TI 2008-117/1
Tinbergen Institute Discussion Paper

Do Auctions and Forced Divestitures increase Competition?

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Do auctions and forced divestitures increase competition?

Evidence for retail gasoline markets

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August 2, 2011

Abstract

Where markets are insufficiently competitive, governments can intervene by auctioning licenses to operate or by forcing divestitures. The Dutch government has done exactly that, organizing auctions to redistribute tenancy rights for highway gasoline stations and forcing the divestiture of outlets of four majors. We evaluate this policy experiment using panel data containing detailed price information. We find that an obligation to divest lowers prices by over 2% while the auctioning of licenses without such an obligation has no discernible effect. We find weak evidence for price effects on nearby competitors.

JEL classification: D43, D44, L11
Keywords: Divestitures, Auctions, Entry, Policy Evaluation

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1 Introduction

Governments can play an active role in shaping markets. For example, to kickstart competition on new markets, they can auction licenses to operate. This practice is particularly prevalent in telecoms.[1] One problem in the design of such auctions is that established firms already have an advantage vis-à-vis new entrants.[2] As another example, when merging firms are deemed to obtain excessive market power, antitrust authorities can impose divestitures as a remedy. Recent examples in the EU include Air France/KLM, Skandinavisk Tobakskompagni/British American Tobacco, and Arcelor/Mittal.[3] The most renowned divestiture in the US is the separation of AT&T from the Regional Bell Operating Companies (RBOCs) in 1984.[4] With the mergers of large oil companies, US antitrust authorities also required the divestment of a number of assets, including retail gasoline stations.[5]

Such auctions and forced divestitures are supposed to foster competition. Ideally, to evaluate the competitive effect of auctions and forced divestitures, one would need a case in which some outlets are sold, either through an auction or due to a forced divestiture. Then, one would need to be able to observe how prices are affected at such outlets, both in absolute terms and relative to outlets that were not put up for sale. In this paper, we analyze a Dutch policy experiment that amounts to exactly that.

From 2002 to 2024 the Dutch government organizes annual auctions to redistribute the tenancy rights to operate gasoline stations along public highways. Moreover, each of the four major firms had to divest a substantial number of their highway outlets by 2006. The majors could use the auctions to fulfill the divestiture obligation, but also had the ability to sell outlets privately. We thus have a natural

[1] See e.g. Börgers and Dustmann (2003) for an overview of the European experience, or McAfee and McMillan (1996) for the early US auctions.

[2] See Hoppe et al. (2006) or Klemperer (2002).

[3] See European Commission, press releases IP/04/194, IP/08/1053, and IP/06/725 respectively.

[4] For an evaluation of the effects of this breakup, see e.g. Hausman et al. (1993).

[5] These cases include the BP/Amoco merger (http://www.ftc.gov/opa/1998/12/bpamoco.shtm), a joint venture between Shell and Texaco (http://www.ftc.gov/opa/1997/12/shell.shtm) and the Exxon/Mobil merger (http://www.ftc.gov/opa/1999/11/exxonmobil.shtm)

experiment that consists of a series of annual treatments that start in 2002. Before 2006, the treatment consists of the auction combined with an obligation to divest, while from 2006 onwards the treatment is only the auction.

We give an empirical assessment of this policy experiment using a panel of station-specific retail prices for gasoline outlets in the Netherlands from October 2005 until August 2007. This means that some sites were already auctioned before the sample period, while others were auctioned during the sample period. The data were obtained from Athlon, the largest independent car leasing company in the Netherlands with a fleet of over 125,000 cars.

A comparison of sites based on observable characteristics suggests that the sites selected by the government to be auctioned in a given year is a random draw of the set of all highway sites. However, firms may request changes in the initial auction schedule. This happened and we show that this has introduced selection bias in the sites actually auctioned in a particular year: sites that have been auctioned were on average smaller (measured in plot size and number of pumps) and located in areas with lower demand and higher market concentration. To control for unobservable heterogeneity, we us IV estimation. In these estimations, the initial auction announcements by the government serve as an instrument for the set of sites actually auctioned. For sites treated *during* our sample period, we have price observations before and after the auction took place. This allows us to use a difference-in-difference (DID) estimator which does not suffer from the selection issues mentioned above. Moreover, the DID-estimator allows us to separate the effects of the auction and that of the forced divestitures since the treatment in 2005 consists of the auction plus the obligation to divest, while the treatment in 2006 only consists of the auction. Combining these methods allows us to get a reasonable idea of the direct price effects of both auctions and forced divestitures on the auctioned sites themselves, as well as of the indirect effect on prices of nearby competitors.

As far as retail prices are concerned, there is little reason to expect that auctions in themselves have an effect. When an outlet is auctioned and the current owner can also bid, he is likely to win: the current owner has superior information on local market conditions and also has more at stake in the auction.[6] If the current owner does win the auction, he is unlikely to change prices much. This is indeed what we find.

When auctions are combined with an obligation to divest, the story is different. With a divestiture, prices at auctioned stations will fall when the new owner operates more efficiently or – when the transfer in market share is from a major to a minor firm – the new operator is not able the charge a brand premium. When the transfer increases competition, we expect prices to fall as well at neighboring stations. We find that when gasoline outlets are auctioned with an obligation to divest, prices decrease by at least 2% on average. This is a substantial decrease that amounts to some 20% of the total price-cost margin, or some 70% of the highway price premium.[7] Despite the considerable price drops at auctioned sites, we find only weak evidence that prices at the sites of nearby competitors decrease, suggesting that the increased market share of minor firms has only led to a modest increase in competition.

Empirical studies of the effectiveness of license auctions and divestitures are scarce. Lack of appropriate market data is the prime explanation for this paucity of empirical work. Recently, Tenn and Yun (2011) show that divestiture requirements imposed in Johnson & Johnson's acquisition of Pfizer were successful in maintaining the pre-transaction level of competition. Older studies like Elzinga (1969), Ellert (1976) and Rogowsky (1986) study the effects of divestitures in an indirect way.

Also related to our study is Hastings (2004), who estimates the effects of the acquisition of an independent gasoline retailer by a vertically integrated firm and finds that this increases retail gasoline prices by some 4% at competing stations. Taylor *et al.* (2010) replicate her study using high-frequency fleet card data to find

[6] See e.g. Bulow *et al.* (1999), who show that in common value auctions a bidder with a small advantage has a much higher probability to win. Note that this is exactly the reason to expect an incumbency advantage in license auctions described in the first paragraph.

[7] Hosken et al. (2008) report an average gross retail margin in the US of 12%. In the Netherlands, Shell claims on its website that the average margin amounts to 13.5 cent, which boils down to 11%. In our analysis, we find a highway premium of some 3%.

effects of a much smaller magnitude.[8]

Our study has a number of advantages over these two. First, our data is far more complete, covering over 80% of all gasoline stations, against some 20-30% in the Hastings (2004) and Taylor *et al.* (2010) studies. Our fleet card data shares the high frequency property of Taylor *et al.* (2010), without undersampling (or even ignoring) minor brands and independents as both Hastings (2004) and Taylor *et al.* (2010) do (see Table 1). Second, in our study stations carrying different brands are being treated, which is not the case when studying a takeover. This eliminates a potential bias. Third, we can also study the price effect of treated firms, rather than only that of competing firms. Hastings (2004) and Taylor *et al.* (2010) are not able to do so due to a lack of data.

The paper proceeds as follows. The next section describes the Dutch market for retail gasoline and gives the details of the auction format relevant for our analysis. We summarize the outcomes of past auctions. Section 3 introduces the data. We provide summary statistics and present evidence that the set of auctioned sites cannot be considered random. Section 4 outlines our estimation strategy and discusses how we deal with non-randomness in the auction schedule. Section 5 presents the results, while section 6 concludes.

2 The auctions and forced divestitures

2.1 Introduction

In the 1990s, the Dutch market for retail gasoline at highways was suffering from a lack of competition. For outsiders, it was impossible to enter the market: current tenants held infinite concessions and the creation of new sites along existing highways was impossible. Market concentration was high. Out of a total of 250 highway stations, 225 carried one of four major brands (Esso, Shell, Texaco or BP).[9] Long negotiations led to an agreement in 2000 to address the lack of competition with

[8] See Hastings (2010) for a reply.

