The effect of entry restrictions on price. Evidence from the retail gasoline market*

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Abstract

I exploit a change in Spanish regulations that created a quasi-experimental environment in which to test the effect of entry restrictions on retail gasoline equilibrium prices. In February 2013, a Central Government reform allowed gasoline stations to operate in industrial and commercial areas. This deregulation led to a high number of new market entrants over the following two years in these newly designated free entry areas. By isolating markets exposed to entry and markets not affected by new entrants, and using a difference-in-difference approach, gasoline retail prices are found to fall on average by at least 1.2% in the free entry areas. This result is economically significant, representing one fifth of the average retail margin. Moreover, if adopted by every gasoline station, the price reduction would imply savings in gasoline expenditure alone of around 179 million euros per year, in the lowest reduction scenario. Additionally, the results show that the equilibrium price reduction is greatest when the entrant is unbranded and that the effect decreases with the number of entrants and over time.

JEL Classification number: L71; L51; D43
Keywords: Gasoline retail market; Entry restrictions; Price competition; Public policy evaluation; Difference in difference

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1 Introduction

According to the latest statistics published by the European Commission, the transport industry represents 4.6% of Gross Domestic Product (GDP) and depends on oil and oil products for more than 96% of its energy needs. Moreover, by transport mode these statistics show that, within the EU, 27.75% of passenger transport is made by car and 45% of freight is transported by road. A comparison with US figures for 2012 shows that the transport industry represents 2.9% of GDP, while 89.5% of annual long distance trips are made by personal vehicle and 71.3% of freight is transported by road. In the US, 92% of the countrys energy needs in the transport sector are met by oil. Clearly, therefore, gasoline continues to be essential for any economic activity and for human life in general. Additionally, gasoline prices have a great impact on the productivity of firms and family expenditure, not only directly, but also indirectly as they affect the price of almost every product consumed.

Gasoline retail markets present an oligopoly structure around the world and retail gasoline prices are a source of constant concern for national governments. For example, retail gasoline markets have been under investigation by antitrust authorities in several countries, including in the UK, where the Office of Fair Trading conducted a review of the road fuel sector to understand the causes of price rises in 2012-2013. In Spain, the National Competition Commission published a series of reports between 2009 and 2012 expressing concerns about how Spanish prices and trading margins had increased, placing it toward the top of European price and margin rankings. In the United States, the Federal Trade Commission recently conducted an investigation to determine whether increments in retail gasoline prices were attributable to market manipulation or other anticompetitive behavior. Similarly, various measures have been introduced to limit control over retail prices in the sector, such as the divorcement laws in some US states and the recently adopted price regulations in Austria.

Government concerns for the retail gasoline market are reflected in the vast number of studies undertaken by researchers in the field. In academia, both industry structure and price behavior have been the focus of economic studies undertaken from a wide range of approaches. In an attempt at summarizing this literature, Eckert (2013) reviews empirical studies of the retail gasoline markets and identifies over 75 such articles since 2000.

One line of study in this empirical literature is to analyze the effects of potential reform measures. Specifically, studies have analyzed the impact of sales below cost regulations, bans on self-service stations, divorcement laws, and, sales taxes. However, to date, no studies have examined the effect of entry restrictions nor the impact on the market once they are lifted. Moreover, while we would expect the lifting of such restrictions to lead to the entry of new firms into the market, the literature does not report an unequivocal effect of new market entry on equilibrium prices. Indeed theoretical models predict different outcomes with some claiming that entry may lower the equilibrium price and others just the reverse.

Relaxing entry restrictions constitutes a potential policy for tackling concerns about gasoline prices, however, the effects of such a policy remain unexplored. Indeed, the theoretical literature fails to provide a clear guide as to what we might expect one the barriers to entry are removed. In this paper, I seek to fill this gap by empirically analyzing the effect that entry restrictions have on equilibrium prices. In addressing this question, I use a quasi-experimental environment created following a change in Spanish regulations in February 2013. The exogenous entry decisions due to the change in regulation provided me with this unique data set to explore the effects of deregulation.

I use a difference-in-difference approach applied to retail prices, demand and supply drivers and geographical data for the Metropolitan Area of Barcelona. I test the robustness of my results for the estimation techniques, different price measures and a heterogeneous response due to pre-existing differences in the treated and control groups. Additionally, I perform a placebo test and analyze the dynamic effects of the reform. I report that removing barriers to entry implies a reduction in retail gasoline price of at least 1.2%. This result is significant, representing one fifth of the average retail margin, when considering the lowest reduction scenario. Moreover, if the price reduction were to be adopted by every gasoline station, the reduction would imply savings in gasoline expenditure alone of more than 179 million euros per year. The results also show that when an unbranded gasoline station enters the market, the reduction in the equilibrium price is greater. In this sense, the evidence reported here supports the early findings of Hastings (2004) and Sen (2005).

A number of related empirical papers have analyzed the effect of market structure on gasoline retail prices, specifically seeking to determine how the number of competitors in the market impacts prices. Barron et al. (2004) performed a cross-sectional analysis of the one-day price in four different areas of the United States to contrast empirically the relationship between the number of competitors in the market, average price and price dispersion. The authors found that an increase in the seller density decreases both the average equilibrium price and price dispersion. In contrast, using a three-year panel of stations located in sub-urban Washington DC, Hosken et al. (2008) found that the number of competitors in the market has no influence on price. Finally, Tappata and Yan (2013), analyzed the relationship between margins and market size with a data set of isolated geographical markets located near entrances to national parks and, therefore, exposed to demand shocks. The authors used the past number of visitors to the park to instrument for market size and entry/exit decisions. Their results show that entry affects equilibrium in a non-monotonic way, leading to a large price reduction in markets with few incumbents, while the effect diminishes in markets with more than six or seven firms. I seek to add to this literature by analyzing an external shock generated by a public policy decision in urban areas. This article does not attempt to shed light on the relationship between the number of competitors and price, as the first two related papers, rather its objective is to provide evidence as to how the market responds to an additional entry. My results are complementary to those of Tappata and Yan (2013), as my data set let me analyse the effect of an additional entrant in urban areas, rather than in isolated markets.

To the best of my knowledge, this is the first article of its kind to assess the effect on prices of entry barriers to the retail gasoline market. Similarly, it is one of very few empirical contributions to the debate concerned with the effect of entry in a differentiated product market. Therefore, the results reported here are interesting because of their relevance not only to future gasoline market regulation (or deregulation), but also to a vast number of sectors that are subject to zoning restrictions, including for example the grocery industry.

The rest of the paper is organized as follow. Section 2 conducts the literature review. In Section 3, the gasoline retail market in Spain is presented. Section 4 reports the identification strategy, the data set, results and robustness checks. Finally, the article ends by drawing a number of conclusions.

2 Literature Review

This article draws on two related strands of the literature: first, studies that analyze policy impacts on the structure of the gasoline retail market and its equilibrium prices; and, second, studies that analyze what happens to the equilibrium price when a firm enters the market.

Policy reforms in the gasoline retail market Gasoline retail market reforms can be classified into those introduced to protect small retailers from going out of the market and those aimed at directly controlling retail prices. Among those in the first group, the policy that has attracted most research attention has been that of sales-below-cost (SBC) regulation. Among this literature, we found the work of Fenili and Lane (1985), Anderson and Johnson (1999), Johnson (1999) and Skidmore et al. (2005) for the US gasoline market, and, more recently, Carranza et al. (2015), analysing Canada.

Gasoline prices have been found both to increase (Fenili and Lane 1985; Anderson and Johnson 1999)and decrease (Skidmore et al. 2005) following this policy reform. These contradictory outcomes may be due to the different duration of the periods under analysis (Skidmore et al. 2005), with the policy presenting a differential effect in the short (rise) and long (fall) terms. As far as market structure is concerned, the studies coincide in showing that the total number of gasoline outlets increases in the presence of the regulation (Skidmore et al. 2005, Carranza et al. 2015). Moreover, Carranza et al. (2015) found that after the policy was introduced, the stations became relatively more homogeneous in terms of the type of services that they offered, and that the policy caused a long-run decrease in station-level sales.

