformation $e^{\hat{\beta}_S} - 1$ measures the percentage change in the southern area compared to the control group after the tax reform. The analogous interpretation applies to β_N for the northern group.

The key identifying assumption is that, absent the treatment, average price and quantity in both southern and northern stations would have followed the same trend compared to the control group over time. Figure 2 presents descriptive evidence supporting this assumption by plotting the evolution of the trend for the three groups for total gasoline (regular and premium). Panel (a) shows that, prior to the reform, average prices were similar across all groups. After the reform, exempted firms (blue line) were able to lower prices significantly. In contrast, stations on the northern side of the border (red line) displayed a price trend similar to that of the control group (green line). For quantities, Panel (b) shows similar trends for the three groups before the reform, and the divergence after December 2015, where quantities increased for southern stations and declined for northern ones compared to the control group. Given the pre-trends, it would be difficult to attribute the post-reform divergence in outcomes to anything other than the tax change of December 2015.[10]

The final sample in the pre-period is described in Table 2. There are 88 stations that receive the tax exemption (and are close to the border), 157 stations close to the border that do not receive the exemption, and 564 stations located in regions far from the border, which I assume are not affected by the tax change. For the year before the reform, Panel (A) in Table 2 shows the difference in quantities sold by the three groups, where the control group sells around 21 and 38 thousand liters more compared to the south and north groups, respectively. On the other hand, the three groups set virtually the same price, both for regular and premium. Panel (B) in Table 2 presents the distribution of stations. There is some overrepresentation of the YPF brand in the area around the border. The explanation

[9] Counties correspond to Argentine *departamentos* (*partidos* in the Province of Buenos Aires), the administrative level below provinces.

[10] The evolution in levels for regular and premium gas is depicted in Figures A.6 and A.7.



(a) Price (b) Quantity

Note: This figure displays the monthly evolution of average retail prices (Panel (a)) and quantities sold (Panel (b)) for total gasoline (regular and premium). The three series correspond to tax-exempt stations south of the new tax border (blue), non-exempted stations north of the border (red), and a northern control group located far from the border (green). Prices are the average between regular and premium gasoline in nominal pesos; quantities are total station sales for regular and premium gasoline in the administrative data standardized to October 2015. Both panels use the same group definitions: 180km ($\approx$ 112miles) north and south of the border. The control group consists of gas stations $550 - 750$ km $(342 - 466$ miles) located north of the border. A vertical marker indicates November 2015, when the reform was passed. The right axis shows the number of gas stations for each treatment group. Author's calculations based on Argentine Ministry of Energy data.

Figure 2: Price and quantity evolution around the border and control group, total gasoline

comes from the underrepresentation of Shell stations, whereas the *White Flags* stations have a constant share in the three regions. Panel (C) in Table 2 shows that the number of nearby stations is fairly similar in the three groups, especially for the smaller radius, even though there exists important variance for each of the different distance variables within each group.

3.2 Heterogeneity and Distance to the Border

If consumers are crossing the border, firms closer to it might internalize this behavior and would have the strongest incentives to adjust their prices. After presenting the average results, I will test for heterogeneity in the responses with respect to distance to the border. I divide gas stations into bins based on their distance to the tax border—where, for each station, the relevant distance is to its nearest competitor on the opposite side—and estimate Equation 2:

$$Y_{i,t} = \alpha + \sum_{k=1}^{B} \beta_k \times Post_t \times I_i\{k\} + \eta_t + \psi_i + \epsilon_{i,t} \tag{2}$$

where $I_i\{k\}$ is an indicator equal to 1 if station i falls into bin k. The total number of bins is B, with an equal number on each side of the border, and the control group consists of firms located $550 - 750$ km ($342 - 466$ miles) from the border, as before. Equation 1 is the special case in which $B = 2$. The identifying assumptions are identical to those in the main specification.

The number and length of the bins involve a trade-off: more bins imply narrower regions, which can identify more point estimates at the expense of a smaller number of stations per bin, generating noisier estimates. The main specification uses 4 bins of width 45 km (≈ 28 miles) on each side. These bins are wider than what the literature typically treats as a geographical market (Davis et al., 2025; Stolper, 2024; Taylor & Muehlegger, 2025), reflecting the lower station density of this rural region (Figure A.2). Robustness to alternative bin widths is reported in Figures A.3, A.4, and A.5; the main conclusions are unchanged.

Disaggregating by distance serves two additional purposes. First, it tests whether seemingly low responses at the aggregate level are effectively null responses or reflect aggregation bias from using extensive regions. Second, it sheds light on how far the effects of CBS reach from the border.

3.3 Further Identification Concerns

Beyond the parallel-trends assumption, the interpretation of the estimated treatment effects requires addressing several additional identification concerns.

Another important assumption is the absence of significant anticipation effects before the policy is implemented. In this context, anticipation effects would take the form of

Table 2: Descriptive Statistics: Before the Reform

Panel A: Price and quantity

	South		North		Control (550-750 km)	
VARIABLES	Mean	(SD)	Mean	(SD)	Mean	(SD)
Quantity (thousands liters)						
Regular Gas	96.17	(68.62)	78.88	(79.17)	116.92	(108.00)
Premium Gas	43.31	(38.89)	38.20	(45.00)	47.46	(51.38)
Price (pesos/liter)						
Regular Gas	16.67	(1.06)	16.80	(0.71)	16.78	(1.04)
Premium Gas	18.26	(1.00)	18.36	(0.78)	18.45	(1.09)

Panel B: Distribution Stations

	South		North		Control (550-750 km)	
BRAND	N	%	N	%	N	%
Esso	7	8.0	14	8.9	62	11.0
Oil Com.	1	1.1	7	4.5	47	8.3
Other	1	1.1	1	0.6	30	5.3
Petrobr	11	12.5	15	9.6	22	3.9
Shell	6	6.8	14	8.9	95	16.8
White	23	26.1	47	29.9	135	23.9
YPF	39	44.3	59	37.6	173	30.7
Total	88	100.0	157	100.0	564	100.0

Panel C: Number stations around

		South		North		Control (550-750 km)	
DISTANCE	N	$r = 1$	$r = 5$	$r = 1$	$r = 5$	$r = 1$	$r = 5$
Euclidean	Mean	4.06	10.95	4.10	11.18	3.99	18.01
[km]	(SD)	(3.03)	(8.58)	(3.38)	(13.14)	(3.05)	(19.93)
Driving	Mean	2.16	9.44	1.96	8.46	1.88	12.29
[km]	(SD)	(1.36)	(7.69)	(1.15)	(9.09)	(1.07)	(12.90)
Driving	Mean	1.59	7.06	1.38	6.01	1.29	7.21
[minutes]	(SD)	(0.78)	(5.92)	(0.66)	(4.83)	(0.59)	(6.24)

Note: This table reports descriptive statistics for gasoline stations in the period 2014 –2017 before the reform for the treated and control groups. The distance on each side of the border is 180 km ($\approx$ 111.8 miles) . Panel A shows average monthly quantities (thousand liters) and prices at the station level. Prices are expressed in December 2015 pesos and are residualized from common seasonal patterns. Panel B reports the distribution of stations by brand. Panel C reports the average number of competitors within different radii r. Distances are measured as straight-line distance using the *geodist* package (Picard, 2010) and as driving distance/time using the *osrmtime* package (Huber & Rust, 2016) with OpenStreetMap OSRM. Author's calculations based on Argentine Ministry of Energy data.

consumers delaying their consumption until the tax cut takes effect (Coglianese et al., 2017). Firms could also react by lowering prices in the south before the reform and increasing them after the reform in response to excess demand. The nature of the reform, attached to the political cycle, makes such anticipatory behavior implausible. Even if some consumers would have delayed consumption until the full implementation of the tax cut, there exists a physical constraint for intertemporal substitution. At most, a vehicle can shift one tank of consumption into the post-reform period. It does not seem very likely that these anticipatory behaviors could last more than a few days or have the strength to bias the estimates. In any case, the availability of monthly data addresses these effects, which should be concentrated in the months around the reform. Section 7.3 will deal with dynamic effects and show no evidence of anticipatory behavior.

A further concern for the southern region is the *original* tax border established in 1998 (Figure 1). That border created incentives of the same kind as the new one, but in the opposite direction: before the reform, the southernmost region remained exempt, so firms there enjoyed a favorable tax differential and arbitrage ran from north to south within the affected areas. The reform eliminates that differential along the original line, which in principle could shift consumption from south to north of the old border—where the treated group *South* is defined. Two identification issues follow. First, stations on the southern side of the *new* border could be contaminated by reallocation tied to the disappearance of the old border, biasing estimated effects upward; therefore results for stations located too close to the original border should be interpreted with caution. Second, and more serious, if some stations classified as "south" in the distance window actually lie south of the original border, the same coefficient would mix the new-border treatment with the negative effect on firms on the southern side of the old line. To avoid that, I restrict the sample to gas stations located north of the original border.

In principle, the change in incentives around the original border would allow an analogous analysis with a separate indicator for stations on the southern side of that border. This paper focuses on the new border because the goal is a counterfactual of firms that competed under similar conditions and were then shocked by a location-based rule that was not tailored to the ex-post tax differential they would face. The old border, by contrast, is associated with a long distortion that plausibly induced reallocation toward the south of the original line—as suggested by Figure 1—and station density is lower there, which would further compound the power problems of such an estimation.

In terms of identification, the coefficient β_S captures not only the effect of CBS but also the own-price elasticity of demand. Lowering the price of gas might induce a genuine increase in demand that is unrelated to CBS. The sum of these two effects is captured by β_S. If the elasticity is relatively high, this would bias the coefficient upward. This is less of a concern in this setting for two reasons. First, the literature generally finds a low elasticity of gasoline demand.[11] Second, the availability of quantity data on both sides of the border

[11] At least at the aggregate level, see Levin et al. (2017) for a review on the subject and a discussion of

allows me to test whether quantities increase in the south and decrease in the north at the same time, consistent with CBS. On the northern side, this concern is less relevant unless there is a significant price response. The estimation of Equation 2 will further help address this identification concern, as the effects should vary with distance to the border.

In this paper I am not addressing how local governments react to the federal tax change, assuming implicitly that they are not responding to CBS. A central debate in the CBS literature is how local governments set optimal taxes in response to CBS (Agrawal, 2012, 2015; Christiansen, 1994, 2003; Kanbur & Keen, 1993; Ohsawa, 1999). If local governments are reacting to CBS, the interpretation of the price change would then be a sum of firm and local government responses. This is not a concern for the quantity estimation as I am using the final retail price at the pump, which is the one that consumers observe and pay. A future line of research would be to analyze how local governments respond to the tax change by adjusting local taxes. At the moment, I am not aware of any homogeneous dataset that collects local taxes set by local governments over different periods in time. Even if in theory local taxes might be adjusting, the total effect on prices would be small given that local taxes are negligible compared to federal ones.[12]

Another caveat in this setting is the entry and exit of firms. If, because of the tax cut, a significant number of firms enter the market in the south, the coefficient would be biased towards zero, as the quantity demanded would then be divided by more firms. On the other side of the border, the inverse would happen. Given that, in the short run, reallocation seems unlikely because of the (literally) sunk costs associated with each gas station, I will focus this analysis on the window around the tax reform 2014 –2017 , using only a balanced panel of firms that do not enter or exit the market in that period. The long-run discussion is deferred to Section 6.

4 Price Responses Around the Border

Table 3 shows the results of estimating Equation 1. Prices in the exempted region fall by 21.6% after the reform. Given the tax structure, prices drop 9.1 percentage points less than they would with full pass-through, which would imply a drop of 31%. The implied pass-through of 70.3% is in line with the lower bound of the literature, which estimates a range from 70% to close to 100% (see Alm et al., 2009; Chouinard and Perloff, 2004; Genakos and Pagliero, 2022; Marion and Muehlegger, 2011).[13]

The effect on prices in the non-exempted region is theoretically ambiguous. If demand is shifting towards the south because of CBS, prices might decrease in the non-exempted area—a phenomenon known in the literature as *cross-border pass-through* (Bajo-Buenestado & Borrella-Mas, 2019). But prices could also rise in the non-exempted region

how the elasticity can vary at the firm level compared to the aggregate level.