[9] In 72% of these cases the oil company was the concession holder, while the other cases concerned privately-owned stations supplied by one of the majors (NMa, 2006, p. 29).

three measures:

1. the auction of every highway concession to create the possibility of entry;

2. an obligation to divest for the four majors to lower market concentration;

3. a territory restriction that stipulates that two stations on the same highway in the same driving direction cannot operate under the same brand name.[10]

2.2 Auctions

To facilitate entry on the highway gasoline market, the current infinite concessions are canceled and replaced by concessions for a 15-year period. The new concessions are allocated through auctions. Starting in 2002, each year some 8 to 15 stations are sold sequentially in an annual first-price sealed-bid auction. By 2024, tenancy rights for all highway sites on state-owned land will have been sold at least once.[11]

Every interested party can participate in an auction, including the current owner. When the concession is auctioned for the first time the proceeds go to the current concession holder as a compensation for the loss of the original infinite lease. If the current concession holder is also the winner of the auction, however, he has to pay the Dutch state the difference between his own bid and the second highest bid. With independent private values it can be shown that this rule preserves the current owner's incentive to bid competitively, in the sense that his equilibrium bid strategy is the same as in a standard first-price sealed-bid auction in which the proceeds go to a third party.[12] The payment of the current concession holder is capped at 15%

[10] Note that this does not rule out that a company *owns* more stations in such an area.

[11] Some 10-15 highway stations are located on private land; these stations are not auctioned.

[12] Consider a standard first-price sealed-bid auction with n bidders, where each bidder's valuation x is drawn from some probability distribution $F(x)$. Denote by $G(x)$ the probability distribution of the highest of $n-1$ valuations: $G(x) = F(x)^{n-1}$. The equilibrium bid function β is then known to satisfy

$$\beta(x) = \frac{1}{G(x)} \int_0^x yg(y)dy, \qquad (1)$$

Now suppose that bidder 1 has to pay the difference between its own bid and the second-highest bid if it wins the auction, but pockets the proceeds of the auction if it does not, while the rules for the other bidders do not change. If bidder 1 would indeed still behave according to (1), it is optimal for the other bidders to do so as well, as their incentives are unaffected. Suppose that it has valuation of his own bid. Three months after the auction the site is transferred to the new concession holder. After the auction of a site the territory restriction comes into force. The participation fee for the auction in a given year is set at €1,500.

2.3 The auction schedule

An explicit aim of the government is for the auction schedule to be 'balanced' in the sense that each year sites of varying sizes (in terms of volume) and in various parts of the country are auctioned. In December 2001, a list of sites to be auctioned in each of the years 2002-2008 was published in the *Staatscourant*, the official Dutch Government Gazette. Each December the Staatscourant publishes an updated list of sites to be auctioned over the next 7.5 years.[13]

An oil company can make a written request to exchange the position in the auction schedule of two of its concessions. The government will grant a request if it is made at least one year before the auction and does not affect the balance of the auction schedule. Since it is not specified how exactly this balance is measured, we need to control for possible endogeneity in our analysis (see also Section 3.3).

INSERT TABLE 2 ABOUT HERE

For our purposes, it is useful to distinguish between:

- *unchanged* sites, for which the planned auction date did not change,

---

x but behaves as if its true value is z, while it uses (1). Denote the highest value among all other bidders as Y_1. Bidder 1's expected payoffs then are

$$\begin{aligned} U_1(x,z) &= (1-G(z))\,E\left(\beta(Y_1)\mid Y_1>z\right) + G(z)\left(x-\left(\beta(z)-E\left(\beta(Y_1)\mid Y_1<z\right)\right)\right) \\ &= \int_z^{\omega}\beta(y)g(y)dy + G(z)\left(x-\left(\frac{1}{G(z)}\int_0^z yg(y)dy - \frac{1}{G(z)}\int_0^z \beta(y)g(y)dy\right)\right) \\ &= \int_0^{\omega}\beta(y)g(y)dy + G(z)x - \int_0^z yg(y)dy \end{aligned}$$

Taking the FOC with respect to z yields $g(z)x - zg(z) = 0$, which is satisfied for $z = x$. Thus, for all x, it is indeed a best reply for firm 1 to bid according to (1), hence it is a Nash equilibrium for all bidders to do so.

[13] In 2004, no auction took place, and the entire auction schedule was delayed by one year.

- *inserted* sites which are auctioned earlier than originally planned because of a request by the oil companies, and,

- *postponed* sites which are auctioned later than originally planned because of a request by the oil companies.

Table 2 provides an example. Site “Hendriks”, owned by Total, is announced in 2001 to be auctioned in 2008. This does not change in later editions of the Staatscourant. We therefore label Hendriks an unchanged site. Shell Bergh-Z is an unchanged site as well: in 2003, its auction is still planned for 2009, consistent with the first announcement in 2002, 7.5 years before the auction. BP Witte Paarden first appears in the 2002 announcement, to be auctioned in 2009, but in 2003 it is no longer listed. The site must therefore be ‘postponed’: it will now be auctioned at some future date. The 2003 announcement includes two sites to be auctioned in 2009 that did not appear in 2002: BP Hoefplan and Esso Wons. As their first announcement appears less than 7.5 years before the planned auction, these must be ‘inserted’ sites.

2.4 Divestitures

The four majors are obliged to divest a total of 48 stations (25 for Shell, 10 for Esso, 8 for Texaco, and 5 for BP) by March 15, 2006. Which specific stations the majors wanted to divest is their own choice. Of course, the auction provides one natural avenue to meet (part of) this obligation. But the total number of stations auctioned by 2006 is only 32, so necessarily stations have to be sold privately as well. Private sales do not affect the auction schedule. The auction of a station will go through even if it is sold privately a few months before the planned auction date. The obligation to divest is only temporary. After 2006, the majors are allowed to increase the number of highway stations that they own provided that they do not violate the territory restriction. No new highway locations will become available before the end of the first round of auctions in 2024, except those already planned before 2000 (NMa, 2006).

2.5 Discussion

The playing field changed for major oil companies in the Netherlands around 2000. They learned that over the course of the next five years they had to divest a total of 48 highway gasoline stations. Moreover, all highway stations would be auctioned over the next 15 to 20 years. Naturally, each major firm will make a list of its least-preferred stations that it is most willing to sell. Generally speaking, these will be the stations that generate the lowest profits due to a low turnover, or due to a low profit margin as local competition is fierce. For the majors, if the major is lucky, some of these least-preferred stations will be scheduled for auction before 2006. Yet, the schedule for these early auctions may also include some of its more-preferred stations. In that case, the firm has two options. First, it could bid aggressively in the auction in an attempt to retain the station. Second, it could try to exchange the station that is up for auction with a less profitable station scheduled for auction after 2006. If it succeeds, the firm avoids a payment to the government while it can reap the benefits of the more-preferred station for some additional years. For our analysis, this implies that we need to correct for a potential selection problem.

3 Data

We use a fleet card data set which contains regular price quotes for 3,585 gasoline retail outlets in the Netherlands for the period October 1, 2005 - August 4, 2007, of which 237 located at the highway. In a report, Dutch competition authority (NMa) identifies a total of 4,319 outlets in the Netherlands in 2005, of which 256 highway outlets (2006, p. 8). This implies that our data covers about 83% of all outlets and about 93% of all highway outlets. Table 1 shows that the market shares of the different brands in our sample (in terms of number of outlets) is very similar to those reported by the NMa, both for highway and off-highway outlets. This in contrast to both Hastings (2004) and Taylor *et al.* (2010) whose data only cover a small sample of the market and undersample or even ignore some brands.

INSERT TABLE 1 ABOUT HERE

We restrict attention to regular unleaded gasoline. The price at a particular station on a given day is observed if at least one fleet card owner bought gasoline there. For the average highway outlet we have price quotes for 61% of all days considered. We append our station data with geographic coordinates that enable us to calculate Euclidean distances between stations. Data on the characteristics of each gasoline station were obtained from Experian Catalist Ltd.[14]

We distinguish between gasoline stations located along the network of major roads ('highway stations'), and those that are not ('non-highway stations') . Compared to other countries, the Netherlands has a highly connected network of highways. The fraction of highway stations is over 5%. In surrounding countries it is only 1-3%. Highway stations are estimated to account for 25% of all gasoline sold.[15]

3.1 Local market concentration

To get an idea about the environment in which Dutch gasoline stations operate, we look at some simple statistics. Site density is highest in the more densely populated western part of the country. Table 3 shows that (non-)highway sites tend to cluster with other (non-)highway sites. Most non-highway sites have no highway site within five kilometer, but most highway sites have one within just one kilometer. The reason is that many highway sites come in pairs, one in each driving direction. The average non-highway site has 3.6 non-highway sites within two kilometers, while

[14] We are not the first to use fleet card data to study retail gasoline markets. The first such studies appeared in the early 1990s and used low-frequency price data (monthly or weekly) at a relatively high level of aggregation (city or state averages) over a long period of time (typically 5 to 10 years) to analyze issues such as asymmetric price adjustments and price-cost margins. See for example Bacon (1991), Castanias and Johnson (1993), Borenstein and Shepard (1996) and Borenstein *et al.* (1997). More recently, attention has shifted towards higher frequency (daily or even bi-hourly) price data of individual stations for shorter periods of time (up to one year). Examples include Abrantes-Metz *et al.* (2006), Atkinson (2009), Doyle and Samphantharak (2008), Eckert *et al.* (2004), Noel (2007a, 2007b, 2009) and Wang (2009).

