Department of Economics

Three Essays in Competition and Consumer Policy

Luca Aguzzoni

Thesis submitted for assessment with a view to obtaining the degree of
Doctor of Economics of the European University Institute

Florence, October 2011

EUROPEAN UNIVERSITY INSTITUTE
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ABSTRACT

The thesis is made up of three chapters:

The first chapter estimates the effects of antitrust investigations on the market value of the investigated firms. This analysis offers insights on the performance of competition policy and its enforcement. The interest is on investigations carried out by the Italian Competition Authority between 1991 and 2007. We find that the start of the investigation is associated with an average drop of 0.6% in the market value of investigated firms and a later infringement decision implies an average drop of 1%. The event associated with the highest impact is the decision of the last Court of Appeal. When the last Court upholds the Authority's infringement decision the market value of firms drops between 3% and 6%. Interestingly, there is no effect when the last Court annuls the Authority's decision.

The second and third chapters study the effects, on retail fuel prices, of a price comparison policy (a typical consumer policy intervention) introduced in the Italian pay-toll highways refueling market. In particular, the second chapter performs an empirical analysis while the third chapter presents an agent based computational economic (ACE) model, which aims to rationalize the empirical evidence and to inform the policy design.

Differently from what was expected (by policy makers and consumers associations), the empirical analysis finds that the price comparison policy is associated with a small, but statistically significant, increase in the average price of fuel (0.55 euro cents per liter). Nevertheless, despite this average increase in fuel prices, the policy might help (active) consumers make informed choices and save around 1 euro cent per liter.

The ACE model predicts that the introduction of price comparison has a limited effect on market prices as price competition among retailers is only marginally fostered. In addition, the model suggests that consumers that make use of price comparison might save around 0.5 euro cents per liter. These results are consistent with the empirical findings in suggesting that the price comparison policy had a limited impact on fuel retail prices and the overall effect on consumers is mixed.

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CHAPTER 1

ANTITRUST INVESTIGATIONS AND FIRMS' MARKET VALUE: EVIDENCE FROM THE ITALIAN CASE

Abstract

This paper uses standard event studies techniques (parametric and non-parametric) to estimate the impact of antitrust investigations on the stock price of investigated firms. The interest is on the investigations carried out by the Italian Competition Authority between 1991 and 2007. We consider two types of violations: abuse of dominance and cartelization practices. The investigations we study consist of three different steps: start of investigation, Authority's decision, Court of Appeal's decision. The start of the investigation is associated with an average drop of 0.6% in the market value of investigated firms. An infringement decision by the Authority implies an average drop of 1% in the market value (although marginally statistically significant). An acquittal decision has no effect on market value. Finally, the decision of the last Court of Appeal, when it upholds the Authority's decision, has the greatest impact on firms' market value (drop between 3% and 6%).

1.1 Introduction

This paper studies the stock market reaction to antitrust investigations. The interest is on the antitrust investigations carried out by the Italian Competition Authority (ICA), also known with the name of "Autorità Garante della Concorrenza e del Mercato" (AGCM), between 1991 and 2007. We look specifically at the two types of antitrust investigations: abuse of dominance cases and anticompetitive agreement cases (i.e. cartels cases). These are violations of article 2 and article 3 of the Italian competition law (these two articles can be seen as the Italian counterpart of article 101 and article 102 of EU competition law)[1].To estimate the impact of antitrust investigations on the market valuation of the investigated

[1]Notably, this study does not deal with merger investigations cases.

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firms we use standard event studies techniques[2].

A typical Italian antitrust investigation consists of three main events: 1) start of the investigation; 2) decision of the Antitrust Authority; and 3) decisions of the Courts of Appeal[3]. For the last two events we can further distinguish between two different realizations. Indeed, both the Authority's and the Court of Appeal's decision can have "positive" or "negative" effects on firms, while we assume that the start of the investigation is always a negative realization for the investigated firms. About the Authority's decision we can observe an *infringement* (negative) decision or an *acquittal* (positive) decision. For the Court of Appeal's decision we can observe an *upheld* (negative) decision or an *annulment* (positive) decision.

In the empirical analysis we look in turn at these three different events and their realizations and we estimate the market reaction over some defined event windows. To our knowledge this is the first work that applies event study techniques to study Italian antitrust investigations. Close to this paper there are similar studies that consider antitrust investigations carried out by other authorities. Bosch and Eckard (1991) conduct a similar study for the US by looking at federal indictments (they find that indicted firms lose around 1.08% of their market value) whereas Bizjak and Coles (1995) look, always for the US, at the effects of private antitrust litigations (they find an average wealth loss of around 0.6% after the filing of the private action), Mariniello (2006) study the OFT (Office for Fair Trading) investigations in the UK[4] Finally Langus et al. (2010) study the impact of EU investigations relating to art. 101 and art. 102 TFU violations (they find a reduction of around 2% following a dawn raid). Although it is not the aim of this paper, this work might also be related to the literature that studies the impact of antitrust authorities. About the Italian Competition Authority a paper with this clear aim is Sabbatini (2008).

The analysis is conducted both on the full sample (1991-2007) and on two smaller subsamples (1991-1997 and 1998-2007). In doing so we can check if the market reaction to antitrust investigations has changed over time. We break our sample around 1998 because there is evidence of a change in the enforcement of competition law and use of fines (Ghezzi and Polo (2001)). For the empirical analysis we build a novel dataset consisting of 155

[2] In particular we follow two estimation procedures to perform both a parametric and a fully non-parametric event studies. Event studies is an established tool in economics and finance studies that has been widely applied to examine antitrust events (for a recent survey see Cichello and Lamdin (2006)).

[3] In this paper we look in detail only at the decision by the last court of appeal known as *Consiglio di Stato*. The ruling of the *Consiglio di Stato* is independent of the ruling of the first court of appeal (i.e. the TAR, *Tribunale Amministrativo Regionale*).

[4] Only the single firms event studies are reported as there is no aggregation over firms.

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case/firms observations in which the investigated firm is listed (or it is owned by a listed parent company).

For the event start of investigation we find that firms on average lose 0.6% (statistically significant) of their market value. Interestingly, there is a clear difference between the impact observed pre-1998 and the one post-1998 as in the former sample the loss is around 1% (statistically significant) whereas in the latter sample we find no effect.

At the stage of the Authority's decision we distinguish between two events: infringement decisions and acquittal decisions. For the former we find a negative effect that is around 1% (but marginally significant), for the latter case we find a positive effect but not statistically significant. As for the event start of investigation we find that, for infringement decisions, the market reaction is much stronger (negative) and significant in the period pre-1998.

About the last Court of appeal we distinguish between upheld and annulment decisions. When the last Court upholds the Authority's decision we find a negative impact of around 2% (statistically significant). For the annulments we find a positive effect but not statistically significant[5].

The paper proceeds as following. Section 1.2 describes Italian antitrust enforcement and the relevant details of Italian antitrust investigations. Section 1.3 describes how we apply event studies techniques to perform the analysis and describes the selected sample. Section 1.4 presents the results of the empirical analysis. Finally Section 1.5 concludes the paper.

1.2 Italian antitrust enforcement

The application of competition law in Italy is a matter of recent history. Only in 1990, exactly one hundred years after the American Congress passed the Sherman Act, the Italian legislator drew up a law to discipline market competition. It is with the law no. 287 of 10th October 1990 (The Competition and Fair Trading Act) that competition policy starts in Italy. The same act, beyond introducing competition law, also establishes the body responsible for its enforcement: the Italian Competition Authority (ICA), known with the name of "Autorità Garante della Concorrenza e del Mercato" (AGCM[6]). Since its origin, the Authority was granted the status of *independent agency* to shield it from the interference of the government and the other political institutions.

[5] For the event last court of appela we do not perform a pre-1998 post-1998 comparison because of the small sample size.

[6] In the rest of the paper we will refer to the Italian Competition Authority both with the acronym of ICA or AGCM.

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The Authority monitors the functioning and competition of markets and intervenes whenever competition rules are violated. It has two main areas of action: on the one hand it oversees over agreements (cartels), mergers, acquisitions and cases of abuse of dominant position; on the other hand it oversees over misleading and comparative advertising, and on conflict of interest.

The ICA cooperates and works jointly with the European Commission as the Italian competition act was drawn in light of the already established European Competition law. There are in place specific guidelines that determine the area of competence of the two jurisdictions, however the distinction is not always clear. Indeed, there are cases, such as mergers and acquisition cases, for which the distinction is clear cut (for example it is based profits thresholds), while for other cases, such as cases of cartels and abuses of dominant position, the distinction is less clear. For these latter cases, usually, whenever the case has only national relevance it falls within the single member state jurisdiction. On the contrary, for cases that have an impact on the commerce between member states, the European law should be applied. Nonetheless, member states are free to apply stricter restriction on their domestic market. However even when only national jurisdiction is applied, member states should not leave unpunished conducts that violate the Article 101 or 102 of the European Law[7].

In this paper the interest is on two kinds of competition law infringements: cartels (anticompetitive agreements) and abuses of dominant position. Hence we restrict our analysis to violations of article 2 and the violation of article 3 of the Italian competition law. These two articles are the Italian counterpart of article 101 and article 102 of EU competition law. Article 2 regulates cartels (we do not consider concentrations cases) while Article 3 disciplines abuses of dominant position. In addition to these two types of violations, we also consider violations of Article 101 and Article 102 of the EU competition law directly enforced by the Italian Competition Authority.

The next sessions describe the different steps (i.e. events) of a typical Italian antitrust investigation. We describe how the Italian Competition Authority works, takes its decision and communicates its decision and we show how we exploit these features for the purposes of our analysis.

[7]Council Regulation (EC) No 1/2003 of 16 December 2002 on the implementation of the rules on competition laid down in Articles 81 and 82 of the Treaty (Text with EEA relevance)

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1.2.1 How an antitrust investigation develops

To study the impact of antitrust investigations we first have to analyze and understand how the competition Authority operates and what its specific features imply for our analysis. The first event of interest is the start of the investigation. This event, as we explain below, is itself the outcome of a preliminary confidential pre-investigation following internal examinations or external complaints.

Indeed, the AGCM, among other duties, continuously monitors the market. Within the Authority's instruments for monitoring the market there is the launching of the so called "Indagini conoscitive del Mercato" which are general market-wide investigations meant to study in depth the functioning of a specific market and to deter and uncover possible infringements of competition law. Such general investigations can be started either by the Authority itself or following the suggestions of other public bodies (other regulatory authorities, government, public administration). As an offspring of these general market investigations the Authority can then undertake specific firm level investigations for competition law infringement. In addition to internal examinations, also external formal complaints can be presented to the Authority in order to report competition practices deemed anticompetitive. Subjects that can file such formal complaint are firms public administration bodies, and even private citizens.

Once there is a formal request to start an investigation, either from within the Authority or from an external complaint, the case is then assigned to the relevant directorate, within the AGCM, that proceeds with a preliminary analysis of the case. The preliminary analysis serves to provide evidence to the Authority on whether there are the conditions to proceed with a formal investigation or whether the case should be dropped. In case the decision is to proceed, the parts directly involved (firms) are contacted and the Authority formally starts the antitrust investigation.

Usually, the Authority sets also a timeframe of 240 days within which it is bound to close the investigation. During the investigation phase the relevant information about the case is collected. Both the plaintiff (in case there is one) and the defendant are expected to collaborate with the Authority and can access the unrestricted documentation supporting the case. The antitrust officials have also power of inspection and can dawn raid[8] the headquarters and offices of the accused firms to seek evidence of violation.

Usually, 30 days before the set end of the investigation the Authority communicates the

[8] In this activity the Antitrust officials are attended by the Guardia di Finanza officers (a police corp responsible for border control and for investigating fraud) .

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results of the preliminary inquiry to the parts. Then the parts have some time (until 5 days before the end of the investigation) to present further memoirs.

At the end of this process there is the second event of interest of the antitrust investigation, that is the decision of the antitrust Authority. At this stage all parts convene at the final hearing in front of the *Collegio* (College), composed by the President and the four *componenti* (members) of the Authority. The Authority's decision is then taken in private, by a majority vote by the 5 members of the *Collegio*, taking into consideration the results of the investigation and the final documentation provided by the parts.

In case the defendant is found guilty of violating competition law, the ICA orders the ceasing of the identified anticompetitive conduct. Moreover, the Authority can also inflict a monetary fine up to 10% of the convicted firms' annual turnover, depending on the gravity of the violation. A fine is also contemplated in cases in which convicted firms fail to put an end to their misconducts. In extreme cases, when convicted firms repeatedly fail to cease the anticompetitive behaviors, the Authority can also order the closure of the firms for up to 30 days. The Italian legislator, in line with European law, did not envisage any penal consequences for competition law violation.

After the first two above events the final event of interest is represented by outcome of the appeal procedure that for the Italian case consists of two separate stages. Convicted firms can indeed appeal against the antitrust Authority decisions to the regional administrative Court (*Tribunale Amministrativo Regionale,* TAR). To this Court private subjects can appeal against decisions of the public administrations or other public bodies such as the Antitrust Authority. Usually the TAR decides on the legitimacy of the Authority's decision and verify if there were irregularities in the decisional process. TAR decisions can then be appealed both by the AGCM, in case the TAR annuls the Authority's decision, and the defendant, in case the TAR upholds the Authority's decision. The Court of last resort is called the *Consiglio di Stato* (CdS) after whose decision the sentence come to judgement.

After identifying the three events of interest, in the next section, we describe how the information about the outcome of these events is disclosed to the public.

1.2.2 Disclosure of event information

For the purpose of our research it is important to look at how the different bodies taking the relevant event decisions (i.e. AGCM, TAR and CdS) communicate their decisions to the

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public. Indeed, whenever novel information, about the antitrust proceedings, is released to the general public the investors of the firms under investigation acquire novel information that might change their future expected profitability of the firms and as a consequence their present market valuation.

For the sake of our analysis, we focus first on the two critical communications made by the Authority: the official start and the end of the investigation. Then we consider the information disclosure taking place after the decision of the Courts of Appeal.

About the ICA we find that the Authority is conscious of the possible destabilizing effects its news might bring to the stock market[9]. Hence, the Authority claims to release information only when stock markets are closed. About the events we find that soon after formally starting an investigation the Authority releases a press statement on its website[10]. At the same time major press agencies are informed. By doing an extensive analysis on the press coverage of these releases we indeed find that, for all the considered investigations, the major newspapers and press agencies report the news the day after the ICA publishes a notice of the investigation on its website.

Furthermore we find no evidence of leakages of information during the internal pre-investigation stage as we find notice of the start of an antitrust investigation only after the Authority official press release.

The second critical press release made by the ICA relates to the end of the investigation, in which the Authority communicates its final decision about the case. Differently from the start of the investigation, at this stage investors might have already formulated some expectation about the final decision of the Authority. However, there is no reason to believe that investors correctly anticipate the decision, for instance the exact amount of the fine or the extent to which the Authority's decision will affect certain practices. Hence also at this stage there is some degree of novelty in the information disclosed.

The press statement of the Authority at the end of the investigation phase appears to be quite transparent as a large amount of unrestricted information relating to the discussed case is made public. Again, when we perform a check on the press coverage of past events, we find that major newspapers and press agencies report the news the day after it appears

[9] For instance see: "Istruttoria Antitrust su Mediaset ma il comunicato era un abile falso", La Repubblica, 30 October 2007.

[10] *Comunicati Stampa,* www.agcm.it. Together with the press statement the authority also publishes relevant unrestricted documentation about the case supporting its decision to start the investigation.

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on the ICA's website. Moreover, during the entire investigation process we find no evidence of leakages of information about the Authority proceedings[11].

Thus for what concerns the Authority information disclosure procedure we can state it is, at the same time consistent, timing, transparent and prudential about the management of sensible information.

Unfortunately we do not find the same transparency and the same clarity when we deal with the last event of the antitrust proceeding. Indeed both the TAR and the Consiglio di Stato decisions do not communicate in a similar straightforward and transparent way. Also, for both the TAR and the Consiglio di Stato, we find the presence of two important stages of the decisional process, the decision (*sentenza)* and the publication of the judgment (*deposito della sentenza)* that do not take place at the same time and both might have some degree of novelty. Sometimes it can be the case that, at the first stage (decision), investors already have all the relevant information about the case (annulment or not of the ICA's decision) while other times relevant details might be disclosed only at the stage of the publishing of the judgment. Moreover, around these two dates we find, on the press, an increasing level of speculation about the final outcome of the cases. Hence it is clear that for these last stage the date of the event is not exactly identified.

The following section presents some descriptive data on the antitrust investigations, and incidental decisions, carried out by the Italian Competition Authority between the period that goes from 1991 (when enforcement of competition law started in Italy) until July 2007.

1.2.3 Italian antitrust investigations

This paper studies the impact of the different events of a typical antitrust investigation on the market value of the investigated firm(s). The interest is on the investigations carried out by the Italian Competition Authority in the period between 1991 and 2007. The above sections described the different steps of a typical Italian antitrust investigation, including the appeal procedure, and document the process of information disclosure relevant to each step. This section provides some information about the sample of antitrust investigations carried out by the ICA and explains the selection of the sub-sample of cases for the empirical analysis.

To conduct the empirical analysis we first build a dataset containing all the publicly available antitrust investigations, for which the ICA has already reached a decision, from 1991

[11] Officials of the antitrust authority are also bound by an ethical code not to disclose sensible information.

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until June 2007[12]. We then select only the decisions relating to cases of abuse of dominance and cartels case. As Table 1.1 shows we find a total of 159 cases. Among these cases, 15 lead to an acquittal decision whereas the other 144 ended with an infringement decision. The infringement decision makes always provision for the ceasing of the anticompetitive practice but it is not always followed by a fine. This is especially true during the early years of antitrust enforcement Ghezzi and Polo (2001). Indeed for only around half of the infringement cases (77 cases) the Authority's infringement decision also makes provision for a fine for some, or all, the convicted firms involved in the antitrust case.

Table 1.1: Authority decisions by type of violations, cases

		Type		
Decision	Tot. Cases	Cartel	Abuse	Cartel & Abuse
Infringement and fine	77	51	23	3
	48%	66%	30%	4%
Infringement w/o fine	67	28	29	10
	42%	42%	43%	15%
No Violation	15	3	4	8
	10%	20%	27%	53%
Total	159	82	56	21
	100%	52%	35%	13%

Note 1: Selected Italian antitrust investigation from 1991 until June 2007. Art. 2 infringements only includes cartel cases.
Note 2: art.101 and art. 102 infringements are included respectively in art. 2 and art. 3 infringements.
Note 3: Percentages in column 2 are column percentages; while in column 3, 4 and 5 are row percentages

Table 1.1 also presents the breakdown of the Authority's decisions by type of infringement. Looking at the breakdown for the entire sample, we notice that the majority of the investigations are for cartel violations (52%) while only 35% are abuse of dominance cases. Another, 13% of cases are investigations for both types of violations[13]. The breakdown is

[12] For all antitrust investigation it is possible to access the unrestricted documenatation via the Authority's website (www.agcm.it). For each case we find the documents making the case for the start of the investigation and those relative to the Authority's final decision. Such documents provide an accurate description of the case under discussion with details about all actors involved. It is from these documents that we can recover the judgement relative to each individual firm, and in case of a fine the amount of the individual fine.

[13] These are cases in which a dominant firm is investigated both for abuse of dominance and for anticom-

slightly different if we focus on those cases for which the ICA inflicts a fine. Indeed, among these cases about 66% are related to cartel infringements, 30% abuse of dominance and only 4% of cases refer to both types of violations. Regarding the cases in which there is an infringement decision without fine, the percentage of cartel cases is similar to the percentage of abuse of dominance cases (42% and 43% respectively). From this descriptive analysis it is clear that cartel cases are more likely to imply a fine.

Among the analyzed 159 cases, those for which the investigation resulted in a clearance is relatively small (less than 10% of the total). Most likely conducting a confidential pre-investigations before starting the official investigation limits the number of "mistakes".

In our analysis we are mainly interested at the effects that an antitrust investigation has at the individual firm level. We hence disaggregate the sample analyzed in the above table and look at the individual firms that are involved in each case[14].

Table 1.2: Authority decisions by type of violations, firms

		Type		
Decision	Tot. Firms	Cartel	Abuse	Cartel & Abuse
Infringement and fine	388	336	27	25
	70%	87%	7%	6%
Infringement no fine	127	71	37	19
	23%	56%	29%	15%
No violation	37	12	4	21
	7%	32%	11%	57%
Total	552	419	68	65
	100%	76%	12%	12%

Note 1: Selected Italian antitrust investigation from 1991 until June 2007. Art. 2 infringements only includes cartel cases.
Note 2: art.101 and art. 102 infringements are included respectively in art. 2 and art. 3 infringements.
Note 3: Percentages in column 2 are column percentages; while in column 3, 4 and 5 are row percentages

From table 1.2 we can see that in the 159 cases selected, there are a total of 552 firms involved (observations) [15]. The fined firms account for the 70% of the total while only

petitive agreements with its smallers competitors.

[14] This is possible because from the documentation of the authority's proceedings we can recover all the relevant information relating to the single firms involved in each case.

[15] Some firms are repeat offenders and appear in the sample more than once. We indeed define the

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23% of the firms violated antitrust law but did not receive any fine. This finding might be consistent with the fact that cartel cases in which a large number of firms is involved are more likely to be punished in a harsher way. Finally, for only the 7% of the observations in our sample the investigation resulted in a clearance of the case. As expected, among the fined cases the great majority of observations relates to cartel decisions (87% of the sample). The high number of firms involved in cartel violations compared to the number of firms involved in abuse of dominance should not be surprising since cartels usually involve groups of firms whereas abuse of dominance usually involves a single offender. For the cases in which there is an infringement decision without fines the majority of observations relate again to cartel violations (56%). About the category *No Violation* it is the joint *Cartel and Abuse* investigation that has the highest percentage of observations (57% of the total).

About the magnitude of the sanctionatory activity we find that, between 1991 and July 2007, the Authority has fined firms for a total of roughly 2 billions euros with the amount of individual fines ranging from symbolic fines (of one thousand euros) to a record fine of 290 million euro[16]. The Authority's decisions are usually appealed. Among the antitrust cases, in which the Authority issues a fine, we find that in 78% of cases at least a convicted firm appeals against the decision. When we look at the firm level data we find that 331 of the 388 fined firms appealed against the decision. At the individual firm level we also find that the average fine is about 5 million euros however given a standard deviation of 20 million euro the sample is highly dispersed. Also, a median fine of only 0.24 million euros suggests that the fine distribution is highly right skewed.

1.2.3.1 Antitrust fines

From chart 1.1 we can see the evolution over time of the average fine given by the ICA for cartel and abuse of dominance cases. The chart shows the average fine (bars), by year, from 1992[17] until 2007 (monetary values are expressed in 2007 euros). The chart clearly shows that during the last years of activity the average amount of fines increased sharply. Moreover, we find that in the period 1992-1997 the Authority inflicts a fine in only 43% of the infringement cases whereas in the subsequent period (1998-2007) the Authority inflicts a fine in the 65% of the infringement cases[18]. As remarked by Ghezzi and Polo (2001), it is

case/firm combination as our observation of interest.

[16] Fine given to the firm ENI SpA for the case Eni-Trans Tunisian Pipeline in 2006.

[17] No fine was inflicted in year 1991 by the Italian Competition Authority.

[18] The exceptionally high value registered in year 2006 is explained by the record high fine of 290 million euro given to ENI SpA for the case Eni-Trans Tunisian Pipeline in 2006.

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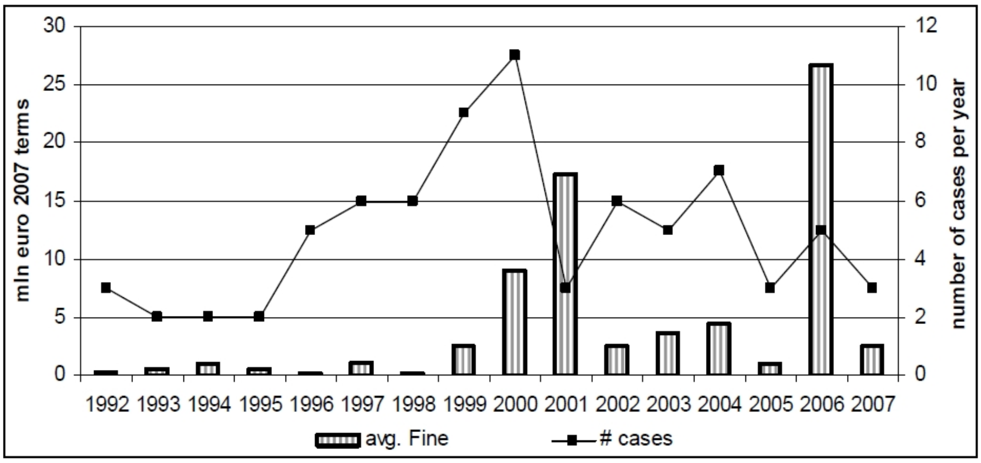
Figure 1.1: Antitrust Fines and Cases



only from 1997 that fines become to be used more frequently and systematically. During the early years of the ICA fines were mostly seen as the last resort and, for this reason, they were used only in exceptional cases. Also, from the chart we can see (solid line) that the majority of the cases that are in our sample are in the years between 1997 and 2004, as in the two periods 1992-1996 and 2005-2007 the Authority administered a relatively lower number of cases.

Given the judicial process in place, the fine inflicted by the Italian Competition Authority might be annulled or reduced during the appeal procedure. To study the impact of antitrust fines on firms' market value we also have to look at the realizations of these events. For instance, in the extreme case that in the appeal procedure all decisions taken by the antitrust Authority are annulled we would expect no reaction after an antitrust conviction.

Hence, below we provide some descriptive analysis about the relationship between the initial fine and the final fine a convicted firm has eventually to pay.[19]. Table 1.3 shows the breakdown of initial and final fine by type of violation[20]. On average fines for abuse of dominant position are the highest. However we do find wide variation in the amount of the fine, given the high standard deviations the group averages. If we compare the average final fine with the average initial fine for the three groups we find that the size of the final fine,

[19]We take as final fine the fine amount established in the decision of the last court. The last court is usually the Consiglio di Stato, however not all the cases are appealed beyond the Authority's decision or beyond the first court of appeal.

[20]For this table we restricted the sample to the cases that had both initial and final fine (i.e. cases that were appealed and that come to judgement).

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as a share of the initial fine, goes from 56% to 78%.

Table 1.3: Initial and final fine by type of Violation

Types of violation	Firms	Average fine mln euro Initial fine	Final fine	Ratio Final/Initial
Cartel	286	4.52	2.52	56%
		(13.72)	(10.47)	
Cartel & Abuse	25	1.89	1.48	78%
		(2.57)	(2.64)	
Abuse	26	10.92	7.13	65%
		(31.38)	(22.99)	
Total	337	4.82	2.8	58%
		(15.4)	(11.59)	

Note 1: Standard deviations in parenthesis

We investigate further the issue of fine reduction and, in the next table, we compare the average reduction found in the Italian sample against the average reduction that is found for the European Commission fines.

Table 1.4: Antitrust fines reduction: Italian vs European cases

	period	# cases	Fine reduction avg. reduction	stdv	Share upheld cases % of upholded	stdv
ICA all	1991-1997	19	22%	42%	79%	42%
	1998-2007	53	31%	40%	64%	48%
	1991-2007	72	28%	40%	68%	47%
EC all	1979-1997	39	36%	42%	59%	50%
	1998-2004	22	28%	37%	73%	46%
	1979-2004	61	33%	40%	64%	48%

Note 1: ICA all, Italian Competition Authority Cartel and Abuse of dominance cases
Note 2: EC all decisions by European Commission Cartel and Abuse of dominance cases

Table 1.4 presents the descriptive statistics for two different samples. The sample *ICA all* refers to cartel and abuse of dominance investigations carried out by the Italian Competition Authority, whereas the sample *EC all* refers to the investigations, for the same sort of violations, carried out by the European Commission. The sample covers slightly different

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periods of time respectively 1991-2007 for the Italian sample and 1979-2004 for the European sample. For both samples we present two statistics, the average percentage reduction in fine[21], and the percentage of cases that are upheld[22] by the last Court of appeal. The statistics are presented for the whole period and for two sub-periods with the time break fixed at the year 1998 for both samples.

Looking at the whole period for both samples we see that the average fine reduction and the percentage of cases that are upheld is fairly similar for the two samples with the average fine reduction being around 30% and the average share of upheld cases being around 66%. However, if we break the time period at the year 1998[23] we can isolate two opposite trends. For the Italian sample we find that the average fine reduction increased over time while the share of upheld cases decreased. The opposite is true for the European sample where average fine reduction decreased and the share of upheld cases increased. It seems that for the Italian case the higher use of fines after 1998 was accompanied by both a higher level of reduction in fines and higher annulment of the decisions taken by the antitrust Authority.

These findings can help us shade some light on the results of the econometric exercise both when we look only at the Italian data and when we compare the Italian results to the findings at the European level[24].

So far we have only showed some descriptive statistics about the investigations and incidental decisions of the Italian Competition Authority. To study the impact of these events on the investigated firms' market value we have to resort to econometric techniques. Also, to undertake this study we need to restrict our sample to only those firms that have some traded security, which means, that we will be focusing on those cases in which at least a firm is listed on a stock market.

[21] We only consider cases that are appealed and that come to judgement. The percentage fine reduction is computed as follows: $\%\, reduction = \frac{(final\ fine - initial\ fine)}{initial\ fine}$.The data are first averaged at the single antitrust case level from which we then compute the sample average, This is done because large cartel cases, in which it is likely that all firms experience the same level of reduction whould otherwise bias our estimate.

[22] We consider a case upheldеd if the final fine is weakly higher than the two third of the initial fine.

[23] For both the Italian and the European antitrust enforcement we can find some elements that justify the presence of a structural break around 1998. For the Italian case see Ghezzi and Polo (2001) and for the European case see Langus et al. (2010).

[24] The findings at the European level are taken from Langus et al. (2010).

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1.3 Using Event Study to detect the impact of antitrust investigations

Event study methodology uses financial market data[25] to measure the impact of economics events on firm's market value. This methodology has been widely used in financial and economics literature and typical events examined are corporate and financial decisions, regulatory changes and legal actions. Event studies have been extensively applied to antitrust in particular to the study of mergers and to a lesser extent to the study of abuse of dominance or price fixing practices (for a survey see Cichello and Lamdin (2006))

Event studies estimate the abnormal return of a firms' security at the realization of the event of interest and, assuming market rationality, provide the measure of the (unanticipated) change in firms' market value that is due to the event realization. In this paper we perform what is known as a short term event study that looks at the market reaction immediately after the event realization and capture the (unanticipated) expected impact of the news. The impact captured using short term event study might differ from the actual long term effect of the event realization. Indeed, only long term event studies or other measures that look at firms' realized profits over time might be employed to estimate the long term actual impact of the news. However, these two latter methods would require a longer time horizon and would introduce other biases. Indeed, as the time window considered gets larger it will be even more difficult to isolate the effect of the event of interest from other confounding events that may occur over time. For these reasons, assuming rational expectations, short term event studies provide a powerful tool to identify the unanticipated expected effect of the event.

Our empirical analysis relies on the theoretical foundation of event studies set forward by Campbell et al. (1997). Following their approach the typical roadmap of a standard event study evolves according to the following step procedure:

- Define event of interest
- Define period in which security price is analyzed (i.e. Event window)
- Selection criteria for the inclusion of firms
- Compute normal return and derive abnormal return
- (Statistically) Test results

[25] Event studies are usually performed using data about common stock prices. However other types of traded securities might be used.

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In the following sections we present how we adapt the above event study methodology to our case study.

1.3.1 Event definition

This paper studies the impact of antitrust proceedings on firms' market value. The investigation and decision process cannot be summarized by a single event as they consist of several subsequent steps. Hence, we consider all the three main events (described above in section 1.2.1) that characterize a typical Italian antitrust inquiry.

1. Start of an antitrust investigation
2. Decision of the Italian Competition Authority:
    (a) infringement (with or without a fine), or
    (b) acquittal
3. Decisions of the last Court of appeal (only in case of appeal)[26].

Along the investigation process the above events mark clearly the time at which novel information reach the investors. Under the efficient market assumption, with rational investors, the price of a security should reflect the discounted sum of future dividends and at any given point in time the security prices fully reflect all available information. Hence, we expect the price to be highly correlated with current and expected future profitability of the firm. We claim that the outcome of each of the above steps adds new information about the expected future profitability of the firms. Therefore, if the event realization is unanticipated (i.e. it has not yet been discounted by investors), it should be reflected in an immediate change in the price of the security.

As stated, we then treat the realizations of the three events as unanticipated. However, we could also think that the realizations of the above steps are not totally unexpected. For instance, the market might form an opinion or speculate about the likelihood of the outcome of each single event. Still we can think that at each stage the realization of the event adds some new information to the market. Hence, our estimates will identify that part that was not (correctly) anticipated by the market.

[26] In the Italian system before the last court of appeal, the Consiglio di Stato, there is a first court of appeal called Tribunale Amministrativo Regionale (TAR). However we do not study this event as it is only an intermediate step between the ICA decision and the final decision of the last court of appeal.

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Taking, as an example, the first event (i.e. the start of investigation) the stock market might have already formed the expectation that some firms will be investigated by the ICA. However, the exact timing and extent of investigation is unknown until it realizes.

Similarly, if we think about the second event (i.e. the decision of the competition Authority) the stock market might well expect a negative decision implying the ceasing of the anticompetitive practice and a possible fine (both reducing the profitability of the firm), however only when the Authority releases its decision the market will know the exact details and implication of the decision. The same can be said for the Court of Appeal where some degree of unexpectedness remains as the Court of appeal can also overturn the decision of the competition Authority.

In conclusion as we cannot control for the part that the market has already discounted what we aim to estimate is the stock market reaction to the unanticipated information that each event carries.

1.3.2 Event window

After assuming that markets are efficient and investors rational we have to make conjectures about the timing of market reaction. If the reaction is quick we could expect that the price of the security is discounted the same day an unexpected news hits the market. However, there might be reasons why we should extend this event window. In similar studies (for instance, Langus et al. (2010); Bittlingmayer and Hazlett (2000) and Bosch and Eckard (1991)) it is common to find event window of three or more days. Usually, having a time horizon that starts some days before the event allows to include in the analysis potential lead effects (e.g. leakages of information acquired by the market). Similarly, having a time horizon that includes some days after the event accommodates for late reactions to the announcement.

Indeed, there is no specific rule on how to choose the event window. However, there is a clear trade-off, indeed as we include more days we allow for the possibility that other events confound our results, thus losing precision in our estimates. In this paper to define the size of the event window of reference, for each event, we study the process of information disclosure. Hence, for each event we define an event window that depends on the observed information disclosure pattern.

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1.3.3 Selection criteria

To perform the event study we need the stock price of the investigated firms' traded securities. Therefore we have to restrict our starting sample to only those firms that are listed on a stock market[27]. This first selection reduces our starting sample from 552 observations to 155. Table 1.5 shows the breakdown of this selection by the type of violation and type of firm. The columns from 2 to 5 ("All listed") refer to all the observations for which either the single investigated firm or its mother company are listed. In the columns from 6 to 9 ("Italian sub-sample") we further reduce the sample of listed firms to only those firms that are listed in the Italian stock market (Milan Stock Exchange also known as Borsa Italiana) and for which the investigated firms are *directly* listed (i.e. the financial entity listed coincide with the firm under investigation[28]).

We perform this latter selection in order to gain in the significance and magnitude of our estimates. Indeed, the closer is the match between the business unit under investigation and the financial entity for which we have the stock price the lower is the bias in our estimates. This is an issue that naturally arises in this type of study. Indeed, the announced event (in this case an antitrust event) has to significantly affect the expected profitability of the financial entity under study if we are to expect a measurable effect[29].

From table 1.5 we see that the distribution of the observations across the types of decision is similar to what we saw in table 1.2 for all firms (not only the listed ones). As expected the observations relating to cartels in which there is an infringement decision accompanied by a fine are in great number compared to the other types of violation and decisions. Also, the distribution of the observations in the two selected sub-samples seems to follow the proportions found in the initial larger sample.

For instance, in the sample *All Listed* we have 99 observations in which the ICA finds a violations of competition law and also inflicts a fine. Then we have 41 cases in which the infringement decision is not followed by a fine and 15 observations in which the ICA finds

[27] In case of multiple listing we give preference to stock listed on the Italian stock market. In case firms only had foreign listing we choose the most important in terms of capitalization.

[28] For instance if a firm under investigation is owned by a listed mother company we do not include it in our sample. This selection method does not rule out that the listed company might be a multi-product firm and the antitrust investigation might involve only a single product.

[29] For instance, in our study there are many cases in which the firm under investigation is the Italian subsidiary of a foreign based multinational. If the Italian firm does not account for a high share of the total aggregate revenue and it is not independetly listed than we would expect that an Italian antitrust investigation would have only a marginal impact on the stock prices of the multinational firm. Other studies have used different approaches, for instance Mariniello (2006) includes in his sample only the firms for which the activity under investigation represents at least 10% of total aggregate turnover.

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no violations of competition law.

Table 1.5: Sample Selection

	a) All listed				b) Only Italian			
Decision	Cartel	Abuse	Cartel & Abuse	Total	Cartel	Abuse	Cartel & Abuse	Total
Infringement and fine	78	14	7	99	32	11	0	43
	79%	12%	7%	64%	74%	20%	0%	58%
Infringement no fine	20	17	4	41	12	12	3	27
	49%	27%	9%	26%	44%	29%	10%	36%
Acquitt	7	2	6	15	0	1	3	4
	47%	9%	29%	10%	0%	13%	43%	5%
Total	105	33	17	155	44	24	6	74
	68%	16%	10%	100%	59%	23%	8%	100%

Note 1: This sample includes all the observations for which the firm is listed in a stock market
Note 2: This sub-sample includes only the firms listed in the Italian stock market and for which the financial entity correspond to the investigated firm
Note 3: Percentages in columns 5 and 9 are column percentages; while in columns 2, 3, 4, 6, 7 and 8 are row percentages

When we look at the sub-sample *Only Italian* we have a total of 74 observations, roughly half of the *All listed* sample. Among these observations there are 43 observations related to infringement decisions in which the Authority inflicts a fine. Then there are 27 observations of infringement decisions (without fine) and only 4 cases in which there is an acquittal decision.