[12] See Porto et al., 2019 for a discussion on the local taxes in the province of Buenos Aires.

[13] The price drop is almost identical for premium and regular gas.

if there is a change in demand elasticity for the northern stations (Hindriks & Serse, 2019). To illustrate this case, assume two types of consumers, mobile and non-mobile shoppers, with the former having lower travel costs and a higher elasticity of demand. If, after the tax, most of the mobile shoppers go to the southern region to purchase gas and the northern firms face mostly inelastic consumers, then the price will tend to rise in the north.[14] The equilibrium price would then be the result of two forces: a lower demand that pushes the price downwards but a more inelastic one that pushes it up. If the two effects offset, the result is zero change in prices—paradoxically the same outcome as in a situation where CBS does not take place at all. Empirically, β_N has positive point estimates, even though the magnitude is 15 times smaller than β_S and not always statistically significant.[15]

Price convergence at the border has been used as an indirect test for the existence of CBS: prices on the two sides of the border should converge as nearby firms compete for the same mobile consumers, while firms farther away—facing more captive demand—should respond less (Bajo-Buenestado & Borrella-Mas, 2019; Coyne, 2017; Doyle & Samphantharak, 2008; Tsvetanov, 2024). The empirical prediction is therefore a price gradient with respect to distance to the border.

The results of estimating Equation 2 are presented in Figure 3. The blue estimates show the percentage change in prices for each bin following the policy. Consistent with the average results, firms in the exempted area exhibit lower prices after the tax change while prices remain virtually unchanged in the non-exempted area. Within each region, however, no visible price gradient appears: the price response is homogeneous regardless of firms' locations, in both the southern and northern bins. The convergence prediction therefore fails to hold, even at the narrower intervals closest to the border.

The literature that finds a lack of price convergence often associates it with the absence of CBS. That is, either markets are not well integrated and the costs of traveling are too high (Louis & Taylor, 2016), or consumers and firms are inattentive or uninformed about the tax change and do not internalize the price differential (Hindriks & Serse, 2019). It could also be the case that historically the price differential had been so sharp that firms had already exited the market and the new tax change did not make any difference (Jansen & Jonker, 2018).

The absence of CBS is not the only explanation, however: price convergence may fail even when CBS is substantial. In a perfectly competitive market with constant marginal cost, pass-through is uniform across locations, so no price gradient arises regardless of how many consumers cross the border (Doyle & Samphantharak, 2008). Alternatively, prices may be rigid: governments often cap retail prices in energy markets (Guenette, 2020; Kojima, 2013), and multi-establishment firms frequently set uniform prices across different regions (Friberg et al., 2022; Messner et al., 2024).[16] If only mobile (and elastic) shoppers

[14] Despite increased mark-ups, if northern firms face significant fixed costs, the fall in quantity could be large enough to induce exit in the long run, which I will analyze in Section 6.

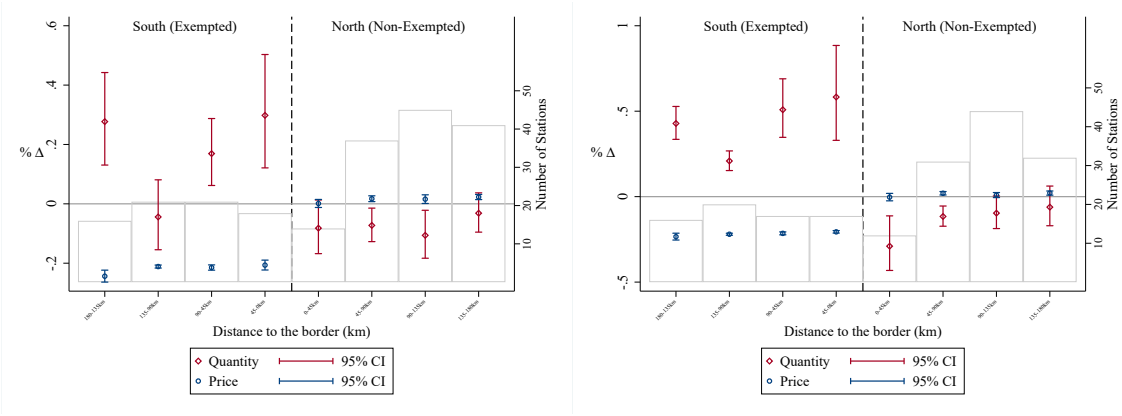
[15] See for example Figure 8 for different definitions of the treatment groups.

[16] Both mechanisms imply a horizontal supply curve and hence a pass-through of 100%, at odds with the

Table 3: Estimation of Equation 1 for price

	(1)	(2)	(3)
Log of price	Regular Gas	Premium Gas	Total Gas
Post × North (non-exempted)	0.0166 ***	0.0137 ***	0.0155 ***
	(0.0036)	(0.0042)	(0.0039)
Post × South (exempted)	-0.2460 ***	-0.2466 ***	-0.2429 ***
	(0.0071)	(0.0058)	(0.0064)
Constant	2.7745 ***	2.8756 ***	2.8208 ***
	(0.0006)	(0.0006)	(0.0006)
Observations	33891	30457	34004
Number of id	717	665	717
North	139	128	139
South	77	72	77
Control	501	465	501
R-squared	0.9360	0.9573	0.9456
Equivalent full pass-through	-0.307	-0.307	-0.307
Pass-through = $\frac{e^{\beta}-1}{Full}$	71.0%	71.2%	70.3%
CI (95%)	[67.4- 74.6]	[68.3- 74.1]	[67.0- 73.5]
Clustered standard errors in parentheses			
$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$			

Note: This table reports coefficient estimates and standard errors from regressions of Equation 1, where the dependent variable is the log of the retail price in pesos per liter. Columns correspond to regular and premium gasoline, and the average of the two, as indicated in the headings. Regressions are estimated using station-by-month data for the period 2014 –2017 . $South_i$ ($North_i$) is an indicator for stations located within 180 km ($\approx$ 111.8 miles) south (north) of the tax border; the control group consists of stations located $550 - 750$ km ($342 - 466$ miles) from the border. All specifications include station fixed effects and month fixed effects. Standard errors are clustered at the county level. For the treatment indicators, $e^{\hat{\beta}} - 1$ gives the percentage change relative to the control group. Significance level: ***0.01, **0.05, *0.10.



(a) Regular Gasoline

(b) Premium Gasoline

Figure 3: Bins and estimates on price and quantities

Note: Each panel reports estimated post-reform effects by distance bin from Equation 2 for regular (left) and premium (right) gasoline. Blue markers are price effects (percent change relative to the control group); red markers are quantity effects. Bins are defined by driving distance to the closest station on the other side of the tax border; the control group is stations $550 - 750$ km ($342 - 466$ miles) from the border (see text). Point estimates and confidence intervals correspond to the regression specification in Section 3.2. Tabulated coefficients appear in Tables A.2–A.3. Grey bars count the number of stations. Author's calculations based on Argentine Ministry of Energy data.

with no transportation cost are switching their consumption (Hindriks & Serse, 2019), then firms will have little incentive to move prices heterogeneously.

5 Quantity Results

The price results presented in Section 4 are uninformative about whether CBS is taking place. Given the drop in prices in the exempted area and virtually no change in the north, the natural next step is to examine quantities sold on each side of the border, since CBS is fundamentally about consumers shifting their purchase decisions. The estimates are presented in Table 4, where the specification follows Equation 1 but using the log of quantities as the dependent variable. On average, quantities increase by 22.5% in the exempted region. The increase is larger for the higher-quality gasoline, at 41.4% for premium gas compared to 15.6% for regular gas.

Combined with the price drops estimated above, a naive calculation of own-price elasticity would imply values of 0.72 for regular and 1.89 for premium gasoline—three to six times larger than typical estimates of gasoline demand elasticity (Brons et al., 2008; Coglianese et al., 2017; Hughes et al., 2008; Levin et al., 2017; Li et al., 2014). Under CBS, the

incomplete pass-through documented in Table 3.

southern coefficient will capture both the demand response of local consumers and the reallocation of buyers from the north. The coefficient β_N on quantities provides a direct test of this hypothesis. Table 4 confirms the prediction: quantities in the non-exempted area decrease by 7.4%. As in the south, the response is smaller for regular (7.3%) than for premium gas (11.4%).

To analyze the response with respect to the distance to the border, the red estimates in Figure 3 report the quantity responses for both fuel types. In the exempted region, the quantity increase is concentrated among stations closest to the border and falls off with distance.[17] The same pattern appears on the non-exempted side: quantities decline most sharply close to the border and attenuate with distance, though significant effects persist as far as 135 km ($\approx$ 84 miles) from the line. CBS therefore extends well beyond stations immediately adjacent to the border.

Taken together, the results document a setting with strong cross-border reallocation of quantities alongside no price convergence at the border. To the best of my knowledge, this is the first paper to document this combination of patterns identified from a tax change within an institutionally homogeneous region. These results add to a literature that has identified CBS through quantity data by bringing firm-level pricing into the same setting, allowing the price and quantity responses to CBS incentives to be examined jointly (Asplund et al., 2007; Banfi et al., 2005; Burstein et al., 2024; Campbell & Lapham, 2004; Chandra et al., 2014; Jansen & Jonker, 2018; Teixidó et al., 2024). Two closely related studies combine information on prices and quantities. Harding et al. (2012), in the context of cigarettes, use Nielsen transaction-level data to show that the pass-through of taxes is lower close to the border and that the probability of buying out-of-state rises with higher taxes. In contrast to the monotonic gradient documented above, Friberg et al. (2022) document a non-monotonic effect of distance on quantities in response to exchange rate changes in grocery stores along the Norwegian-Swedish border.

Absent quantity data, inferring the presence or absence of CBS from prices alone requires assumptions about market structure. This lesson applies not only to CBS in particular but to a broader set of questions related to taxation in general. Several papers test the heterogeneous pass-through of a tax along a dimension where firms differ to gain insights into the existence of a related phenomenon: the number of competitors to infer market power (Genakos & Pagliero, 2022), the identity of the tax remitter to infer monitoring costs and tax evasion (Kopczuk et al., 2016), the type of stores where anti-profiteering measures can be implemented to estimate redistributive effects of taxes (Benzarti et al., 2024), or regional wealth where firms are located to estimate the progressivity of gas taxes (Stolper, 2024).

[17] Results for the southernmost bins should be interpreted with caution. Figure 3 shows a quantity increase in those bins that is likely driven by the presence of the old border, as discussed in Section 3.

Table 4: Estimation of Equation 1 for quantity

	(1)	(2)	(3)
Log of quantity	Regular Gas	Premium Gas	Total Gas
Post × North (non-exempted)	-0.0754 ***	-0.1206 ***	-0.0769 ***
	(0.0228)	(0.0386)	(0.0250)
Post × South (exempted)	0.1449 **	0.3463 ***	0.2031 ***
	(0.0592)	(0.0461)	(0.0481)
Constant	4.1428 ***	3.2406 ***	4.4221 ***
	(0.0043)	(0.0051)	(0.0040)
Observations	33768	30291	33884
Number of id	717	664	717
North	139	127	139
South	77	72	77
Control	501	465	501
R-squared	0.9394	0.9369	0.9489
Clustered standard errors in parentheses			
$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$			

Note: This table reports coefficient estimates and standard errors from regressions of Equation 1, where the dependent variable is the log of monthly quantity sold (thousand liters). Columns correspond to regular and premium gasoline, and the sum of both, as indicated in the headings. The sample period is 2014 –2017 , and the sample is restricted to the balanced panel of stations that operate continuously during this window. $South_i$ ($North_i$) is an indicator for stations located within 180 km ($\approx$ 111.8 miles) south (north) of the tax border; the control group consists of stations located $550 - 750$ km ($342 - 466$ miles) from the border. All regressions include station fixed effects and month fixed effects. Standard errors are clustered at the county level. For the treatment indicators, $e^{\hat{\beta}} - 1$ gives the percentage change relative to the control group. Significance level: ***0.01, **0.05, *0.10.

