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 to test the effect of the relaxation of entry restrictions on the equilibrium retail price of diesel. In February 2013, a Central Government reform permitted gasoline stations to operate in industrial and commercial areas. Over the following 2-year period, this deregulation led to a high number of new market entrants in these newly designated free entry areas. By isolating markets exposed to entry and markets unaffected by new entrants, and adopting a difference-in-difference approach, results show that gasoline stations exposed to a new market entrant within a one-mile radius lower their prices by an average 1.04%. This result is significant, representing almost one fifth of the average retail margin. Additionally, the results show that the reduction in the equilibrium price is caused by the first market entrant and that the effect decreases over time.

Keywords Gasoline retail market · Entry restrictions · Price competition · Public policy evaluation · Difference in difference

JEL Classification L71 · L51 · D43

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1 Introduction

Retail gasoline markets around the world are characterized by imperfect competition and retail gasoline and diesel 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 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 to other kinds of 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 to summarize 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 (Fenili and Lane 1985; Anderson and Johnson 1999; Johnson 1999; Skidmore et al. 2005; Carranza et al. 2015), bans on self-service stations (Johnson and Romeo 2000), divorcement laws as a means for preventing predation from the refiner-owned service stations against their franchised dealers (Barron and Umbeck 1984; Vita 2000), sales taxes (Doyle and Samphantharak 2008), and auctioning licenses to operate stations (Soetevent et al. 2014). However, to date, no studies have examined the effect of entry restrictions nor their impact on the market once they are lifted. Moreover, though 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. Increasing prices with entry are explained by search costs (Satterthwaite 1979; Stiglitz 1987; Schulz and Stahl 1996) while decreasing prices are expected in models of spatial competition (Gabszewicz and Thisse 1980; Anderson and Palma 1992). In the middle, more than one paper has reported mixed results (Salop 1979; Chen and Riordan 2007; Janssen and Moraga-González 2004).

In the empirical literature, a number of related papers have analyzed the effect of market structure on retail gasoline 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 3-year panel of stations located in suburban 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.

Relaxing entry restrictions constitutes a potential policy for tackling concerns about diesel prices; however, the effects of such a policy remain unexplored. In this paper, I seek to fill this gap by empirically analyzing the effect that entry restrictions have on equilibrium prices. Additionally, I expand on previous related articles by analyzing an external shock generated by a public policy decision in urban areas. This article does not as such attempt to shed light on the relationship between the number of competitors and price, rather its objective is to provide evidence as to the way in which the market responds to an additional entry.

In addressing this question, I use a change in Spanish regulations introduced in February 2013. The market entries attributable to this regulatory change provided me with a 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 identify as treated stations all gasoline stations within a one-mile radius of an area where the deregulation was active and entry took place, and use as a control group all stations located in a one-mile radius of areas that were deregulated, but that did not experience entry. I conclude that removing barriers to entry implies a reduction in retail gasoline price of 1.04%. This result is significant, representing almost one fifth of the average retail margin. The results also show that the equilibrium price reduction is caused by the first entry and that this effect decreases over time.

I test the robustness of my results for the estimation techniques, different price measures, heterogeneous response due to pre-existing differences in the treated and control groups and different geographical size markets (one- and two-mile radiuses). Additionally, I perform a placebo test and analyze the dynamic effects of the reform.

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. The rest of the paper is organized as follows. Section 2 offers an overview of the retail gasoline market in Spain and the policy reform. Section 3 provides information on the data set. Section 4 reports the identification strategy. The results and robustness checks are presented in Sects. 5 and 6. Finally, the article ends by drawing a number of conclusions.
2 Background to the policy reform

In this section I present the context in which the policy reform took place and describe the policy itself. The Spanish retail gasoline market began its operations around 1930. From that date to 1984, it remained under full government control and under the management of the public company CAMPSA. In 1984, a period of transition was ushered in, until the markets eventual liberalization in 1992. In that year, REPSOL (a public firm which would later be privatized) took over all the activities that had previously formed part of CAMPSA’s monopoly, except that is for the oil pipe network and a number of CAMPSA service stations that were transferred to the only two private firms then operating in the Spanish oil market, CEPSA and British Petroleum (BP). From this juncture onwards, these three firms are considered jointly as the incumbents.

Although the oil market was liberalized in 1992, competition in the retail gasoline market was not especially intense because of the asymmetries that existed between the competitors. To address this situation, various liberalization and competition-oriented measures were introduced in the years that followed. Thus, in the service station network, gasoline prices were fully liberalized in 1996, and, diesel price liberalization and unrestricted network access to third parties were introduced in 1998, the year in which Spain’s Energy Regulator was created. In 2000, daily price reporting from gasoline stations was made mandatory, and restrictions on the opening of new stations were imposed at the provincial level on companies with a market share greater than 30% and on those with a 15–30% share. Similarly, large commercial areas were allowed to house a gasoline station. Finally, the last measure to be introduced prior to February 2013 was the lifting of the minimum distance restrictions imposed between stations in 2001.