Table 1.6 presents some descriptive statistics about average and total fines for the selected observations. For the whole sample we can see that in aggregate terms the observations selected account for a total of 1.63 billion euro of fines given by the Italian Competitions Authority, with an average fine of 16 million euro (however as suggested by the high standard deviation the fine amount varies considerably across observations). If we compare these data with the data presented in section 1.2.3 we can see that the observations, in the selected sample represent only the 25% (99 out of 388) of the total number of observations for which there is an infringement decision accompanied by a fine. However, the fines inflicted in these 99 observations represent the 80% of the total fines given by the Authority for cartel or abuse of dominance cases.

Also the average fine for the selected sample amounts to 16 million euro whereas in the starting sample the average fine is only 5 million euro. This follows from the fact that fines

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Table 1.6: Sample Selection Fines

		ICA fine		Final fine	Avg. Ratios	
		All (ml. euro)	Appeal (ml. euro)	Appeal (ml.euro)	final/initial	fine/cap.
All listed	Avg.	16.47	15.87	9.70	57%	0.39%
		(38.01)	(28.75)	(22.41)	(45%)	(0.87%)
	total	1630.98	1222.22	746.76	61%	
Only Italian	Avg.	23.98	21.24	15.10	48%	0.72%
		(51.72)	(35.19)	(29.96)	(46%)	(1.15%)
	total	1031.00	658.59	468.19	71%	

Note 1: Standard deviations in parenthesis
Note 2: The total for the final initial fine ratio represent the ratio between total initial fine and total final fine
Note 3: The capitalization refers to the single stock, i.e. it is not consolidated
Note 4: The columns Appeal only selects those observations for which there was an appeal against AGCM decision

are proportional to firms turnover and as listed firms are usually larger, than non listed ones, we expect to find higher absolute level of fines among listed firms.

In Table 1.6 we also compare the fine inflicted by the ICA to the fine that firms actually pay.after the appeal procedure (we compare only the fines for the observations that appeal against the Authority's decision and for which the appeal procedure has ended). In aggregate terms we see that during the appeal procedure a total of 475 million euro of fines is annulled (from 1222 million to 746 million) and the total final fines represent only the 61% of the total initial fine. Among the observations in which we have an appeal the average fine declines from 15.9 million euro to 9.7 million euro and on average the final fine that each firm has to pay is only the 57% of the initial fine. In order to compare the size of the fine to the capitalization of the convicted firms we also compute the average fine capitalization ratio for the selected samples. From the table we see that on average the fine inflicted by the ICA represents the 0.4% of the investigated firms' capitalization.

Table 1.6 also presents the same statistics for the sub-sample of Italian firm. This smaller sample accounts for roughly 1 billion euro in terms of total fine and the average fine is around 24 million. Hence it seems that the Italian firms fined by the Authority receive, on average, higher fines than foreign based ones. In addition, when we select only the observations for which there is an appeal, we find that during the appeal procedure there is a reduction of 200 million euro in fines with the total sum collected by the Authority being the 71% of the

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initial level. When we look at the impact on the individual firms we find that on average the final fine only represented the 48% of the initial fine.

Table 1.7: Antitrust fines reduction: Italian vs European cases with listed firms

			Fine reduction		Share upheld cases	
	period	# cases	avg. reduction	stdv	% of upthelded	stdv
ICA all	1991-1997	8	25%	46%	75%	46%
	1998-2007	28	37%	42%	63%	49%
	1991-2007	36	34%	43%	66%	48%
EC all	1979-1997	16	39%	45%	56%	51%
	1998-2005	21	33%	39%	71%	46%
	1979-2005	37	35%	41%	65%	48%

Note 1: ICA all, Italian Competition Authority Cartel and Abuse of dominance cases
Note 2: EC all decisions by European Commission Cartel and Abuse of dominance cases

Finally, Table 1.7 compares the selected sample of listed firm to a sample of listed firms fined by the European Commission[30]. The comparison is similar to the one carried out in section 1.2.3.1 (see Table 1.4) the only difference is that both samples (the Italian and the European) are now restricted only to listed firms, the ones used in the econometric exercise. Again we look at the average reduction in fines[31] after the appeal procedure and at the share of cases that are upheld[32] by the last Court. This analysis confirms the results found for the entire sample (table 1.4) where we find that over the all period the Italian and European sample exhibit similar percentage fine reductions and share of upheld cases. Also the data confirms the above findings that for the Italian sample the average reduction in fine increased over time while the share of upheld cases decreased and the opposite is true for the European sample. Hence it seems that the samples of listed firms are subjects to the same patterns of the non-listed firms.

Again, these findings can help us shade some light on the results of the econometric exercise both when we look at the Italian data in isolation and when we compare the Italian

[30] This sample of European Commission cases is the one used in Langus et al. (2010).

[31] We only consider cases that are appealed and that come to judgement. The percentage fine reduction is computed as follows: $\%\,reduction = \frac{(finalfine - initial\ fine)}{initial\ fine}$.The data are first averaged at the single antitrust case level from which we then compute the sample average, This is done because large cartel cases, in which it is likely that all firms experience the same level of reduction whould otherwise bias our estimate.

[32] We consider a case upheld if the final fine is weakly higher than the two third of the initial fine.

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results to the findings at the European level[33].

1.3.4 Normal and abnormal returns

The aim of event studies is to examine the performance of firms' securities around the date of the event. The challenge is then to disentangle the effect of the event from other firm level or market level effects. This is usually done by looking at the abnormal return, as defined by equation 1.1. Such an equation says that the abnormal return, AR_{it}, of security i at time t is given by the actual return, R^*_{it} [34], minus the expected normal return R^n_{it} (X_t is the conditioning information). Where the normal return represents the return that would be expected had the event not taken place.

$$AR_{it} = R^*_{it} - E\left[R^n_{it}|X_t\right] \tag{1.1}$$

Hence, before being able to say something about the abnormal return we first have to make some assumptions about how we construct the normal return. In this paper we estimate normal returns assuming that individual firms' returns are related to the market return as in the *market model* (equation 1.2). The market model relates individual firms' return to a constant α and the return on a market index, R^*_{mt} (i.e. $X_t = \alpha + \beta R^*_{mt}$), and a firm specific return, ϵ_{it}, unrelated to the overall market and with an expected value of zero. This model is widely used in financial and event study literature and although other statistical and economic models have also received some interest[35] it has been shown that they do not offer particular gains over the market model (Campbell et al. (1997)).

$$R^n_{it} = \alpha + \beta R^*_{mt} + \epsilon_{it} \tag{1.2}$$

We then proceed by estimating, in the estimation window, the unknown parameters α and β that are then used, in the event window, to estimate the normal and abnormal return. We estimate the parameters of the market model with two different methods(see section

[33] The findings at the European level are taken from Langus et al. (2010).

[34] Here return R_{it} is defined as the percentage change in the price of a securities i between two consecutive trading days (i.e.t and $t-1$). R_{it} is defined as: $R_{i,t} = \ln(P_{i,t}) - \ln(P_{i,t-1})$;where P_{it} is the price of security i at time t.

[35] For instance the simplest choice could be the *constant mean market* model in which X_t is only a constant. Other statistical models have been proposed like the *factor model* and the *market adjusted return model.* In addition also some economic models have been put forward like the *capital asset pricing model* and the *arbitrage pricing theory model* (for a description of these model see Campbell et al. (1997)).

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1.7 in Appendix B for details): firstly using a standard OLS estimation (section 1.7.0.3); and secondly using a non-parametric approach (section 1.7.0.4) known as Theil's estimation (firstly suggested by Theil (1950) and reported in Dombrow et al. (2000)).

1.3.4.1 Abnormal returns and aggregation

Once established the method to compute the, daily and firm's specific, normal return we can estimate the abnormal return for all the days in the event window (values between $T_2$and T_3 in Figure 1.2). In our analysis the interest is not exclusively on the abnormal performance observed on the day of the event but it extends to the abnormal performance observed over an interval of days around the date of the event. Indeed depending on the specific event (start of investigation, Authority's decision, last Court decision) and on the observed pattern of information disclosure (i.e. release of unexpected news), we restrict our attention to event windows of varying length, that at least contain more than one day. Also the interest will not be on the single stock price reaction but on the average reaction across the sample. Hence we have to aggregate the daily and individual abnormal return both over the length of the event window and over the observations in the sample.

We define the daily average abnormal return (DAAR) as the aggregation across firms of the daily abnormal returns as in (1.3). Then by aggregating the DAAR over the event window we define the cumulative average abnormal return (CAAR) as in (1.4). When we do not aggregate across firms but only over time at the single firm level we define the cumulative abnormal return (CAR) as in (1.5).

$$DAAR_t = \frac{1}{N}\sum_{i=1}^{N} AR_{it} \tag{1.3}$$

$$CAAR = \sum_{t=T_2}^{T_3} DAAR_t \tag{1.4}$$

$$CAR_i = \sum_{t=T_2}^{T_3} AR_{it} \tag{1.5}$$

The above three measures are used to investigate the impact of the event under different perspectives. The CAAR provides evidence on the average (unexpected) effect of the event on an average firm during the selected event window. The DAAR shows the average impact of the event at each different day of the event window. Finally, by looking at the CAR we can study the heterogeneous impact of the event on the cross section of observations.

1.3.5 Testing for statistical significance

The above statistics measure the magnitude of the impact of the event but are not informative about the statistical significance of the impact. In the empirical analysis we then employ standard test statistics to say something about the statistical significance of the effect. In section 1.7.1 (Appendix B) we outline the testing framework and provide a test statistics for each of the aggregate abnormal return measures. As we adopt two different estimation strategies for the market model, we also adopt two different testing framework. For the OLS estimates we use a standard parametric test (section 1.7.1.1) while for the Theil's method estimates we adopt a nonparametric test (section 1.7.1.1). Both tests are derived under the null hypothesis that the event under study has no impact on the mean or variance of returns (i.e. the expected abnormal return equals zero).

1.4 Results

In the following sections we present the results of the empirical analysis. The events that we consider for the empirical analysis are:1) Start of the investigation; 2) Decision of the Italian Competition Authority; 3) Decision of the last Court of appeal (see 1.3.1).

For each event we first define an hypothesis that we then test in the empirical analysis. To formulate our hypotheses we follow the results of the modelling framework outlined in Langus et al. (2010). In their paper, the authors model the procedure of a typical European antitrust investigation to predict the sign of the impact of three different events: the dawn raid; the Commission's decision; and the decision of the last Court of appeal. The same modelling framework seems to well represent also a typical Italian antitrust investigation. Therefore, we use their results to inform our hypotheses about the sign of the impacts that are of interest for our case. Box 1 summarizes these predictions[36]. For the event *Start of the investigation* we predict to observe a negative effect. For the event *Decision of the ICA* and *Decision of the last Court of appeal* we differentiate the predictions based on the outcome of the event. In the former case we predict a negative effect after a decision of infringement (with or without fine, although they will differ in magnitude with the latter been higher). Differently we predict a positive effect after a decision of acquittal. For the last event (Court of appeal's decision) we predict a negative impact when the Court uphold the Authority's decision and we predict a positive effect when the Authority's decision is annulled.

[36] In Langus et al. (2010) the authors have no information about the acquittals, neither at the dawn raid stage nor at the Commission's decision stage. Differently in this paper we also observe these type of occurrences, however the sign of the impacts are not affected by this.

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1. Start of an antitrust investigation (- negative sign)
2. Decision of the Antitrust Authority, divided in:
   (a) infringement decision
      i. with sanction (- negative sign)
      ii. without sanction (- negative sign)
   (b) acquittal (+ positive sign)
3. Decision of the last Court of Appeal (Consiglio di Stato), divided in:
   (a) upheld Authority decision (- negative)
   (b) annull Authority decision (+ positive)

Box 1: Prediction of the sign of the effects of the event

Given the above predictions for each specific event we formulate the following one sided hypotheses:

Hypotheses	+ positive sign	- negative sign
null	$H_0: AR < 0$	$H_0: AR > 0$
alternative	$H_A: AR > 0$	$H_A: AR < 0$

Finally we also make use of another set of results developed in the same paper. Langus et al. (2010) find that, both with a higher fine or a higher probability that the last Court of appeal upholds the fine, we should expect a stronger impact of the events on the stock market valuation of firms. We investigate this latter set of hypotheses looking at two different samples: the first for the years from 1991 to 1997 and the second from 1998 onwards. This two periods appear to be different under both dimensions, the level of fines and the likelihood to have an upheld decision.

The following sections present and discuss the results for each event.

1.4.1 Start of an antitrust investigation

Following the predictions outlined in the above section, for this event we test the hypothesis that the start of the investigation has a negative impact on the market value of the investigated firms. The event study aims to measure the market reaction to the unexpected portion of the event realization. For what concerns this event we do not find evidence of the market shortly anticipating the event. Indeed when we look at the press coverage we find evidence of event coverage only after the event realization, usually one or two days after the ICA issues a press release. At the start of the investigation investors should not anticipate

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Table 1.8: Start of Investigation Results

	All firms (N=154)		Italian sel. (N=73)	
Event Window	OLS	Theil	OLS	Theil
31 days (-20;+10) AR	0.34	1.52	1.92	3.26
test	0.34	−1.06	1.44	0.19
11 days (-5;+5) AR	−0.14	0.20	1.28	1.76
test	−0.28	−0.56	1.68	1.46
6 days (-1;+5) AR	−0.22	−0.01	0.98	1.24
test	−0.56	−0.34	1.63	1.51
3 days (0;+2) AR	−0.66***	−0.57**	−0.59*	−0.46
test	−2.62	−1.95	−1.53	−1.09

Estimation Window (-230;-30)
Abnormal Returns as percentage
One-sided test, significance levels *** 1% ** 5% * 10%

which investigation will lead to an infringement decision and which not, therefore we can look at our aggregate sample of 154 observations altogether. We also estimate the impact only for the selected subsample of Italian observations.

Table 1.8 presents the results. Given the type of event and the evidence gathered about the process of information disclosure, for this event we are mostly interested on the market reactions taking place immediately after the event realization, that is in the window (0;+2). When we look at the full sample we find that after the start of the investigation, on average, the market value of the firms under investigation decreases by around 0.66% (significant at 5%). Also, as robustness check, we find that this estimate has the same magnitude and statistical significance for the two estimation methods employed (OLS and Theil). When we look only at the Italian selection we find an impact of similar magnitude and sign however only the OLS estimate is statistically significant (at 10%). Surprisingly, it seems that the market reaction on the foreign firms is higher than the reaction for Italian firms. When we look at the cross section of observations we find that, in the event window (0;+2) 87 observations have a negative sign while 67 have a positive sign.

In table 1.8 we also report the estimates for other selected event windows. However, we do not find any evidence of others statistically significant effects in longer event windows. Given the nature of the event and the clear event date realization we are indeed more confident in restricting our analysis only to the shorter event window.

We also check if the market reaction to the event has changed over time. As we argued

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Table 1.9: Start of Investigation Results: Pre vs Post 1998

Pre-1998	All firms (N=61)		Italian sel. (N=41)	
Event Window	OLS	Theil	OLS	Theil
3 days (0;+2) AR	-1.06^{***}	-0.98^{**}	-0.44	-0.35
Test	-2.56	-2.01	-0.81	-0.72

Post-1998	All firms (N=93)		Italian sel. (N=32)	
Event Window	OLS	Theil	OLS	Theil
3 days (0;+2) AR	-0.40	-0.30	-0.79^{*}	-0.59
Test	-1.26	-0.77	-1.44	-0.77

Estimation Window (-230;-30)
Abnormal Returns as percentage; One-sided test, significance levels *** 1% ** 5% * 10%

above we separate our sample in two periods Pre-1998 and Post-1998.

Table 1.9 presents the event study for the two sub-periods. From the results we can see that the impact of the event is higher during the Pre-1998 period (for the full sample we find a statistically significant effect of -1%). Surprisingly it seems that in the period Post-1998, despite the higher resort to fines from the ICA, the event start of the investigation has no statistically significant impact on the market value of the firms under investigation.

We investigate further this issue and we regress the 154 individual CARs[37] (3-day event window, 0;+2) on a constant, on the firm capitalization, on a time trend and on a dummy for the observation in the Italian subsample[38], on a dummy for the type of infringement (abuse or cartel) and on a dummy that captures the outcome of the Authority's decision (infringement or acquittal). Table 1.15 (Appendix C), in the first column, presents the results of this regression. About the presence of a time effect we find that the coefficient of the time trend (datestart) is positive and statistically significant (at 1%)[39]. We can interpret the coefficient as that, ceteris paribus, an extra year is associated with a higher 3-day CAAR by 0.15 percentage points. This is consistent with the findings that over time we observe a decrease

[37] The CAR is the individual firm's Cumulative Abnormal Return (cfr. 1.3.4.1)

[38] As said above the observations in the Italian subsample are those observations for which the firm under investigation is Italian and it is not owned by a mother company, hence it is directly listed on a stock market (usually the Milan stock market). For these observations we expect to find a higher effect than in the case the listed financial entity is the mother company or the firm is a foreign firm.

[39] We also check for the presence of a quadratic time trend but we reject this specification in favor of the linear trend.

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in the (negative) effect of the event *start of investigation*. From the regression we obtain some other interesting findings. The coefficient of the dummy that controls for the outcome of the decision (infringed) is not significant. It seems that at this first stage the market reaction is independent of the future decision of the Authority (infringement or acquittal decision). Also we find that the coefficient of the Italian dummy is not statistically significant. Hence the impact for Italian firms, directly listed in the stock market, is not different than the impact of any other firm in the sample. The coefficient on market capitalization is positive and significant meaning that firms with higher capitalization are less affected by antitrust investigations[40] and that the magnitude of the impact is not correlated with the firms' market capitalization. Finally the coefficient on the type of infringement is not significant.

Overall these results suggest that the event start of investigation has a limited impact on the market valuation of investigated firms and the effect is limited to the days immediately after the event realization. Also, differently from what expected, the market reaction for Italian firms is not greater than that for the foreign based (or owned) firms. Moreover there is evidence that while before 1998 the impact was sizeable, over time this effect vanished. The regression analysis also suggests that the sample does not suffer from correlation issues as regression estimates confirms the results of the aggregated event study.

1.4.2 Decision of the Antitrust Authority

In this section we study the central event of the investigation, the decision of the antitrust Authority. In our modelling, at the decision stage, the Authority can take two decisions: infringement, in case a firm is found guilty; and acquittal in case no evidence has been found[41]. In turn, in case of an infringement decision, the Authority can sanction or not the convicted firms. Finally, in case of a fine, the Authority sets the amount of the fine.

Following the predictions developed in Box 1 we proceed to test them on our sample. We start by looking at the infringement decisions and then we discuss the acquittal decisions.

1.4.2.1 Infringement decisions

Table 1.10 reports the estimates of the event study conducted for the infringement decisions. For this analysis we further divide the sample in *Fine* and *No fine* cases in the attempt

[40] Another explanation might be that firms with higher capitalizations are firms that are foreing and multiproduct hence we expect a lower impact on their market valuation.

[41] In our sample we do not have any case in which there is an application of leniency or in which the case is closed with committments taken by the parts.

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to isolate the effect of the fine. As for the event start of investigation, also the Authority's decision takes place during a specific and clearly identified time period. We can be quite confident of correctly identifying the date of this event. Indeed by inspectioning the press coverage around the date of the event we do find evidence that the information disclosure takes place the same day or immediately the day after the Authority's press release. For this reason also in this case we look mainly at the CAAR observed during a short event window. For this case we select as our best estimate the event window, of length 3 days, that span from one day before the event to one day after the event. The event date is chosen as the day in which we find press coverage about the decision, and this is usually the day after the Authority issues an official press release. We then include the day before and the day after the event to capture for the (possible) early and late responses to the news. Nonetheless, table 1.10 also reports the estimates for larger event windows to check for other movements around the date of the event.

Starting from the fined cases and the All firms sample, we find that the impact, on the 3-day window, has a negative sign but it is not statistically significant for both the parametric and nonparametric estimates. We find some negative (between 0.5% and 0.9% respectively for Theil's and OLS estimates) and statistically significant (at 10%) estimates when we allow the event window to include up to 5 days after the event date. This might be justified on the ground that when foreign traded firms are included it might take more time before we observe a reaction to events taking place in Italy. Differently if we restrict the analysis only to the Italian subsample of fined cases we do find a sizeable impact of around 1% and 1.3%, respectively for the OLS and the Theil's estimates, and both estimates are statistically significant (respectively at 10% and 5%). For this event in presence of a fine we find that Italian firms directly listed experience a higher loss in market value than foreign ones.

When we look at the subsample of cases in which there is an infringement decision not followed by a fine we still find an effect of the expected (negative) sign, however at the 3-day event window only the nonparametric estimate for the All firms subsample is statistically significant (at 10%).

Also for this event we check if the stock market reaction has changed over time. In table 1.11 we present two event studies, conducted on the whole sample of infringed firms (we do not differentiate between fined and not fined). The first event study refers to the decisions taken before 1998 and the second to the decisions taken after. In line with what we find for the event *start of the investigation* for the decision we find an even more marked difference

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Table 1.10: Authority Decision Results:Infringed

	Fine				No fine			
	All firms (N=98)		Italian Sel. (N=42)		All firms (N=41)		Italian Sel. (N=27)	
Event Window	OLS	Theil	OLS	Theil	OLS	Theil	OLS	Theil
31 days (-20;+10)	0.13	1.75	0.33	2.71	1.88	3.11	1.77	2.93
test	0.10	0.39	0.13	0.82	1.09	0.35	0.79	−0.03
11 days (-5:+5)	−0.87	−0.34	0.00	0.71	0.44	0.74	0.38	0.63
test	−1.14	−0.84	0.00	0.74	0.45	−0.06	0.30	0.06
6 days (-1...+5)	−0.91*	−0.54*	−1.29	−0.74	−0.26	−0.08	−0.08	−0.01
test	−1.53	−1.37	−1.17	−0.67	−0.33	−0.05	−0.08	0.08
3 days (-1...+1)	−0.43	−0.28	−1.06*	−1.32**	−0.60	−0.54*	−0.60	−0.55
test	−1.10	−1.27	−1.62	−1.85	−1.19	−1.48	−0.92	−1.26

Estimation Window (-230;-30)
Abnormal Returns as percentage; One-sided test, significance levels *** 1% ** 5% * 10%

between the two time periods. Before 1998 the estimates have all a negative sign and are between -1.1% and -1.55% with a higher impact for Italian firms. The estimates are strongly statistically significant (at 1%) and the two estimation methods offer very similar results.

As in the previous section we find evidence that in the second period (Post-1998) the market does not react to antitrust events and indeed we do not find any statistically significant estimate. Again despite the Authority making a more systematic use of its sanctionatory power, after 1998, we find that antitrust infringement decisions and fines have, on average, no effect on the market value of the convicted firms.

We also conduct a regression analysis to examine further the presence of a time trend and to inspect the determinants of the individual CARs. We regress the 3-day CARs (event window -1;+1), for the 139 observations for which there is an infringement decision, on a constant, on a time trend, on the size of the fine, on a dummy for the type of infringement, on a dummy for the Italian subsample and on the market capitalization of the financial entity. Table 1.15 (Appendix C), column 2, presents the results for this regression. The coefficient on the time trend (t_datedec) is positive and statistically significant (at 10%) suggesting that an extra year is associated with a higher CAR by a 0.14 percentage points The magnitude of the trend is similar to the one found for the start of the investigation. Also we find that the coefficient on the Italian dummy is negative but not statistically different from zero. Most likely once we control for capitalization (significant at 10%) we do not find differences

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Table 1.11: Authority Decision Results:Infringed, Pre vs Post 1998

Pre-1998	All firms (N=54)		Italian sel. (N=38)	
Event Window	OLS	Theil	OLS	Theil
3 days (-1...+1)	-1.17^{***}	-1.09^{***}	-1.55^{***}	-1.45^{***}
test	-2.62	-2.81	-2.71	-2.44

Post-1998	All firms (N=85)		Italian sel. (N=31)	
Event Window	OLS	Theil	OLS	Theil
3 days (-1...+1)	-0.04	0.11	-0.41	-0.14
test	-0.10	-0.01	-0.47	-0.09

Estimation Window (-230;-30)
Abnormal Returns as percentage; One-sided test, significance levels *** 1% ** 5% * 10%

across firms. About the other regressors we find that neither the amount of the fine nor the dummy for the type of infringement have coefficients that are statistically significant. To analyze these two regressors further we run two other regressions in which we regress the CARs only on the fines and on the fines as a share of the firms' market capitalizations. The results for these two regressions are presented in table 1.15 (Appendix C), columns 3 and 4. None of these coefficients is statistically different from zero. Finally we also check whether the magnitude of the CARs depends upon the type of infringement. To do so we regress the 139 3-day CARs on a dummy that takes the value of 1 when the type of infringement is an abuse of dominance and 0 in case of a cartel case. The results from this regression (table 1.15 Appendix C, columns 5) finds that the type of violation has no statistically significant effect on the estimated CARs. Hence we conclude that the main determinant for the size of the individual CARs are the time period, and the capitalization of the convicted firms.

1.4.2.2 Acquittal decisions

At the decision stage the other type of action the Authority can take is to acquit the firm(s) under investigation. In our sample we do not have many of these cases as the Authority conducts a preliminary investigation before opening a new case and many decisions to not proceed further are taken at that stage. Nonetheless given the sample we have we can test the prediction developed in box 1 also for this type of event. In particular we test the hypothesis that after an investigation was started the event *Authority acquit the firm* should have a positive impact on the market value of the firm object of the acquittal decision.

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Table 1.12: Authority Decision Results:Acquitted

	All firms (N=15)		Italian sel. (N=9)	
Event Window	OLS	Theil	OLS	Theil
31 days (-20;+10)	2.97	2.41***	3.57	2.55
	0.98	1.36	0.85	1.17
11 days (-5;+5)	1.38	1.15	1.76	1.32
	0.80	0.92	0.74	0.85
6 days (-1;+5)	0.35	0.29	0.29	0.14
	0.26	0.53	0.15	0.36
3 days (-1;+1)	0.51	0.51	0.69	0.68
	0.58	0.74	0.56	0.76

Estimation Window (-230;-30)
Abnormal Returns as percentage; One-sided test, significance levels *** 1% ** 5% * 10%

In the regression analysis we find that for the event *start of the investigation* the impact is similar across the firms and it is not related to the final outcome of the investigation. This might well suggests that after a "negative" event, a positive event (acquittal decision) should have a positive impact on firms' market valuation, at least to recoup the small loss in value we find at the start of investigation stage.

We perform another even study and again we select as our event window of reference the 3-day event window that contains the day of the event plus the day before and the one after. Table 1.12 presents the results for this window and other different event windows of interest.

When we look at the full sample or only at the Italian subsample we find that the estimates have all the expected (positive) sign but almost none is statistically significant. Across all the event window there is only one statistically significant estimate (the Theil estimates for the 31 days window). However, the respective OLS estimates is not significant and moreover there is no reason to expect a reaction to take place during that event window. Hence we are confident in saying that the we do not find any impact following an acquittal decision once an investigation had started. In part this finding is consistent with the findings of the first event in which we saw that the market barely reacts after the start of an investigation.

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1.4.3 Last Court of Appeal's decision

The last stage of the antitrust trial that we consider is the decision of the last Court of appeal. As we said above there is another intermediary step between the decision of the Authority and the last Court of appeal, however since it is only an intermediate step before the final judgement we restrict our attention to the final decision.

However, we do observe some cases in which, after the first appeal, the case is not brought to a third Court and we consider this decision as being the last and final decision.

The last Court can take two different decisions either to upheld or annul the decision of the Authority. In section 1.2.2 we document that for this last stage the information disclosure process is less clear cut with respect to the previous stages. Indeed, we find that: firstly, the decisions of the last Court of appeal do not receive the same press coverage observed for the first two events; secondly, although we know the dates in which the last Court takes its decisions we do not know when part or the all content of these decisions is made public.

We find press coverage about the decisions of the last Court either around the date of the decision, taken by the jury room (*camera di consiglio*), or around the date in which there is the publication of the judgement (*pubblicazione dispositivo sentenza*). For this type of event we take as our event window of reference a relatively larger window of a total of 31 days going from 20 days before the event date until 10 days after the event. The event date is instead considered the date in which we find some press coverage about the decision (we use the first day in which we find the news). When we do not find any press coverage of the decision, we use the date in which the jury room takes the decision.

1.4.3.1 Last Court upheld the Authority's decision

In this part we present the results for the event in which the last Court upheld the decision of the antitrust Authority. Table 1.13 presents the results. The first four columns of table 1.13 report the estimate for all the observations (in which we found evidence of a final decision). The last four columns report the estimates for the subsample of observation for which we find some press coverage. For event, following the predictions developed in box 1, we test the hypothesis that an upheld decision from the last Court has a negative impact on firm's market value. If we look at the first four columns for the 31-day event window we see that the event has a negative impact of around 2% and the estimates are statistically significant either when we look at the full sample or when we look at the Italian subsample (although the Theil estimate is not statistically significant).

If we restrict our analysis only to the observations for which we find press coverage we

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Table 1.13: Last Court Decision Results:Upheld

	(a) full sample				(b) observations with press coverage			
	All firms (N=49)		Italian Sel. (N=17)		All firms (N=21)		Italian Sel. (N=10)	
Event Window	OLS	Theil	OLS	Theil	OLS	Theil	OLS	Theil
31 days (-20;+10) AR	-2.82^{*}	-2.03^{**}	-5.03	-2.05^{*}	-3.58^{**}	-3.05^{***}	-6.71^{**}	-5.04^{***}
test	-1.40	-2.02	-1.08	-1.54	-1.74	-2.45	-2.29	-2.59
11 days (-5;+5) AR	-0.89	-0.30	0.19	1.51	-0.92	-0.38	-0.62	0.25
test	-0.78	-1.11	0.07	0.05	-0.79	-1.13	-0.37	-0.67
6 days (-1;+5) AR	-0.77	-0.35	-0.27	0.69	-1.20	-0.65	-2.10^{*}	-1.30
test	-0.85	-1.12	-0.13	0.26	-1.30	-1.19	-1.59	-1.04
3 days (-1;+1) AR	0.12	0.31	0.98	1.46	-0.73	-0.38	-0.43	-0.04
test	0.21	0.54	0.72	2.17	-1.20	-0.92	-0.51	0.16

Estimation Window (-230;-30)
Abnormal Returns as percentage; One-sided test, significance levels *** 1% ** 5% * 10%

see that the size of the impact gets larger, going from -3% to -6.7% (with the impact being larger for the Italian selection) and we gain statistical significance. This suggests us that those events that receive higher attention from the media (for which we find press coverage) are also associated with a higher loss in market value.

The shorter event windows do not capture any significant effects. However given the rumors and speculations we find around the date of the event we believe that the full effect for this event is better captured by a relatively longer event window.

We run a regression on the individual CARs also for this event to analyze the presence of a time trend and the determinants of the CAR's magnitude. We regress the CARs on a time trend, on the CARs found at the decision stage, the CARs found at the start of investigation, on a dummy for Italian firms, and on a dummy for the presence of a press release. Table 1.16 (Appendix C, column 1)presents the estimates of this regression. For this data we find a statistically significant time trend. Also, we find that the CAR at the decision stage is positively correlated with the CARs at the last Court stage (the coefficient for the CAR at the start of investigation is not significant). This suggests that the impact at both stages is of the same (negative) sign. If a firm has lost market value after the infringement decision it is likely to lose value also after an upheld decision of the last Court. The other coefficients are not statistically significant.

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Table 1.14: Last Court Decision Results:Annulments

	(a) full sample				(b) observations with press coverage			
	All firms (N=27)		Italian Sel. (N=14)		All firms (N=22)		Italian Sel. (N=13)	
Event Window	OLS	Theil	OLS	Theil	OLS	Theil	OLS	Theil
31 days (-20;+10) AR	−0.50	0.73	2.53	4.68	0.19	1.25	4.15*	5.92
test	−0.25	−0.66	0.89	0.04	0.09	−0.70	1.46	0.31
11 days (-5;+5) AR	0.56	1.28	2.42	3.31	0.61	1.29	2.87**	3.65
test	0.50	0.23	1.50	0.69	0.51	−0.15	1.76	0.70
6 days (-1;+5) AR	0.56	0.84	0.85	1.22	0.51	0.73	1.33	1.59
test	0.64	0.37	0.67	0.20	0.53	−0.07	1.04	0.27
3 days (-1;+1) AR	−0.48	−0.34	−0.46	−0.26	−0.66	−0.52	1.59	0.18
test	−0.84	−0.75	−0.56	−1.08	−1.08	−1.12	0.27	−0.80

Estimation Window (-230;-30)
Abnormal Returns as percentage; One-sided test, significance levels *** 1% ** 5% * 10%

1.4.3.2 Last Court annuls the Authority's decision

In this last part we look at the impact of the annulment decisions. As stated in box 1 for this event we predict to find a positive impact on the market value of firms. We predict to find this positive reaction both in the case in which the last Court only annuls the fine and also in the case in which the last Court finds no infringements hence we do not differentiate between these two cases.

Table 1.14 presents the results for this event study and likewise the upheld event we present the analysis for two different subsamples depending on whether we find or not press coverage of the event.

When we look at the 31-day event window we find that most of the estimates have the expected (positive) sign. However only one estimate is statistically different from zero (at 10%). Moreover when we restrict the sample only to those observations for which we find press coverage we find a higher (positive) impact of this event but still we do not gain much statistical significance. For shorter event windows the results do not change, we still find mostly positive effects but the estimates are not statistically significant. We do find a statistically significant estimate (2.87% significant at 5%) in the 11-day event window.

We run a regression on the individual CARs to analyze the presence of a time trend and the determinants of the CAR's magnitude. We regress the CARs on the same regressors

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we used in the case of an upheld decision. Table 1.16 (Appendix C, column 2) presents the estimates of this regression. From the estimates we find that the CAR found at the start of investigation stage is negatively correlated with the CAR found in case of annulment. Hence, a negative CAR at the initial stage is associated with a positive CAR after an annulment decision. This suggests that there is a possibility to recoup the lost market value. The effect seems to be strong as the coefficient is larger than 1 (in absolute terms). Hence if the loss in market value at the initial stage was of 1% there is a gain of 1.2% after an annulment decision. This large effect might realize because the total loss might not be all concentrated around the start of investigation but also in other periods and the gains after the annulment have to make up for all losses due to the antitrust investigation. Differently, the coefficient of the CAR at the decision stage is not significant. The regression does not find a statistically significant impact for having a press release or for Italian firms

1.5 Conclusions

This paper studies the stock market reaction to antitrust investigations carried out by the Italian Competition Authority. We study two types of antitrust investigations: abuse of dominance cases and cartel cases. To estimate the impact of antitrust investigations on the market valuation of the investigated firms we use standard event studies techniques. In particular we follow two estimation procedures and perform both a parametric and a fully non-parametric event study.

We define the three events of a typical Italian antitrust investigations: 1) start of the investigation; 2) decision of the Authority; and 3) decisions of the Court of Appeal. The analysis is conducted both on the full sample (1991-2007) and on two smaller sub-samples (1991-1997 and 1998-2007). We do so to check if the market reaction to antitrust investigations has changed over time. We break our sample around the year 1998 because there is evidence of a change in the enforcement of competition law and in the use of fines made starting from 1998 (Ghezzi and Polo (2001)). For the empirical analysis we build a novel dataset consisting of 155 case/firms observations in which the investigated firms are listed (or owned by a listed parent company).

For the event start of investigation we find that firms on average lose 0.6% (statistically significant) of their market value. Interestingly there is a clear difference between the impact observed pre-1998 and post-1998. In the first sample there is a loss of around 1% (statistically significant) whereas in the latter sample we find no effect.

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At the stage of the Authority's decision we distinguish between two events: infringement decisions and acquittal decisions. For the former we find a negative effect that is around 1% (but marginally significant), for the latter we find a positive effect but not statistically significant. As for the event start of investigation we find that, for infringement decisions, the market reaction was much stronger (negative) and significant in the period pre-1998.

About the last Court of appeal we distinguish between upheld and annulment decisions. When the last Court upholds the Authority's decision we find a negative impact of around 2% (statistically significant). For the annulments we find a positive effect but not statistically significant[42].

Our estimates suggest some interesting findings. First of all the size of the impact, on the market value of the investigated firms seems to be in line with the effects found in similar papers (effects ranging from 0.6% to 2%, see Bizjak and Coles (1995), Bosch and Eckard (1991), Mariniello (2006) and Langus et al. (2010)). In general it is informative to observe that larger impact is found at the last stage of the whole process, after the last Court of appeal decision.

This might happen, at least for the Italian case, because the Court of appeal seems to reject a considerable number of cases. We find that 35% of the cases are annulled by the last Court of appeal (27 annulled cases against 49 upheld) and that the final fine is on average only the 57% of the initial fine. This evidence might well suggests that investors might wait until the last step before assessing the impact of the investigation on the market value of firms[43].

To this respect it is not surprising that the start of investigation and the last Court decision are the event with higher statistical significance while the Authority's decision leaves the market almost unaffected on average. The start of investigation represents the "surprise" that a firm might be subject to a fine and to the ceasing of anticompetitive practice, and hence it entails some market reactions, while the last Court confirms or invalidates this expectation. It is in line with this argument the fact that in case of acquittal or annulments the market does not react.

[42] For the event last court of appeal we do not perform a pre-1998 post-1998 comparison because of the small sample size.

[43] This view is also shared by some article in the specialized press. See for instance "LE REGOLE TEMPI DURI PER I GARANTI Antitrust", La Stampa, Section Economia, Raphael Zanotti, February 11, 2007; "In borsa Eni ed Erg più forti dell'indagine Antitrust", Milano Finanza, Giovanna Nardi, January 25, 2007; and "L'Antitrust disarmata" La Stampa, Front Page, February 13, 2007.

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Another interesting finding relates to the change in the average market reaction that we find over time. Despite the Authority being "tougher" after 1998 the market seems to react less to antitrust interventions. Both for the start of investigation and for the Authority's decision we find much larger and significant effects for the pre-1998 period than the post-1998 one. At a first sight this might contradict the fact that antitrust enforcement actually got tougher. However this might be explained by two other facts. First of all the share of upheld cases went from 75% (pre-1998) to 63% (post-1998) and the average fine reduction went from 25% to 37% over the same period of time. Secondly the market, over time, might have learned that Authority's decisions might only be transitory and might well be annulled in the appeal process. Market reactions might have been larger in earlier periods (antitrust enforcement in Italy started only in 1990) to then reduce as market operators learned about the high rate of annulments.

