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The Impact of Process Innovation on Prices:

Evidence from Automated Fuel Retailing in The Netherlands*

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Abstract

In the last decade, many European countries have seen a sharp increase in the number of automated fueling stations. We study the effect of this process innovation on prices at stations that are automated and their competitors using a difference-in-differences matching strategy. Our estimates show that prices at automated stations drop by 1.7 to 3.2% immediately after conversion and stabilize at this lower level. Unlike previous studies, our estimates do not reveal a difference in impact between early and later adopters of automation. Indicative of competitive spillovers, prices at stations within 2 km of an automated station decrease on average with a precisely estimated 0.2%.

Keywords: technology adoption, retail gasoline markets, pricing
JEL classification: C22, L13, L81

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1 Introduction

In the last decade, unstaffed or automated fueling stations have proliferated across Europe. Automated fueling sites are completely unstaffed and payment is by debit or credit card at the pump only. According to one large study, 7.7% of all service stations in the European Union were unstaffed in 2012, but with large cross-country differences.[1] In Scandinavian countries such as Denmark and Sweden the share of automated stations was found to be over 60% in 2012 while in other countries such as Italy and Hungary it was less than 1%.

Declining fuel volumes and a desire to cut fixed staffing cost seems to drive the increased activity of converting service stations into unstaffed sites.[2] At converted sites, this may lead to lower price levels if realized cost reductions are partially or fully passed onto consumers. There may also be competitive spillovers in the form of lower prices at nearby competitors. The aim of this study is to empirically estimate the extent to which these direct and competitive effects on prices are statistically and substantially significant.

The literature on technology evolution distinguishes between product and process innovations. Whereas in the early stage of a product technology's life cycle product innovation is the most important mode of innovation, in the later stages when the product design stabilizes, process innovations that enable firms to produce the same output using less input take over as the dominant innovation mode (Adner and Levinthal, 2001). The transition to automated fuel retailing is a good example of a process innovation in an industry in the later stages of its technology cycle. In the Netherlands, full service stations where an attendant at the forecourt delivers fuel into the customer's car disappeared in the 1970s and 1980s. Nowadays, all staffed stations are self service stations where customers pump their own gas.[3]

Given the rise of non-cash debit and credit card payments in everyday transactions,

[1] Civic Consulting (2014, Table 89). Four regions in Spain have recently blocked the rise of unstaffed stations by adopting legislation that requires all service stations to have at least one employee present during opening hours. This happened following pressure from an alliance of employers, unions and consumers that cited potential safety risks, job losses and barriers to people with disabilities. `http://economia.elpais.com/economia/2015/02/06/actualidad/1423251729_289297.html`.

[2] See CBRE (2012).

[3] The same development has happened in many other countries. Basker et al. (2017) study the adoption of self service pumps in the U.S between 1977-1992 and show how this shifting of work to customers biases upward conventional measures of labor productivity.

the adoption of automats that allow for payment at the pump instead of at a cashier only is a natural next step. Automation lowers a firm's cost while leaving the physical product that consumers receive unaltered. Moreover, whereas the impact of process innovations on prices is mostly hard to measure because either the innovations themselves are invisible and/or price data are not easily available, retail gasoline markets are instead very favorable to studying this question empirically. First, automation is a visible event which allows us to pin down the date of automation rather precisely. Second, fuel prices are readily available at the station-level and cleanly measured. This allows us to use stations as the unit of observation in our analysis. Third, because oil companies already frequently adjust their retail prices to reflect fluctuations in the price of inputs (notably the oil spot price), changing prices due to automation does not involve any additional menu cost. The absence of menu cost increases the likelihood that price effects, if any, become apparent shortly after the event of automation. Despite these advantages, we are the first to address the impact of automated fuel retailing on prices.

Our paper is related to other papers on technology adoption in retail markets, such as Foster, Haltiwanger and Krizan (2006), Basker (2012, 2015) and Carranza, Clark and Houde (2015). Foster *et al.* (2006) compare the productivity gains across multiple retail sectors. They find that most of the gains in labor productivity are due to the entry of more productive firms that replace less productive ones. As in the current study, Carranza *et al.* (2015) turn to gasoline markets and study the effect of price floor regulations on technology adoption. They find that such regulations reduce efficiency by barring the entry of low-cost retailers but do not increase prices. Basker (2012) studies the effects of introducing barcode scanners on labor productivity and prices. Basker (2012) finds that the introduction of scanning reduces the wage bill by 4.5%.

Most related to our work is Basker (2015), in which she considers the impact of scanners on the prices of various grocery products. She estimates that these prices have decreased by at least 1.4% because of the introduction of scanners. As with the introduction of scanners, the event of fuel station automation speeds up the checkout process and there is no impact on the physical product received by consumers, which Basker (2015) calls a "pure" process innovation.[4] Also, similar to the introduction of scanning in the U.S. retail sector, the diffusion of automation in the Netherlands happened gradually. This phase-in feature makes it possible to identify the price effect of automation by comparing the price levels before and after automation of treated sites with those of a suitably selected set of control stations that did not experience automation. The detection of treatment effects is possible as long as the price effects of automation materialize quickly enough relative to time for the phase-in to complete (Duflo, Glennerster and Kremer, 2007). The empirical estimates show that this condition holds in our case.

We employ an extensive data set that contains price quotes of about 85% of all outlets in the Netherlands for the time period 10/2005-12/2013. The Dutch market is an interesting market to consider because with a market share for automated stations that grows from 12.5% in 2005 to 29.2% in 2013, it can be classified as a market in transition: The share of automated stations is sufficiently large for its economic impact to be estimable. At the same time, the share is sufficiently small not to be considered a mature automated fuel retailing market that has already reached a new steady state equilibrium. As a consequence, our study focuses on the short-run effects of automation, i.e. the price effects in the stage of development where adoption of automated fuel retailing is at about 30% at the end of the sample period. Table 1 shows that the share of automated stations has steadily increased in the period considered with no signs of this trend leveling off near the end. With the proportion of unstaffed sites at 12.5% in October 2005, the Dutch market has witnessed more than a doubling of the proportion of unstaffed stations in the period we consider.

Our estimation sample of 2,934 individual off-highway stations with on average more than 1,800 price quotes per station allows us to include day fixed effects in all our regressions. These pick up time-variant shocks common to all sites, such as price fluctuations due to developments in the international oil market. Next to that, station-level fixed effects are included such that identification of our key parameters is based on within-site

[4]Basker (2015) notices that next to a faster checkout, scanning may save on labor cost because the technology eliminates the need to place price stickers on each item in stock. As a consequence scanning stores are able to change prices more frequently than non-scanning stores. Such a difference does not apply to automated and non-automated fuel stations.

variation. This is a notable difference with Basker (2015) who is constrained in her analysis by the fact that the scanner installation mostly happened in the 1970s and 1980s, a period for which only quarterly city-level average price data are available. As a result, she cannot make pre- and post-installation price comparisons at the store level nor study the competitive spillovers to nearby stores. The high-frequency nature of our price data allows us to match the automation events to pre- and post-automation price data at the site level and to investigate the presence of competitive spillovers.

We use a difference-in-difference matching strategy that creates balance between the treatment group of stations that are automated and a control group of stations that do not experience automation. We match treated and control units using the propensity score. The fact that new price quotes arrive almost daily enables a precise identification of post-event price movements in the weeks following automation. This informs us whether unit cost reductions are passed through to consumers immediately or with a lag.

Our main empirical findings are the following. First, our difference-in-difference matching estimates show that automation reduces the prices by an average 2.1-4.0 eurocents per liter (cpl) before taxes. This corresponds to a decrease of 1.7 to 3.2% in the price paid by consumers.[5] Second, the price adjustment happens instantly in the week of automation and the prices stay at these lower levels in the months following. Third, the estimates and the coefficient plots of lagged price responses show statistically significant competitive spillovers to neighboring service stations within 1 km and 2 km radius. On average, prices at stations within 1 km of a site that is automated decrease by 0.27%; for all stations within a 2 km range, the average impact is a 0.19% decrease in prices. Finally, in contrast to Basker (2015), who shows that early adopters of scanning infrastructure contribute disproportionately to the observed price decreases, we find that the impact of automation is similar for service stations that were automated in different points of time. A main reason for this difference in finding is provided by the different nature of the innovation. Whereas the introduction of scanning opens up possibilities for grocery stores to adopt complementary processes that help to optimize the use of inputs, such as

[5]Including the VAT of 19%, the reduction is 2.5-4.8 cpl or, compared to the average retail price of €1.52 in the period considered, 1.7 to 3.2%. For the introduction of scanners, Basker (2015) finds an average price decrease of at least 1.4%.

tracking sales and changing worker schedules, automation of fuel stations offers much less scope for such complementary innovations.

