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The impact of price ceiling regulation: evidence from the retail gasoline market

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Abstract
We examine the short-run impact of price ceilings on retail gasoline prices in isolated oligopolistic markets, uniquely observing for which stations the regulation was binding and for which it was not. Leveraging daily pricing data and a difference-in-differences methodology, we find that, while binding stations naturally lowered prices, non-binding stations increased theirs, though there is substantial heterogeneity. Among non-binding stations, those with more favorable characteristics for collusion adjust prices faster, move closer to the ceiling, and exhibit lower price dispersion, consistent with more effective coordination. Our results provide evidence that the price ceiling acted as a focal point for collusion among non-binding stations, consistent with channels identified in tacit collusion theory.

Keywords: price ceiling; tacit collusion; focal point; retail gasoline markets; oligopoly
JEL codes: L1, L5, L8

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1. Introduction

Price ceilings are a significant policy instrument employed by governments around the world to regulate the market and control the prices of essential goods and services, like food, housing, or fuel. By setting a maximum allowable price, price ceilings aim to protect consumers from excessively high prices and ensure affordability, particularly in times of economic instability, such as during periods of inflation or large demand (e.g., COVID-19) or supply (e.g., Russian invasion, Iran war) shocks. While the intention behind implementing price ceilings is typically to promote equity and accessibility, they can also give rise to unintended consequences, such as shortages, reduced quality of goods, distortions in market dynamics and general misallocation of resources. Although the long-term effects of price ceiling regulation have been discussed in the literature (Knittel and Stango, 2003; Contín-Pilart et al., 2009; Davis and Kilian, 2011; Carranza et.al, 2015; Byrne and de Roos, 2019; Li and Wu, 2022; Cui et al., 2023), there is surprisingly little evidence on its short-term price and competition effects.

The theory on price ceiling regulation gives ambiguous predictions. If the price ceiling is set too high, then most firms would be unaffected, resulting in minimal market impact. If the ceiling is set such that is binding for all, some firms may be unable to continue operating and exit the market, causing serious market structure concerns. If the ceiling is set such that is binding for some firms, but not for others, then there might be two opposing forces. For the binding firms, prices must decrease, which benefits consumers. For non-binding firms, however, the outcome is uncertain. On the one hand, they face tougher competition from binding firms that have reduced their prices, which puts them under pressure to decrease their own prices. On the other hand, the price ceiling may serve as a focal point, potentially encouraging them to tacitly collude (Schelling, 1960; Tirole, 1988) and increase prices to the detriment of consumers. Which of these forces will prevail, and as a result, what will be the overall effect on prices, is an empirical question.

Studying the effects of price ceiling regulation is empirically challenging. This is because these policies are typically implemented at the national level, making it difficult for researchers to find a good comparison group. The main approach in the literature is to compare products with a price ceiling with other products that do not have any restrictions within the same country (Ma, 2007; Genakos et al., 2018). This comparison, however, may be problematic, especially if there are significant substitution effects between the two groups of products. Moreover, the absence of cross sectional variation within a single market makes it difficult to study how firms respond across different market conditions.

In this paper, we estimate the impact of a temporary price ceiling regulation imposed on the retail gasoline market. We exploit a unique natural experiment in which the Greek government decided to impose a retail price cap for unleaded 95 gasoline only across several islands in May 2011 (see Figure 1). The policy aimed to address the "excessively" high prices in these prefectures, which along with the country's general economic downturn (this is at the beginning of the Greek economic crisis[5]) had a negative impact on consumer welfare. Unleaded 95 was by far the most used fuel and it was considered a necessity good.

We take advantage of the fact that the policy was applied only to a set of islands, which define distinct local markets with no substitutability across them, and was not binding for all stations to study both the overall effect of the regulation and how firms responded under different market conditions. To estimate the average price impact, we compare unleaded 95 prices at gas stations on the affected islands with those in the Attica prefecture, which was

[5] The Greek economy experienced a real GDP per capita contraction of roughly 20 percent between 2007 and 2017 (Chodorow-Reich, et al,, 2023). The Greek depression has no precedent among modern middle- and high-income economies (Gourinchas, et al., 2016).

excluded from the regulation and serves as our control group. Distinguishing which stations were truly bound by the regulation allows us to study the differential impact on binding and non-binding stations for the first time in the literature, as well as how this variation relates to market structure, demand, and firm characteristics.

Using a difference in difference methodology and daily pricing data for unleaded 95 during 2011 for all gas stations located on affected Greek islands (treatment group) and the prefecture of Attica (control group) we examine both the introduction of price ceiling regulation and its tightening. There are two key lessons from the policy evaluation study.

First, we find that the implementation of price ceiling regulation reduced average prices by 1.7%, which amounts to 0.03€ per liter, or approximately, 450,000€ in monthly savings for consumers in the treated areas. But the regulation had a differential impact on binding and non-binding gas stations. As expected, it led binding stations to reduce their prices (-2.4%). More interestingly, however, it caused non-binding stations to increase their prices by 0.6% on average.

Second, we show that there is substantial heterogeneity in the behavior of non-binding stations and we explore the characteristics that drive this behavior. In particular, among non-binding stations, those with more favorable characteristics (higher population density, lower transportation distance, more tourist arrivals, higher income, and lower competition) coordinate more effectively, adjusting prices faster, reaching levels closer to the ceiling, and having lower price dispersion. We present evidence that the observed price increases among non-binding stations are attributable to tacit collusion, and we investigate several channels identified in the existing literature as potential mechanisms. Our analysis reveals that non-binding stations are more likely to raise prices in less competitive markets, when anticipated demand is higher, when the reputational costs of collusion are lower, and when price monitoring across firms is easier.

Overall, the results of our ex-post policy evaluation highlight some unexpected consequences of the price ceiling regulation. We provide evidence that such policies create incentives for tacit collusion, and show that this ability is not uniform across stations, identifying the characteristics that facilitate this behavior. Our paper both relates to and contributes to several strands in the literature. First, it contributes to the small number of studies that focus on regulation as tacit collusion facilitator. Knittel and Stango (2003) show that mandatory price ceilings in the US credit card market had the unexpected effect of increasing average prices by providing a focal point for collusion to unconstrained firms. However, they use a statistical model to identify collusive behavior, without knowing which firms were really constrained. Genakos et al. (2018) and Dimas and Genakos (2020), who investigated the repeal of maximum markup regulation in the Greek market for fresh fruits and vegetables, discovered that deregulation resulted in a significant decrease in both retail and wholesale prices, providing indirect evidence that markup ceilings served as a focal point for wholesaler coordination. Yet, they do not have firm-level data to distinguish between binding and non-binding firms. More recently, Lewis (2015) provides evidence that odd-numbered pricing points can be used as focal points to facilitate tacit collusion in the US, while Faber and Janssen (2019) show that suggested prices can play a similar role in the Dutch gasoline market. Zhang et al. (2020) and Martin and Verboven (2025) analyze price ceiling regulation in the Chinese and Belgian market, respectively, also corroborate the focal point hypothesis. These papers analyze the markets while price ceiling legislation is already in place. As a result, they are unable to examine the impact of price cap regulation or to monitor the differences between binding and non-binding stations independently.

Second, our work also relates to the literature analyzing competition and price coordination in the retail gasoline market, e.g., Borenstein and Shepard (1996) and Lewis (2012) for the US market, Eckert and West (2005) for Canada, Foros and Steen (2013) for Norway, Andreoli-Versbach and Franck (2015) for Italy and Byrne and de Roos (2019) for Australia. What these studies show is that firm coordination is possible and can emerge endogenously through repeated interaction. However, whether government regulation helps or hinders this process is an open research question and the focus of this paper.

Lastly, from a policy perspective, this study contributes to the literature on the effects of regulation on efficiency and economic performance (see, for example, Djankov et al., 2002; Scarpetta and Tressel 2002; Blanchard, 2004; Katsoulacos et al., 2017), and informs the debate on whether government intervention, or demand-side industrial policy more broadly, is called for in some essential products for consumers under special circumstances.

2. Institutional and policy change background

In 2010, the inability of the Greek government to borrow funds from international markets led to a €110 billion bailout loan from the European Commission, the European Central Bank, and the International Monetary Fund. As part of the loan agreement, the Greek government implemented a series of structural reforms with the aim to reduce the regulatory burden and increase the competitiveness of the economy (Katsoulacos, et al., 2017). But, as the crisis deepened in 2011, the Ministry of Development and Competitiveness, which is responsible for monitoring product markets in Greece together with the relevant regulatory authorities, started considering more drastic measures. Although gasoline and oil prices had been set freely, the government reserved the right to set a price ceiling if it considered that the market was not functioning adequately. In particular, under Greek law (N. 3054/2002) the minister in charge could impose a temporary price ceiling in cases of well-documented “excessive profits” phenomena. This law provision was meant as a short-term measure of last resort and had been rarely used in practice until then.