Another policy established to protect small retailers was analyzed inJohnson and Romeo (2000), namely, bans on self-service stations. In a study of stations in Oregon and New Jersey, the authors found that such bans have affected the retail market structure by slowing down the penetration of convenience store tie-ins, and have resulted in higher retail margins. Furthermore, the authors also found that the bans provided little protection to smaller outlets, thus failing to achieve their primary objective.

Among the policies aimed at directly controlling retail prices, Barron and Umbeck (1984) and Vita (2000) studied the effect of divorcement laws, introduced in the United States, prohibiting the control of gasoline stations by refiners. Both articles found evidence that divorcement laws increased gasoline retail prices. Indeed, according to Barron and Umbeck (1984)BaUm84, the policy resulted in gains for the competitors of the divorced stations, who raised their prices, resulting in losses for both the affected stations and consumers. As a side effect of divorcement, the authors reported a reduction in the number of service hours provided by the affected stations. Finally, Doyle Jr and Samphantharakc (2008) considered the temporary suspension, and subsequent reinstatement, of the gasoline sales tax in Illinois and Indiana following a price spike in 2000 to study the effects of sales taxes on retail prices. Their results coincided with most studies analyzing the way in which shocks to crude oil prices and wholesale gasoline prices are passed through to retail prices. The authors reported evidence of asymmetries in the pass-through of reductions and increments in taxes to retail gasoline prices. Thus, their results suggested that 70% of the tax reduction was passed on to consumers in the form of lower prices, while between 80 and 100% of the tax reinstatements were passed on to consumers.

To sum up, the literature has analyzed the effect on gasoline retail prices of sales taxes, divorcement laws, sales-below-cost regulations and bans on self-service stations. Overall, the results show that policy reforms often fail to achieve their expected goals.

Expected effect on prices of deregulation The theoretical literature fails to reach a consensus on whether the effect of a new entrant in a differentiated product market is to lower or raise the average equilibrium price.

Despite the common belief that it should lower the equilibrium price, in markets with differentiated products there are several theoretical papers that conclude just the opposite. The explanations for this price rise are various. For example, Satterthwaite (1979), Stiglitz (1987) and Schulz and Stahl (1996), found that an increment in the number of firms causes an increment in equilibrium price when consumers face search costs. Specifically, Satterthwaite (1979) found this outcome for reputation goods, while in the model developed by Schulz and Stahl (1996), prices increase with the number of firms due to the existence of economies of scope in the search. Rosenthal (1980) found that the same outcome can also be due to the inability of sellers to charge different prices to different buyers, that is, those over whom they have market power and those over whom they do not.

Likewise, more than one paper has reported mixed results. In this stand of the literature, results seem to vary mostly because of consumer preferences. For example, Salop (1979)'s model of spatial competition with an outside good typically results in a new entrant lowering the equilibrium price. However, the author also found demand curves to be kinked and when market equilibrium lies at the kink of the demand curve, increases in market size lead to price rises. Applying the spokes model of non-localized competition, Chen and Riorda (2007) showed that an increase in the number of firms reduces the price if consumers value products highly, but raises the price if consumer value is in an intermediate range. Finally, for Janssen and Moraga-González (2004), the outcome does not depend solely on consumer search intensity but also on the number of incumbents in the market. The authors examined an oligopoly model in a costly sequential search to discover price settings, finding that when consumers search with high intensity, an entry reduces the price when the number of competitors in the market is low, but raises the price when the number of competitors is high.

Finally, a number of theoretical papers, most notably Gabszewicz and Thisse (1980), Perloff and Salop (1985), and Anderson and Palma (1992), make predictions in line with the commonly held belief that the equilibrium price decreases with a new entry; the former and the latter in models of spatial competition, and Perloff and Salop (1985) in a model of non-localized competition, when preferences are bounded.

Theory, therefore, fails to provide a unique effect of deregulating entry on equilibrium prices. Empirical contributions in this area are of fundamental importance, therefore, to shed light on the best way to regulate or deregulate a vast number of markets.

To conclude, the expected effect of lifting entry barriers is unclear according to theoretical studies, while available empirical evidence indicates that such policies might have a different impact to that expected. In the gasoline retail market, past studies have analyzed the effect on gasoline retail prices of sales taxes, divorcement laws, sales-below-cost regulations and bans on self-service stations. Nevertheless, the effect of removing entry barriers in this market remains unexplored. This article seeks to fill this gap in the literature.

3 Background to the policy reform

In this section I present the context in which the policy reform took place and describe the policy itself. As the current market configuration reflects its historical evolution, I start by briefly describing the history of the Spanish gasoline market before discussing its current structure and the policy reform.

The Spanish gasoline retail market began its operations around 1930. From the outset until 1984, it was fully controlled by the government via the public company CAMPSA. During those years, private companies were only allowed to participate in refining and were obliged to sell their gasoline to the public company. Gasoline stations were granted 75-year concessions and were only allowed to buy their product from CAMPSA and to resell it at a priced fixed by the government.

In seeking admission to the European Union (EU), Spain initiated the re-organization of the sector in 1984. In the period from 1984 through 1992, known as the transition years toward liberalization, a parallel network of gasoline stations was created selling imported products from the EU. Crude oil imports were liberalized and barriers on the import of oil products from other EU countries were reduced until they were finally lifted in 1992. A particular milestone in this period was the founding of the public company REPSOL in 1987, to which the government transferred all public oil and gas activities. In 1992, the oil monopoly was lifted and modern retail gasoline market era began. Any distinction between the official and parallel gasoline station networks was avoided, while CAMPSA was fully privatized, changing its name to CLH and providing logistics and transportation activity. The privatization process of REPSOL was also initiated and completed by 1997.

As expected, in common with most countries, Spains gasoline market entered its modern era with one market leader, a legacy of the decades-long oil monopoly. The company occupying this spot was REPSOL, followed closely by CEPSA, a Spanish company that founded in the early years of the countrys oil activity. A third firm that had operated for years in the Spanish market was the British gasoline firm BP. Henceforth, I refer to these three as the incumbents.

With the liberalization of the market in 1992, competition in the retail gasoline market was not especially intense because of the asymmetries between competitors. As a result, several liberalization and competition-oriented measures were introduced in the following years. In the gasoline station network, prices were fully liberalized and unrestricted access to third parties to the network was introduced in 1998, the year in which Spains Energy Regulator was created. In 2000, daily price reporting from gasoline stations was made mandatory, and restrictions on the opening of new gasoline stations were imposed at the provincial level on companies with a larger than 30% market share and on those with a 15 to 30% share. Similarly, large commercial areas were allowed to house a gasoline station. Finally, the last measure prior to February 2013 was the removal of the minimum distance restrictions imposed between stations in 2001.

As discussed in the introduction to this Section, both the initial configuration of the market and the subsequent reforms have conditioned the evolution of the Spanish retail gasoline market. Table 1 shows the market shares of the principal competitors in 1995 and over the last three years, as well as the total number of stations they each operate. As can be observed, and in contrast with other cases, the number of stations has experienced constant growth over the last 20 years. Indeed, between 1995 and 2004 this growth is estimated at about 70%. Among the incumbents, REPSOL decreased its participation by 22 points and CEPSA by 10, while BP maintained its share throughout the period. In 1995, almost 80% of gasoline stations were operated by one of the two major brands, but by 2014 they accounted for only 47% of the supply. Hence, most of the growth in these years can be attributed to unbranded gasoline stations and to the emergence of supermarket chains as a competitor in the gasoline retail market.

In short, before the policy reform examined in this study, the Spanish market had experienced constant growth in the number of gasoline stations due, in the main, to new, unbranded competitors. As Table 1 shows, this tendency was accentuated by the policy reform whose impact on prices is the subject of this article.

Table 1: Spanish retail gasoline market. Share by brand and total number of stations. 1995; 2012-2014

Brand	1995	...	2012	2013	2014
Repsol	55%		35%	34%	33%
Cepsa	24%		15 %	14 %	14%
BP	6		6 %	6 %	6 %
Galp	2%		6%	6 %	5%
Disa (Shell)	1.6%		5 %	5 %	5%
Other Wholesalers	-		5%	5 %	6%
Supermarkets	-		3 %	3 %	3%
Unbranded	-		16 %	18 %	20%
Cooperatives	-		6 %	6 %	5%
Total	6,327		10,424	10,617	10,712

Note: 1995 data from Cavero and Bello (2007); 2012-2014 data from Spanish Association of Operators of Oil Products (AOP).