Another explanation to the above findings is given by Sabbatini (2008) where, in his assessment of the Italian antitrust enforcement, he asserts that cartels do not simply break up after uncovering them, as cartel might very well continue as "well established" rules.

Finally it is difficult to say if the estimated effects are large or small, however we believe it is informative to compare these effects with those found in similar papers and to look at the evolution of market reactions over time. This type of analysis can then provide evidence on the market perception on antitrust investigations considered in their entirety (from the start of the investigation to the appeal process).

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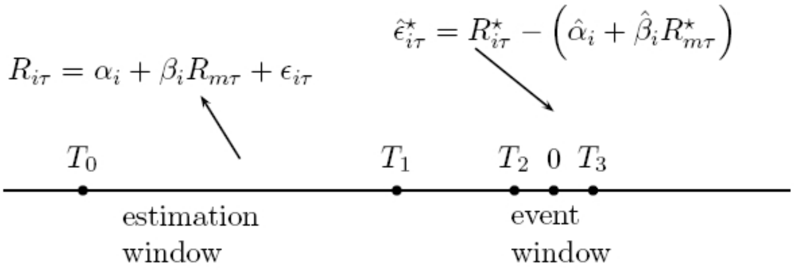
1.6 Appendix A: Figures

Figure 1.2: Event Study Timeline



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1.7 Appendix B: Model estimation

1.7.0.3 OLS estimation

In the first approach we follow the standard OLS estimation of the market model outlined in Campbell et al. (1997). For each firm in our sample we use OLS to estimate the market model in 1.6. There we regress the daily returns of security i, on a constant, and on the daily market return R_m [44].

$$R_{it} = \alpha_i^{OLS} + \beta_i^{OLS} R_{mt} + \epsilon_{it} \tag{1.6}$$

The model 1.6 is estimated in the estimation window represented in Figure 1.2 as the space between T_0 and T_1. There is no specific rule on how to choose the estimation window and for our study we take a window of 201 days that span from 230 to 30 days before the event realization. The length of the event window is usually chosen to be between 100 and 250 days[45]. It is also common not to extend the estimation window to the days just before the event as to avoid the model picking up movements in the returns due to information leakages.

Under general conditions OLS is a consistent estimator of the market model parameters and under the normality condition it provides the minimum variance among all unbiased estimators. From the OLS regression, for each firm, we obtain the parameters $\widehat{\alpha^{OLS}}$ and $\widehat{\beta^{OLS}}$ that we will use to estimate the expected normal return (R_{it}^n in equation 1.1) for each day of the event window. For each day in the event window we can then compute the abnormal return AR_{it}^{OLS} as in equation 1.7 (where R_i^* and R_m^* are respectively the actual return on the firm i security and the actual return of the chosen market index).

$$AR_{it}^{OLS} = R_{it}^* - (\widehat{\alpha_i^{OLS}} + \widehat{\beta_i^{OLS}} R_{mt}^*) \tag{1.7}$$

1.7.0.4 Theil's method

OLS estimation of the market model is the traditional choice in the majority of event studies. However Dombrow et al. (2000) show that when the normality condition fails to hold other non-linear estimators may be preferred. The same authors argue for the adoption

[44]The market return is chosen to be the leading market index of the stock market in which the firm security i is listed. The market indexes used are Milan Mibtel, DAX 200, FTSE 100, Dow Jones Industrials, Nikkei 225, Swiss Market, OMX Helsinki, SBF 250 and Brussels all share.

[45]Corrado (2010) suggests using an estimation window of 250 days that corresponds approximately to the number of trading days in a calendar year.

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of robust statistics when the underlying distribution of the errors is uncertain. They then propose to use a nonparametric estimator, suggested by Theil (1950), for its high efficiency and ease of computation and implementation[46]. In contrast to OLS the Theil's estimator does not need any distributional assumptions and can be implemented as follows[47]:

for each case in the sample:

1. Sort the L_1 ($L_1 = T_1 - T_0 + 1$) data pairs of (R_{mt}, R_{it}) in ascending order of R_{mt}.

2. Separate the data into two groups based on the median[48] .

3. Calculate the slope parameters $\beta^{Theil}_{i,(j,j+\frac{L_1}{2})}$, in 1.8, for all the $\frac{L_1}{2}$ pair and choose the median value, β^{Theil}_{i}.

$$\beta^{Theil}_{i,(j,j+\frac{L_1}{2})} = \frac{R_{i,j+\frac{L_1}{2}} - R_{i,j}}{R_{m(j+\frac{L_1}{2})} - R_{mj}} \tag{1.8}$$

4. Use the estimated $\widehat{\beta^{Theil}_{i}}$ to estimate the L_1 parameters:

$$\widehat{\alpha^{Theil}_{it}} = R_{it} - \widehat{\beta^{Theil}_{i}} R_{mt}$$

.

5. Choose $\widehat{\alpha^{Theil}_{i}}$ as the median of the L_1 $\widehat{\alpha^{Theil}_{it}}$.

Then, similarly to equation 1.7, for each day and firm we proceed to estimate the non-parametric abnormal returns as in equation 1.9.

$$AR^{Theil}_{it} = R^*_{it} - (\widehat{\alpha^{Theil}_{i}} + \widehat{\beta^{Theil}_{i}} R^*_{mt}) \tag{1.9}$$

Notice that given the median based nature of this estimator the undue influence of outliers is removed. Both the OLS and Theil's estimators are easy and fast to compute and implement. However the latter one does not need any distributional assumptions on the error term. Moreover, Dombrow, Rodriguez and Sirmans find that Theil's nonparametric estimation has relatively greater power, than OLS, to detect abnormal performance in presence of non normally distributed errors and offers comparable results to OLS under normality.

[46] For an event study that uses the Theil's estimator see Nicolau (2001) and Saleh (2007).
[47] The step procedure follows closely the methodology outlined in Dombrow et al. (2000).
[48] In case of an odd numbered interval we drop the median observation.

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1.7.1 Testing for statistical significance

1.7.1.1 Parametric test

The parametric test builds upon the OLS estimators to derive the statistical properties of abnormal returns. Under the assumption that asset returns are jointly multivariate normal, independently and identically distributed through time, it can be showed that the OLS model estimated in (1.6) is a consistent and efficient estimator for the market model parameters. From the OLS model it is then possible to derive the statistical properties of the abnormal returns, under the null hypothesis of zero abnormal returns (Campbell et al. (1997)):

$$E[\widehat{AR}_i|R_{mt}] = 0 \tag{1.10}$$

$$V_i = I\sigma_\epsilon^2 + X_i^*(X_i'X_i)^{-1}X_i^{*'}\sigma_\epsilon^2 \tag{1.11}$$

where V_i is the variance covariance matrix of abnormal returns. X_i^* and X_i are respectively a $((T_3 - T_2)$ X 2) and a $((T_1 - T_0)$ X 2) matrices of regressors (market return, R_m,and a constant) at the event window and at the estimation window. σ_ϵ^2 is the variance of the errors estimated from the OLS estimation of the market model.

From this we can then estimate:

$$V = \frac{1}{N^2}\sum_{i=1}^{N} V_i \tag{1.12}$$

that is the aggregate variance matrix of the average daily abnormal returns.

From the above results we can then construct the three test for the above statistics 1.3, 1.4, and 1.5. These are respectively:

$$\widehat{VAR}(DAAR_t) = v_{tt} \tag{1.13}$$

where $v_{tt}$is the (t, t) element of the variance covariance matrix V.

$$\widehat{VAR}(CAAR) = \iota' V \iota \tag{1.14}$$

where ι is a vector of 1s of dimension $(T_3 - T_2)$.

$$\widehat{VAR}(CAR_i) = \iota' V_i \iota \tag{1.15}$$

From which we can derive the appropriate three tests as:

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$$J_t^{DAAR} = \frac{DAAR_t}{\sqrt{\widehat{VAR}(DAAR_t)}} \overset{a}{\sim} N(0,1) \tag{1.16}$$

$$J^{CAAR} = \frac{CAAR}{\sqrt{\widehat{VAR}(CAAR)}} \overset{a}{\sim} N(0,1) \tag{1.17}$$

$$J_i^{CAR} = \frac{CAR_i}{\sqrt{\widehat{VAR}(CAR_i)}} \overset{a}{\sim} N(0,1) \tag{1.18}$$

where the distributional results are for large samples and not exact because the estimator of the variance appear in the denominator.

Non Parametric Test

For the nonparametric estimates, derived by estimating the market model using Theil's method, we also use a nonparametric test statistics. Hence we follow the advice of Dombrow et al. (2000) and perform what they call a complete nonparametric event study. The non-parametric test we use is known as the rank test and was outlined by Corrado (1989). The test is developed as follow. First, for each case, compute abnormal returns for all the days considered both in the estimation and event window. Then for every case i convert all the daily abnormal returns into their rank within the distribution of abnormal returns of that case.

$$K_{it} = rank(\widehat{AR}_{it}) \tag{1.19}$$

Higher values of rank K denote an higher abnormal return. This transformation turns the distribution of the abnormal returns into a uniform distribution of the possible ranks. Under the null hypothesis of zero abnormal returns the expected rank is just one plus half the number of days considered (if we run the analysis for 250 days the expected rank is 125,5). Then two tests, depending on the level of aggregation, are computed as follow:

$$S_t^{DAAR} = \frac{\frac{1}{N}\sum_{i=1}^{N}(K_{t,i} - (\frac{T+1}{2}))}{\sqrt{\frac{1}{T}\sum_{t=1}^{T}\left[\frac{1}{N}\sum_{i=1}^{N}\left(K_{t,i} - (\frac{T+1}{2})\right)\right]^2}} \overset{a}{\sim} N(0,1) \tag{1.20}$$

The test in 1.20 refers to daily average abnormal return estimates and T represents the sum of days both in the event and estimation window (i.e. $T = T_1 - T_0 + T_3 - T_2 + 2$).

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When we aggregate the daily average abnormal return to construct the CAR measure we then use the following test proposed by Cowan (1992) in which he extends the original test proposed by Corrado (1989) to multi day event window assuming that the daily return ranks within the window are independent.

$$S^{CAAR} = (T_3 - T_2 + 1)^{\frac{1}{2}} \frac{\frac{1}{T_3-T_2+1} \sum_{d=1}^{T_3-T_2+1} \left[\frac{1}{N} \sum_{i=1}^{N} (K_{d,i} - (\frac{T+1}{2})) \right]}{\sqrt{\frac{1}{T} \sum_{t=1}^{T} \left[\frac{1}{N} \sum_{i=1}^{N} \left(K_{t,i} - (\frac{T+1}{2})\right) \right]^2}} \overset{a}{\sim} N(0,1) \tag{1.21}$$

Under the null hypothesis of zero abnormal returns on the event window and using the result that the asymptotic null distribution of test 1.20 and test 1.21 are standard normal we can then test the null hypothesis.

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1.8 Appendix C: Tables

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Table 1.15: Regressions Results: Start of Investigation and Decision

	(1)	(2)	(3)	(4)	(5)
VARIABLES	CAR_start	CAR_dec	CAR_dec	CAR_dec	CAR_dec
	(0;+2)	(-1;+1)	(-1;+1)	(-1;+1)	(-1;+1)
t_datestart	0.147**				
	(2.202)				
capmvc	8.76e-05***	8.17e-05*			
	(2.814)	(1.894)			
fin_ent_ita	0.315	-0.690			
	(0.628)	(-1.227)			
infringed	0.940				
	(1.148)				
Abuse	-0.0178	-0.844			-0.377
	(-0.0337)	(-1.325)			(-0.656)
t_datedec		0.140*			
		(1.916)			
fine		-0.0111	-0.00608		
		(-1.331)	(-0.760)		
fine_capmvc				22.16	
				(0.639)	
Constant	-16.59**	-14.05*	-0.407	-0.527*	-0.366
	(-2.512)	(-1.900)	(-1.463)	(-1.926)	(-1.172)
Observations	154	139	139	139	139
R-squared	0.098	0.085	0.004	0.003	0.003

t-statistics in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

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Table 1.16: Regressions Results: Last Court

	(1)	(2)
VARIABLES	CAR_last upheld	CAR_last annulled
CAR_dec	0.792**	0.236
	(2.010)	(0.363)
CAR_raid	-0.222	-1.206***
	(-0.419)	(-2.317)
t_datedec	0.872*	-0.128
	(1.825)	(-0.251)
fin_ent_ita	-2.410	5.186
	(-0.849)	(1.024)
has_press	-1.133	1.628
	(-0.423)	(0.348)
Constant	-90.81*	6.613
	(-1.861)	(0.126)
Observations	48	27
R-squared	0.185	0.318

t-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

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Table 1.17: Firms and Antitrust Cases

ART.	Firm	Start of Inv. date	Decision date	Last Court date	Fine AGCM (ml euro)	Final fine (ml euro)	Decision type	Appeal type	Case id
2	Allstate	21/09/1999	31/07/2000	28/02/2002			1	0	I377
2	Azuritalia Spa	21/09/1999	31/07/2000	28/02/2002	0.000	0.000	1	1	I377
3	Fiat Ferroviaria	15/05/1998	13/01/1999		0.008	0.008	1	0	A209
2	Cam Petroli Srl	08/06/2001	06/03/2003	12/05/2004	0.017	0.017	1	1	I474
2	UNICALCESTRUZZI Spa	24/09/1996	11/04/1997	11/04/1997	0.019	0.019	1	0	I210
2	Merck Sharp, Dohme Italia Spa, Istituto Gentili Spa, Neopharmed Spa	18/03/1998	19/03/1999		0.034	0.034	1	0	I333
2	Cementir Spa	17/04/1995	27/03/1996		0.076	0.076	1	0	I123
2	Assitalia Spa	24/11/1997	17/12/1998		0.093	0.093	1	0	I305
2	Elyo Italia Srl, Opam Oils Spa, Nelsa Srl	08/06/2001	06/03/2003	12/05/2004	0.102	0.102	1	1	I474
2	ASSITALIA-LE ASSICURAZIONI D'ITALIA Spa, FATA ASSICURAZIONI Spa	31/05/1996	10/10/1997	12/01/2001	0.114	0.114	1	1	I193
2	ZURIGO Sa	31/05/1996	10/10/1997	12/01/2001	0.114	0.114	1	1	I193
2	Unipol Spa	24/11/1997	17/12/1998	28/01/2004	0.114	0.114	1	1	I305
2	Agip Petroli Spa, Atriplex Srl	08/06/2001	06/03/2003	12/05/2004	0.118	0.118	1	1	I474
2	Abbott Spa	12/05/1999	16/03/2000	02/07/2002	0.172	0.098	1	1	I328
2	Unicem Spa	17/04/1995	27/03/1996		0.185	0.185	1	0	I123
101	Milte Italia Spa	15/07/2004	20/10/2005	23/01/2008	0.198	0.149	1	1	I623
3	Alitalia Spa	04/03/1996	21/11/1996		0.214	0.214	1	0	A102
2	CALCESTRUZZI Spa, ITALCALCESTRUZZI Spa	24/09/1996	11/04/1997	11/04/1997	0.309	0.309	1	0	I210
2	Cemencal Spa, Calcestruzzi Spa	22/01/1992	28/05/1992		0.393	n.a.	1	0	I32
2	Italcementi Spa	17/04/1995	27/03/1996		0.442	0.442	1	0	I123
3	Telecom Italia Spa	07/01/1997	06/11/1997	09/02/2007	0.491	0.491	1	1	A178
2	Mediaset Spa (RTI)	12/03/1997	28/12/1998	23/10/2001	0.507	0.507	1	1	I283B
2	EMI Italiana Spa	01/11/1996	22/10/1997	17/11/2000	0.536	0.536	1	1	I207
3	Telecom Italia Spa	17/07/1998	11/02/2000		0.645	0.645	1	0	A255
2	Heinz Italia Srl	12/05/1999	16/03/2000	02/07/2002	0.667	0.530	1	1	I328
2	Toro Spa	28/06/1993	09/06/1994	18/01/1997	0.698	0.000	1	2	I74
2	Polygram Italia Srl	01/11/1996	22/10/1997	17/11/2000	0.707	0.707	1	1	I207
2	Nestlè Italia Spa	12/05/1999	16/03/2000	02/07/2002	0.719	0.644	1	1	I328
2	Nutricia Spa, Milupa Spa	12/05/1999	16/03/2000	02/07/2002	0.756	0.332	1	1	I328
2	Sony Music Entertainment Spa	01/11/1996	22/10/1997	17/11/2000	0.773	0.773	1	1	I207
2	Generali Spa	28/06/1993	09/06/1994	18/01/1997	0.827	0.000	1	2	I74
2	Warner Musica Italia Spa	01/11/1996	22/10/1997	17/11/2000	0.847	0.847	1	1	I207
2	Unipol Spa	28/06/1993	09/06/1994	18/01/1997	0.897	0.000	1	2	I74
2	Lloyd Adriatico Spa	28/06/1993	09/06/1994	18/01/1997	0.909	0.000	1	2	I74
2	Fondiaria Spa	28/06/1993	09/06/1994	18/01/1997	0.929	0.000	1	2	I74
2	Milano Spa	28/06/1993	09/06/1994	18/01/1997	0.944	0.000	1	2	I74
101	AstraZeneca Spa	17/11/2004	29/05/2006	13/12/2007	0.975	0.000	1	2	I639
101	Boero Attiva Marine, Protective Coatings Genova S.p.A.	20/04/2005	09/02/2007		1.080	pending	1	0	I646
101	International Paint Italia S.p.A.	20/04/2005	09/02/2007		1.080	pending	1	0	I646
2	Augusta Assicurazioni Spa	21/09/1999	31/07/2000	28/02/2002	1.081	0.000	1	2	I377
2	Assitalia Spa	28/06/1993	09/06/1994	18/01/1997	1.212	0.000	1	2	I74
2	RAS Spa	28/06/1993	09/06/1994	18/01/1997	1.238	0.000	1	2	I74
2	Farmades Spa, Schering Spa	09/02/2000	07/12/2000	06/03/2007	1.395	n.a.	1	2	I337
101	Milupa Spa, Nutricia Spa	15/07/2004	20/10/2005	23/01/2008	1.567	1.175	1	1	I623
2	Alitalia Spa	20/12/2000	08/08/2002	20/06/2007	1.582	1.582	1	1	I446
3	SNAM Spa	26/11/1997	10/03/1999		1.851	1.851	1	0	A221
2,3	Schindler Spa, G. Caimi Elevatori Srl	02/03/1999	29/05/2000	31/01/2001	1.934	0.000	1	2	A256
3	Italcementi Spa	21/07/1994	01/03/1995	19/04/2000	1.937	0.000	1	2	A76
2	Vittoria Assicurazioni Spa	21/09/1999	31/07/2000	28/02/2002	1.994	0.000	1	2	I377
2,3	Abbott Spa	26/04/2001	15/05/2003	29/11/2005	2.000	2.000	1	1	I461
2	Sai Spa	28/06/1993	09/06/1994	18/01/1997	2.077	0.000	1	2	I74
2	Helvetia	21/09/1999	31/07/2000	28/02/2002	2.106	2.070	1	1	I377
102	ENEL S.p.A.	15/03/2002	12/12/2003	24/05/2006	2.500	2.500	1	1	A333

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2	Sisal Spa	24/07/2003	07/12/2004	01/11/2007	2.800	1.395	1	1	I570
2	Sodexho Pass Srl	09/07/2001	01/07/2002	17/08/2005	3.193	0.973	1	1	I463
101	Nestlè Italia Spa	15/07/2004	20/10/2005	23/01/2008	3.300	2.475	1	1	I623
101	Heinz Italia Srl, Plada S.r.l.	15/07/2004	20/10/2005	23/01/2008	3.301	2.476	1	1	I623
2	Linde Spa	24/03/2004	20/05/2006	24/01/2008	3.600	0.000	1	2	I603
2,3	Kone Italia Spa	02/03/1999	29/05/2000	31/01/2001	3.631	0.000	1	2	A256
2,3	Ceam Srl, Elevat Ascensori Srl, Otis Spa	02/03/1999	29/05/2000	31/01/2001	3.761	0.000	1	2	A256
102	ENI Spa	02/11/2001	07/10/2004	06/06/2006	4.500	n.a.	1	1	A329
2	Royal, SunAlliance, Royal Insurance, Lloyd Italico Spa	21/09/1999	31/07/2000	28/02/2002	5.310	0.000	1	2	I377
2,3	Bayer Spa	26/04/2001	15/05/2003	29/11/2005	6.000	6.000	1	1	I461
2	SOL	24/03/2004	20/05/2006	24/01/2008	6.800	0.000	1	2	I603
2,3	Ortho Clinical Diagnostics Spa	26/04/2001	15/05/2003	29/11/2005	7.500	7.500	1	1	I461
2	Lottomatica Spa	24/07/2003	07/12/2004	01/11/2007	8.000	4.953	1	1	I570
2	Allianz Subalpina Spa	21/09/1999	31/07/2000	28/02/2002	8.038	n.a.	1	0	I377
101	Total Spa	17/12/2004	20/06/2006	20/11/2007	8.860	8.860	1	1	I641
2,3	Roche Diagnostics Spa	26/04/2001	15/05/2003	29/11/2005	9.000	9.000	1	1	I461
2	Holcim Calcestruzzi S.r.l., Cave Rocca S.r.l., Holcim Cementi	09/04/2003	09/08/2004		9.800	0.000	1	0	I559
2	Unicalcestruzzi Spa	09/04/2003	09/08/2004		11.000	0.000	1	0	I559
2	Calcestruzzi Spa , Cemencal Spa	09/04/2003	09/08/2004		11.850	0.000	1	0	I559
2	Rivoira Spa, SIAD	24/03/2004	20/05/2006	24/01/2008	14.000	0.000	1	2	I603
3	Coca-Cola Bevande Italia Srl	06/07/1998	17/12/1999	25/06/2002	15.808	15.808	1	1	A224
2	Gemeaz Cusin Srl	09/07/2001	01/07/2002	17/08/2005	16.356	2.210	1	1	I463
2	AXA Spa	21/09/1999	31/07/2000	28/02/2002	16.872	16.840	1	1	I377
2	Fondiaria Spa	21/09/1999	31/07/2000	28/02/2002	17.005	16.990	1	1	I377
2	Shell Italia Spa	13/10/1999	08/06/2000	28/06/2001	18.418	0.000	1	2	I165
2	Milano Spa	21/09/1999	31/07/2000	28/02/2002	20.562	20.550	1	1	I377
2	Winterthur Spa	21/09/1999	31/07/2000	28/02/2002	21.294	21.280	1	1	I377
2	Air Liquide	24/03/2004	20/05/2006	24/01/2008	23.100	0.000	1	2	I603
2	Toro Spa, Nuova Tirrena	21/09/1999	31/07/2000	28/02/2002	24.272	16.110	1	0	I377
2	Unipol Spa, Meie Spa, Maeci Spa, Duomo Spa	21/09/1999	31/07/2000	28/02/2002	25.428	17.040	1	1	I377
102	Alitalia Spa	18/07/2000	13/07/2001	15/05/2007	26.852	0.000	1	2	A291
2	Assitalia Spa	21/09/1999	31/07/2000	28/02/2002	27.364	27.320	1	0	I377
2	Erg Petroli Spa	13/10/1999	08/06/2000	28/06/2001	28.931	0.000	1	2	I165
2	Generali Spa, Fata Spa	21/09/1999	31/07/2000	28/02/2002	32.146	30.520	1	1	I377
2	Sai Spa, Nuova MAA	21/09/1999	31/07/2000	28/02/2002	40.171	36.310	1	1	I377
2	RAS Spa	21/09/1999	31/07/2000	28/02/2002	48.944	48.960	1	1	I377
2	Philip Morris (total fine)	04/07/2001	28/03/2003	16/07/2003	50.000	50.000	1	1	I479
2	Telecom Italia Mobile Spa	08/01/1999	01/10/1999	13/12/2000	51.869	19.570	1	1	I372
101	Shell Italia Spa	17/12/2004	20/06/2006	20/11/2007	56.460	56.460	1	1	I641
3	Telecom Italia Spa	25/11/1999	02/05/2001	27/01/2007	59.524	31.600	1	1	A285
101	Esso Italiana Srl	17/12/2004	20/06/2006	20/11/2007	66.690	66.690	1	1	I641
2	Esso Italiana Srl	13/10/1999	08/06/2000	28/06/2001	75.929	0.000	1	2	I165
2	Agip Petroli Spa	13/10/1999	08/06/2000	28/06/2001	111.558	0.000	1	2	I165
101	ENI Spa	17/12/2004	20/06/2006	20/11/2007	117.000	117.000	1	1	I641
3	Telecom Italia Spa	13/06/2003	19/11/2004	14/02/2006	152.000	115.000	1	1	A351
102	ENI Spa	04/02/2005	15/02/2006		290.000	pending	1	0	A358
2,3	SIP	28/07/1992	05/04/1993				2	0	A27
3	SIP	08/07/1993	03/11/1993				1	0	A55
2,3	ANSALDO TRASPORTI Spa	30/06/1993	23/12/1993				1	0	I78
3	ALITALIA Spa	29/10/1993	03/08/1994				1	0	A58
3	AUTOSTRADE Spa	08/02/1994	05/08/1994				1	0	A68
2,3	FIAT Spa	30/06/1993	01/03/1994				2	0	I79
2,3	ANSALDO TRASPORTI Spa	30/06/1993	01/03/1994				2	0	I80
2,3	FIAT FERROVIARIA Spa	30/06/1993	01/03/1994				2	0	I80
2,3	ABB TRAZIONE Spa	30/06/1993	01/03/1994				2	0	I80
3	SIP	05/07/1994	16/06/1995				1	0	A64

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Table 1.17: Firms and Antitrust Cases

ART.	Firm	Start of Inv. date	Decision date	Last Court date	Fine AGCM (ml euro)	Final fine (ml euro)	Decision type	Appeal type	Case id
3	STET-SIP - Società Italiana per l'Esercizio delle Telecomunicazioni Spa	05/07/1994	10/05/1995				1	0	A65
3	SIP - Società Italiana per l'Esercizio delle Telecomunicazioni Spa	05/07/1994	11/01/1995				1	0	A71
3	PIONEER HI-BRED ITALIA Spa	03/01/1996	22/08/1996				2	0	A124
3	AUTOSTRADE Spa	01/02/1996	12/07/1996				1	0	A84
2,3	TELECOM ITALIA MOBILE Spa	29/09/1995	19/06/1996				2	0	A90
2,3	MOTOROLA ITALIA Spa	29/09/1995	19/06/1996				2	0	A90
2	MONTESHELL Spa	21/04/1995	03/04/1996				1	0	I124
2	FINA ITALIANA Spa	21/04/1995	03/04/1996				1	0	I124
2	CIRIO FINANZIARIA Spa	11/08/1995	27/03/1996				1	0	I134
2	Arnoldo Mondadori Editore Spa	07/12/1995	09/07/1996				1	0	I157
2	RCS Libri, Grandi Opere Spa	07/12/1995	09/07/1996				1	0	I157
2,3	TELECOM ITALIA Spa	11/11/1995	03/05/1996				1	0	I167
2	UNILEVER ITALIA Spa	17/05/1996	08/01/1997				1	0	I212
2	NESTLE' ITALIANA Spa	17/05/1996	08/01/1997				1	0	I212
3	TELECOM ITALIA Spa	15/11/1996	12/06/1997				1	0	A156
2	ALLIANZ PACE Spa	31/05/1996	10/10/1997				1	0	I193
2	AUGUSTA ASSICURAZIONI Spa	31/05/1996	10/10/1997				1	0	I193
2	SAI Spa, MAA ASSICURAZIONI Spa	31/05/1996	10/10/1997				1	0	I193
2	GENERALI Spa	31/05/1996	10/10/1997				1	0	I193
2	LA FONDIARIA Spa, LA PREVIDENTE ASSICURAZIONI Spa, MILANO ASSICURAZIONE Spa+B79	31/05/1996	10/10/1997				1	0	I193
2	L'ABEILLE Spa	31/05/1996	10/10/1997				1	0	I193
2	MILANO ASSICURAZIONE Spa	31/05/1996	10/10/1997				1	0	I193
2	RAS Spa	31/05/1996	10/10/1997				1	0	I193
2	TORO ASSICURAZIONI Spa	31/05/1996	10/10/1997				1	0	I193
2	UNIPOL Spa	31/05/1996	10/10/1997				1	0	I193
2	AXA ASSICURAZIONI Spa	31/05/1996	10/10/1997				1	0	I193
2	INTERCONTINENTALE ASSICURAZIONI Spa;	31/05/1996	10/10/1997				1	0	I193
101,102	STREAM Spa	15/04/1999	21/06/2000				2	0	A274
101,102	TELEPIÙ Srl	15/04/1999	21/06/2000				2	0	A274
3	TELECOM ITALIA Spa	17/09/1999	13/07/2000				1	0	A280
3	ALITALIA Spa	18/07/2000	05/12/2001				2	0	A306
2	BLU	27/10/2000	19/07/2001				2	0	I445
2	WIND TELECOMUNICAZIONI Spa	27/10/2000	19/07/2001				2	0	I445
2	TELECOM ITALIA MOBILE	27/10/2000	19/07/2001				2	0	I445
2	OMNITEL PRONTO ITALIA	27/10/2000	19/07/2001				2	0	I445
2	American Express SERVICES EUROPE Ltd	12/02/2001	08/07/2002				2	0	I452
2	THE DINERS CLUB EUROPE Spa	12/02/2001	08/07/2002				2	0	I452
2,3	AUTOGRILL Spa	23/09/2002	28/07/2003				1	0	I523
102	MEDIASET S.p.A.	24/03/2005	28/06/2006				1	0	A362
102	GlaxoSmithKline S.p.A.	13/03/2005	21/02/2006				1	0	A363
102	Enel S.p.A.	13/04/2005	27/12/2006				1	0	A366
101,102	TELECOM ITALIA MOBILE S.p.A	01/03/2005	28/05/2007				2	0	A357
101,102	VODAFONE OMNITEL N.V	01/03/2005	28/05/2007				1	0	A357

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Table 1.17: Firms and Antitrust Cases

ART.	Firm	Start of Inv. date	Decision date	Last Court date	Fine AGCM (ml euro)	Final fine (ml euro)	Decision type	Appeal type	Case id
102	Merck Sharp, Dohme (Italia) S.p.A	14/03/2005	25/03/2007				1	0	A364
102	ENI S.p.A.	18/11/2005	09/03/2007				1	0	A371
102	GNL Italia S.p.A	18/11/2005	09/03/2007				1	0	A371

Decision types: 0 acquittals; 1 infringement.
Appeal types: 0 no appeal; 1 appeal to only 1st court; 2 appeal to 1st and 2nd court.

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CHAPTER 2

DOES PRICE COMPARISON MATTER? THE CASE OF ITALIAN HIGHWAY REFUELING

Abstract

This paper estimates the effects, on retail prices, of a fuel price comparison policy introduced in the Italian pay-toll highways. The policy deployed roadside electronic displays to compare fuel prices. We find that the price comparison policy is associated with a small, but statistically significant, increase in the average price of unleaded fuel of around 0.55 euro cents per liter. The average price of diesel fuel also increases by around 0.5 euro cents per liter. The market minimum prices (the prices that active consumers are expected to pay) increases by roughly the same amount. Nevertheless, the policy might help some consumers. A consumer who, after the policy change, would chose the lowest price retailer would save around 1 euro cent per liter, for both fuels (i.e. a saving of 0.77% on the per liter price). The above figures gain economic significance when compared to the observed price range (the difference between the market maximum and minimum price is on average 2 euro cents per liter) and retailers' margins (around 4 euro cents per liter). We conclude that although the policy can help some consumers finding lower prices it has not been effective in fostering competition among retailers.

2.1 Introduction

Price transparency is often perceived, by consumers, their advocates and policy makers, as a factor that increases market competition and in turn lowers retail prices. The consumer protection argument is that availability of price information should help consumers make informed choices, increase price sensitivity and consequently intensify competition and lower prices. Economists do not share this view and acknowledge that retailers also can take advantage of price transparency. However, theoretical models that consider both sides of the market often makes ambiguous predictions regarding the impact of transparency on retail prices. Hence whether transparency lowers retail prices remains an empirical issue.

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This paper studies the implementation of a price transparency policy implemented with the explicit aim to help consumers make informed choices and with the implicit aim to lower retail prices[1]. The market we study is the Italian pay-toll highway refueling market, a market that accounts for more than 10% of all the automotive refueling in Italy. Fuels prices are not advertised and consumers learn fuel prices only after their decision to stop at the service area[2]. These features imply that consumers face relatively high search costs (mostly in terms of cost of time) that make it optimal to just shop randomly[3].

In April 2007 the Italian parliament approved a law (Law 2 April 2007 n.40) with the aim to foster price competition among retailers and reduce retail prices. The law required highway concessionaires to introduce some remedies to increase price information and ease price comparison. The most important remedy was the deployment of physical roadside price comparison electronic displays (for a picture see Figure 2.7) comparing self-service fuel prices of the next four consecutive refueling stations. The roadside displays were gradually introduced in three different waves between July 2007 and January 2009. The policy was welcomed by consumers associations that had demanded the adoption of price comparison devices for a long time. Moreover, expected savings of up to 8 euro cents per liter were foreseen (a saving of 3.2 euro for a 40 liter refill or a 6% reduction on fuel expenditure[4]).

In this paper we estimate the impact of the roadside price comparison displays on prices. We test the effect of slashing search cost for consumers while retailers remain informed about prices. The outcome variable of interest is the final (per liter) price, asked by retailers, for a self-service refill of unleaded and diesel fuel, the two main fuel variants[5].

We consider the effects for two types of consumers, active and inactive, where we say that a consumer is active if she acquires and processes price information before making any

[1]Price transparency in general can mean that buyers and sellers are fully informed about prices. Since there are several dimensions in which buyers or sellers might lack information price transparency remedies are all those actions or policies that aim to provide more information to market participants. In this paper whenever we refer to price transparency we refer to the implemented price comparison policy unless otherwise stated.

[2]There are of course repeated sales, hence commuters might learn fuel prices over time.

[3]Consumers' search costs are at least higher in the (Italian) pay-toll highway than in normal roads. For instance to learn the retail price consumers need to slow down, divert their trip and enter the service area while on normal roads usually the learning process only entails looking at a price board.

[4]If we consider a reference price for unleaded and diesel fuel of 1.3 euro per liter. Consumers' association CODACONS expected savings up to 8 euro cents per litre.17 July 2007 (ANSA news).

[5]In Italy it is common to have both the self-service and the full-service refill options at each service stations. The price comparison policy is specifically targeted to self-service prices.

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purchase, and inactive otherwise.

We look at four different price measures considered relevant to understand the effect of price comparison on the two categories of consumers. First, we look at the average price as this is the expected price paid by consumers that are inactive both before and after the policy change. Second, we look at the minimum price as this price is the relevant price for consumers that are active both before and after the policy change[6]. Third, we look at the savings made by those consumers who are inactive before the policy change and that become active after the introduction of the policy[7]. Finally, we estimate the potential savings that a consumer would make if she would switch from inactive to active after the policy change. This last measure identifies the incentive to become active after the policy change.

We have a panel dataset in which we observe the daily prices asked by 170 service stations for a total of 255 days between October 2007 and November 2008[8]. For a subset of 87 stations we know the date of the policy implementation (this group refers to the second wave of the policy implementation, Summer 2008). To control for time varying factors, other than the policy change, that might confound our estimates, we exploit the step wise implementation strategy and define a control group composed of those stations that received the treatment during the first wave[9]. We then estimate the average effect of the policy on treated stations (i.e. we estimate the ATT[10]).

We find that, after the introduction of the policy, an inactive consumer is expected to pay 0.55 euro cents more, per liter, for unleaded fuel, and 0.5 euro cents more for diesel fuel (respectively an increase of 0.42% and 0.38% on a reference price of 1.3 euro per liter). This translates in negligible total expenditure increases on a typical refill of 40 liters (respectively

[6] These consumers are those that in absence of the roadside price comparison displays use the price comparison website to learn fuel prices.

[7] These consumers are those that without the roadside display do not find optimal to search prices on the price comparison website.

[8] The daily prices were collected through a *spider,* a software that, on a daily bases (although with some gaps), downloaded the retailers' price information from the publicly available website made available by ASPI.

[9] For these stations we only observe prices after the policy introduction. Hence, on these stations, we cannot estimate the effect of the policy introduction with a before and after setting. In an appendix (available from the author) we show how, for these stations, we can estimate the effect of the policy using a matching estimator. However given the reduced sample size the results does not seem to be robust to changes in the matching strategy.

[10] ATT: Average treatment on the treated

22 and 20 euro cents)[11].

To estimate the effect of the policy on consumers that are active both before and after the policy change we consider the minimum prices asked in the four-stations submarkets (the sub-markets defined by the roadside display). We find that also this category has not benefited from the policy. Consumers in this group pay slightly higher prices as the increase in minimum prices are between 0.55 and 0.8 euro cents.

Consumers that become active after the policy change are marginally better off. They save 1 euro cents per liter (or 0.77% for a reference price of 1.3 euro per liter) compared to the average price they paid before the policy change, when inactive. Finally the price incentive to become active after the policy change is in the order of 0.8 euro cents per liter (0.6% for a reference price of 1.3 euro per liter).

We conclude that the policy objective to lower fuel retail prices was not achieved. The opposite happened, a small but significant upward shift in prices. The provision of widespread price information to consumers has not triggered any price competition on the retailer side. The only source of potential savings for consumers is given by the availability of price comparison and the possibility, for active consumers, to find lower prices.

The paper proceeds as follows: Section 2.2 reviews the literature; Section 2.3 presents the case study; Section 2.4 describes the data, presents some descriptive analysis and presents the results; Section 2.6 draw some conclusions.

2.2 Related literature

This paper is directly related to the literature that studies the relationship between price transparency and competition. Three recent surveys discuss price transparency in relation to market competition and consumers policy (Garrod et al. (2008), SCA (2006), Armstrong (2008)). In general, this literature concludes that increased price transparency on the firm's side unambiguously facilitates oligopolistic coordinations while increased price transparency on the consumer's side can have ambiguous effects. The argument is that increased price transparency on the consumer's side, despite lowering search cost (i.e. pressure to lower prices), also implies a more elastic demand and this produces two, indirect[12], contrasting

[11] A typical medium-sized car in the Italian market has a fuel tank of around 40 liters.