[15] Only 0.9% of all gasoline is sold through supermarkets, far less than in France (54%) or the UK (29%). The number of unmanned "express" stations increased rapidly, from 14% in 2002 to 22% in 2005. Most of these are non-highway stations.

the average highway site only has 1.4. This reflects that most highway stations are located outside cities and villages.

INSERT TABLE 3 ABOUT HERE

When we increase the distance from 1 to 10 kilometer, the number of neighboring highway sites increases 4-fold (2.40/0.53) for highway sites, but 38-fold (42.85/1.14) for non-highway sites. This reflects that most sites in the same driving direction along the same highway are at least 20 kilometer apart. Whenever we see a higher concentration of highway sites at a shorter distance, it is caused by highway interchanges. Our analysis will include market concentration measures that reflect the local density of both highway stations and non-highway stations.

3.2 Price determinants

One of the aims of this paper is to study the effect of the auctions on local competition. To address that issue, we first need to determine to what extent (and at what distance) gasoline stations act as local competitors. For that purpose, we estimate the following baseline reduced form equation:

$$p_{i,t} = c_t + \theta X_{i,t} + \epsilon_{i,t}. \quad (2)$$

The c_t's are day dummies that absorb daily price fluctuations common to all outlets. Station-specific variables are captured by $X_{i,t}$. We include a dummy for the four major brands, highway dummies, highway-year interaction dummies, major-highway interaction dummies, dummies for the Total and the Q8 brand, and dummies that reflect whether the site is company owned, is an unmanned express station, and whether hot drinks are available. The vector also includes plot size area, shop area, the number of pumps and interactions of the highway dummy with year dummies to account for trends in price differences in highway and non-highway sites. Border dummies that are 1 if the station is located in a zip code adjacent to the German or Belgian border, and measures of local market concentration are included as well.

For the latter we use the log of the number of highway and non-highway sites within 1 kilometer, and at a 1-2, 2-5 and 5-10 kilometer distance. We interact these with the highway dummy to allow for a different impact of local market concentration on highway and non-highway stations. To allow for local differences in demand we include the log of the number of private cars owned within a distance of 20 kilometer.[16] The index t is added to $X_{i,t}$ because a number of these variables (such as brand name or being an express station) may change value during the period of data collection.

INSERT TABLE 4 ABOUT HERE

Table 4 reports the results. Model A includes day dummies, geographical and site characteristics, highway-year interaction dummies and our measure of local demand. The estimates show that prices at highway stations are some 2.5-2.9% higher. This difference increases somewhat over time. [17] Majors charge higher prices on average, part or all of which may a "brand premium". Prices are considerably lower at unmanned express stations. Sites that are larger in terms of area size and number of pumps charge higher prices, as do sites in areas with higher local demand. Prices are higher at sites close to the Belgian border, but no effect is found for the German border.

Model B also includes local concentration measures interacted with the highway dummy variable. The local concentration of non-highway stations does turn out to affect prices. For non-highway stations, effects are significant up to two kilometer, the effect being strongest at 1-2km. Doubling the number of non-highway stations within 2 kilometer decreases prices by a total of 0.6%. Highway sites are only strongly

[16]We used information at the 4-digit zip code level (8.7 km^2 on average) provided by Statistics Netherlands on the number of private cars in 2006, using the midpoint of the zip code as point of reference. Taking a distance of 5 or 10 kilometer does not affect the results. Given the size of the zip codes, smaller distances are not informative.

[17]Possible explanations for the highway price premium include cost differences (for example, tenants of highway stations have to pay the government a usage fee dependent on actual volume sold), and demand factors. For commuters, frequenting a highway station may be more convenient as it saves time. Moreover, highway stations often have much larger on-site convenience stores, and consumers may be willing to pay extra for their gasoline because of this.

affected by non-highway stations closer than one kilometer, which implies that they must be near a highway entry or exit, or at a road running parallel to the highway.

The local concentration of highway stations seems to have little effect. For highway stations, we find a significant *positive* correlation between price and the number of other highway stations between 1 and 5 kilometer. Most probably this proxies for the positive demand effect of being close to an intersection of highways. Based on these estimates, we will use a distance of 5 kilometer ($Y_H = 5$) for other highway stations and a distance of 1 kilometer ($Y_{NH} = 1$) for non-highway stations in our assessment of the indirect price effects of the auctions.

3.3 Random treatment assignment

In this subsection, we examine to what extent the auctioned sites are a random draw from the set of all highway sites. Table 5 relates our classification of sites (unchanged, postponed, or inserted) to the outcome of the auction (ownership transfer, no ownership transfer, and the average highest bid) and shows for each category the average highest bid. The table suggests that majors have indeed used the opportunity to request changes in the auction schedule to get rid of their less-preferred sites. For sites that changed ownership the average highest bid is considerably lower than for those that did not (€2.3 mln. vs. €4.5 mln.). The majority of sites that changed ownership (12 out of 21) were inserted by the owner. Especially the auctions in 2003 and 2005 saw a high number of such sites.[18] Strikingly, 92% of all inserted sites changed ownership, while only 26% of unchanged sites did. Most ownership changes involved a transfer from a major to a minor.

Thus, whenever a site was inserted it almost always changed ownership in the auction. This is particularly true before 2006 when the obligation to divest was still in place. Unchanged sites were far more likely to be won by the current owner. As a consequence, it is hard to maintain that the auctioned stations form a random subset.

[18]Requests for changes had to be made at least on year before the auction. This rules out the auctioning of inserted sites in 2002.

INSERT TABLE 5 ABOUT HERE

As a further check on the randomness of the auction schedule, we consider whether treated and non-treated sites differ in terms of their observable characteristics. Table 6 presents summary statistics. We distinguish site characteristics that have not changed during the period of data collection (plot size, shop area, the number of pumps, whether the site is operated by a major before the auction, and whether the site is company owned before the auction) and those that are possibly endogenous because they may be affected by ownership changes (volume sold, the shop sales, the own regional market share and the local Herfindahl-Hirschman (HHI) index).[19] We have also included the exogenous Euclidean measures for local demand and local market concentration.

INSERT TABLE 6 ABOUT HERE

A comparison of the sites initially selected by the government with the remaining set of highway sites (columns (3) and (1) in Table 6) does not reveal large biases in observable characteristics. At the five percent significance level (two sided *t*-tests), we only find that company owned sites are somewhat overrepresented in the government's schedule and that the number of other highway sites within a range of 5 and 10 kilometer is a bit lower.[20] Importantly, we do not identify important biases in terms of site magnitude (measured in plot size and number of pumps), local market demand (measured by the number of privately owned cars within 20 kilometer distance) and local market concentration (measured by the HHI). From this we conclude that the government schedule can be considered random.[21]

[19] Own market share is measured as the number of sites with this brand name within a 5 km distance divided by the total number of sites within this distance. The local HHI is measured as the sum of squared market shares of the 18 main brands. Volume sold according to estimates by Experian Catalist Ltd.

[20] The overrepresentation of company owned sites in the published auction schedule can be explained by the prescription in the covenant between government and oil companies that sites currently operated by private parties only enter the auction schedule in 2019.

[21] Also, if the sector would have had a large influence on the original schedule, we would not observe so many changes.

This does not hold for the samples of auctioned sites. A comparison with the set of non-auctioned (control) sites (columns (5) and (11)) reveal significant differences for a large number of observable characteristics. At auctioned sites, the number of pumps and local demand is significantly lower at the 1 percent level, whereas local market concentration is significantly higher. In terms of plot size, auctioned sites are significantly smaller at the 5 percent level. This in particular holds for the subset of 11 auctioned sites inserted at a later date in the schedule (column (7)), although significance levels are somewhat lower here due to the small sample size. All this is consistent with the hypothesis that major firms have used the option to request changes in the auction schedule to get rid of their less attractive sites.