Deregulation of entry Against a backdrop of economic crisis and with an unemployment rate of about 25%, on 22 February 2013, the Spanish Government enacted Royal Decree-Law 4/2013 on 'measures to support entrepreneurs and to stimulate growth and job creation' and introduced normative reforms in different sectors of the economy. Among these reforms, the law regulating the hydrocarbon sector (Law34/1998) was modified in several respects. Specifically, two measures were introduced in the retail gasoline market: the first concerned the deregulation of market entry, and, the second, vertical contract agreements.

In the case of market entry, the reform added a paragraph to the previous law establishing that all land uses for commercial activities, malls, commercial parks, zones for vehicle inspection, and, industrial zones were from that moment on also deemed compatible with the use for gasoline stations. In this way, local governments obtained unrestricted entry to these areas, despite previous urban planning laws that prevented local authorities from introducing such measures.

As for the vertical agreements, the Decree-Law established a one-year contract duration, renewable for a maximum period of three years. In addition, it banned the introduction of clauses influencing or determining the retail price in future contracts in cases where the dealer owned and operated the gasoline station.

4 The effect of free entry (deregulation) on gasoline equilibrium prices

4.1 Identification and Estimation Methods

The objective of the article is to identify the effect on equilibrium retail prices of deregulating the entry of gasoline stations in industrial and commercial areas. Specifically, I seek to measure the average effect of entry on the prices charged by gasoline stations competing with the entrant in specified areas. As my agents are gasoline stations, following Hastings (2004), I consider all gasoline stations located in a one-mile radius of an industrial area. Ideally, I should count on randomly selected deregulated areas and areas in which the restriction is still effective, giving me a perfect counterfactual to measure the impact of deregulation on price. However, the legislation was applied indiscriminately across the country and so I cannot conduct a perfectly randomized experiment. I therefore identify the effect on the competitors price of entry due to deregulation by estimating a counterfactual.

Although deregulation was introduced in both commercial and industrial areas, it only had an entry effect in the case of the latter. This can be explained by the fact that since 2000 commercial areas had been able to open gasoline stations and so this regulation served merely to further the previous reforms. This means that here I only consider industrial areas with market entrants and industrial areas without any entrants following deregulation, and, therefore, I have gasoline stations exposed to competition due to deregulation and stations that have not suffered this same exposure.

An obvious concern here is that industrial areas with new market entrants may differ in some respects from industrial areas without. For example, entry might have occurred in areas with more intense industrial activity and with higher traffic. In this case, the lower prices charged by competitors in these areas might be correlated with greater competition and not only with the effect of a new market entrant in the newly deregulated area. To tackle this potential problem, I first applied difference-in-difference methods to a longitudinal data set of different stations (competitors in industrial areas and otherwise), eliminating differences between areas and, hence, in the conditions of the two groups of gasoline stations that are invariant over time. Additionally, in a second stage, I applied matching procedures to control for different demand and supply drivers. In this way, I used the price changes of stations located within a one-mile radius of industrial areas with no new entrants (control group) to measure what would have happened to stations located within a one-mile radius of industrial areas with new entrants (treated group), in the absence of deregulation. By comparing changes in the outcomes of these two groups, I was able to control for observed and unobserved time-invariant area characteristics that could affect retail prices of gasoline.

I estimated the following two-way fixed effect linear regression model:

$$p_{itj} = \beta_o + \beta_1 int_t + \beta_2 D_i t + \beta_3 x_i + \beta_3 x_j + \lambda_m + \lambda_y + \sigma_j + \epsilon_{it} \tag{1}$$

where p_{itj} is the logarithm of the monthly price of gasoline charged by gasoline station i located in municipality j in period t; int_t represents the international price of gasoline in period t; $D_i t$ is a dummy variable that takes a value of one when gasoline station i is a competitor of a new entrant in a deregulated area in period t; x_i is a vector of control variables that vary by gasoline station; x_j is a vector of control variables that vary by municipality and time; λ_m represents month dummies and λ_y year dummies, σ_j is a municipality dummy; and, ϵ_{it} is a gasoline station time-varying error and is assumed to be independently distributed. The logarithmic specification of price improves the normalization of the dependent variable and facilitates the interpretation of the policy dummy as a percentage.

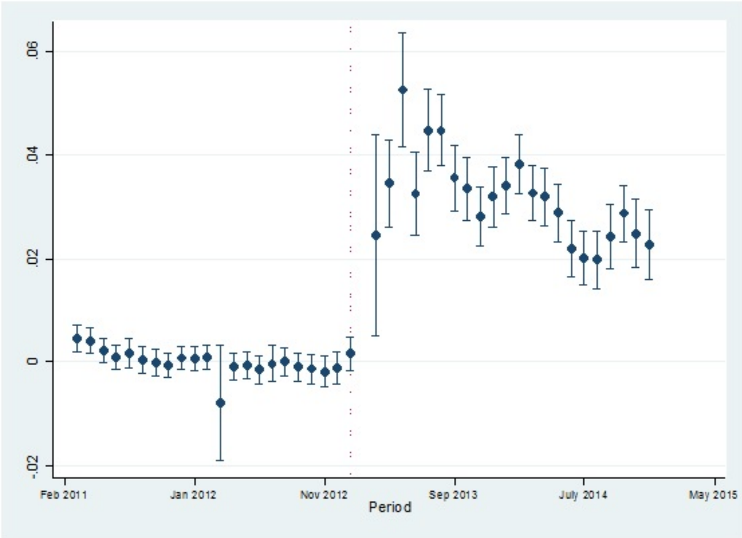
Recall that in this case, $D_i t$ do not take the value of 1 for all treated observations after February 2013. Instead, it depends on the dynamics of the entrance of gasoline stations throughout the post-deregulation period. Hence, $D_i t$ takes a value of 1 after gasoline station i is exposed to an entrant from period t and onwards.

Besides controlling for seasonality with the month dummy λ_m and for common shocks on a yearly basis λ_y, especially relevant in the context of a crisis, I also include the international price int_t and specific gasoline station characteristics x_i as control variables. The variable int_t is used as a proxy of the wholesale price that gasoline stations have to pay, controlling for specific time shocks of the principal determinant of gasoline prices (Chouinard and Perloff 2007). The vector of controls of specific gasoline station characteristics, x_i, incorporates a group of dummy variables indicating the brand, if during the period there is a change in brand, the presence of a coffee shop, store and carwash, whether the gasoline station is open 24 hours, and, location in an urban or industrial area, or on a main road or highway; and, a group of index variables including the number of pumps and number of competitors within a one-mile radius. Non-variant time variables are only included in specifications without fixed effects.

Finally, I control for municipality characteristics that vary across time and that are related to the size of the market for each gasoline station. Following previous articles, I include population; number of cars, trucks and motorbikes; and, gross family income per capita (Vita 2000; Skidmore et al. 2005).

The estimate of interest in the model, β_2, represents the difference-in-difference effect of deregulation of entry on retail gasoline equilibrium prices. The key identifying assumption for this approach is that the change in the prices of competitors in areas without entry is an unbiased estimate of the prices that the treated gasoline stations would have charged in the absence of entry (Meyer 1995). As this assumption is not observable, I provide evidence that it holds by testing the existence of parallel trends between the two groups before entry. I do this in two ways. First, I perform a mean test by period and the results show that I cannot reject the equality in means between groups prior to February 2013 (see Figure 1). Second, I test for equality between average changes in line with Galiani et al. (2005). The approach involves estimating the transformed version of equation 1 so as to consider the control observations over the entire period and the treated observations in the pre-treatment period. The policy variable is replaced in this equation by dummies variables for each group and period. Finally, the equality of the estimators for the control and treated groups is tested. The results of the test indicate that I cannot reject the hypothesis of parallel trends. Estimation results and tests are presented in Appendix A.

Figure 1: Mean difference by period. Treated and control groups



Notes. The dot line divides periods in pre and post reform. The null hypothesis of equality of means between control and treated groups cannot be rejected for every period before deregulation. After deregulation the alternative hypothesis is accepted.