6 The Long Run

The short-run results reveal an asymmetric adjustment: quantities fall sharply in the northern, non-exempted region while they rise in the south, yet northern firms display virtually no price response to the adverse demand shock they face. If northern stations cannot recover lost sales by reducing prices, their revenue will be structurally lower than it was before the reform. Faced with persistently lower quantities, exit might arise as the margin of adjustment. Symmetrically, southern stations experiencing a significant volume increase may attract new entrants. A natural question, therefore, is whether the price differential is strong and persistent enough to translate into a long-run reallocation of firms across the border. This section evaluates the probability of survival of firms in each region from two years after the policy onward.

To test whether the tax change affected the probability of survival of a firm, I estimate a linear probability model based on Equation 3:

$$D_{i,t} = \alpha + \sum_t \beta_S^t \times South_i \times I_t\{t\} + \sum_t \beta_N^t \times North_i \times I_t\{t\} + \psi_i + \eta_t + \epsilon_{i,t} \quad (3)$$

where $D_{i,t}$ is a binary variable equal to 1 if the firm i is operating in the market in period t. The sample includes every month from 2008 to 2022. The sample consists of the balanced set of firms active in the market in the short-run analysis. For the periods after the reform, the coefficient β_S^t is interpreted as the marginal change in the probability of survival of a southern firm compared to the control group in month t. For months prior to the reform, β_S^t has a slightly different interpretation, identifying the marginal change in the probability of operating in the market compared to the control group. The analogous interpretation applies for the north group. By construction, $D_{i,t} = 1$ for every month in 2014–2017, so those months will be omitted in the estimation.

The identifying assumption for Equation 3 is that, absent the tax change, exit rates would have evolved similarly across the southern, northern, and control groups. This assumption is plausible because the sample is the balanced set of firms active during the short-run window—a group of stations that were operating under comparable market conditions at the time of the reform, differentiated only by location relative to the border. The estimation strategy focuses on the extensive margin for the cohort of firms operating at the time of the shock. New entry after the reform is governed by location rules that depend on post-shock conditions and are outside the scope of this paper. Figure A.14 provides supporting evidence for the identifying assumption: pre-reform coefficients for both groups are close to zero and statistically indistinguishable from the control group, consistent with parallel pre-trends in survival rates. Figure A.9 corroborates this by showing a similar evolution in the total number of firms across regions before the reform.

The results are presented in Figure 4. The red and blue lines represent the estimated coefficients for each period for the northern and southern groups, respectively. Two vertical

lines mark the short-run analysis period (2014–2017), which is omitted from the estimation. The probability of survival for the southern group starts to increase two years after the policy compared to the firms in the control group, rising to almost 5 percentage points seven years after the policy change. The red line in Figure 4 shows the coefficients for the northern stations. Two years after the policy, the survival rate of northern stations begins to decline relative to the control group, falling to 5 percentage points below it seven years after the policy, which generates a relative difference of 10 percentage points between the southern and northern groups.[18]

The panel structure of the data allows me to show that the firms negatively shocked in the short run are those most likely to exit in the long run, bridging two branches of the empirical literature on CBS. On the one hand, papers analyzing short-run responses exploit shocks in CBS incentives, taking location decisions as given.[19] On the other hand, papers that characterize long-run equilibria abstract from shocks to CBS incentives (Chiou & Muehlegger, 2008; Manuszak & Moul, 2009). The key requirement for detecting firm exit is that the shock is both large and persistent. The quantity responses documented in Section 5 show a sharp, immediate divergence across the border, and Figure A.8 confirms that the price differential remained substantial over the following years despite subsequent tax adjustments.[20]

Two related studies link CBS conditions to firm survival. Campbell and Lapham (2004) find that exchange rate differentials increase aggregate store counts in US border counties facing Canada. Baggs et al. (2016) find no significant effect of exchange rates on firm survival at the Canada-US border, though they do find effects on employment and sales. This paper complements their results by using tax variation rather than exchange rates, by exploiting direct firm-level quantity data, and by identifying both positive and negative survival effects depending on which side of the border a firm is located on. More broadly, the results speak to the vast literature examining how state tax differentials affect business location at geographical borders (Bartik, 1985; Holmes, 1998; Peltzman, 2016; Rohlin et al., 2014), providing a context where firm exit is induced by tax differentials via CBS.

7 Robustness

This section complements Sections 4 and 5 along four dimensions to explore the nature of the results. First, because of the market structure and differences in brand concentration,

[18] It is worth noting that the difference in the survival rate for the northern group gets closer to zero towards the end of the period, around 2022. A possible interpretation is that this is the time it took to adjust to the new long-run equilibrium.

[19] A non-exhaustive list of examples is Bajo-Buenestado and Borrella-Mas, 2019; Burstein et al., 2024; Coyne, 2017; Doyle and Samphantharak, 2008; Gehrsitz et al., 2021; Harding et al., 2012; Hindriks and Serse, 2019; Jansen and Jonker, 2018; Louis and Taylor, 2016; Messner et al., 2024; Teixidó et al., 2024; Tsvetanov, 2024.

[20] In March 2018 the ad-valorem tax was substituted by a per-unit tax, updated several times for inflation.

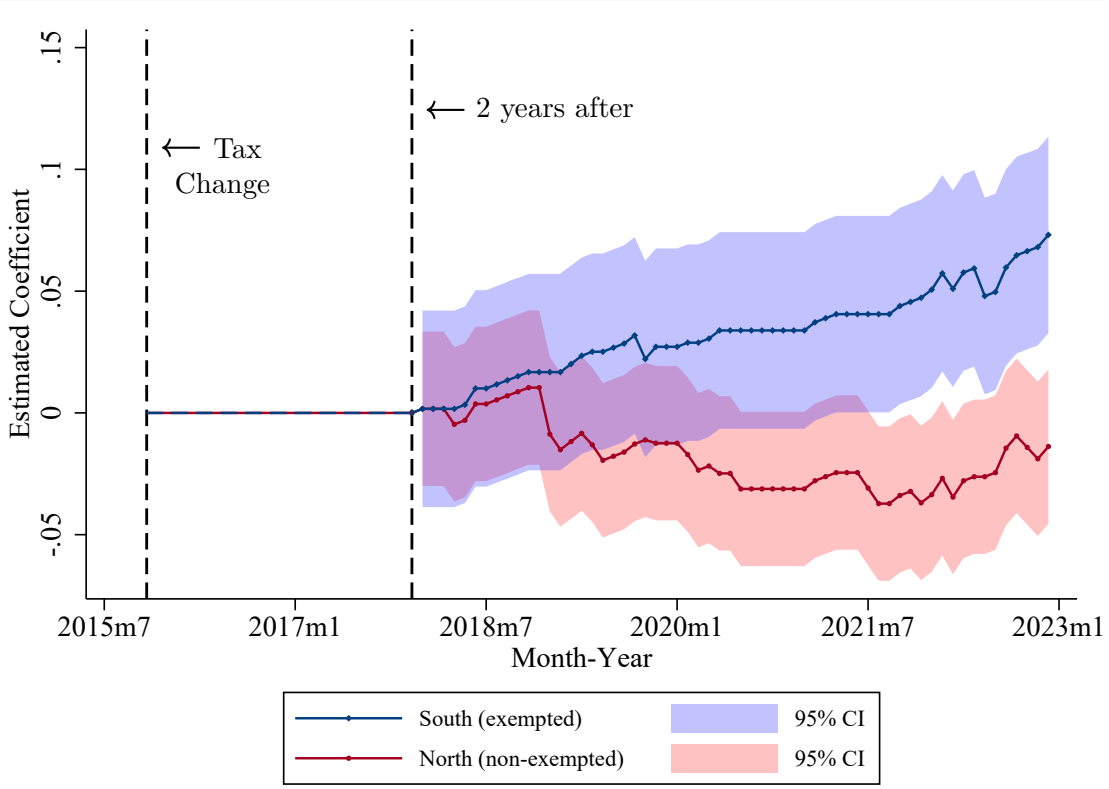


Figure 4: Estimated coefficients for the probability of operating in the market

Note: The figure plots coefficient estimates from Equation 3 for the probability that station i is active in month t, relative to the northern control group. Blue (red) corresponds to the southern exempted (northern non-exempted) border sample. The balanced short-run window around the reform is omitted in estimation by construction (see text). Vertical markers indicate the reform date and the window labeled in the figure (two years before/after). Shaded bands are confidence intervals at 95% levels. Standard errors are clustered at the county level. Author's calculations based on Argentine Ministry of Energy data.

I investigate whether there are heterogeneous effects by brand that explain the results. Second, I assess whether the number of competitors around each gas station can reflect differences in pass-through (Genakos & Pagliero, 2022). Third, I analyze whether there exist anticipation effects or lags in the implementation of the price adjustment by exploiting a dynamic analysis. Fourth, I employ several definitions of the treatment groups to measure the distance at which one can find effects of CBS.

7.1 Heterogeneous Effects by Brand

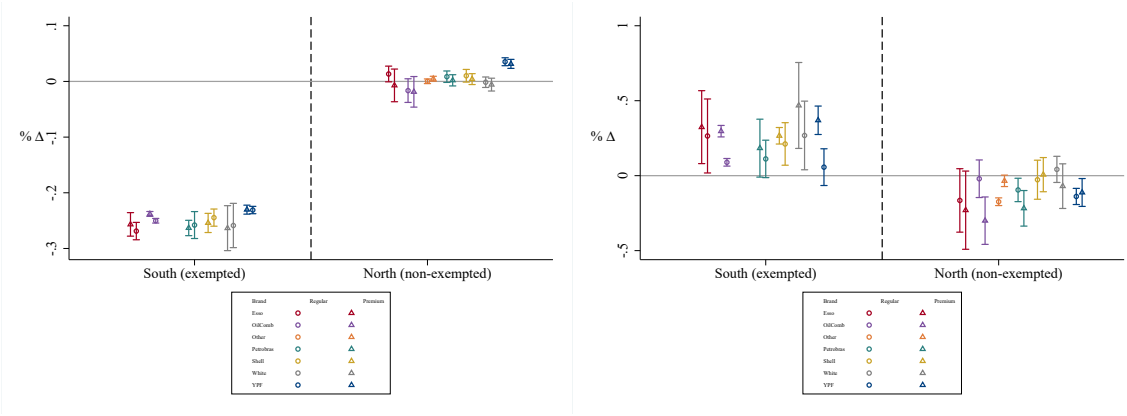
As explained in Section 2, there are differences in brand concentration, with YPF—a government-owned company—accounting for more than 50% of the market. A natural question is whether the leader sets a uniform price across the country: if so, the average price response would mainly capture a single firm's decision, and the absence of a price gradient documented in Section 4 could reflect price rigidity rather than the pricing response of firms to CBS incentives. Such uniform pricing can arise from government influence over a state-owned firm, as is common in energy markets (Guenette, 2020; Kojima, 2013), or from multi-establishment firms setting uniform prices across regions (Friberg et al., 2022; Messner et al., 2024). To assess this explanation, I estimate the price and quantity responses separately by brand and examine whether smaller brands follow a different strategy from the leader. The estimated equation is:

$$Y_{i,t} = \alpha + \sum_{k=1}^{K} \beta_S^k \times South_i \times Post_t \times I_k\{k\} + \sum_{k=1}^{K} \beta_N^k \times North_i \times Post_t \times I_k\{k\} + \eta_t + \psi_i + \epsilon_{i,t} \quad (4)$$

which is analogous to Equation 1 but uses an interacted variable for each brand k. Figure 5 depicts the result for both types of gas. There is no significant differentiation across brands, although the point estimates for YPF are slightly higher compared to the others. Indeed, Table A.6 shows the 95% confidence interval for the pass-through for the different brands and the overlap between them. The quantity estimates show a similar lack of heterogeneity. In the south, YPF shows a somewhat smaller increase for regular gasoline, but this is offset by a larger increase for premium. Overall, the data do not reveal a clear differentiation between coefficients for any particular brand.