Table 1 shows the market shares of the principal competitors in 1995 and in the period 2010–2014, as well as the total number of stations each of them operated. As can be observed, the number of stations has experienced constant growth over the last 20 years. Indeed, between 1995 and 2014 this growth was estimated at about 70%. Among the incumbents, REPSOL’s market share fell by 22 percentage points and CEPSA’s 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 supply as measured by market share in terms of the number of stations. This means that 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 retail gasoline market.

In short, before the policy reform examined in this study, the Spanish market had experienced constant growth in the number of gasoline stations mainly due to new unbranded competitors. As Table 1 shows, however, 2012 was an exception to this general pattern. The number of unbranded competitors actually fell as they lost 1% of market share. Following the policy reform, though, there was a recovery in this upward growth trend and it even became more accentuated.

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 econ-
Table 1  Spanish retail gasoline market

| Brand         | 1995 | 2010 | 2011 | 2012 | 2013 | 2014 |
|---------------|------|------|------|------|------|------|
| Repsol        | 55%  | %    | 35%  | 35%  | 34%  | 33%  |
| Cepsa         | 24%  | 14%  | 14%  | 15%  | 14%  | 14%  |
| BP            | 6    | 6%   | 7%   | 6%   | 6%   | 6%   |
| Galp          | 2%   | 6%   | 6%   | 6%   | 6%   | 5%   |
| Disa (shell)  | 1.6% | 5%   | 5%   | 5%   | 5%   | 5%   |
| Other wholesalers | –   | –    | –    | 5%   | 5%   | 6%   |
| Supermarkets  | –    | 3%   | 3%   | 3%   | 3%   | 3%   |
| Unbranded     | –    | 18%  | 17%  | 16%  | 18%  | 20%  |
| Cooperatives  | –    | 6%   | –    | 6%   | 6%   | 5%   |
| Total         | 6327 | 10,238 | 10,309 | 10,424 | 10,617 | 10,712 |

Share by brand and total number of stations. 1995; 2010–2014
1995 data from Cavero and Bello (2007); 2010–2014 data from Spanish Association of Operators of Oil Products (AOP)

Among these reforms, the law regulating the hydrocarbon sector (Law 34/1998) 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. This meant that from that moment on no authorization to entry to these areas from the local authorities was needed. Before deregulation, in common with other municipal areas, these areas were subject to zoning restrictions. Although entry was not forbidden per se, gasoline stations needed to seek authorization from local governments whose response depended upon their urban planning regulations.

Deregulation, however, had a great impact on entry to industrial areas: in the 20 months following the change 38 new gasoline stations entered the market, while in the previous 2 years the number of entrants to industrial areas had been just 18. Clearly, the policy reform was of great importance in the decision to enter these areas.

As for the vertical agreements, the Decree-Law established a 1-year contract duration, renewable for a maximum period of 3 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. However, as retail price setting by the wholesaler/refinery is incompatible with Article 101 of the European Union Treaty, this practice had not been allowed in Spain since 2001, following the termination of the oil monopoly. As such, the 2013 regulation should not constitute a real change in price setting mechanisms.

The regulations modifying vertical contracts and prohibiting wholesalers from setting the price are only applicable to gasoline stations whose land or buildings are not owned by the oil company (i.e. the facility is dealer owned). Thus, considering that all the stations in which the dealer report their daily price to the Ministry are dealer
owned, whether they are REPSOL, BP or CEPSA branded, this gives a sample of 48 gasoline stations. Of these, approximately two thirds belong to the control group. This suggests that, if the change in vertical contracts also affects pricing, the fall in prices recorded after entry would be downward biased.

Finally, in the case of the possibility of ending vertical contracts, I control for brand changes in the price estimations. Yet, there is only one station that actually shifts from being branded to unbranded after the deregulation in the treated group and three in the control group. Here again, if the control is insufficient, the bias of the entry effect would be downward.

3 The data

I assess the impact of market entry deregulation on diesel retail prices in Spain by drawing on data from the Metropolitan Area of Barcelona. My data set contains information for 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 webpage, where it provides daily price information for geo-referenced gasoline stations for the whole of Spain. No historical data are stored on the site so prices had to be downloaded each day to create the data set. The Ministries records 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. I constructed treated and control groups in the free, open-source geographic information system QGIS. Here, I delimited the one-mile radius influence zones around the industrial areas. 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 the margin. 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 cases, one or two); and, second, because it would mean including locations with lower prices for the

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1 I do not count gasoline stations contracts.
2 The margin variable was constructed by subtracting the international price from the net price as a proxy for the wholesale price.
entire period and give rise to problems of self-selection in the sample. Nevertheless, results hold when considering these latter stations. I do, however, take into account all these excluded stations when calculating the degree of competition faced by each gasoline station in the sample over time. The competitor variable changes over time according to the entries and exits that occurred in the market.