[12] Increased price trasparency on the consumer's side generates direct and indirect effects. The direct effect is given by the pressure that the newly informed (or better informed) consumers will exert on lower

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effects. On the one hand, firms' deviations from collusive prices are more profitable. On the other hand,in case the deviation is discovered, the punishments will be harsher (lower profits) (Møllgaard and Overgaard (2006)). About the interplay of consumer's side and firm's side effects the literature on price transparency concludes that the effect is ambiguous and dependent on the market specific features (Møllgaard and Overgaard (2006)[13], Waterson (2003)).

This view is somehow in contrast with the conclusions drawn from traditional models that look at the issue of price dispersion. These models postulate that increased price transparency leads to lower average prices (for a survey see Armstrong (2008) and Garrod et al. (2008)). The two classical examples are: 1) the Varian (1980)[14] model where both the average price paid by informed consumers and the (higher) average price paid by uninformed consumers decrease as the share of informed consumers increases; and 2) the Salop, Stiglitz model (Salop and Stiglitz (1977)) where the average price in the market falls as the search cost decreases.

This paper contributes to the recent literature on price transparency, and price dispersion, that revamped during the last decades to study the role of information and communication technologies (ICT) in solving long term market information problems. Indeed, after the mass adoption of the internet by consumers in the late nineties, and the success of online shopping, economists looked with increased attention at the online price information provision and the dynamics of online prices. In particular in the early days of internet the common view was that internet had the potential to reduce, if not slash to zero, the search

prices. The indirect effects are given by the fact that firms, in their pricing decisions, take into account the changes in the type of demand (composed of more informed consumers).

[13]Møllgaard and Overgaard (2006) refer to Nilsson (1999), Møllgaard and Overgaard (2001), Møllgaard and Overgaard (1999), Schultz (2004), Schultz (2005).

[14]Some recent papers based on the Varian (1980) model draw conclusions on the effect of increased price information (Morgan et al. (2006), Waldeck (2008), and Lach and Moraga-González (2009)). The paper by Morgan et al. find that as the share of informed consumers exogenously increase, keeping fixed the number of firms, the expected price paid by both informed and uniformed consumers decreases. In the other paper Lach and Moraga-González introduce an increased heterogeneity in consumers types that results in more realistic (bell-shaped) price distribution. They conclude that policies aimed at increasing the amount of price information can affect the distribution of prices and welfare and that the magnitude of the effects vary depending on the shopping behavior of consumers. Waldeck (2008) establish that the link between information and pricing is not trivial. He also finds an inverse-U shaped relationship between price dispersion and the share of informed consumers. Then he finds that higher price information might lead to higher price dispersion and that more intensive search by active consumers might lead to higher average posted prices.

costs entailed by consumers (The Economist, 1999[15], cited in Baye et al. (2004), and Waterson (2003)). It follows that reduced price dispersion and lower prices, at least in online markets, were expected. Similar savings could then be expected on "real" markets when the same easiness of price comparison and low search costs are available.

Since then, several papers attempted to test whether online markets, with price comparison sites[16], were indeed close to perfect competition as it was originally thought. These papers find that online price comparison sites offer sizable savings (among these papers Smith and Brynjolfsson (2001), Ellison and Ellison (2009), Baye et al. (2004), Morton et al. (2001), Brown and Goolsbee (2002)). However, in a review of the literature Baye et al. (2006) concludes that price dispersion seems to be a pervasive feature of both off-line and online markets and persistent differences between informed and uninformed consumers are still the key to understand price dispersion.

Our paper is then naturally related to the vast literature that looks at the refueling market, especially the vast empirical literature on fuel prices. A recent paper Hosken et al. (2008) reviews both the main theories put forward to study fuel prices, and the empirical studies that attempt to relate these theories with empirical evidence. In their review of the theories[17] they conclude that although each theory is helpful to explain some aspects of fuel prices none offer an explanation for the observed price dynamics. In their empirical analysis they focus on the US market and they conclude that retail margins vary substantially over time and that stations do not follow simple pricing rules as pricing strategy are both heterogeneous[18] and dynamic[19]. Similar features applies to retail fuel market in Europe (Lach and Moraga-González (2009), Netherlands, and Foros and Steen (2008), Norway).

The aim of this paper is to contribute to the empirical literature on fuel prices and price transparency. However, it is beyond the scope of this paper to link the empirical findings

[15] The Economist, November 20, 1999, p. 112

[16] A price comparison site can just be seen as an information clearinghouse that is defined as a third party that provides price information to consumers (possibly at a cost). Examples of clearinghouses are price comparison websites, newspapers that compare prices of different firms, magazines. In this paper we study the effect of a clearinghouse that applies advances in ICT to physical retail markets. The information clearinghouse is central to derive the Varian (1980) results.

[17] Hosken et al. (2008) review five different types of pricing behavior models: 1) Static games with pure strategies; 2) Static games with mixed strategies; 3) Repeated games with collusions; 4) History dependent demand/Asymmetric price adjustment; and 5) Edgeworth cycles.

[18] Stations with very low or very high prices tend to keep their position more than stations which price is closed to the mean price.

[19] Stations often change their relative position in the pricing distribution.

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with the many theories proposed to study price transparency and fuel prices. It has been showed that the available theories have limits and are not suitable to predict or describe market outcomes, unless we resort to strong assumptions (Møllgaard and Overgaard (2006), Waterson (2003), Hosken et al. (2008)). Hence, the scope of the paper is to exploit the (exogenous) introduction of a price comparison policy to provide empirical evidence on the effects of price transparency on fuel prices in the Italian highway refueling market.

Finally, in our paper we study a technological platform that is similar to what in the theory is defined as a clearinghouse. Other papers looked at the effects of clearinghouses for online markets. Our paper studies the impact of a an ICT based clearinghouse applied to off-line markets[20]. Thus in our study we also look at the potential for (innovative) information technologies when they are applied to off-line markets (vs the online marketplaces where extensive price comparison is present).

2.3 Case study framework

This section provides an overview of the case study. The scope of our analysis is to evaluate the effect of a consumer policy, implemented in a specific market (highway[21] refueling) taking as given the market structure in place (i.e. an exogenous change in price information available to consumers). This section first describes the price comparison policy and its practical implementation. Then, it briefly describes the Italian highway refueling market with the purpose to explain the environment under study and motivate the relevance of our case study.

2.3.1 The policy

In early 2007 the Italian government, through a decree-law[22], committed to foster competition and increase consumer protection in some consumer sensitive markets, among which the fuel market[23]. The parliament then, some months later, approved the government decree

[20]The clearinghouse we study in our paper differs from other clearinghouses applied to off-line markets (for instance websites dedicated to off-line markets, or newspapers, etc...) as, in this setting, price comparison and purchasing decision can almost be simultaneous, very much like in online shopping.

[21]In this paper when we refer to "highway(s)" we refer to pay toll high capacity roads designed to carry fast motor traffic.

[22]Decree Law 31st January 2007 n.7.

[23]Other markets considered were: Fixed Line and Mobile Phone; Internet Services; Car Insurance; Mortgages; Airline Tariffs; and "best before date" in food products.

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and turned it into law (Law 2 April 2007 n.40[24]). From Art.2 of the law, (the one regulating fuel market) we can read the objectives of the legislator as follow: (1) foster competition, and (2) price transparency; (3) guarantee an adequate level of knowledge about cost of service, and (4) facilitate the comparison of alternative offers.

Although the policy objectives are stated clearly, the law is not so clear about the measures to be taken as to reach the proposed four objectives. Indeed, the law limits only to recommend the dissemination of information about prices (in comparative form) using the already available channels or by predisposing new ones. The law then delegates to an Interministerial Committee of Economic Planning (CIPE) the definition of the specific guidelines. Accordingly, the CIPE, in July 2007, published its guidelines were it prescribed concessionaires of main national roads (pay-toll or not) to predispose a price comparison information system. Nonetheless, also the CIPE guidelines delegated to a forthcoming act from the Ministry of Transport the definition of the exact procedures to follow.

In the mean time, in anticipation to both the CIPE guidelines and the exact ministerial specifications, *Autostrade per l'Italia* (henceforth ASPI), the largest Italian pay-toll highway concessionaire, decided to implement the proposed price information measures before the 2007 summer holiday (taking place usually in August, when millions of drivers use highways to reach their holiday destination). As it appears clear from ASPI's press releases of the time the decision to act in advance of further regulation or specification was to offer an information service to their customer. This decision has indeed been presented, and marketed directly by ASPI, within the category of *Customer Information Services* together with the decision to install information displays about traffic conditions and the decision to offers several other customer oriented products to facilitate (and incentive) highway driving[25].

There is no evidence that ASPI received particular pressures from public bodies (i.e. government or parliament) to implement the price information policy, as at January 2009 it is still the only highway concessionaire to have acknowledged Law 40/7 and complied to it [26]. We take these facts as evidence that how ASPI acted in the design and implementation

[24] Law 40/7 henceforth.

[25] ASPI, both during the summer period and other periods of the year, launched several customer oriented initiatives (for instance free coffe between 00.00-05.00, traffic information in English, dedicated area in stopping area for babies and pets, help in travel planning). Source: Press conference presentations: "Via Libera all'estate" 2007; "La via per l'estate" 2008; Website www.autostrade.it.

[26] Eventually it happened that the specifications adopted by ASPI did not match the ones eventually approved in the CIPE guidelines of July 2007. The CIPE specifications set a maximum of 3 consecutive stations for each comparison panel (while ASPI display has 4 consecutive stations). Moreover ASPI display mark with a highly visible green dot the cheapest station and this is not required by CIPE guidelines.

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stage was mostly independent and driven by internal company considerations.

In practice what ASPI did during the months between April and July 2007 was: 1) First, to create a software platform that stations[27] managers could use to communicate, in real time, the fuel prices offered at their premises; 2) Second, to post these prices (Figure 2.10, Appendix B), catalogued by highway code, kilometer and direction, in an apposite section within the ASPI website (www.autostrade.it); 3) Finally, to install physical price comparison displays[28] (like the one in figure 2.7 and 2.8 Appendix B) along their highway network.

Thus, the two price comparison measures adopted are:1) Price comparison website; and 2) Physical price comparison displays. Although both measures offer the same type of informative content they differ under some dimensions. The comparison website is accessible only through an internet connection (thus there is no simultaneity between information and purchase[29]), lists all the stations on the ASPI network, but it entails some positive search cost (i.e. being aware of the service; time to open the browser; locate the desired highway and pool of stations). On the other hand, the roadside comparison display only lists four consecutive stations, it is available for free to everyone (driving by), it entails almost no search cost[30], and the price information and the purchase decision can be potentially simultaneous[31]. Given these characteristics we assume that the latter measure (the roadside display) has the highest potential to disseminate price information and provide price comparison among retailers that offer close substitutes.

The two price comparison measures also differ in the way they have been introduced. Indeed, shortly after the creation of the software platform the price comparison website was already online covering all the refueling stations on the ASPI network. On the other hand, the deployment of the roadside displays could not be as instantaneous. It was indeed a long process that started in summer 2007, with only few displays installed, and came to an end

[27] We use the words "station", "refueling station", "service station", "retailer", interchangeably to refer to a facility that offer the refueling service.

[28] We use the words "physical" or "road" comparison panel to refer to a tangible price comparison device (a "big" display) installed next to the roadway.

[29] Although recent development in mobile technologies make it possible to browse the web also on the move.

[30] There could be an attention cost. To process the information on the panel the driver has to divert some of his cognitive ability from driving to the acquisition of the informations. Still we assume this cost is a fraction of the cost required to access the online version.

[31] The stations listed on the panel are usually within a distance of 2 to 100 km.

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in late 2008 when eventually all stations on the ASPI network were covered by the road displays. This timing difference implied that some stations were required to post their prices both on the price comparison website and on the roadside comparison display since July 2007 while, at the same time, some other stations only had to post their prices on the price comparison website, and only at a later stage were then covered by the roadside comparison display.

In our analysis we argue that we can exploit this timing difference to estimate the impact of the introduction of the roadside price comparison displays. To do that we exploit the price comparison website, for the purpose of data collection[32]. Then we use information about the timing of deployment of new roadside displays to identify the effect of the policy.

The following section describes the ASPI deployment decision and it explains how we can exploit it to estimate the impact of this price information measure on market prices.

2.3.2 The policy implementation

Differently from the price comparison website the deployment of the roadside price comparison displays was not simultaneous for all stations on the ASPI network. Institutional and physical constraints, together with corporate decisions, brought to a deployment in several stages. As we discussed above, ASPI had since the beginning all the intention to deploy the price comparison displays as soon as possible on its network. However, it was not possible to install all the displays in such a short time because of limitation imposed by procurement law. Indeed, concessionaire of pay-toll highways are required to issue a call for tender, with European wide publicity, for works with starting value higher than Euro 221,000.

Therefore ASPI decided to split the deployment in two stages: First stage, deploying only 10 displays as to overcome the limitation imposed by the public procurement law; Second stage, for the remainder of the project, issue a call for tender, with European wide publicity, assign it and complete the deployment to cover all the ASPI network. For the purpose of our analysis we identify two clear phases of deployment. The first phase took place in July 2007 in which all the first 10 displays were installed; the second phase took place in July 2008 when other 24 displays were installed (Table 2.5 Appendix A shows the display position and time of deployment). We then exploit these two phases design to empirically test the impact of the introduction of the price comparison policy.

[32]The introduction of the price comparison website was simultaneous for all the stations on the ASPI network. We assume the effect of the website is the same for all the station and the estimate we find take as baseline the case in which prices are posted on the web.

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2.3.2.1 Phase 1- A quasi experiment?

During the first phase only 10 price comparison displays were installed, covering 38 service stations[33]. As we said above this resulted as an outcome of the trade-off between the limitations imposed by the institutional constraints and ASPI commitment to offer an information service to its customers before the 2007 summer holiday. Given the low number of displays installed ASPI had to make a location decision for these first displays. Such location decision was not random. Indeed, the objective was to expose the highest number of customer to this new service. Hence, ASPI targeted the highway sections[34] with the highest road traffic levels. These turned out to be the outbound highway sections close to the largest cities along the ASPI network. The cities targeted in the first wave of deployment were Naples, Rome, Florence, Bologna and Milan. Although not random, the location decision was based on an observable characteristic (the traffic level, and possibly the outbound direction).

Hence, as long as we find other highway sections with similar traffic levels and market conditions we can argue that conditioning on the observable traffic level the decision to *treat*[35] some service stations, instead of others, was indeed random. We could exploit this feature and use a *matching on observable* estimator to estimate the impact of the policy during the first phase. Unfortunately, it turns out that the sample size is very limited for this type of exercise and consequently the results are not robust to slight changes in the matching strategy[36].

2.3.2.2 Phase 2- Before and after analysis

After July 2007 no other display was installed until July 2008 when the second wave of deployment started. As mentioned above the second phase started only after the assignment of the full procurement, for which ASPI had to issue a call for tender with European wide publicity. The aim of the second phase was to finish the project started in 2007 and cover all the ASPI network with roadside price comparison displays (a total of 53 displays

[33]Actually there are 40 stations that are covered by this first 10 panels, however two stations were not surveyed by the website we were monitoring. Only later this inconsistency was fixed.

[34]We use the words "section" and "segments" interchangeably to refer to a part of motorway between an entry point and the first exit point on the same direction. Usually there is never more than one service station for a single segment. Usually for each stations on one segment there is one on the opposite segment, on the opposite side of the highway.

[35]We adopt the treatment evaluation terminology where we refer to treated units whenever we mean those units that are directly affected by the policy. In our case treated stations are those stations that are required to post their prices on the roadside comparison panel.

[36]The matching on observables estimates are presented in an appendix available from the author.

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were planned) within the end of 2008. Of this second phase we analyze a specific wave of deployment that took place in July 2008 just before the summer break.

In this wave a total of 24 roadside displays were installed (concerning other 87 stations) bringing the total number of active price comparison displays to 34 (concerning a total of 125 stations). Again our objective is to estimate the impact of the introduction of these displays.

Differently from the first wave at this stage we do not know the process behind the selection of the new display location. Thus we cannot use the matching on observable estimator that we could use in the first phase estimation. However for the second wave we have available a richer set of information. We indeed recorded the daily prices, for all stations on the ASPI network, since July 2007. Thus once the second wave took place we are able to compare the prices before and after the installation of the new displays (something we could not do for the first phase due to the lack of before price information). Accordingly, in the empirical analysis we use a difference in differences (DID) and before and after (BA) estimators to estimate the effect of the policy change.

2.3.3 The highway refueling market

This section provides some information about the Italian highway fuel market. It is beyond the scope of this paper to illustrate in detail and model the market structure in place. We take the market structure as given and we simply assess the effects of increasing price transparency on the consumer side when retailers are fully informed. We remain agnostic about the role of each players (retailers, oil company, highway concessionaire) in the reaction to the treatment as we are only interested on the effect on the final price offered to consumers. Still this section offers an overview of this specific market as to guide us in interpreting and evaluate our findings.

In this paper we study the Italian highway refueling market. In Italy there are more than 6500 Km of pay-toll highways and although this network only accounts for the 2% of the national road surface, its roads sustains about 25% of the national transportations needs[37]. ASPI is the main pay-toll highway concessionaire and has concessions for roughly 3000 Km (almost half of the entire national network). Its network covers almost all the country with exception of very few regions (Sardinia, Sicily, Calabria, Trentino Alto Adige). On Italian highways there are more than 450 service stations (of which 210[38] on the ASPI

[37] Source AISCAT Association of Italian Highway Concessionaire, www.aiscat.it.
[38] Year 2007.

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network) selling a range of fuel products (typically: unleaded, premium unleaded, diesel, premium diesel). Stations operating on the highways represent only the 2% of the total service stations operating nationwide, however they supply more than 10% of the total fuel consumption (respectively 6% for unleaded and 15% for diesel fuel) (Unione Petrolifera 2006 and 2007). By volume the most sold fuel on highways is diesel that accounts for more than 75% of total fuel supply (UP (2007)).

The range of fuels sold by each stations is considered homogeneous (for instance oil companies even share refineries in some cases). That is, within each category of fuel, products offered by different brands are qualitatively the same, the only differences that might arise come from brand differentiation not related to the quality of the fuel (advertisement, corporate social responsibility, loyalty programme).

There are eight major brands that operates on the Italian highways: Agip, Esso, Erg, Shell, Q8, Total, Api/IP, Tamoil (differently from ordinary roads on the highways there are very few "independent" retailers[39]). All these competitors are vertically integrated firms that are active at every stage from the production to the distribution process. For what concerns the end market they all can rely on an extensive network of service stations distributed all over the country. Such stations can be directly owned by the oil companies or given in concession to third parties that owns and manage them.

The price setting happens usually in two stages. In the first stage the oil company indicates to the station manager a "suggested price". At the second stage the station manager can discretionally change that price, within a range imposed by the oil company. This range is implicitly determined by two contractual conditions: the lower bound is given by the price at which the station manager buys the fuel (assuming they do not sell at a loss); the upper bound is usually a ceiling on the price the station manager can practice (usually determined by the oil company in relation to the "suggested price").

Thus the station manager's freedom in setting prices appears to be somehow limited, although oil companies seems to sustain that at the station level managers can still pursue an independent pricing strategy (AGCM (2007)). Nonetheless, oil companies retain powerful instruments to influence these possible independent strategies (i.e. the suggested price and contractual relations). Indeed by looking at past publications of "suggested prices" [40](not

[39] Usually known as "pompe bianche" (white pumps). These independent retailers buy the fuel at the wholesale market, directly from refineries, and then sell it. They are usually characterized by very low expenditure in marketing or branding and are popular for offering lower prices or discounts

[40] Available on the "Staffetta Quotidiana" an energy sector magazine.

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anymore available after a recent Antitrust ruling) it appears clear that, although oil companies publish a reference "national suggested price", for all the category of fuels and different types of service (self-service vs full service), they also set a variety of price differentials (to be applied to the reference price) targeting smaller groups of stations (usually defined by location). It is not rare that oil companies set a suggested price almost for each single station (this is exactly the case for some stations located on the highways). However, for the sake of our analysis we are not too concerned about the actual shares of power in the pricing decision. As we have access to the price posted by the station managers (that in turn represent the price asked at the pumps) we only focus on the final price as it has all the information we need to perform our study.

2.4 Case Study

In the following sections we first describe our sample, we then present some descriptive statistics on the refueling market, and finally we employ econometric techniques to estimate the impact of the price comparison policy.

2.4.1 The service station sample

To conduct our analysis we collected information on 178 service stations (about 40% of total Italian highway service stations and 85% of ASPI service stations) operating on the ASPI network. These stations were selected for two reasons: 1) availability of price information; 2) location. As we have already mentioned, we collected the daily price information directly from the ASPI website[41]. For what concerns the location, among the sample of all the service stations along the ASPI network, we selected only those located in places relevant for our analysis (i.e. either they were treated stations or had the potential of being included in the control sample).

However in the empirical analysis we will not use all the 178 stations indeed depending on the estimation technique we use and depending on the assumption we make we construct every time an appropriate control group that we then compare to the treatment group.

For what concern the time dimension of our analysis we have collected price information, for unleaded and diesel fuel (both in the self-service typology[42]), for a total of 255 days. The

[41] We programmed a "spider" that every day downloaded price information from ASPI website (www.autostrade.it).

[42] Usually stations offers both self and full service. However we restirct our analysis only to self service. Indeed, price comparison website and roadside panels only advertise self service prices and full service prices are not advertised.

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days are not consecutive (there are some gaps[43]) and span from 26/10/2007 until 11/11/2008. Again in the econometric analysis we will not make use of all the time information as we will have to discard some time periods or days[44], but this will be explained in more details in the next sections.

Our panel dataset is unbalanced as for some days we do not have price information for some retailers. We believe this is due to technical problems not to a strategic use of the price information platform. However, we consider this issue and, as a robustness check, we restrict our sample only to those stations for which we have all the daily prices.

As we do not observe relevant control variables (for demand and supply factors) with enough time variation at the local level we favor a fixed-effect estimation and we impose station-level fixed-effect as to capture time invariant factors that might influence pricing. Time effects should then control for the cross-section variation in demand and supply specific factors.

2.4.2 Descriptive statistics

In this section we present some descriptive statistics to guide the econometric analysis and the interpretation of the findings.

Service stations located along the toll highways, on average, sell almost three times more unleaded fuel and eight times more diesel fuel compared to the average station located in a non-toll road (Table 2.1). This could be taken as evidence that stations along the highway can experience higher variations in total profits due to small variations in final price (holding demand constant). However, stations along the highway are usually of bigger size (higher fixed costs) and have also to remunerate the highway concessionaire, in addition to the oil company and both these factors might depress the size of final profits.

[43] For instance during summer 2008 when the second wave of panel deployment took place no data were collected. Since we could not recover exactly on which day each new panel was turned on we decided to restart collecting data once the second wave was over and we knew all the new panel were active.

[44] For instance some Sundays there were some nation wide promotions on refueling. We discard the Sundays from our sample because we might pick up price effects that are not related to the introduction of the price comparison panels.

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Table 2.1: Average Fuel Sales per stations 2006 (ml litres)

location	toll highways	ordinary roads
service stations #.	461	21989
Unleaded	2.22	0.73
Diesel	6.19	0.74
Total	8.41	1.47

Source: elaboration from Unione Petrolifera 2006, 2007

If we look at price levels we can observe a peculiar characteristic of the retail refueling market under study: prices seldom move at the station level. For instance if we plot retail prices against the oil company "suggested price" (the price that should reflect cost variations) we observe that prices at the stations level follow almost one to one the variations in the suggested price without having significant independent movements.

Figure 2.1 shows this relation, over a period of two months, for two randomly chosen AGIP and two randomly chosen Tamoil retailers. The two solid lines are the prices set by the oil companies and the other two dashed lines depict the retailers' prices . From the figure it seems that once the oil company's suggested price changes the retailers fix a margin (positive or negative) and keep the price fixed until a new movement in the suggested price takes place.

Thus, differently from other papers that have looked at the refueling market (for instance Foros and Steen (2008)), we do not observe significant (non-cost related) price variation neither within the same day nor within the same week. For instance we also check (Table 2.6 Appendix A) if price changes happen systematically on a specific day of the week. We find that on average, over the period we consider, 84% of the time stations did not change their prices, 9% of the time there was a price increase and 7% of the times there was a price decrease. The other evidence we find is that almost no station increases the price on a Sunday. Hence, it does not seems there is any preferred day for a price increase or a price reduction.

We also check for evidence that retailers adopt mixed strategies in their price setting decision. In doing so we check that retailers for instance do not always price at the bottom or at the top of the price distribution. For the purpose of our study if we find evidence that retailers adopt mixed strategies it means that retailers can put in practice an effective price competition (for similar analysis see Lach and Moraga-González (2009)) Figure 2.11 and Figure 2.12 (Appendix B) present some charts, respectively for unleaded and diesel fuel,

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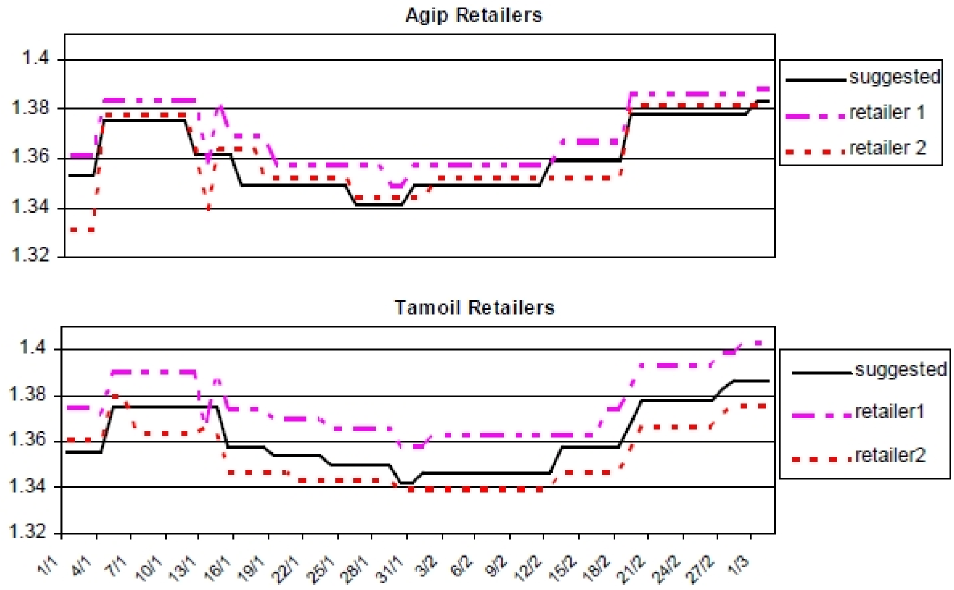
Figure 2.1: Suggested and Retailer Price Variation



depicting the frequency of stations that posted a price in the first, second, third and fourth quartile of the price distribution[45].

In each chart the y-axis represent the percentage of days each stations posted a price in that quartile. For instance if a station always had the maximum price we should expect it to appear on the chart of the fourth quartile with a $y = 1$, and not to appear in any of the other charts. By inspecting the charts we can see that there are only few retailers whose prices are always within a quartile of the price distribution (in the chart we do not find significant bar in correspondence of $y = 1$), this suggests us that retailers posted prices that were in more than one price quartile of the daily regional price distribution. Also we can see, from the charts about the first quartiles, that for both unleaded and diesel fuel very few stations never priced in the first quartile. Indeed we can say that 95% (90%) of retailers priced unleaded (diesel) fuel in the first quartile at least once. Again we can take this as evidence that stations vary their position within the price distribution. Thus although we saw that firms do seldom change prices, when they do change it the change is sizeable as

[45] A similar chart can be found in Lach and Moraga-González (2009). To construct this chart we use the prices in levels. For each day, by regions (to account for local price differences), we look at the price distribution and assign stations to the relevant quartile. We then average across time this data to construct a measure of permanence in each quartile for each station. The data refer to 100 stations for 96 days. Sunday prices were not considered as during Sunday some nationwide promotions offered price discounts.

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this most likely moves them into a different part of the price distribution. This can be taken as evidence that in the market there is scope for price competition.

Table 2.8 (Appendix A) presents some descriptive statistics about the sample of service stations. During the first phase of display deployment about 21% of service stations are treated whereas after the second wave a total of 62% of stations are treated. About the brands representation in our sample Agip appears to be the predominant brand with almost 30% of service stations. Tamoil is the second more frequent brand with a share of 20% and Esso follows closely at 16%. The majority of the stations in our sample are located along the two main ASPI highways the A1 (33%) and the A14 (30%). About the geographical distribution we can see that the majority of service stations are located in the Center (43%) another 37% are located in the North and the remaining 21% is located in the South of Italy.

On average stations in our sample have more than twenty thousand vehicles transiting every day on their relevant highway segment (although only a fraction actually stops at the station for refilling services). About 76% of these transits are made by light weight vehicles (cars or vans) and the rest is made up by heavy weight vehicles (trucks). However stations differ widely in their average transits and the transit distribution looks very right skewed.

2.4.2.1 Observed price dispersion

In this section we provide some information about the average price dispersion. In the econometric analysis we estimate the impact that price comparison displays have on final prices. Thus in order to say something about the magnitude of this effect we need to present some statistics about the average price dispersion that a customer might expect to find when refilling her car. Table 2.7 (Appendix A) presents some estimates of the observed price dispersion. In that table we present an estimate derived from our sample and we compare it against estimates of price dispersion we found in other studies. For our measure of price dispersion we constructed a sample of service stations and we computed the range (Max price - Min price) for the service stations competing in the same local market [46]. We find

[46] The sample consisted of the service stations of our ASPI sample that were not affected by the first wave of price comparison panel deployment. Prices refer to a single day 6/2/2008 (between Phase 1 and Phase 2) and 84 service stations were included in the sample.

We computed the range instead of other measures of dispersion to compare our results with those of other studies that also computed the range. However for our sample we can compute the standard deviation that we use later.

The stations are defined to be competing in the same market when they are within a distance of less than 100 km.

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that on average a customer on the highway can expect prices to vary by less than 2 euro cents among the different retailers on a 100 Km interval. This implies a maximum potential saving of less than 1 euro for a typical refill of 40 liters.

We can then compare our results with those of other studies. If we look at the average range of price dispersion on another toll highway [47] we find estimates very similar to ours (i.e. less than 2 euro cents). On the other hand if we look at price range estimates for service stations operating in normal road we find a much higher price dispersion. This dispersion varies depending on the data source going from 4 euro cents per liter (a savings of almost 2 euros for a 40 liter refill) according to ISTAT (Italian National Institute of Statistics) [48], to an upper bound of more than 10 euro cents per liter (savings of about 5 euros for a 40 liter refill) according to FIGISC (Italian Federation of the Refilling Service Station Managers).

Thus we can conclude that on average customers on a toll-highways can expect to find a lower price dispersion (lower scope for savings) than the one they can otherwise find on normal roads.

Another insightful price comparison is to compare the level of fuel prices on the highway segments and on the close-by urban area. This helps us say something about the possible degree of competition we can expect between service stations on the urban area and those on the highway. Table 2.9 presents the mean price for unleaded and diesel fuel in five provincial area (Milan, Bologna, Florence, Rome, Naples). The mean price level is reported for both the service stations operating on the toll highway and for those operating on normal streets. By comparing the means we can observe that the average price level is higher among the highway service stations. However there is a significant overlap between the two price distributions (for both unleaded and diesel fuel). Thus there seems to be scope for competition between highway and normal street refilling[49]. For instance retailers operating on highway could use price comparison displays to attract frequent travellers by offering a price comparable or lower to prices available on normal roads.

The above evidence both on price dispersion and price levels are consistent with our expectations. Stations on the highway enjoy higher degree of market power compared to

[47] A22, managed by Autostrada del Brennero SpA. On the website http://www.autobrennero.it/ they publish a weekly report on fuel prices.

[48] ISTAT collects this data to produce its price inflation indexes.

[49] The mean for normal street retailers might be underestimated. Indeed we could only get price information about the twenty cheapest service stations operating at the province level.

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stations operating on normal roads. Hence highways fuel prices are expected to show both a lower dispersion and higher levels.

We can also compare the maximum and minimum price we observe in our Italian highway stations dataset to the price ranges found in other recent studies that look at fuel markets. For instance, Lach and Moraga-González (2009) find that in the non-highway fuel market in The Netherlands the difference between maximum and minimum price ranged from a minimum of 17 euro cents to a maximum of 48 euro cents (per liter). Theses differentials imply potential[50] savings in the range of 6.8 to 19.2 euro per a 40 liter refill. Although these estimates might overestimate the potential savings in The Netherlands it seems that the Italian fuel market, both on the highway and on normal roads offers a much lower scope for potential savings. This in turn might have an impact on the willingness to shop for the lowest price if consumers perceives that their attention to prices do not produce economically meaningful savings.

2.4.2.2 Highway use and customer attitude to fuel refill

In this section we provide some evidence about the average use[51] of the highway and customers attitude to self service refilling. During the year 2007 the average travel on the ASPI network was 80 Km long. Respectively 75 Km for light weight vehicles (that represents the 80% of total transits) and 99.7 Km for heavy weight vehicles. During the year the traffic level seems to be quite constant (except for a peak of light weight travels in August). A substantial share of trips, 1/3 for light and 1/4 for heavy weight vehicles, is less than 25 Km long and these trips are mainly concentrated around the metropolitan areas on both inbound and outbound directions.

For what concerns drivers refilling habits we can get some information from two surveys published by ACI (Italian Automotive Club, 2008 and 2002). These studies report that about 40% of drivers favours self service refilling always or often while another 33% opts for the self service only occasionally. The lower price of the self service seems to be determinant in the choice of the service for about 45% of the respondent whereas the rest favoured it for its flexibility. The same studies also provide some information about the customer loyalty

[50] The price differential is computed at the national level, hence the maximum and minimum price might be posted by stations that do not belong to the same submarket.

[51] Data come from two surveys: Autostrade per l'Italia 2007, Conference Presentation "Estate 2007. "Via libera in sicurezza "; and Autostrade per L'Italia 2008, Conference Presentation "La via per l'estate. Le vacanze iniziano in autostrada".

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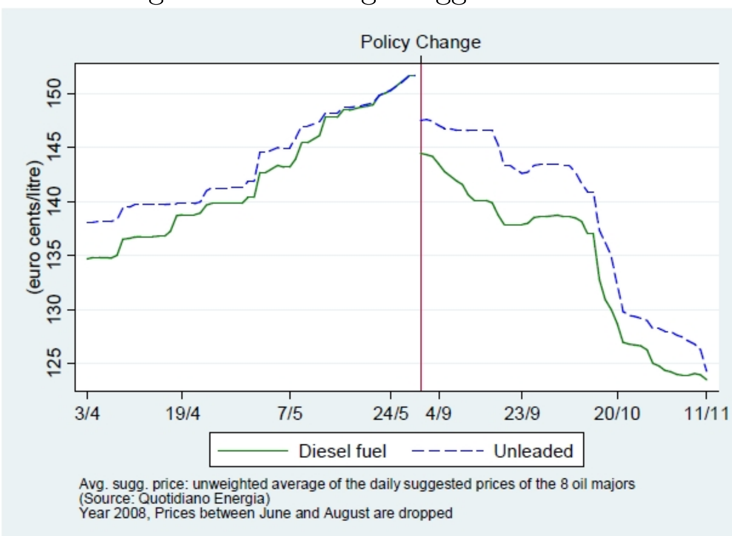
and the choice of the service stations. They report that proximity and lower price are the two determinants for the choice of the refilling station. In the urban area most of the drivers always refill from the same station whereas when outside the urban area there is no fidelity to the single service station. We can conclude that the self service refilling is relevant for around half of the population of drivers hence it is a relevant and increasingly more important way of refill.

2.5 Empirical analysis

2.5.1 Graphical analysis

Figure 2.2 shows the average suggested price for both diesel and unleaded fuel between April and November 2008[52]. Suggested prices are the prices that the oil majors "suggest" to their retailers to practice. In the year 2008 retail fuel prices varied widely as the oil price soared during the first part of the year to reach record high prices of more than 140 US$ per barrel in July 2008. After the July peak, in the second part of the year, the oil price sharply declined and eventually, in December 2008, Oil was traded at around 40 US$ per barrel (prices are for the NYMEX, WTI/Light Sweet crude).

Figure 2.2: Average Suggested Price



Retail fuel prices are directly affected by oil price movements and price variations are

[52]The graphical and the econometric analysis exclude the period between June and August 2008. During those months the panel deployment took place and they are therefore excluded from the analysis.

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almost immediately passed through to retail prices[53]. In our paper we compare retail fuel prices at different points in time, hence we need to control for cost related price variation in order to identify the variation in prices due to the increased price transparency[54]. We can control for cost variation in two ways: 1) taking the difference between prices at the retailer level and the suggested price of the respective oil major; 2) taking the difference between prices at the retailer level and a fuel reference price (henceforth Opal price[55]). In our analysis we favor the latter approach as the suggested price might also include strategic pricing decisions in addition to the common cost shocks and to the cost shocks specific to the single oil major.

Figure 2.3 and Figure 2.4 present the group averages of the residual prices (retail price less the respective Opal price) for the period before and the period after the policy change. The control group consists of those stations that received the treatment in year 2007 (38 stations) while the treatment group consists of those stations that receive the treatment in summer 2008 (70 stations). By studying the two series before the policy change we can inspect whether the parallel trend assumptions appears to be satisfied. We can see that both for unleaded and diesel fuel the two groups averages are subject to similar fluctuations. The difference with Opal prices are in general positive and this is expected as the fuel on highways is on average more expensive than the one available on other roads.

In the empirical analysis we follow a difference in differences approach (with multi-periods) to estimate the impact of the policy. From the graphical analysis we can already inspect the raw differences between the two groups' prices to look for possible effects. Figure 2.5 and Figure 2.6.show the differences between the groups price series over time. The difference between the two groups averages is greater for unleaded than it is for diesel. This is explained by the fact that in the treated group there are more stations, compared to the control group, that belong to regions[56] in which there are higher taxes for unleaded fuel

[53]In this paper we are not interested in providing a link between the price variations of oil and the variations in retail prices. Since we have a treatment and control group we control for the possible different adjustments in retail prices following increasing or decreasing oil prices. For two contrasting view on asymmetric price adjustments see Borenstein et al. (1997) and Galeotti et al. (2003).

[54]If we had a balanced panel both for treated and control station the cost differences would not be an issue as the control group would already account for those cost related differences.