7.2 Number of Competitors

The nature of competition is a key determinant of pass-through in imperfectly competitive environments (Weyl & Fabinger, 2013). In gasoline markets specifically, Genakos and Pagliero (2022) finds that pass-through increases with the number of competitors, rising from 0.4 in monopoly markets to complete pass-through in markets with four or more rivals. The baseline specification pools stations that may face different intensities of competition;



(a) Price (b) Quantity

Figure 5: Estimated coefficients for price and quantity differentiated by brand

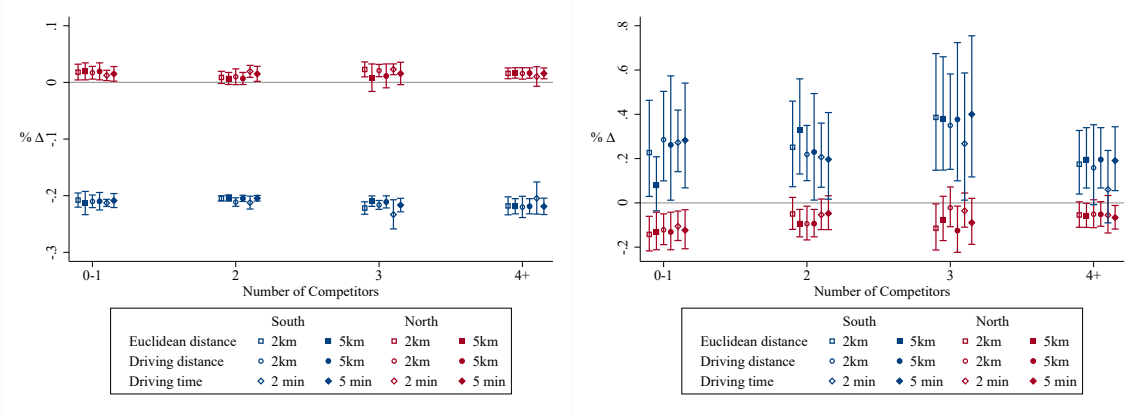
Note: Point estimates from Equation 4 with log price (left) and log quantity (right), interacted with brand indicators for $South_i \times Post_t$ and $North_i \times Post_t$. The sample and fixed effects match the baseline short-run specification (station and month FE). Standard errors are clustered at the county level. Corresponding regressions are in Tables A.6–A.5. Author's calculations based on Argentine Ministry of Energy data.

if the number of competitors varies systematically with distance to the border, this heterogeneity could confound the price gradient—or its absence—documented in Section 4. This section therefore tests whether the price and quantity responses vary with the number of competitors around each gas station. The estimated equation is

$$Y_{i,t} = \alpha + \sum_{c=0}^{3+} \beta_S^c \times South_i \times Post_t \times I_c\{c\} + \sum_{c=0}^{3+} \beta_N^c \times North_i \times Post_t \times I_c\{c\} + \eta_t + \psi_i + \epsilon_{i,t} \quad (5)$$

which is analogous to Equation 1 but uses an interaction based on the number of competitors within a certain radius. The number of competitors is defined using the three distance variables—Euclidean, driving distance, and driving time—for different radii (2 and 5 km, or 2 and 5 minutes). Figure 6 depicts the estimated coefficient for different types of distance measures to define the number of competitors.[21] Overall, there is no clear pattern of heterogeneous effects that arise from the number of competitors in terms of pricing. A similar pattern appears for quantities, but the percentage fall in the north seems to be stronger for less competitive environments for regular gas. In general, the different definitions of distance to count the number of competitors do not present any major difference.

[21] See regular and premium gas in Figures A.10 and A.11.



(a) Average price

(b) Quantity

Figure 6: Total gas (regular + premium)

Note: Estimated effects from Equation 5 for total gas (regular and premium), splitting treated stations by the number of competitors within a fixed radius (Euclidean distance, driving distance, or travel time, as indicated on the horizontal axis). Left: average log price; right: average log quantity. All regressions include station and month fixed effects; standard errors are clustered at the county level. Author's calculations based on Argentine Ministry of Energy data.

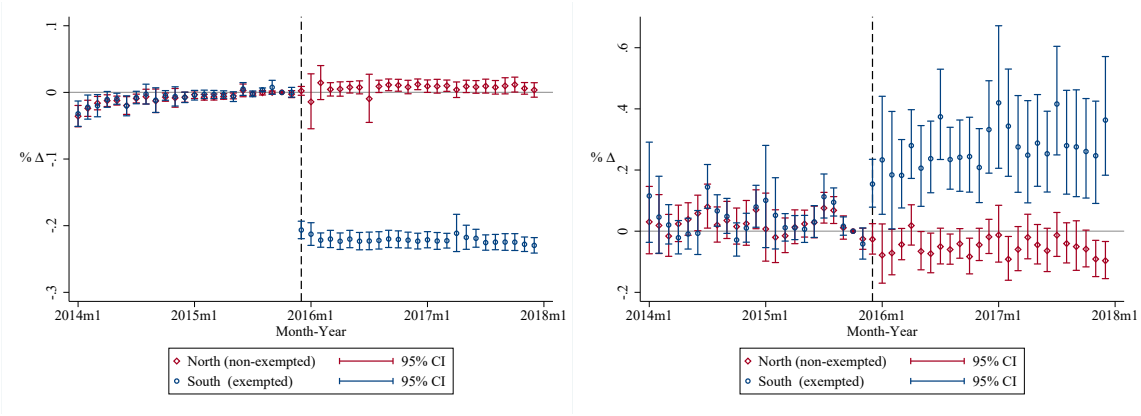
7.3 Dynamic Effects

The methodology in Section 3 estimates a single average post-treatment effect, which masks any variation in the response over time. Two concerns motivate a dynamic specification. First, because the reform was approved in Congress before it took effect, consumers could have anticipated the tax cut and delayed purchases, which would contaminate the pre-period and overstate the initial jump in quantities (Coglianese et al., 2017). Second, consumers and firms may learn about the price differential only gradually, so that the response builds over time rather than appearing at once. This section estimates the month-by-month evolution of prices and quantities around the reform. The estimated equation is:

$$Y_{i,t} = \alpha + \sum_{t=\text{Oct15}-L}^{\text{Oct15}+F} \beta_S^t \times South_i \times I_t\{t\} + \sum_{t=\text{Oct15}-L}^{\text{Oct15}+F} \beta_N^t \times North_i \times I_t\{t\} + \eta_t + \psi_i + \epsilon_{i,t} \quad (6)$$

which is analogous to Equation 1 but uses an interacted binary variable for each month. L and F are the respective lags and leads for before and after the period (January-2014 and December-2017). The base period is October 2015, the month before the reform was approved in Congress. Figure 7 depicts the evolution for total gas.[22]

[22] See regular and premium gas in Figures A.12 and A.13.



(a) Price (b) Quantity

Figure 7: Total gas (regular + premium)

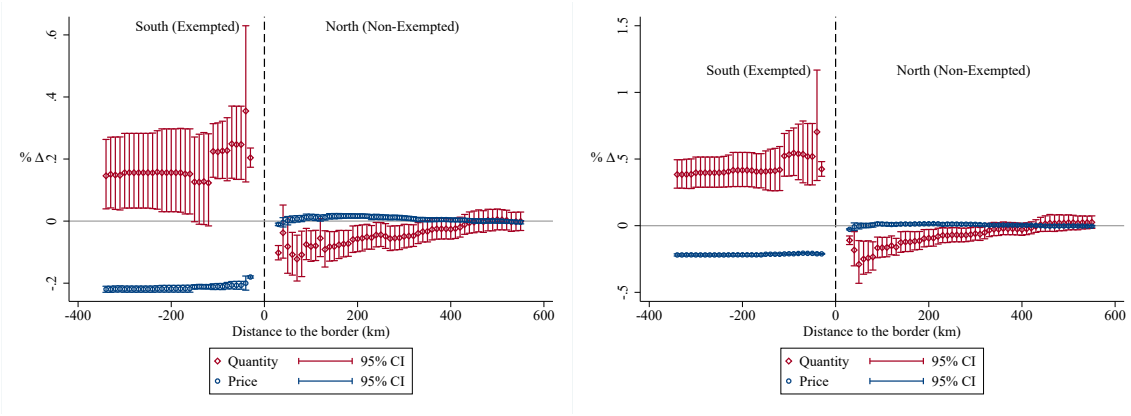
Note: Dynamic difference-in-differences estimates from Equation 6 for total gas (regular and premium), with event months relative to October 2015 (omitted baseline). Coefficients are interactions of month dummies with $South_i$ and $North_i$; the control group is stations far from the border (see text). Left: average log retail price; right: log quantity. Regressions include station and month fixed effects; standard errors are clustered at the county level. Author's calculations based on Argentine Ministry of Energy data.

The pre-trends for prices are similar for both groups and around zero, meaning no significant difference from the control group before the reform and suggesting no anticipatory effects. After the tax change, the effect on prices is immediate and permanent for both regions. In terms of quantities, the southern group presents evidence of a weak increasing trend over the post-treatment period, consistent with a gradual learning process among consumers that continues well beyond the month of the reform. The magnitude of this trend is, however, relatively small compared to the initial jump observed in the first month after the policy change. For northern stations, quantities are not distinguishable from zero in most months, even though the point estimates are negative. The pattern differs by fuel type for this group: premium gas drops in the first months and then recovers, whereas regular gas remains more stable over time.

7.4 Changing the Definition of the Treatment Groups

This section analyzes the sensitivity of the results to the definition of the treatment bands around the border. Beyond robustness, the exercise is informative: the distance at which the effects vanish measures the spatial reach of CBS. In the baseline estimation, the exempted and non-exempted groups were defined as gas stations within 180 km ($\approx$ 111.8 miles) south and north of the border. Figure 8 shows the results of estimating Equation 1 under alternative distance cutoffs for each treatment group.

The price results are not sensitive to the definition of the treatment bands. The base-



(a) Regular Gas

(b) Premium Gas

Figure 8: Different extensions for defining each treatment group

Note: Sensitivity of estimates from Equation 1 to widening the treated bands north and south of the tax border. The horizontal axis indexes alternative distance cutoffs for inclusion in $South_i$ and $North_i$ (see text). All specifications include station and month fixed effects; standard errors are clustered at the county level. Author's calculations based on Argentine Ministry of Energy data.

line pattern is preserved throughout: a sharp price drop in the southern region and a slight increase for the northern group. The quantity results also support the baseline conclusions, with the effects strongest for narrow bands and a clear asymmetry between the two regions as the bands widen. The northern coefficients attenuate toward zero as the band expands—adding stations far from the border, which are largely unaffected, dilutes the average effect—and lose significance at around 250 km ($\approx$ 156 miles). The southern coefficients, by contrast, do not converge to zero: because the exempted region is finite, widening the southern band never incorporates unaffected stations (Section 3).

8 Conclusion

This paper discusses the limitations of a standard test for cross-border shopping (CBS): price convergence at the border. I exploit a large and exogenous reform to Argentina's federal gasoline excise tax, enacted in November 2015 and effective the following month, which expanded a pre-existing tax-exempt zone in southern Argentina and cut the applicable excise from 55.8% to 0%—implying a roughly 30% drop in the tax-inclusive retail price under full pass-through. The reform was politically motivated rather than driven by contemporaneous market conditions, took effect within days of being announced, and generated a sharp geographic discontinuity in the tax burden faced by otherwise comparable gasoline stations on either side of the border.

To study the responses of consumers and firms, I build a novel station-level monthly

panel covering the universe of Argentine gas stations, which jointly observes retail prices, quantities sold, geographic coordinates, and brand affiliation. I employ a difference-in-differences approach that compares stations on each side of the border to a control group located far from the border and plausibly unaffected by the policy.

The results document strong evidence of CBS after the policy was implemented. In the short run, exempted stations south of the new border cut prices by approximately 22%—a pass-through of roughly 71%—and quantities sold rose by 22.5%, while non-exempted stations just north of the border experienced declines of 7.4% with little to no adjustment in prices. Decomposing these effects by distance to the border, quantity responses are concentrated near the border and attenuate with distance. In contrast, prices remain homogeneous within each region, with no evidence of price convergence.

In the long run, this reallocation of demand translates into a divergence in firm survival rates: seven years after the reform, the probability of remaining active rises by roughly 5 percentage points for southern stations and falls by a similar amount for northern stations near the border, generating a relative gap of about 10 percentage points.

The implications extend beyond the specific context of Argentina's gasoline market. In the absence of quantity data, using price convergence to infer cross-border shopping requires assumptions about the underlying market structure. More broadly, these results provide a cautionary note for the growing empirical literature that uses heterogeneity in tax pass-through across firms, locations, or competitors to draw inferences about market power, tax evasion, regulatory frictions, or the distributional incidence of taxation.