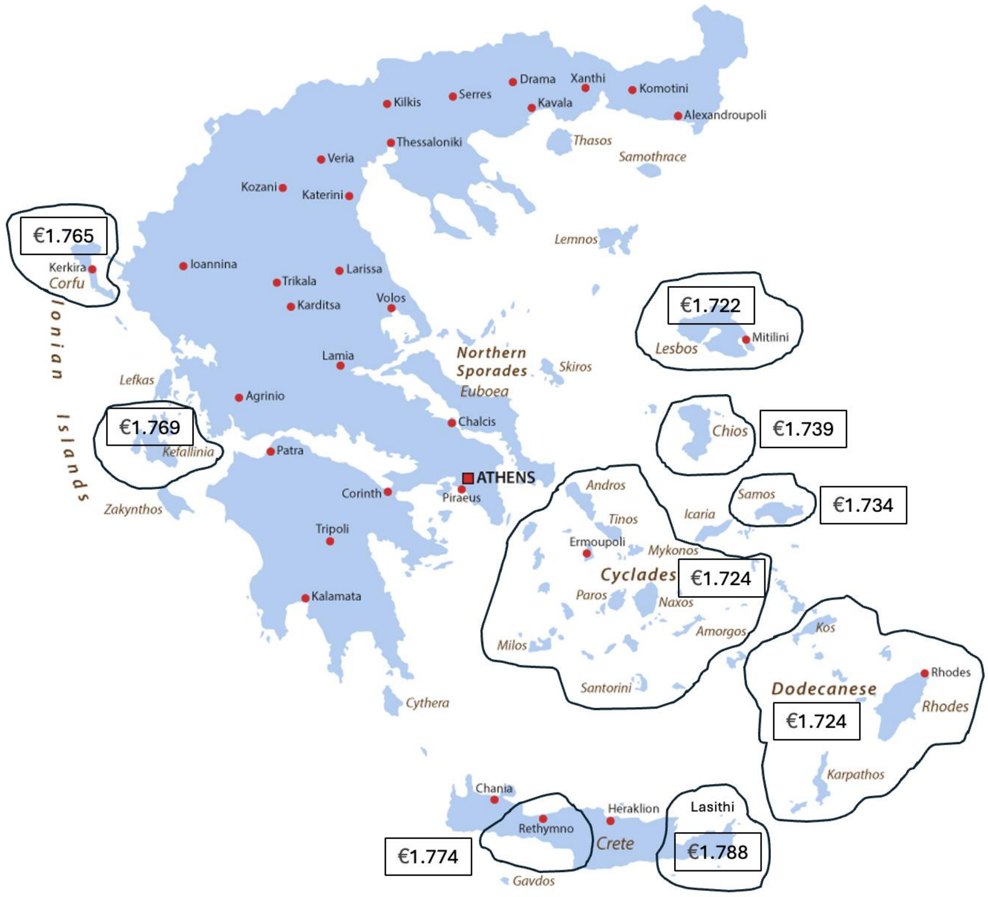
On May 3rd, 2011, the Ministry announced the implementation of a retail price cap regulation for unleaded 95 petrol products exclusively, in a handful of prefectures that mostly included islands (see Figure 1) but not the mainland, beginning the following day. The price ceiling only applied to unleaded 95, not any other petrol product, because it was the most commonly used fuel and was considered a necessity. The motivation for enacting this legislation was to address the "excessively" high prices of the previous several weeks in certain prefectures, which, along with the country's general economic crisis, had a severe influence on consumer welfare. The law allowed for the price ceiling restriction to be updated and extended over time until pricing levels returned to “normal”. As a result, it was unknown how long the price ceiling would be in effect when it was first implemented.[6]

In this paper, we focus on affected prefectures that include islands, taking advantage of the fact that each island constitutes a separate market with distinct local competition (see, Genakos and Pagliero, 2022; Dimitrakopoulou, et al., 2024). The refilling process for the gas stations located in islands is conducted by ships that leave from the port of Piraeus (in Attica, near the capital of Athens) to reach each island. Since Greece’s mainland was excluded from this price cap regulation, we use prices of the same product (unleaded 95) from gas stations in the prefecture of Attica, which includes Athens, as our control group. Given that we are using prices of the same product as a control group, we will next demonstrate that the parallel trends assumption is easily satisfied in this environment. Moreover, we control for local competition and match local markets in Attica and the islands so that observationally they face similar

[6] The ceiling was eventually renewed only once, on May 19th, and repealed on July 4th, lasting two months in all.

competitive conditions. We use final consumer retail gasoline prices that include both per unit and percentage taxes, as well as the refinery cost.[7] Excise duties and VAT remained constant throughout our study period. We focus on the introduction of the price ceiling regulation, as it marked the first intervention and gas stations had no prior experience with such restrictions. Moreover, a higher number of non-binding stations existed at that time compared to the subsequent tightening.

FIGURE 1 – PRICE CEILING REGULATION ACROSS GREEK ISLANDS



Notes: The figure shows the different price ceilings imposed across the various islands.
Source: Authors' calculations based on data from the Greek Ministry of Development.

[7] Retail gasoline prices are defined as follows: $P_{retail} = (P_{refinery} + excise\ duty\ \&\ fees + profit\ margin) \times (1 + VAT)$, where the profit margin refers to the combined wholesale and retail level profit margin.

3. Data

We combined several datasets for our analysis. First, we obtained daily station-level retail prices for unleaded 95 during 2011 for all gas stations located on affected Greek islands and the prefecture of Attica. The data on prices was officially collected by the Greek Ministry of Development and Competitiveness through a reporting system, which required managers of each petrol station to record retail prices daily. Second, we measured the number of gas stations operating on each island and cross-checked them using external information from Yellow Pages and company reports. Using Google maps, we geo-located each gas station both in Attica and in the islands and computed measures of local competition based on the number of competitors within a given radius.[8] These are conventional methods of measuring local competition used by competition authorities and researchers alike. Uniquely in our environment, arbitrage across islands is basically impossible, as the cost of transporting a car by ferry outweighs the potential savings in fuel cost. Note that our sample gas stations on the islands are independently operated, with pricing decisions made locally. Furthermore, we evaluated each island and found no brand or ownership concentration issues in line with the results from the sectoral market investigation by the Hellenic Competition Commission (Hellenic Competition Commission, 2024).

Our final sample includes more than 43,000 observations at the product-station level for 1,478 gas stations located on 24 islands and in the Attica region. Figure 1 shows the affected islands along with the initial price ceiling level imposed. Table 1 reports the first price ceiling by prefecture (column 1) and various statistics of the price distribution (columns 2-6) from the day before the introduction of the regulation. It is noteworthy that the price ceiling falls between the minimum and maximum price observed on each island and is set below the average price.

[8] Similar datasets have been also used in Genakos and Pagliero (2022), Dimitrakopoulou et al. (2024) and Genakos and Kampouris (2026).

This is further evidenced by the high proportion of gas stations for which the ceiling is binding: as shown in column 7, the average percentage of binding stations is 84%. Nonetheless, there exists a proportion of stations for which the ceiling is not binding (column 8).

TABLE 1 - SUMMARY STATISTICS (€ cents per 1000 liters)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Affected areas	First price ceiling	Av. Price $_{(t-1)}$	Standard Deviation $_{(t-1)}$	Median $_{(t-1)}$	Min Price $_{(t-1)}$	Max Price $_{(t-1)}$	% stations binding	# non-binding stations
Lasithi	1788	1809.4	27.8	1810	1750	1869	85%	9
Rethymno	1774	1780.2	34.9	1774	1699	1859	63%	21
Samos	1734	1773.3	19.9	1768.5	1714	1806	96%	1
Ikaria	1734	1852.3	19.2	1855	1817	1870	100%	0
Limnos	1722	1812.4	12.8	1810	1798	1830	100%	0
Lesvos	1722	1725.9	35.1	1725.5	1668	1850	55%	26
Chios	1739	1783.0	29.7	1789	1739	1850	100%	0
Cephalonia	1769	1833.0	37.5	1843	1749	1892	94%	2
Andros	1724	1777.0	30.7	1785	1740	1840	100%	0
Antiparos	1724	1794.5	7.8	1794.5	1789	1800	100%	0
Ios	1724	1777.0	67.4	1785	1706	1840	67%	1
Kea	1724	1722.5	71.8	1722.5	1675	1770	50%	1
Milos	1724	1793.0	13.0	1790	1775	1810	100%	0
Mykonos	1724	1791.8	10.8	1798	1774	1799	100%	0
Naxos	1724	1757.9	52.8	1754	1680	1893	75%	5
Paros	1724	1729.2	30.0	1720	1690	1796	40%	9
Folegandros	1724	1810.0	35.4	1810	1785	1835	100%	0
Sifnos	1724	1830.7	9.8	1825	1825	1842	100%	0
Syros	1724	1772.9	27.7	1779	1730	1820	100%	0
Tinos	1724	1782.3	37.1	1790	1705	1814	86%	1
Santorini	1724	1850.0	10.0	1845	1840	1865	100%	0
Rhodes	1724	1725.3	25.1	1724	1669	1789	62%	21
Kos	1724	1737.8	38.6	1734.5	1680	1799	67%	4
Corfu	1765	1785.2	29.3	1788	1689	1860	81%	14

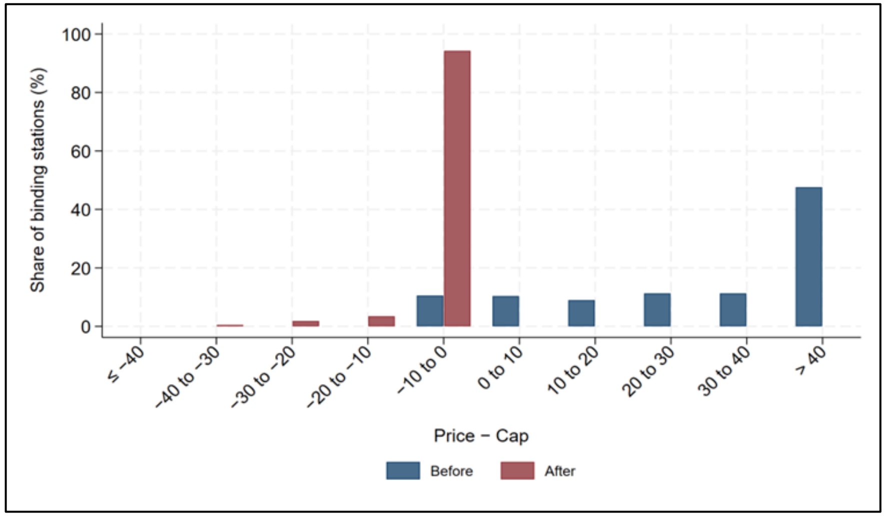
Notes: Table reports the first (column 1) price ceiling by prefecture and different statistics of the price distribution (columns 2-6) from the day before the introduction of the regulation. Columns 7 and 8 report the percentage of binding stations and the number of non-binding stations per island.
Source: Authors' calculations based on data from the Greek Ministry of Development.