[55]We use two different price indexes, both provided by OPAL (WoodMacResearch available from Datastream):Unleaded ITA Inc. Tax E/kL, for unleaded; and Diesel ITA Inc. Tax E/kL for diesel fuel. Although the index is based on surveyed Italian prices we believe the index is not endogenous as our highway sample of stations only represents the 0.8% of stations operating in Italy.

[56]Campania, Molise, Puglia.

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Figure 2.3: Treatment and Control Prices (Unleaded)

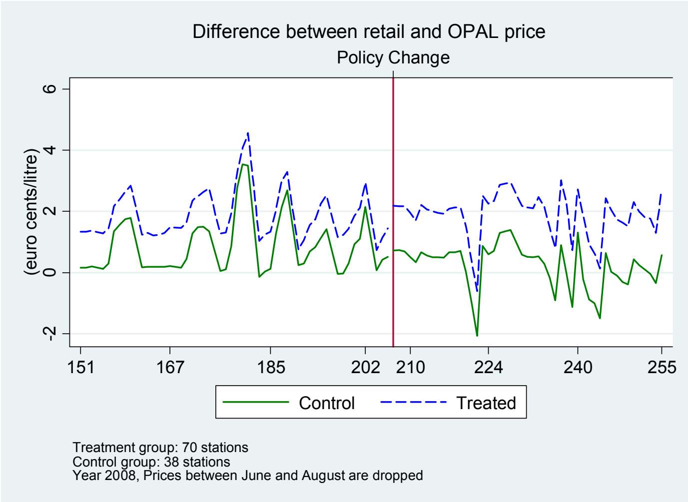
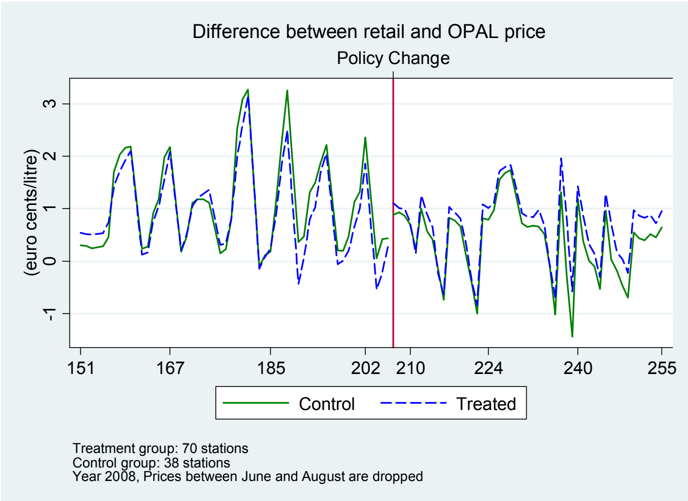


Figure 2.4: Treatment and Control Prices (Diesel)



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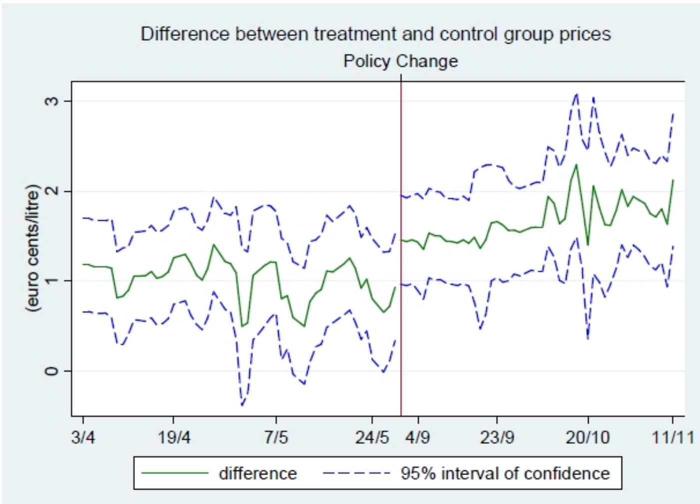
but not for diesel. Absent this different tax regime for diesel the difference between the two group prices is much smaller and closer to zero.

To draw some first evidence on the effect of the policy change we can look at the levels of the group differences before and after the policy change. From the charts we see that after the policy change both differences get larger as the mean difference series shifts upwards in both charts. Moreover the effect seems to be larger for unleaded than for diesel fuel. From this simple graphical analysis we cannot say much about the significance of these effects. By inspecting the 95% interval of confidence (the area between the dashed lines in both charts) it seems that there is substantial overlap before and after the policy change.

We then proceed with a formal econometric analysis to estimate the size of the effects and to judge on the significance of the estimates.

Figure 2.5: Group differences (Unleaded)



2.5.2 Econometric analysis

Ideally, to test the effect of increased price information on retail prices, the researcher would randomly assign price comparison displays to half of the fuel stations operating on the pay-toll highway network. These stations would correspond to the treatment group and the control group would consist of the other half of stations. Then a simple comparison of the average posted prices across the two groups would estimate the causal relationship between price information and prices. Random assignment would indeed guarantee that the difference between the outcome variables of interest is independent of all the other factors that might influence fuel retail prices.

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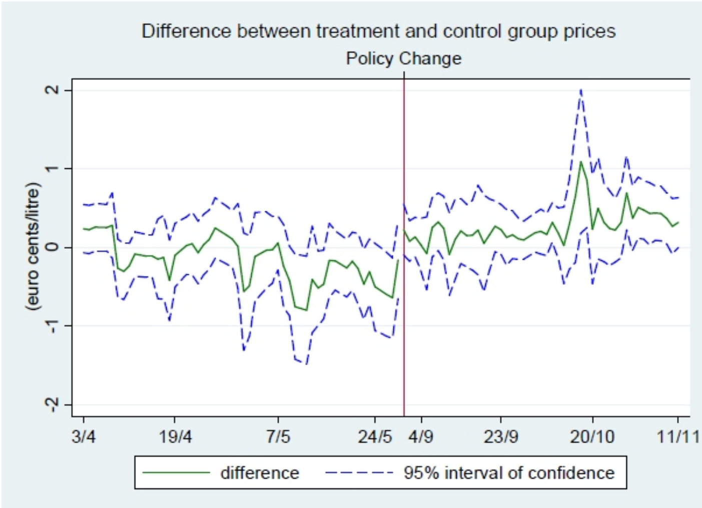
Figure 2.6: Group differences (Diesel)



Unfortunately such a theoretical implementation strategy is not the one adopted by ASPI when it installed the fuel price comparison electronic displays along its pay-toll highway network. Hence, a simple cross-section comparison of treated and non-treated stations might produce biased estimates due to an omitted variable problem[57]. However, we can exploit some features of the adopted implementation strategy to consistently estimate the causal effect of price information on retail prices. We claim that we can exploit the panel structure of the dataset, and the fact that we observe the same stations before and after the policy change, to reduce the omitted variable bias and consistently estimate the effect of price information on fuel retail prices. Moreover, we can use the stations treated in 2007 (and not affected by the 2008 policy) to control for time varying factors that affect the pricing for highway fuel retailers but that are not related to the policy. Thus we control for time varying factors, exogenous to the policy, using two strategies. Underlying fuel cost variations are accounted by differencing price levels with Opal reference price, and time varying industry specific factors are accounted for using the price variations in the control group.

The main equation we estimate is the following:

$$p_{it} = \mu + \alpha_i + \sum_{t=1}^{t=T} \gamma_t D_t + \delta TrEff_{it} + \varepsilon_{it} \tag{2.1}$$

where p_{it} is the price asked by retailer i at time t, μ is a constant, α_i is a station specific

[57]The simple cross-section comparison would fail to control for all those (unobserved) stations specific factors that are unrelated to the policy but that affect prices.

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effect, the series of D_t is the full set of time dummies, $TrEff_{it}$ is the interaction effect that takes the value of 1 only for those stations that are affected by the price transparency policy after the policy change and takes the value of 0 in all the other cases, and ε_{it} is the error term. It follows that we estimate equation 2.1 using station specific fixed-effects[58]. The fixed-effect estimator is the consistent estimator when the station-specific effect is correlated with the regressor[59]. The standard error is estimated using the standard robust variance estimator with cluster at the individual retailer level. This relaxes the assumption that repeated observations for the same retailer are independent. The above approach already accounts for possible serial correlation at the retailer level. As a robustness check, given our data are serially correlated, we also estimate a variant of equation 2.1 in which we impose an AR(1) error structure.[60]

A problem with the above estimation approach might come from the fact that it does not account for possible correlation between retailers belonging to the same display. Indeed, it could be plausible that prices of retailers that are compared on the same display might be correlated.

$$p_{ipt} = \mu + \alpha_p + \sum_{t=1}^{t=T} \gamma_t D_t + \delta TrEff_{it} + \varepsilon_{ipt} \tag{2.2}$$

To control for this problem we then estimate a different equation where we specifically account for shocks at the time-display level. We estimate equation 2.2 where we regress the price of day t for display p and for retailer i on a mean μ, on the day fixed effects as above, and on the policy interaction effect $TrEff_{it}$.The error term takes the following structure $\varepsilon_{ipt} = \nu_{pt} + \eta_{ipt}$,where ν_{pt} is the display-day shock and the η_{ipt} is the idiosyncratic individual component. We then follow the approach outlined in Angrist and Pischke (2009) and Bertrand et al. (2004) where it is shown that the simplest and most widely applied way

[58] In the econometric analysis we also test if a random effect model is more appropriate. We perform Hausman tests and we always reject the null and conclude that the fixed-effect estimator is the consistent estimator.

[59] In our econometric analysis we have only one regressor, the interaction effect $TrEff_{it}$.Still this might well be correlated with the station specific effects if the stations treated in the second phase have systematic differences from the control group stations. Note that the control group stations are those stations with the highest traffic levels and are all located on the outbound direction of the highways departing from five big cities (Naples, Rome, Florence, Bologna, Milan).

[60] Given we use as control group the 2007 treated stations, the proposed econometric approach correctly identifies the ATT only under some conditions, notably that the effect of the treatment received in 2007 can be captured by a time invariant constant and that the 2007 treatment does not affect the price trend for 2007 treated stations. In Appendix C we deal with this issue and we propose some robustness checks. We thank Saul Lach for pointing this out.

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to deal with this type of serial correlation is to estimate the model with standard errors clustered at the display level (and not at the day-display level).

Finally, as a further robustness check, we also control for clustering and serial correlation by running a simple difference in differences on the time averaged values where the time periods are collapsed in only two time periods, one before and one after the policy change. Both in Angrist and Pischke (2009) and Bertrand et al. (2004) this simple aggregation is identified as a simple, conservative and transparent approach to account for serial correlation.

2.5.2.1 Econometric results

Table 2.2 presents the results of the econometric analysis conducted on mean prices, both for unleaded and diesel fuel. This analysis is informative about the effect of price comparison on the average price that an uniformed consumer is expected to pay on a pay-toll highway. Indeed, as the uninformed consumer does not observe price comparison (or even if she observes it, she does not process the price information, i.e. inactive consumer), she is expected to refill randomly and hence pay the average price. If we compare the average price before and after the policy change we can then test if the policy had any effect on this price measure. Table 2.2 presents the results from the four types of regressions (for each fuel) explained in the above section. The first two regressions are the fixed-effects regressions with robust variance with clusters at the station level (columns 1 and 5) and fixed-effects regressions with AR(1) disturbances (columns 2 and 6)[61]. The third regression is the fixed effect regression with clusters at the display level (columns 3 and 7). Finally the last two regressions are the difference in difference regressions on the data collapsed in the two periods (columns 4 and 8).

For what concerns unleaded fuel the estimates suggest that the policy is associated with an increase in the average price of around 0.55 euro cents. The magnitude of this effect is mostly unchanged across the different regressions and the estimates are highly significant in all the regressions considered. About diesel fuel we find similar results, although the magnitude of the effect seems to be slightly lower around 0.5 euro cents on average. Also in this case the estimates are highly statistically significant in all four econometric specifications.

We can then conclude that the policy has not benefited the category of the uninformed consumers that are now expected to pay a slightly higher price when refilling (randomly)

[61] We do test for the presence of first order autocorrelation in our data and we reject the hypothesis of no first order autocorrelation. The test is the Wooldridge test for autocorrelation in panel data (STATA command xtserial) and the test results are F(1, 69) = 478.603 and F(1, 69) = 560.166, respectively for unleaded and diesel fuel, implying rejection of the null hypothesis in both cases.

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Table 2.2: Effect on mean prices, Unleaded and Diesel fuel

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Method	FE	FE AR(1)	FE	DID 2 per.	FE	FE AR(1)	FE	DID 2 per.
	FE unl	FE unl	FE unl	2 unl	FE dies	FE dies	FE dies	2 dies
Dep. Var.		res p unl				res p dies		
period				-0.648***				-0.716***
				(-5.893)				(-6.139)
Group				1.055***				-0.250
				(4.147)				(-1.560)
TrEffect	0.556***	0.535***	0.556***	0.586***	0.488**	0.523***	0.487***	0.540***
	(3.120)	(5.790)	(3.080)	(3.337)	(2.583)	(5.536)	(2.615)	(2.847)
Constant	0.952***	-1.349***	0.292***	0.868***	0.428***	-1.043***	0.602***	1.102***
	(15.36)	(-6.210)	(3.579)	(5.309)	(6.969)	(-4.823)	(6.382)	(10.15)
Observations	9,959	9,851	9,959	212	9,959	9,851	9,959	212
R-squared	0.393			0.132	0.377			0.047
Number of id	108	108	108		108	108	108	
ID FE	YES	YES	NO	NO	YES	YES	NO	NO
Day FE	YES	YES	YES	NO	YES	YES	YES	NO
Display FE	NO	NO	YES	NO	NO	NO	YES	NO
robust SE	YES	NO	YES	NO	YES	NO	YES	NO
AR(1)	NO	YES	NO	NO	NO	YES	NO	NO
chi2 Haus	40.87				46.14			
df Haus	20				20			
p Haus	0.00387				0.000772			

t-statistics in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Dep. var. is the residual price (i.e. price - OPAL price)

in those stations that were affected by the price comparison policy. However, such a price increase has a marginal effect on the fuel expenditure of this category of consumers as an average refill of 40 liters is only 20 euro cents more expensive. The size of the price increase represents only the 0.4% (on a reference price of 1.3 euro per liter). In terms of annual fuel expenditure this might be reflected in an increase of around 20 euros for a typical consumers refilling her car twice a week[62].

As a robustness check we run the same regressions on two other samples. The first where we change the dependent variable to be the difference between the posted price and

[62] This most likely is an overestimate of the effect as it implies that all the refills take place on the highway. Refilling the car twice a week on the highway might be plausible for frequent users of the highway.

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the company specific suggested price, and the second where we reduce the time horizon of the study and we only select the stations for which we have all the daily price information (i.e. we construct a balanced panel). The results of these estimates confirm the above findings (see 2.10 and 2.11 Appendix A).

By looking at the average price we only test the effect of the policy on uninformed or inactive consumers. To say something about the other categories of consumers we do need to look at different measures of price. The other two categories of consumers are: 1) those that are informed both before and after the price comparison policy[63]; and 2) those that become informed only after the policy change. In table 2.3 we present the econometric results for a set of regressions in which the dependent variable is the minimum price asked by the stations belonging to the same submarket, where the submarkets are identified by those stations which prices are posted on the same display[64]. In this exercise we run two different types of regressions, one in which we have fixed-effects and errors clustered at the display level and one in which we impose an AR(1) error structure.

If we consider the minimum price asked in these submarkets we can estimate the effect of the policy on consumers that are informed both before and after the policy change. About this comparison we can see (column 1 and 2 for unleaded, and 2 and 3 for diesel fuel) that also this category of consumers has not benefited from the policy. The prices that consumers in this group pay are again slightly higher following the policy change The increase in the minimum prices are between 0.55 and 0.8 euro cents (depending on the fuel type and regression specification). These price increases imply an increase in fuel expenditure weakly higher than the one suffered by uninformed or inactive consumers[65].

To estimate the impact of the price comparison policy on those consumers that were not informed before the policy change and become informed only after the policy change we can

[63] These consumers are those that before the road side price comparison policy had been using the price comparison website to obtain price information.

[64] This is the case both for the stations in the treated and those in the control group. Also the stations in the control group are assigned to a display being affected by the same policy already from the year before.

[65] We also run similar regressions for the maximum price and for the difference between the maximum and the minimum price, again at the submarket level. In this regression we find that for unleaded fuel the price distribution seems to shift up by the same amount as the maximum price increase by roughly the same amount of the minimum price and the difference between the two is not significantly different. Differently for diesel fuel we do not find an increase in maximum price and accordingly the difference between the maximum and minimum price is marginally reduced. For the estimates see tables 2.12 and 2.13 Appendix. Notice that we cannot perform an analysis on the distribution of prices within the submarkets as we only have four stations in each submarket.

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Table 2.3: Effect on the (sub-markets) minimum prices, Unleaded and Diesel fuel

	(1)	(2)	(3)	(4)
	fe	xtregar	fe	xtregar
Measure	min p	min p	min p	min p
Dep. Var.	unleaded price		diesel price	
TrEffect	0.542*	0.706***	0.567**	0.893***
	(1.827)	(4.365)	(2.299)	(5.569)
Constant	135.3***	-3.828***	131.2***	-1.503***
	(871.9)	(-11.96)	(1,262)	(-5.038)
Observations	2,569	2,542	2,569	2,542
ID	27	27	27	27
ID FE	YES	YES	YES	YES
Day FE	YES	YES	YES	YES
chi2 Haus	12.20		4.327	
df Haus	4		4	
p Haus	0.0159		0.364	

t-statistics in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$

look, for each single submarkets, at the average difference between the average price, asked before the policy introduction, and the minimum price available after the policy introduction. It is indeed plausible to expect that before the policy all uninformed consumers on average paid the average submarket price. After the policy change those consumers that make use of the price comparison display can then refill at the lowest advertised price of that display.

The maximum saving available is then identified in this difference. Before the roadside price comparison policy, on average, in the affected submarkets consumers paid a premium over the OPAL price of 1.9 euro cents for unleaded and 0.85 euro cents for diesel fuel. After the policy introduction the average premium asked on the minimum submarket price is of 0.9 euro cents for unleaded and -0.17 euro cents for diesel fuel. This implies that for both fuels the maximum saving of the newly informed, or activated consumers, is around 1 euro cents per liter (with a standard error of 0.15 euro cents). This means that this type of consumers can save 40 euro cents on each 40 liter refill (i.e. the 0.77% of the price, for a reference price of 1.3 euro per liter) or achieve a saving of around 42 euro if they refill twice a week for a year.

Finally we can also compute the saving available to an uninformed/inactive consumer that after the policy change might consider to get informed. This measure is given by the

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difference of the average and minimum submarket prices at the same point in time, after the policy change. For both unleaded and diesel fuel this measure is around 0.8 euro cents per liter (with a standard error of 0.09 euro cents per liter). Hence, it is the prospect of saving around 32 euro cents for a 40 liter refill (i.e. the 0.6% of the price, for a reference price of 1.3 euro per liter) that should activate the uninformed consumers.

2.6 Conclusions

This paper studies the implementation of a price comparison policy implemented with the explicit aim to help consumers making informed choices and with the implicit aim to lower retail prices. The market we study is the Italian pay-toll highway refueling market. A market where we usually find a service area every 30-40 kilometers and in each service area there is only one fuel retailer. Retailers sell the same range of fuels (unleaded and diesel fuel) and fuel is considered a highly homogeneous good The market is highly concentrated as only eight brands operates in the considered market. Fuel is sold by final retailers which margins are relatively thin (around 4 euro cents per liter). Fuels prices are not advertised along the highway before entering the service area and consumers know fuel prices only after their decision to stop at the service area.

These features imply that retailers have little incentive to supply price information to consumers and consumers face relatively high search costs (mostly in terms of cost of time) that makes it optimal to just shop randomly. This clearly implies that before the implementation of the price transparency policy each station enjoyed relatively high market power on its local market. These features are partly reflected in the fact that highways fuel prices are in general higher than prices found on normal roads and that the price range (difference between maximum and minimum price) is only around 2 euro cents (per liter).

The price comparison policy we study is the deployment of roadside price comparison displays comparing the fuel prices of the next four consecutive stations. This policy was introduced on the main Italian pay-toll highway network in summer 2008. Before the policy introduction fuel prices were already available (and freely accessible) on the website of the highway network operator (ASPI). Hence it is plausible to assume that retailers had full price information even before the policy introduction. It is also plausible that some consumers were already making use of the price comparison website to obtain price information. Nevertheless, the introduction of the price comparison policy, in the form of roadside electronic displays, dramatically reduces the searching cost for all the consumers passing by the display. Moreover this policy makes almost simultaneous the supply of the price information and the

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decision to refill bringing relevant price information to the consumers exactly at the time when they might think to refill their cars.

The aim of this paper is to estimate the effect of the roadside price comparison policy on the final retail prices posted to consumers.

Theoretical studies that have looked at the issue of price transparency or that have more in general modelled consumers' search and purchasing decision provide contradicting prediction on the effect of such price comparison policy. Some models conclude that the provision of price information should lower average prices (for instance the models based on Varian (1980) and Salop and Stiglitz (1977)) while other models find ambiguous effects dependent on the market specific features (see Møllgaard and Overgaard (2006)[66], Waterson (2003)).

It has then been showed that the available theories have limits and are not suitable to predict or describe market outcomes, unless we resort to strong assumptions (Møllgaard and Overgaard (2006), Waterson (2003), Hosken et al. (2008)). Hence, the scope of the paper is to exploit the (exogenous) introduction of the price comparison policy to provide empirical evidence on the effects of price transparency on fuel prices in the Italian highway market.

It is indeed beyond the scope of this paper to link the empirical findings with the many theories proposed to study price transparency and fuel prices. Rather, the aim of this paper is to contribute to the empirical literature on fuel prices and price transparency and shed some light on the impact of price transparency policies applied to real (as opposed to online) markets.

To perform the empirical analysis we have collected a unique dataset with price information on 178 service stations (about 40% of total Italian highway service stations and 85% of ASPI service stations) operating on the ASPI network. The dataset is a panel as we have collected price information, for unleaded and diesel fuel (both in the self-service typology[67]), for a total of 255 days. The days are not consecutive (there are some gaps) and span from 26/10/2007 until 11/11/2008.

In the econometric analysis, we claim that we can exploit some features of the adopted implementation strategy to consistently estimate the causal effect of price information on retail prices. We claim that we can exploit the panel structure of the dataset, and the fact

[66] Møllgaard and Overgaard (2006) refer to Nilsson (1999), Møllgaard and Overgaard (2001), Møllgaard and Overgaard (1999), Schultz (2004), Schultz (2005).

[67] Usually stations offers both self and full service. However we restrict our analysis only to self service. Indeed, price comparison website and road side panels only advertise self service prices and full service prices are not advertised.

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that we observe the same stations before and after the policy change, to consistently estimate the effect of price information on fuel retail prices. Moreover, we can use the stations treated in 2007 (and not affected by the 2008 policy) to control for time varying factors that affect the pricing for highway fuel retailers but that are not related to the policy. Thus we control for time varying factors, exogenous to the policy, using two strategies: underlying fuel cost variations are accounted by differencing price levels with Opal reference price; and time varying industry specific factors are accounted for using the price variations in the control group.

In the empirical analysis we look at the effect of price comparison on four different measures as different price measures are informative of the effect of price comparison on different categories of consumers. We first look at average prices as this is the relevant price paid by uninformed consumers both before and after the policy change. We then look at the minimum price as this price is the relevant price when we consider consumers that are informed both before and after the policy change. Third, we look at the savings made by those consumers uninformed before the policy change that become informed after the introduction of the policy. Finally we estimate the potential savings that a consumer, still uninformed after the introduction of the policy, could make if she is to become informed. This last measure identifies the incentive to become informed that is in place after the policy change.

Table 2.4: Summary: Effects of price transparency on different price measures

Consumers type		Gain (Loss) (euro cents)		Gain (Loss) (%)	
before	after	unleaded	diesel	unleaded	diesel
uninformed	uninformed	(0.55)	(0.5)	(0.42%)	(0.38%)
informed	informed	(0.5-0.7)	(0.6-0.9)	(0.38% - 0.54%)	(0.46% - 0.7%)
uninformed	informed	1	1	0.77%	0.77%
uninformed	incentive to become informed	0.8	0.8	0.62%	0.62%

From the empirical analysis we find that the two types of consumers that do not change shopping attitude face slightly higher prices after the policy change where the price increase is around 0.5 euro cents per liter or the 0.4% of the reference retail price (assumed around 1.3 euro per liter). If we consider the yearly fuel expenditure this might be reflected in an increase of around 20 euro (for a consumer that refills her car twice a week, each time by 40 liters).

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Consumers that after the policy change become informed are marginally better off as the minimum price they learn with the displays is 1 euro cents per liter lower (or 0.77% cheaper for a reference price of 1.3 euro per liter) than the average price they paid before the policy change, when uninformed. This implies potential savings in the order of 42 euro a year following the above expenditure calculations.

Finally the price incentive to become informed is in the order of 0.8 euro cents per liter implying yearly potential savings of around 32 euro.

From the above analysis it is clear that the price comparison policy did not foster price competition among retailers as neither the average price nor the minimum price appears to be lowered after the price comparison policy. On the contrary these two price measures have slightly increased.

Admittedly the policy seems to benefit only one category of consumers notably the newly informed ones. Moreover the policy still seems to offer some incentives to uninformed consumers that are willing to become informed. Hence we could conclude that overall the policy do offer some savings to consumers. However the size of such savings seems to be extremely small compared to fuel expenditure.

The economic significance of these effects might gain importance if we compare the size of the above effects to the margin of retailers and to the observed price range. The maximum saving attainable is in the order of 1 euro cents per liter and this represent 50% of the average price range found in the highways posted prices (see section 2.4.2.1). Moreover, with average margins in the order of 4 euro cents per liter both the potential savings from using the display and the observed price increases gain economic relevance given the thin area within which prices might move.

It is difficult to reconcile these findings with the available theories. Nonetheless, it is clear that in this market the prediction that increasing price information should lead to lower levels of prices appears to be not consistent with the data.

The pricing decision of retailers seems to be almost unaffected by the policy. This might suggest that retailers are not adversely affected by the policy and keep their former pricing rules (although slightly raising their prices). This can be the case if consumers are not activated by the available savings and a relevant share of the consumers keeps refilling randomly.

We should then understand why consumers would not shop at the lowest available price since price information comes at no cost (there are only distraction costs). One explanation

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might be that the available savings are not enough to activate enough consumers. Since only a small share of consumers is activated the retailers might not find optimal to lower prices as to gain this demand. The result is that consumers do not make use of the displays and prices are not lowered.

Another explanation of these findings might be that the policy does not yet provide enough price information to consumers. We do find price comparison displays only every four stations. However between these stations drivers are free to enter and exit the highways if an exit/entrance gate is available. Although ASPI claims that the price comparison displays should capture the great majority of consumers it might be that the uninformed consumers in the system are still enough not to lead to lower prices.

In conclusion if the policy was expected to deliver savings up to 8 euro cents the policy clearly failed. However, this policy can actually help some consumers achieve some small saving in their fuel expenditure. Moreover, in a dynamic perspective, the policy might "teach" consumers about price comparison and its potential benefits and might prove successful in the future. The development and application of ICT innovations to price comparison in off-line real market might be an interesting area for consumer policy research.

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2.7 Appendix A: Tables

Table 2.5: Panel Location

Phase	id	Highway id	Km	Direction
Phase 1	disp01	A1 Milano-Napoli	9.30	south
	disp02	A1 Milano-Napoli	183.35	south
	disp03	A1 Milano-Napoli	307.55	south
	disp04	A1 Diramazione Roma sud - GRA	17.14	south
	disp05	A1 Milano-Napoli	748.75	north
	disp06	A1 Diramazione Roma nord - GRA	9.90	north
	disp07	A1 Milano-Napoli	259.70	north
	disp08	A1 Milano-Napoli	177.00	north
	disp09	A8 Milano-Varese	2.00	north
	disp10	A14 Bologna-Taranto	38.90	south
Phase 2	disp11	A1 Milano-Napoli	79.80	north
	disp12	A8 Milano-Varese	13.15	south
	disp13	A4 Torino-Trieste	19.20	est
	disp14	A13 Bologna-Padova	104.85	south
	disp15	A13 Bologna-Padova	0.82	north
	disp16	A14 Bologna-Taranto	95.60	north
	disp17	A1 Milano-Napoli	340.30	north
	disp18	A11 Firenze- Pisa Nord	0.15	west
	disp19	A11 Firenze- Pisa Nord	81.15	est
	disp20	A1 Milano-Napoli	432.07	north
	disp21	A1 Milano-Napoli	515.80	south
	disp22	A1 Milano-Napoli	516.00	north
	disp23	A16 Napoli- Canosa	2.58	est
	disp24	A16 Napoli- Canosa	53.45	west
	disp25	A14 Bologna-Taranto	498.15	north
	disp26	A14 Bologna-Taranto	468.20	south
	disp27	A14 Bologna-Taranto	411.77	north
	disp28	A14 Bologna-Taranto	359.75	south
	disp29	A14 Bologna-Taranto	293.57	north
	disp30	A16 Napoli- Canosa	131.60	est
	disp31	A16 Napoli- Canosa	158.46	west
	disp32	A14 Bologna-Taranto	664.00	south
	disp33	A14 Bologna-Taranto	703.56	north
	disp34	A14 Bologna-Taranto	555.43	south

Source: Autostrade per l'Italia

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Table 2.6: Relation between day of the week and price change

Day	Obs.	Price Increase		Price Reduction		No Price Change
		N.	mean(€ cents)	N.	mean(€ cents)	N.
Monday	1320	113	0.014	46	-0.008	1161
		(9%)		(3%)		(88%)
Tuesday	1320	149	0.010	110	-0.006	1061
		(11%)		(8%)		(80%)
Wednesday	1320	185	0.008	112	-0.007	1023
		(14%)		(8%)		(78%)
Thursday	1320	152	0.011	49	-0.006	1119
		(12%)		(4%)		(85%)
Friday	1320	81	0.013	68	-0.008	1171
		(6%)		(5%)		(89%)
Saturday	1320	124	0.011	141	-0.007	1055
		(9%)		(11%)		(80%)
Sunday	1320	19	0.007	98	-0.014	1203
		(1%)		(7%)		(91%)
total	9240	823		624		7793
		(9%)		(7%)		(84%)

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Table 2.7: Price Differentials

Source	Fuel	Service	Road	Range (€ cents) (Max-Min)	Avg. Save (€) 40 liters refill	Period
ISTAT [1]	Unleaded	self	non-toll	4.23	1.69	10/07-10/08
ISTAT	Diesel	self	non-toll	4.71	1.88	10/07-10/08
ISTAT	Unleaded	full	non-toll	3.50	1.40	10/07-10/08
ISTAT	Diesel	full	non-toll	3.88	1.55	10/07-10/08
FIGISC[2]	Unleaded	full	non-toll	11.33	4.53	19/02/2008
FIGISC	Diesel	full	non-toll	13.05	5.22	19/02/2008
PrezziBenzina[3]	Unleaded	self	non-toll	5.40	2.16	20/11/2008
PrezziBenzina	Diesel	self	non-toll	6.10	2.44	20/11/2008
A22[4]	Unleaded	self	toll	1.60	0.64	28/04/2008
A22	Diesel	self	toll	1.60	0.64	28/04/2008
A22	Unleaded	full	toll	1.70	0.68	28/04/2008
A22	Diesel	full	toll	1.60	0.64	28/04/2008
ASPI[5]	Unleaded	self	toll	1.93	0.77	06/02/2008
ASPI	Diesel	self	toll	1.65	0.66	06/02/2008

[1] ISTAT (Italian National Institute of Statistics) monthly price collection;

[2] FIGISC (Italian Federation of the Refilling Service Station Managers) La distribuzione carburanti: libro bianco sulla concorrenza, 3/4/2008

[3] A website were registered users can self report fuel prices (www.prezzibenzina.it)

[4] Pay toll motorway (313 Km long) managed by Autostrada del Brennero SpA. Publish weekly price report on www.autobrennero.it

[5] Sub sample of ASPI service stations

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Table 2.8: The Service Stations Sample

outcome var.	Mean[1]	Std. Dev.	outcome var.	Mean[1]	Std. Dev.
Phase 1	0.21	0.41	Phase 2	0.62	0.49
Agip	0.27	0.45	A1	0.33	0.47
Api	0.04	0.19	A8	0.03	0.18
Erg	0.07	0.25	A11	0.03	0.18
Esso	0.16	0.37	A12	0.02	0.15
Ip	0.00	0.00	A13	0.04	0.21
Kuwait	0.08	0.28	A14	0.30	0.46
Shell	0.06	0.23	A16	0.07	0.25
Som	0.01	0.07	A26	0.06	0.24
Tamoil	0.20	0.40	A27	0.02	0.15
Total	0.12	0.33	A30	0.02	0.15
light transit	17737	11162	South	0.21	0.41
heavy transit	5541	3127	Centre	0.43	0.50
total transit	23274	13768	North	0.37	0.48

[1]Averages over 178 service stations on ASPI network

Table 2.9: Price Comparison Motorway Price Vs Normal Street

	Unleaded (euro cents)				Diesel (euro cents)			
	Normal road[1]		Highway[2]		Normal road		Highway	
Province	mean	s.e.	mean	s.e.	mean	s.e.	mean	s.e.
Bologna	1.177	0.005	1.194	0.004	1.171	0.005	1.188	0.004
Firenze	1.174	0.005	1.190	0.007	1.166	0.005	1.183	0.008
Milano	1.166	0.003	1.193	0.004	1.159	0.003	1.186	0.004
Napoli	1.235	0.009	1.240	0.005	1.209	0.014	1.208	0.006
Roma	1.173	0.002	1.188	0.004	1.165	0.002	1.178	0.003

[1]Mean computed over the twenty cheapest service stations (at the province level).

Data are self reported by users on the website www.prezzibenzina.it

[2]Mean computed at the province level for the service stations in our sample operating on the ASPI highway

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Table 2.10: Effect on mean prices, Unleaded and Diesel fuel, Full sample, Suggested

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Method	FE	FE AR(1)	FE	DID 2 per.	FE	FE AR(1)	FE	DID 2 per.
	FE unl	FE unl	FE unl	2 unl	FE dies	FE dies	FE dies	2 dies
Dep. Var.	res p unl				res p dies			
period				0.337***				0.359***
				(3.309)				(3.732)
Group				1.051***				-0.235
				(4.185)				(-1.595)
TrEffect	0.576***	0.551***	0.577***	0.600***	0.529***	0.579***	0.529***	0.564***
	(3.530)	(6.440)	(3.379)	(3.718)	(3.150)	(6.562)	(2.939)	(3.341)
Constant	-0.750***	-1.226***	-1.354***	-1.586***	-1.604***	-0.930***	-1.376***	-1.900***
	(-12.73)	(-4.463)	(-17.38)	(-10.12)	(-24.72)	(-3.544)	(-14.66)	(-20.11)
Observations	9,959	9,851	9,959	212	9,959	9,851	9,959	212
R-squared	0.206			0.182	0.199			0.195
Number of id	108	108	108		108	108	108	
ID FE	YES	YES	NO	NO	YES	YES	NO	NO
Day FE	YES	YES	YES	NO	YES	YES	YES	NO
Display FE	NO	NO	YES	NO	NO	NO	YES	NO
robust SE	YES	NO	YES	NO	YES	NO	YES	NO
AR(1)	NO	YES	NO	NO	NO	YES	NO	NO
chi2 Haus	40.85				50.81			
df Haus	20				20			
p Haus	0.00389				0.000170			

Robust t-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

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Table 2.11: Effect on mean prices, Unleaded and Diesel fuel, Reduced Sample, OPAL

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Method	FE	FE AR(1)	FE	DID 2 per.	FE	FE AR(1)	FE	DID 2 per.
	FE unl	FE unl	FE unl	2 unl	FE dies	FE dies	FE dies	2 dies
Dep. Var.		res p unl				res p dies		
period				0.324***				0.470***
				(3.439)				(4.190)
Group				0.892***				-0.313*
				(3.067)				(-1.721)
TrEffect	0.398**	0.380***	0.398***	0.438***	0.405*	0.405***	0.405**	0.454**
	(2.424)	(3.779)	(2.828)	(2.796)	(1.948)	(3.671)	(2.208)	(2.415)
Constant	3.952***	1.922***	3.387***	-1.593***	2.914***	0.695***	3.093***	-1.902***
	(26.18)	(52.71)	(24.66)	(-9.362)	(21.40)	(18.27)	(23.41)	(-18.41)
Observations	4,278	4,185	4,278	186	4,278	4,185	4,278	186
R-squared	0.540			0.145	0.497			0.168
Number of id	93	93	93		93	93	93	
ID FE	YES	YES	NO	NO	YES	YES	NO	NO
Day FE	YES	YES	YES	NO	YES	YES	YES	NO
Display FE	NO	NO	YES	NO	NO	NO	YES	NO
robust SE	YES	NO	YES	NO	YES	NO	YES	NO
AR(1)	NO	YES	NO	NO	NO	YES	NO	NO
df Haus	9.930				2.663			
p Haus	1				1			
pH	0.00163				0.103			

Robust t-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

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Table 2.12: Effect on the (sub-markets) maximum prices, Unleaded and Diesel fuel

	(1)	(2)	(3)	(4)
	fe	xtregar	fe	xtregar
Measure	max p	max p	max p	max p
Dep. Var.	unleaded price		diesel price	
TrEffect	0.642**	0.476***	0.256	0.208
	(2.428)	(3.152)	(0.925)	(1.292)
Constant	136.6***	-2.073***	132.5***	0.563
	(1,407)	(-5.291)	(798.4)	(1.335)
Observations	2,569	2,542	2,569	2,542
ID	27	27	27	27
ID FE	YES	YES	YES	YES
Day FE	YES	YES	YES	YES
chi2 Haus	12.63		2.709	
df Haus	4		4	
p Haus	0.0132		0.608	

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.13: Price Range: Maximum - Minimum price

	(1)	(2)	(3)	(4)
Method	FE	FE AR(1)	FE	FE AR(1)
Dep. Var.	diff p unl		diff p dies	
TrEffect	0.0997	-0.145	-0.311	-0.561***
	(0.329)	(-0.766)	(-1.355)	(-2.845)
Constant	1.338***	1.813***	1.276***	2.139***
	(9.076)	(3.333)	(6.473)	(3.716)
Observations	2,569	2,542	2,569	2,542
R-squared	0.311	27	0.286	27
ID	27	27	27	27
ID FE	YES	YES	YES	YES
Day FE	YES	YES	YES	YES

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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2.8 Appendix B: Figures

Figure 2.7: Price Comparison Display (source: www.autostrade.it)

Figure 2.8: Example of Display and Stations Location

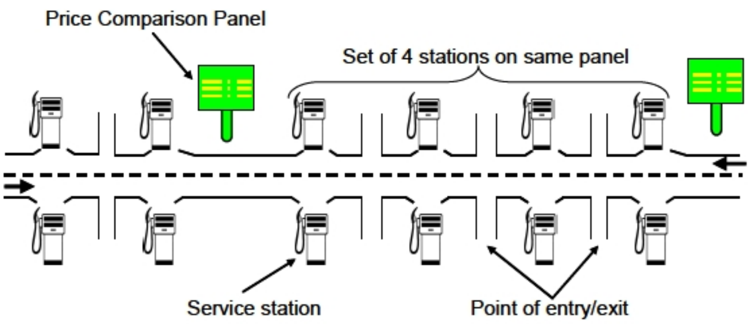

Figure 2.7 shows the design of price comparison displays installed along the ASPI highway network. As we can see from the figure, the display post the brand and prices (self service unleaded and diesel fuel) of four consecutive stations. The stations are ranked by distance to the display (with the closest being first) and the cheapest station is highlighted by a green dot next to its price. ASPI officials, when enquired about the display design, reported that the decision to post only four prices is an outcome of a trade-off between posting many prices (as to offer more information, but with come physical constraints) and assuring a minimum level of comparison among brands. Since on the ASPI network there is a maximum of three consecutive stations all from the same brand they then adopted the 4 stations design. Both in the design stage and in the location decision the managers of the fuel stations were not involved. They are only responsible for the communication of the prices through the

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software platform (that serves both the price comparison website and for the price comparison display).