3.4 Propensity score estimation

To arrive at a set of auctioned and a set of non-auctioned stations that are comparable in terms of observable characteristics, we calculate for each site its propensity score: the predicted probability of having been auctioned given a number of observed site characteristics (Rosenbaum and Rubin, 1983). In Table 7 we therefore do a probit regression to explain which sites are likely to be auctioned. We include as explanatory variables all variables in Table 6, except the possibly endogenous site characteristics. This to avoid ex-post matching.[22] The probit estimates are reported in Table 7. We see that sites are less likely to be auctioned if operated by a major brand, if local demand is high, and if local market share is high. Plot size and number of pumps have no significant effect.

INSERT TABLE 7 ABOUT HERE

We construct a selected sample of auctioned and non-auctioned sites that are similar in terms of their observable characteristics by dropping all sites that have propensity score outside the region of common support that runs from 0.035 to 0.676. This results in dropping 8 auctioned and 63 non-auctioned sites. Table 6 shows that differences in observables between the two groups are indeed much smaller for the

[22]That is, matching on variables that may have changed due to the auction.

selected sample. Our OLS estimate of the price effect of the auction will be based on this selected sample.

4 Estimation Strategy

4.1 Introduction and overview

In this section, we describe our estimation strategy in more detail. As explained above, our experiment consists of a series of annual auctions that start in 2002. Before 2006, however, there is also an obligation to divest, while from 2006 onwards that is no longer the case. Effectively, we thus have two different treatments. Our data consists of prices from October 2005 until August 2007. Hence, some sites were already auctioned before, while others were auctioned during our sample period.

Formally, let $D^S_{i,t}$ be a dummy that is 1 if site i has been treated (auctioned) at time $t' \leq t$. Define a dummy $D^{N,Y}_{i,t}$ that is 1 if within distance Y of site i there is a site that has been treated at time $t' \leq t$. As argued in Section 3.2 we choose $Y_H = 5$ for highway and $Y_{NH} = 1$ for non-highway sites. We assume

$$p_{i,t} = f(D^S_{i,t}, D^{N,Y}_{i,t}, X_{i,t}, \epsilon_{i,t}),$$

with $p_{i,t}$ the natural logarithm of price at site i at time t, $X_{i,t}$ is a vector site-specific explanatory variables and $\epsilon_{i,t}$ a vector of unobserved variables. We are primarily interested in the average treatment effect (ATE): the average price effect of the treatment on treated sites, denoted $\tau^S_p \equiv E_{D^{N,Y},X,\epsilon}[f(1, D^{N,Y}, X, \epsilon) - f(0, D^{N,Y}, X, \epsilon)]$.[23] To identify the spillover effect on local competitors, we also look at the average indirect treatment effect (ITE) given by $\tau^N_p \equiv E_{D^S,X,\epsilon}[f(D^S, 1, X, \epsilon) - f(D^S, 0, X, \epsilon)]$. We use a number of alternative approaches to estimate the ATE and ITE:[24] Ordinary

[23]To be precise, the treatment effect estimates the aggregate of the direct effect on prices at the treated site (i.e. due to the transfer of ownership) plus the indirect effects caused by feedback loops from competitors (i.e. the change in ownership at site i urges its local competitors to decrease prices which again triggers a price response by the new operator at site i).

[24]See Angelucci and De Giorgi (2009) for a similar approach to identifying indirect treatment effects for food and non-food consumption using data on an aid program which targets poor Mexican households and is randomized at the village level. Athey *et al.* (2011) use a similar treatment effects approach in their comparison of sealed and open bid auctions.

Least Squares, Instrumental Variables, Difference-in-Difference, and a Difference-in-Difference matching estimator. We discuss each method in turn.

4.2 Ordinary Least Squares

The first approach is to estimate by ordinary least squares the reduced form

$$p_{i,t} = c_t + \beta D^S_{i,t} + \gamma^H I^H_i \cdot D^{N,5}_{i,t} + \gamma^{NH}(1 - I^H_i) \cdot D^{N,1}_{i,t} + \theta X_{i,t} + \epsilon_{i,t}. \tag{3}$$

This is just (2) with the inclusion of the treatment dummies as explanatory variables. In this regression, β identifies the ATE and the γ's the ITEs. The dummy I^H_i is 1 if station i is located along a highway. We thus allow ITEs to differ for highway and non-highway stations. Throughout, errors are clustered at the station level to account for the fact that price observations for a given station are not independent.

4.3 Instrumental Variables

Implicit in our use of OLS is the assumption that the set of treated sites is independent of the unobserved component ϵ conditional on covariates. As argued in the previous section, this is unlikely to hold. Yet, as we also established that the set of sites originally scheduled for auction in a given year can be considered a random draw of all highway stations, we use these initial announcements as an instrument.

We thus construct a dummy $y_{i,t}$ that is 1 if, according to the initial announcement, site i was scheduled to be auctioned at time $t' \leq t$.[25] As initial schedules are random, the use of this instrument yields unbiased estimates of the price effects of the auctions. One possible complication is that initial announcements may have a direct effect on prices, as station owners may respond strategically to them. This would induce a correlation between the instrument and the disturbance $\epsilon_{i,t}$. As there is at least one year between the announcement and the actual auction, we can test for such strategic effects by including an announcement dummy $r_{i,t}$ that is 1 for any time t following the announcement of the auction of site i.

[25] For example, in Table 2, $y_{i,t}$ for *Hendriks* would be 0 for dates t before the date of the 2008 auction and 1 for all dates after that.

4.4 Difference-in-Difference Estimator

For sites auctioned in 2005 or 2006, we can get around the problem of unobservable heterogeneity by comparing pre-treatment to post-treatment prices. If lower prices are entirely driven by selection bias, we should not observe any price changes at a site that is treated. If they are caused by the treatment, we should observe prices to decrease when sites have been treated. If we compare the effects of the 2005 treatment (that consisted of the auction plus the obligation to divest) with the 2006 treatment (that only consisted of the auction), we can also evaluate whether it was the auction or the obligation to divest that caused possible price decreases. We implement this approach by using a difference-in-difference (DID) estimator.

We divide our sample in three periods: $T_{<05}$ is the period before the 2005 auction, T_{05} that after the 2005 but before the 2006 auction, and $T_{>06}$ that after the 2006 auction. Define $S^{<05}$, S^{05}, and S^{06} as the sets of sites auctioned before 2005, in 2005 and in 2006, respectively. S^{05} and S^{06} form our treatment groups. The control group S^C consists of the 134 sites the auctioning of which was not yet announced at the end of the sample period. The average price deviation from the national average at site i in period T_j is given by $\bar{p}_{i,T_j}$, with $j \in \{<05, 05, >06\}$.

To estimate the effect of the 2005 treatment on the sets of sites S^{05} auctioned in 2005, we estimate the least squares regression

$$\bar{p}_{i,T_j} = \eta_0 I(T_j = T_{05}) + [\eta_1 + \alpha I(T_j = T_{05})] I(i \in S^{05}) + u_{i,T_j}, \tag{4}$$

with $I(\cdot)$ indicator functions, $i \in S^{05} \cup S^C$ and $j \in \{<05, 05\}$. The coefficient η_0 captures the part of the price difference in the period before and after the 2005 auction that is common to treatment and control group; η_1 reflects the average price difference between treatment and control group before the 2005 auction. The coefficient of interest is α, which reflects the effect of the 2005 treatment on the sites auctioned in 2005.[26] To establish the effect of the 2006 treatment, we estimate an

[26]The estimator $\hat{\alpha}$ can be expressed as

$$\alpha_{DID} = \frac{1}{N_{S^{05}}} \sum_{i \in S^{05}} \left(\bar{p}_{i,T_{05}} - \bar{p}_{i,T_{<05}}\right) - \frac{1}{N_{\tilde{S}^C}} \sum_{i \in \tilde{S}^C} \left(\bar{p}_{i,T_{05}} - \bar{p}_{i,T_{<05}}\right),$$

equation equivalent to (4) with $j \in \{05, >06\}$, T_{05} replaced with $T_{>06}$ and S^{05} with S^{06}.

If the auctions themselves lead to lower prices we expect $\alpha < 0$ both for the 2005 and 2006 auction. If the auctions only lead to lower prices in combination with an obligation to divest, we expect $\alpha < 0$ in (4) for the 2005 auction, but $\alpha = 0$ when the 2006 auction is evaluated.

To determine the effect of treatments on prices of nearby highway competitors, we conduct a similar analysis for the 148 highway sites within 5 km of some other highway site. Treatment groups S_Y^{05} and S_Y^{06} are sites within 5 km of sites treated in 2005 and in 2006, respectively. The control group S_Y^C consists of sites that do have non-treated sites but no treated sites within 5 km.