The paper continues as follows. Section 2 provides a classification of retail fueling services to elucidate the differences between staffed and unstaffed stations. Section 3 introduces the data. Section 4 outlines our estimation strategy and Section 5 gives the main results. Section 6 concludes.

2 Retail fueling services

Three main forms of fueling services can be distinguished. At full service stations, an attendant is present at the forecourt who delivers fuel into the customer's car. Next to that, the attendant may offer other services such as cleaning the windows, checking oil, tyres etc. At self service stations, there is no such attendant and customers pump their own fuel. Payment is to a cashier sitting in a shop or booth. For security reasons, it is forbidden for this employee to leave the shop which rules out the performance of services at the forecourt similar to those of attendants at full service stations. The defining characteristic of unstaffed or automated stations is that payment in the shop or booth is not possible. Instead, automats are installed at each pump (or pair of pumps) that enable customers to pay by credit or debit card immediately without visiting the shop or booth.

This classification into self service stations and automated service stations does not strictly delineate stations with and without a shop or stations with and without the possibility to pay at the pump. Especially larger self service stations may have a limited number of one or two pumps equipped with an automat for credit and debit card payments that can be used either 24/7 or only outside the staffed opening hours. A shop may be present at automated stations. In some cases, this shop is operated by the same company that operates the forecourt; in other cases, the shop and forecourt are operated by different companies. In all these cases, shop and fuel sales are however strictly separated in that customers cannot pay for fuel in the shop. At some automated stations without a shop, basic food and non-food items (such as soft drinks, candy bars and cigarettes) can nevertheless be bought at vending machines.

In the Netherlands, full service stations have long given way to stations that operate under either a self service or automated service concept.[6] Unstaffed retailing started in 2000 when the company Tango opened the first unstaffed station of the country. Tango – the brand name is a combination of the words "tank" and "go" – was explicitly founded on the premise that enabling customers to pay directly at the pump would reduce the time to complete a transaction because customers no longer have to queue for payment in the shop or booth. In turn, the high degree of automation lowers cost which the company promises "to return to the customer in the form of a discount."[7]

The possibility and need to pay at the pump has been the defining characteristic that distinguishes all automated fuel stations that have established since then (Section 3.2 gives exact numbers on how many stations had been automated in the period we consider). The change in payment method at the day of automation may be accompanied by other changes such as a change in brand name or a change in shop activities. In the analysis, we account for any brand name changes. Unfortunately, we lack station-level information on changes that happen to the layout or operation of the shop at the time of automation. Our own casual observations suggest that especially small shops up to 15 m^2 – where shop sales are secondary to handling fuel payments – are closed after automation. In the empirical analysis, we will account for this by presenting estimates that condition on the presence or absence of a shop in the pre-automation period.

3 Data

3.1 Data collection and sample selection

For our empirical analysis, we use fleet-card data provided by Athlon Car Lease (Athlon, hereafter), the leading car leasing company in the Netherlands with a fleet of over 125,000 cars.[8] Every day, Athlon captures station-specific retail gasoline prices using information retrieved from fleet-card users who frequent these stations. Athlon's methodology of

[6]At the end of 2011, Shell announced a local test to re-introduce the attendant at one of its stations. (`https://www.tankpro.nl/pompshop/2011/10/07/pompbediende-gaat-weer-helpen-bij-tanken/`). This results did not lead to a wider introduction of this concept. In fact, station owners commonly talk of "full service stations" when referring to self service stations with a shop that sells a wide assortment of items that may include evening meals, snacks, premium coffee, fresh vegetables, etc.

[7]See `https://www.tango.nl/overtango` (visited April 29, 2017) for a short history of Tango and their business concept. In 2004, Tango was taken over by Kuwait Petroleum.

[8]Data from the same source have been used in Soetevent *et al.* (2014) and Heijnen *et al.* (2015).

using card data is very similar to the one used by OPIS, an agency that provides detailed information on gasoline retail prices for the US market.[9] The scope of the data in terms of coverage and frequency of price quotes is also very similar. In total, price quotes of 3,978 different sites are collected which implies a coverage of about 85 percent of all outlets in the country. This number is comparable to the coverage by OPIS of around 90 per cent (Chandra and Tappata, 2011).[10]

We focus on the sales of regular unleaded 95 octane gasoline, known as Euro 95, at off-highway stations. Euro 95 is the most commonly used type of fuel in the Netherlands. We limit attention to off-highway stations for the following two reasons: In two competition cases, the European Commission has argued that highway and off-highway stations constitute separate product markets.[11] Consistent with this, our data shows that highway stations charge prices that are structurally higher than prices at off-highway stations.[12] Moreover, whereas the market share of automated fueling stations has steadily grown off-highway (from 12.5% in Oct. 2005 to 29.2% in Oct. 2013), growth remains modest at the highway.[13]

Our sample covers the time period between October 1, 2005 to December 31, 2013. Besides the price observations of the 243 highway sites, we also drop the price quotes of all 801 sites that entered and/or exited the market in this time period. We drop these sites because our difference-in-difference approach uses price quotes before and after automation and for stations that enter as an automated fueling station, pre-conversion price quotes are lacking by definition. We do include these sites in our calculations of a station's number of local highway and off-highway competitors.

[9]Data collected by the Oil Price Information Service (OPIS) have been widely used in applied papers on pricing and price strategies, examples include Taylor and Hosken (2007); Doyle and Samphantharak (2008), Chandra and Tappata (2011), Myers et al. (2011), Lewis (2012, 2014) and Tappata and Yan (2017).

[10]According to TankPro.nl there were 4,206 gasoline stations in the Netherlands in June 2011, `http://www.tankpro.nl/brandstof/2011/11/30/aantal-tankstations-in-nederland-blijft-stabiel/`. Original source: PetrolView. We count 3,562 active sites in February 2011.

[11]See e.g. European Union (1999), where it is argued in the Exxon/Mobil case that 'in some countries, it is possible to consider fuel retailing on motorways as a separate product market' (point 436). The Dutch antitrust agency has also mentioned differences in regulations and policies on approval for highway and off-highway stations (NMa, 2004).

[12]See Figure B2 in Heijnen *et al.* (2015).

[13]At the highway, the market share of automated fueling stations has increased from 2.6% in Oct. 2005 to 6.8% in Oct 2013

This leaves us with a final sample of 5,335,785 price observations of 2,934 selected off-highway stations. With more than eight years of data, our panel of station-level prices is longer than most daily-price panels that have been used in this literature.[14] Throughout we consider retail prices before excise and value-added taxes. The price data is supplemented with information about the geographic coordinates of the station and the (Euclidean) distances between all pairs of stations.[15] Information on a number of mostly time-invariant station characteristics has been obtained from Experian Catalist Ltd. (the type of ownership, availability of a car wash, the number of pumps etc.). These are used in calculating the propensity scores to find the appropriate control stations.

As in studies using OPIS data, one potential drawback is that prices are not reported for all stations for all days (Tappata and Yan, 2017, p. 205). For stations in our sample, on average price quotes get refreshed every 2.8 days. This again compares well to the widely used OPIS data.[16] This may potentially bias our estimates if the data are not missing at random. Although the missing-at-random assumption is not refutable using the data alone[17] there is reason to suspect that missing data do not impact our results. First, one potential source of non-randomness is that drivers structurally avoid visiting the higher-priced stations which would bias our sample of observed prices towards the lower-end of the price distribution. Although this may be a concern in data composed of transaction data of private drivers, this arguably is less of an issue with fleet card data. Lessees do not pay for the fuel themselves which makes them rather unresponsive to prices. Second, the inclusion of day-specific fixed effects accounts for any bias caused by lessees having a different pattern of frequenting fuel stations than other drivers (they may for example drive relatively less in the weekend). Furthermore, only one lessee per day has to visit a station for a price quote of that station to be recorded. With over 125,000 cars, this sufficient condition is likely to be easily met for most stations.

[14] One exception is Hosken, Silvia and Taylor (2011) who use six years of daily price data.

[15] These data were obtained using Google Earth.

[16] OPIS reports on its web site that prices of the majority of its stations are updated via a daily batch process similarly to ours with "transactions that are from 1-5 days old with the majority of prices being no older than 3 days." `http://www.opisnet.com/about/methodology.aspx#RetailGas`, visited 06/03/2015. The average time between subsequent price quotes in our sample is only 1.6 days, because for a large fraction of our stations, the quotes get refreshed every day.

[17] See Manski (2007, section 2.5) for a thoughtful treatment of this issue.

3.2 Price levels and changes in local market structure

In the period considered, fuel prices in the Netherlands have fluctuated widely. Figure 1 shows that fluctuations in average retail prices follow the dynamics of the crude oil spot price: They move closely in line with the Amsterdam-Rotterdam-Antwerp (ARA) premium unleaded gasoline spot price. The ARA price gradually increased until the onset of the Great Recession in August 2008 initiated a sharp decline. In the years following, prices recovered, reached their previous peaks in Spring 2011, and have remained relatively stable at this level since then.