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Appendix

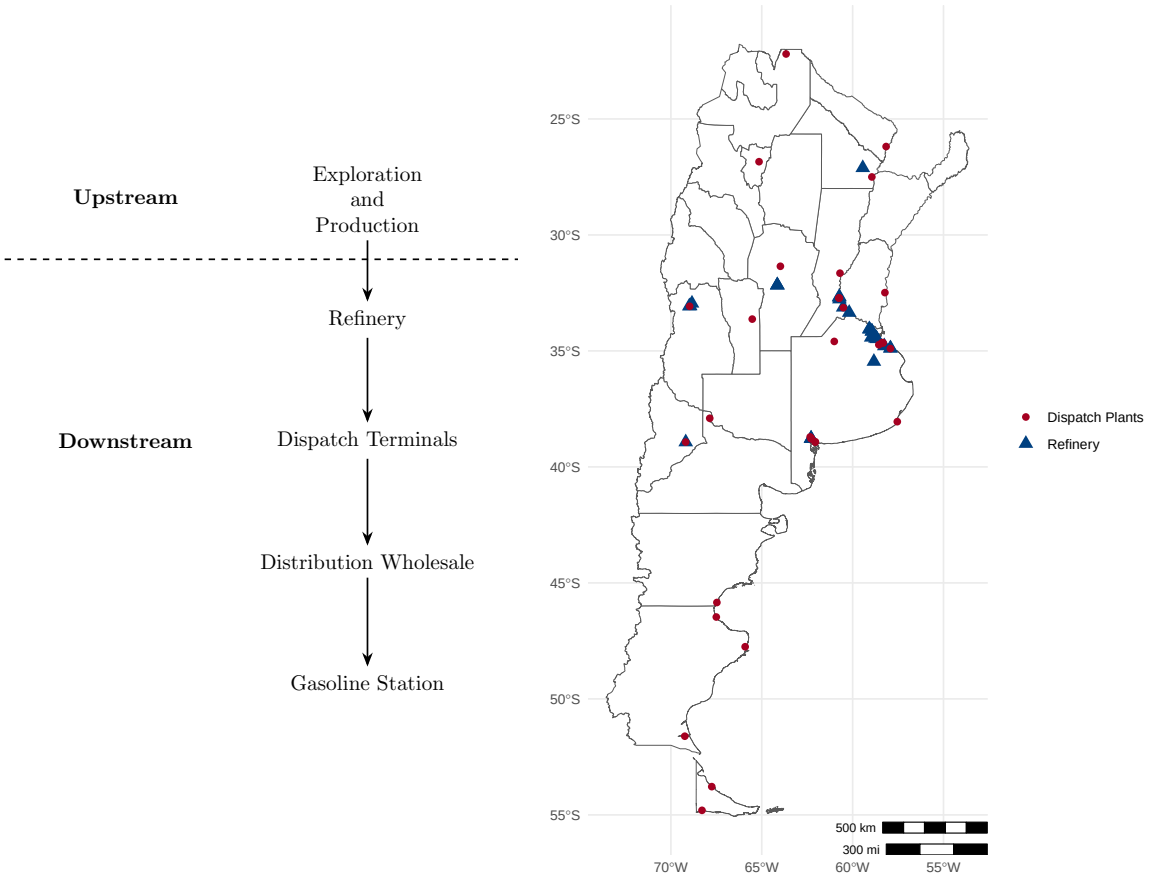


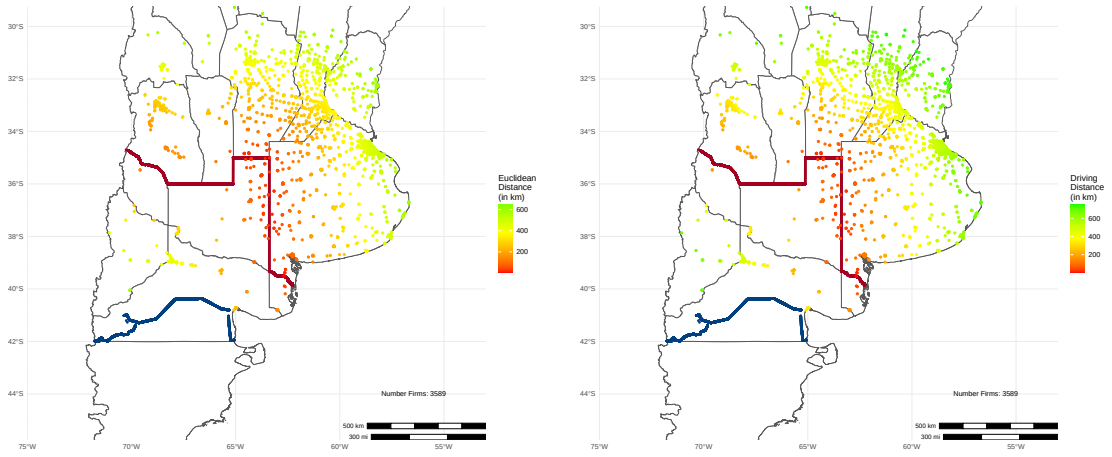
Figure A.1: Refineries and Dispatch Terminals

Note: Stylized schematic of the Argentine downstream chain from refineries to dispatch terminals and retail gasoline stations. Author's illustration for expositional purposes.

Table A.1: Legislative Timeline of Law 27,209 (Amendment to Law 23,966)

Date	Event
April 15, 2015	The legislative bill amending Law 23,966 was introduced in Congress.
May 27, 2015	The bill received preliminary approval (*media sanción*) in the Senate and was sent to the Chamber of Deputies.
August 9, 2015	Primary elections were held. The incumbent-party candidate received 38% of the vote, while the main opposition candidate received 24%.
October 25, 2015	General elections were held. The incumbent-party candidate received 37% of the vote, and the opposition candidate received 34%, triggering a presidential runoff. No candidate reached the 45% threshold required to win the presidency in the first round.
November 4, 2015	The bill was approved by the Chamber of Deputies, where the incumbent party held a majority, but had not yet been enacted.
November 22, 2015	The presidential runoff election was held. The opposition candidate won with 51% of the vote, compared to 49% for the incumbent-party candidate.
November 25, 2015	The law was enacted by the federal government, along with other laws and decrees issued by the outgoing administration.
December 1, 2015	Law 27,209 entered into force, and the tax exemption was fully implemented.
December 10, 2015	The new government took office.

Note: Information is based on official records from the Argentine Senate. See the Senate's official website.



(a) Euclidean Distance: `geodist` package

(b) Driving Distance: `osrm` package

Figure A.2: Distance to the closest station on the other side of the border.

Note: Maps plot the distance from each gasoline station to its nearest competitor located on the opposite side of the new TTLF exemption border. Panel (a) uses straight-line (Euclidean) distance computed with the `geodist` package Picard, 2010. Panel (b) uses route-based driving distance computed with the `osrmtime` package and OpenStreetMap OSRM Huber and Rust, 2016. Author's calculations based on Argentine Ministry of Energy data.

Table A.2: Price Estimates for the Bin Regression

	(1)	(2)	(3)
Log of price	Regular Gas	Premium Gas	Total Gas
Post × South bin 180-135km	-0.2794 ***	-0.2668 ***	-0.2708 ***
	(0.0135)	(0.0133)	(0.0134)
Post × South bin 135-90km	-0.2374 ***	-0.2485 ***	-0.2400 ***
	(0.0033)	(0.0039)	(0.0032)
Post × South bin 90-45km	-0.2416 ***	-0.2420 ***	-0.2399 ***
	(0.0058)	(0.0056)	(0.0049)
Post × South bin 45-0km	-0.2313 ***	-0.2306 ***	-0.2252 ***
	(0.0106)	(0.0047)	(0.0077)
Post × North bin 0-45km	0.0012	-0.0029	-0.0014
	(0.0069)	(0.0111)	(0.0075)
Post × North bin 45-90km	0.0171 ***	0.0195 ***	0.0174 ***
	(0.0049)	(0.0050)	(0.0051)
Post × North bin 90-135km	0.0156 **	0.0090	0.0130 *
	(0.0073)	(0.0074)	(0.0076)
Post × North bin 135-180km	0.0227 ***	0.0206 ***	0.0221 ***
	(0.0043)	(0.0061)	(0.0046)
Observations	33891	30457	34004
Number of id	717	665	717
Within R^2	0.2889	0.3768	0.3147
Clustered standard errors in parentheses			
$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$			

Note: This table reports coefficient estimates and standard errors from regressions of Equation 2, where the dependent variable is the log of the retail price in pesos per liter. Columns correspond to regular and premium gasoline, and the average of the two, as indicated in the headings. Regressions are estimated using station-by-month data for the period 2014 –2017 . Stations are grouped into 4 distance bins of width 45 km ($\approx$ 28 miles) on each side of the border, based on driving distance to the closest competitor across the tax border. Each coefficient measures the post-reform change in prices for stations in a given bin relative to the control group of stations located $550 - 750$ km ($342 - 466$ miles) from the border. All specifications include station fixed effects and month fixed effects. Standard errors are clustered at the county level. For the bin indicators, $e^{\hat{\beta}} - 1$ gives the percentage change relative to the control group. Significance codes: ***0.01, **0.05, *0.10.

Table A.3: Quantity Estimates for the Bin Regression

	(1)	(2)	(3)
Log of quantity	Regular Gas	Premium Gas	Total Gas
Post × South bin 180-135km	0.2445 ***	0.3564 ***	0.2873 ***
	(0.0618)	(0.0342)	(0.0492)
Post × South bin 135-90km	-0.0451	0.1896 ***	0.0529
	(0.0624)	(0.0241)	(0.0439)
Post × South bin 90-45km	0.1563 ***	0.4111 ***	0.2094 ***
	(0.0489)	(0.0575)	(0.0524)
Post × South bin 45-0km	0.2609 ***	0.4592 ***	0.2932 ***
	(0.0744)	(0.0886)	(0.0813)
Post × North bin 0-45km	-0.0854 *	-0.3424 ***	-0.1282 **
	(0.0499)	(0.1132)	(0.0608)
Post × North bin 45-90km	-0.0754 **	-0.1232 ***	-0.0859 ***
	(0.0309)	(0.0339)	(0.0317)
Post × North bin 90-135km	-0.1122 **	-0.1014 *	-0.0955 **
	(0.0458)	(0.0535)	(0.0448)
Post × North bin 135-180km	-0.0319	-0.0633	-0.0313
	(0.0347)	(0.0626)	(0.0388)
Observations	33768	30291	33884
Number of id	717	664	717
Within R^2	0.0149	0.0413	0.0199
Clustered standard errors in parentheses			
$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$			

Note: This table reports coefficient estimates and standard errors from regressions of Equation 2 with the log of monthly quantity sold (thousand liters) as the dependent variable. Stations are grouped into 4 distance bins of width 45 km ($\approx$ 28 miles) based on driving distance to the closest competitor across the tax border. Each coefficient measures the post-reform change in quantities for stations in a given bin relative to the control group of stations located $550 - 750$ km ($342 - 466$ miles) from the border. All specifications include station fixed effects and month fixed effects. Standard errors are clustered at the county level. Significance codes: ***0.01, **0.05, *0.10.