Finally, an error correlation is expected both temporally and at the cross-section level introducing a bias in the results. In such cases, as Bertrand et al. (2004) show, considering an autocorrelated structure of the error term of degree 1 might not be enough to overcome the problem. Hence, following the results of these authors, and given the large number of gasoline stations in this study, I allow for an arbitrary variance-covariance structure by computing the standard errors in clusters by id. I also test the robustness of the results by applying block bootstrap to these id clusters.

4.2 The data

I assess the impact of entry deregulation on gasoline retail prices in Spain by drawing on data from the Metropolitan Area of Barcelona. My data set contains information related to gasoline stations in 168 municipalities of the Barcelona province. Within this geographical area, the Catalan Government has defined 731 industrial areas. Maps of the municipalities and the industrial areas were downloaded from the Department of Territory and Sustainability of the Regional Government. A map of the area is presented in Appendix B.

Daily gasoline station prices were downloaded from the Ministry of Energys internet page, where it provides price information for geo-referenced gasoline stations on a daily basis for the whole of Spain. No historical data are stored on the site so prices had to be downloaded daily to create the data set. The files from the Ministry contain geo-references (latitude and longitude), the name of the gasoline station and the price charged. Gasoline stations within the Metropolitan Area of Barcelona were identified by combining the geo-references provided by the Ministry with the Regional Governments map of the area. This identification procedure was performed in Matlab.

Having identified the Metropolitan Areas gasoline station, I combined their geo-references with the maps for the whole area and the map identifying the designated industrial areas, in shape-file format, in the free, open-source geographic information system QGIS. Here, I delimited the one-mile radius influence zones around the industrial areas for both treated and control groups. Examples of treated and control groups are presented in Appendix B.

The dependent variable in my analysis is the monthly average price of diesel charged by each gasoline station. Although data are available for all types of gasoline, I focus solely on diesel as it represents approximately 70% of the Spanish market. Following the literature, I use the net price in order to omit any tax distortions. Nevertheless, I checked the robustness of my results when using both gross price and margin. I exclude gasoline stations that are located both within a one-mile radius of an industrial area with entry and within another without entry, and which therefore would have to be included in the treated and control groups. Likewise, I exclude from the sample all gasoline stations that closed before the deregulation was introduced in February 2013 and gasoline stations that entered the market after that date. Finally, I do not include the competitors of gasoline stations that entered an industrial area after July 2014 for two reasons: first, because of the small number of observations that would in fact be included after deregulation (in most of the cases, 1 or 2); and, second, because it would mean including locations with lower prices for the entire period and give rise to problems of self-selection in the sample.

I use monthly data to make this series compatible with the international price of diesel, which I use as proxy of the wholesale price. Overall, I have data for 322 gasoline stations. Of these, 94 belong to the treated group and 228 to the control group. The period under analysis extends over a 22-month period before and after the reform was introduced. As I lack data for August 2011, my pre-reform period extends from March 2011 to January 2013, while the post -reform period extends from March 2013 to December 2014. Hence, my sample comprises an unbalanced panel of 14,168 observations.

The international price of diesel was downloaded from Spains Antitrust Institution (CNMC). It corresponds to a weighted average that includes 70% of the Mediterranean Price and 30% of the North Western European (new) price.

Data at the municipal level were downloaded from the Barcelona Provincial Councils website. This includes the following variables: population, number of cars, trucks and motorbikes, and, gross family income per capita. All variables are presented on an annual basis.

As discussed above, the vector of controls of specific gasoline station characteristics includes a group of dummy variables indicating the presence of a coffee shop, store and carwash, whether the gasoline station is open 24 hours, location in an urban or industrial area, or on a main road or highway, and, if the gasoline station changed brand during the period of the analysis. In addition to these variables, I control for the number of pumps and brand. The data was built using Google Earth and the oil companies websites, except for the brand and change of brand variables, that were taken from the Ministry's files. The table below presents the characteristics of the gasoline stations in the treated and control groups in January 2013, prior to the reform.

Table 2: Gasoline stations characteristics. Mean differences in February 2013

Variable	Treated	Control	Diff
Store	0.901	0.858	-0.043
	(0.031)	(0.024)	(0.042)
Coffee shop	0.217	0.328	0.111
	(0.043)	(0.033)	(0.057)
Carwash	0.505	0.545	0.039
	(0.053)	(0.035)	(0.063)
Pumps	6.155	6.388	0.233
	(0.236)	(0.183)	(0.315)
24 hs	0.452	0.454	0.002
	(0.052)	(0.033)	(0.061)
Highway	0.042	0.035	-0.007
	(0.021)	(0.012)	(0.023)
Urban	0.202	0.241	0.039
	(0.041)	(0.028)	(0.052)
Industrial area	0.606	0.417	-0.189**
	(0.051)	(0.032)	(0.060)
Main road	0.149	0.311	0.162**
	(0.037)	(0.031)	(0.053)
CEPSA	0.106	0.114	0.008
	(0.032)	(0.021)	(0.039)
Repsol	0.245	0.281	0.036
	(0.044)	(0.029)	(0.054)
Particular	0.191	0.232	0.040
	(0.040)	(0.028)	(0.051)
Competitors	6.563	4.172	-2.392***
	(0.410)	(0.287)	(0.518)
Cars	82791	70234	-12556
	(14768)	(10243)	(18560)
Trucks	15398	12916	-2482
	(2574)	(1790)	(3241)
Motorbikes	23462	22382	-1080
	(5878)	(4015)	(7305)

Notes. H_0: Equality of means between groups. Statistical significance at 1% (***), 5% (**) and 10% (*).

As is evident in this table, the two groups are largely similar and differ in very few characteristics: namely, the percentage of gasoline stations located in industrial areas and on main roads, and, the number of competitors to which the average gasoline stations belonging to the treated and control groups are exposed. All the estimations control for these differences in characteristics and so they do not interfere in the identification of the effect of deregulation on price.

4.3 Results

In this section I address a number of econometric questions and discuss the results of the estimations. First, the Breusch-Pagan/Cook-Weisberg test of the null hypothesis of constant variance indicated that there might be a problem of heteroscedasticity. Second, the Wooldridge test for autocorrelation in panel data showed that we might have a problem of serial autocorrelation. However, eventually, the correlation between variables proved not to be a concern for most of the variables following an analysis of the correlations and variance inflation factors (VIF), with the exception of the factors expressing demand which are detailed and discussed below. The correlation matrix between variables and the corresponding VIF are presented in Appendix C.

Estimation results of equation 1 are presented in Table 3. Column (1) reports estimates of the fixed effects estimation allowing for an arbitrary variance-covariance structure with standard errors clustered by id. This estimation not only allows me to control for unobservable factors influencing price evolution but also for those differences between stations and municipalities that do not vary over time. Recall that with this technique I also control for gasoline station characteristics and location, given that all these features are time invariant. Moreover, I avoid any estimation bias caused by correlation between the error term, as the estimation was performed with clustered standard errors by id (Bertrand et al. 2004).

The results show that the deregulation of entry has led to a lower equilibrium price of diesel. Competitors exposed to a new market entrant in an industrial area charge on average 1.70% per liter less than competitors not exposed to a new entrant. This result is economically significant as it represents one fifth of the average retail margin of gasoline stations. Moreover, a reduction of around 0.0122 euros per liter represents a saving for the whole of Spain of more than 254 million euros on diesel expenditure per year[1].

All the estimation results for the control variables work as expected. The coefficients for the time variant variables are also reported in also in Table 3. Variations in the international price are translated more than proportionally into the gasoline price. The number of competitors is also significant in explaining differentials: the equilibrium price falls with an additional competitor as the competition to attract consumers becomes more intensive. As for the factors expressing demand, the results are shown only for population and income per capita. The numbers of cars, motorbikes and trucks were excluded from the specification for two reasons: first, these variables were highly correlated with population, showing a VIF of 4189, 2636 and 237, respectively, and their estimated coefficients were unreliable; and, second, while I had population data for the whole period, data for these series from 2014 were missing. The results for population and income were significant and presented the expected sign, that is, an increase in the number of consumers means a reduction in the equilibrium price (market expansion effect) and wealthier areas are subject to higher retail gasoline prices.