Figure 1: Average retail gasoline price and ARA spot price (Oct. 2005 – Dec. 2013).

Table 1 shows two main trends in the period of investigation: a steady increase in the number of automated stations and a decrease in the market share of the Major-6 brands (Shell, Esso, BP, Texaco, TOTAL or Q8) from 66.5% in 2005 to 53.0% in 2013.[18] With a market share over 14 percent, Shell is still the market leader at the end of 2013 despite having lost almost 3 percentage points of its market share since January 2006. Table A.1 in the online appendix provides summary statistics of these trends at the regional level and shows that the share of automated fueling stations has importantly increased in all regions.

[18]The market share of an individual firm is defined as the percentage of all gasoline stations operating under one of the firm's brand names.

Table 1: Development of the fraction of automated and Major-6 stations in the period 2005-2013.

	2005	2006	2007	2008	2009	2010	2011	2012	2013
Automated	0.125	0.154	0.190	0.209	0.230	0.242	0.263	0.279	0.292
Major-4	0.495	0.477	0.459	0.451	0.442	0.436	0.428	0.419	0.407
TOTAL	0.132	0.133	0.123	0.117	0.112	0.106	0.099	0.097	0.097
Q8	0.038	0.034	0.032	0.033	0.031	0.029	0.029	0.028	0.026
# stations	3,350	3,416	3,465	3,498	3,538	3,595	3,613	3,673	3,673

Notes: Statistics for off-highway stations (as per October 1^{st}), including entries and exits. For historical reasons, the group of Shell, Esso, BP and Texaco is often referred to as the group of major firms. However, in terms of market share and brand premium, it is natural to consider Shell, Esso, BP, Texaco, TOTAL and Q8 as the set of major stations. To avoid confusion, we will talk of the Major-4 and Major-6 firms, respectively.

Table 2: Summary statistics on the annual percentage of stations (close to a station) that is newly automated.

year	Percentage converted	A converted station within... 1 km	2 km	5 km	10 km
2006	1.9%	1.9%	6.5%	25.2%	70.6%
2007	2.1%	1.9%	6.6%	25.7%	81.2%
2008	1.7%	2.0%	5.9%	16.7%	48.4%
2009	1.6%	1.5%	4.4%	16.1%	56.7%
2010	1.4%	1.4%	4.0%	13.3%	44.0%
2011	1.6%	1.9%	4.8%	19.5%	63.0%
2012	1.4%	1.6%	3.9%	12.7%	43.5%
2013	1.6%	2.7%	6.4%	23.6%	68.9%
2006-2013	13.3%	12.5%	30.0%	67.6%	94.0%

Table 2 shows that per annum about 1.4 to 2.1 percent of all stations are automated. These conversions induce significant changes in the local market structure. Each year, 1-3% of all off-highway stations experience an increase in the number of automated off-highway competitors within a 1 km radius and these numbers increase to 4-7% and 13-26% for a 2 km and 5 km radius respectively. From 2006 to 2013, the majority of stations (67.6%) has witnessed an increase in the number of its automated competitors within a 5 km range.[19]

[19] This total for the entire period is lower than the sum of the percentages of the individual years because it correct for double-counts: Some stations experience an increase in their number of automated competitors within 5 km in more than one year. For the narrower 2 km radius, 85 stations (2.9% of the total) see their number of automated competitors increase by more than one in the time period 2006-2013.

3.3 The price effects of automation: Descriptive statistics

Our panel covers a period of more than eight years. In each year a considerable number of stations are automated but which stations are automated when is not determined by random assignment. In particular, firms may decide to convert specific stations in response to local market developments, some of which may be unobserved by the researcher. This endogeneity leads to two potential problems. First, the set of stations that is automated may not be representative for the full sample of stations. Second, because of selection effects, the set of stations automated in later years may show a different price response to automation when they operate in a different market context than sites automated earlier. Basker (2015) identifies larger price decreases for early adopters of scanners. She ascribes this to the higher potential of these early adopters to complement the scanners with other process improvements that increase efficiency. To allow for a heterogeneous price response in different periods, we split our price data into two separate sub-samples: the years 2006-2009 and 2010-2013. Throughout, descriptive statistics and estimates will be presented for each of these two sub-samples separately. The high number of automation-events per year combined with the high-frequency price data allows us to do this while retaining sufficient statistical power.[20]

To address possible selection effects, Section 4 introduces a difference-in-difference matching (DDM) method that will, for each period considered, match the price quotes of stations that are automated in that period (the treatment group) with those of a comparable group of stations that do not experience automation in that period (the control group).

Table 3 provides a first glance at the price effect at stations that are automated themselves or that are in the proximity of a station that is automated. To take out price fluctuations due to (international oil) price developments common to all firms, we calculate in Table 3 for each day the nationwide average price and subtract this from the individual price quote to obtain the price residuals. A negative number means that the

[20] In online Appendix B we present the full analysis for an even finer division into four periods of equal length: 2006-07, 2008-09, 2010-11, 2012-13. That analysis shows that the direct and indirect price effects of automation are qualitatively similar for the four periods and also similar to the results for the 2-period split we present in the main text.

price at that day was below the national average. Per station, we compute the average of these price residuals before and after the station (or a station in its close proximity) was automated. Table 3 shows the average price residual across stations. The table suggests that there is a strong and highly significant direct effect on the prices of stations that experience automation ranging from an average drop of 2.6 cpl (cents per liter) of stations that convert in the years 2006-09 to 3.3 cpl for stations that convert in the years 2010-13. However, these numbers do not correct for the fact that some of the stations also experience other changes at the event-date, such as a change in brand name. The regression estimates that we will present later will take this into account. The tentative evidence in Table 3 does not lend support to the presence of competitive spillovers: for none of the set of stations within a 1, 2, 5, or 10 km radius of an automated station we observe a statistically significant drop in prices for any of the periods.

Table 3: Average price deviation (in €) from national average before and after conversion own station or station in neighborhood.

			Average price deviation		
Period	Conversion	#	before	after	Δ
2006-09	self	215	-0.0009	-0.0273***	-0.0264***
	within 1km	197	-0.0048***	-0.0061***	-0.0013
	within 2km	548	-0.0049***	-0.0049***	0.0000
	within 5km	1431	-0.0015**	-0.0018***	-0.0003
	within 10 km	2455	-0.0014***	-0.0012**	0.0002
2010-13	self	150	0.0035*	-0.0297***	-0.0333***
	within 1km	186	-0.0067***	-0.0084***	-0.0016
	within 2km	451	-0.0036***	-0.0047***	-0.0011
	within 5km	1315	-0.0021***	-0.0022***	-0.0001
	within 10 km	2350	-0.0012**	-0.0005	0.0007***

Notes: ***(**,*): statistically different from zero at the 1%-level (5%-level, 10%-level).

The estimates in Table 3 however also suggest that the sample of automated stations is not representative because of selection effects. For all periods we observe that the average price in the neighborhood of stations being automated is already below the national average before the automation date. These deviations are significant at the 5%-level for all ranges; the smaller the range considered, the larger the deviation (in absolute value).

This suggests that stations are automated relatively more frequently in low-price areas. For the automated stations themselves, we find that stations automated in 2010-13 but not in the years 2006-2009 price somewhat above the national average before automation. This may indicate that the set of stations automated in 2010-13 may differ from the set of stations automated in earlier years.

Table 4 explores the differences between converted and non-converted sites in more detail. This table reveals that compared to sites that do not experience a conversion, sites that are automated on average are significantly smaller in term of plot size, volume sold and shopping area. Also, stations in the more rural North and East of the country (zipcodes 7000s to 9000s) where the population density is lower and the road network less connected seems over-represented in the set of automated sites.[21] Looking at the variable Major-6, one observes that whereas in the initial years especially the minor brands are very active in automating their stations, the major six firms increasingly start automating in later years.

It is clear that without further correction these difference between automated and non-automated sites will bias our estimates of the effect of automation on prices. It is to this task of constructing a comparable set of automated and non-automated stations to which we turn next.

4 Empirical strategy and estimation results

4.1 Propensity score matching

The previous section established that the set of stations that has experienced the 'event' of automation cannot be considered a random draw from the set of all stations. To obtain a set of automated and a set of non-automated stations that are comparable in terms of observable characteristics, we calculate for each station its propensity score: the predicted probability of being automated given the observed site characteristics (Rosenbaum and Rubin, 1983). We distinguish the two time periods 2006-09, and 2010-13. Thus for each station two different propensity scores – one for each time period – are calculated.[22]

[21]Figure A.2 in the online Appendix maps the zip code areas in the Netherlands.