Table A.4: Price Estimates for the Bin Regression: Driving Time

	(1)	(2)	(3)
Log of price	Regular Gas	Premium Gas	Total Gas
Post × South bin 180-135min	-0.2718 ***	-0.2714 ***	-0.2643 ***
	(0.0053)	(0.0060)	(0.0066)
Post × South bin 135-90min	-0.2659 ***	-0.2742 ***	-0.2658 ***
	(0.0148)	(0.0095)	(0.0124)
Post × South bin 90-45min	-0.2544 ***	-0.2575 ***	-0.2527 ***
	(0.0049)	(0.0058)	(0.0049)
Post × South bin 45-0min	-0.2386 ***	-0.2533 ***	-0.2377 ***
	(0.0111)	(0.0041)	(0.0090)
Post × North bin 0-45min	-0.0096	-0.0215 **	-0.0143 **
	(0.0066)	(0.0102)	(0.0071)
Post × North bin 45-90min	0.0036	-0.0034	0.0032
	(0.0062)	(0.0067)	(0.0058)
Post × North bin 90-135min	0.0099 **	-0.0027	0.0051
	(0.0039)	(0.0049)	(0.0047)
Post × North bin 135-180min	-0.0020	-0.0093	-0.0033
	(0.0079)	(0.0123)	(0.0096)
Observations	27187	24140	27256
Number of id	581	535	581
Within R^2	0.4213	0.4846	0.4578
Clustered standard errors in parentheses			
$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$			

Note: This table reports coefficient estimates and standard errors from regressions of Equation 2, where the dependent variable is the log of the retail price in pesos per liter and distance to the border is measured as driving time (in minutes) instead of driving distance. Columns correspond to regular and premium gasoline, and the average of the two, as indicated in the headings. Regressions are estimated using station-by-month data for the period 2014 –2017 . Stations are grouped into 4 equal-width bins on each side of the border, based on driving time to the closest competitor across the tax border; the row labels show the limits of each bin in minutes. Each coefficient measures the post-reform change in prices for stations in a given bin relative to the control group of stations located $550 - 750$ minutes from the border. All specifications include station fixed effects and month fixed effects. Standard errors are clustered at the county level. For the bin indicators, $e^{\hat{\beta}} - 1$ gives the percentage change relative to the control group. Significance codes: ***0.01, **0.05, *0.10.

Table A.5: Quantity regressions by brand

	(1)	(2)
Log of quantity	Regular Gas	Premium Gas
Post × North (non-exempted)		
× YPF	-0.1387 ***	-0.1122 **
	(0.0273)	(0.0472)
× Shell	-0.0271	0.0068
	(0.0661)	(0.0578)
× White	0.0419	-0.0699
	(0.0443)	(0.0756)
× Esso	-0.1652	-0.2302 *
	(0.1075)	(0.1324)
× Petrobras	-0.0953 **	-0.2178 ***
	(0.0396)	(0.0602)
× OilComb	-0.0206	-0.3002 ***
	(0.0637)	(0.0803)
× Other	-0.1737 ***	-0.0345 *
	(0.0130)	(0.0194)
Post × South (exempted)		
× YPF	0.0568	0.3694 ***
	(0.0622)	(0.0481)
× Shell	0.2115 ***	0.2661 ***
	(0.0720)	(0.0280)
× White	0.2682 **	0.4682 ***
	(0.1163)	(0.1455)
× Esso	0.2648 **	0.3236 ***
	(0.1253)	(0.1235)
× Petrobras	0.1118 *	0.1835 *
	(0.0634)	(0.0981)
× OilComb	0.0891 ***	0.2967 ***
	(0.0129)	(0.0194)
Constant	4.1428 ***	3.2404 ***
	(0.0040)	(0.0051)
Observations	33768	30291
Number of id	717	664
R-squared	0.9397	0.9372

Clustered standard errors in parentheses

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Note: This table reports coefficient estimates and standard errors from regressions of Equation 4 for the log of monthly quantity sold (thousand liters), estimated with brand interacted with the treatment indicators. Each column corresponds to a different gasoline, as indicated in the headings. The sample is the balanced panel of stations around the tax border for the period 2014 –2017 . All regressions include station fixed effects and month fixed effects. Standard errors are clustered at the county level. Significance codes: ***0.01, **0.05, *0.10.

Table A.6: Price regressions by brand

	(1)	(2)
Log of price	Regular Gas	Premium Gas
Post × North (non-exempted)		
× YPF	0.0354 ***	0.0315 ***
	(0.0036)	(0.0041)
× Shell	0.0102 *	0.0042
	(0.0058)	(0.0050)
× White	-0.0014	-0.0057
	(0.0048)	(0.0058)
× Esso	0.0134 *	-0.0071
	(0.0072)	(0.0149)
× Petrobras	0.0085	0.0020
	(0.0052)	(0.0051)
× OilComb	-0.0165	-0.0187
	(0.0108)	(0.0139)
× Other	0.0003	0.0048 **
	(0.0021)	(0.0022)
Post × South (exempted)		
× YPF	-0.2310 ***	-0.2303 ***
	(0.0034)	(0.0041)
× Shell	-0.2446 ***	-0.2542 ***
	(0.0078)	(0.0087)
× White	-0.2587 ***	-0.2635 ***
	(0.0202)	(0.0205)
× Esso	-0.2688 ***	-0.2568 ***
	(0.0079)	(0.0107)
× Petrobras	-0.2580 ***	-0.2632 ***
	(0.0123)	(0.0070)
× OilComb	-0.2502 ***	-0.2380 ***
	(0.0021)	(0.0022)
Constant	2.7745 ***	2.8757 ***
	(0.0005)	(0.0006)
Observations	33891	30457
Number of id	717	665
R-squared	0.9363	0.9576
Equivalent full pass-through	-0.307	-0.307
Pass-through = $\frac{e^{\beta}-1}{Full}$		
YPF	67.2%	67.0%
CI (95 %)	[65.5 − 68.9]	[64.9 − 69.1]
Shell	70.7%	73.1%
CI (95 %)	[66.7 − 74.6]	[68.7 − 77.4]
White	74.3%	75.5%
CI (95 %)	[64.1 − 84.1]	[65.2 − 85.4]
Esso	76.8%	73.8%
CI (95 %)	[72.9 − 80.6]	[68.4 − 79.0]
Petrobras	74.1%	75.4%
CI (95 %)	[67.9 − 80.1]	[71.9 − 78.8]
OilComb	72.1%	69.0%
CI (95 %)	[71.1 − 73.2]	[67.9 − 70.1]

Clustered standard errors in parentheses

$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

Note: This table reports coefficient estimates and standard errors from regressions of Equation 4 for the log of the retail price in pesos per liter, estimated with brand interacted with the treatment indicators. Each column corresponds to a different gasoline, as indicated in the headings. The sample is the balanced panel of stations around the tax border for the period 2014 –2017 . All regressions include station fixed effects and month fixed effects. Standard errors are clustered at the county level. Significance codes: ***0.01, **0.05, *0.10.

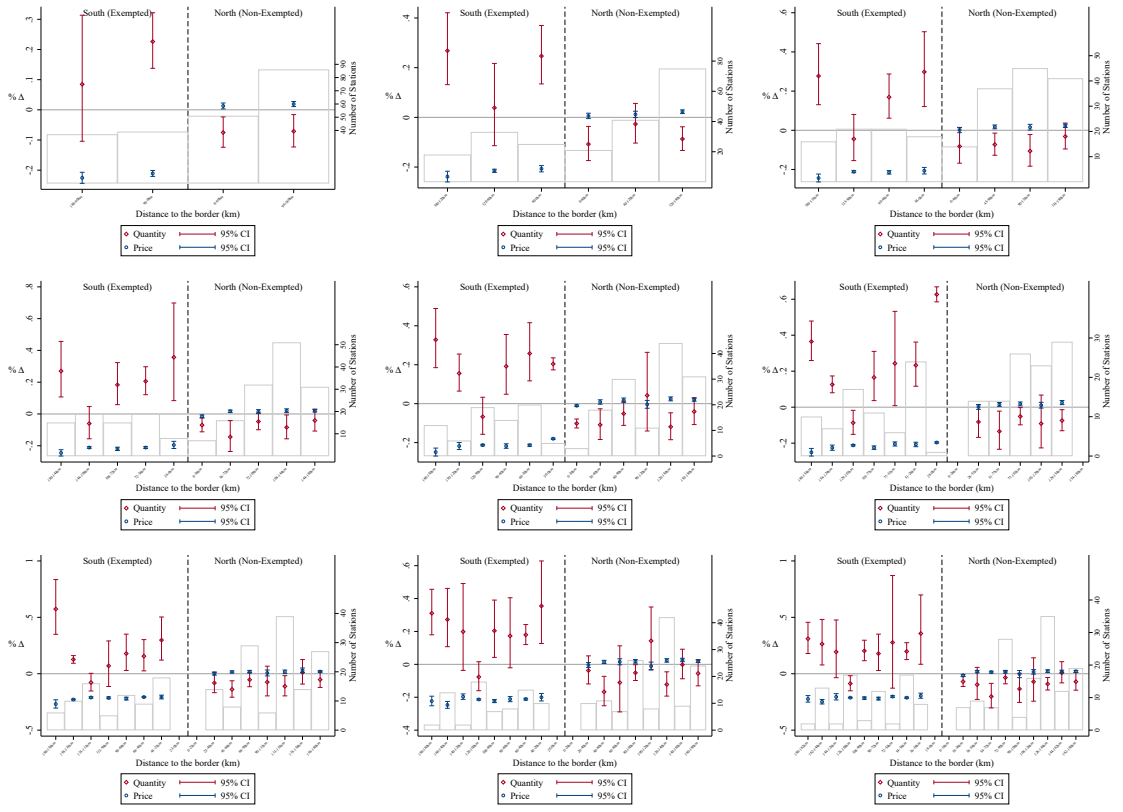


Figure A.3: Robustness to alternative bin widths: Regular Gasoline

Note: Each panel re-estimates Equation 2 splitting the same treatment bands into a different number of equal-width bins per side, from 2 (top left) to 10 (bottom right); the horizontal axis shows the limits of each bin. Group definitions: 180km ($\approx$ 112miles) north and south of the border; control group: $550 - 750$ km ($342 - 466$ miles) . For the treatment indicators, $e^{\hat{\beta}} - 1$ gives the percentage change relative to the control group. Author's calculations based on Argentine Ministry of Energy data.

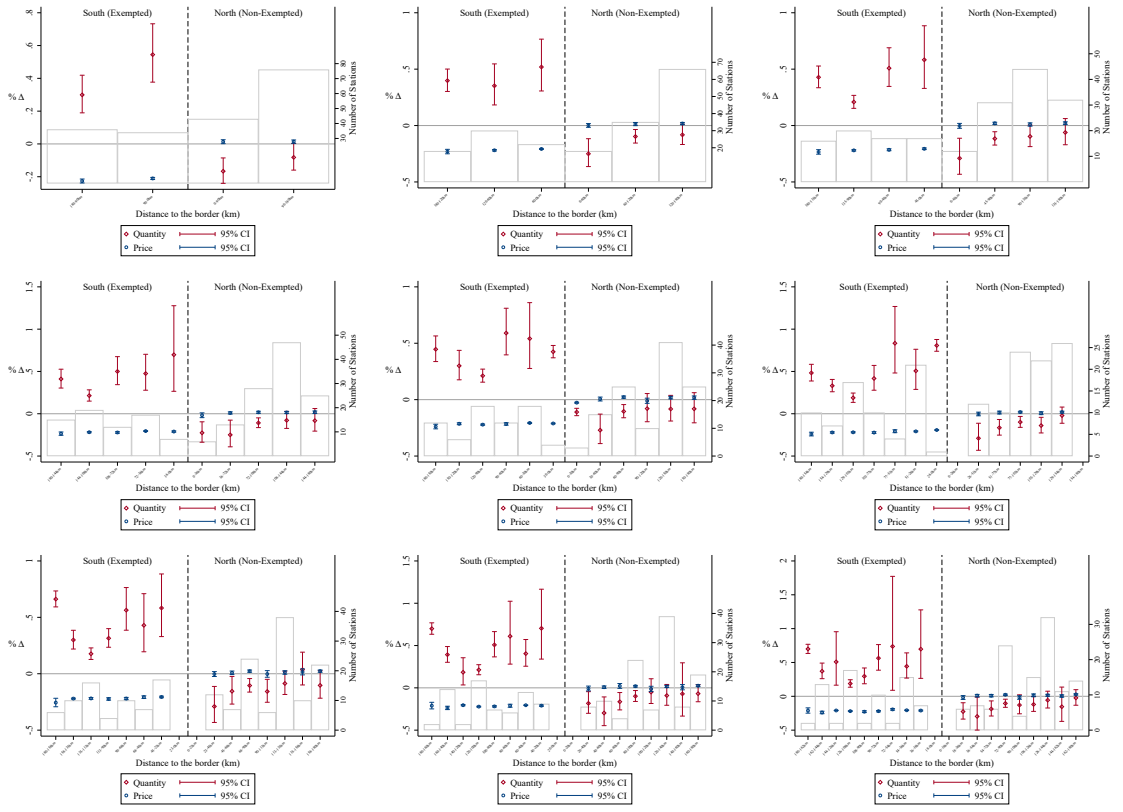


Figure A.4: Robustness to alternative bin widths: Premium Gasoline

Note: Each panel re-estimates Equation 2 splitting the same treatment bands into a different number of equal-width bins per side, from 2 (top left) to 10 (bottom right); the horizontal axis shows the limits of each bin. Group definitions: 180km ($\approx$ 112miles) north and south of the border; control group: $550 - 750$ km ($342 - 466$ miles) . For the treatment indicators, $e^{\hat{\beta}} - 1$ gives the percentage change relative to the control group. Author's calculations based on Argentine Ministry of Energy data.