Figure 2 plots the distribution of differences (retail prices – cap) for unleaded 95 on the treatment islands, one day before the introduction of the regulation (the "Before" histogram) and fourteen days after (the "After" histogram), separately for binding and non-binding stations. For binding stations, there is a clear bunching at zero in the "After" difference distribution, as expected, given that they had to comply with the regulation. For non-binding stations, there seems to be a shift of the "After" distribution to the right, indicating an increase in average prices following the price ceiling, but the pattern is not so clear-cut.

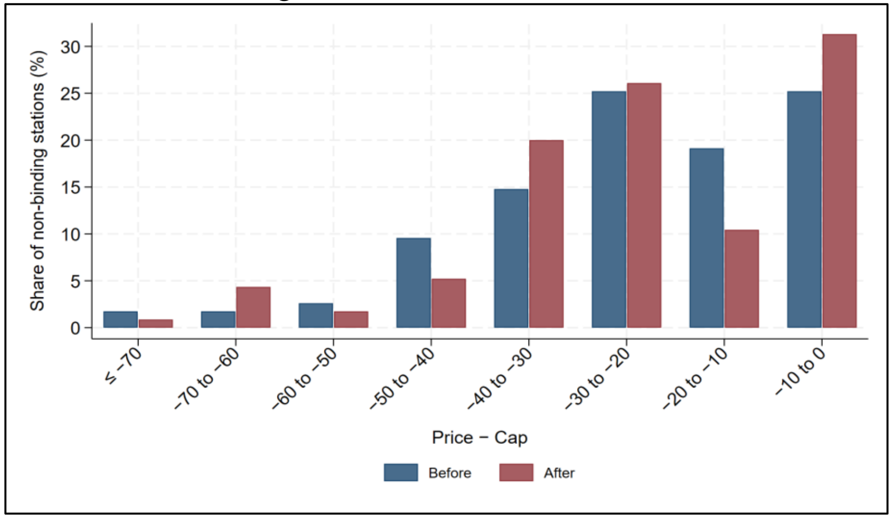
As a result, overall, prices above the ceiling fell dramatically in accordance with the legislation, but prices in petrol stations below the ceiling looked to be rising, evidently taking advantage of the price ceiling. However, the non-binding group's behaviour is not homogeneous and seems to differ in how close to the ceiling different stations go.

FIGURE 2 - DISTRIBUTION OF DIFFERENCES (PRICE-CAP) FOR BINDING AND NON-BINDING STATIONS BEFORE AND AFTER

Panel A Binding stations



Panel B Non-binding stations



Notes: The figure plots the distribution of retail prices for Unleaded 95 on the treatment areas one day before the introduction of the regulation and fourteen days after, separately for binding and non-binding stations.

Source: Authors' calculations based on data from the Greek Ministry of Development.

4. Empirical Methodology

We estimate a traditional event study specification of a two-way fixed effects difference-in-difference empirical framework (Miller, 2023), where we compare prices fourteen days before and after the policy change for the treatment and control group. Our event study specification is as follows:

$$log(P_{ist}) = \beta_0 + \sum_{j=-14}^{14} \beta_{1,T+j}\, Treat_i \times D^{T+j} + \beta_s + \beta_t + \varepsilon_{ist} \qquad (1)$$

where P_{ist} denotes the retail price of unleaded 95, on island i, at gas-station s, on day t, $Treat_i$ is an indicator variable for Unleaded 95 in the islands under regulation, whereas $D^{T+j} = 1$ if the date is the j^{th} day *before* ($j < 0$) or *after* ($j > 0$) the price ceiling regulation. Finally, the model includes gas station (β_s) and day (β_t) fixed effects that capture island- and station-specific characteristics, as well as the macroeconomic shocks that affected the whole economy. Standard errors are clustered at the island (or by prefecture for Athens) level. We run (1) to check for parallel trends between the treatment (unleaded 95 in affected islands) and the control group (unleaded 95 in unaffected Attica) and any likely anticipation effects or leakage of information.

We then focus on measuring both the overall impact of price ceiling regulation and, most interestingly, how the binding versus non-binding stations reacted to the regulation. We estimate models:

$$log(P_{ist}) = \beta_0 + \beta_1\, Treat_i \times Post_t + \beta_s + \beta_t + \varepsilon_{ist} \qquad (2)$$

$$log(P_{ist}) = \beta_0 + \beta_1 Bind_s \times Post_t + \beta_2 NonBind_s \times Post_t + \beta_s + \beta_t + \varepsilon_{ist} \qquad (3)$$

where $Treat_i$ is an indicator variable for unleaded 95 in the islands under regulation, $Post_t$ is an indicator variable for the period following the price ceiling regulation change and $Bind_s$ and $NonBind_s$ are binary indicators that take the value 1 if the price of a treated product on the day before the policy announcement exceeds or does not exceed the price ceiling, respectively, and 0 otherwise.[9] This is a unique feature of our data that allows us to investigate the behaviour of these two types of stations after regulation. Given the lack of anticipation effects or leakage of information, we use day $t \in \{\tau - 1, \tau + \delta\}$, with τ is the date when the price ceiling regulation started and $\delta = 1, \ldots, 14$ is the length of the adjustment-period considered.

5. The effect of price ceiling regulation

5.1 Price ceiling regulation event study

Figure 3 plots the regression coefficients from equation (1) together with their 95% confidence intervals.[10] Panel A examines the overall market effect. Coefficients before the start of the regulation are generally not statistically significant, indicating that there was no anticipation or leakage of information.[11] From the day of implementation onwards all coefficients are significantly negative and stable around the average effect of -1.5%. Panel B looks at the estimated results from the binding stations. They exhibit a similar behavior to the aggregate pattern, only that the negative coefficients are stabilizing at a lower level of -2%. Lastly, Panel C examines the results for the non-binding stations. The difference in reaction is clear. The estimated coefficients start increasing after the second day and they reach a plateau after a week, with a strong upward movement towards the end of the second week. Non-binding stations seem to quickly learn (within a few days) how to coordinate actions to raise prices, and there are indications that, given the uncertainty surrounding the duration of the regulation, they

[9] We did extensive testing to make sure we are capturing truly non-binding stations and not some random price fluctuation below ceiling. We vary the reference period used to classify stations as non-binding using prices observed on $\tau-2$, $\tau-3$, $\tau-4$, or $\tau-5$, as well as using the average price over the pre-policy window $[\tau-5, \tau-1]$. Table A1 in the Appendix shows that the number of non-binding stations as well as the estimated effects on the impact of ceiling regulation are similar across all alternative definitions.

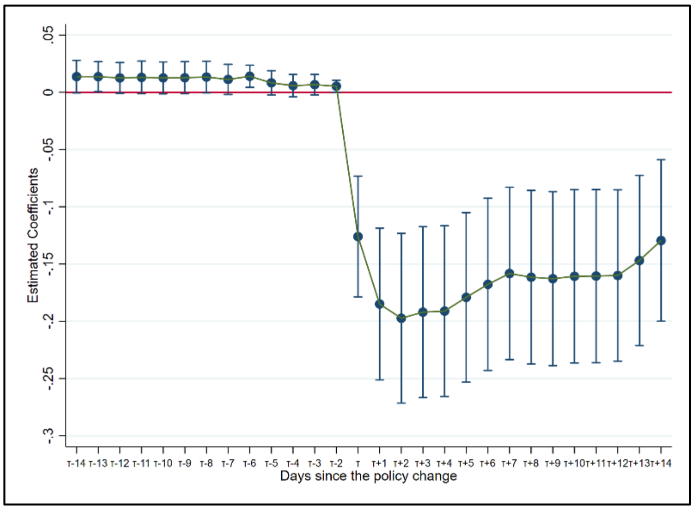
[10] Estimated coefficients are reported in Tables A2 of the Appendix.

[11] Figure A1 also shows graphically that the two-price series follow the same trend before the policy change.

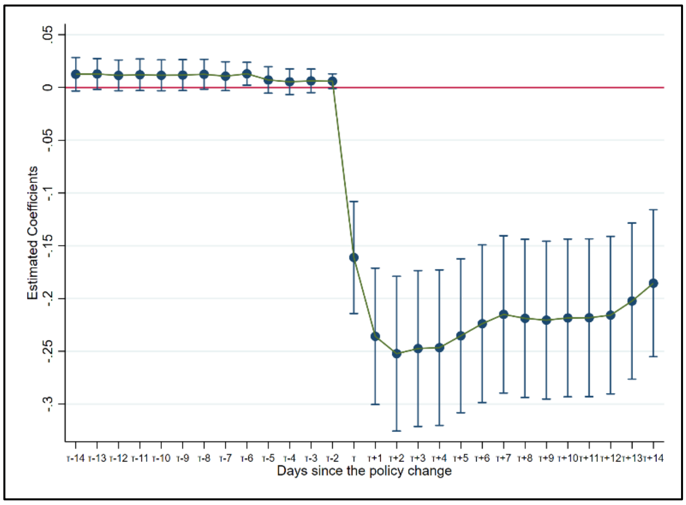
progressively push for even higher prices. Therefore, non-binding stations exhibit a markedly different behavior, which led them to use the regulation as a focal point and raise their prices, despite increased competition from the binding stations' lower prices.