[22]Table A.2 in the online Appendix presents for each period the results of this probit regression. For each time period, an automation-dummy is regressed on the same set of explanatory variables as in

Table 4: Main observable characteristics of automated and non-automated stations.

		Automated stations				Non-automated stations	
Time interval:		**2006-2009**		**2010-2013**			
		Mean	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
		Site characteristics					
Plotsize area		910.73***	(886.24)	1,104.80***	(1,213.99)	1,451.04	(1,541.21)
No. pumps		2.52***	(0.98)	2.54*	(1.07)	2.72	(1.11)
Estimated volume sold		1,524.66***	(885.08)	1,536.15***	(771.15)	2,207.46	(1,319.59)
Shop area		39.59***	(25.90)	41.98***	(24.56)	58.02	(28.31)
Company owned		0.36	(0.48)	0.25***,††	(0.43)	0.40	(0.49)
Major-6		0.33***	(0.47)	0.61†††	(0.49)	0.66	(0.48)
		Local market concentration					
# off-highway	*≤ 2 km*	5.30	(4.97)	4.64	(3.92)	5.23	(4.43)
sites at ...	*≤ 5 km*	18.93*	(13.75)	16.37***,†	(10.67)	20.58	(14.31)
	≤ 10 km	59.64*	(34.10)	55.24***	(30.63)	64.18	(36.72)
# highway	*≤ 2 km*	0.12	(0.45)	0.14	(0.61)	0.13	(0.52)
sites at ...	*≤ 5 km*	0.79	(1.27)	0.82	(1.33)	0.92	(1.35)
	≤ 10 km	3.09	(2.38)	3.39	(2.80)	3.38	(2.49)
		Geographical characteristics					
Zipcode...	*1000s*	0.13	(0.33)	0.12	(0.33)	0.11	(0.31)
	2000s	0.09	(0.28)	0.04***	(0.20)	0.11	(0.31)
	3000s	0.12	(0.33)	0.10	(0.30)	0.14	(0.35)
	4000s	0.06***	(0.23)	0.07	(0.26)	0.11	(0.32)
	5000s	0.09**	(0.29)	0.11	(0.31)	0.14	(0.35)
	6000s	0.07*	(0.26)	0.16*,†††	(0.37)	0.11	(0.32)
	7000s	0.21***	(0.41)	0.21***	(0.41)	0.12	(0.32)
	8000s	0.14***	(0.34)	0.13**	(0.33)	0.08	(0.27)
	9000s	0.10	(0.30)	0.06	(0.24)	0.07	(0.26)
German border		0.02	(0.15)	0.03	(0.16)	0.04	(0.18)
Belgian border		0.01*	(0.07)	0.03	(0.16)	0.02	(0.15)
# priv. owned cars ≤ 20km		263.80**	(189.41)	253.33**	(171.03)	295.12	(197.96)
# sites		215		150		2537	

Notes: Standard deviations in parentheses; Data on site characteristics provided by Catalist Ltd. Plotsize area and shop area in sq. m; volume in '000s litres per annum; shop sales in €'000s per annum; privately owned cars in '000s.
[a] Gives the total number of stations per group. Sample sizes vary across rows of the table due to missing values.
***(**,*) : statistically different from the non-automated stations at the 1%-level (5%-level, 10%-level).
†††(††,†) : statistically different from the stations automated in 2006-2009 at the 1%-level (5%-level, 10%-level).

We use single nearest neighbor matching without replacement: For every treated station that is automated, the matched control station is determined to be the station with the propensity score closest to the score of the treated station. We set the maximum distance between the propensity score of a treated unit and its closest control match at 0.10. Treated units without a control within this distance are dropped to prevent that matched stations are in fact substantially different.[23]

To avoid interference with other events, the pool of potential control stations is limited to those stations that, in the given time bracket ±180-days, were not automated themselves and that are not within a 5 km range of a station that is automated. This is not a severe limitation as this leaves us with a set of potential controls of 1,400 to 1,500 stations per period. Some of the treated stations possibly also experience an indirect effect in the time period considered because they are within a 5 km range of another station that is automated. In order to isolate the direct effect of becoming automated from the indirect effect, we drop these sites from the analysis. For this reason, the number of automated stations in Table 5 is smaller than in Table 4.[24] Table 5 shows that the matching procedure leads to a selected sample of treatment (automated) and control (non-automated) stations that is similar in terms of observables. In fact, only for two of the characteristics we find statistical significant differences at the 10%-level.

Table 4. For site variables such as 'Estimated volume sold' pre-2006 values are used to avoid *ex post* matching: matching on variables whose values may have changed due to the automation process studied.

[23]No stations were eliminated because of this threshold.

[24]Of course, part of the estimated treatment effect picks up the indirect feedback effects caused by neighboring stations decreasing their prices in response to the lower prices of their automated competitor. Competitors that respond by adopting automation themselves arguably have more leeway to reduce prices. In dropping sites within 5 km range that both experience automation in the same period, we ignore the latter effect. For this reason, our estimates of the treatment effect can be considered a conservative estimate of the total price effect (direct plus indirect effect) of automation.

Table 5: Main observable characteristics of treatment and control stations

		Automated stations				Non-automated stations			
Time interval:		**2006-2009**		**2010-2013**		**2006-2009**		**2010-2013**	
		Mean	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
		Site characteristics							
Plotsize area		809.55	(722.46)	976.13	(1,037.73)	891.01	(1,181.78)	958.03	(885.41)
No. pumps		2.55	(0.91)	2.55	(1.12)	2.79	(1.04)	2.37	(0.89)
Estimated volume sold		1,510.60	(962.61)	1,588.44	(834.41)	1,636.19	(914.55)	1,608.15	(931.83)
shop area		43.22	(26.86)	41.03	(25.57)	41.38	(27.18)	43.79	(25.29)
Company owned		0.35	(0.48)	0.25	(0.44)	0.34	(0.48)	0.26	(0.44)
Major-6		0.34	(0.47)	0.61	(0.49)	0.29	(0.46)	0.70	(0.46)
		Local market concentration							
# off-highway	*≤ 2 km*	5.86	(4.78)	4.65	(3.84)	6.53	(5.27)	4.79	(4.29)
sites at ...	*≤ 5 km*	21.55	(14.03)	16.27	(11.29)	22.66	(14.81)	17.89	(12.92)
	≤ 10 km	65.05	(36.51)	55.16	(32.37)	70.63	(40.14)	58.48	(30.87)
# highway	*≤ 2 km*	0.14	(0.44)	0.11	(0.61)	0.11	(0.48)	0.06	(0.33)
sites at ...	*≤ 5 km*	0.70	(1.06)	0.89	(1.45)	0.58	(0.96)	0.85	(1.34)
	≤ 10 km	3.39	(2.37)	3.58	(2.90)	3.37	(2.49)	3.44	(2.67)
		Geographical characteristics							
Zipcode...	*1000s*	0.12	(0.32)	0.11	(0.32)	0.08	(0.28)	0.11	(0.32)
	2000s	0.12	(0.32)	0.04*	(0.19)	0.14	(0.35)	0.00	(0.00)
	3000s	0.12	(0.32)	0.12	(0.33)	0.17	(0.38)	0.16	(0.37)
	4000s	0.05	(0.21)	0.07	(0.26)	0.06	(0.23)	0.09	(0.28)
	5000s	0.06	(0.25)	0.06	(0.24)	0.07	(0.26)	0.03	(0.16)
	6000s	0.06	(0.23)	0.16	(0.37)	0.06	(0.25)	0.15	(0.36)
	7000s	0.26	(0.44)	0.25	(0.43)	0.26	(0.44)	0.27	(0.45)
	8000s	0.13	(0.34)	0.12	(0.33)	0.13	(0.34)	0.15	(0.36)
	9000s	0.10*	(0.30)	0.06	(0.24)	0.04	(0.19)	0.05	(0.22)
German border		0.03	(0.16)	0.04	(0.19)	0.06	(0.25)	0.05	(0.22)
Belgian border		0.00	(0.00)	0.04	(0.19)	0.00	(0.00)	0.03	(0.16)
# priv. owned cars ≤ 20km		284.25	(210.82)	245.17	(177.07)	317.95	(223.43)	245.27	(165.57)
# sites[b]		110		81		110		81	

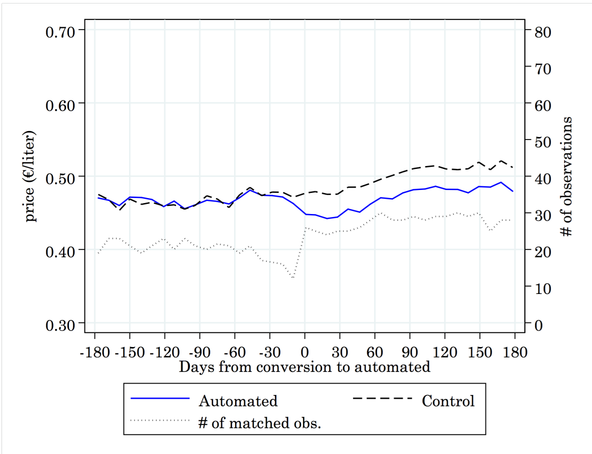
Notes: Standard deviations in parentheses; Data on site characteristics provided by Catalist Ltd. Plotsize area and shop area in sq. m; volume in '000s litres per annum; shop sales in €'000s per annum; privately owned cars in '000s.
[a] Gives the total number of stations in the treatment or control group. Sample sizes vary across rows of the table due to missing values.
***(**,*) : statistically different from the set of Non-automated Stations at the 1%-level (5%-level, 10%-level).