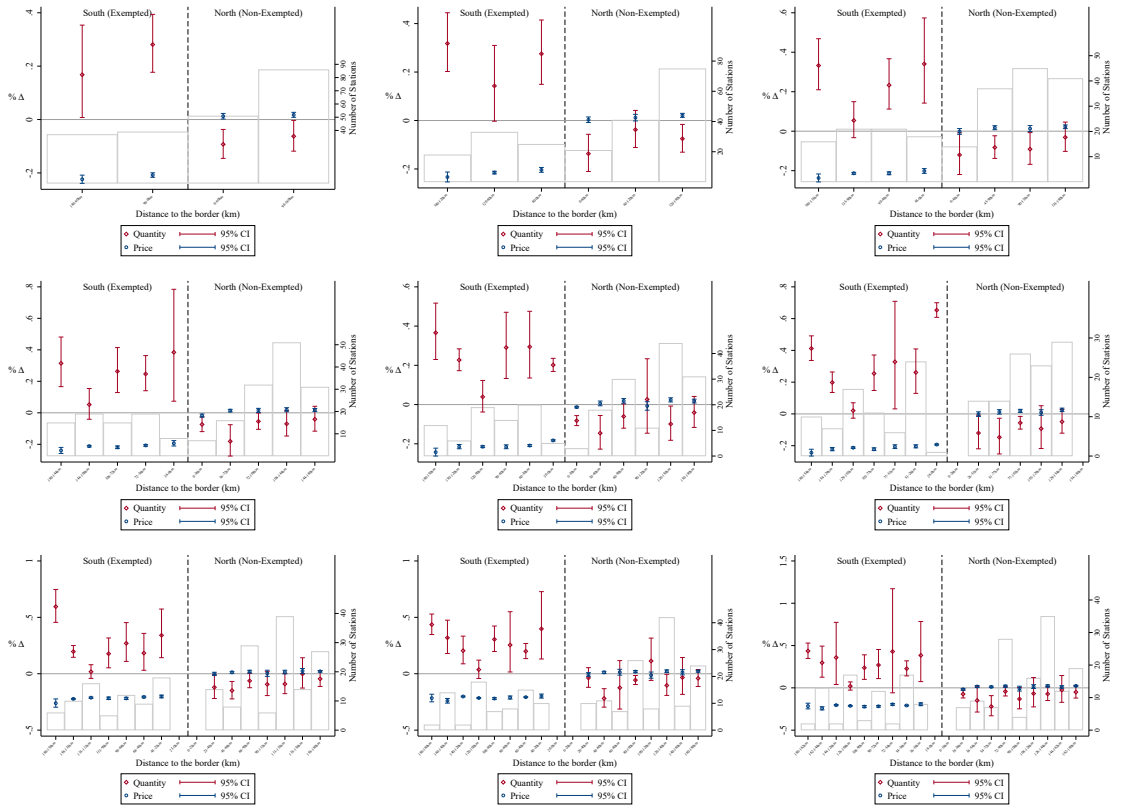
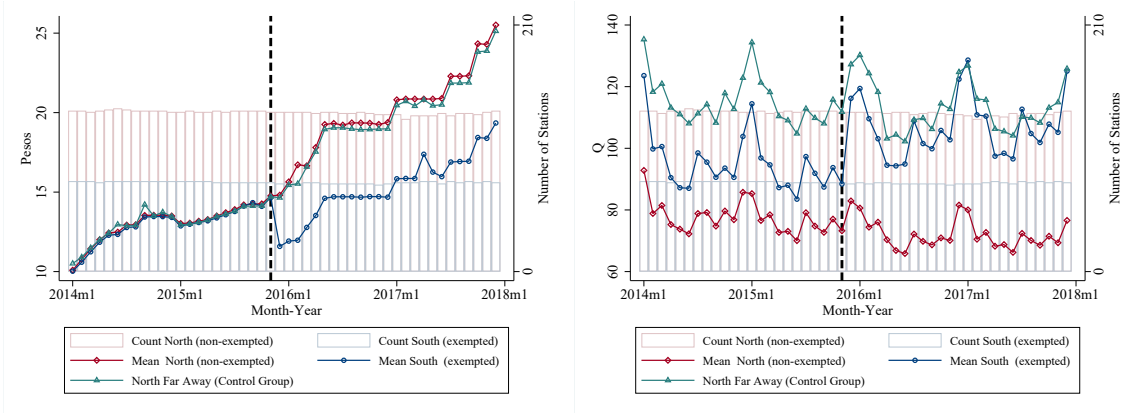


Figure A.5: Robustness to alternative bin widths: Total Gasoline (Regular + Premium)

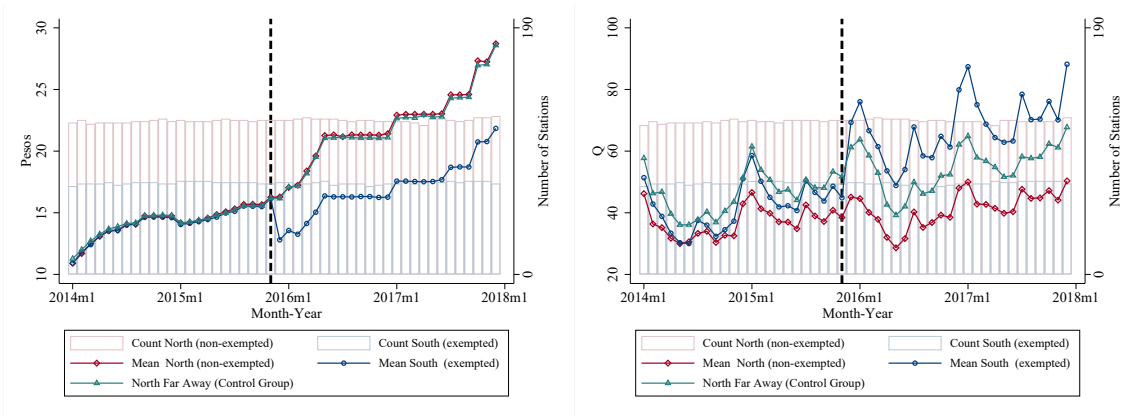
Note: Each panel re-estimates Equation 2 splitting the same treatment bands into a different number of equal-width bins per side, from 2 (top left) to 10 (bottom right); the horizontal axis shows the limits of each bin. Group definitions: 180km ($\approx$ 112miles) north and south of the border; control group: $550 - 750$ km ($342 - 466$ miles) . For the treatment indicators, $e^{\hat{\beta}} - 1$ gives the percentage change relative to the control group. Author's calculations based on Argentine Ministry of Energy data.



(a) Price (b) Quantity

Note: This figure displays the monthly evolution of average retail prices (Panel (a)) and quantities sold (Panel (b)) for regular gasoline for three groups: tax-exempt stations south of the new border (blue), non-exempted stations north of the border (red), and the northern control group (green). Group definitions: 180km ($\approx$ 112miles) north and south of the border; control: $550 - 750$ km ($342 - 466$ miles) . Vertical marker: December 2015 reform. Author's calculations based on Argentine Ministry of Energy data.

Figure A.6: Price and quantity evolution around the border and control group: Regular Gas



(a) Price (b) Quantity

Note: This figure displays the monthly evolution of average retail prices (Panel (a)) and quantities sold (Panel (b)) for premium gasoline for three groups: tax-exempt stations south of the new border (blue), non-exempted stations north of the border (red), and the northern control group (green). Group definitions: 180km ($\approx$ 112miles) north and south of the border; control: $550 - 750$ km ($342 - 466$ miles) . Vertical marker: December 2015 reform. Author's calculations based on Argentine Ministry of Energy data.

Figure A.7: Price and quantity evolution around the border and control group: Premium Gas

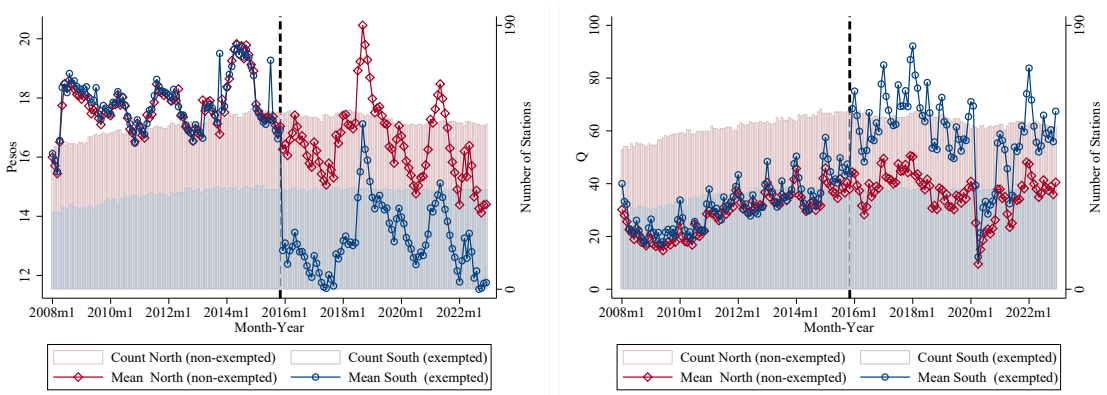


Figure A.8: Evolution of real price and quantities around the new tax border. Premium Gasoline

Note: Long-run evolution of average real retail prices (left) and quantities (right) for premium gasoline at stations located within 180 km of the tax border, excluding the originally exempted area. Lines show monthly averages for southern (exempted) and northern (non-exempted) stations; bars show the number of stations in each region. The dashed vertical line marks November 2015. Prices are in December 2015 pesos. Author's calculations based on Argentine Ministry of Energy data.