FIGURE 3 - DYNAMIC EFFECT OF PRICE CEILING INTRODUCTION

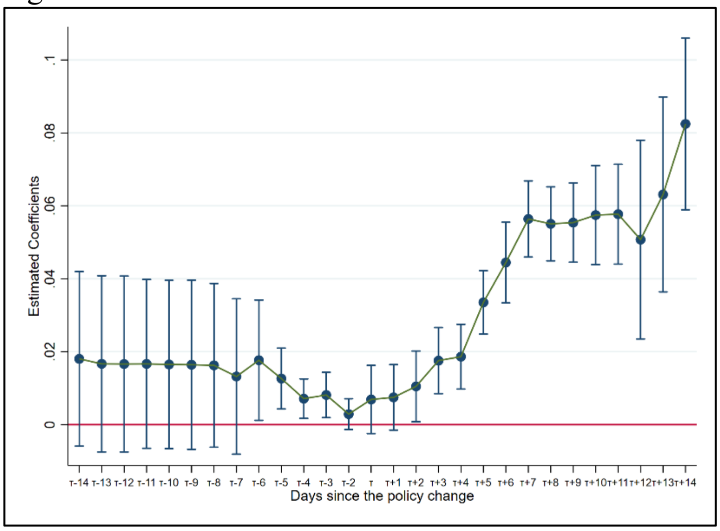
Panel A. All stations



Panel B. Binding stations



Panel C. Non-Binding stations



Notes: The figure plots the regression coefficients reported in Table A2 together with their 95% confidence intervals for the market overall (Panel A), the binding stations (Panel B) and the non-binding stations (Panel C).
Source: Authors' calculations based on data from the Greek Ministry of Development.

5.2 Aggregate effect of price ceiling regulation

Table 2 reports the estimated coefficients of models (2) and (3).[12] Looking at the treated areas only, the average price decreases following the implementation of the price ceiling regulation (column 1). When we add the control group in column 2, the causal impact of price cap regulation is somewhat reduced to -1.68%, indicating that the control group's price also falls, but the regulation effect is still strongly significant. Given that the average price for unleaded 95 on the treated islands was 1.78€ before regulation, the price ceiling introduction translates to a price reduction of 0.03€ per liter. With an average monthly consumption of about 15m liters, this intervention amounts to approximately 450,000€ savings per month for the consumers in the treated areas.[13]

In column 3 we explore the differential impact between binding and non-binding stations. Stations that were charging prices above the regulated price ceiling were forced to decrease their prices by -2.22% on average. On the other hand, stations for which the regulation was not binding, increased their prices by 0.38%. This clearly highlights both the benefits of price ceiling regulation for consumers, as well as its unintended consequences. When binding, price ceiling regulation drives prices down, benefiting consumers. However, for non-binding stations, the regulation seems to act as a focal point incentivizing them to increase their prices, which weakens competition and harms consumers.

After fifteen days the regulated price ceilings got updated, so in the last two columns we pool the data from both incidents. The updated ceiling regulation reduces average prices by an additional 0.12%, as we can see in column 4. Hence the overall average effect of price ceiling regulation was to reduce prices by -1.8% relative to their pre-regulation level. When we split

[12] All coefficients and standard errors are multiplied by 10.

[13] Data on total consumption of petroleum products by region and prefecture from the Hellenic Statistical Association. The savings are calculated for the whole market using average prices and assuming a linear demand schedule.

the effect between the binding and the non-binding stations in the last column, we can see that the impact on the two types of stations is of similar size now (-0.16% for the binding versus 0.17% for the non-binding) but moving in opposite directions. Therefore, overall binding stations reduced their prices by -2.4%, while non-binding stations increased their prices by 0.6%. We do not have information on the quantity sold by each type of station but based on overall consumption obtained from the Hellenic Statistical Authority, we can confidently conclude that unless more than 82% of the quantity consumed went to non-binding stations, the regulation was successful in increasing net consumer surplus.

TABLE 2 - PRICE CEILING REGULATION

	(1)	(2)	(3)	(4)	(5)
Dependent variable	ln($Price_{ist}$)	ln($Price_{ist}$)	ln($Price_{ist}$)	ln($Price_{ist}$)	ln($Price_{ist}$)
Sample	Treatment (1st price ceiling)	Treatment & Control (1st price ceiling)	Treatment & Control (1st price ceiling)	Treatment & Control (1st & 2nd price ceiling)	Treatment & Control (1st & 2nd price ceiling)
Time window	$\{\tau - 1, \tau + 14\}$	$\{\tau - 1, \tau + 14\}$	$\{\tau - 1, \tau + 14\}$	$\{\tau-1, \tau+14\}$ U $\{\tau-1, \tau+14\}$	$\{\tau-1, \tau+14\}$ U $\{\tau-1, \tau+14\}$
$Treat_i \times Post_t$ (1)	-0.206***	-0.168***		-0.168***	
dummy=1 after 3rd May 2011	(0.037)	(0.035)		(0.035)	
$Treat_i \times Post_t$ (2)				-0.012*	
dummy=1 after 19th May 2011				(0.006)	
$Bind_s \times Post_t$ (1)			-0.222***		-0.222***
			(0.035)		(0.034)
$Bind_s \times Post_t$ (2)					-0.016**
					(0.006)
$NonBind_s \times Post_t$ (1)			0.038***		0.038***
			(0.005)		(0.005)
$NonBind_s \times Post_t$ (2)					0.017*
					(0.009)
Observations	8,250	22,155	22,155	43,725	43,725
Within R^2	0.839	0.958	0.962	0.961	0.961
Station FE	yes	yes	yes	yes	yes
Day FE	yes	yes	yes	yes	yes

Notes: All coefficients and standard errors are multiplied by 10. The dependent variable is the logarithm of retail price of product j, on island i, in gas station g, and day $t \in \{ \tau - 1, \tau + \delta\}$, where τ is the date of the price ceiling regulation and δ=14. Standard errors are clustered at the island level and are reported in parentheses below coefficients. ***, **, * mark statistical significance at the 0.01, 0.05 and 0.10 level respectively.
Source: Authors' calculations based on data from the Greek Ministry of Development.

5.2 Robustness

Table 3 provides several robustness tests focusing on the divergent behavior of binding versus non-binding gas stations.

First, we investigate different time windows around the introduction of regulation. One might worry that just taking one day before the event might affect the estimates in case there was any leakage of information. As we saw in Figure 3, there is no indication of an anticipation effect, nevertheless it is a useful exercise. In column 1, we extend the time window to five days, while in column 2 we allow for ten days before the price ceiling regulation. The results are very similar to the benchmark estimates in Table 2, column 3.

Second, the regulation was applied to different islands, but for Crete, the largest island in Greece, it was implemented in only two out of its four municipalities (Lasithi and Rethymno, as shown in Figure 1). These municipalities have many gas stations, raising the concern that Crete's situation might be unique compared to other islands, as consumers can easily travel across municipalities to refuel. In column 3, we exclude these two areas from our estimation and the results remain qualitatively unaffected.

Next, we address the potential concern that market structure and competition in islands may be particularly local and different from the urban Attica market. To alleviate this concern, we apply a matching procedure, whereby we only keep local markets in the Attica region that have similar number of competitors to the affected islands, excluding any other local market. As we can see in Table 3, column 4, the results from the matching DiD estimator are very similar to the benchmark results.

In column 5, we experiment with a different control group. Instead of using unleaded 95, we utilize diesel prices from gas stations in Attica. The idea is that diesel is used by different types of cars and there is no possible cross-subsidization between the two products.[14] The estimated coefficients indicate that the effect on binding stations is slightly reduced, while that of the non-binding stations becomes stronger, but both are very close in magnitude to the benchmark results.

Lastly, we also test the robustness of the common trend assumption in (1) using the period before the change in regulation to estimate the impact of a placebo treatment. In the last column of Table 3, we select the period before the regulation and choose fifteen days before the actual start of the regulation as an arbitrary date of the fictitious reform. The results show that the fictitious policy has no impact on the treatment or the control group.

After establishing the difference in behavior between binding and non-binding stations, we now explore the factors that facilitate non-binding stations' price increases and provide evidence that price ceiling regulation creates incentives to raise prices and serves as a focal point for tacit collusion among non-binding firms.

[14] All the pre-trend tests with respect to the new control group have been performed to verify its suitability. Results are not reported here but are available on request.

TABLE 3 - ROBUSTNESS

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln($Price_{ist}$)	ln($Price_{ist}$)	ln($Price_{ist}$)	ln($Price_{ist}$)	ln($Price_{ist}$)	ln($Price_{ist}$)
Sample	Treatment & Control (1st price ceiling)	Treatment & Control (1st price ceiling)	Treatment & Control (1st price ceiling) - no Crete	Treatment & Control (1st price ceiling) - matching local markets	Treatment & Control (1st price ceiling) - diesel control	Treatment & Control (1st price ceiling) - placebo
Days	$\{\tau - 5, \tau + 14\}$	$\{\tau - 10, \tau + 14\}$	$\{\tau - 1, \tau + 14\}$	$\{\tau - 1, \tau + 14\}$	$\{\tau - 1, \tau + 14\}$	$\{\tau - 16, \tau - 1\}$
$Bind_s \times Post_t$	-0.227***	-0.231***	-0.246***	-0.219***	-0.178***	0.001
	(0.033)	(0.033)	(0.038)	(0.017)	(0.035)	(0.006)
$NonBind_s \times Post_t$	0.032***	0.027***	0.037***	0.041***	0.083***	-0.008
	(0.006)	(0.008)	(0.006)	(0.007)	(0.007)	(0.016)
Observations	28,063	35,448	20,385	18,915	22,935	22,155
Within R^2	0.952	0.950	0.954	0.956	0.979	0.991
Station FE	yes	yes	yes	yes	yes	yes
Day FE	yes	yes	yes	yes	yes	yes

Notes: All coefficients and standard errors are multiplied by 10. The dependent variable is the logarithm of retail price of product *j*, on island *i*, in gas station *g*, and day $t \in \{\tau - \delta, \tau + \delta\}$, where τ is the date of the price ceiling regulation and δ=1,...,16. Standard errors are clustered at the island level and are reported in parentheses below coefficients. ***, **, * mark statistical significance at the 0.01, 0.05 and 0.10 level respectively.
Source: Authors' calculations based on data from the Greek Ministry of Development.