4.5 Difference-in-difference Matching Estimator

Finally, we use a difference-in-difference matching (DDM) estimator over the region of common support to see whether pre-treatment prices differ from post-treatment prices relative to sites that have similar observable characteristics (Smith and Todd, 2005). This method controls for observable heterogeneity.

We match each treated site in the selected sample with the two closest non-treated sites and *vice versa*, where closeness is measured in terms of the distance between the propensity scores estimated in Section 3.4. Effectively, the estimator compares prices at treated sites to prices at non-treated sites with similar observable characteristics.

For the 2005 auction, the estimator is

$$\hat{\alpha}_{DDM} = \frac{1}{N^H} \left[\sum_{i \in S} \left(\overline{p_{i,T_{05}}} - \overline{p_{i,T_{<05}}} \right) - \left(\widehat{\overline{p_{i,T_{05}}} - \overline{p_{i,T_{<05}}}} \right) \right], \quad (5)$$

with $\widehat{\overline{p_{i,T_2}} - \overline{p_{i,T_1}}}$ the average price difference at the matched sites.[27] We implement

with N_S the number of elements in S.

[27] As our panel is unbalanced, we compare matched sites in terms of price deviations from the national average rather than in terms of absolute price differences. The latter approach leads to biased estimates when the number of (non)treated sites in periods with high average prices differs from that in periods with low average prices. The national average price at a given date t, $\bar{p}_t$, is the unweighted average of all (highway and non-highway) price quotes for that date in our data.

this estimator and compute robust analytical standard errors following Abadie and Imbens (2006).[28] The estimator for the 2006 auction is similar.

5 Empirical results

5.1 Price effects of the auctions

Table 8 reports the results of our first two approaches. We first focus on the direct effects. Columns (1) and (2) present OLS regressions of the reduced form equation (3) using the selected sample, as explained in Section 3.4. These regressions include the variables of Model B of Table 4 as explanatory variables.

INSERT TABLE 8 ABOUT HERE

Column (1) shows that auctioned sites are on average 0.6% cheaper. This difference is not significant. In column (2) we distinguish between auctioned sites those that did and those did not change ownership in the auction. The largest (but insignificant) effects occur for sites that changed ownership. Their prices are some 1.1% ($= 0.2 + 0.9$) lower. When looking at the full sample in column (3), this price difference grows to 1.6% and becomes significant. However, this estimate does not take into account the heterogeneity in both observable and unobservable characteristics between treated and non-treated sites. Hence the observed lower prices may simply reflect that such sites are inherently less attractive.

To confront this issue, columns (4) and (5) report the results of the second stage of a two-stage least squares regression using the full sample, and using the initial auction announcements as an instrument. The estimates for the direct auction effect are small and far from significant. But note that in this study the selection bias is directly related to the obligation to divest, one of the effects we want to estimate. Arguably, the methods we use to remove selection bias may therefore also remove much of the effect of the divestitures. Hence what we estimate here is primarily the effect of the auction, not that of the obligation to divest. Finally, column (5) includes

[28] We use the psmatch2-module developed for STATA by Leuven and Sianesi (2009).

an announcement dummy to test for strategic effects of the auction announcement. We do not find such an effect.

Summing up, we only find small and insignificant effects on prices of auctioned stations. We only find a significant effect for stations that changed ownership due to the auction if we do not control for heterogeneity.

5.2 Effects of the 2005 and 2006 auctions

Table 9 gives the DID estimates of equation (4) and the DDM estimates in equation (5). Estimated effects are also shown in Figure 1. In this figure, the treatment effect of the 2005 auction equals the drop in the solid green line between "<2005" and "2005-2006"; likewise, the treatment effect of the 2006 auction equals the drop in the dotted blue line between "2005-2006" and ">2006". The figure also shows the effect of the 2005 auction on sites auctioned in 2006, on sites auctioned before 2005, and on sites not yet auctioned. Unsurprisingly, all of these effects are virtually zero. The same holds for the corresponding effects of the 2006 auction.

INSERT TABLE 9 AND FIGURE 1 ABOUT HERE

The table and the figure provide a number of insights. First, average prices of sites treated before 2005 are consistently some 1.5% lower than those in the control group throughout the considered time period. Unsurprisingly, the 2005 and 2006 auctions had no impact on pricing at these sites. Second, for sites treated in 2005, pre-treatment prices are similar to those in the control group. After the 2005 auction, prices at these sites decrease by on average 1.2%, but this decrease is not significant ($p = 0.152$). Figure 1 shows that post-treatment prices are close to those at sites treated before 2005.

Third, pre-treatment prices at sites treated in 2006 are 0.8% higher than those in the control group, but this difference is not significant. The 2006 auction has little impact on prices at sites auctioned in that year; with 0.4%, the average price decrease is small and statistically insignificant. Table 9 also gives the DDM estimates for the treatment effect α. These estimates are very similar in sign and size to the

DID estimates except that the DDM estimate of the effect of the 2005 auction is significant at the 10% level.

Thus, these estimates suggest that sites auctioned before or in 2005 are some 1.5% cheaper than those that are not. The results for the auctions in 2005 suggest that most of this effect is due to the auction, and not the result of sample selection. For the auctions in 2006, we find no effect on prices.

INSERT FIGURE 2 ABOUT HERE

The different effects of the 2005 and 2006 auctions may be caused by the fact that in 2005 the majors still had to divest concessions while in 2006 this was no longer the case. If that is true, the effect of the 2005 treatment should be driven by ownership changes, in turn caused by the obligation to divest. Figure 2 separates the treatment effect of the 2005 auction conditional on whether a site changed ownership. The dashed line represents the total effect and is the same as that in Figure 1. The figure shows no treatment effect for sites that did not change ownership ($p = 0.919$) but a significant decrease of 2.2% for sites that did ($p = 0.033$). Combined with the lack of an effect for the 2006 auction, this suggest that price decreases are indeed fully caused by the forced divestitures.

This 2.2% can be considered a lower bound on the true effect of forced divestiture, for two reasons. First, as Figure 2 shows, pre-treatment prices at sites that did change ownership were already some 1.7% lower than those at sites that did not ($p = 0.111$, one-sided t-test). This suggests that when sites are chosen by the government, the price effect will be higher. Second, as we argued earlier, not all divestitures were done through the auction. Hence there are also highway sites in our control group that were sold because of the obligation to divest.[29] However, the price decrease that we find may not be entirely due to an increase in competition. As we argued, part of the price decrease after a change from a major to a minor firm may be because the new operator is not able to charge a brand premium. In Table 4, we estimated this

[29]We were not able to find out the identity of these sites.

brand premium to 1.5% at most. That leaves a competitive effect of the obligation to divest of at least 0.7%.

5.3 Indirect treatment effects

Table 8 suggests that the auctions did not have a discernable impact on prices of nearby highway competitors. In all specifications the estimated coefficients are negative but insignificant. Interestingly, columns (2) and (3) suggest that the average non-highway site is 1.7% more expensive if it is near a treated site that did not change ownership rather than near one in the control group. Such a difference does not occur when the nearby treated site *did* change ownership ($1.7 - 1.5 = 0.2$). This again suggests that a current tenant is more likely to win the auction if the site is located in an area where stations charge a higher price. In turn, it suggests that firms have mainly selected sites for auction in areas that are more competitive.

INSERT TABLE 10 AND FIGURES 3 AND 4 ABOUT HERE

Table 10 shows the estimates for the indirect treatments for highway sites while Figure 3 shows prices of the three treatment groups S_Y^{05} and S_Y^{06} compared to the control group S_Y^C. Most notably, prices at sites close to a site treated before 2005 are significantly and consistently about 1.3% lower. Again, as we only observe post-treatment prices here, we cannot identify whether this is a competitive effect or a selection effect. With competitive effects, however, we would expect prices at sites close to those treated in 2005 and 2006 to decrease after the auction. Table 10 and Figure 3 do not show a significant effect for the 2005 auction. For highway sites close to sites auctioned in 2006, prices stay flat.

We also split the stations in S_Y^{05} into two groups: those that are close to a site that did change ownership, and those that are not. Figure 4 shows that spillover effects in prices are limited to the first subset. At those sites, prices drop by 1.7% on average, but this is not significant($p = 0.159$), partly due to the small sample size. Remember that in our sample, most ownership changes coincide with a rebranding from a major to a minor brand. With the risk of overinterpreting an insignificant result, we note that our estimate of 1.7% is roughly halfway between Hastings' (2004) estimate of 4% and Taylor *et al.*'s (2010) estimate of 0.3% of the price change due to the rebranding of a competitor from an independent to a major brand.