Column (2) presents the results for the fixed effect estimation when considering only the competitors of unbranded entrants. As can be observed, the number of branded retailers that entered the market during the period was very low (2), so much so that this sub-sample differed from the original by just 10 gasoline stations. The results show that when an unbranded station enters the market, the price reduction is slightly higher. Indeed, according to the estimations, industrial areas with an unbranded entrant experience a reduction of 1.77% in price in comparison to the prices charged in areas without an entrant. This result provides support for previous evidence presented in Se05: an increase in the participation of unbranded retailers implies greater reductions in equilibrium price.

Finally, Column (3) reports the estimation results when considering only those gasoline stations exposed to one entrant. In this instance, the number of gasoline stations in the treated group falls from 90 to 54, given that 36 gasoline stations in the sample are subject to competition from more than one entrant. The results show that in those areas where just one entry was recorded following deregulation, the reduction in equilibrium price of the competitors was on average 1.39%. This indicates that the impact of the first entrant on the competitors price is greater than the impact of subsequent entrants; however it also show

[1] Considering a consumption of 20,910,000 tons at country level for the year 2013 as reported by AOP

that the decrease in price is intensified by the following entries.

Table 3: Estimation results

Variable	All sample	Only unbranded	One entrant
D(DiD)	**-0.0170*** **	**-0.0176*** **	**-0.0139*** **
	(-0.0044)	**(-0.0046)**	**(-0.0057)**
Int	1.034***	1.035***	1.036***
	(-0.009)	(-0.009)	(-0.01)
Competitors	-0.007***	-0.006***	-0.005***
	(-0.002)	(-0.002)	(-0.002)
Population	-3.23 $e^{-}07$*	-3.33 $e^{-}07$*	-3.73 $e^{-}07$*
	(-1.84 $e^{-}07$)	(-1.82 $e^{-}07$)	(-1.80 $e^{-}07$)
Income	2.19 $e^{-}05$*	2.02 $e^{-}05$*	2.003$e^{-}05$*
	(-1.16 $e^{-}05$)	(-1.16 $e^{-}05$)	(-1.21 $e^{-}05$)
Constant	-1.244***	-1.226***	-1.225***
	(-0.173)	(-0.174)	(-0.183)
Brand dummies	✓	✓	✓
Titularity change	✓	✓	✓
Month dummies	✓	✓	✓
Year dummies	✓	✓	✓
R^2	0.847	0.845	0.844
Joint significance test	2387.12***	2209.79***	1979.89***
Wooldridge test	22.332***	21.86***	19.46***
BreuschPagan /CookWeisberg test	326.78***	279.93***	276.54***
Observations	13,755	13,315	12,009
Ids	322	312	282

Notes. Fixed effects estimation, clustered standard errors by id. Statistical significance at 1% (***), 5% (**) and 10% (*). Wooldridge test H_0: No first $-$ order autocorrelation; Breusch $-$ Pagan /Cook $-$ Weisberg test H_0: Constant variance.

Table 4 explores the dynamic effect of the policy. Using the same controls as in the previous specifications and a fixed effect estimation with clustered standard errors by id, I estimate the effect on the equilibrium price of entry in the first six months following deregulation (March-August 2013), in the following six-month period (September 2013-February 2014), in the six-month period one year after deregulation (March-August 2014), and, during the last four months of the sample study (September-December 2014). The results show that the effect of the policy decreases over time. The average reduction in price during the first six months following deregulation was about 3.39%, while one year later, the reduction is estimated to be less than half that value, standing at about 1.45%.

Table 4: Dynamic effect of the reform

Variable	First 6 months	7-12 months	13-18 months	19-22 months
D(DiD) 0-6	**-0.0340***** **(-0.0057)**			
D(DiD) 7-12		**-0.0183***** **(-0.0047)**		
D(DiD) 12-18			**-0.0145***** **(-0.0043)**	
D(DiD) 18-22				**-0.0137***** **(-0.0051)**

Notes. Fixed effects estimation, clustered standard errors by id. Statistical significance at 1% (***), 5% (**) and 10% (*).

4.4 Robustness Checks

I adopt various strategies to check the robustness of my results.

First, I check the robustness of the results respect to the estimation methods. Table 5 shows, in columns (1) and (2), a pooled estimation of equation 1 and a fixed effects estimation with block bootstrap by id. For the pooled estimation a panel specific AR-1 autocorrelation structure and panel-level heteroscedastic error were assumed. Although this estimation is likely to give worse outcomes than the others due to a different structure of autocorrelation in the error term, it allows me to include control variables that might influence the price but which are invariant or vary very little over time, such as gasoline station characteristics and their location, and to check the significance of the estimators. The number of gasoline stations is lower in this estimation, because the characteristics of some gasoline stations are missing due to a lack of information. As observed, the results hold for both specifications, though the reduction in price due to deregulation for the pooled estimation is a little lower (c. 1.59%). This result might differ with respect to the other two estimations because the autocorrelation treatment is different and because of the lower number of observations in the pooled estimation.

Second, I test the robustness of the results with respect to the price measure used. Columns (3) and (4) present the results for the logarithm of gross price and margin. As expected, the percentage reduction in price is lower when using the gross price, though the results hold. The results of the price margin confirm that the effect of deregulation is to reduce retailer margins by around a fifth.

Third, I run a placebo test to check that the effect is only found when entry takes place. The placebo involves dropping all treated observations and assigning treatment randomly to controls. Then I re-estimate equation 1. In total, I have 87 new treated stations and 141 controls. As can be observed in Column (5), the variable of interest is not significant when the experiment is run with the control observations.

Table 5: Robustness checks

Variable	Pooled (1)	Bootstrap (2)	Gross Price (3)	Margin (4)	Placebo (5)
D (DiD)	**-0.0159***	**-0.0170***	**-0.0138***	**-0.185***	**0.0025**
	(-0.0028)	**(-0.0046)**	**(-0.0028)**	**(-0.0617)**	**(-0.0033)**
Controls	✓	✓	✓	✓	✓
Titularity change	✓	✓	✓	✓	✓
Brand dummies	✓	✓	✓	✓	✓
Month dummies	✓	✓	✓	✓	✓
Year dummies	✓	✓	✓	✓	✓
R^2	0.93	0.847	0.823	0.292	0.846
Observations	11,513	13,755	13,764	12,330	9,692
Ids	268	322	322	322	228

Notes. (1) Praise-Winsten corrected standard errors for AR-1 autocorrelation structure and panel-level heteroscedastic errors; (2)Fixed effects, block bootstrap by id. (3-5) Fixed effects estimation, standard errors clustered by id. Statistical significance at 1% (***), 5% (**) and 10% (*).

Finally, I check the robustness of the results with respect to the heterogeneous response of the control and treated groups to pre-existent differences. Here, the first concern might be that treated and control groups differ in pre-existent characteristics that might bias results. As reported in the data section, stations in the two groups differ in terms of the percentage of gasoline stations located in industrial areas and on main roads, and with respect to the number of competitors that each gasoline station has within a one-mile radius distance.

The second concern might be that there exist differences in preexistent characteristics conditioning price evolution between areas with entry and areas without entry. Specifically, areas with new entrants might differ in terms of their demand and supply factors and this, rather than deregulation, might account for differences in price evolution.

To overcome these concerns, I first perform matching procedures and estimate equation 1 with the observations that have common support. Matching procedures eliminate the potential bias by pairing gasoline stations subject to entry (treated group) with gasoline stations without entry (control group) with similar characteristics and exposed to the same level of demand and competition prior to deregulation. Hence, following Rosenbaum and Rubin (1983), in a first step I estimate the probability of being treated conditional on the pretreatment characteristics of the gasoline stations and demand of the area (z) and match treated and control gasoline stations regarding this estimated probability, known as the propensity score. This is Pr(z)= Pr($D = 1|z$).