6. Price ceiling as focal point for tacit collusion: Behavior heterogeneity and mechanisms

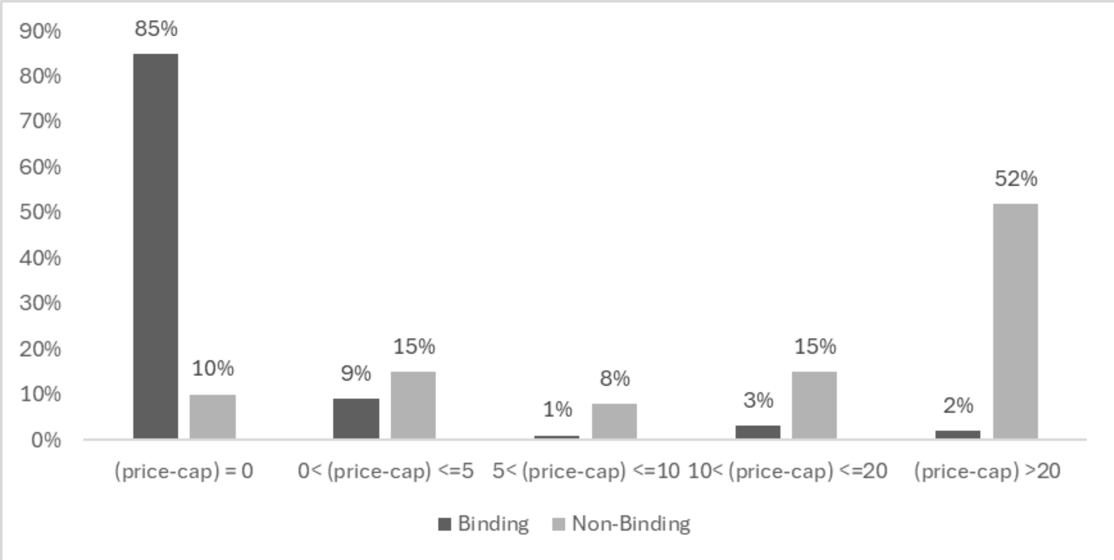
Being able to identify firms not constrained by the price ceiling regulation before its implementation provide us with a unique opportunity to explore their price coordination ex post and the likely mechanisms that facilitate or impede tacit collusion.

Heterogeneity in collusive behavior. We start by documenting some interesting differences in firm behavior and their correlated characteristics. Figure 4 plots the price-cap distribution of gas stations fourteen days[15] *after* the regulation and compares the distances from the price cap for the stations we know regulation was binding versus the ones for which it was not binding. The differences are very clear. The vast majority of binding stations (85%) price at the ceiling, with the second highest percentage (9%) being within €0.005 per liter and the rest having larger differences. By contrast, non-binding stations display a very different distribution, with only 10% of stations pricing at the ceiling, while 38% within €0.02 per liter of the cap and the remaining 52% at a greater distance.[16] In other words, non-binding stations exhibit distinct behavior: approximately half position their prices close to the regulatory ceiling (48% with differences of less than €0.02 per liter), while the other half maintain a more substantial margin below the cap (52% with differences exceeding €0.02 per liter). Identifying these two groups of stations as "close" (less than €0.02 per liter difference) and "far" (more

[15] This is the first phase of the regulation until its first revision. This is the most interesting part as firms react and adjust to the unexpected regulation event.
[16] It is important to note that not all gas stations subject to binding regulation set prices at the ceiling, and conversely, some non-binding stations also price at the ceiling. Consequently, inferring whether a firm was constrained based solely on ex-post observed pricing behaviour—as in Knittel and Stango (2003) or Martin and Verboven (2025)—may be misleading.

than €0.02 per liter difference), it is also interesting to check whether they also differ in other observable characteristics.[17]

FIGURE 4 - PRICE-CAP DISTRIBUTION AFTER 14 DAYS OF THE CEILING REGULATION



Notes: The figure plots the distribution of price-cap distribution of gas stations fourteen days after the regulation for binding and non-binding gas stations.
Source: Authors' calculations based on data from the Greek Ministry of Development.

Table 4 provides a summary of various characteristics. Notice first that the stations that end up pricing very close to the ceiling fourteen days after the regulation (average distance €0.008 per liter) also started very close to the ceiling (average distance €0.016 per liter) one day before the regulation. In contrast, the stations that had prices further away from the ceiling (average distance €0.029 per liter one day before the regulation), end up even further from the ceiling (average distance €0.035 per liter fourteen days after the regulation). In addition, stations that end up pricing close to the ceiling came from higher population density areas, are closer to the main refilling port (Peiraeus), receive more tourists, and have higher average income and lower

[17] It should be emphasized that comparisons between the two groups are not intended to support any causal interpretation, rather, the objective is to investigate more thoroughly the characteristics that lead some stations to increase their prices towards the ceiling, while others remain at a greater distance.

number of competitors. In other words, there seems to be a number of characteristics that allow firms to price higher and may also facilitate collusive behavior.

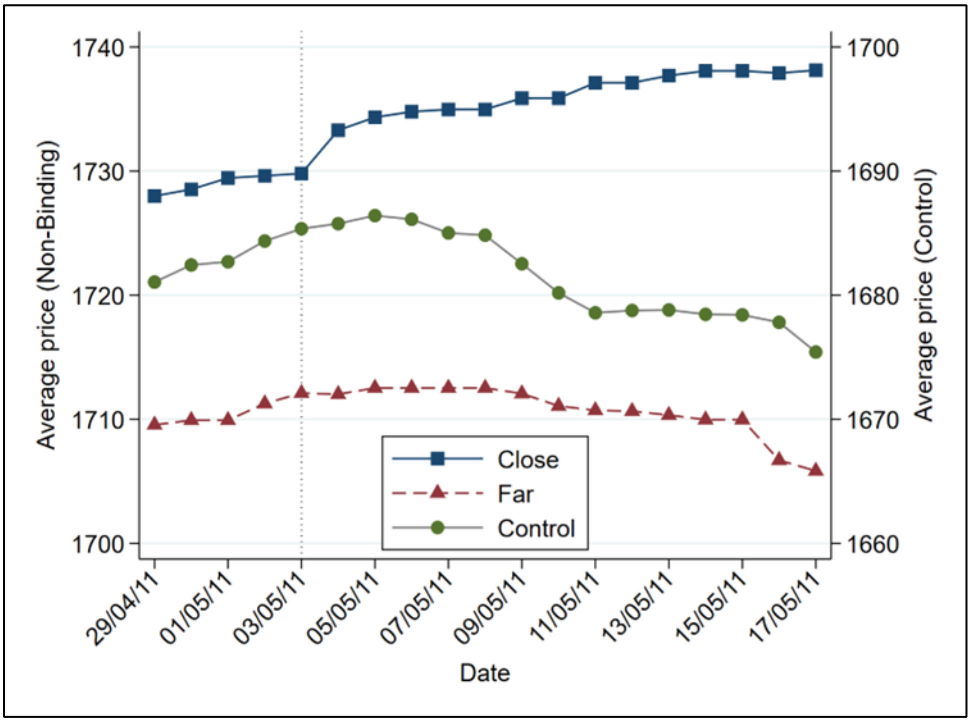
TABLE 4 – NON-BINDING STATIONS CHARACTERISTICS

	(1) Close	(2) Far	(3) p-value
Distance from ceiling (τ-1) (€)	0.016	0.029	0.000
Distance from ceiling (τ+14) (€)	0.008	0.035	0.000
Population density (#inhabitants/Km^2)	85	77	0.000
Distance from Peiraeus (Km)	183	191	0.000
Income (€)	16,031	15,668	0.000
#Competitors within 2Km	2.2	3.2	0.000
Tourists arrivals	21,767	20,864	0.000

Notes: Island socioeconomic and geographic characteristics were obtained from the Hellenic Statistical Authority. Tourist arrivals refer to arrivals by air or sea. Income per capita is based on data released by the Independent Authority of Public Revenue.
Source: Authors' calculations based on data from the Greek Ministry of Development.

Figure 5 plots the evolution of average prices for these two groups of non-binding stations, as well as the control group. The two groups' pricing trajectories differ substantially. The "close" group steadily increases its price throughout the whole period, even though the price of the control group clearly indicates a downward movement from the second week onwards. This suggests that tacit collaboration was far more effective for this group. In contrast, the "far" group, only slightly increases its price during the first week and then follows the general negative price trend, but with a much smaller slope. This suggests that some degree of coordination is also occurring among this group of stations; however, achieving such coordination appears to be considerably more challenging.

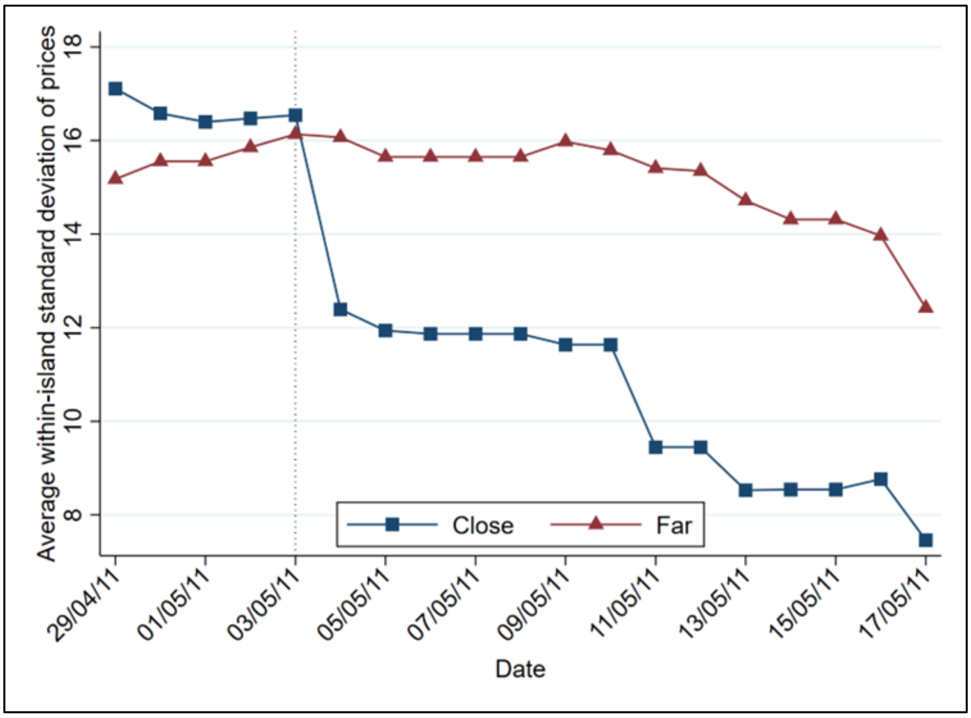
FIGURE 5 - AVERAGE PRICE EVOLUTION FOR FAR VS. CLOSE NON-BINDING STATIONS AND CONTROL GROUP



Notes: The figure plots average prices from 5 days before to 14 days after the introduction of the price ceiling, separately for binding stations and for the Close and Far groups among non-binding stations.
Source: Authors' calculations based on data from the Greek Ministry of Development.