4.1.1 Selection of price observations

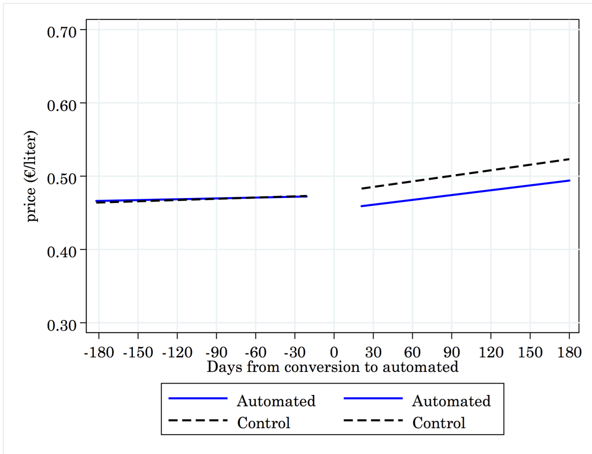
An important indicator of the similarity of the matched treatment and control stations – and thus for the success of matching – is whether price levels and price trends at matched treatment and control stations are similar in the pre-conversion period. However, such a comparison is not straightforward because stations convert at different calendar days. We address this as follows. For each treatment station i, we limit attention to the 180-day period around the date of conversion t_i. As pre-treatment price observations of station i we take the set of price quotes p_{it} with $t \in \{t : -180 \leq t - t_i < 0\}$. The post-treatment price observations is the set p_{it} with $t \in \{t : 0 \leq t - t_i \leq 179\}$. For the control station matched to station i, we similarly only include the price quotes observed within the 360-day window around the date of conversion t_i. It may happen that for a given day t, a price quote is observed for the treatment station but not for the matched control station, or vice versa. To prevent this imbalance from biasing our results, we only include days for which price quotes of both the treatment and matched control station are observed.

In the left panels of Figure 2, the lines show the development of the average value of the matched price quote for the treatment (solid) and control (dashed) group. Notice that within each period the pre-automation and post-automation price levels and trends are similar between the two groups. It is important that the prices in the two groups show parallel trends in the pre-automation period because the difference-in-differences estimator that we will apply only provides an unbiased estimate of the effect of automation if absent automation, the outcomes in the two groups would have followed parallel trends (Duflo *et al.*, 2007).

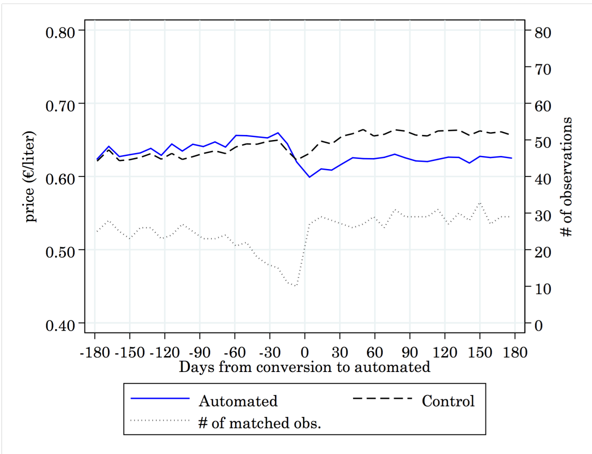
This empirical similarity in pre-treatment outcomes between the two groups is indicative of the "exogeneity" of the event of becoming automated to other station and regional characteristics that may correlate with price. However, in the final weeks days before automation, this similarity seems somewhat less pronounced with the median price in the treatment group exhibiting a downward trend relative to the control group. This is driven by two anticipatory effects. First, some of the stations in the treatment group shut down a couple of days before being automated. The dotted lines in panels 2a and c which represent the number of daily matched price quotes (scale on the right y-axis) in



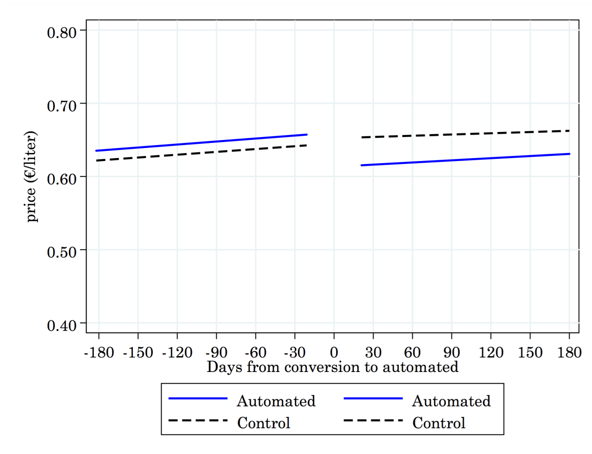
(a) 2006-09: Mean values of matched price quotes.



(b) 2006-09: Pre- and post-conversion price trend.



(c) 2010-13: Mean values of matched price quotes.



(d) 2010-13: Pre- and post-conversion price trend.

Figure 2: Prices, pre- and post-conversion price levels and trends for treatment and control groups.

Notes: In plotting the price trends, price quotes within 21 days of the conversion date have been excluded.

our data. These noticeable drop these lines show in the final 30 days before automation is proof of such an effect. This may generate some selection bias for this period. Second, the stations that continue to operate till the date of automation may anticipate the new price regime and already adjust their price level in the final days before automation.

Panels (b) and (d) plot for the treatment and control group the linear pre-conversion and post-conversion trend, excluding the transition period of 21 days before the conversion date until 21 days after that date. Both panels exhibit similar pre- and post-conversion price trends for treatment and control groups. For the time period 2006-09, formal F-tests do not reject any of the null hypotheses that pre-automation price levels and trends are identical in treatment and control group at the 5% significance level ($p = 0.500$ and $p = 0.3663$, respectively. For the period 2010-13, both the null hypothesis of identical levels ($p = 0.005$) and identical trends ($p = 0.046$) are rejected at the 5%-level, although panel $2d$ shows that the pre-conversion difference in trend is economically not substantial. The graphical analysis shows a clear and substantial post-automation increase in the price difference between the two groups.

4.2 Difference-in-difference matching estimates

In this section we complement the visual evidence of the preceding section with statistical evidence by providing difference-in-difference matching (DDM) estimates. However, before presenting the results it is instructive to spell out the identifying assumptions. Let T be the set of stations in the treatment group. The set of control stations C consists of all stations $j = N(i)$ that have been matched to a station $i \in T$ by the nearest-neighbor matching procedure. We call $M_i \equiv \{i, N(i)\}$ a matched pair of stations. Let t_i denote the calendar date at which station i has been automated. For treatment stations i, the treatment dummy $D_{it} = 1$ for $t \geq t_i$, and 0 otherwise. We further define for each station $k \in M_i$ an index t'_k which denotes the number of days from the conversion date of treatment station in pair M_i. That is, $t'_k \equiv t - t_i$ for $k \in M_i$ $(\forall i, k, t)$, such that $S_k^{pre} \equiv \{t : -180 \leq t'_k < 0\}$ and $S_k^{post} \equiv \{t : 0 \leq t'_k \leq 179\}$ denote the 180-day pre- and post-treatment period of station k, respectively. In what follows, we suppress the subindex k in t' when the interpretation is clear from the context.

Define p_{1it} as potential price at station i at day t conditional on station i having been automated; similarly, p_{0it} is the potential price at station i at time t conditional on not having been automated. Under the assumption that the difference $E[p_{1it} - p_{0it}|i,t]$ is a constant, say γ, the observed price p_{it} can be written as:

$$p_{it} = c_i + c_t + \gamma D_{it} + \beta X_{it} + \epsilon_{it}, \tag{1}$$

with c_i and c_t site and calendar day fixed effects, X_{it} time variant station characteristics on which we have information (brand name changes and changes in the number of local competitors); $E[\epsilon_{it}|i,t] = 0$.

Our DDM estimates use the difference in average pre- and post-treatment prices at treatment and control sites:

$$\bar{p}_{i,Pre} = \frac{1}{N_{i,Pre}} \sum_{t \in S_i^{pre}} p_{it} \text{ and } \bar{p}_{i,Post} = \frac{1}{N_{i,Post}} \sum_{t \in S_i^{post}} p_{it},$$

with $N_{i,Pre}$ ($N_{i,Post}$) the number of pre-treatment (post-treatment) price quotes of station i.