I use monthly data, primarily, given the variability and availability of the control variables. As most of the controls vary on a yearly basis, I expect them to work better if the dependent variable is expressed on a monthly rather than on a daily basis. Additionally, I perform a robustness check with the margin, which is calculated (on a monthly basis) using the international price of diesel as a proxy for the wholesale price, which supports my decision. Nevertheless, I control for the robustness of results using daily data. Overall, I have data for 392 gasoline stations. Of these, 164 belong to the treated group and 228 to the control group. The period under analysis extends over a 22-month period, both before and after the reform was introduced. 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 16,775 observations.

Information on the international price of diesel was downloaded from Spain’s Antitrust Institution (CNMC). This 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 Council’s 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.

The characteristics of the gasoline stations studied herein include a group of dummy variables indicating the presence of a coffee shop, store and carwash, whether the gasoline station is open 24 h, located 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 also use the number of pumps, the brand, and the number of competitors that they face in the geographical market (within a one- or two-mile radius). The data were built using Google Earth and the oil companies websites, except for the brand and change of brand variables, which were taken from the Ministry’s records, and the number of competitors that was constructed in Matlab.

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).

As the table makes clear, the two groups are largely similar, only differing a small number of characteristics: namely, the percentages of gasoline stations located in industrial areas and on main roads, the number of unbranded stations, 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 with the identification of the effect of deregulation on price. Unobservables conditioning price evolution from pre-existent differences are taken into account in the section reporting the outcomes of the robustness checks (6).

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3 It includes 22 months, as data for August 2011 was not available.
| Variable             | Treated 0.897     | Control 0.86  | Diff −0.039  |
|----------------------|-------------------|--------------|--------------|
|                      | (0.024)           | (0.024)      | (0.035)      |
| Coffee shop          | 0.27              | 0.33         | 0.059        |
|                      | (0.035)           | (0.033)      | (0.05)       |
| Carwash              | 0.49              | 0.545        | 0.055        |
|                      | (0.04)            | (0.035)      | (0.054)      |
| Pumps                | 6.47              | 6.388        | −0.084       |
|                      | (0.19)            | (0.183)      | (0.26)       |
| 24 h                 | 0.454             | 0.454        | −0.0002      |
|                      | (0.04)            | (0.033)      | (0.051)      |
| Highway              | 0.061             | 0.035        | −0.026       |
|                      | (0.019)           | (0.012)      | (0.021)      |
| Urban                | 0.22              | 0.24         | 0.022        |
|                      | (0.032)           | (0.028)      | (0.043)      |
| Industrial area      | 0.55              | 0.42         | −0.14**      |
|                      | (0.04)            | (0.033)      | (0.051)      |
| Main road            | 0.16              | 0.31         | 0.15***      |
|                      | (0.031)           | (0.031)      | (0.053)      |
| CEPSA                | 0.091             | 0.114        | 0.023        |
|                      | (0.023)           | (0.021)      | (0.031)      |
| Repsol               | 0.27              | 0.281        | 0.012        |
|                      | (0.034)           | (0.029)      | (0.046)      |
| Unbranded            | 0.16              | 0.232        | 0.087**      |
|                      | (0.029)           | (0.028)      | (0.041)      |
| Supermarket          | 0.024             | 0.018        | −0.007       |
|                      | (0.12)            | (0.009)      | (0.014)      |
| Competitors          | 6.9               | 4.172        | −2.73***     |
|                      | (0.4)             | (0.287)      | (0.46)       |
| Cars                 | 760,031           | 70,234       | −5768        |
|                      | (13,038)          | (10,243)     | (17,141)     |
| Trucks               | 13,052            | 12,916       | −136         |
|                      | (1799)            | (1790)       | (2605)       |
| Motorbikes           | 21,645            | 22,382       | 736          |
|                      | (5165)            | (4015)       | (6740)       |
| Max. Obs.            | 164               | 228          | –            |

Mean differences in February 2013

$H_0$: equality of means between groups. Statistical significance at 1% (***) 5% (**) and 10% (*). Max. Obs. refers to the maximum of observations by group. Some gasoline stations characteristics are not available for the 392 stations, so in those cases the t test was calculated with less observations.

4 Identification and estimation methods

The objective of this study 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 prices charged by gasoline stations competing with the entrant in specified areas. Taking into account that the geographi-
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...cally relevant retail gasoline market is generally considered in the literature to be local (one or two miles being the most frequent measures) and, following Hastings (2004), I consider all gasoline stations located in a one-mile radius of an industrial area. I also test whether these results change when applying a two-mile radius. Ideally, I should employ 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 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 greater availability of land and the lower costs to entry in industrial areas. Moreover, commercial areas are, generally, more occupied and tend to be located more centrally, that is, in districts where a new gasoline station would meet with strong opposition from people in the neighborhood. Finally, an earlier regulation in 2000 encouraged large retailers to open gasoline stations, which meant part of the commercial areas had already, in part, been liberalized. This means that here I only consider industrial areas with market entrants and industrial areas without any entrants following deregulation, and, therefore, I include gasoline stations exposed to competition due to deregulation and stations that have not been exposed to the same competition.