Figure 6 further corroborates the evidence that coordinating prices was much easier for the "close", rather than the "far" group, by plotting the standard deviation of prices over time. As illustrated, the "close" stations adjust their prices synchronously, resulting in a continuous decline in price deviations within this group. In contrast, price dispersion among the "far" stations decreases only marginally, indicating an absence of clear consensus in their pricing behavior direction. Looking at individual gas stations among the "close" group, we do not detect any clear leadership behavior, such as the one documented in Byrne and de Roos (2019) for example. Remember that these are independent gas stations across islands (even though they may have the same brand name) and hence we should not expect any coordination across islands.

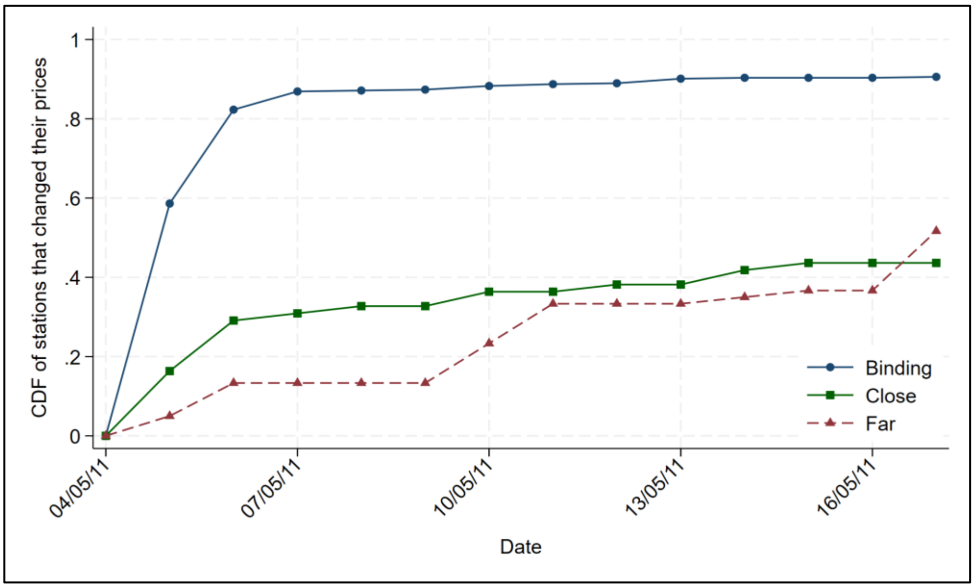
FIGURE 6 – STANDARD DEVIATION OF PRICES FOR FAR VS. CLOSE NON-BINDING STATIONS



Notes: The figure plots the standard deviation of prices within each island for each group (Close and Far), restricting the calculation to islands that have at least three stations in the group, from 5 days before to 14 days after the introduction of the price ceiling.
Source: Authors' calculations based on data from the Greek Ministry of Development.

Finally, in Figure 7 we examine the speed of adjustment, measured by the cumulative frequency of stations changing prices in the days following the introduction of the price ceiling, for the binding stations and the two groups of non-binding stations. As we can see, binding stations respond immediately within the first couple of days, as one would expect given that they have to adjust prices to the ceiling. On the other hand, stations that start "close" to the ceiling exhibit higher frequency of adjustment compared to the stations that start "far" from the ceiling. In particular, 34.6% of close stations change price within the first five days after the policy, compared to 13.6% of far stations, with the difference being significant (p-value = 0.009). This evidence suggests that coordinating actions toward the ceiling is faster for stations near the ceiling, most likely because the price cap acts as a magnet and the focal point is clear. In contrast, coordination towards the ceiling is more difficult, and hence slower, for stations farther away.

FIGURE 7 – SPEED OF ADJUSTMENT FOR BINDING & NON-BINDING STATIONS



Notes: The figure plots the cumulative frequency of stations that changed their prices between τ and τ +δ, where τ is the date of the introduction of the price ceiling and δ=1,…,14, separately for binding stations and for the Close and Far groups among non-binding stations.
Source: Authors' calculations based on data from the Greek Ministry of Development.

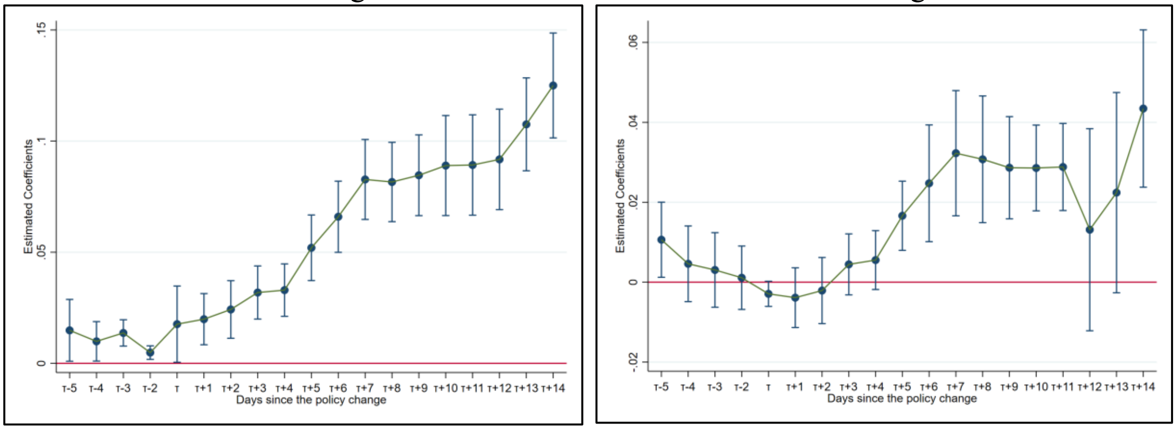
Therefore, it appears that the group of stations that starts near to the ceiling is better able to use the regulation as a focal point to come close, but not price at, the ceiling. The group of stations that starts far, has more difficulty coordinating actions. We estimate a dynamic DiD specification for both groups of firms to validate these differences. Figure 8 plots the regression coefficients together with their 95% confidence intervals.[18]

Panel A plots the "close" stations: all coefficients are positive and significant after the regulation and increase steadily throughout the period. In contrast, as seen in Panel B for the "far" stations, the price increases are significantly smaller in magnitude and oscillate over time, albeit they are still positive overall. Consequently, while "close" stations raise their prices, converging towards—though not reaching—the price ceiling, the stations in the "far" group, but to a markedly lesser extent and with considerable instability in their pricing behavior.

[18] Estimated coefficients are reported in Table A3 of the Appendix.

FIGURE 8 – PRICE CEILING REGULATION EFFECT FOR FAR AND CLOSE NON-BINDING STATIONS

Panel A: Close non-binding stations Panel B: Far non-binding stations



Notes: The figure plots the regression coefficients reported in Table A3 together with their 95% confidence intervals for and for the Close (left) and Far (right) groups among non-binding stations.
Source: Authors' calculations based on data from the Greek Ministry of Development.

Next, we subject the tacit collusion hypothesis to empirical scrutiny by exploring the various channels that previous work has identified as plausible mechanisms.

Competition. Tacit collusion is generally viewed as easier to maintain in more concentrated markets (Tirole, 1992; Knittel and Stango, 2003). If collusion is driving the results in Tables 2 and 3, we should expect a larger impact in markets with fewer competitors. To test this claim, we divide local markets as "high" or "low" competition, with low competition corresponding to monopoly or duopoly markets. We interact this variable in model (3). Table 4, column 1, reports the coefficients of interest. As we can see, markets with fewer competitors have a statistically significant larger increase in prices than markets with more competitors, in line with the hypothesis that firms in more concentrated markets react more to the collusive incentives than otherwise.

Cyclical demand. Another theoretical prediction of the tacit collusion hypothesis refers to how firms respond in periods of high demand. Theoretical predictions differ, with Rotemberg and Saloner (1986) arguing that it is more difficult to sustain collusion in period of high demand, because high current demand raises the present gains from cheating. However, their model assumes demand shocks are independently and identically distributed (i.i.d.). In contrast, Haltiwanger and Harrington (1991) assuming a cyclical demand derive the exact opposite result. Demand in our environment is highly cyclical, with thousands of tourists visiting these islands every summer. Hence, *a priori*, we would expect the Haltiwanger and Harrington (1991) prediction, that collusion is easier to sustain when demand is increasing, to be a better fit in this case.

To test this hypothesis, we collect additional data on tourist arrivals in the treated islands. We construct a measure of total tourist inflow by summing arrivals through airports and ports. Islands are then classified as having "high" or "low" tourism, depending on whether their total number of arrivals is above or below the median.[19] We interact this tourism variable in model (3). As shown in Table 4, column 2, islands with a higher number of tourist arrivals experience a larger and statistically significant increase in non-binding station prices. Hence, predictable demand increases in our environment seem to further incentivize non-binding firms to increase their prices in line with the Haltiwanger and Harrington (1991) theoretical prediction.

Branding and gas station reputation. A third possible channel we explore is that of reputation. As we emphasize earlier, all of these stations are dealer owned and they just use different brand names under franchisee agreements.[20] Hence the idea we want to explore here is that internationally recognized branded stations (such as BP or Shell) would be less likely to collude than Greek branded stations (such as Aegean or Elinoil) because they have a wider reputation to protect. We create a "Greek" vs. "Foreign" indicator and interact it in model (3).