From (1), we get that

$$\bar{p}_{i,s} = c_i + \bar{c}_s + \delta \bar{D}_{i,s} + \beta \bar{X}_{i,s} + \bar{\epsilon}_{i,s} \qquad s \in \{Pre, Post\}, \tag{2}$$

where $\bar{c}_s = \sum_{t \in S_i^s} c_t / N_{i,s}$, $\bar{X}_{i,s} = \sum_{t \in S_i^s} X_{i,t}/N_{i,s}$ and $\bar{\epsilon}_{i,s} = \sum_{t \in S_i^s} \epsilon_{i,t}/N_{i,s}$. Note that $\bar{D}_{i,s} = 1$ if $i \in T$ and $s = Post$ and 0 otherwise.

We have that

$$E[\bar{p}_{i,Post}|i \in C] - E[\bar{p}_{i,Pre}|i \in C] = \bar{c}_{Post} - \bar{c}_{Pre} + \beta[\bar{X}_{i,Post} - \bar{X}_{i,Pre}]$$

and

$$E[\bar{p}_{i,Post}|i \in T] - E[\bar{p}_{i,Pre}|i \in T] = \bar{c}_{Post} - \bar{c}_{Pre} + \gamma + \beta[\bar{X}_{i,Post} - \bar{X}_{i,Pre}].$$

The causal effect of interest is the population difference-in-differences

$$\begin{aligned} E[\bar{p}_{i,Post}|i \in T] - E[\bar{p}_{i,Pre}|i \in T] - \{E[\bar{p}_{i,Post}|i \in C] - E[\bar{p}_{i,Pre}|i \in C]\} &= \\ \gamma + \beta \left\{ [\bar{X}_{i,Post|i \in T} - \bar{X}_{i,Pre|i \in T}] - [\bar{X}_{i,Post|i \in C} - \bar{X}_{i,Pre|i \in C}] \right\} & \end{aligned}$$

which is the joint price effect of all changes that happen at the station level at the date of automation: the effect of the automation itself, γ, plus the effect caused by changes in other station characteristics, β, for example a change in brand name that accompanies automation.

We will estimate the treatment effect using the sample analog of this difference in population means. As before, a price quote p_{it} of a treatment (control) site i is only included if the price p_{jt} of the matched control (treatment) site j is also observed at day t. This implies that $N_{i,s} = N_{j,s}$, $s \in \{Pre, Post\}$, for any pair matched pair of stations. Importantly, without this restriction $\bar{c}_s$ in equation (2) may be different for matched treatment and control sites if they are observed at different dates. That would potentially bias our estimates of the treatment effect.

As mentioned in the previous section, another bias arises if treatment stations already adjust prices in the pre-treatment period in anticipation of the upcoming conversion. If part of the effect already materializes in the pre-treatment period, this will lead to a downward bias of our estimates. We check for this by testing for Granger causality (Granger, 1969) in the following way. We regress prices on a sequence of weekly lags and leads plus time and station fixed effects c_t and c_i:

$$p_{it} = c_i + c_t + \delta_0 D_{it} + \sum_{\tau=1}^{26} \delta_{-\tau} D_{i,t-7\tau} + \sum_{\tau=1}^{25} \delta_{\tau} D_{i,t+7\tau} + \beta X_{it} + \epsilon_{it}. \tag{3}$$

At the right-hand side of the equation, the coefficient δ_0 gives the effect on the day of conversion, the lags in the first sum the effects in the first 26 weeks post-treatment ($\delta_{-1}, \dots \delta_{-26}$) and the leads in the second sum the anticipatory effects in the 25 weeks preceding treatment ($\delta_1, \dots \delta_{25}$).[25] In the absence of anticipatory effects, the sum of lead effects, $\sum_{\tau=1}^{25} \delta_\tau$, should equal 0. As before, we will estimate (3) separately for both time periods to check whether the effect of conversion is similar across years.

[25]Week 26 preceding treatment is the omitted period.

5 Estimation results

5.1 Direct effects

We next separately estimate the regression difference-in-difference equation (1) for each time period, including for each station $k \in M_i$ the price quotes p_{kt} within the 360-day window around the conversion date t_i. Table 6 presents the estimates in the row labeled "none". For all periods, we find strongly significant effects of automation on the price levels at automated sites with the estimated effect ranging from 2.1 cpl in 2006-09 to 4.0 cpl in 2010-13.

However, these estimates may be biased if stations adjust their prices in anticipation of the upcoming conversion. We test for the presence of such anticipatory effects by estimating equation (3), clustering the errors at the treatment pair level. Figure 3 plots the estimated δ-coefficients. For all periods, the plots show a clear treatment effect. Prices at treated stations drops immediately at the event date $t' = 0$ and robustly stay at this lower level in the six months following. The bottom line of Table 6 shows that the null hypothesis of the sum of lagged effects being equal to zero is firmly rejected ($p < 0.001$) for all periods. Table 6 also gives p-values for tests of the null hypothesis that the sum of leads equals zero. These tests cannot reject the null of no anticipatory effects at any of the conventional significance levels. Still, the insignificant but slightly downward trend in the δ-coefficients in the final three weeks (final week) before conversion in the time period 2006-2009 (2010-2013) in Figure 3 may suggest some anticipatory effect. To assess how possible anticipatory effects influence our estimates, Table 6 also reports the estimated treatment effects for sub-samples of the data that take into account a transition period by excluding price quotes within 14, 28 or 42 days of the conversion date.[26] In all cases, the estimates are identical to those without a transition period which leads us to conclude that anticipatory effects do not importantly bias the estimated treatment effects.

The effect of automation is comparable across the years considered. If anything, it is somewhat larger in the later time period. This result differs from Basker (2015) who finds that early adopters of scanning contribute disproportionately to the price decreases.

[26]Similar to the 21 days around the conversion date not being considered in Figure 2b and d.

There are some natural explanations for this difference. The first is simply that we consider a somewhat shorter time period than Basker: *ceteris paribus*, firms that adopt the same process innovation within eight years from each other are likely to be less heterogeneous than when the time of adoption is up to twelve years apart. Another in our view more important reason can be found in the different nature of the innovation. Scanner quality increased over time and prices fell, such that over time also smaller sized stores increasingly installed scanners. This makes selection effects more probable. Basker (2015, p. 358) argues that the earlier adopters had probably more to gain from scanning because they could combine them with the adoption of complementary technologies and processes, enabling larger price reductions. The scope for such efficiency enhancing complementary innovations however seems limited for the process innovation of automating service stations.

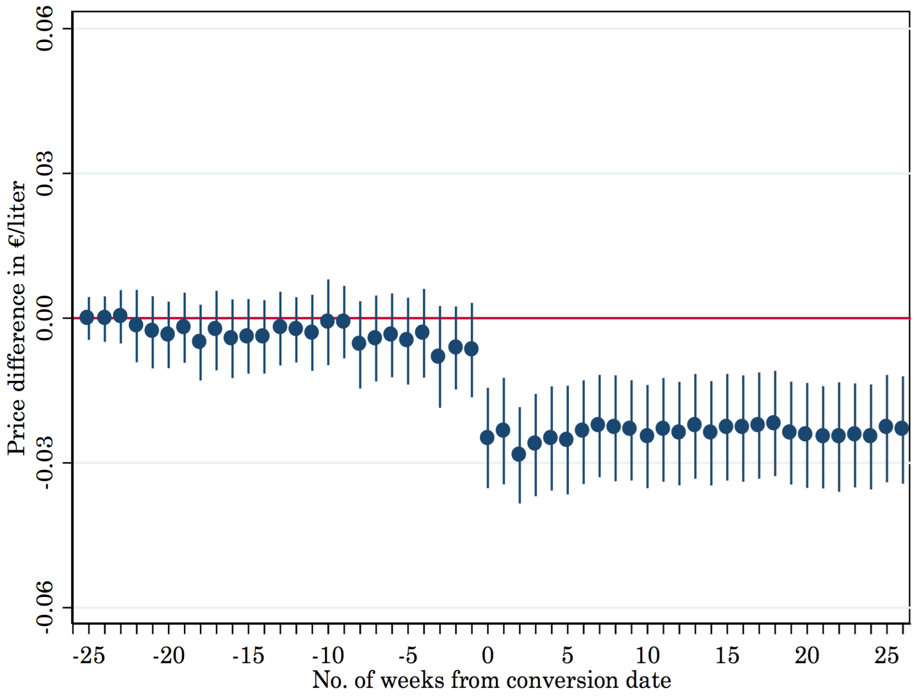
The estimated direct price effect is not only statistically significant but also economically important. A 1 to 2.5 percentage point drop is non-negligible compared to a gross retail margin that in the Netherlands is estimated to be about 12%.[27] Also, with 5.5 billion liters of Euro95 being annually sold in the Netherlands, potential savings in fuel by consumers amount to millions of euro per annum. Of course, extrapolating the price effect out of sample is problematic because in the longer run automation may cause for example changes in market concentration.[28]