I estimate the propensity score for each gasoline station using a logit regression with two different specifications. First, I estimate the propensity score conditional on the characteristics of gasoline stations that differed in the treated and control groups. The form of the estimation is the following:

$$P(D_i = 1|z) = \alpha + \beta_0 Z + \epsilon_i \tag{2}$$

where Z is a vector representing all the characteristics of the gasoline stations in the treated and control groups that present different means, that is, location in an industrial area, location on a main road, and, number of competitors.

The second specification I estimate calculates the probability of being treated conditional on the pre-existent level of demand of the areas in which the gasoline stations are located and the number of competitors. Specification 2 follows the same form as Specification 1, but in this case Z is a vector representing level of demand and number of competitors to which gasoline station i is exposed, including the number of cars, trucks and motorbikes for 2012 and the number of competitors of gasoline station i. In this specification I did not include the variables of location in an industrial area and on a main road, since according to Specification 1 they are not relevant for explaining the treatment. Finally, unlike the previous estimations, I included the number of cars, trucks and motorbikes instead of population. As I undertake a cross-sectional estimation, these disaggregated variables represent demand more accurately than population for the year 2012.

Having obtained the propensity score for both specifications, in each case I then matched the observations using the first-nearest neighbor algorithm; in other words, for every treated observation on common support the algorithm looks for the control observation with the closest propensity score. After matching each treated observation with its closest control, I dropped all remaining observations.

After this matching process, I was able to eliminate the potential bias due to differences in the characteristics between gasoline stations as well as that due to differences in demand and the number of competitors. The results for the logistic regressions and mean differences test between groups are presented in Appendix D.

In addition, I adopted a second strategy to check the robustness of results regarding the heterogeneous response to pre-existing differences between the treated and control groups. This strategy involved keeping the treated gasoline stations during a shorter period of time. Hence, in this sub-sample I only have competitors near industrial areas where entry took place after deregulation. For this reason, I would expect the areas to be similar with regard to demand drivers and market concentration.

For this sub-sample, the treated and control groups were constructed as follows. I classified gasoline stations according to the period in which entry occurred. I retained as my treated group all the competitors exposed to a market entrant in the first eight months after deregulation, and, I built my counterfactual with all the gasoline stations that were not exposed to an entrant in those first eight months, but which were exposed to an entry in the following eight-month period. In total, I have 42 treated and 36 control units for a period of time of eight months pre- and post-deregulation. The pre-deregulation period extends from June 2012 to January 2013, and the post-deregulation period from March to October 2013. Monthly dummies were included to control for seasonality. Overall, the sub-sample comprises 1,248 observations.

The results for both strategies are presented in Table 6. Column (1) reports estimates for Specification 1 of the matching strategy, Column (2) presents the results for Specification 2, and Column (3) for the restricted sample strategy.

As can be seen, the results are robust to every specification. The effect of deregulating the market is a reduction in the gasoline retail equilibrium price, even after controlling for heterogeneous responses due to differences in gasoline station characteristics, demand across areas and the number of competitors across gasoline stations. The matching procedure samples report a decrease in the equilibrium price due to deregulation of 1.24% and 1.14%, depending on the specification, while the restricted sample strategy estimates a 2.84% decrease in price. The differences between the strategies are attributable to the differences in the period of time analyzed for each strategy. While the matching samples cover the 22 months pre- and post-reform, the restricted sample only analyzes differences in the price evolution of the treated and control groups during the first 8 months pre- and post-reform. Recall that Table 4 shows that the effect of the reform is decreasing over time.

Table 6: Robustness checks. Matching and restricted sample estimation results

	Matching		Restricted
Variable	(1)	(2)	(3)
D (DiD)	**-0.0124****	**-0.0114****	**-0.0284*****
	(-0.0048)	**(-0.0047)**	**(-0.00745)**
Controls	✓	✓	✓
Brand dummies	✓	✓	✓
Month dummies	✓	✓	✓
Year dummies	✓	✓	✓
R^2	0.838	0.848	0.821
Joint significance test	1289***	1445***	262.43***
Wooldridge test	12.51***	12.04***	52.95***
BreuschPagan /CookWeisberg test	175.33***	318.23***	96.11***
Observations	7,664	7,977	1,239
Ids	180	188	78

Notes. (1) and (2) samples selected by matching procedures; Specifications 1 and 2, respectively. Restricted: restricted sample using only 8 months before and after deregulation. All results are from a fixed effects estimation with standard errors clustered by id. Statistical significance at 1% (***), 5% (**) and 10% (*). Wooldridge test H_0: No first $-$ order autocorrelation; Breusch $-$ Pagan /Cook $-$ Weisberg test H_0: Constant variance.

5 Conclusions

In this article I have estimated the effect of entry restrictions on retail gasoline equilibrium prices by exploiting a quasi-experimental environment created by a change in Spanish regulations. In February 2013, a Central Government reform allowed gasoline stations to operate in industrial and commercial areas. This deregulation led to a high number of new market entrants over the following two years in these newly designated industrail 'free entry' areas.

I have adopted a difference-in-difference approach, applied to a geo-referenced database, to assess the effect of deregulation on the equilibrium price. I also provide evidence of the robustness of the results reported by implementing several estimation techniques, with different price measures and performing a placebo test. The results show that prices fell by about 1.7% as a result of the deregulation of entry. This represents a saving of more than 254 million euros in annual expenditure on diesel alone.

Moreover, these results hold when controlling for heterogeneous responses due to pre-existent differences in gasoline stations between groups and between areas. By applying matching procedures, I report that gasoline retail prices were at least 1.2% lower for gasoline stations exposed to entry than for those not exposed to new entrants following deregulation. In a second strategy, in which I compared only treated stations with future treated stations during a shorter period of time, I obtained a price decrease due to deregulation of around 2.84%. However, the difference between the samples is attributable to differences in the time period analyzed, since the impact of the reform proved to be higher in the first few months and to be decreasing in time thereafter.

The results show that the marginal impact on price attributable to entry falls with the number of entrants. In other words, the first entrant causes a greater impact than subsequent market entrants, and that this same dynamic is associated with the impact of the reform over time. In the first six months following deregulation, gasoline stations exposed to a new entrant charge on average 3.4% less than their matched pair not subject to exposure. However, one year later the effect had been almost halved, with the price charged being just 1.45% less.

The results reported here are also very much in line with those found in Sen (2005), that is, the impact on prices is higher when the entrant is an unbranded station.

Additionally, and as expected, my empirical results are consistent with modeling the demand for gasoline as a discrete choice model using the logit, in line with Houde (2012). As shown by Anderson and Palma (1992), when modelling demand with the logit, a new market entry leads to a decrease in the average retail price. In this sense, I can rule out the existence of significant search costs in the gasoline retail market and a strong consumer preference for a particular gasoline station or brand. Although there is a differentiation of product geographically, the difference between stations is not great enough for gasoline stations to behave as monopolies.

The results presented here are, I believe, not only of interest for gasoline retail markets, but they should also be particularly informative for public policy makers concerned with other sectors that still operate restrictions on market entry. In particular, the results should be of fundamental importance to the grocery sector which continues to be characterized by a highly regulated access, given its impact on family expenditure.

Finally, as discussed, market entry following deregulation has been primarily of unbranded retailers. As such, I expect regulation has not only affected equilibrium prices but also market structure. Therefore, future lines of research could usefully assess the effect of market entry regulations on the structure of the gasoline retail market and on social welfare.

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Appendix A Parallel trends identifying assumption

Table A.1: Parallel trends test. Estimation results. Part 1.