6 Discussion and conclusion

Governments often try to influence the competitiveness of markets by auctioning licences to operate, or by forcing divestitures. This paper set out to empirically identify the effects of these policies on Dutch market prices for retail gasoline. All licences to operate gasoline stations along public highways will be auctioned. In addition, the four major companies had to divest 48 of their licences before 2006. We constructed a panel data set based on two years of fleet-card price data, supplemented by information on the characteristics of individual sites.

The sites that were initially planned to be auctioned in a given year were a random sample of all highway sites. However, firms were allowed to request changes, and we find strong evidence that sites put up for auction were less profitable than the originally scheduled sites. In this way, firms were able to get rid of less attractive sites without having to bid aggressively for some attractive sites they did not want to lose. For our analysis, it implies that auctioned sites are not a random sample of all highway sites.

Once we account for this selection bias using initial auction announcements as an instrument for actual auctions, any effect of auctions and divestitures on prices disappears. However, with the use of this instrument, we possibly underestimate the effect of the divestitures.

For sites auctioned during our sample period, we have pre-auction as well as post-auction price quotes. This allows us to use a difference-in-difference estimator to get around the selection issue. For sites auctioned in 2005 we find a post-auction price decrease of 1.2%. This is fully driven by sites that changed ownership in the auction: they become 2.2% cheaper. For sites auctioned in 2006, there is no price effect of the auction. Contrary to 2005, an obligation to divest is no longer in place in 2006. This strongly suggests that not the auction itself, but rather the obligation to divest ultimately drove the price effects in 2005. However, since we only find only weak (i.e. statistically insignificant) evidence of competitive spillovers between auctioned sites and price levels at nearby competitors, the observed price decrease at auctioned sites does not seem to be fully driven by increased competition. Lower operating costs of the new operator and/or the disappearance of a brand premium are likely other explanations for the observed drop in prices.

Our analysis suggests that auctioning existing rights to operate on an established market does not increase competition. Auctions augmented with an obligation for incumbent firms to divest some of their rights may have a modest pro-competitive effect.

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Table 1: Comparison of brand shares across Athlon sample and sample Dutch competition authority (NMa).

	All sites		Highway sites	
	Athlon	NMa	Athlon	NMa
Brand	(1)	(2)	(3)	(4)
	Major brands			
Shell	27.75	26.17	15.63	14.50
Esso	17.37	17.58	9.12	8.30
Texaco	14.41	16.41	12.27	12.60
BP	12.92	12.89	9.27	9.10
	Minor brands			
Total	10.17	10.55	12.80	13.50
Q8	6.14	5.08	3.46	3.80
Gulf	3.81	3.13	4.16	6.40
Tinq	1.69	1.56	2.71	1.50
Tamoil	1.27	1.17	2.98	1.80
Argos	0.64	0.78	0.95	0.70
AVIA	0.42		4.69	5.00
Amigo	0.42		0.38	0.40
Brand Oil	0.21	1.56	1.07	1.30
Firezone	0.00	0.39	0.79	0.60
Elan	0.00		0.77	
other	2.75	2.73	18.94	20.50
Total sites	3585	4319	237	256

Notes: Brand shares are given in percentages and calculated on the basis of the number of outlets. In columns (1) and (3), outlets that changed brand name during the period of data collection are counted as one half for each of the brands involved. Brand shares reported in columns (2) and (4) are from NMa (2006, p. 10 and p. 13)

Table 2: Example of changes in the auction scheme.

	Staatscourant 2001		Staatscourant 2002		Staatscourant 2003	
Name	*Year*	*Current owner*	*Year*	*Current owner*	*Year*	*Current owner*
Hendriks	2008	Total	2008	Total	2008	Total
Shell Bergh-Z			2009	Shell	2009	Shell
BP Witte Paarden			2009	BP		
BP Hoefplan					2009	BP
Esso Wons					2009	Esso

Table 3: Local market concentration: number of (non-)highway sites within Y kilometer.

		number of other sites within...					
		0.1 km	1 km	2 km	5 km	10 km	20 km
		other non-highway sites					
Non-highway sites (3348)	min.	0	0	0	0	0	1
	mean	0.05	1.14	3.56	13.77	42.85	141.87
	median	0	1	3	11	38	132
	max.	2	10	18	55	142	389
		highway sites					
	min.	0	0	0	0	0	0
	mean	0.00	0.02	0.10	0.73	2.72	9.39
	median	0	0	0	0	2	9
	max.	1	4	4	6	11	22
		non-highway sites					
Highway sites (237)	min.	0	0	0	0	0	7
	mean	0.00	0.20	1.42	10.41	39.34	136.19
	median	0	0	1	8	34	129
	max.	1	3	10	34	119	356
		other highway sites					
	min.	0	0	0	0	0	1
	mean	0.02	0.53	0.60	0.98	2.40	8.98
	median	0	1	1	1	2	9
	max.	1	2	2	4	6	21

Table 4: Regression of (log) price on explanatory variables.

	Model A		Model B	
	coefficient	**s.e.**	**coefficient**	**s.e.**
Geographical characteristics				
highway*2005	0.0255**	(0.0026)	0.0276**	(0.0068)
highway*2006	0.0272**	(0.0026)	0.0293**	(0.0068)
highway*2007	0.0288**	(0.0026)	0.0309**	(0.0068)
German border	-0.0009	(0.0019)	-0.0024	(0.0018)
Belgian border	0.0086**	(0.0021)	0.0074**	(0.0021)
Site characteristics				
Company owned	-0.0101**	(0.0007)	-0.0090**	(0.0007)
Major	0.0151**	(0.0009)	0.0153**	(0.0009)
Major*highway	0.0033	(0.0029)	0.0031	(0.0029)
Express	-0.0269**	(0.0015)	-0.0263**	(0.0015)
Total	0.0157**	(0.0011)	0.0151**	(0.0011)
Q8	0.0141**	(0.0018)	0.0143**	(0.0018)
Plot size (area)	3.36e-07†	(1.92e-07)	-1.26e-07	(6.53e-07)
# pumps	0.0005†	(0.0003)	0.0006*	(0.0003)
hotdrinks	0.0016	(0.0010)	0.0015	(0.0010)
shop area	7.72e-06	(1.55e-05)	1.68e-05	(4.35e-05)
Local market concentration				
ln(# non-highway sites+1) at...				
$\leq$ *1 km*(1-highway)*			-0.0021**	(0.0007)
$1-2$ *km*(1-highway)*			-0.0038**	(0.0006)
$2-5$ *km*(1-highway)*			-0.0005	(0.0005)
$5-10$ *km*(1-highway)*			-0.0004	(0.0009)
$\leq$ *1 km*highway*			-0.0159**	(0.0053)
$1-2$ *km*highway*			-0.0020	(0.0023)
$2-5$ *km*highway*			-0.0011	(0.0019)
$5-10$ *km*highway*			-0.0018	(0.0025)
ln(# highway sites+1) at...				
$\leq$ *1 km*(1-highway)*			0.0043	(0.0050)
$1-2$ *km*(1-highway)*			0.0005	(0.0018)
$2-5$ *km*(1-highway)*			0.0014†	(0.0007)
$5-10$ *km*(1-highway)*			0.0004	(0.0006)
$\leq$ *1 km*highway*			0.0002	(0.0038)
$1-2$ *km*highway*			0.0107†	(0.0056)
$2-5$ *km*highway*			0.0063*	(0.0025)
$5-10$ *km*highway*			0.0003	(0.0020)
Local demand				
$\ln(\#priv.ownedcars \leq 20km)$	0.0055**	(0.0005)	0.0073**	(0.0010)
Day dummies	YES		YES	
R^2	0.3014		0.3234	
obs	*1039363 (full sample)*		*1039363 (full sample)*	

Notes: Plot size area and shop area in sq. m.; privately owned cars in '000. Standard errors are clustered at the station level; Controls for missing site characteristics have been included; **(*/†) denotes significance at the 1% (5%/10%) level.

Table 5: Movements in the scheduled year of auction and auction outcome.