To avoid the concern that industrial areas with new market entrants may differ in some respects from industrial areas without, 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 step, I applied matching procedures. 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 the retail prices of gasoline.

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

$$p_{itj} = \beta_0 + \beta_1 D_{it} + \beta_2 x_{it} + \beta_3 x_{jt} + \sigma_i + \lambda_t + \epsilon_{it}$$

(1)

where $p_{itj}$ is the logarithm of the monthly price of diesel charged by gasoline station $i$ located in municipality $j$ in month $t$; $D_{it}$ 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 time $t$; $x_{it}$ is a vector of control variables that vary by gasoline station and time; $x_{jt}$ is a vector of control variables that vary by municipality and time; $\sigma_i$ are the gasoline stations fixed effects; $\lambda_t$ are time fixed effects; and, $\epsilon_{it}$ is a gasoline station time-varying error

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4 Competitors of an entrant within the industrial area are also included as treated stations.
5 Due to the urban nature of the geographical space under analysis, the sample is too small to test the robustness of results to other geographical distances, such as five- or ten-mile radiuses.
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, $D_{it}$ does 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, $D_{it}$ takes a value of 1 after gasoline station $i$ is exposed to an entrant from time $t$ and onwards.

Besides controlling for common shocks on a monthly basis with $\lambda_t$, and, for time invariant characteristics of the gasoline stations with $\sigma_i$, I also include a vector of controls by gasoline station $x_{it}$ that incorporates the number of competitors that each gasoline station faces in time $t$, whether the gasoline station $i$ experiences a change in ownership during the entire period, and, brand dummies. Finally, I control for municipality characteristics that vary across time and that are related to the size of the market for each gasoline station. In line with previous studies, I include population; number of cars, trucks and motorbikes; and, gross family income per capita (Vita 2000; Skidmore et al. 2005). Municipality dummies could not be included because of collinearity problems. However, I estimate a second specification in which I include time trends by municipality.

In this specification, I do not include the gasoline stations characteristics, that is, the group of dummy variables indicating the presence of a coffee shop, store and carwash, whether the gasoline station is open 24 h, and whether it is located in an urban or industrial area, or on a main road or highway. The reason for excluding these variables is that they are all time invariant, and their effect on prices is captured by the fixed effects. However, these variables are used for characterizing the treated and control groups and for the estimations without fixed effects.

The estimate of interest in the model, $\beta_1$, represents the difference-in-difference effect of deregulation of entry on retail diesel 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. First, Fig. 1 shows that the average price in treated and control groups are almost equal before deregulation. Second, I perform a mean test by time and the results show that I cannot reject the equality in means between groups prior to February 2013 (see Fig. 2).

Finally, an error correlation is expected both temporally and at the cross-section level, which introduces a bias into the results. In such cases, as Bertrand et al. (2004) show, considering an autocorrelation structure of the error term of degree 1 might not be enough to overcome the problem. Hence, 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 and by collapsing the data set in one period pre- and one period post-deregulation.

Next, I also estimate the impact of each entry into an industrial area. That is, as some areas in the sample are exposed to more than one entry, I disaggregate the effect of the first entry and the marginal impact of a second or third entry to the same area. To do so, I estimate the following equation:
where $D1_{it}$ is a dummy variable that takes a value of one when gasoline station $i$ is a competitor of a first entrant in a deregulated area in time $t$ and onwards; $D2_{it}$ is a dummy variable that takes a value of one when gasoline station $i$ is a competitor of a second entrant in a deregulated area in time $t$ and onwards; and, $D3_{it}$ is a dummy variable that takes a value of one when gasoline station $i$ is a competitor of a third entrant in a deregulated area in time $t$ and onwards. In this specification, $\delta_1$ represents the difference-in-difference effect of the first entry on the retail equilibrium price of gasoline, while $\alpha_1$ and $\gamma_1$ show the marginal impact of the second and third entries, respectively, on the same deregulated area.
5 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 there might also be a problem of serial autocorrelation. However, following an analysis of the correlations and variance inflation factors (VIF), this proved eventually not to be a concern for most of the variables, with the exception of the factors expressing demand that are detailed and discussed below. The correlation matrix between variables and the corresponding VIF are presented in “Appendix C”. Estimation results of Eq. 1 are presented in Table 3. Column (1) provides the fixed effects estimation by 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 gasoline stations 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.\(^6\)

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, on average, charge 1.34% per liter less than their competitors who do not suffer this exposure.\(^7\) This result is economically significant as it represents one fifth of gasoline stations average retail margin.

All the estimation results for the control variables were as expected. The coefficients for the time variant variables are also reported in Table 3. The number of competitors is significant in explaining differentials: the equilibrium price falls with each 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 population data were available for the whole period, the data for the 2014 series 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 of the fixed effects estimation controlling for specific Municipality time trends. As observed, the effect of deregulation on the equilibrium price holds when adding this extra control: according to this estimation the equilibrium price decreases by 1.33% after deregulation.