Variable	Coef.	Robust St. Err.	z	P>| z |
treated march 2011	0.118	0.0035	33.46	0.000
control march 2011	0.118	0.0026	45.31	0.000
treated april 2011	0.126	0.0035	36.20	0.000
control april 2011	0.126	0.0026	48.47	0.000
treated may 2011	0.091	0.0035	26.33	0.000
control may 2011	0.090	0.0026	34.89	0.000
treated june 2011	0.095	0.0035	27.40	0.000
control june 2011	0.093	0.0026	36.30	0.000
treated july 2011	0.095	0.0035	27.56	0.000
control july 2011	0.093	0.0025	36.60	0.000
treated september 2011	0.108	0.0035	31.35	0.000
control september 2011	0.106	0.0025	41.82	0.000
treated october 2011	0.112	0.0035	32.43	0.000
control october 2011	0.109	0.0025	42.93	0.000
treated november 2011	0.133	0.0034	38.62	0.000
control november 2011	0.129	0.0025	51.23	0.000
treated december 2011	0.122	0.0034	35.39	0.000
control december 2011	0.119	0.0025	47.13	0.000
treated january 2012	0.149	0.0034	43.23	0.000
control january 2012	0.119	0.0025	47.13	0.000
treated february 2012	0.156	0.0034	45.25	0.000
control february 2012	0.151	0.0025	60.12	0.000
treated march 2012	0.176	0.0034	51.13	0.000
control march 2012	0.167	0.0025	66.50	0.000
treated april 2012	0.188	0.0034	54.80	0.000
control april 2012	0.183	0.0025	72.84	0.000
treated may 2012	0.169	0.0034	49.15	0.000
control may 2012	0.164	0.0025	65.23	0.000
treated june 2012	0.128	0.0034	37.57	0.000
control june 2012	0.124	0.0025	49.09	0.000
treated july 2012	0.157	0.0034	46.02	0.000
control july 2012	0.152	0.0025	60.38	0.000
treated august 2012	0.209	0.0034	61.18	0.000
control august 2012	0.206	0.0025	81.66	0.000

Notes: All control variables included. Praise-Winsten corrected standard errors for AR-1 autocorrelation structure and panel-level heteroscedastic errors.

Table A.2: Parallel trends test. Estimation results. Part 2.

Variable	Coef.	Robust St. Err.	z	P>| z |
treated september 2012	0.223	0.0034	65.42	0.000
control september 2012	0.22	0.0025	86.83	0.000
treated october 2012	0.21	0.0034	62.64	0.000
control october 2012	0.21	0.0025	82.48	0.000
treated november 2012	0.179	0.0034	52.09	0.000
control november 2012	0.174	0.0026	68.09	0.000
treated december 2012	0.166	0.0034	48.26	0.000
control december 2012	0.162	0.0026	62.89	0.000
treated january 2013	0.181	0.0035	52.37	0.000
control january 2013	0.178	0.0026	69.45	0.000
treated march 2013	0.179	0.0035	51.48	0.000
control march 2013	0.179	0.0026	69.62	0.000
treated april 2013	0.146	0.0036	40.65	0.000
control april 2013	0.146	0.0026	56.49	0.000
treated may 2013	0.138	0.0037	37.78	0.000
control may 2013	0.135	0.0026	52.51	0.000
treated june 2013	0.140	0.0038	36.43	0.000
control june 2013	0.133	0.0025	52.59	0.000
treated july 2013	0.161	0.004	39.98	0.000
control july 2013	0.158	0.0025	64.48	0.000
treated august 2013	0.163	0.004	39.78	0.000
control august 2013	0.162	0.0024	66.70	0.000
treated september 2013	0.180	0.004	44.99	0.000
control september 2013	0.180	0.0024	74.86	0.000
treated october 2013	0.158	0.0043	37.10	0.000
control october 2013	0.160	0.0024	67.21	0.000
treated november 2013	0.143	0.0064	22.32	0.000
control november 2013	0.146	0.0024	61.58	0.000
treated december 2013	0.143	0.008	18.30	0.000
control december 2013	0.153	0.0024	65.07	0.000
treated january 2014	0.137	0.0088	15.60	0.000
control january 2014	0.144	0.0023	61.65	0.000
treated february 2014	0.140	0.0097	14.32	0.000
control february 2014	0.143	0.0023	61.85	0.000
treated march 2014	0.128	0.0103	12.46	0.000
control march 2014	0.133	0.0023	58.46	0.000
treated april 2014	0.130	0.0121	10.74	0.000
control april 2014	0.131	0.0022	59.07	0.000
treated may 2014	0.129	0.0131	9.83	0.000
control may 2014	0.207	0.0032	64.16	0.000

Notes: All control variables included. Praise-Winsten corrected standard errors for AR-1 autocorrelation structure and panel-level heteroscedastic errors. $R^2 = 0.9456$.

Table A.3: Hypothesis test: $H_0 : treated_t - control_t = 0$

Variable	chi2	$Prob > chi2$	H_0 vs. H_a
march 2011	0.00	0.9679	H_0
april 2011	0.00	0.9922	H_0
may 2011	0.11	0.7444	H_0
june 2011	0.30	0.5838	H_0
july 2011	0.42	0.5167	H_0
september 2011	0.49	0.4840	H_0
october 2011	1.02	0.3123	H_0
november 2011	1.29	0.2554	H_0
december 2011	0.77	0.3790	H_0
january 2012	1.29	0.2553	H_0
february 2012	1.59	0.2076	H_0
march 2012	6.41	0.0114	H_a
april 2012	2.25	0.1337	H_0
may 2012	1.76	0.1851	H_0
june 2012	2.07	0.1505	H_0
july 2012	2.26	0.1330	H_0
august 2012	0.70	0.4016	H_0
september 2012	1.08	0.2995	H_0
october 2012	1.58	0.2086	H_0
november 2012	1.60	0.2057	H_0
december 2012	1.25	0.2633	H_0
january 2013	0.53	0.4685	H_0
march 2013	0.00	0.9441	H_0
april 2013	0.00	0.9846	H_0
may 2013	0.93	0.3353	H_0
june 2013	3.17	0.0752	H_0
july 2013	0.39	0.5304	H_a
august 2013	0.15	0.7021	H_0
september 2013	0.00	0.9449	H_0
october 2013	0.33	0.5643	H_0
november 2013	0.27	0.6059	H_0
december 2013	1.67	0.1960	H_0
january 2014	0.57	0.4508	H_0
february 2014	0.12	0.7307	H_0
march 2014	0.22	0.6373	H_0
april 2014	0.01	0.9338	H_0
may 2014	0.03	0.8566	H_0