Position in schedule...	2002	2003	2005	2006	2007	**total**	**average highest bid (mln. €)**
				No change in ownership			
...unchanged	5	3	6	6	5	**25**	**4.33**
...postponed					1	**1**	**2.31**
...inserted					1	**1**	**10.04**
average highest bid							**4.46**
				Change in ownership			
...unchanged	4	3	1		1	**9**	**3.97**
...postponed						**0**	–
...inserted		4	6	1	1	**12**	**1.02**
average highest bid							**2.28**
Change in ownership from...							
...major to major		1	1		1	**3**	**7.04**
...major to minor	3	6	4	1	1	**15**	**1.68**
...minor to major						**0**	–
...minor to minor	1		2			**3**	**0.51**
Change in ownership among...							
...all auctioned sites	44%	70%	54%	14%	22%	**44%**	
...unchanged sites	44%	50%	14%	0%	17%	**26%**	
...postponed sites					0%	**0%**	
...inserted sites		100%	100%	100%	50%	**92%**	
Sites inserted among...							
...all auctioned sites	0%	40%	46%	14%	22%	**27%**	
...sites that changed ownership	0%	57%	86%	100%	50%	**57%**	

Columns 2002–2007 are grouped under "Year of Auction".

Table 6: Summary statistics characteristics highway sites (as measured per end of sample period).

	All Sites		Government Announcements		Auctioned Sites						Non-auctioned Sites			
					Full Sample		Inserted Sites		Selected Sample		Full Sample		Selected Sample	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Geographical characteristics														
German border	0.03	(0.18)	0.04	(0.20)	0.05	(0.22)	0.09	(0.30)	0.06	(0.25)	0.03	(0.17)	0.04	(0.21)
Belgian border	0.04	(0.19)	0.06	(0.24)	0.10*	(0.31)	0.09	(0.30)	0.10	(0.30)	0.03	(0.16)	0.03	(0.17)
Site characteristics‡														
Company owned	0.89	(0.31)	0.95*	(0.22)	0.87	(0.34)	0.73†	(0.47)	0.90	(0.30)	0.89	(0.31)	0.90	(0.31)
Major brand[1]	0.74	(0.44)	0.68†	(0.47)	0.54**	(0.51)	0.55	(0.52)	0.55	(0.51)	0.79	(0.41)	0.72	(0.45)
Car wash	0.02	(0.13)	0.00†	(0.00)	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)	0.02	(0.14)	0.00	(0.00)
Plot size (area)	3753	(2936)	3529	(1815)	2872*	(1710)	1673*	(1227)	3258	(1555)	3927	(3095)	3304	(1618)
# pumps	3.79	(2.04)	3.51†	(1.68)	3.00**	(1.56)	2.27*	(0.79)	3.19	(1.64)	3.95	(2.09)	3.40	(1.33)
hotdrinks	0.93	(0.25)	0.96	(0.20)	0.82**	(0.39)	0.64**	(0.50)	1.00	(0.00)	0.95	(0.21)	1.00	(0.00)
shop area	86.22	(26.82)	87.55	(25.79)	82.34	(27.59)	63.57*	(23.22)	83.71	(26.92)	86.87	(26.71)	83.21	(24.35)
Local market concentration														
# highway sites at...														
≤ 1 km	0.54	(0.53)	0.57	(0.50)	0.56	(0.55)	0.36	(0.67)	0.58	(0.50)	0.54	(0.53)	0.57	(0.51)
1 – 2 km	0.07	(0.30)	0.05	(0.22)	0.05	(0.22)	0.09	(0.30)	0.03	(0.18)	0.08	(0.32)	0.06	(0.27)
2 – 5 km	0.36	(0.74)	0.34	(0.70)	0.31	(0.69)	0.55	(0.93)	0.32	(0.70)	0.38	(0.75)	0.36	(0.73)
5 – 10 km	1.39	(1.38)	1.12*	(1.23)	0.77**	(0.96)	0.64†	(0.81)	0.81	(0.98)	1.52	(1.41)	1.22	(1.23)
# non-highway sites at...														
≤ 1 km	0.20	(0.52)	0.17	(0.48)	0.18	(0.60)	0.36	(0.92)	0.06	(0.25)	0.20	(0.50)	0.10	(0.30)
1 – 2 km	1.22	(1.54)	1.06	(1.34)	0.87	(0.98)	1.09	(1.22)	0.84	(1.00)	1.29	(1.62)	1.08	(1.54)
2 – 5 km	9.04	(6.58)	8.36	(6.52)	7.10*	(5.73)	6.91	(6.30)	7.45	(5.80)	9.42	(6.69)	8.30	(6.16)
5 – 10 km	29.10	(17.12)	29.20	(17.83)	23.38*	(15.83)	17.27*	(7.40)	24.13	(17.41)	30.23	(17.18)	27.14	(14.89)
Local demand														
# priv. owned cars ≤ 20km	272	(173)	249†	(172)	181**	(132)	133**	(91)	189	(140)	290	(175)	234	(136)
Possibly endogenous site characteristics														
Estimated volume sold‡	5035	(3292)	5166	(3310)	4441	(3159)	2777*	(1928)	4934	(3196)	5149	(3313)	4553	(2543)
Estimated shop sales‡	1160	(1072)	1150	(1011)	1096	(1105)	661	(570)	1111	(1120)	1171	(1069)	1013	(841)
own market share	0.25	(0.16)	0.23	(0.16)	0.27	(0.21)	0.23	(0.14)	0.29	(0.23)	0.24	(0.15)	0.24	(0.14)
local HHI	2062	(1438)	2089	(1582)	2601**	(2186)	2225	(1067)	2540	(2265)	1956	(1217)	2023	(1232)
# sites	236		98		39		11		31		197		134	
of which inserted	11		0		11		11		6		0		0	
of which postponed	11		11		0		0		0		11		9	

Notes: Government Announcements: Initial announcements only (not 'inserted' announcements following a firm request)
Auctioned sites: sites auctioned in 2002, 2003, 2005 or 2006. Plot size area and shop area in sq. m; volume in '000 litres per annum; shop sales in €'000 per annum; privately owned cars in '000; own market share and HHI: based on market shares of the 18 main brands within 5 km distance.
Government Announcements, Auctioned Sites (Full Sample) and Inserted Sites: †,*,**: statistically different from all remaining sites at 10, 5 and 1 percent respectively.
[1] : for auctioned stations: pre-auction value; ‡: Data provided by Catalist Ltd.

Table 7: Probit regression of auction dummy on explanatory variables.

Sample:	Full	
Mean (Dependent Variable):	0.0991	
	coefficient	s.e.
Geographical characteristics		
German border	0.146	(0.180)
Belgian border	0.390*	(0.206)
Site characteristics		
Company owned	-0.021	(0.079)
Major brand[1]	-0.127*	(0.065)
Plot size (area)	-6.30e-06	(1.31e-05)
# pumps	-1.88e-04	(0.016)
hotdrinks[2]	-	-
shop area	-2.10e-04	(9.43e-04)
Local market concentration		
# highway sites at...		
≤ 1 km	0.015	(0.044)
1 – 2 km	-0.090	(0.098)
2 – 5 km	0.005	(0.032)
5 – 10 km	-0.038*	(0.018)
# non-highway sites at...		
≤ 1 km	-0.108	(0.069)
1 – 2 km	0.004	(0.018)
2 – 5 km	0.005	(0.004)
5 – 10 km	0.002	(0.002)
Local demand		
# priv. owned cars ≤ 20km	-0.055†	(0.028)
# obs.	216	
Pseudo R^2	0.186	
P-value	0.006	
	LR chi2(16)	33.77

Note: Reported results are marginal effects. Plot size area and shop area in sq. m; volume in mln. litres per annum; shop sales in €'000,000 per annum; privately owned cars in '000.000; own market share and HHI: based on market shares main brands within 5 km distance.

[1]: for auctioned stations: pre-auction value; [2]: this variable is dropped because all sites in the sample supply hot drinks. **(*/†) denotes significance at the 1% (5%/10%) level.