Table 4 shows the marginal impact of entrants, i.e. the effect caused by the first entrant and the marginal impact of having a second or third entrant in the same indus-

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\(^6\) Results also hold when considering an arbitrary variance-covariance structure with standard errors clustered by Municipality.

\(^7\) If we only consider competitors outside the industrial area, the impact is 1.28%.
Table 3  Estimation results

| Variable                  | All sample (1)             | All sample (2)             |
|---------------------------|-----------------------------|----------------------------|
| D(DiD)                    | $-0.0134^{***}$             | $-0.0133^{***}$            |
|                           | (0.0032)                    | (0.003)                    |
| Competitors               | $-0.006^{***}$              | $-0.0066^{***}$            |
|                           | (0.001)                     | (0.001)                    |
| Population (in millions)  | $-0.24^*$                   | $-0.245^*$                 |
|                           | (0.15)                      | (0.147)                    |
| Income (in millions)      | 9.35                        | 11                         |
|                           | (10.7)                      | (1.1)                      |
| Constant                  | $-0.32^{**}$                | $-0.34^{**}$               |
|                           | (0.16)                      | (0.16)                     |
| Station fixed effects     | ✓                           | ✓                          |
| Time fixed effects        | ✓                           | ✓                          |
| Temporal tendency Mun.   | –                           | ✓                          |
| Brand dummies             | ✓                           | ✓                          |
| Titularity change         | ✓                           | ✓                          |
| $R^2$                     | 0.855                       | 0.856                      |
| Joint significance test   | 176.31^{***}                | 579.85^{***}               |
| Wooldridge test           | 140.012^{***}               | 43.88^{***}                |
| Breusch Pagan/Cook Weisberg test | 313.4^{***}                | 618.29^{***}               |
| Observations              | 16,775                      | 16,775                     |
| Ids                       | 392                         | 392                        |

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. All sample (1) is the estimation with period fixed effects. All sample (2) is the estimation with municipality temporal trend.

The effect of entry restrictions on price: evidence from…

...trial area. Results show that the effect of the reform is closely related to the first market entry. A second or third entry in the same industrial market area does not seem to generate further reductions in price.

Table 5 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 on a quarterly basis. That is, I estimate the difference between treated and control groups in every quarter. As there is a period of 16 months of entry following deregulation, I estimate whether there is a differentiated effect of the policy in the following four quarters: the first 4 months after deregulation (March–June 2013), from July 2013 to October 2013, from November 2013 to February 2014, and, in the 4-month period 1 year after deregulation (March–June 2014).

The results show that the effect of the policy decreases over time. The average reduction in price during the first 4 months following deregulation was about 1.6% compared to 0.7% over the last 4-month period analyzed. Note that the coefficients...
Table 4  Marginal impact of entrants

| Entrant | DiD     |
|---------|---------|
| First   | −0.0139*** (0.003) |
| Second  | −0.0008 (0.004) |
| Third   | 0.011 (0.007) |

Controls ✓
Station fixed effects ✓
Time fixed effects ✓

R² 0.856
Observations 16,775
Ids 392

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

Table 5  Dynamic effect of the reform on time

| Period | DiD     |
|--------|---------|
| First 4 months | −0.016*** (0.005) |
| 5–8 months | −0.015*** (0.004) |
| 9–12 months | −0.012*** (0.003) |
| 13–16 months | −0.007*** (0.003) |

Controls ✓
Station fixed effects ✓
Time fixed effects ✓

R² 0.855
Observations 16,775
Ids 392

show the cumulative effect of the reform over time. Hence, the effect of the deregulation on equilibrium prices over time appears to be decreasing, as new entries imply a smaller additional price reduction over time.

6 Robustness checks

I adopt various strategies to check the robustness of my results. First, I test their robustness in relation to the estimation methods and to different price measures adopted in the related literature, namely, the gross price and retailers margin. I also perform a falsification test. Additionally, I compare the results of estimations using monthly data
with the same data but expressed on a daily basis, to capture whether increasing price variability changes the results or not. Second, I test the robustness of my results with respect to the heterogeneous response, by adopting two different strategies: namely, propensity matching and by restricting the sample. Finally, I estimate the results using another distance measure used in the related literature: a two-mile radius.

### 6.1 Methods, measures, data and frequency

First, I check the robustness of the results with respect to the estimation technique used. Column (1) in Table 6 shows a pooled estimation of equation 1 with time fixed effects. This estimation assumed a panel specific AR-1 autocorrelation structure and panel-level heteroscedastic error. Although it is likely to give worse outcomes than the fixed effects estimation 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 only slightly over time (such as, gasoline station characteristics and their location) and to check the significance of the estimators. The number of gasoline stations used in this estimation is lower, because the characteristics of some gasoline stations are missing due to problems of data availability. As observed, the main result holds, although the reduction in price due to deregulation for the pooled estimation is slightly lower (c. 1.05 and 1.34%). This result might differ with respect to the fixed effects estimation because the autocorrelation treatment is different and because of the lower number of observations in the pooled estimation.