[19] The median is calculated based on the distribution of arrivals in islands with non-binding gas stations.
[20] Almost all stations in the islands under examination are branded and there are very few independent stations. Moreover, there is no local (island level) concentration in ownership, so the level of competition is defined based on the number of gas stations.

Results in Table 4, column 3, reveal that indeed Greek branded stations increase their prices more than foreign branded ones following the price ceiling regulation.

Income. Another possible channel is that firms in islands with higher income populations are easier to coordinate actions due to lower demand elasticity. For example, (Stroebel and Vavra, 2019) provide evidence that wealthier consumers are less price sensitive, allowing firms to sustain higher markups. To examine this hypothesis we use declared income data by postal code from the Ministry of Finance to split the treated islands as above ("high") or below ("low") the median income. Table 4, column 4, shows that, indeed, price coordination is higher in richer islands, but the differences are not statistically significant.

Population density. A fifth possible channel for coordination is through comparison frictions and the search mechanism. Higher population density is associated with shorter distances between competing firms. On the one hand, greater spatial proximity reduces consumer search costs by making price comparisons easier, which intensifies competition and tends to lower prices (Varian, 1985; Ellison and Ellison, 2009). At the same time, greater spatial proximity increases price transparency among firms, facilitating monitoring and tacit coordination (Stigler, 1964; Byrne and de Roos, 2019). The net effect between these two forces is an empirical question. We operationalize search frictions by looking at population density and categorizing islands as above ("high") or below ("low") the median density and interact this variable in model (3). Table 4, column 5, highlights that gas stations in more densely populated islands are better able to coordinate actions and increase their prices more.

Cost differences. The last possible channel that we explore relates to cost differences. Theory predicts that as costs increase, collaboration at the focal point becomes easier to maintain. We operationalize this by measuring the distance of each island from Piraeus, which is the primary distribution center for gasoline products in Greece. We split islands as above (“high”) or below (“low”) the median distance and interact this variable in model (3) as in previous cases. Table 4, column 6, demonstrates that non-binding stations in islands further from Piraeus are raising their prices more, but the differences are not statistically significant.

While none of these channels provides conclusive proof on their own, taken together, they make a compelling case for price ceiling regulation providing strong incentives and serving as a focal point for tacit collusion among non-binding firms. At the same time, this collusive behavior exhibits significant heterogeneity in how quickly and how close to the ceiling it pushes prices up.

TABLE 4 - FOCAL POINT MECHANISMS

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln($Price_{ist}$)	ln($Price_{ist}$)	ln($Price_{ist}$)	ln($Price_{ist}$)	ln($Price_{ist}$)	ln($Price_{ist}$)
Sample	Treatment & Control (1st price ceiling)	Treatment & Control (1st price ceiling)	Treatment & Control (1st price ceiling)	Treatment & Control (1st price ceiling)	Treatment & Control (1st price ceiling)	Treatment & Control (1st price ceiling)
Interaction	Local Competition	Tourists Arrivals	Greek vs Foreign	Wealth	Population density	Distance from Piraeus
$NonBind_{s\_1}$ × $Post_{t\_1}$ × Low/Greek	0.055***	0.031***	0.040***	0.036***	0.031***	0.035***
	(0.009)	(0.006)	(0.005)	(0.006)	-0.006	(0.005)
$NonBind_{s\_1}$ × $Post_{t\_1}$ × High/Foreign	0.030***	0.043***	0.032***	0.040***	0.043***	0.044***
	(0.006)	(0.004)	(0.004)	(0.006)	-0.004	(0.007)
F-test (p-value) Low = High	*0.037*	*0.093*	*0.007*	*0.642*	*0.093*	*0.273*
Observations	43,725	43,725	43,725	43,725	43,725	43,725
R^2	0.951	0.952	0.951	0.952	0.952	0.952
Station FE	yes	yes	yes	yes	yes	yes
Day FE	yes	yes	yes	yes	yes	yes

Notes: All coefficients and standard errors are multiplied by 10. The dependent variable is the logarithm of retail price of product *j*, on island *i*, in gas station *g*, and day $t \in \{\tau - 1, \tau + 14\}$, where τ is the date of the price ceiling regulation. Standard errors are clustered at the island level and are reported in parentheses below coefficients. ***, **, * mark statistical significance at the 0.01, 0.05 and 0.10 level respectively.
Source: Authors' calculations based on data from the Greek Ministry of Development.

7. Concluding remarks

In this paper, we present the first systematic evidence of the short-run impact of price ceiling regulation on the retail gasoline market. We exploit a unique natural experiment and identify the impact of regulation by comparing unleaded 95 prices on gas stations in Greek islands affected by the policy change with those in the unaffected Attica prefecture. Having data both before and after the policy change uniquely allows us to see for which stations the price ceiling was not binding and to analyze their behavior.

Our results show that while stations for which the regulation was binding naturally reduced their prices, there is substantial heterogeneity in the behavior of non-binding stations. In particular, among Among non-binding stations, those with more favorable characteristics for collusion adjust prices faster, move closer to the ceiling, and exhibit lower price dispersion, consistent with more effective coordination We present evidence that the observed price increases among non-binding stations are attributable to tacit collusion, and we investigate several channels identified in the existing literature as potential mechanisms. Our analysis reveals that non-binding stations are more likely to raise prices in less competitive markets, when anticipated demand is higher, when the reputational costs of collusion are lower, and when price monitoring across firms is easier.

Overall, the results of our ex-post policy evaluation point to some unexpected consequences of a commonly discussed, yet little studied type of regulation. We provide evidence that such policies create incentives for tacit collusion, and show that this ability is not uniform across stations, identifying the characteristics that facilitate this behavior.

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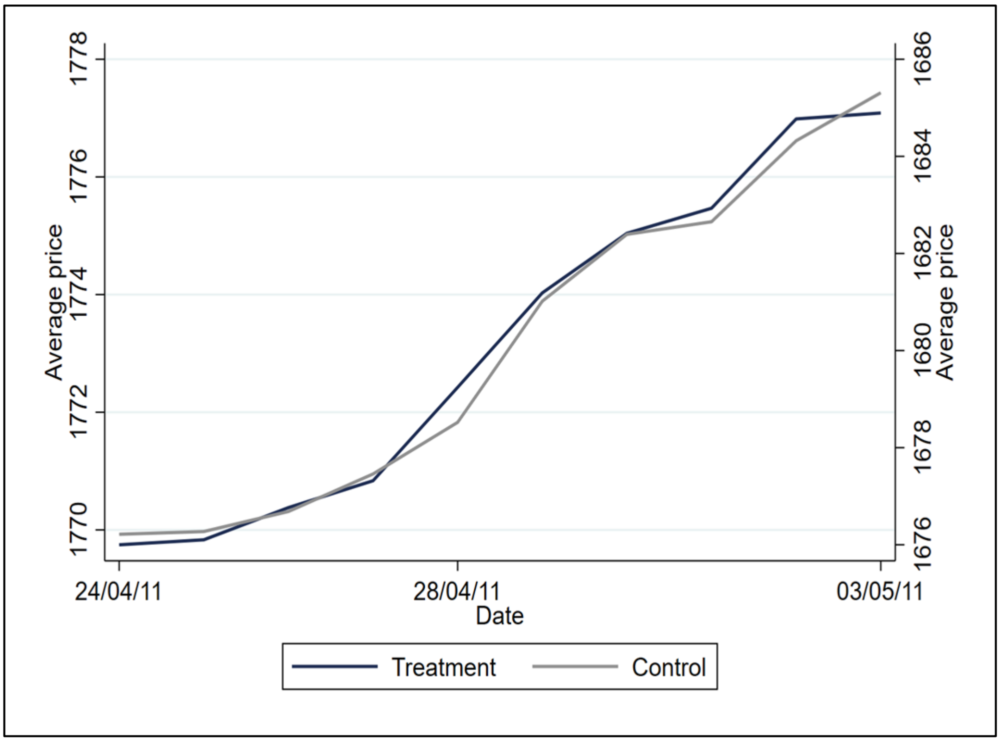
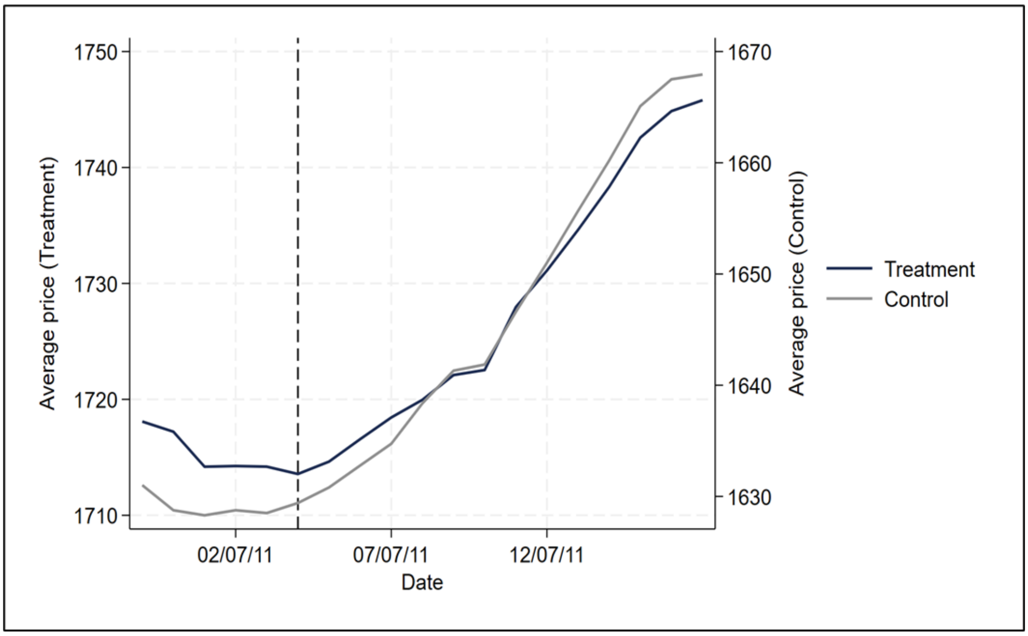
SUPPLEMENTAL APPENDIX

FIGURE A1 - AVERGE PRICE OF UNLEADED 95 IN TREATED AND CONTROL AREAS BEFORE PRICE CEILING REGULATION



Notes: The figure plots the evolution of the average price for Unleaded 95 in treated (24 islands) and control (Attica region) gas stations for ten days before the introduction of the price ceiling regulation.
Source: Authors' calculations based on data from the Greek Ministry of Development.