Table 8: Direct and indirect price effects auctions. Dependent variable: $p_{i,t}$

	OLS			IV	
	(1)	(2)	(3)	(4)	(5)
	Own price effect				
Direct auction effect	-0.0065	-0.0024	-0.0017	-0.0024	-0.0017
	(0.0040)	(0.0055)	(0.0054)	(0.0042)	(0.0048)
Auctioned×Change		-0.0092	-0.0144*		
		(0.0076)	(0.0072)		
Post announcement					-0.0027
					(0.0026)
	Effect on price competitors				
Non-highway stations ≤ 1 km					
Indirect auction effect (γ^{NH})	0.0084	0.0170†	0.0171†	0.0184*	0.0129
	(0.0078)	(0.0088)	(0.0087)	(0.0092)	(0.0153)
Indirect effect×Change		-0.148	-0.0150		
		(0.0096)	(0.0095)		
Post announcement					0.0086
					(0.0144)
Highway stations ≤ 5 km					
Indirect auction effect (γ^{H})	-0.0036	-0.0006	-0.0028	-0.0033	-0.0049
	(0.0035)	(0.0041)	(0.0038)	(0.0038)	(0.0043)
Indirect effect ×Change	-0.0044	-0.0028			
		(0.0061)	(0.0053)		
Post announcement					0.0034
					(0.0027)
Day dummies	YES	YES	YES	YES	YES
Instruments	-	-	-	Init.	Ann.
R^2	0.2981	0.2987	0.3225	0.3203	0.3211
obs	*1011678*	*1011678*	*1039363*	*1039363*	*1039363*

Notes: Specifications include fixed day effects and the same set of explanatory variables as Model B in Table 4 with one exception: the major dummy used here is 1 if the site was owned by a major firm *at the time of the auction announcement.* Controls for missing site characteristics have been included. Estimates in columns (3), (4) and (5) include all price observations. Estimates in columns (1) and (2) include all non-highway sites and highway sites with a propensity score in the interval [0.035, 0.676].

[1] for auctioned stations: value at time announcement;

Standard errors are clustered at the station level; **(*/†) denotes significance at the 1% (5%/10%) level.

Table 9: Direct price effect auctions: Difference-in-difference estimates.

Treatment group	DID η_1	DID α	DDM α
Sites auctioned in 2005	-0.0015 (0.0056)	-0.0115 (0.0080)	-0.0085† (0.0049)
Sites auctioned in 2006	0.0085 (0.0081)	-0.0039 (0.0110)	-0.0000 (0.0016)

Notes: DID estimates: the treatment group is the group of all sites auctioned in either 2005 or 2006; the control group is formed by the set of 134 highway sites the auctioning of which was not yet announced at the end of the sample period. η_1 estimates price differences between the treatment and the control group before the policy change occurs and α is the DID-estimator of the effect of the auction on the treatment group.
DDM estimates: the treatment group is the selected set of sites auctioned in either 2005 or 2006 of which the characteristics are reported in columns 6 and 7 of Table 6; the control group is the selected set of 134 sites of which the characteristics are reported in columns 10 and 11 of Table 6. $^{**}(^*/^\dagger)$ denotes significance at the 1% (5%/10%) level.

Table 10: Indirect price effect auctions: Difference-in-difference estimates.

Treatment group	DID η_1	DID α	DDM α
Sites within 5 km site auctioned in 2005	0.0038 (0.0060)	-0.0060 (0.0085)	-0.0054 (0.0047)
Sites within 5 km site auctioned in 2006	0.0037 (0.0065)	0.0014 (0.0092)	0.0016 (0.0013)

Notes: DID estimates: the treatment group is the group of all sites within 5 km of a site auctioned in either 2005 or 2006; the control group is formed by the set of 116 sites with at least one other highway site within 5 km distance, but none of these having been auctioned at the end of the data collecting period. η_1 estimates price differences between the treatment and the control group before the policy change occurs and α is the DID-estimator of the effect of the auction on the treatment group.
DDM estimates: the treatment group is the group of all sites within 5 km of a site, belonging to the selected sample, auctioned in either 2005 or 2006; the control group is formed by the set of 76 sites in the selected sample with at least one other highway site within 5 km distance, but none of these having been auctioned at the end of the data collecting period. $^{**}(^*/^\dagger)$ denotes significance at the 1% (5%/10%) level.

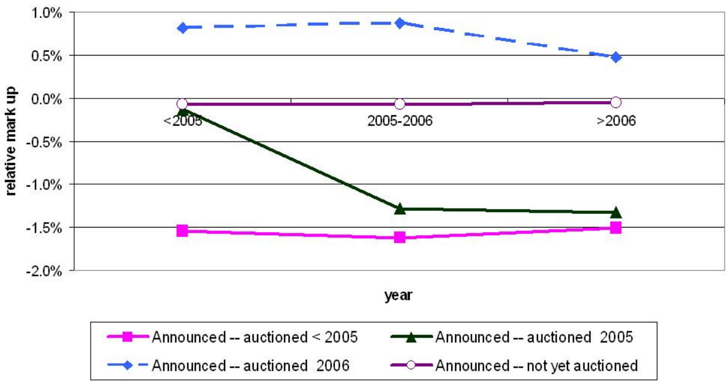

Figure 1: Treatment effect on treated.

Note: 18 highway sites in the sample were auctioned before 2005; 13 in 2005; 7 in 2006; 63 site were announced but not yet auctioned after the 2006 auction; the relative mark up is measured against the 134 highway sites the auctioning of which was not yet announced at the end of the sample period.

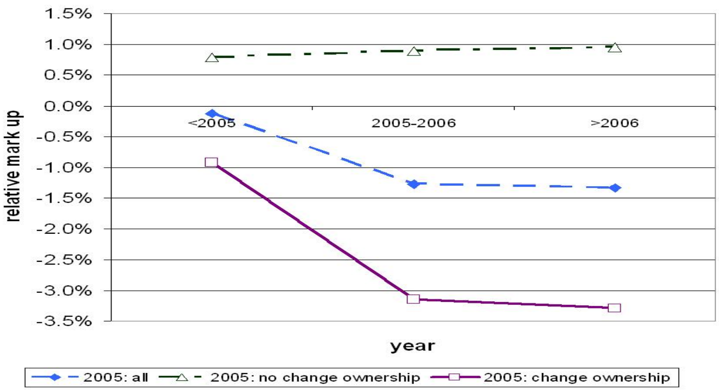

Figure 2: Treatment effect on treated: 2005 auction.

Note: 13 highway sites in the sample were auctioned in 2005; 7 of these changed ownership in the auction, 6 did not. The relative mark up is measured against the 134 highway sites the auctioning of which was not yet announced at the end of the sample period.

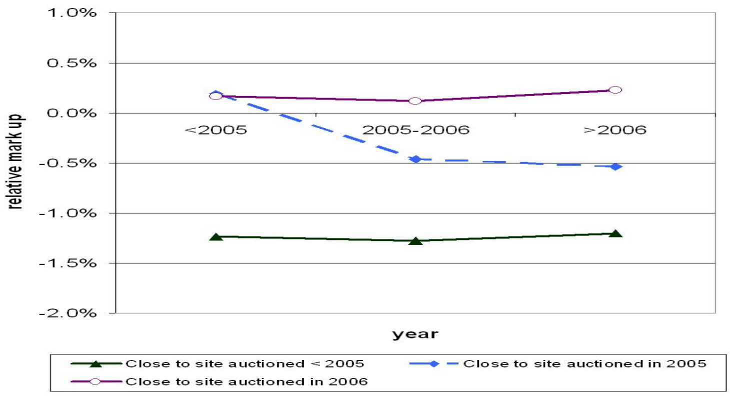

Figure 3: ITE on other highway sites within 5 km. auctioned site.

Note: 13 highway sites are within 5 km of a site auctioned before 2005; 10 within 5km from a site auctioned in 2005 and 10 within 5 km from a site auctioned in 2006. The relative mark up is measured against the set of 116 highway sites with at least one other highway site within 5 km distance, but none these having been auctioned at the end of the sample period.

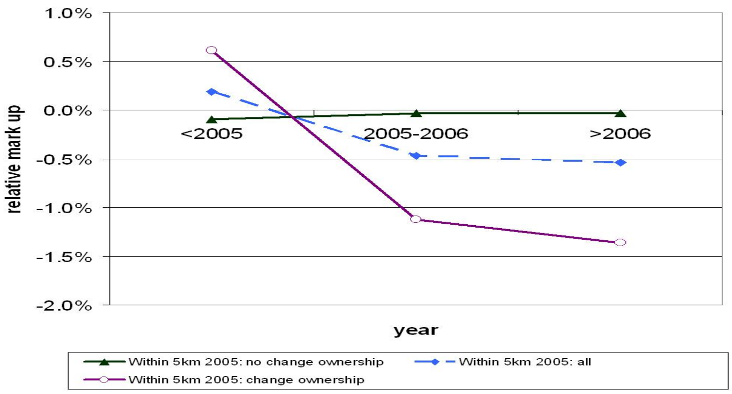

Figure 4: ITE on other highway sites within 5 km. auctioned site: 2005 auction.

Note: 10 highway sites are within 5 km of a site auctioned in 2005; 4 of these changed ownership in the 2005 auction, 6 did not. The relative mark up is measured against the set of 116 highway sites with at least one other highway site within 5 km distance, but none these having been auctioned at the end of the sample period.