Second, I test the robustness of my results by considering two alternative solutions proposed by Bertrand et al. (2004). Column (2) presents the results of the block bootstrap estimation by gasoline station. Column (3) shows the results of estimating equation 1 with the data collapsed into one single pre-reform period and one single post-reform period. Results hold considering both solutions (1.34 and 1.23% reduction, respectively).

Third, I test the robustness of the results with respect to the price measure used. Columns (4) and (5) present the results for the logarithm of gross price and margin, respectively. As expected, the percentage reduction in price is lower when using the gross price (c. 0.87%), though the results hold. The results for the price margin confirm that the effect of deregulation is to reduce retailer margins by around a fifth (c. 19.9%).

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

Given the availability of data reported on a daily basis, I also check whether results change when using these data. Controlling only for gasoline station and month fixed effects, I obtain significant and almost equal results for both specifications. With daily data, the effect of deregulation on prices is estimated to be 2.69%, compared to 2.67% with monthly data. Hence, increasing variability does not seem to modify the results.
Table 6  Robustness checks

| Variable                  | Pooled (1) | Bootstrap (2) | Two periods (3) | Gross price (4) | Margin (5) | Placebo (6) |
|---------------------------|------------|---------------|-----------------|-----------------|------------|-------------|
| D (DiD)                   | −0.105***  | −0.0134***    | −0.0123***      | −0.0087***      | −0.199***  | 0.00097     |
|                           | (0.0021)   | (0.0027)      | (0.0037)        | (0.0021)        | (0.046)    | (0.0035)    |
| Time inv. controls        | ✓          | –             | –               | –               | –          | –           |
| Time var. controls        | ✓          | ✓             | ✓               | ✓               | ✓          | ✓           |
| Time fixed effects        | ✓          | ✓             | ✓               | ✓               | ✓          | ✓           |
| Station fixed effects     | –          | –             | ✓               | ✓               | ✓          | ✓           |
| $R^2$                     | 0.94       | 0.855         | 0.876           | 0.85            | 0.367      | 0.0647      |
| Observations              | 14,118     | 16,775        | 784             | 16,784          | 15,086     | 9692        |
| Ids                       | 328        | 392           | 392             | 392             | 392        | 228         |

(1) Praise-Winsten corrected standard errors for AR-1 autocorrelation structure and panel-level heteroscedastic errors; (2) fixed effects, block bootstrap by id. (3–6) Fixed effects estimation, standard errors clustered by id. Statistical significance at 1% (***) , 5% (**) and 10% (*)
6.2 Heterogeneous response

In this subsection, I check the robustness of the results with respect to the heterogeneous response of the control and treated groups to pre-existing differences. Here, the first concern might be that the treated and control groups differ in their pre-existing characteristics and that these 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.

The second concern might be that there are differences in the pre-existing characteristics that might condition the evolution in prices in the areas with entry and those without. 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.

In order to allay these concerns, I perform matching procedures and estimate equation 1 with the observations that enjoy 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. 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 with regard to this estimated probability, that is, their propensity score. This can be simply stated as \(Pr(z) = Pr(D = 1|z)\). The match was made using the first-nearest neighbor algorithm, without replacement. Hence, for every treated observation, I retained just one control with the closest propensity score.

I estimate the propensity score for each gasoline station using a logit regression. The form of the estimation is as follows:

\[
P(D_i = 1|z) = \alpha + \beta_0 Z + \epsilon_i
\]

(3)

where \(Z\) is a vector representing all the relevant characteristics of the gasoline stations in the treated and control groups detailed in Table 1. Unlike in the previous estimations, here 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.

On completion of 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. I end up with 121 stations in the treated group and 121 in the control group. The results for the logistic regression and mean differences test between groups are presented in “Appendix C”.

In addition, I adopted a second strategy to check the robustness of results regarding the heterogeneous response due to pre-existing differences. The treated and control groups were constructed as follows. I classified gasoline stations according to the month in which entry occurred. I retained as my treated group all the competitors exposed to a market entrant in the first 8 months after deregulation, and I built my
counterfactual with all the gasoline stations that were not exposed to an entrant in those first 8 months, but which were exposed to an entry in the following 8-month period. In total, I have 91 treated and 57 control units for two 8-month periods, pre- and post-deregulation, respectively. The pre-deregulation period extends from June 2012 to January 2013, and the post-deregulation period from March to October 2013. This sub-sample only contains competitors near those 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.

The results for both strategies are presented in Table 7. Column (1) reports estimates for the matching strategy, and Column (2) 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 equilibrium retail price of diesel, 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 1.04%, while the restricted sample strategy estimates a 1.52% 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 5 shows that the effect of the reform is higher in the first 8 months.