5.2 Unobserved changes in shop operations

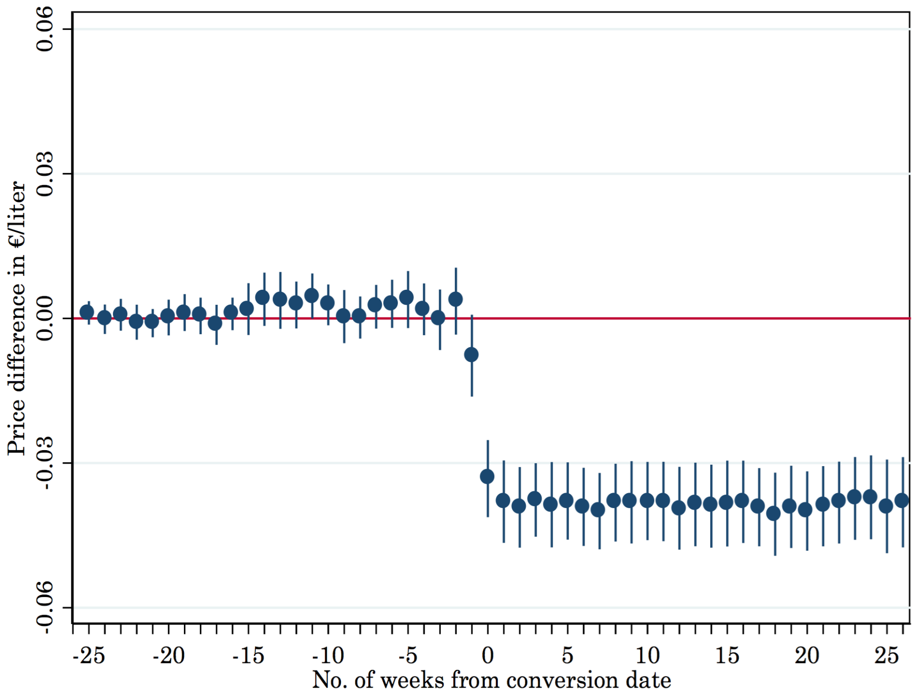
The regression estimates in the previous section account for brand name changes that coincide with the conversion of a self service station to an automated service station. It is however possible that next to changes in payment infrastructure and brand name, a conversion is also combined with a change in shop operations. In the most extreme case, the shop will be closed. As mentioned previously, we unfortunately lack station-

[27] Reported for August 2011 by BOVAG (2011, p. 25, Fig. 3.3), the Dutch trade organization for employers in mobility. Hosken et al. (2008) also report an estimate of 12% for the average gross retail margin in the U.S.

[28] Articles in the magazine for station owners support the view that the impact of automation on price levels that we identify in this paper for the period 2007-2013 has continued in the out-of-sample years that followed: "However, the overall growth of the unstaffed segment increasingly forces staffed locations to give higher price discounts" (TankPro, 2016).



(a) 2006-09.



(b) 2010-13.

Figure 3: Temporal effects conversion on price level automated sites.
Notes: Price difference between treatment and control stations in the weeks before and after conversion. The vertical spikes indicate 95% confidence intervals. Each graph plots for one of the time periods considered (2006-09, 2010-13) the regression coefficients of the weekly lead and lag dummy variables. Standard errors clustered at the treatment-group level.

Table 6: Price effect of conversion on automated stations. Dependent variable: p_{it}.

Time period:	2006-09 (1)		2010-13 (2)	
Transition period...				
...None	-0.021***	(0.004)	-0.040***	(0.004)
obs.	16,452		17,384	
...14 days	-0.021***	(0.004)	-0.040***	(0.004)
obs.	15,524		16,846	
...28 days	-0.021***	(0.004)	-0.040***	(0.004)
obs.	14,458		15,714	
...42 days	-0.021***	(0.004)	-0.040***	(0.004)
obs.	13,276		14,484	
p-values F-tests:				
$\delta_1 + .. + \delta_{25} = 0$	0.3647		0.4779	
$\delta_3 + .. + \delta_{25} = 0$	0.4038		0.3602	
$\delta_5 + .. + \delta_{25} = 0$	0.4334		0.3310	
$\delta_7 + .. + \delta_{25} = 0$	0.4513		0.3957	
$\delta_{-1} + .. + \delta_{-26} = 0$	0.0000		0.0000	

Notes: DID regression estimates of equation (1). Each cell represents a separate regression. All regressions include station-level and day fixed effects and time variant station characteristics. Robust standard errors in parentheses; errors are clustered at the matched treatment pair level. ***,** ,* : statistically significant from zero at the 1%-level, 5%-level, 10%-level.

level information on such changes at the time of automation. As a result, the estimates in Table 6 may present the combined effect on prices of the payment innovation and unobserved shop changes not accounted for by the time variant brand dummies.

To assess the extent to which these unobserved changes bias our results, we divide our treatment group into two sub-samples based on whether or not a shop was present in the period before automation and perform the analysis for both groups separately. The idea is that stations without a shop pre-automation are very unlikely to experience a change in their shop operations at the time of automation. The estimates for this subgroup would thus isolate the effect of the change in payment infrastructure, whereas the estimates for the second subgroup would present the joint effect.

Table 7: Price effect of conversion on automated sites with/without a shop in the pre-automation period. Dependent variable: p_{it}.

Time period:	2006-09		2010-13	
	Pre-treatment		Pre-treatment	
	No shop	Shop	No shop	Shop
	(1)	(2)	(3)	(4)
Transition period...				
...None	-0.021***	-0.021***	-0.035***	-0.043***
	(0.004)	(0.005)	(0.005)	(0.004)
...14 days	-0.021***	-0.021***	-0.035***	-0.043***
	(0.005)	(0.005)	(0.005)	(0.004)
...28 days	-0.021***	-0.020***	-0.035***	-0.044***
	(0.005)	(0.005)	(0.005)	(0.004)
...42 days	-0.021***	-0.020***	-0.034***	-0.044***
	(0.005)	(0.005)	(0.005)	(0.004)

Notes: DID regression estimates of equation (1). Per time period, each row represents a separate regression. Number of observations per cell identical to Table 6. All regressions include station-level and day fixed effects and time variant station characteristics. Robust standard errors in parentheses; errors are clustered at the matched treatment pair level. ***,**,* : statistically significant from zero at the 1%-level, 5%-level, 10%-level.

Table 7 gives the results. A comparison of columns (1) and (3) with the corresponding entries in Table 6 shows that the estimates for the time period 2006-09 are very similar. For the time period 2010-13 however, the estimates for the group of no-shop stations are notably smaller in absolute sense than the corresponding estimates reported in Table 6.

Together with the observation that we previously found the biggest price impact for the period 2010-13, this suggests that the estimates in column (2) of Table 6 at least partly reflect the effect of unobserved changes in shop operations.

That said, the estimates presented in Table 7 do not qualitatively change our main findings: Independent of the presence of a shop before conversion, automation has a very significant and consistent negative impact on prices. For the final period, the price decrease is somewhat larger for the group of stations with a shop before automation. Although this difference of 0.8-1.0cpl is not statistically significant, it again suggests that part of the estimated effect in Table 6 is not caused by innovations in the payment process but should be ascribed to unobserved changes in shop operations.

5.3 Competitive effects

Next we consider whether the lower prices at converted stations lead to price changes at local competitors. In estimating these indirect or spillover effects, we use a procedure similar to the one used to estimate the direct effect. First we identify for all periods the stations within a range of 1 km (2 km) of a station that was automated in this period. We consider the 1 and 2 km range for two reasons. First, the before/after price deviations in Table 3 do not suggest an indirect treatment effect at wider ranges. Second, for wider ranges, the majority of the stations will be in the treatment group (see Table 2) leaving the set from which we can draw the set of controls correspondingly small. We limit the pool of stations considered to off-highway stations that were not automated themselves in the given time period or half a year before or after.[29] We also exclude the 85 stations that experienced multiple automation within a 2 km range in the period 2006-2013.

Again, nearest neighbor propensity score estimation is used to match a station experiencing a conversion within a 1 km (2 km) range with a control station.[30] We subsequently use this selected sample to estimate a regression equivalent to equation (3) but with the dummy variable D_{it} now reflecting "being within 1 km (2 km) range of a station that has

[29] For example, for the period 2006-2009, stations automated between July 1, 2005 and June 30, 2010 are excluded.