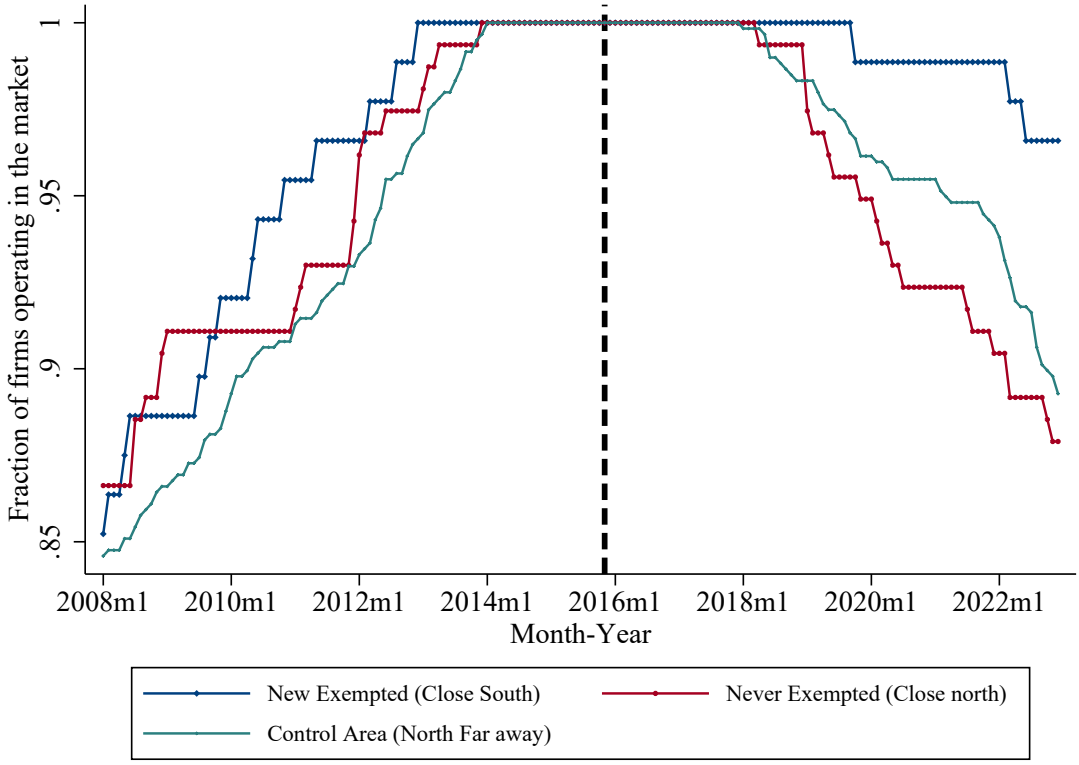
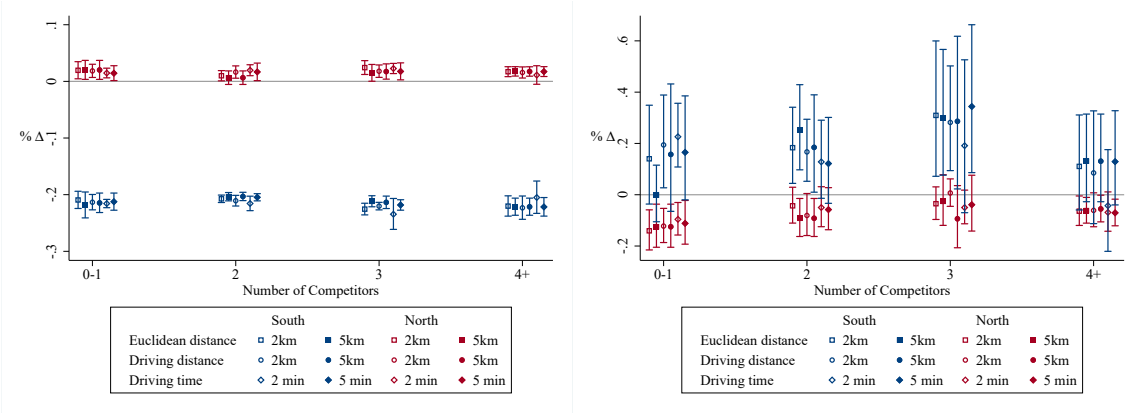


Figure A.9: Number of gasoline stations around the tax border, balanced sample (normalized).

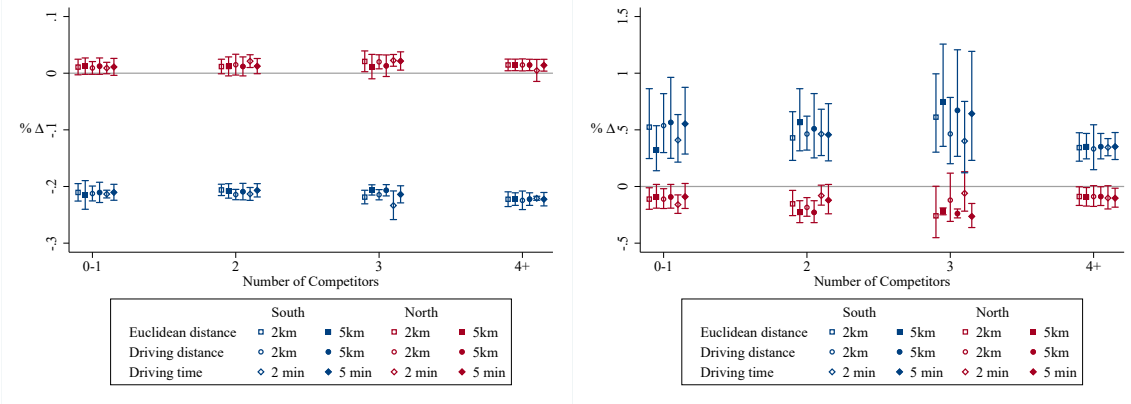
Note: Count of active gasoline stations by month and region for the subset of stations that operated two years after and before the reform, normalized to 100 in the month immediately preceding December 2015. Regions: 180km ($\approx$ 112miles) north and south of the border; the control group consists of stations $550-750$ km ($342-466$ miles) . Author's calculations based on Argentine Ministry of Energy data.



(a) Price (b) Quantity

Figure A.10: Regular gas

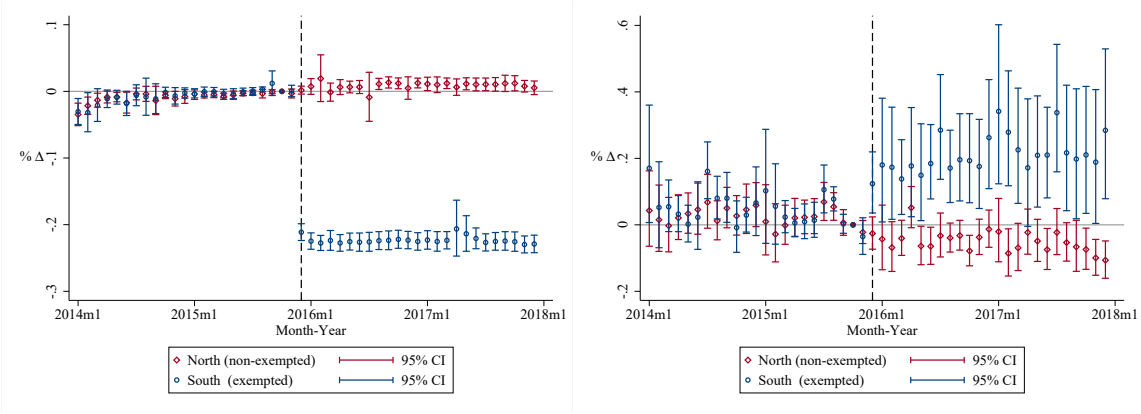
Note: Estimated effects from Equation 5 for regular gasoline, splitting treated stations by the number of competitors within a fixed radius (Euclidean distance, driving distance, or travel time, as indicated in the legend). Left: log price; right: log quantity. All regressions include station and month fixed effects; standard errors are clustered at the county level. Author's calculations based on Argentine Ministry of Energy data.



(a) Price (b) Quantity

Figure A.11: Premium gas

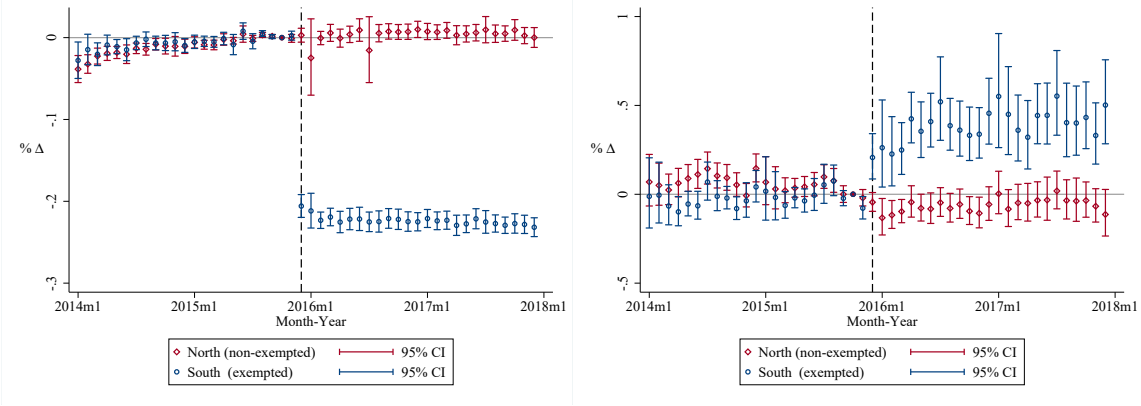
Note: Estimated effects from Equation 5 for premium gasoline, splitting treated stations by the number of competitors within a fixed radius (Euclidean distance, driving distance, or travel time, as indicated on the horizontal axis). Left: log price; right: log quantity. All regressions include station and month fixed effects; standard errors are clustered at the county level. Author's calculations based on Argentine Ministry of Energy data.



(a) Price (b) Quantity

Figure A.12: Regular gas

Note: Dynamic difference-in-differences estimates from Equation 6 for regular gasoline, with event months relative to October 2015 (omitted baseline). Coefficients are interactions of month dummies with $South_i$ and $North_i$; the control group is stations far from the border (see text). Left: log retail price; right: log quantity. Regressions include station and month fixed effects; standard errors are clustered at the county level. Author's calculations based on Argentine Ministry of Energy data.



(a) Price (b) Quantity

Figure A.13: Premium gas

Note: Dynamic difference-in-differences estimates from Equation 6 for premium gasoline, with event months relative to October 2015 (omitted baseline). Coefficients are interactions of month dummies with $South_i$ and $North_i$; the control group is stations far from the border (see text). Left: log retail price; right: log quantity. Regressions include station and month fixed effects; standard errors are clustered at the county level. Author's calculations based on Argentine Ministry of Energy data.

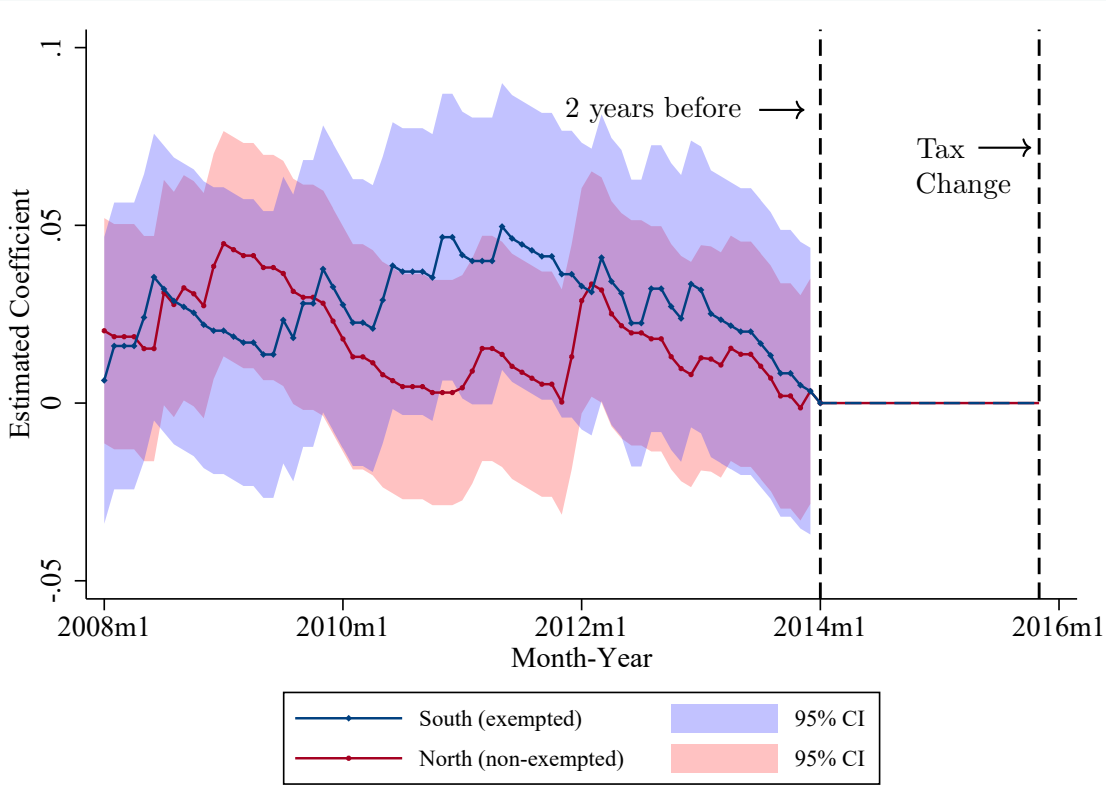


Figure A.14: Estimated coefficients for the probability of operating in the market

Note: The figure plots coefficient estimates from Equation 3 for the probability that station i is active in month t, relative to the northern control group. Blue (red) corresponds to the southern exempted (northern non-exempted) border sample. The balanced short-run window around the reform is omitted in estimation by construction (see text). Vertical markers indicate the reform date and the window labeled in the figure (two years before/after). Shaded bands are confidence intervals at 95% levels. Standard errors are clustered at the county level. Author's calculations based on Argentine Ministry of Energy data.