Table 7 Robustness checks

|                      | Matching (1)          | Restricted (2)        |
|----------------------|-----------------------|-----------------------|
| D (DiD)              | 0.0104*** (0.004)     | 0.0152** (0.004)      |
| Controls             | ✓                     | ✓                     |
| Time fixed effects   | ✓                     | ✓                     |
| Station fixed effects| ✓                     | ✓                     |
| $R^2$                | 0.849                 | 0.819                 |
| Joint significance test | 640.87***             | 26.92***              |
| Wooldridge test      | 83.47***              | 6238.67***            |
| Breusch Pagan/Cook Weisberg test | 476.29***          | 9.63**                |
| Observations         | 10,397                | 2357                  |
| Ids                  | 242                   | 148                   |

Matching and restricted sample estimation results

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.
Table 8  Estimation results: 2 miles radius

| Variable               | (1)                  | (2)                  |
|------------------------|----------------------|----------------------|
| D(DiD)                 | \(-0.0137^{***}\)   | \(-0.0136^{***}\)   |
|                        | (0.003)              | 0.003                |
| Controls               | ✓                    | ✓                    |
| Station fixed effects  | ✓                    | ✓                    |
| Time fixed effects     | ✓                    | ✓                    |
| Temporal trend Mun.    | –                    | ✓                    |
| \(R^2\)               | 0.851                | 0.852                |
| Joint significance test| 248.37^{***}        | 377.07^{***}         |
| Wooldridge test        | 143.755^{***}        | 51.552^{***}         |
| Breusch Pagan/Cook Weisberg test | 510.84^{***}    | 1960.01^{***}        |
| Observations           | 17,722               | 17,722               |
| Ids                    | 414                  | 414                  |

Fixed effects estimation, clustered standard errors by id. Statistical significance at 1% (***) and 5% (**), 10% (*). (1) is the estimation with month fixed effects. (2) is the estimation with municipality time trend. Wooldridge test \(H_0\): no first—order autocorrelation; Breusch–Pagan/Cook–Weisberg test \(H_0\): constant variance

6.3 Two miles radius

In this section, I perform a robustness check by changing the geographical market considered in the analysis. To this end, I use a common measure adopted in the literature and estimate the effect of deregulating entry by considering competitors within a two-mile radius of all industrial areas. The data set has 234 gasoline stations in the treated group and 184 gasoline stations in the control group. In total, the set comprises 414 gasoline stations in a 44-month period. Recall that the period corresponds to the 22 months prior to deregulation and the 22 months after the reform was introduced (i.e. from March 2011 to December 2014). As some stations entered after March 2011 and some exited before the end of the period, it is an unbalanced panel, and, in total, it includes 16,030 observations.

I estimate equation 1 with the same time variant control variables as those used when testing for a geographical market with a one-mile radius, namely, the number of competitors that each gasoline station faces in each month (in this case employing a two-mile radius), the population and income of the Municipality in which each station is located, the brand, and if there has been a change of brand. Additionally, I use gasoline station fixed effects to control for time invariant characteristics and time fixed effects or Municipality time trend to control for common shocks. The estimation is performed with clustered errors by id.\(^8\) The results are presented in the Table 8.

The results show that when considering a geographical market with a two-mile radius, the main results hold. Deregulating entry, and the consequent entrance of

\(^8\) Results hold with clustered errors by Municipality.
gasoline stations, reduces the equilibrium price by about 1.37%, considering the fixed effects estimation and the estimation with Municipality time trends.

7 Conclusions

In this article, I have estimated the effect of entry restrictions on the equilibrium retail price of gasoline by exploiting a change in Spanish regulations. In February 2013, a Central Government reform permitted gasoline stations to operate in industrial and commercial areas. Over the following 2-year period, this deregulation led to a high number of new market entrants in these newly designated industrial 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 that use different price measures (gross price and margin), daily (as opposed to monthly) data, and a zone of influence with a two-mile radius. Additionally, I run a placebo test, which indicates that the effect found can be attributed to the policy reform. Finally, I also show that these results hold when controlling for heterogeneous responses due to pre-existing differences in gasoline stations between groups and between areas. The results for the matching estimation show that, when considering a geographical market with a one-mile radius, prices fell by about 1.04% as a result of the deregulation of entry. This result is significant, representing almost one fifth of the average retail margin.

The results show that the impact on price attributable to market entry is due to the first station. A second and third entrant in the same industrial area does not result in further price reductions. Moreover, the results show that the marginal impact of the reform is decreasing over time. In the first 4 months after deregulation, gasoline stations exposed to a new entrant charge on average 1.6% less than their matched pair not subject to exposure. However, 1 year later, the marginal impact of entrants is reduced to 0.7%.

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

Finally, as discussed, market entry following deregulation has primarily involved unbranded retailers. As such, the expectation is that regulation has affected not only equilibrium prices but also market structure. This means that future lines of research could usefully assess the effect of market entry regulations on the structure of the retail gasoline market and on social welfare.
Appendix A: The data

See Figs. 3 and 4.