Figure 2.9: Italian toll-motorway network (source: www.autostrade.it)

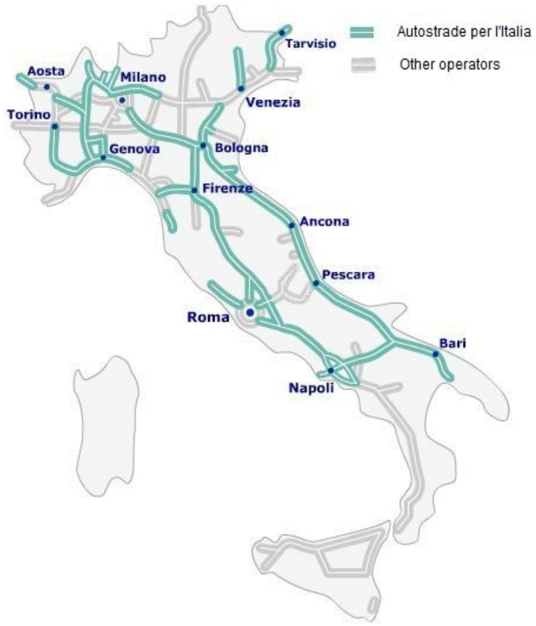

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Figure 2.10: Screenshot ASPI price comparison website (source: www.autostrade.it)

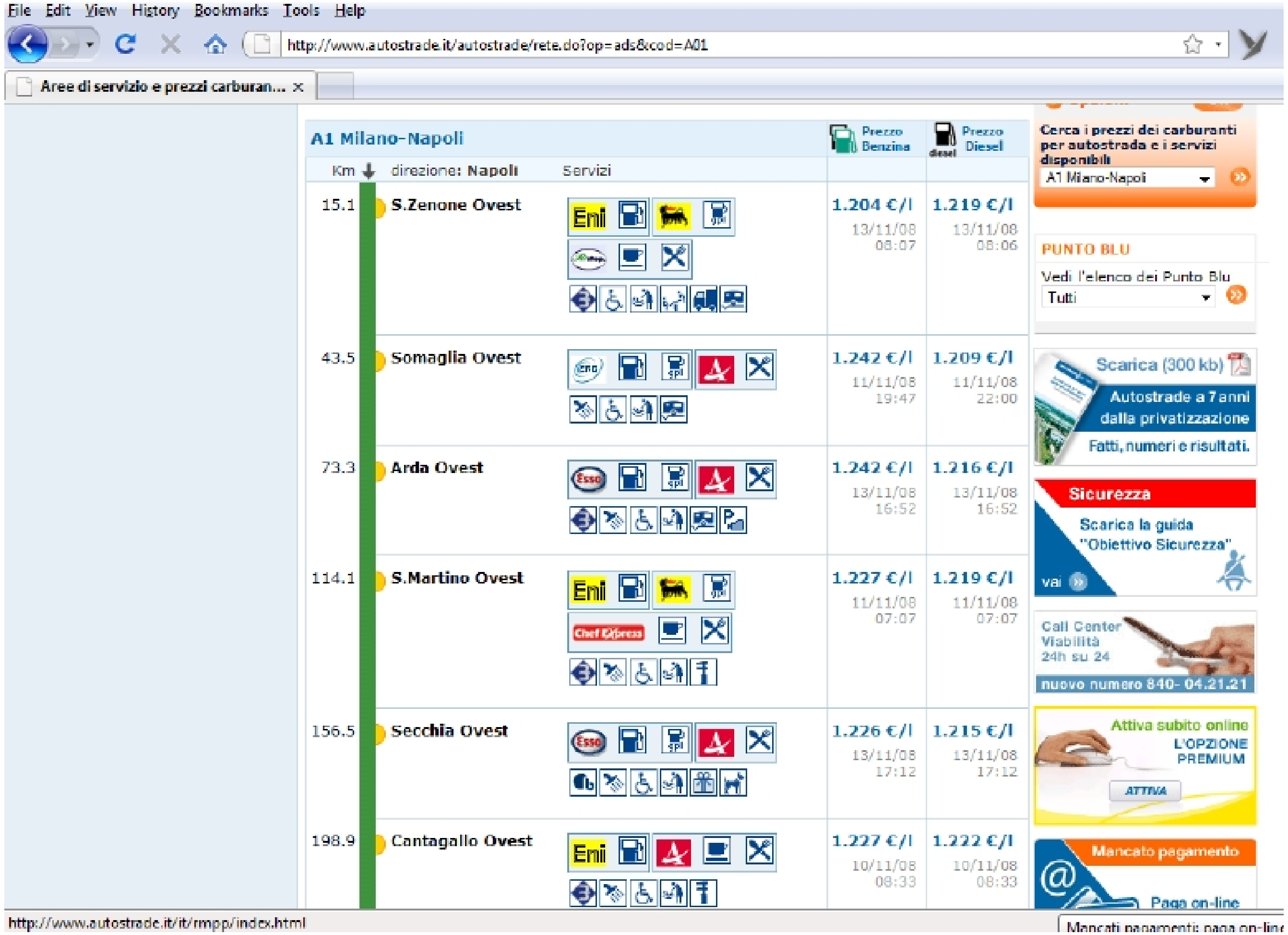

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Figure 2.11: Time spent in each quartile of the regional price distribution (unleaded fuel)

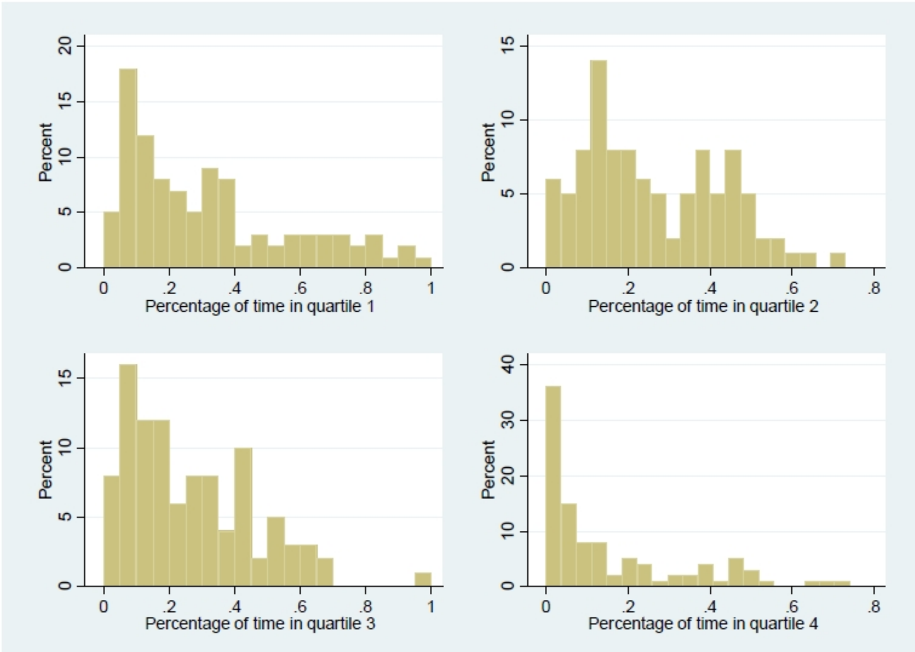

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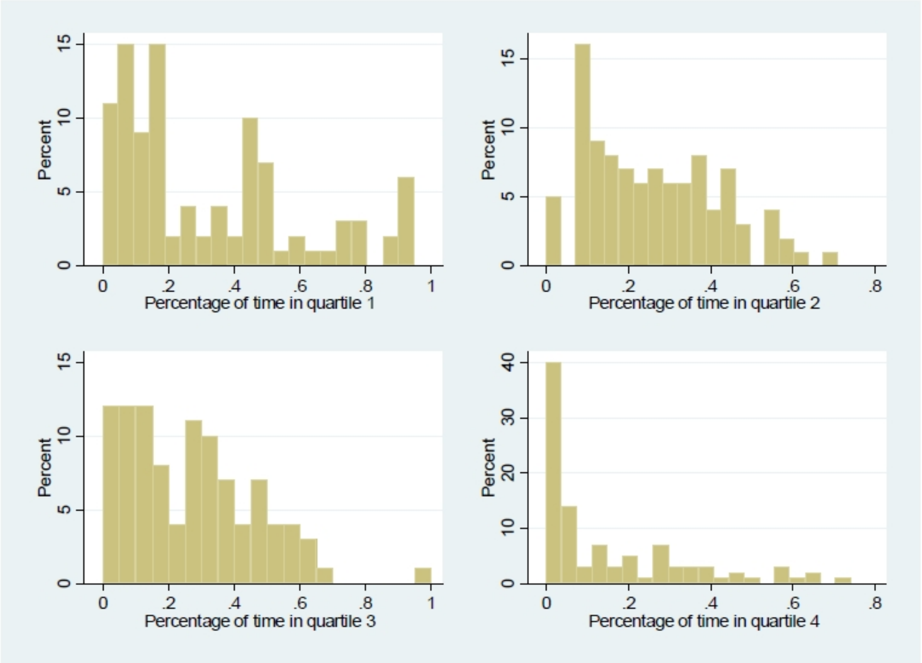
Figure 2.12: Time spent in each quartile of the regional price distribution (diesel fuel)



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2.9 Appendix C: Identification strategy and robustness checks

In this paper we study the price comparison policy introduced in the Italian highway refueling market in summer 2008 (affecting around 70 refueling stations) and we adopt a difference in differences (DID) approach to estimate the average treatment effect on the treated (ATT).

The econometric analysis takes as the unit of interest the single refueling station, builds a treatment and control group, and exploits the time dimension of the dataset to estimate the effect of the policy.

The difference in differences approach let us control for two distinct sources of unobserved heterogeneity, that might otherwise bias our estimates. Firstly, given we observe the same stations over time we can control for time invariant unobserved individual effects (both in the treatment and control group). Secondly, through a control group we can also estimate, and difference out, unobserved time effects common to both the treatment and control group.

The employed difference in differences approach is admittedly not conventional. Indeed, while the treatment group is conventional, as it includes refueling stations that change their treatment status over time (not treated before summer 2008 and treated after summer 2008) the control group is not conventional. Indeed, differently from conventional DID studies the control groups is not made up of *truly untreated* stations. The control group consists of refueling stations that were themselves treated but at an earlier stage, summer 2007 (38 stations). Nevertheless, it is worth noting that the treatment status does not change among the control group in the time period considered for the econometric analysis. Control group stations receive the treatment in summer 2007 whereas the econometric analysis only considers the period between April 2008 and November 2008[68]. Indeed, over the all period that concerns the econometric analysis the control group stations remain equally treated by the policy received in summer 2007.

The choice of this control group comes at the cost of a further assumption in addition to the standard assumptions usually made in DID studies. The assumption is that the effect of the summer 2007 policy is time invariant. Notice that a similar assumption is also usually made in standard DID studies where the estimated effect of the policy is usually taken to be constant over time. Indeed, for the sake of this study we make exactly the same assumption about the effect of the 2008 price comparison policy that we take to be constant over time.

[68] This allows for the summer 2007 policy to be fully "absorbed" by affected retailers and reduce the risk of introducing in the sample periods of time in which the response to the policy varied.

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The intuition is that as long as the effect of the summer 2007 policy is constant this effect is just a fixed time invariant effect that adds up to the stations' fixed effects and that is differenced out in the first difference of the DID approach.

In the following we offer a more formal discussion of this identification approach resorting to a potential outcome exercise (the following follows the exposition in Angrist and Pischke (2009)).

Adopting a potential outcome approach we have that the prices asked by retailers are:

$$p_{0igt} = \text{the price asked by retailer } i \text{ at time } t \text{ in absence of price comparison} \quad (2.3)$$

$$p_{1igt} = \text{the price asked by retailer } i \text{ at time } t \text{ with price comparison active} \quad (2.4)$$

Moreover we assume that in absence of any treatment the expected price asked by retailers is additive and given by :

$$E(p_{0igt}|g,t) = \overline{\alpha}_g + \gamma D_t \quad (2.5)$$

Where g represents the group dummy with $g = 1$ identifying the treatment group and $g = 0$ the control group. The above equation says that without any policy change the expected price in a given group is given by a time effect γ (in case $D_i = 1$) and a common group effect $\overline{\alpha}_g$. Notice that the common group effect $\overline{\alpha}_g$ can be estimated as the average of the stations' fixed effects $\overline{\alpha}_g = \frac{1}{N_g}\sum_{i=1}^{N_g}\alpha_i$ for all $i \in (G = g)$ (where N_g is the number of stations in a given group). Also, for the stations treated in summer 2007 we can decompose the stations' fixed effects in two components, a common part due to the 2007 policy and a fixed effect specific to each station (i.e. $\alpha_i = \alpha_i^* + \lambda$) such that the common group component can be decomposed in the following way: $\overline{\alpha}_0 = \frac{1}{N_0}\sum_{i=1}^{N_0}\alpha_i = \lambda + \frac{1}{N_0}\sum_{i=1}^{N_0}\alpha_i^* = \lambda + \overline{\alpha}_0^*$.

Finally, we assume that the causal effect of the policy $E(p_{1igt} - p_{0igt}|g,t)$[69] is constant and equal to δ.

Given the above assumptions the single stations prices can be represented by the following equation:

[69] This is a hypothetical difference as for each individual station at any given point in time we can only observe one outcome.

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$$p_{igt} = \alpha_i + \gamma D_t + \delta TrEff_{igt} + \varepsilon_{igt} \tag{2.6}$$

where p_{igt} is the price asked by retailer i at time t[70], α_i is the station specific fixed effect, D_t is a time dummy that takes the value of 1 after the policy change (summer 2008), $TrEff_{it}$ is the interaction effect that takes the value of 1 only for those stations that are affected by the price comparison policy after the policy change, and takes the value of 0 in all the other cases, and ε_{igt} is the error term.

Assuming that $E(\ \varepsilon_{igt}|g,t) = 0$ we can compute the following first differences:

$$E(p_{igt}|g=0, D_t=1) - E(p_{igt}|g=0, D_t=0) = \overline{\alpha}_o^* + \lambda + \gamma - (\overline{\alpha}_o^* + \lambda) = \gamma \tag{2.7}$$

$$E(p_{igt}|g=1, D_t=1) - E(p_{igt}|g=1, D_t=0) = \overline{\alpha}_1 + \gamma + \delta - \overline{\alpha}_1 = \gamma + \delta \tag{2.8}$$

From the above it follows that the difference in differences would then take out the common time effect and isolate the causal effect of the 2008 policy change δ.

Hence, as long as the parameter λ is constant and time invariant this is differenced out in the first difference together with the station fixed effects.

On the other hand, the effect of the treatment received in 2007 (among control group stations) might be correlated with unobserved time varying variables. This implies that the common trend assumption fails to hold and the previous estimate is biased.

Suppose that the time effect is group specific such that γ_0 is the control group time effect and γ_1 is the treatment group time effect. Then we would obtain that the ATT is biased with the bias given by the difference between the two time trends.

$$ATT = \delta + (\gamma_1 - \gamma_0) \tag{2.9}$$

However, given we have more than three periods, we can estimate the two time trends (and the parameter δ) to assess the extent of the bias (this is a robustness check that also Angrist and Pischke (2009) suggest to perform).

Table 2.14 presents the results of a model in which we allow for two different time trends. From the regression results we can conclude that the estimates of the ATT are similar

[70] Prices are detrended to account for oil price variations using the OPAL italian reference price. The OPAL reference price is a daily average of italian retail fuel prices. We use the diesel and unleaded reference price.

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to the ones found in models with the common time trend, suggesting a slight increase in prices around 0.5 euro cents per liter. For unleaded prices we do find that both trends are positive and statistically significant, nonetheless we do not reject the hypothesis that the two parameters are equal. For diesel prices we do find that the coefficients associated to the two trends are not statistically different from zero.

As a robustness check we also perform a simpler before and after (BA) estimation in order to identify the ATT. The BA estimation is only performed on the treatment group, where the dependent variable is the detrended fuel price (prices are detrended using the Italian OPAL reference price). In this BA framework we also allow for stations fixed effects to control for time invariant station specific characteristics.

Table 2.14 presents the results of the BA estimation. Both for unleaded and diesel fuel we estimate a positive an statistically significant ATT, confirming the finding of a price increase after the price comparison policy. However, the size of the coefficients are about twice the size of the coefficients estimated in the DID analysis.

The findings of the BA estimation are consistent with the results of the DID analysis. Indeed, the DID analysis shows that both the treatment and the control group are subject to a positive trend in their prices. Hence the simpler BA estimation overestimates the effect of the policy.

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Table 2.14: Robustness checks

VARIABLES	(1) DID unl unl	(2) DID dies dies	(3) BA unl unl	(4) BA dies dies
TrEffect	0.432**	0.548**	0.879***	0.923***
	(2.251)	(2.091)	(7.687)	(7.509)
dayt0	0.00495**	0.00171		
	(2.589)	(1.246)		
dayt1	0.00731***	0.000577		
	(3.143)	(0.220)		
Constant	0.945***	0.428***	-0.478	-2.091
	(14.76)	(6.852)	(-2.172)	(-19.61)
Observations	9,959	9,959	4800	4800
R-squared	0.393	0.377	0.15	0.11
Number of id	108	108	50	50
ID FE	YES	YES	YES	YES

Robust t-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

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CHAPTER 3

DOES PRICE COMPARISON MATTER? AN ACE ANALYSIS OF HIGHWAY REFUELING

Abstract

Price comparison is a market remedy often introduced to lower consumer search cost and, in turn, intensify price competition among retailers. However, theoretical and empirical evidence is not conclusive on the effects of such policy. This paper studies the introduction of a major price comparison policy on the Italian highway refueling market. We design an agent-based computational economics (ACE) model of the highway refueling market. We then use the ACE model as a testing platform on which introducing price comparison and derive predictions on the likely effect of the policy. The model predicts that the introduction of price comparison has a limited effect on market prices. Consumers that make use of price comparison might save around 0.5 euro cents per liter. However, price competition among retailers is only marginally fostered, as the price asked by stations falls, on average, by only 0.17 euro cents. These results are consistent with the available empirical findings.

3.1 Introduction

Lately, the study and design of remedies in consumer markets has received a wide attention by economists. This follows the recognition that consumers' behavior significantly impact the competitive performance of industries (Waterson (2003)). Consequently, there has been growing attention toward policies designed to empower consumers and to facilitate consumers' decisions (for a survey on consumers market remedies see Garrod et al. (2008)). Hence, two disciplines that were quite apart in the past, consumer policy and competition policy, converged significantly (Armstrong (2008)) and, in Armstrong's opinion, this has to do with the fact that "behavioral economics" is more and more informing the way economists think.[1].

[1] For a survey on the empirical evidence supporting behavioral economics see DellaVigna (2009). Also antitrust authorities are considering the impact of behavioral economics on competition policy for instance

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This paper studies price comparison, a common consumer market remedy that aims to reduce consumer search cost by providing price information. This remedy received special attention during the last decade as the use of price comparison is widespread in online markets, where we can find price comparison websites for almost any service or product (for a review on price comparison see Garrod et al. (2008)). However price comparison is not a peculiarity of the internet as it can also be applied to brick and mortars markets[2].

The price comparison policy we study was introduced in year 2007, in the Italian highway refueling market. The policy consisted in the deployment of road-side price comparison electronic displays[3]. The declared objective of the policy was to provide effective price comparison and foster consumer search and competition among retailers[4]. The implicit objective of the policy was to lower fuel prices[5].

To study the introduction of price comparison we propose an agent based computational economics (ACE) model where autonomous consumers and retailers give rise to market interactions. We then introduce the price comparison policy and study the resulting market outcomes to derive insights on the likely impact on consumers and retailers.

The designed ACE model, as any other model, is an abstraction of reality. We do not aim to design an ACE model that represents all the details and features of the highway gasoline market. Rather, we model the salient institutional details to resemble the Italian highway refueling market features and the channels that relate price information to purchasing and pricing decisions.

This paper contributes to the theoretical literature on competition and consumer policy by offering a model that incorporates three important elements:1) dynamic interactions

see Bennett et al. (2010) and Armstrong and Huck (2010).

[2] An example are price comparison sections within magazines. Moreover, advancement in information technologies have made the gathering and distribution of price information increasingly easier and cost effective opening new posibilities to price comparison (think for instance to price comparison delivered directly to mobile phones before visiting a brick and mortar shop).

[3] Displays are positioned on the side of the highway and they post and compare fuel prices of the successive four refueling stations. We refer to these price comparison devices either as "panels" or "displays". For an explanatory picture of one display see Figure 3.8 Appendix A.

[4] The policy was introduced to comply with Law 2 April 2007 n.40 that required highway concessionaires to introduce remedies to increase price information and to ease price comparison.

[5] Before the introduction of the policy along the highway there was no price information. Drivers had no means to choose retailers based on prices and retailers could enjoy local monopolies. Price comparison was then expected to undermine retailers' market power and lead to lower prices.

Consumers' association CODACONS expected savings up to 8 euro cents per litre or 6% of the starting price (ANSA news 17 July 2007).

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between consumers and retailers; 2) behavioral decision making in consumers and retailers; and 3) heterogeneous consumers and retailers.

The empirical evidence (see Chapter 2) suggests that the price comparison policy did not foster competition among retailers. Posted fuel prices do not decrease after the introduction of price comparison. Nevertheless, the findings suggest that consumers activated by the policy might save an average of 1 euro cents (per liter) if they are to use the price comparison displays to make informed purchasing decisions. Hence, although the policy does not foster price competition among retailers the presence of price comparison can still be beneficial to some categories of consumers, those activated by the policy. The benefits to final consumers appear nevertheless minimal[6].

Current economic models fall short of providing explanations on the effect of the policy studied in this paper (for a discussion see section 2.2, in Chapter 2). Moreover, in the considered setting there are some key elements that increase the complexity of the task.

First of all, given the linear structure of the highway, retailers face very different levels of demand and different shares of potentially activated consumers[7]. Also, given the complexity of the setting, it is unrealistic to postulate that retailers know the share of active *vs* inactive consumers[8] and form their strategies based on that. It is more plausible to assume that retailers will learn over time what types of consumers they are facing. Models that do not account for these features might fail to consider important elements that are likely to drive the results.

The objective of the paper is to use the ACE model firstly to explain and rationalize the empirical results of Chapter 2 and secondly to explore the role of ACE modelling in guiding policy design[9].

[6] A consumer that refuels her car once a week (40 litres) might save around 20 euros each year.

[7] Given the linear structure of the highway not all highway segment have the same potential demand. Also, the first refueling station after the price comparison display face the highest share of activated consumers while the fourth station after the display face the lowest share of activated consumers. This is an important issue considering the fact that stations are usually at 40 km distance and the average length of trips is 80 km.

[8] Activated consumers are those consumers that after the introduction of the policy make informed purchasing decisions and select the lowest price retailer. On the other hand, inactive consumers are consumers that either do not pay attention to the price comparison display or cannot use the price comparison information (for instance the lowest price retailer is not along their journey or they enter and exit the highway without encountering any price comparison display).

[9] For a recent paper on the use of ACE models for economic policy design see Dawid and Neugart (2011).

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Accordingly we perform two simulation exercises: In the first simulation (*Policy Impact Simulation*) we change some key consumers and retailers parameters to study the relationship between these model parameters and the estimated policy effects. In the second simulation (*Policy Design Simulation*) we increase the density of price comparison display to assess to what extent higher information dissemination might increase policy effects.

The *Policy Impact Simulation* finds that retailers' prices are only marginally affected by the introduction of the policy. On average we find only a small reduction in average posted prices of 0.17 euro cents per liter. However, consistent with the empirical findings, we find that drivers that are activated by the policy experience higher savings that, on average, amount to 0.5 euro cents per liter. As expected, we also find that, as we increase the share of drivers that are activated by the policy, the gain for consumers increases, as prices are further reduced. However, even in cases in which a high share of consumers are activated (75% of consumers) the maximum savings implied by the policy amount to only 1.5 euro cents per liter.

The *Policy Design Simulation* suggests that by increasing the density of price comparison displays the average effect of the policy improves for consumers. The average price paid by activated consumers decreases by a further 0.3 euro cents whereas the average posted price decreases by a further 0.14 euro cents.

The paper proceeds as follows: Section 3.2 briefly discusses some related literature; Section 3.3 describes the policy change (3.3.1) and the refueling market under study (3.3.2 and 3.3.3); Section 3.4 outlines the agent based computational economics model; Section 3.5 presents the simulation exercise and its results; and Section 3.7 draws some conclusions.

3.2 Related literature

This paper is directly related to the literature that studies the relationship between price transparency and competition and to the literature that applies agent based models to study retail markets.

Price comparison is a price transparency policy often implemented in consumers markets. The relationship between price transparency, market competition and consumers policy has been object of several theoretical and empirical studies (for three recent surveys see Garrod et al. (2008), SCA (2006), Armstrong (2008)).

The suggestion to use ACE models to inform consumer policy desing and policy assessment can also be found in a discussion paper of the UK Office for Fair Trading (OFT) OFT (2009).

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In general, this literature concludes that increased price transparency on the firm's side unambiguously facilitates oligopolistic coordinations while increased price transparency on the consumer's side is not necessarily beneficial to consumers. Overall, given the interplay of consumer's side and firm's side effects, this literature concludes that the effect of price transparency is ambiguous and dependent on the market specific features (Møllgaard and Overgaard (2006)[10], Waterson (2003)). This view is somehow in contrast with the conclusions drawn from traditional models that look at the issue of price dispersion. These models postulate that increased price transparency leads to lower average prices. The classical examples are: 1) the Varian (1980)[11] model where both the average price paid by informed consumers and the (higher) average price paid by uninformed consumers decrease as the share of informed consumers increases; and 2) the Salop, Stiglitz model (Salop and Stiglitz (1977)) where the average price in the market falls as the search cost decreases.

Also, about price comparison websites there is a vast empirical literature that finds that online price comparison offers sizable savings although high price dispersions remains a pervasive feature (among these papers Smith and Brynjolfsson (2001), Ellison and Ellison (2009), Baye et al. (2004), Morton et al. (2001), Brown and Goolsbee (2002)).

Other studies in the agent based literature that are mostly close to this paper are the works of Waldeck and Darmon (2006), Kirman and Vriend (2001) and Heppenstall et al. (2006). Waldeck and Darmon (2006) consider a game similar to the one in Varian (1980). Similar to Varian they derive a Nash Search Equilibrium (NSE) and compare it to the pricing of adaptive sellers that use a reinforcement learning (RL) algorithm. They find that when there is Bertrand competition the reinforcement learning price distribution converges to the NSE price distribution. Also, they find that when there is a change in buyer's search behavior the average price and the variance implied by RL pricing exhibit variations that

[10] Møllgaard and Overgaard (2006) refer to Nilsson (1999), Møllgaard and Overgaard (2001), Møllgaard and Overgaard (1999), Schultz (2004), Schultz (2005).

[11] Some recent papers based on the Varian (1980) model draw conclusions on the effect of increased price information (Morgan et al. (2006), Waldeck (2008), and Lach and Moraga-González (2009)). The paper by Morgan et al. find that as the share of informed consumers exogenously increase, keeping fixed the number of firms, the expected price paid by both informed and uniformed consumers decreases. In the other paper Lach and Moraga-González introduce an increased heterogeneity in consumers types that results in more realistic (bell-shaped) price distribution. They conclude that policies aimed at increasing the amount of price information can affect the distribution of prices and welfare and that the magnitude of the effects vary depending on the shopping behavior of consumers. Waldeck (2008) establishes that the link between information and pricing is not trivial. He also finds an inverse-U shaped relationship between price dispersion and the share of informed consumers. Then he finds that higher price information might lead to higher price dispersion and that more intensive search by active consumers might lead to higher average posted prices.

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are consistent with the variations found in the NSE. In our paper we use a reinforcement learning algorithm that is similar to the one proposed by Waldeck and Darmon (2006).

Kirman and Vriend (2001) build an agent based model of the fish market in Marseille, France. They model adaptive sellers and buyers and their model is capable to explain the two main stylized facts of the Marseille fish market: relatively high price dispersion and high loyalty.

Finally, Heppenstall et al. (2006) model retail gasoline market in the UK with an hybrid model that combines agent based elements on the sellers side and a spatial integrated model on the consumer side. Their model is able to reproduce the spatial differences seen in the real market (higher prices in rural areas as opposed to lower prices in urban areas). Also their model is consistent with the "rocket and feathers" behavior often found in the gasoline price literature.

3.3 The policy and market framework

3.3.1 The price comparison policy

This section briefly describes the details of the price comparison policy under study.

In early 2007 the Italian government, through a decree-law[12], committed to foster competition and increase consumers protection in some sensitive markets, among which the refueling market[13]. Some months later, the parliament approved the government's decree and turned it into a law (Law 40/7 henceforth[14]). For what concerns the intervention on the refueling market, the objectives of the legislator are clearly stated in the law (article 2) and are the following: (1) foster competition, and (2) price transparency; (3) guarantee an adequate level of knowledge about cost of service, and (4) facilitate the comparison of alternative offers.

Autostrade per l'Italia (henceforth ASPI), the largest Italian pay-toll highway concessionaire, decided to implement the proposed price information measures before the 2007 summer holiday period (taking place usually in August, when millions of drivers use toll-highways to reach their holiday destination).

In practice, what ASPI did during the months between April and July 2007 was: 1) To

[12]Decree Law 31st January 2007 n.7

[13]Other markets considered were: Fixed Line Phone and Mobile Phone, Internet Services, Car Insurance, Mortgages, Airline Tariffs, and "best before date" in food products.

[14]Law 2 April 2007 n.40.

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create a software platform for stations[15] managers to communicate, in real time, the fuel prices offered at their premises; 2) Post the recorded prices in an apposite section within the ASPI website[16] (Figure 3.10, Appendix A); 3) Finally, they started to install physical road-side price comparison display [17] (like the one in figure 3.8 and 3.9 Appendix A) along their highway network.

Hence, two price comparison policies were adopted:1) Price comparison website; and 2) Physical roadside price comparison display. Although both policies offer the same type of informative content they differ under some dimensions. On the one hand, the comparison website is accessible only through an internet connection (thus there is no simultaneity between information delivery and purchase[18]), it lists all the stations on the ASPI network, and it still entails some positive search costs[19]. On the other hand, the roadside comparison displays only list the next four consecutive stations, they are freely available to everyone (driving next to them), they entail almost no search cost[20], and the acquisition of price information and the purchase decision can be potentially simultaneous[21]. Given these characteristics we assume that the latter measure (the roadside displays) has the highest potential to effectively disseminate price information and provide price comparison among stations that are close substitutes[22].

3.3.2 The highway refueling market

This section provides some information about the Italian highway gasoline market. It is beyond the scope of this paper to model each detail and characteristic of the market. Rather, in our agent based model we aim to represent and capture the salient characteristic

[15] We use the words "station", "refueling station", "service station", "retailer" , "filling station" interchangeably to refer to a facility that offer the refueling service.

[16] Prices are posted online at www.autostrade.it and are catalogued by highway code, kilometer and direction.

[17] We use the words "physical" or "road" comparison panel to refer to a tangible price comparison device installed next to the roadway.

[18] Although recent development in mobile technologies make it possible to browse the web also on the move.

[19] Be aware of the service, cost of the connection, time to open the browser, locate the desired highway and set of possible refilling stations, etc...

[20] There could be an attention cost. To process the information on the panel the consumer has to divert some of her cognitive ability from driving to the acquisition of the price information. Still we assume this cost is a fraction of the cost required to access the online version.

[21] The stations listed on the panel are usually within a distance of 2 to 100 km.

[22] The products sold by the different retailers are seen as substitutes, this is even more true when we consider highway traffic that is generally less "loyal" than local/urban traffic.

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and aspects that play a key role in the relationships of interested (i.e. price information, purchasing decision and pricing decisions). In our model we only consider retailers and consumers and we remain agnostic about the role of the players that we do not explicitly model (oil companies, highway concessionaire). Still this section offers an overview of the market as to guide the interpretation of the model and the evaluation of the findings.

We study the Italian highway refueling market and more precisely the price competition between retailers located along the highways. In Italy there are more than 6500 Km of pay-toll highways and although this network only accounts for the 2% of the national road surfaces, its accounts for about 25% of the national transportations needs[23]. ASPI is the main pay-toll highway concessionaire and has concession for roughly 3000 Km (almost half of the entire national network). Its network covers almost the entire country with the exception of very few regions[24]. On the Italian highways there are more than 450 service stations (of which 210 on the ASPI network) that sell the same range of fuel products (typically: unleaded, premium unleaded, diesel, premium diesel). The stations operating on the highways represent only the 2% of the total service stations active nationwide, however they supply more than 10% of total fuel (respectively 6% for unleaded and 15% for diesel fuel). By volume the most sold fuel on the highway is diesel that accounts for more than 75% of total fuel supply[25]

The range of fuels sold by each filling station is considered homogeneous. That is, within each category of fuel, products offered by different brands are qualitative the same, the only differences that might arise come from brand differentiation not related to the quality of the fuel (e.g. advertisement, corporate social responsibility, loyalty programme).

There are eight major brands that operate on the Italian highways: AGIP, ESSO, ERG, SHELL, Q8, TOTAL, API/IP, TAMOIL (differently from the ordinary roads on highways there are very few "independent" retailers[26]). All these competitors are vertically integrated firms that are active at every stage from the production to the distribution process. For what concerns the end market they can all rely on an extensive network of service stations distributed all over the country. Such stations can be directly owned by the oil companies or given in concession to third parties that own and manage them.

[23] Source AISCAT Association of Italian Highway Concessionaire, www.aiscat.it

[24] The regions are Sardinia, Sicily, Calabria, and Trentino Alto Adige.

[25] Unione Petrolifera, Notizie 4/2006, and Notizie 5/2007, www.unionepetrolifera.it .

[26] Usually known as "pompe bianche" (white pumps). These independent retailers buy fuel at the wholesale market, directly from refineries. They are usually characterized by very low expenditure in marketing or branding and are popular for offering lower prices or discounts.

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Of importance for our modelling exercise is that the price setting usually happens in two stages. In the first stage the oil company indicates to the station manager a *suggested price*. At the second stage the station manager can discretionally change that price, within a range imposed by the oil company. This range is implicitly determined by two contractual conditions: the lower bound is given by the price at which the station manager buys the fuel (assuming they do not sell at a loss); the upper bound is usually a ceiling on the price the station manager can practice (usually determined by the oil company in relation to the *suggested price*).

3.3.3 Highway use and customer attitude to refueling

This section provides some evidence about the average use of the highway and customers attitude to self service refueling. During the year 2007 the average distance travelled on the ASPI network was 80 Km long. Respectively 75 Km for light weight vehicles (that represent the 80% of total transits) and 99.7 Km for heavy weight vehicles. During the year the traffic level seems to be quite constant (except for a peak of light weight travels in August). A substantial share of trips, 1/3 for light and 1/4 for heavy weight vehicles, is less than 25 Km long and these trips are mainly concentrated around the metropolitan areas on both inbound and outbound directions[27].

For what concerns drivers refueling habits we can draw some information from two surveys published by ACI (the Italian Automotive Club)[28]. These studies report that about 40% of drivers favours self service refueling always or often while another 33% opts for the self service only occasionally. The lower price of the self service seems to be determinant in the choice of the service for about 45% of the respondent whereas the rest favoured it for its flexibility. The same surveys also provide some information about customer loyalty and choice of the service stations. They report that proximity and lower price are the two key determinants for the choice of the refueling station. In the urban area most of the drivers always refuel from the same station whereas when outside the urban area there is no fidelity to the single service station but there is some level of brand loyalty.

[27] Data come from two surveys: Autostrade per l'Italia 2007, Conference Presentation "Estate 2007. "Via libera in sicurezza "; and Autostrade per L'Italia 2008, Conference Presentation "La via per l'estate. Le vacanze iniziano in autostrada".

[28] ACI, Rapporto Annuale 2002 e Rapporto Annuale 2008.

3.4 An ACE model of highway refueling

This section describes the agent-based model of the highway refueling market. The purpose of the model is to study the determinants of price competition among retailers, to offer insights on the interpretation of the empirical results and to study the implications for policy design.

The model is built and run on the software Netlogo[29] and is made up of four main objects: 1) the road network, the space on which agents live, move, interact and vanish (die); 2) the refueling stations (retailers); 3) the drivers (consumers); 4) the price comparison displays.

Each type of agent has some group specific decision rules and each agent, within the same type, makes use of the same decision rules (this does not imply that all agents take the same decisions as decision rules are state dependent). Nevertheless, agents of the same type differ in their state variables.(e.g. for drivers: fuel tank, level of fuel; for retailers: suggested price, efficiency).