Appendix B The data

Figure B.1: Metropolitan Area of Barcelona

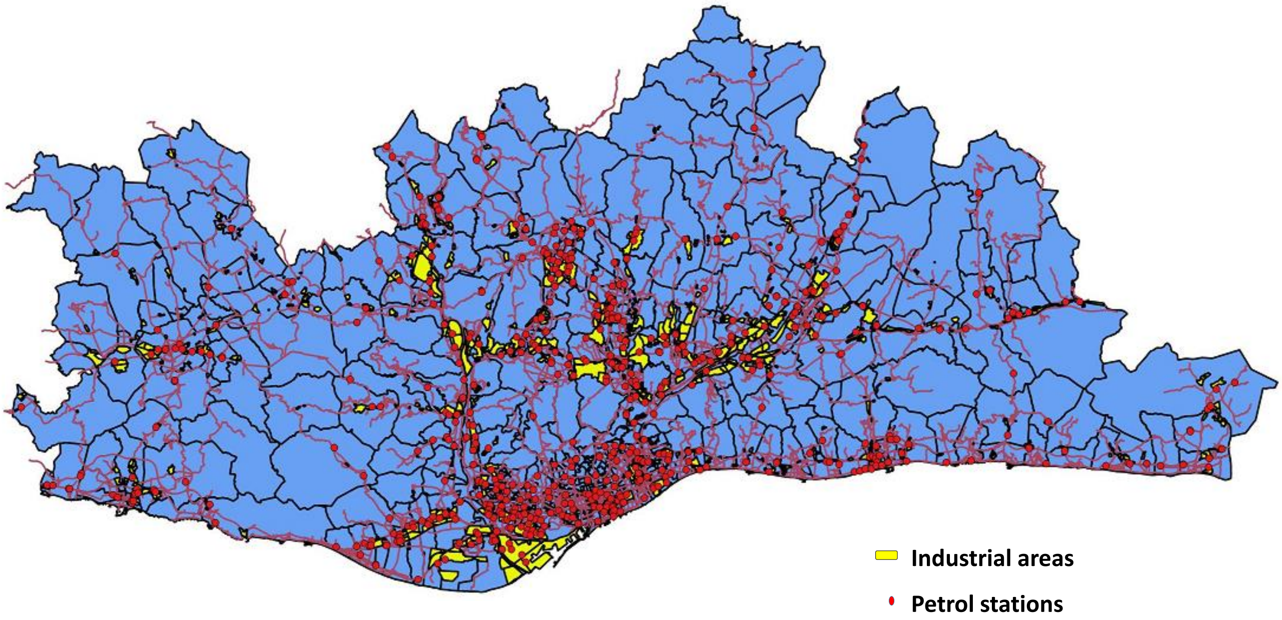
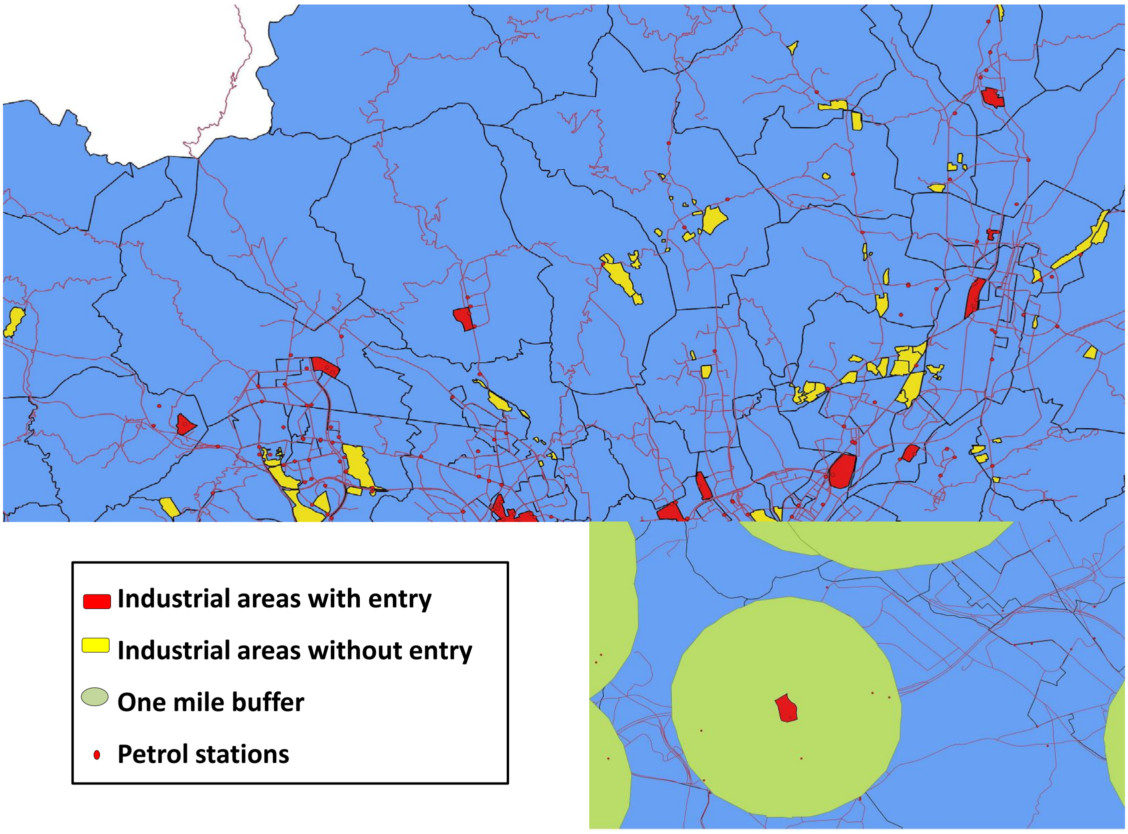


Figure B.2: Industrial areas and treated and control groups construction



Appendix C Correlation between variables

Table C.1: Correlation matrix

Variable	lprice	int	store	comp	coffe	road	pop	ind	high	wash	24	pumps	D	Y	mbik	car	truck	change
lprice	1																	
int	**0.8543**	1.0000																
store	0.0878	0.0044	1.0000															
comp	-0.0950	-0.0224	-0.0547	1.0000														
coffe	0.0529	0.0036	0.1719	-0.1491	1.0000													
road	0.0615	-0.0005	0.1938	-0.3589	0.1231	1.0000												
pop	0.0679	0.0042	-0.0692	0.3875	-0.0381	-0.1767	1.0000											
ind	-0.0954	-0.0003	-0.2113	0.3204	-0.1428	**-0.5272**	0.0771	1.0000										
highway	0.0677	-0.0007	0.0848	-0.0532	0.1177	-0.1310	-0.0485	-0.2046	1.0000									
wash	0.0224	0.0018	0.1932	-0.0546	0.0507	0.0745	-0.0295	0.0994	-0.1051	1.0000								
24 hs	0.0226	-0.0031	0.1478	0.0331	-0.0215	-0.0459	0.0434	-0.1351	0.1788	0.0542	1.0000							
pumps	0.0720	0.0042	0.2428	-0.0033	0.1260	0.0432	0.0382	-0.0527	0.2145	0.0357	0.1848	1.0000						
D	-0.3409	-0.2750	0.0067	0.2284	-0.0501	-0.0843	-0.0296	0.0980	-0.0156	0.0032	0.0065	-0.0494	1.0000					
Y	0.0621	0.0006	-0.0105	0.0667	-0.0779	-0.0161	**0.5723**	-0.0673	-0.0104	0.0502	0.0519	-0.0013	-0.1158	1.0000				
mbike	0.0743	0.0029	-0.0690	0.3209	-0.0241	-0.1472	**0.9941**	0.0622	-0.0543	-0.0236	0.0365	0.0378	-0.0443	**0.5981**	1.0000			
cars	0.0677	0.0065	-0.0714	0.3963	-0.0386	-0.1808	**0.9990**	0.0833	-0.0503	-0.0266	0.0422	0.0369	-0.0254	**0.5614**	**0.9911**	1.0000		
trucks	0.0659	0.0081	-0.0719	0.4132	-0.0404	-0.1857	**0.9970**	0.0884	-0.0504	-0.0280	0.0390	0.0367	-0.0217	**0.5481**	**0.9860**	**0.9992**	1.0000	
change	-0.0735	-0.0734	0.0158	-0.0876	-0.002	0.0333	-0.0498	0.0454	-0.027	0.0188	0.0906	-0.0686	-0.0373	-0.049	-0.0418	-0.0515	-0.0532	

Table C.2: Variance Inflation Factor

Variable	lprice	road	pop	Y	mbik	car	truck
lprice	–	–	–	–	–	–	–
int	1	–	–	–	–	–	–
road	–	–	–	–	–	–	–
pop	–	–	–	–	–	–	–
ind	–	1	–	–	–	–	–
Y	–	–	1.92	–	–	–	–
mbik	–	–	240.94	–	237.44	770.07	770.07
car	–	–	4218.70	4189.41	43.64	–	43.64
truck	–	–	2678.02	2636.80	69.34	69.34	–

Note: Tolerance rule $VIF < 10$

Appendix D Robustness checks

Table D.1: Logistic regression. Probability of having an entrant after deregulation

	Specification 1				Specification 2			
Variable	Coeff.	Std. Dev.	z	$P > \|z\|$	Coeff.	Std. Dev.	z	$P > \|z\|$
Industrial area	0.36	0.30	1.19	0.234	–	–	–	–
Main road	-0.034	0.39	-0.87	0.38	–	–	–	–
Competitors	0.105	0.030	3.47	0.001	0.994	0.39	2.52	0.12
Trucks	–	–	–	–	-0.0005	0.0002	-2.34	0.019
Motorbikes	–	–	–	–	-0.0001	0.00004	-2.93	0.003
Cars	–	–	–	–	0.0001	0.00005	2.57	0.010

Note: All dependent variables correspond to year 2012, before deregulation.

Table D.2: Matching samples. Mean differences in February 2013

	Specification 1			Specification 2		
Variable	Treated	Control	Diff			
Industrial area	0.606	0.417	-0.189	0.61	0.5	-0.11
	(0.051)	(0.032)	(0.060)	(0.052)	(0.053)	0.074
Main road	0.149	0.128	-0.021	0.156	0.189	0.033
	(0.037)	(0.035)	(0.051)	(0.038)	(0.041)	(0.056)
Competitors	6.564	7.276	0.713	6.2	7.03	0.83
	(0.410)	(0.467)	(0.622)	(0.38)	(0.52)	(0.64)