FIGURE A2 - AVERGE PRICES OF UNLEADED 95 BEFORE AND AFTER DEREGULATION



Notes: The figure plots the evolution of the average price for Unleaded 95 in treated (24 islands) and control (Attica region) gas stations for five days before and fourteen days after deregulation.
Source: Authors' calculations based on data from the Greek Ministry of Development.

TABLE A1 – DEFINITION OF BINDING STATIONS AND PRICE CEILING REGULATION

	(1)	(2)	(3)	(4)	(5)	(6)
Estimation Method	FE	FE	FE	FE	FE	FE
Dependent Variable	log(price)	log(price)	log(price)	log(price)	log(price)	log(price)
Time Window	[τ-1,τ+14]	[τ-1,τ+14]	[τ-1,τ+14]	[τ-1,τ+14]	[τ-1,τ+14]	[τ-1,τ+14]
Reference date	τ-1	τ-2	τ-3	τ-4	τ-5	Average [τ-5, τ-1]
$Bind_s \times Post_t$	-0.222***	-0.224***	-0.227***	-0.229***	-0.230***	-0.227***
	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)
$NonBind_s \times Post_t$	0.038***	0.034***	0.031***	0.029***	0.026***	0.030***
	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)
Observations	22,155	22,155	22,155	22,155	22,155	22,155
Number of binding stations	435	430	423	419	416	424
Number of non-binding stations	115	120	127	131	134	126
R-squared	0.962	0.962	0.962	0.962	0.962	0.962
Station FE	YES	YES	YES	YES	YES	YES
Date FE	YES	YES	YES	YES	YES	YES

Notes: All coefficients and standard errors are multiplied by 10. The dependent variable is the logarithm of retail price of product j, on island i, in gas station g, and day t. Standard errors are clustered at the island level and are reported in parentheses below coefficients. ***, **, * mark statistical significance at the 0.01, 0.05 and 0.10 level respectively.

Source: Authors' calculations based on data from the Greek Ministry of Development.

TABLE A2 - DYNAMIC EFFECT OF PRICE CEILING REGULATION

	(1)	(2)	
Dependent variable	ln(Price$_{ist}$)	ln(Price$_{ist}$)	
Sample	Treatment & Control (1st price ceiling)	Binding	Non-Binding
Treat$_i$ × Post$_{t-14}$	0.014*	0.013	0.018
	(0.007)	(0.008)	(0.011)
Treat$_i$ × Post$_{t-13}$	0.014**	0.013*	0.017
	(0.006)	(0.007)	(0.011)
Treat$_i$ × Post$_{t-12}$	0.013*	0.011	0.017
	(0.007)	(0.007)	(0.011)
Treat$_i$ × Post$_{t-11}$	0.013*	0.012	0.017
	(0.007)	(0.007)	(0.011)
Treat$_i$ × Post$_{t-10}$	0.013*	0.012	0.017
	(0.007)	(0.007)	(0.011)
Treat$_i$ × Post$_{t-9}$	0.013*	0.012	0.016
	(0.007)	(0.007)	(0.011)
Treat$_i$ × Post$_{t-8}$	0.013*	0.013*	0.016
	(0.007)	(0.007)	(0.011)
Treat$_i$ × Post$_{t-7}$	0.011*	0.011	0.013
	(0.006)	(0.007)	(0.010)
Treat$_i$ × Post$_{t-6}$	0.014***	0.013**	0.018**
	(0.005)	(0.005)	(0.008)
Treat$_i$ × Post$_{t-5}$	0.008	0.007	0.013***
	(0.005)	(0.006)	(0.004)
Treat$_i$ × Post$_{t-4}$	0.006	0.005	0.007**
	(0.005)	(0.006)	(0.003)
Treat$_i$ × Post$_{t-3}$	0.007	0.006	0.008**
	(0.004)	(0.005)	(0.003)
Treat$_i$ × Post$_{t-2}$	0.005**	0.006*	0.003
	(0.003)	(0.003)	(0.002)
Treat$_i$ × Post$_t$	-0.126***	-0.161***	0.007
	(0.026)	(0.026)	(0.004)
Treat$_i$ × Post$_{t+1}$	-0.185***	-0.236***	0.007*
	(0.032)	(0.031)	(0.004)
Treat$_i$ × Post$_{t+2}$	-0.197***	-0.252***	0.010**
	(0.036)	(0.036)	(0.005)
Treat$_i$ × Post$_{t+3}$	-0.192***	-0.247***	0.018***
	(0.036)	(0.036)	(0.004)
Treat$_i$ × Post$_{t+4}$	-0.191***	-0.247***	0.019***
	(0.036)	(0.036)	(0.004)
Treat$_i$ × Post$_{t+5}$	-0.179***	-0.235***	0.034***
	(0.036)	(0.036)	(0.004)
Treat$_i$ × Post$_{t+6}$	-0.168***	-0.224***	0.044***
	(0.037)	(0.036)	(0.005)
Treat$_i$ × Post$_{t+7}$	-0.158***	-0.215***	0.056***
	(0.037)	(0.036)	(0.005)
Treat$_i$ × Post$_{t+8}$	-0.162***	-0.219***	0.055***
	(0.037)	(0.037)	(0.005)
Treat$_i$ × Post$_{t+9}$	-0.163***	-0.220***	0.055***
	(0.037)	(0.036)	(0.005)
Treat$_i$ × Post$_{t+10}$	-0.161***	-0.218***	0.057***
	(0.037)	(0.036)	(0.006)
Treat$_i$ × Post$_{t+11}$	-0.161***	-0.218***	0.058***
	(0.037)	(0.036)	(0.006)
Treat$_i$ × Post$_{t+12}$	-0.160***	-0.216***	0.051***
	(0.036)	(0.036)	(0.013)
Treat$_i$ × Post$_{t+13}$	-0.147***	-0.202***	0.063***
	(0.036)	(0.036)	(0.013)
Treat$_i$ × Post$_{t+14}$	-0.129***	-0.185***	0.082***
	(0.034)	(0.034)	(0.011)
Observations	42,833	39,498	30,218
R-squared	0.940	0.952	0.955
Station FE	YES	YES	YES
Day FE	YES	YES	YES

Notes: All coefficients and standard errors are multiplied by 10. The dependent variable is the logarithm of retail price of product j, on island i, in gas station g, and day t. Standard errors are clustered at the island level and are reported in parentheses below coefficients. ***, **, * mark statistical significance at the 0.01, 0.05 and 0.10 level respectively.

Source: Authors' calculations based on data from the Greek Ministry of Development.

TABLE A3 – DYNAMIC EFFECTS FOR CLOSE AND FAR NON-BINDING STATIONS

	(1)	(2)
Dependent variable	$\ln(Price_{ist})$	$\ln(Price_{ist})$
Sample	Close & Control	Far & Control
$Treat_i \times Post_{t-5}$	0.015**	0.011**
	(0.006)	(0.004)
$Treat_i \times Post_{t-4}$	0.010**	0.005
	(0.004)	(0.004)
$Treat_i \times Post_{t-3}$	0.014***	0.003
	(0.003)	(0.004)
$Treat_i \times Post_{t-2}$	0.005***	0.001
	(0.001)	(0.004)
$Treat_i \times Post_{t}$	0.018**	-0.003*
	(0.008)	(0.001)
$Treat_i \times Post_{t+1}$	0.020***	-0.004
	(0.005)	(0.003)
$Treat_i \times Post_{t+2}$	0.024***	-0.002
	(0.006)	(0.004)
$Treat_i \times Post_{t+3}$	0.032***	0.004
	(0.006)	(0.003)
$Treat_i \times Post_{t+4}$	0.033***	0.006
	(0.006)	(0.003)
$Treat_i \times Post_{t+5}$	0.052***	0.017***
	(0.007)	(0.004)
$Treat_i \times Post_{t+6}$	0.066***	0.025***
	(0.007)	(0.007)
$Treat_i \times Post_{t+7}$	0.083***	0.032***
	(0.008)	(0.007)
$Treat_i \times Post_{t+8}$	0.082***	0.031***
	(0.008)	(0.007)
$Treat_i \times Post_{t+9}$	0.085***	0.029***
	(0.008)	(0.006)
$Treat_i \times Post_{t+10}$	0.089***	0.029***
	(0.010)	(0.005)
$Treat_i \times Post_{t+11}$	0.089***	0.029***
	(0.011)	(0.005)
$Treat_i \times Post_{t+12}$	0.092***	0.013
	(0.011)	(0.011)
$Treat_i \times Post_{t+13}$	0.108***	0.022*
	(0.010)	(0.011)
$Treat_i \times Post_{t+14}$	0.125***	0.043***
	(0.011)	(0.009)
Observations	19,640	19,740
R-squared	0.960	0.958
Station FE	YES	YES
Day FE	YES	YES

Notes: All coefficients and standard errors are multiplied by 10. The dependent variable is the logarithm of retail price of product j, on island i, in gas station g, and day $t \in \{\tau - 1, \tau + 14\}$, where τ is the date of the price ceiling regulation. Standard errors are clustered at the island level and are reported in parentheses below coefficients. ***, **, * mark statistical significance at the 0.01, 0.05 and 0.10 level respectively.
Source: Authors' calculations based on data from the Greek Ministry of Development.