The underlying time dimension is divided into days that in turn are divided in steps. During each step of the day agents move and interact and once all the daily steps are concluded some *end of the day* accountings takes place and, then, the new day starts.

The model is run for a total of 1000 days. The model always start with price comparison displays turned off. These are then turned on from day 501 through day 1000.

The following subsections describe in detail the four objects of the model.

3.4.1 Road network

The road network is the environment on which agents are created and located, on which agents move, interact, conclude their transactions and finally vanish. The road network is designed to resemble a possible simplified highway road segment. Notice that the model only considers the highway environment and does not model the ordinary road environment, which is necessarily linked to the highway. Figure 3.1 depicts a type of road segment used in the model (other road settings can be found in Figure 3.11 Appendix A). In these settings the environment is made up of the following objects. An highway (the straight black vertical line) on which drivers can drive in two directions (north $\rightarrow$ south and south $\rightarrow$ north). Then, there are various *gates* from which consumers can enter or exit the highway. Finally, at the two ends of the straight line there are two other gates that represent the connections of the studied segments with the other contiguous segments of the highway.

[29] NetLogo itself: Wilensky, U. 1999. NetLogo. http://ccl.northwestern.edu/netlogo/. Center for Connected Learning and Computer-Based Modeling, Northwestern University. Evanston, IL.

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Drivers generated on these latter gates represent the drivers that continue their trips from the contiguous segments while drivers that vanish at these gates are those drivers that continue their trips on the contiguous segments. Drivers generated at these two gates can either travel along the entire segment and vanish at the opposite end or vanish at one of the urban gates along the segment.

At the other nine gates, represented by the small squares on the east side of the highway road, are the urban entry/exit points that connect the highway to the ordinary road network. Also consumers entering the highway at these points can either move north or south. Again, drivers generated at these gates can either travel along the segment to reach and vanish at one of the ends or vanish at one of the other gates along the segment

The road network is made up of squares (*patches in NetLogo)* and each square is taken to have a nominal length of 6.6 Km. Drivers drive on the right side of the road. The total length of the highway segment is then taken to be 320 Km. Given that the average distance travelled on the highway is around 75 to 100 Km we take this segment to be representative.

The road network also identifies the places on which we find refueling stations and price comparison displays. There are eight pairs of station areas (one area for each direction). The areas for stations are located at the midpoint between two different gates.

Stations are located at six patches of distance and that in turn represents a distance of around 40 Km that is the average distance between refueling areas we find in the real market. The map also identifies the locations for the roadside price comparison displays.

For example this deployment settings depicts the introduction of two price comparison displays, one for the north and one for the south direction, and displays are located around mid-point of the highway segment.

This is not the only network we could implement as we could have varied the distance between stations or varied the number of gates. Also, we could have made the model more sophisticated for instance by modeling the normal road network. Brought to an extreme we could have also used a real road network on which running our model. However, since we look for predictions that we can generalize and we believe that the exact road design does not impact on the final results we decided to implement the simplest possible road network but that still incorporates all the relevant characteristics of a typical highway segment.

3.4.2 Retailers: refueling stations

In our model the sellers are the refueling stations. Stations are located on the service areas defined in the road network. There is only one retailer for each service area. Also,

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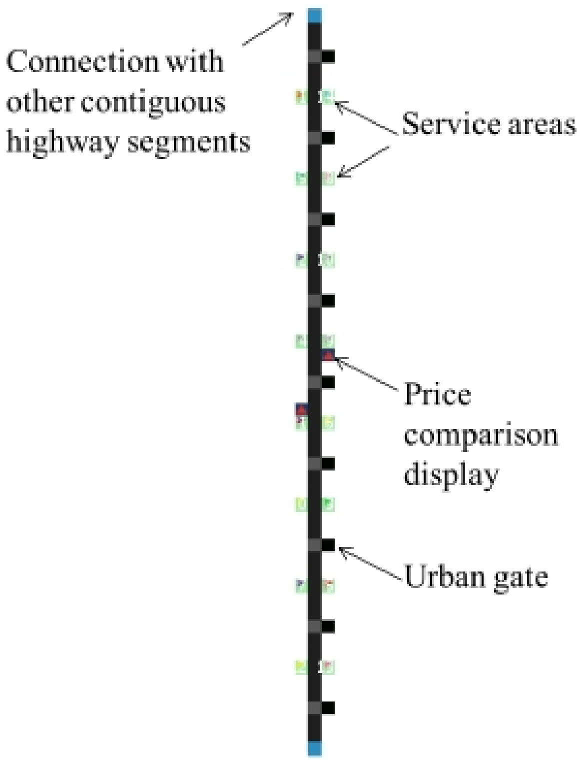
Figure 3.1: The road network



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there is only one type of fuel (all cars run on the same fuel) and stations are the only supplier of fuel in the market. In our model each station is an independent entity and there is no brand affiliation. Stations have unlimited capacity to serve customers and independently set their prices (we do not model coordination).

Retailers choose prices within a given range, although the range and levels can change from station to station. For each station is drawn a *suggested price* (on average 100 euro cents per liter and such price remain constant and does not change over time[30]). Then for each station is also drawn a maximum mark-up on the suggested price (on average 1.2 euro cents per liter[31]). A minimum price is also drawn and is given by the suggested price less a random component with an of average 3 euro cents per liter[32].

The wholesale cost of fuel is then derived by applying a discount, on average -1.5 euro cents per liter, on the minimum price. Given this modelling the average maximum margin that a retailer can realize is around 4 euro cents per liter and this figure is consistent with the maximum retailer margin found in the market.

The heterogeneity in stations is given by the actual realizations of their suggested price (that capture brand positioning) and the specific realizations for the maximum and minimum price and wholesale cost that capture differences in stations efficiency or stations contractual power (for instance a station with a very low realization for the wholesale cost represent a highly efficient station or a station with high contracting power).

Given the above defined price range retailers can adopt four pricing strategies: 1) highly undercut competitors' prices, by 2 euro cents; 2)slightly undercut competitor's prices, by 1 euro cents; 3) match the average competitors' price; 4) price at their own maximum price. We decided that the minimum undercut had to be of 1 euro cents as to be "visible" to drivers.

For each station the set of competitors is given by the closest two stations (the closest to the north and to the south), in case no price comparison display is active (this set span an area of 80 Km). In case of an active price comparison display, if the station is subject to the display (the station is *treated*) then the set of competitors is given by all the other stations treated by the same display otherwise the previous rule applies.

To decide which price strategy to adopt stations follow algorithm explained below.

[30] Hence we do not account for fluctuations in the oil price for instance.

[31] The maximum differential of 1.2 euro cents per liter is consistent with the differential found in the agreements between retailers and the oil companies in Italy.

[32] Also the figure of 3 euro cents is consistent with the agreements between retailers and oil companies.

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3.4.2.1 Price setting

Each service station decides autonomously its price following the same decision process. Stations start with a random rule (strategy) and then, after an evaluation period (first three weeks of the model), make their price setting decisions once a week. Once the new price is decided the stations keep it fixed for the following week and only at the end of the week, depending on the rule, on weekly profits and on past profits, might decide to change price strategy.

Stations set their prices following a reinforcement learning (RL) algorithm. The algorithm we use is similar to the one implemented by Waldeck and Darmon (2006). Stations can choose between four different price rules (the four rules seen above) and any rule can (potentially) be chosen at any stage. Each rule is characterized by a *fitness* value. Where for fitness we mean the ability of the rule to fulfill stations' objective (i.e. maximize profits). When the model is initialized each rule r (for each stations i) starts with the same fitness (as in eq. 3.1, where $start\_profits$ are the median profits realized in the first weeks of evaluation). As rules are chosen, by stations, their fitness is updated accordingly to eq. 3.2. Thus if at day t rule r is played, the fitness of rule r become a weighted average of the previous fitness, given by that rule, and the current profits. The weight is given by the parameter α that can be though of as the "memory" of the retailer.

$$F_0^{r,i} = F0(start\_profits)\ \forall r, i \tag{3.1}$$

$$F_{t=t+1}^{r,i} = (1-\alpha)F_t^{r,i} + \alpha\pi_t \quad \text{with } \pi_t = (p_t - c)Q_t \tag{3.2}$$

Stations attach a probability to each of their rule depending on their fitness. Rules are thus selected through a trial and error process in which stations tend to select and play more often those rules that returned higher payoffs.

As in Waldeck and Darmon (2006) the probability that rule r is played in period t by station i is given by:

$$prob\ (play\ r)_t^i = \frac{\exp(\frac{\overline{F}^r}{\tau})}{\sum\limits_{r=1}^{R}\exp(\frac{\overline{F}^r}{\tau})} \text{ with } \tau > 0 \tag{3.3}$$

where $\overline{F}^r$ is the normalized fitness defined as: $\overline{F}^r = \frac{F^r - F^{\min}}{F^{\max} - F^{\min}}$; and τ is called the *temperature* parameter where for $\tau \to 0$ stations tend to play only the rules that returned the highest profits and for $\tau \to \infty$ stations randomly pick rules.

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In our implementation of RL we consider two types of profits[33]. In a first implementation we consider normal profits as given by the difference between retail price and wholesale cost multiplied by the quantity of fuel sold. In another specification, we correct this profit measure by the fluctuation in the potential demand. Indeed only looking at profits might offer a very noisy picture as demand does not change only on the basis of the asked price but also depends on the fluctuations in the potential demand (drivers on the highway). In the second measure a station's manager get a noisy signal about the amount of drivers passing by the station's premise and considers fluctuation in demands when comparing profits[34].

3.4.3 Consumers: drivers

Drivers are the consumers of our model. Each driver is endowed with a car (cars are characterized by a fuel tank and an efficiency level (fuel consumptions)). We distinguish between three types of drivers: active, very active and captive. The three categories should represent the three types of consumers affected by the policy. The very active are those consumers that are highly price sensitive and even in the absence of the displays make use of the price comparison website and are always informed about all prices. The active consumers are those consumers that are *activated* by the policy and use only the road-side display to gather price information. The captive consumers are those that do not use the price information and keeps unchanged their refueling habits.

3.4.3.1 Refueling decision

In our model on average, every day, there are 1500 drivers that are created and might potentially be in need of refueling (around 500 do refuel their cars every day). Active drivers are a fixed fraction of total drivers and very active drivers are a fixed fraction of active drivers. Anytime a driver stops to refuel she fills up to tank capacity. All three types of drivers have two common refueling strategies. 1) Emergency: if they see their low fuel warning light on they stop at the first refueling stations. 2) Normal: each driver has a level of fuel after which she consider to refuel (around 1/3 of the tank), once this level is reached, randomly some drivers, even if active or very active, might stop at a random station and refuel[35].

[33] When we refer to profits we always refer to *(price - wholesale cost of fuel) x quantity.* Hence we do not inlcude other fixed or variable costs.

[34] We believe this is a plausible and realistic assumption. When demand vary the comparison of weekly profits is not trivial. For instance if after a week of high profits there is a week of low profits the station manager needs to understand if the low profits are due to the price chosen or to the lower potential demand.

[35] This account for those stops which main purpose is not refilling the car but rather using other services of the service area (food and drinks, or physical needs for instance). Given that a driver has already stopped

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Active and very active drivers differ from captive ones for a third strategy. When they reach the level at which they consider refueling (1/3 of the tank) they process the acquired price information and pick the station with the lowest price that is within their autonomy and travelling direction. Then they will only shop at this station. Notice that not only highway stations are considered. Active drivers also know the prices they might find outside the highway and if the price they know is lower than the one available on the highway, and their autonomy lets them reach their preferred stations, then they won't refuel on the highway.

3.4.4 Price comparison displays

Price comparison displays can be installed along the highway and, when turned on, send price information to active consumers. Displays always send price information about the next four successive service stations. Displays are also direction sensitive and send price information only to those drivers going in the direction facing the front of the display. In the model we test three different implementation settings of the policy that vary in the density of displays (see Figure 3.11 Appendix A for the three maps). The first implementation introduces only two price comparison displays affecting half of the stations (8 stations out of 16). The second introduces four displays affecting all the stations in the model (although a single station can be on maximum one display). The last setting introduces a display for each highway gate to reach the maximum possible price information coverage. A total of 16 displays are installed and necessarily the same station is covered by more than one display.

3.4.5 Key parameters

The ACE model we design necessarily includes many parameters that remain invariant along the simulation. For a full list of the parameters and their possible values we refer to the software code (see Appendix B). In this section we highlight and briefly describe what we consider the key parameters of the model (Table 3.1). Some of these key parameters are kept fixed in the simulation while other vary. By doing so on the one hand we perform a sensitivity test of the estimates and, on the other hand, we also investigate the impact of some key parameters on the model's behavior and predictions.

for other reasons might well decide to refill instead of losing other time.

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Table 3.1: Key model parameters

Parameter	Description	Value(s)
memory		0.5; 0.75
temp	reinforcement learning parameters	0.1; 0.05
F0		3
EvalWeeks	# of weeks in which the initial profits are evalued	3
week length	# of days in a simulated week	7
AvgCars	average number of drivers generated in each step of the model	300
RL-share-based	Reinforcement learning algorithm based on a noisy signal about # of customer as fraction of drivers passing by the stations. Otherwise the RL algorithm is only based on the profits made by the refueling station	On; Off
very-p-stve	share of very active consumers	0.3; 0.15
p-stve-t	share of active consumers	0.1; 0.25; 0.5; 0.75
fuel-concern	treshold at which drivers start considering refilling options	0.33
emergency-fuel	threshold of the emergency fuel level	0.1
t-red-bias	Share of drivers that enter form the two ends of the highways. Otherwise drivers enter form the gates along the way	0.4
per-exit	Percentage of drivers that exit at each gate	0.2

The first three parameters in Table 3.1 enter the reinforcement learning algorithm and are explained in section 3.4.2.1. We set the parameter $F0$ at the value of 3 such that the initial fitness of all rule is set to a relatively high value (i.e. three times the value of the median profits realized during the evaluation period). This ensure that at least during the first stages retailers tend to explore all the available rules. The *memory* parameter enters the equation of the RL fitness function. We model two different levels of *memory*, a first setting in which *memory* is set equal to 0.5 (equal weight between current profits and past profits) and a second setting in which *memory* is set equal to 0.75 (current profits are valued more than the past realizations). The *temp* parameter (*temperature* of the RL algorithm) is also varied. It takes the values of 0.1 and 0.05.where the former value implies a tendency to vary more frequently the price strategy and the latter value imply a tendency to settle for the strategy(ies) that delivers the highest payoffs.

EvalWeeks is set equal to 3 and represents the number of weeks in which the model is run to estimate the starting profits (during this period retailers keep fixed and randomly

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assigned price strategy). The week (*weeklength*) has the standard length of 7 days (although the model allows for shorter or longer week length). Notice that retailers make their pricing decisions at the beginning of each week, so by shortening the week length retailers will consider more often the opportunity to change price strategy.

AvgCars is set to 300 and represents the average number of cars that are generated in each cycle of the model (cars are generated with a random Poisson process). In each day there are five cycles in which cars are generated and the average numbers of cars created in each day is around 1800.

RL-share-based identifies the type of information retailers use to assess their weekly sales. We distinguish between two cases. In the first case retailers only look at their weekly profits. However this measure might be very noisy as the fuel sold in one station depends on the price of the station, on the average size of the tank of the driver, on the average fuel level that drivers have when refueling and on the size of the potential demand that varies randomly each week. To overcome this possible issue we also propose a second method to assess the efficacy of the chosen rule. We assume that retailers can observe a noisy signal of their potential customer base (just by looking at the number of cars passing by the station's premise). Hence, in this second case, instead of profits, retailers will consider the share of cars refueling at their premises times the margin they earn as a share of costs.

The two parameters *p-stve-t* and *very-p-stve* control the share of active drivers and the share of very active drivers. Active drivers are those drivers that will make use of the price display, if they see one turned on along their way, whereas very active drivers are a subset of active drivers and are informed of all prices since they use the price comparison website. For these two parameters we do an extensive analysis and vary them in the simulation exercise.

Fuel-concern and *emergency-fuel* set the thresholds (in terms of the size of the fuel tank) at which drivers start considering refueling and at which drivers are "desperate" to refuel (as to avoid running out of fuel). The fuel tank capacity is a random number between 35 and 55 liters.

The last two parameters *t-red-bias* and *per-exit* determine where cars are been created. There are two options, cars can be created at the two ends of the highway or at the urban intersections. The former mimic the travel of drivers coming from other highway segments while the latter mimic the persistent entrance of cars at each urban segments. The parameter

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t-red-bias identify the share of cars that are created at the two ends of the highway (if *t-red-bias=1* no cars enter at the urban intersections), and the parameter *per-exit* controls the share of cars that exits at each urban intersection.

3.5 Simulations

The aim of this paper is to study the introduction of the price comparison policy. The outcome of interest is the fuel price paid by the different categories of consumers (captive, active and very active) and the purpose is to estimate the effect, on retail prices..

To estimate the effect of the policy we use a regression framework where we take the simulated data and we estimate a before and after OLS regression to estimate the impact of the policy. This approach replicates what a researcher could do with observational data in a standard treatment evaluation exercise. The advantage in our setting comes from the fact that we use experimental data and we can ignore many issues arising in non-experimental treatment evaluation studies.

For each single simulation run, we can estimate the *policy impact*. If the policy impact is not significantly different from zero the policy has no statistically significant effect on consumers' prices otherwise, if the impact is statistically different from zero, we allow for positive (lower prices) or negative (higher prices) effects of the policy.

Once we estimate the policy impact, for each run of the model, we can then look at the distribution of the policy impact for a fixed parameter choice. Also, we can investigate the relationship between the policy impact and the chosen model parameter choice. This can be done by regressing the estimated policy impact using the chosen parameters as regressors.

Other than the parameter choice, the ACE model we propose depends also on pure random factors. By varying the random seed of the model we also test the sensitivity of our results to the random realizations of the model.

In the first simulation (*Policy Impact Simulation*) we investigate the role of the following key parameters: *memory* (2); *temp* (2); *RL-share-based* (2); *very-p-stve* (2); *p-stve-t* (4) (the numbers in parentheses indicate the different realizations considered, for the values taken by these parameters please refer to Table 3.1). Given the introduced variation in these parameters we obtain a total of 64 model variants that are each run for 10 times changing the underlying random seeds. This implies a total of 640 runs. The analysis of the results is then performed in a 2-step regression framework where we first estimate, for each run, the policy impact. Then, we regress the estimated 640 policy impacts on the model parameters.

The results of these last regressions shed light on the relationships between the effects of the policy and the simulated characteristics of the market.

The second simulation (*Policy Design Simulation*) looks in detail at the impact of the display settings (the display density) on the estimated policy effect. The aim is to study the relationship between different display deployment settings and the resulting policy effects. The three different settings vary in the density of price comparison displays introduced in the market. With a higher density of displays there is higher information diffusion and higher inter-relations between retailers (the price of the same station is posted in more than one display and hence is compared against more stations). Indeed each station can be compared to a minimum of three stations, in case of minimum display density, to a maximum of six stations in case of maximum density. This last exercise should highlight the trade-offs between providing more price information (that inevitably entails higher costs) and the effect on prices. Together with the three display settings we also vary two key consumer parameters: *very-p-stve* (2); *p-stve-t* (4). The total number of model variants is then 24 and for each of these specifications we run the model 10 times with different underlying random seeds. We then investigate the generated outcomes with the same regression framework highlighted above.

3.6 Results

Before presenting the results we briefly present the *macro* evolution of the key variables that capture the behavior of the simulated model.

Figure 3.2 shows the evolution of the average posted price, between day 0 and day 1000, for a typical simulation run. Stations enter the simulation with a randomly assigned price, hence the average price at the beginning of the simulation reflects this randomness and the average price is around 100 euro cents per liter with a price range (difference between maximum and minimum price) of 4 euro cents. The figure tells that stations learn relatively quickly to increase prices and the average price during the first 100 days of simulation increase by 2 euro cents as most of the stations start to price near their capped maximum price.

The average price then fluctuates always around these high levels as only occasional price dips seem to appear. At day 500 of the simulation the price comparison policy is turned on and retailers can react to it. The figure suggests that, for this run, the policy has no clear effect as the average price line does not seem to exhibit structural breaks in proximity of the policy change.

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Figure 3.2: Evolution of average price (all stations)

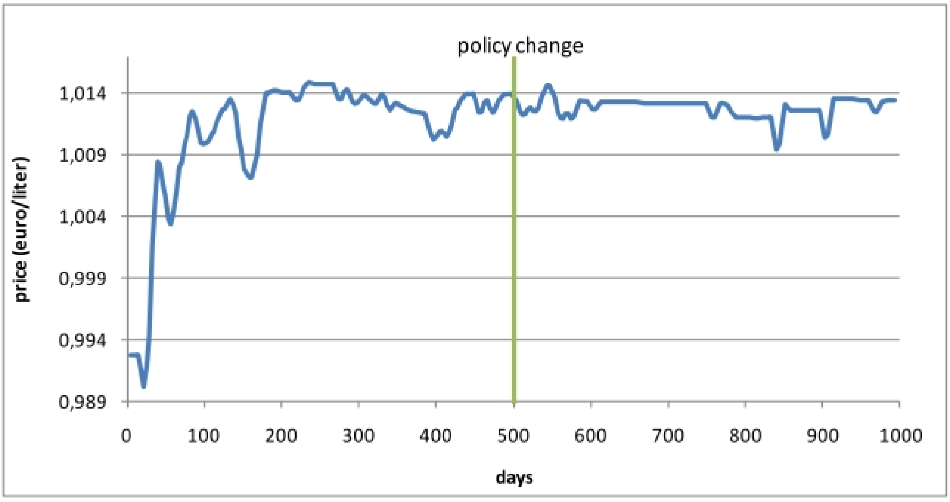

Figure 3.3 shows instead the average price paid by the different categories of consumers. During the first half of the simulation we can only distinguish between two types of consumers: very active and captive. Indeed during this first phase only very active consumers have access to price information.

Accordingly the price paid by this latter category of consumer is the lowest from the start of the simulation. Only after the introduction of the roadside price comparison display we can distinguish between three different types of consumers. Where, in additions to the previous two categories we now have active consumers.

During the second half of the simulation we have that captive consumers keep paying the highest price while very active consumers pay the lowest price and active consumers pay a price in between. Notice that very active consumers are expected to always pay a lower price than active ones as they have full information about all prices and can therefore better plan their fuel refills at the cheapest station, choosing from a higher set of stations.

We can also look at the evolution of the types of rules that stations decide to play. Figure 3.4 shows the share of stations that play the same rule in a given day. The figure tells that more than 50% of stations play the fourth rule (set price equal to maximum price). A share between 30 and 40% of stations select the third rule (match the average of competitors' prices) and the rest of stations select the undercutting pricing rule (Rule 2 undercuts by 1 euro cents while Rule 1 undercuts by 2 euro cents).

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Figure 3.3: Average price paid by consumers

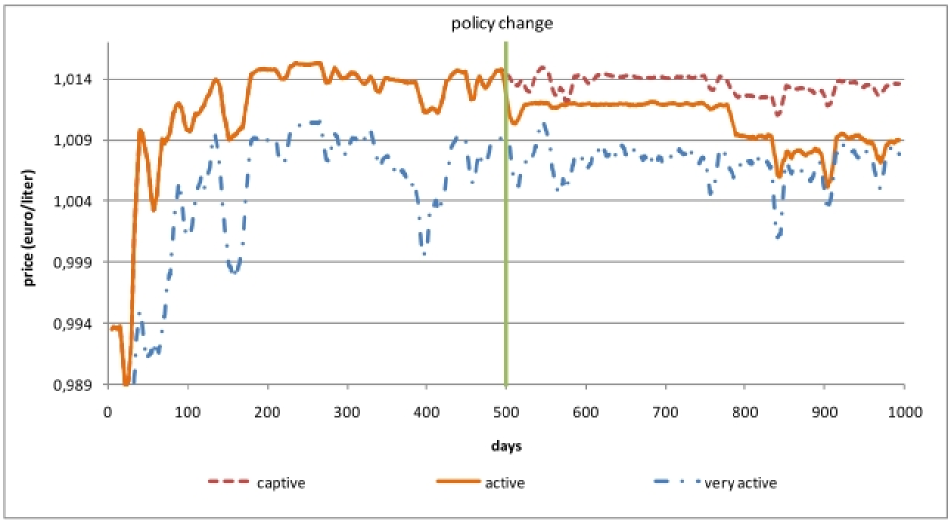
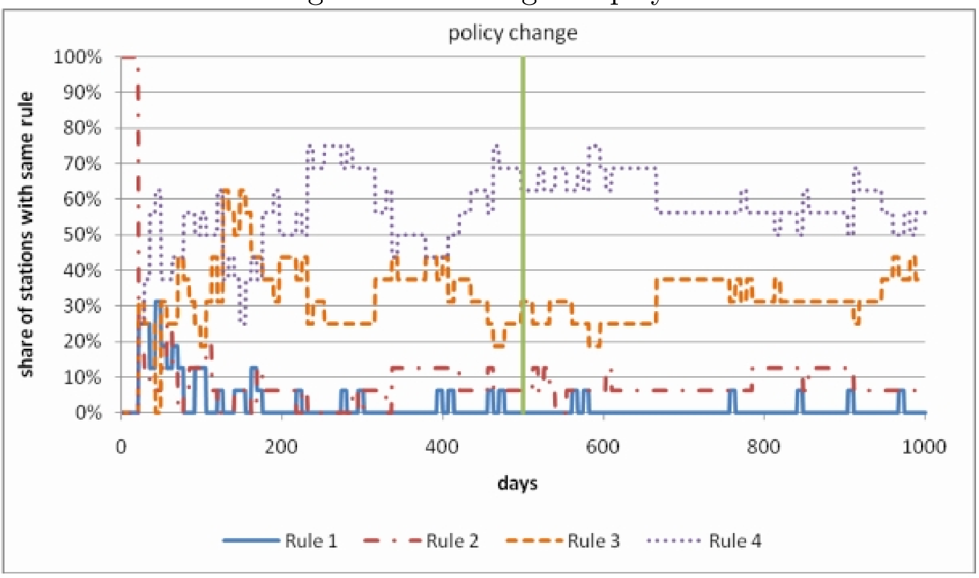


Figure 3.4: Pricing rule played



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3.6.1 *Policy Impact Simulation*

In the first simulation exercise we estimate the effect of the policy and we investigate the relationship between some key model parameters and the estimated effects.

The parameters space we explore is found in table 3.2 and the display setting used is Setting 1 (see figure 3.11). The use of this latter setting let us discriminate between "treated" and "non treated" stations[36]. This simulation generates 64 different combinations of parameters that are each run 10 times with different underlying random seeds, for a total of 640 simulations.

Table 3.2: Sim 1: The changed parameters

Parameter	Value(s)
memory	0.5; 0.75
temp	0.1; 0.05
RL-share-based	On; Off
very-p-stve	0.3; 0.15
p-stve-t	0.1; 0.25; 0.5; 0.75

Each simulation is run for a total of 1000 days and every 7 days stations assess their pricing policy and might change price and price strategy (since price strategy depends on competitors' prices a station might change price even if it keeps constant the pricing strategy). The price comparison policy is activated starting from day 501. Each day we retrieve from the model the outcomes of interest, that we then use in the econometric analysis. Of the 1000 days we exclude the first 200 days and the days between day 501 and day 700. This is done to exclude from the regression analysis the two "adjustment periods".

For each of the 640 model specifications we then estimate the following OLS regression:

$$mean\_price_t = \alpha + \beta^{Policy} D + \epsilon_t \tag{3.4}$$

where D is a dummy variable taking value of 1 after the policy change (after day 501), the dependent variable is the outcome variable of interest (e.g. mean price asked by stations on display or off display; mean price paid by captive consumers), finally the subscript t identifies the day of the simulation. .

[36]We adopt the terminology *treated* and *non treated* (instead of the more usual *treated* and *contol)* since the non treated unit, differently from the treatment evaluation literature, are not used as a proper control group in this setting.

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Given the above regression we estimate 640 different policy impact of which we can draw the distribution.

Figure 3.5 shows the distribution of the estimated policy impact when we run the above regression for the mean price asked by treated and non treated stations. The estimated effects are both centered around zero but have a marked different distribution. The distribution for the treated stations is clearly left skewed.

The average of the policy impact is -0.17 euro cents for treated stations and 0.014 euro cents for non treated stations. If we perform a t-test on the policy impact averages we find that only the mean associated to treated stations is statistically different from zero at 5%. Therefore it seems that on average the policy change is associated with a decrease in the price asked by treated station while it leaves unchanged the pricing of non treated stations.

Table 3.3: Summary of policy impact

Variable	Obs	Mean	Std. Dev.
b_p_treated	640	-0.17	0.246
b_p_non_treated	640	0.015	0.114
beta_p_capt	640	-0.029	0.091
b_p_active	640	-0.514	0.363
b_p_very active	640	-0.111	0.232

We also look at the average price paid by the three types of drivers (captive, active and very active). The active category is the category that is expected to gain the most from the introduction of price comparison while benefits to captive or very active drivers might only come indirectly as an externality.

Figure 3.6 shows the three parameters distributions. The policy impact distributions for captive drivers and very active drivers are both centered around zero with the average of the parameters being respectively -0.03 and -0.11 euro cents. The estimated parameter for the active drivers exhibits much higher variation with an average of -0.51 euro cents.

Hence, from the distributional analysis, it seems that the estimated effect of the policy on the price paid by active consumers varies substantially depending on the specific choice of the model parameters.

For the other two categories of drivers, captive and very active, the effect of the policy does not seem to be too influenced by the choice of the model parameters.

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Figure 3.5: Simulation 1: Distribution of policy impact (average asked price)

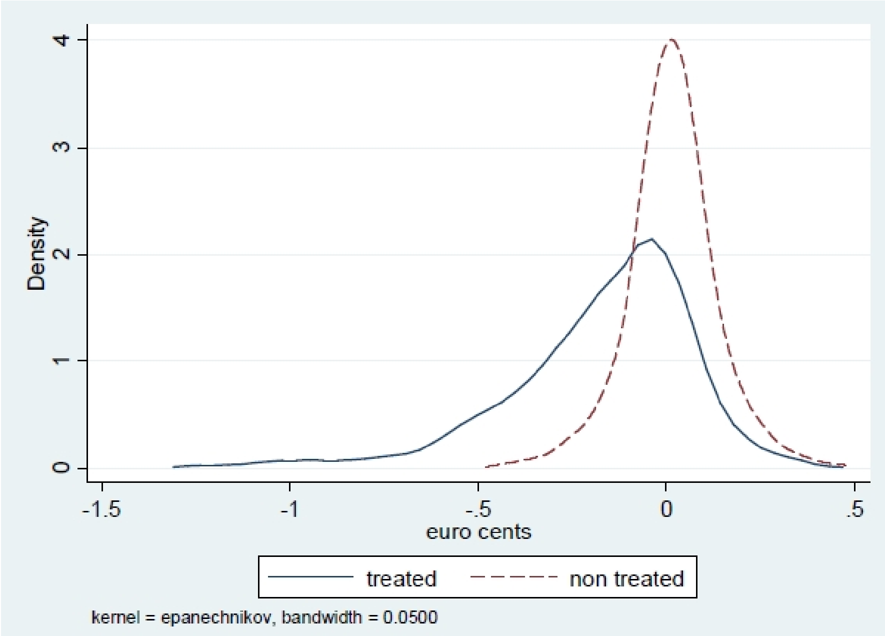
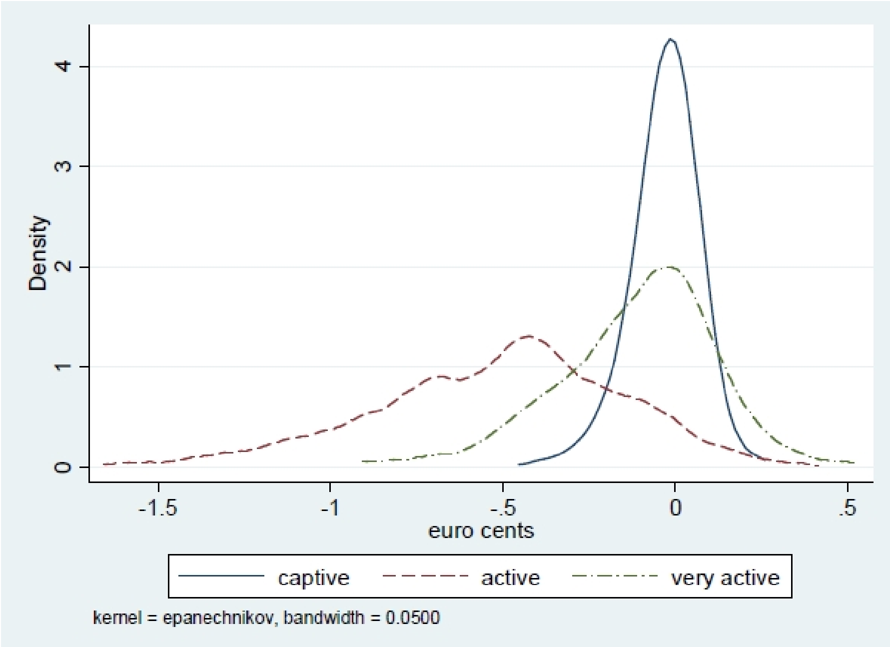


Figure 3.6: Simulation 1: Distribution of policy impact (average paid price)



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Nevertheless, overall the effect of the policy change on the prices paid by drivers seems again to be limited. The negative support of the distribution of the policy impacts for active drivers is bounded at around -1.7 euro cents.

Table 3.4: The impact of model parameters on policy impact

	(1)	(2)	(3)	(4)	(5)
VARIABLES	b_p_treated	b_p_non_treated	beta_p_capt	b_p_active	b_p_very active
pstvet	−0.543∗∗∗	0.0370∗∗	−0.0840∗∗∗	−0.432∗∗∗	−0.365∗∗∗
	−0.0325	−0.018	−0.0141	−0.0551	−0.0339
temp	0.358	−0.0656	0.0597	−0.0279	0.435
	−0.321	−0.178	−0.14	−0.545	−0.336
rlshare	−0.0744∗∗∗	−0.0039	−0.011	−0.0756∗∗∗	−0.0277∗
	−0.0161	−0.00891	−0.00698	−0.0273	−0.0168
verypstve	0.138	−0.0644	−0.0122	0.335∗	0.258∗∗
	−0.107	−0.0594	−0.0465	−0.182	−0.112
memory	−0.0576	−0.116∗∗∗	−0.0820∗∗∗	−0.141	−0.105
	−0.0643	−0.0356	−0.0279	−0.109	−0.0672
Constant	0.062	0.0938∗∗∗	0.0594∗∗	−0.288∗∗∗	0.0234
	−0.0554	−0.0307	−0.0241	−0.0941	−0.0579
Observations	640	640	640	640	640
R-squared	0.325	0.025	0.069	0.105	0.168

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

To study the heterogeneity in the parameter estimates we then employ a regression analysis to shed light on the relationship between the model parameters and the effect of the policy. In this part of the analysis the attention is on the same outcome variables object of the distributional analysis. The regressions take the form of 3.5 where we regress the policy impact β^{Policy} for each simulation run r on the simulation specific parameters.

$$\beta_r^{Policy} = \alpha + \gamma_1 temp + \gamma_2 rlshare + \gamma_3 verypstve + \gamma_4 memory + \epsilon_t \tag{3.5}$$

Table 3.4 presents the results of these set of regressions.

The most important model parameter seems to be the share of active consumers, the consumers that make use of the price comparison policy when this is activated. In the simulation this share takes four different values: 0.1, 0.25, 0.5, 0.75. From the regression we find that by increasing the share of active consumers we expect a reduction in all price

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measures except for the price asked at non treated stations that increases. The stronger effect is found for the price asked at treated station where an increase of 0.25 in the share of active consumers lowers the asked price by 0.13 euro cents. For the same increase the price paid by active consumers is reduced by 0.1 euro cents while the price paid by very active consumers decreases by less than 0.1 euro cents.

The model parameter that controls the share of very active consumers (a share of active consumers) does not seem to have any significant effect on the policy impacts for asked prices and for the price paid by captive drivers. Nevertheless, it has a significant effect on the policy impact estimates for the price paid by the two category of active drivers. This parameter takes two values in the simulation 0.15 and 0.3. Hence by increasing this share by 0.15 the estimates of the policy impact for both prices paid by active and very active drivers increase respectively by 0.05 euro cents and 0.04 euro cents. Hence, the more very active consumers we have in the model the lower is the effect of the policy in reducing the price paid by consumers using information.

The parameter rlshare is also significant for some regressions. Rlshare is a dummy variable and when it takes the value of 1 retailers make their strategy assessment not only looking at profits but also by looking at profits adjusted by potential demand, as to have a more precise (although noisy) estimate of the pricing strategy performance. The regression suggests that when retailers can better assess their strategies the policy is more effective. In this case, the policy change is associated with a further reduction of 0.07 euro cents in the price asked and in the price paid by active consumers while the effect on very active driver is a reduction of 0.02 euro cents.

Of the two reinforcement learning parameters that are varied in the model, *temp* and *memory*, only the latter seems to influence the estimated effect of the policy. When retailers put a higher weight on past profits (higher memory) the policy introduction is associated with a reduction in the price asked by non treated stations and the price paid by captive consumers. Therefore a higher memory limits the pressure to increase prices for captive drivers once information is disseminated.

In conclusion the *Policy Impact Simulation* suggests that the possible maximum savings implied by the price comparison policy are limited. Drivers activated by the policy (active drivers) on average might save 0.5 euro cents. Captive drivers are not affected by the policy whereas very active drivers save an average of 0.1 euro cents.

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Moreover we study how some key model parameters influence the estimated effect of the policy. The model parameter that explain most of the variation in the estimated effects is, as expected, the share of active drivers.

Consistently with the empirical findings the savings to consumers appear to be low. On a yearly basis an active consumer might save an average of 10 euro in his fuel expenditure[37]. Still a saving of 0.5 euro cents when compared to the average observed price range of 2 euro cents per liter and the average retailer margin of 4 euro cents gains economic significance (the saving represent respectively the 25% and the 12.5% of these figures).

In the next subsection we study how changes in the density of the price comparison displays influence the effectiveness of the policy.

3.6.2 *Policy Design Simulation*

In this second simulation we study how the three proposed display settings (see figure 3.11) influence the effectiveness of the policy.