![Map of Metropolitan Area of Barcelona](image1)

**Fig. 3** Metropolitan Area of Barcelona

![Map of Industrial areas and treated and control groups construction](image2)

**Fig. 4** Industrial areas and treated and control groups construction

Appendix B: Correlation between variables

See Tables 9 and 10.
| Variable | lprice | int  | store | comp  | coffe | road | pop  | ind  | high | wash | 24 h | 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| −0.0224| −0.0547|  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 h     |  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| −0.0003|  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|  0.0066| −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|  1.0000 |      |      |      |      |       |      |      |      |      |       |
Table 10  Variance inflation factor

| Variable | lprice | road | pop | Y     | mbik   | car | truck |
|----------|--------|------|-----|-------|--------|-----|-------|
| lprice   | −      | −    | −   | −     | −      | −   | −     |
| int      | 1      | −    | −   | −     | −      | −   | −     |
| road     | −      | −    | −   | −     | −      | −   | −     |
| pop      | −      | −    | −   | −     | −      | −   | −     |
| ind      | −      | 1    | −   | −     | −      | −   | −     |
| Y        | −      | −    | 1.92| −     | 240.94 | −   | −     |
| 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| −     |

Tolerance rule VIF < 10

Appendix C: Robustness checks

See Tables 11 and 12.

Table 11  Logistic regression

| Variable            | Coeff. | SD    | z     | P > |z| |
|---------------------|--------|-------|-------|-----|-----|
| Industrial area     | 0.20   | 0.35  | 0.58  | 0.56|
| Main road           | 0.06   | 0.39  | 0.15  | 0.88|
| Highway             | 1.08   | 0.64  | 1.69  | 0.091|
| Carwash             | −0.03  | 0.27  | −0.11 | 0.91 |
| Store               | 0.6    | 0.51  | 1.17  | 0.24|
| Pumps               | 0.005  | 0.06  | 0.08  | 0.939|
| Coffee shop         | −0.26  | 0.30  | −0.85 | 0.39|
| 24 h                | −0.12  | 0.28  | −0.42 | 0.68|
| Cepsa               | −1.45  | 0.86  | −1.68 | 0.09|
| Unbranded           | −1.37  | 0.80  | −1.71 | 0.09|
| Repsol              | −0.88  | 0.83  | −1.07 | 0.29|
| Income              | −0.0001| 0.0006| −2.05 | 0.040|
| Trucks              | −0.0006| 0.0003| −2.24 | 0.025|
| Cars                | 0.0001 | 0.0006| 2.23  | 0.026|
| Motos               | −0.0007| 0.0004| −1.82 | 0.069|
| Competitors         | 0.13   | 0.04  | 3.26  | 0.001|

Probability of having an entrant after deregulation
All variables correspond to February 2013. The rest of brand dummies were included in the estimation. The variable change of titularly was excluded by the estimation. $R^2$ 0.1389. Observations 316
| Variable            | Treated | Control | Diff  |
|---------------------|---------|---------|-------|
| Store               | 0.89    | 0.88    | −0.008|
|                     | (0.028) | (0.029) | (0.04) |
| Coffee shop         | 0.297   | 0.297   | 0     |
|                     | (0.04)  | (0.04)  | (0.06) |
| Carwash             | 0.53    | 0.52    | −0.008|
|                     | (0.04)  | (0.04)  | (0.06) |
| Pumps               | 6.5     | 6.57    | 0.066 |
|                     | (0.19)  | (0.23)  | (0.3) |
| 24 h                | 0.43    | 0.43    | −0.008|
|                     | (0.04)  | (0.04)  | (0.06) |
| Highway             | 0.061   | 0.035   | −0.026|
|                     | (0.019) | (0.012) | (0.021) |
| Urban               | 0.23    | 0.24    | 0.008 |
|                     | (0.04)  | (0.04)  | (0.05) |
| Industrial area     | 0.50    | 0.45    | −0.06 |
|                     | (0.04)  | (0.04)  | (0.06) |
| Main road           | 0.21    | 0.26    | 0.06  |
|                     | (0.037) | (0.04)  | (0.055) |
| CEPSA               | 0.124   | 0.124   | 0     |
|                     | (0.03)  | (0.03)  | (0.04) |
| Repsol              | 0.29    | 0.32    | 0.03  |
|                     | (0.04)  | (0.04)  | (0.06) |
| Unbranded           | 0.16    | 0.19    | 0.025 |
|                     | (0.03)  | (0.03)  | (0.05) |
| Supermarket         | 0.025   | 0.017   | −0.008|
|                     | (0.01)  | (0.01)  | (0.02) |
| Competitors         | 5.9     | 5.2     | −0.66 |
|                     | (0.34)  | (0.44)  | (0.55) |
| Cars                | 66,936  | 70,597  | 3661  |
|                     | (13,538)| (13,305)| (19,380)|
| Trucks              | 12,604  | 13,064  | 459   |
|                     | (2365)  | (2327)  | (3388) |
| Motorbikes          | 18,375  | 21,374  | 2999  |
|                     | (5334)  | (5223)  | (7618) |
| Observations        | 121     | 121     | −     |

Mean differences in February 2013

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

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