[30] Tables A.3 and A.4 in the online Appendix summarize the characteristics of the selected sample of treatment and control stations. None of the chararceristics is significantly different between the treatment and control group at the 10%-level for any of the periods.

been automated."

The estimated δ-coefficients are plotted in Figure 4. Note that the scale of the vertical axis is one-sixth the scale of Figure 2. The plots reveal a small but significant and persistent competitive effect following the event date. The significance of the competitive spillovers is confirmed in Table 8 which shows that for the 2 km range the null hypothesis on the joint insignificance of the lagged δ-coefficients is rejected for both time periods at the 5%-level. For the 1 km range, this null hypothesis is also rejected ($p < 0.001$) but only for the later period 2010-13. In most cases, the null hypothesis that the sum of the leads equals zero is not rejected. For the 2 km range, we have two rejections at the 10%-level for the 2010-13 period. Panel Figure 4d shows that in this case the pre-automation price level at stations neighboring an automating station is somewhat *higher*, not lower, than in the set of comparable control stations. If anything, this will slightly bias downward the estimated competitive spillovers reported in the final column of Table 8.

In sum, the plots in Figure 4 reveal significant and persistent competitive spillover effects to the prices of stations within a 1 to 2 km range of a site that is automated. The magnitude of the average impact on prices at competitors within a 1 km (2 km) range is 0.40 cpl or 0.27% (0.30 cpl or 0.19%).[31]

6 Conclusions

This paper has investigated the consequences of the sharp increase in automated fuel retailing that has been observed in several European countries. One market where these developments have played out is the Dutch retail gasoline market in the years from 2005 to 2014. This market is the focus of our empirical analysis.

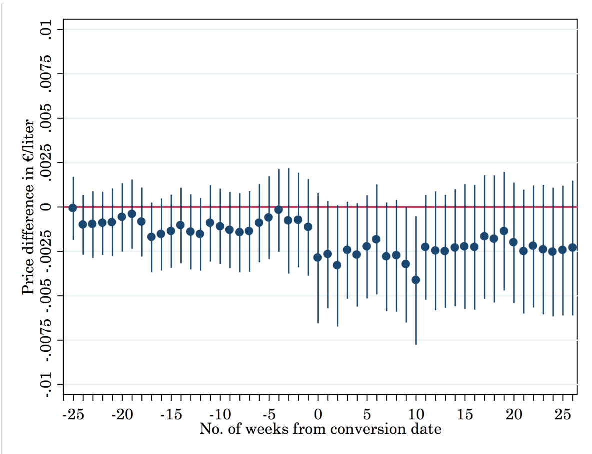
Using a difference-in-differences matching method to estimate the effect of automation on prices, we find that automated stations reduce pre-tax prices with on average 1.7 to 3.2% instantly after the implementation of this process innovation and stabilize at this lower level in the months following. Following Basker (2015), we empirically allow for the possibility that the early adopters of automation have a larger impact on price

[31] We take the averages across the two time periods and perform a calculation similar to fn. 5: Including the VAT of 19%, the average reduction is 0.2 and 0.4 cpl, respectively. Compared to the average retail price of €1.52 in the period considered, this amounts to 0.27 and 0.19%. respectively.

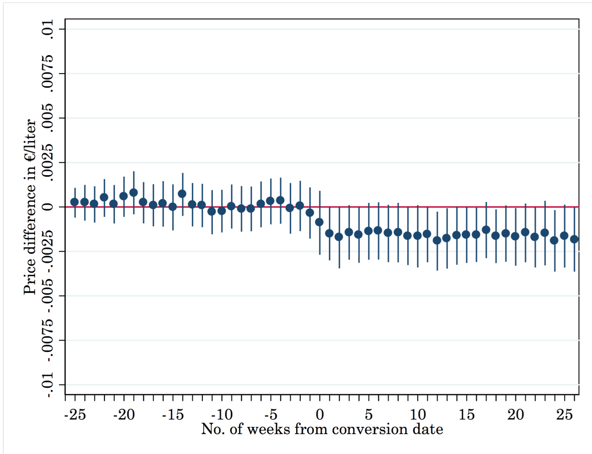
Table 8: Indirect Treatment Effect of conversion on stations within 1 and 2 km range.

ITE:	within 1 km		within 2 km	
Time period:	2006-09	2010-13	2006-09	2010-13
Transition period:				
None	-0.002**	-0.005***	-0.002***	-0.003***
	(0.001)	(0.001)	(0.000)	(0.001)
14 days	-0.001	-0.005***	-0.002***	-0.003***
	(0.001)	(0.001)	(0.000)	(0.001)
28 days	-0.001	-0.005***	-0.002***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
42 days	-0.001	-0.005***	-0.002***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
p-values F-tests:				
$\delta_1 + .. + \delta_{25} = 0$	0.2827	0.2212	0.7774	0.1327
$\delta_3 + .. + \delta_{25} = 0$	0.2705	0.2582	0.7378	0.1002
$\delta_5 + .. + \delta_{25} = 0$	0.2407	0.2757	0.7297	0.0828
$\delta_7 + .. + \delta_{25} = 0$	0.2214	0.2471	0.7339	0.0833
$\delta_{-1} + .. + \delta_{-26} = 0$	0.1118	0.0002	0.0408	0.0149

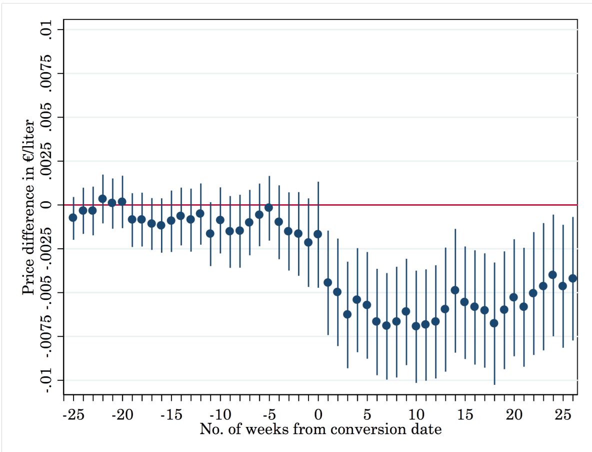
Notes: Each cell represents a separate regression. The number of observations is between $37,776 - 50,913$ (1 km) and $101,029 - 139,989$ (2 km). All regressions include station-level and day fixed effects and time variant station characteristics. Robust standard errors in parentheses; errors are clustered at the matched treatment pair level. $^{***}(^{**},^{*})$: statistically significant at the 1%-level (5%-level, 10%-level).



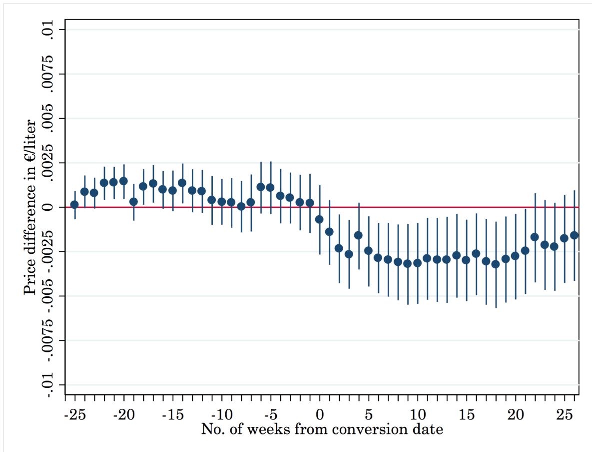
(a) 2006-09: sites within 1 km.



(b) 2006-09: sites within 2 km.



(c) 2010-13: sites within 1 km.



(d) 2010-13: sites within 2 km.

Figure 4: Competitive temporal effects of conversion on the price levels at sites within 1 km and 2 km ranges of a converted site.

Notes: Price difference between treatment and control stations in the weeks before and after a nearby conversion. The vertical spikes indicate 95% confidence intervals. Each graph plots for one of the time periods considered (2006-09, 2010-13) the regression coefficients of the weekly lead and lag dummy variables. Standard errors clustered at the treatment-group level.

levels than the later adopters. Basker (2015) has identified such a heterogeneous effect for the adoption of scanning technology by US grocery stores, but our estimates for automated fuel retailing indicate that the price impact is similar-sized across years. The high-frequency nature of our price data at the station-level also enables the investigation of competitive spillovers to neighboring stations. The coefficient plots show clear and statistically significant competitive spillovers to neighboring service stations within 1 km and 2 km radius. On average, this indirect effect is about one-tenth the size of the direct effect: post-automation, average prices at stations within 1 km (2 km) of a converted site, immediately and persistently decrease by 0.27% (0.19%).

In view of the billions liters of fuel annually sold and an estimated gross retail margin of about 12%, our estimates are not only a statistically significant but also economically important. We hope our results will fuel the public debate in some European countries on whether or not automated stations should be prohibited through legislation.

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