In this simulation, together with the display setting we also vary the key model parameters that control the share of active drivers (the values taken can be seen in table 3.5). Given the variation in the model parameters and the three display settings we generate 24 different model specifications. As before, for each of these specifications we run 10 different simulations with a different random seed. Then we analyze the results using the same regression analysis outlined in the previous regression.

Table 3.5: Sim 2: Key model parameters

Parameter	Value(s)
very-p-stve	0.3; 0.15
p-stve-t	0.1; 0.25; 0.5; 0.75

Figure 3.7 shows the distribution of the estimated policy impact that captures the effect of the policy on the outcome variable of interest. In this chart we study the policy impact on four variables: the price asked by treated stations (notice that we have non treated stations only with Setting 1); the price paid by captive drivers; the price paid by active drivers and the price paid by very active drivers.

[37] This if we assume a drivers refuels her car once a week (40 liters) for 52 weeks in a year. This assuming that quantity demanded is constant.

For the treated stations we find that the support of the distribution is similar to the one found in the above simulation and the implied savings have a comparable magnitude. Also it seems that Setting 3, the one with higher density of price comparison displays, is the one associated with the higher savings.

About the price paid by captive drivers we find that the three distributions of policy effects can be clearly ranked with Setting 3 being associated with the highest savings. The same can be said for the charts that look at the price paid by active and very active drivers. However, in the four charts we find that the three distributions overlap considerably.

Hence different densities of comparison displays can be consistent with similar effects of the policy. We look further into this by means of a regression analysis. In this analysis we regress the estimated policy impact on the share of active and very active drivers and on the dummies that capture the density settings.

Table 3.6 presents the results of these four regressions. The regression estimates for the parameters that capture the share of active and very active drivers are very much comparable to those found in the above *Policy Impact Simulation*. About the dummies that control for the density of price comparison displays we find the following.

The price comparison density implied by Setting 2, when compared to the baseline given by Setting 1, is associated with a further reduction of 0.16 euro cents in the price paid by captive and very active consumers.

The price paid by active consumers does not seems to be affected by this increased density as the effect of the policy seems constant. Thus expanding the price comparison coverage to the all network implies lower prices to captive drivers and to very active drivers but not to drivers that are activated by the policy.

When we look at the impact of the third setting, the one with highest density of price comparison, we find that this setting is associated with a higher impact of the price comparison policy and this is true for all four prices considered.

We also find that although the policy further reduces by 0.14 euro cents the price asked by treated stations, all drivers seems to benefit by a further reduction in prices in the order of 0.3 euro cents. Increasing the density of displays improves the effectiveness of the policy and further reduces prices for all category of consumers.

However the size of this reduction, although statistically significant, might be very small. For the single drivers such an extra saving only imply a total saving of only 6 euro on an

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Figure 3.7: Simulation 2: Distribution of policy impact

annual basis[38]. However, if we consider the active consumers as a whole the higher density of displays might bring higher savings in the order of 6 million euro on a yearly basis[39]. Such figure might justify the stepping up of the policy as it could finance around 300 more displays only in a single year[40].

[38]This if we assume a drivers refuels her car once a week (40 liters) for 52 weeks in a year.

[39]Assuming that 50% of fuel on the highway is purchased by active drivers: (3.8 billion litres) x 50% x 0.003 euro=5.7 million euro

[40]We estimate that a single price comparison display could cost up to 20,000 euro. This because in the first phase of display deployment Autostrade per l'Italia could not issue a tender worth more than 200,000 euro. In this first phase only 10 displays were installed.

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Table 3.6: The impact of model parameters and display density on policy impact

	(1)	(2)	(3)	(4)
VARIABLES	b_p_treated	beta_p_capt	b_p_active	b_p_very active
pstvet	−0.680***	−0.607***	−0.717***	−0.764***
	−0.0492	−0.0463	−0.0773	−0.0736
verypstve	0.555***	0.440***	0.444*	1.012***
	−0.162	−0.153	−0.255	−0.243
Setting 2	−0.0181	−0.165***	−0.0251	−0.164***
	−0.0298	−0.0281	−0.0468	−0.0446
Setting 3	−0.140***	−0.293***	−0.309***	−0.312***
	−0.0298	−0.0281	−0.0468	−0.0446
Constant	−0.0274	0.118***	−0.305***	−0.00823
	−0.0466	−0.0438	−0.0731	−0.0696
Observations	240	240	240	240
R-squared	0.493	0.552	0.378	0.426

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

3.7 Conclusions

This paper studies the introduction of a major price comparison policy introduced in the Italian highway refueling market, starting in summer 2007. The main feature of the policy consisted in the deployment of road-side price comparison electronic displays. These displays, positioned on the side of the highway, post and compare fuel prices (unleaded and diesel fuel) of the successive four refueling stations.

The declared objectives of the policy was to provide effective price comparison to drivers, which should have fostered competition among retailers and lead to lower fuel prices[41]. Indeed, before the introduction of the policy along pay-toll highways there was no price information, hence drivers had virtually no mean to choose where to refuel based on price information.

In the previous chapter we perform an empirical assessment, of this policy change, collecting price information before, and after the deployment of the price comparison displays.

[41] The policy was introduced to comply with Law 2 April 2007 n.40 that required highway concessionaires to introduce some remedies to increase price information and ease
price comparison.

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The empirical investigation suggests that the price comparison policy did not foster competition among retailers as posted fuel prices do not decrease after the introduction of price comparison. Nevertheless, the empirical findings suggest that consumers activated by the policy might save an average of 1 euro cents per liter if they are to use the price comparison display to inform their purchasing decisions. Hence, although the policy do not foster price competition among retailers the presence of price comparison can still be beneficial to some categories of consumers, those activated by the policy.

This paper models the introduction of price comparison through an agent based computational economics (ACE) model that first explicitly design the behavior and characteristics of consumers and retailers, and then simulates market interactions.

The paper contributes to the theoretical literature on competition and consumer policy by offering a model that incorporates three important elements:1) dynamic interactions between consumers and retailers; 2) behavioral decision making in consumers and retailers (retailers have adaptive learning features); and 3) heterogeneous consumers and retailers.

The purpose is to use the ACE model firstly to rationalize the empirical results and secondly to explore the role of ACE modelling in improving the policy design.

We run two simulation exercises to study the impact of price comparison on prices. In addition, in the simulations we change some key consumers and retailers parameters to study the relationship between these assumptions and the estimated policy effects.

In the first simulation (*Policy Impact Simulation*) we investigate the average effect of the price comparison policy and the role of some key consumers and retailers parameters in explaining the variation in the policy impact.

The second simulation (*Policy Design Simulation*) looks in detail at the impact of the display density. The objective is to study the relationship between different display densities (number of display deployed) and the resulting policy impact.

In the first simulation exercise we find that retailers' prices are only marginally affected by the introduction of the policy. On average we find only a small reduction in average posted prices of around 0.17 euro cents per liter. However consistent with the empirical findings, we find that drivers that are activated by the policy experience higher savings that on average amount to 0.5 euro cents per liter. On the contrary both captive and very active consumers on average are not better off after the policy introduction.

As expected, we also find that as we increase the share of drivers that are activated by the policy the gain for consumers increases. However, even in cases in which a high share of

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consumers are activated the maximum savings implied by the policy amount to a maximum of about 1.5 euro cents per liter.

In the second simulation exercise the results suggest that by increasing the density of price comparison displays the average effect of the policy improves for all consumers. Indeed, the average price paid by all consumers (captive, very active and active) decreases by a further 0.3 euro cents, compared to the baseline setting. Again we find that the effect is higher on the price paid by consumers than on the price asked by retailers that only decreases by a further 0.14 euro cents.

We conclude that the ACE model is capable to explain the relatively low levels of savings attained by consumers (drivers) that were activated by the policy. The simulated results are indeed consistent with the empirical evidence.

Moreover we find that increasing the share of active consumers, or increasing the density of price comparison displays, might offer some additional gains to consumers and these seems to be substantials when we consider consumers as a whole (instead of looking at the benefit for the single consumer).

The ACE model explain the low level of savings attainable to active drivers but does not fully explain the empirical finding that average posted fuel prices increase after the introduction of price comparison. The ACE model finds that for an average model parameters choice the posted (asked) fuel price marginally decrease after the price comparison is introduced. Anyhow, the distribution of the policy impact has also a positive support, hence the model does not rule out a price increase following the introduction of price comparison.

About this point we note that in the ACE model we do not include any coordination between retailers. Hence we show how simple unilateral decisions might be consistent with the observed price rigidity. By introducing minimal coordinations between retailers we might reconcile the simulated effects with the empirical findings.

For future research the priority would be to extend the scope of the model and endogenize consumer search to make it dependent on the available savings and on consumers' perceptions about price comparison effectiveness.

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3.8 Appendix A: Figures

Figure 3.8: Price comparison display (source: www.autostrade.it)

Figure 3.9: Example of displays and stations location

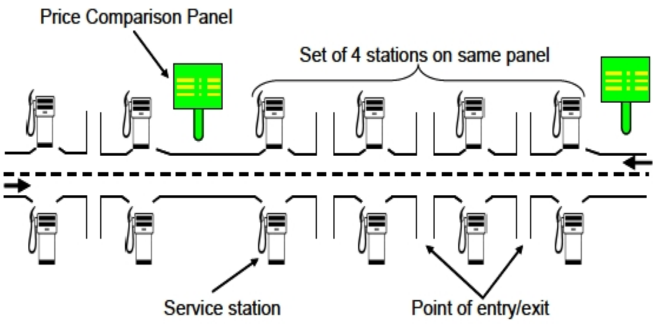

Figure 3.8 shows the design of price comparison displays installed along the ASPI pay-toll highway network. The display posts the brands and prices (of self service unleaded and diesel fuel) of the four next consecutive stations. The stations are ranked by distance to the display (with the closest being first) and the cheapest station is highlighted by a green dot next to its price. ASPI officials, when enquired about the display design, reported that the decision to post only four prices is an outcome of a trade-off between posting many prices (as to offer more information, but with inevitable physical constraints) and assuring a minimum level of comparison among brands. Since on the ASPI network there is a maximum of three consecutive stations all from the same brand they decided to adopt the four stations design. Both in the design stage and in the location decision the managers of the refueling stations were not involved. They are only responsible for the communication of prices through the

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software platform (that serves both the price comparison website and the price comparison displays).

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Figure 3.10: Screenshot of the ASPI price comparison website (source: www.autostrade.it)

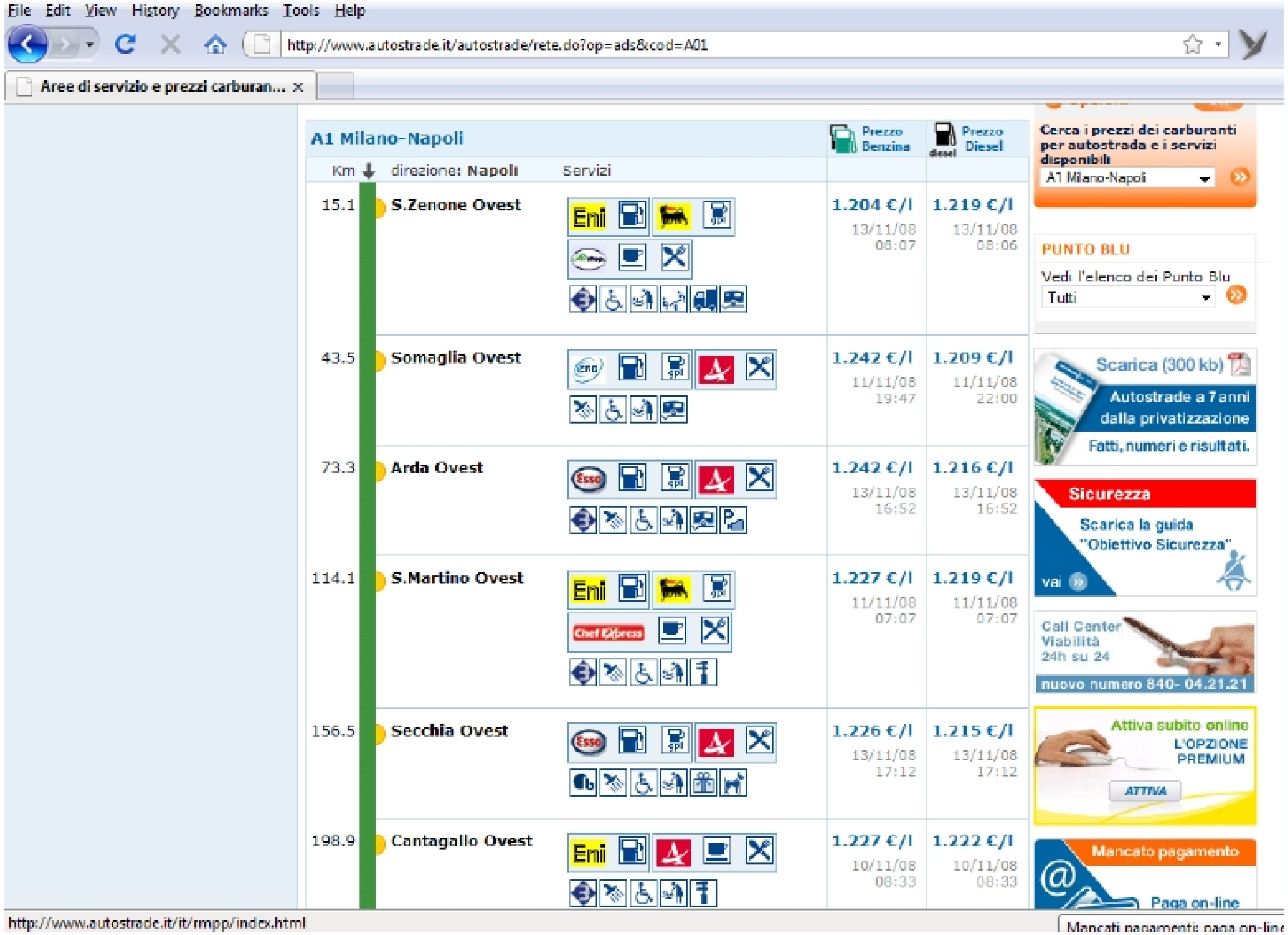

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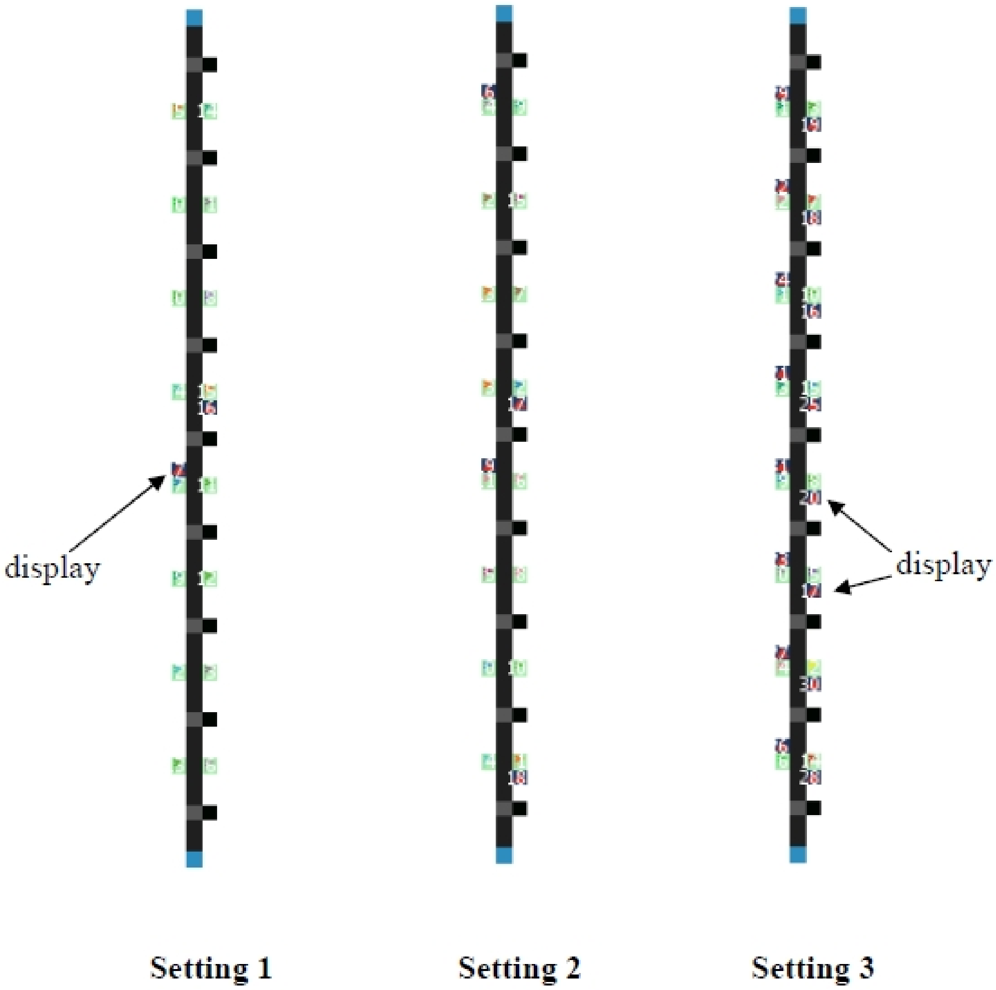
Figure 3.11: Displays settings



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3.9 Appendix B: The code

```
;; SET GLOBALS VARIABLE

globals [dayw  green-areas  service-areas intersections hw-intersections
        cycles
        private-intersections days station-access NumStations Hw-stations
        NS-stations
        mean-p-t-capt mean-p-t-pstve mean-p-t-vpstve Hw-p-stations
        Hw-np-stations rnd
        mean-panel min-panel max-panel range-panel mean-np min-np
        max-np range-np mean-pstve
        info-ref mean-capt no-info-ref mean-vpstve vinfo-ref
        panel-status   num-rule avg-rule-p avg-rule-np
        checked
        num-cars
        ref-type
        ]

;; DEFINE BREEDS AND OWN-VARIABLES

breed [stations station]    ;; define the stations turtles
breed [cars car]  ;; define the cars turtles
breed [panels panel]  ;; define the panels turtles

patches-own [intersection? road? road-class road-cl1? road-cl2? road-cl8?
            private-intersection? station? hw-intersections? max-hw-int?
            min-hw-int?]            ;; define patches variables

cars-own [fuel ppl capacity reserve refilled bestp ref-station cheapest
            choosen? start n-refill p-stve? consider-refill-level
            refill-today? price st-id rnd-t short info? vpstve?]
            ;; define cars variables

stations-own [ownpmin ownpmax price dprofits sold dsold  cumprof  wprofits
            strategy competitors st-id s-direction t-customer w-customer
```


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```
            panel? cost competitors-p competitors-p1
            max-w-prof min-w-prof sign-thrsld p-incr ref-prof
            fitness Nfitness prob-p old-wprofits rule av-margin played rnd-st
            d-p-customer w-p-customer avg-prop avg-prop-p start-avg-prop
            sugg-price pan-id]    ;; define stations' variables

panels-own [p-direction list-stations pan-id]



;; CREATE THE ENVIRONMENT AND INITIALIZE MODEL

to setup

  clear-all

  set  world-to-import "highway.csv"

  import-world world-to-import


  set  p-stve-t 0.25
  set  emergency-fuel 0.1
  set  memory 0.5
  set  week-length 7
  set  AvgCars 300
  set  EvalWeeks 3
  set  F0 3
  set  price-setting "RL-based"
  set  temp 0.05
  set  fuel-concern 0.25
  set  fix-seed false
  set  very-p-stve 0.15
  set  Activate-Panel false
  set  t-red-bias 0.4
```


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```
  set  per-exit 0.2


  set checked false
  set  num-rule 4
  set rnd new-seed
  random-seed new-seed
  set ref-type n-values 3 [0]
  set num-cars 0
  if-else fix-seed
  [
  set rnd 137
  random-seed rnd ]

  [
  set rnd new-seed
  random-seed new-seed ]


  setup-map           ;; Setup the map
  setup-stations      ;; Place stations

  if AvgCars > 0 [setup-cars]     ;; Place cars
  setup-intersection  ;; Identify intersections
  set dayw 0          ;; Initialize day of the week at 0
  set days 0          ;; Initialize days
  set mean-p-t-capt [0]
  set mean-p-t-pstve [0]
  set mean-p-t-vpstve [0]
  setup-panels



end
```


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```
;; Setup Procedures

to setup-map
  set service-areas patches with [pcolor = 68]
  ask patches with [(pcolor > 0) and (pcolor < 9)] [set road-class pcolor ]
  ask patches [set road? ifelse-value ((pcolor > 0) and (pcolor < 9)) [true] [false]
  ask patches [set road-cl1? ifelse-value (pcolor = 1) [true] [false] ]
  ask patches [set road-cl2? ifelse-value (pcolor = 2) [true] [false] ]
  ask patches [set road-cl8? ifelse-value (pcolor = 8) [true] [false] ]
end



to setup-stations                       ;; Create stations
  set-default-shape stations "flag"
  set cycles 0
  ask service-areas [sprout-stations 1 [set st-id ((count stations) - 1) ]]
  ask stations [
     set sugg-price  precision (random-normal 1.005 0.003) 3
     set strategy 1
     set label who
     set sold 0 set dsold 0
     set dprofits 0
     set cumprof 0
     set wprofits 0
     let diff1 precision (random-normal 0 0.003) 3
     let diff2 precision (random-normal 0 0.003) 3
     set ownpmin (precision (1000 * (sugg-price - (0.03 + diff1))) 3)
     set ownpmax (precision (1000 * (sugg-price + 0.012 + diff2)) 3)
     set cost (ownpmin - (0.01 + random-float 1 / 100))
     set av-margin (precision (ownpmax - ownpmin) 3)
     set t-customer 0
     set w-customer 0
     set d-p-customer 0
     set w-p-customer 0
```


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```
    set avg-prop 0
    set avg-prop-p 0
    set start-avg-prop []
    face one-of neighbors4 with [road?]
    set panel? false
    set max-w-prof 0
    set min-w-prof 0
    set sign-thrsld 0
    set p-incr 0.01
    set ref-prof 0
    set rule 1
    set price (precision ((ownpmin + random-float av-margin   ) / 1000 ) 3 )
    set rnd-st random-float 1
    set pan-id []
    set competitors-p1 []
     ]

set NumStations count stations

 ;; set highway station group
set Hw-stations stations with [[road-cl1?] of patch-ahead 1]

ask Hw-stations [set s-direction ifelse-value (heading = 270) [0] [180]]
ask n-of (floor (NumStations / 2)) stations [set strategy -1]
ask stations [

   set fitness n-values (num-rule) [(F0 * 1)] ; fitness is set during the
                                                evaluation week
   set Nfitness n-values (num-rule) [0] ; normalized fitness
   set prob-p n-values (num-rule) [0] ; probability to play rule
   set played n-values (num-rule ) [0] ; number of time rule is played
   set old-wprofits 0 ; initialize old profit variable

 ]
end
```


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```
to setup-panels      ;; Create panels
  set-default-shape panels "triangle"                    ;; change shape
  ask patches with [pcolor = 103] [sprout-panels 1 [
      face one-of neighbors4 with [road-cl1?]
      set p-direction ifelse-value (heading = 270) [0] [180]
      set color red
      set pan-id who
      set label who ]

      ]
  assign-stations-to-panel

end



;; Create cars
to setup-cars
  set-default-shape cars "car top"                    ;; change shape

end

to gen-cars [numcars]
    ask patches with [pcolor = 95] [
      sprout-cars int ((t-red-bias) * numcars )  [
      set color 15
      start-cars
      set short false
      set num-cars (num-cars + 1)
     ]
     ]

    ask patches with [pcolor = 3] [
      sprout-cars int ( ( 0.7 + random-float 0.6)*(1 - t-red-bias)*numcars/9) [
      set color 45
```


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```
       start-cars
       set short true
       set num-cars (num-cars + 1)
      ]
      ]
end

to start-cars

      assign-destination-to-cars                ;; assign destination to cars

      set rnd-t random-float 1
      set capacity ( 35 + random-float 20 )
      set reserve (emergency-fuel * capacity)
      if reserve < 8 [set reserve 8]
      set fuel (reserve + random-float (capacity - reserve))
      set consider-refill-level (reserve + fuel-concern * (capacity - reserve) )


      if fuel > capacity [set fuel capacity]
      if fuel < reserve [set fuel reserve]

      set label round(fuel)
      set ppl (precision (1 + random-float 2) 2)  ;; set car efficiency
                                                     patches per litres

      set refilled 0
      set bestp (precision (random-normal 0.985 0.02) 3)
      set ref-station []
      set cheapest no-turtles
      set n-refill 0
      set p-stve? ifelse-value (random-float 1 < p-stve-t) [true] [false]
      set info? false
      ifelse rnd-t < very-p-stve
        [set vpstve? true]
```


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```
      [set vpstve? false]
     set refill-today? false
     set price 0
     set choosen? false
end

to  assign-destination-to-cars
  if pycor > 24 [set heading 180]
  if pycor < -21 [set heading 0]
  if pycor > -21 and pycor < 25  [
                  let rnd-head random-float 1
                  ifelse rnd-head < 0.5
                  [set heading 0]
                  [set heading 180]
                  ]

  fd 1
  set start 1
end


to assign-stations-to-panel
  ask panels [
  ifelse p-direction = 0
  [
  let nextstations min (list 4 (count Hw-stations with [(s-direction =
                        [ p-direction] of myself)
                        and (ycor > [ycor] of myself )]))

  set list-stations
  sublist sort-by [[distance myself] of ?1 < [distance myself] of ?2]
          Hw-stations with[(s-direction = [p-direction] of myself)
          and (ycor > [ycor] of myself )] 0 nextstations

  ]
```


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```
[
let nextstations  min (list 4 (count Hw-stations with
  [ (s-direction = [p-direction] of myself) and (ycor < [ycor] of myself )]))

set list-stations
sublist sort-by [[distance myself] of ?1 < [distance myself] of ?2]
        Hw-stations with [(s-direction = [p-direction] of myself)
        and (ycor < [ycor] of myself )] 0 nextstations
]
]

ask panels [
  foreach list-stations [
    ask ? [
    set panel? true
    set pan-id fput [pan-id] of myself pan-id
    ]

  ]
]

set Hw-p-stations stations with [panel?]
set Hw-np-stations Hw-stations with [not panel?]

ask Hw-np-stations
  [ set competitors (other Hw-stations in-radius 7 with
              [ s-direction = [s-direction] of myself])]

ask Hw-p-stations
  [ set competitors (other Hw-stations in-radius 7 with
              [ s-direction = [s-direction] of myself])]


ask Hw-p-stations [
```


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```
    foreach pan-id [
      foreach sort (Hw-p-stations with [s-direction = [s-direction] of myself
              and member? ? pan-id])[
        set competitors-p1 fput [ycor] of ?1 competitors-p1
      ]

    ]

  let miny min competitors-p1
  let maxy max competitors-p1

  set competitors-p (other Hw-stations with [s-direction=[s-direction] of myself
              and ycor <= maxy and ycor >= miny])]

end



to setup-intersection      ;;setup intersections for road and driving use
  ask patches with [road?]
  [
    set hw-intersections? false
    let num-rd-nbrs ((count neighbors4 with [road?]) - (count neighbors4
                                                  with [road-cl8?]))
    set intersection? ifelse-value (num-rd-nbrs > 2)
      [true] [false]
    if num-rd-nbrs = 2 [
      if ((length remove-duplicates [pxcor] of (neighbors4 with [road?])=2)
      and (length remove-duplicates [pycor] of (neighbors4 with [road?])=2))
        [set intersection? true]
    ]
  ]
  set intersections (patches with [road-cl2?]) with [intersection?]
  ask intersections [  set min-hw-int? false   set max-hw-int? false]
  set hw-intersections (patches with [road-cl1?]) with [intersection?]
```


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```
  ask hw-intersections [
  set min-hw-int? false
  set max-hw-int? false
  set hw-intersections? true
  set pcolor 3
  if pycor = max [pycor] of hw-intersections [set max-hw-int? true]
  if pycor = min [pycor] of hw-intersections [set min-hw-int? true] ]

end



to-report intersection [l1 l2]
let overlap []
foreach l1 [if member? ? l2 [set overlap fput ? overlap]]
report overlap
end





;; ACTIONS


;; Main procedure
to go

  tick
  set num-cars 0
  day-week
  drive
  set days ticks

  if not checked and Activate-Panel [
```


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```
  ask Hw-p-stations [set competitors competitors-p]
  set checked false
  ]

  ask stations [calculate]

  if (dayw = week-length) and (days >= (week-length) * EvalWeeks)
  [

  if price-setting = "RL-based"  [ask stations [set-p-RL-based]]
  ]


end




;; Time Procedure

to day-week
  ifelse dayw < week-length
  [
  set dayw (dayw + 1)
  ]
  [
  set dayw 1
  ]
end


;; BUYERS


;; Driving
```


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```
to drive

  set mean-p-t-pstve []
  set mean-p-t-vpstve []
  set mean-p-t-capt []
  loop [

  set cycles (cycles + 1)



  if Activate-Panel = true [
    ask panels [send-prices-panel cars in-radius 1 with
      [ p-stve? and fuel > 0 and heading = [p-direction] of myself]]
        ;; panels send price info to cars
  ]

  ask Hw-stations [    ;; station on the highway refills only cars going
                        in the right direction (stations on North
                        side serve only cars going north)

    refill (cars in-radius 1) with [heading = ([s-direction] of myself)
                          and (fuel < reserve) and (not choosen?)] 2 0

    refill (cars in-radius 1) with [choosen? and cheapest = myself] 2 1

    refill (cars in-radius 1) with [heading = ([s-direction] of myself)
    and (fuel < consider-refill-level) and (not choosen?)
    and ([rnd-st] of myself)<random-float 1 and random-float 1<0.3] 2 2

    set d-p-customer (d-p-customer + count (cars in-radius 1) with
                [ heading = ([s-direction] of myself)])
  ]
```


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```
  if AvgCars > 0 [drive-cars]
  if (count cars with [fuel > 0]) = 0 [
  set cycles 0
  stop]
  ]

end


to drive-cars
  ask cars with [ fuel > 0]
  [
  fd 1
  set fuel (fuel - 1 / ppl)
  ]

  ask cars with [p-stve? and (fuel <= consider-refill-level)
        and (not choosen?)] [choose-cheapest-t]

  if cycles < 5 [


  gen-cars random-poisson AvgCars

  ]

  let num-tour-die int (per-exit * count cars with [[pcolor = 3]
  of patch-here and (start = 1)]) ;car that exit highways at intersection



  ask n-of num-tour-die cars with [[pcolor=3] of patch-here and (start = 1)][
        if refill-today?
      [
        ifelse info?
          [ifelse vpstve?
```


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```
            [set mean-p-t-vpstve fput price mean-p-t-vpstve]
            [set mean-p-t-pstve fput price mean-p-t-pstve] ]

          [set mean-p-t-capt fput price mean-p-t-capt]
        ]

      die] ;car that exit highways at intersection

  ask cars with [[pcolor = 95] of patch-here and (start = 1) ]
  [
      if refill-today?
      [
        ifelse info?
           [ifelse vpstve?
            [set mean-p-t-vpstve fput price mean-p-t-vpstve]
            [set mean-p-t-pstve fput price mean-p-t-pstve] ]

          [set mean-p-t-capt fput price mean-p-t-capt]
        ]
      die]

end



;; Refilling

to send-prices-panel [customers]
  ask customers [set ref-station [list-stations] of myself]
end

to choose-cheapest-t
  if vpstve? [            set ref-station   sort Hw-stations
          with [(s-direction = [heading] of myself) ] ]
  let myycor ycor
```


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```
  let autonomy (fuel * ppl)
  let p []
  let poss-ref-stations []
  let corr (cos heading)
  if not (empty? ref-station) [
  foreach ref-station [
  if (([ycor] of ? - myycor) * corr) <= autonomy and
    (([ycor] of ? - myycor) * corr) > 0
  [ set poss-ref-stations lput ? poss-ref-stations set p lput[price] of ? p]
  ]

  set ref-station poss-ref-stations set poss-ref-stations []
  ]


  if not (empty? ref-station) [
    set p (min p)


    set cheapest one-of filter [[price] of ? <= p] ref-station
    set choosen? true



     if (bestp < p) and (short = true)
     [
     let dist-best random 200
     if dist-best < autonomy
     [set fuel capacity
     set choosen? false]
     ]

   ]

end
```


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```
to refill [customers types id]  ;; rule that tells driver to refill

  if any? customers
  [
    let myid st-id  let pmyid price
    let fsold sold
    ask customers [
    set n-refill (n-refill + 1)
    set fsold (fsold + (capacity - fuel))
    set fuel capacity
    set price pmyid
    set st-id myid
    set refill-today? true
    set choosen? false
    set cheapest no-turtles
    if id = 1 [set info? true]
    ]

    let num-ref-type item id ref-type
    set ref-type replace-item id ref-type ( 1 + num-ref-type )
    set t-customer (t-customer + count customers)

    set sold fsold
  ]
end




;; STATIONS
```


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```
;; Accounting

to calculate    ;; stations calculates own profits
  set dprofits ((price * 1000 - cost) * sold)          ;; dayly profits
  set cumprof (cumprof + (dprofits / 1000))     ;; cumulative profits
  set dsold sold
  set sold 0


  if dayw = 1 and  days = week-length + 1 [set min-w-prof wprofits set
    ref-prof wprofits]



  if dayw = 1
  [


        if (days < (week-length) * EvalWeeks) [
          if wprofits > max-w-prof [set max-w-prof wprofits]
          if wprofits < min-w-prof [set min-w-prof wprofits]
          set sign-thrsld (max-w-prof - min-w-prof)
          set ref-prof (max-w-prof + min-w-prof) / 2
          ]

        if (days < (week-length) * EvalWeeks) and days >= week-length [
          set start-avg-prop fput (w-customer/w-p-customer) start-avg-prop

          ;set fitness n-values (3) [(F0 * ref-prof)]
          ifelse RL-share-based
          [set fitness n-values (num-rule)[(F0*( (price*1000 - cost)/cost)
           * (median start-avg-prop) )]]
```


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```
        [set fitness n-values (num-rule) [F0 * (  ref-prof )]]
          ]

      if (days >= (week-length) * EvalWeeks) [
        set ref-prof ( (1 - memory) * ref-prof + memory * wprofits)]

      set old-wprofits wprofits
      set wprofits dprofits
      ; calculate number of visits
      set w-p-customer d-p-customer
      set w-customer t-customer
      set d-p-customer 0
      set t-customer 0
]


if dayw > 1
[
set wprofits (wprofits + dprofits)

; calculate number of visits
set w-p-customer ( w-p-customer + d-p-customer)
set d-p-customer 0

set w-customer ( w-customer + t-customer)
set t-customer 0

]

if dayw = week-length [

set avg-prop ( precision (w-customer / w-p-customer) 5)
let rnd-avg-prop random-normal avg-prop 0.025

set avg-prop-p (((price * 1000 - cost) / cost)  * rnd-avg-prop)
```


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```
  ]


end

;; Price settings decisions

to Norm-fitness
  let Fmin min fitness
  let Fmax max fitness
  let Fmax-Fmin (Fmax - Fmin)
  set Nfitness []
  foreach n-values (length fitness) [?]
  [
  ifelse Fmax-Fmin != 0
  [set Nfitness lput (((item ? fitness) - Fmin) / Fmax-Fmin) Nfitness]
  [set Nfitness lput 0 Nfitness]
  ]
end


to calculate-prob-p
  let sumf 0
  foreach n-values (length Nfitness) [?]
  [
  set sumf (sumf + exp ((item ? Nfitness) / temp))
  ]
  set prob-p []

  foreach n-values (length Nfitness) [?]
  [
  set prob-p lput ((exp ((item ? Nfitness) / temp ) ) / sumf ) prob-p
  ]
end
```


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```
to select-price
  set rule int (random-float 1 * (length Nfitness))
  let startp rule
  let rnd-num (random-float 1)
  loop
  [

  if rnd-num < (item startp prob-p) [
    ;set price (precision ((ownpmin + av-margin / 4 * (startp) ) / 1000 ) 3 )
    set rule startp
     ;show "rule 1 used"
    ;if rule = 0 [ set price (precision (ownpmin / 1000 ) 3 ) ]
    if rule = 0 [ set price (precision (min [price] of competitors - 0.02) 3)  ]
    if rule = 1 [ set price (precision (min [price] of competitors - 0.01) 3) ]
    if rule = 2 [ set price (precision (mean [price] of competitors) 3) ]
    if rule = 3 [ set price (precision (ownpmax / 1000 ) 3 ) ]
    if price < (precision(ownpmin/1000)3)
[set price (precision (ownpmin / 1000 ) 3 ) ]
    if price > (precision(ownpmax/1000)3)
[setp price (precision (ownpmax / 1000 ) 3 ) ]
    stop]

  ifelse startp = ((length Nfitness) - 1)
    [set startp 0]
    [set startp (startp + 1)]

  if startp = rule [
    ;show "rule 3 used"
    set startp int (random-float 1 * (length nfitness ))
    ;show startp
    ;set price (precision ((ownpmin + av-margin / 4 * (startp) ) / 1000 ) 3 )
```


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```
    set rule startp
    ;if rule = 0 [ set price (precision (ownpmin / 1000 ) 3 ) ]
    if rule = 0 [ set price (precision (min [price] of competitors - 0.02) 3)  ]
    if rule = 1 [ set price (precision (min [price] of competitors - 0.01) 3) ]
    if rule = 2 [ set price (precision (mean [price] of competitors) 3) ]
    if rule = 3 [ set price (precision (ownpmax / 1000 ) 3 ) ]
    if price < (precision (ownpmin/1000)3)
[set price (precision(ownpmin / 1000 ) 3 ) ]
    if price > (precision (ownpmax/1000)3)
[set price (precision(ownpmax / 1000 ) 3 ) ]
    stop]

  ]


end


to set-p-RL-based
  let Ft-1 item rule fitness
  ifelse RL-share-based
    [set fitness replace-item rule fitness
(avg-prop-p * memory + (1 - memory)*Ft-1)]
    [set fitness replace-item rule fitness
( wprofits  * memory + (1 - memory)*Ft-1)]
  Norm-fitness
  calculate-prob-p
  select-price
  let num item rule played
  set played replace-item rule played (1 + num)
end
```


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