Do the poor pay disproportionately more for increasing market concentration? A study of retail petroleum markets*

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Abstract

A central tenet of industrial organisation is that increasing/decreasing market concentration is associated with increased/reduced markups. But do these variations affect every consumer to the same extent? Previous literature finds price dispersion exists even for homogeneous goods, at least partially as a result of heterogeneity in consumer engagement with the market. We link this heterogeneity to the impact of changing market concentration on markups. With 18 years of station-level motor fuel price data from Western Australia and information on instances of local market exit and entry, we apply a non-parametric causal forest approach to explore the heterogeneity in the effect of exit/entry. The paper provides evidence of the distributional effect of changing market concentration. Areas with lower income experience a larger increase in petrol stations' price margin as a result of market exit. On the other hand, entry does not benefit the same low-income areas with a larger reduction in the margin than in high-income areas. Policy implications include further focus on increasing engagement by low income consumers.

***K*eywords** inequality · market concentration · income · consumer search · causal forests

JEL: L11, L40, D12, D63

1 Introduction

The relationship between market concentration and prices is among the most intensively researched theoretical and empirical topics in industrial organisation. There is substantial evidence showing that increased concentration is associated with increased prices and that more competition lowers prices. This evidence includes analyses of the impacts of changing concentration from market exit and entry. These findings have served as the basis for economic policies to liberalise markets and promote competition for efficiency and consumer welfare benefits.

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But this literature has focused mainly on average effects. In this paper, our interest shifts away from these average effects to breaking down the effects. by income group. We ask the question: does increasing or falling market concentration affect everyone the same way? To find an answer, we look at a homogeneous good, retail petroleum. Walrasian theory would suggest that in a homogeneous goods market, consumers pay the same price, therefore if entry and exit affect prices, the price change will be the same for every consumer. But extensive search literature has proven that this is not the case if consumers differ in how much they engage with the market (Salop & Stiglitz 1977, Varian 1980), and in their willingness to pay (Diamond 1987). If finding low prices requires consumers to engage in costly search, and consumers differ in their search costs, and if consumers also differ in their willingness to pay, economic theory and evidence show that in equilibrium it can be optimal for different stores to charge different prices for the same good. Much empirical works supports these arguments (Woodward & Hall 2012, Allen et al. 2014, Lach & Moraga-González 2017, Wilson & Price 2010, Stango & Zinman 2016). A stylised and somewhat simplistic synopsis of these papers is that a high willingness to search is associated with lower prices and vice versa. Logically, it would follow from these findings that if changing market concentration changes the equilibrium price, the price change will reflect this heterogeneity in consumers' engagement with the market.

Our main objective is to find out whether this is the case, and more specifically, whether heterogeneity in consumer characteristics is associated with distributional effects, i.e. do lower-income households pay more or less for increasing concentration, and do they benefit more or less from an increase in competition? This would depend on whether low/high-income households have lower or higher willingness to pay and whether they are more or less likely to search. The answer is not intuitively obvious. For example, households on higher income may be expected to search more, and pay less than poor households, or the other way around: richer households have higher consumption and so stand to make higher absolute savings from search, but their opportunity costs of time are likely to be higher than poorer households. Earlier works that have linked search and income - reviewed by Byrne & Martin (2021) - offer mixed evidence, although they point more towards the conclusion that lower-income consumers search less.

To provide empirical evidence for this question, we use the geographical and temporal variation of market structure and prices in local petrol retail markets in Western Australia to design a natural experiment exploring the impact of increasing and decreasing local market concentration on the retail margin. In this process we make use of the local variation in demographic characteristics to investigate the relationship between the impact of changing market concentration and demand-side heterogeneity (with a particular focus on income). By employing an event study design, we can adjust the event window in a way that ensures that exit and entry can be considered exogenous in our experiments. This employs the assumption that exit and entry are not driven by short-term changes in the retail margin, something that we support with our data.

Our paper makes contributions to two main strands of work. First, we draw from the recent empirical literature on price dispersion in retail petroleum. These works are motivated by the observation that price dispersion exists, even with homogeneous goods and where price information is readily available. Closest to our work is Lach & Moraga-González (2017), who look at the relationship between the number of firms in a market and retail petrol price dispersion. Using data from the Dutch petrol retail market they find that price distributions in less competitive markets first-order stochastically dominate price distributions in more competitive markets and that consumer gains from the increasing competition are larger for more informed consumers. Lach & Moraga-González (2017) make an important theoretical contribution, by showing that increased competition has an effect on prices only when it changes the level of consumer informedness. Pennerstorfer et al. (2020) set out on a similar task but assume sequential search, and formulate a theoretical model in which they link consumer information to price dispersion and predict an inverted U-shaped relationship. They test this model with retail petrol price data about Austria. To proxy for consumer information, they assume that drivers with longer commuting distances are more informed of petrol prices than those who commute less. Finally, Byrne & de Roos (2017) look at the intensity of search in the petrol market, using the same data as ours - but accessing the number of visits to the FuelWatch website and instead of changes in market structure, investigate search intensity as a function of price dispersion. In the merger retrospective literature that are also studies that looked at retail petroleum markets, the evidence is mixed. Simpson & Taylor (2008) find no evidence of higher prices following exit through mergers, Hastings (2004) and Taylor et al. (2010) find price increases of different magnitudes. Regarding entry, Barron et al. (2004) show that adding one fuel station within a local market (i.e., 2.4 km ring) leads to a price reduction that varies across cities from 1.84 to 5.26 US cents per litre. However, Hosken et al. (2008) use a larger dataset and find no relationship between firm density and market price.

To add to this stream of empirical literature, instead of focusing on the magnitude of price dispersion, we look at the impact of changes in market concentration on the expected price (profit margin). We do this, because we do not observe individual shopping decisions, neither do we observe the exact location of consumers within each local area. Instead, our interest is in looking at the asymmetric impact of a change in the competitive pressure exerted on firms, conditional on local average consumer characteristics. Unlike most previous works, we use information on the time of exit/entry in each local market (as opposed to exploring different levels of concentrations in a cross-section of local markets) to estimate their impact on the price of each petrol station that remained (or had been incumbent) in the market. Our quasi-experimental design allows us to look at how the retail margin of the petrol stations that remain in the market change, after one petrol station exits from their proximity.

Second, we offer new evidence to the growing body of literature on the distributional impact of market power. One of the pioneering papers on this, Baker & Salop (2015) set out the problem and offered an agenda for further work. Some of these works focus on the link between market power and inequality (Ennis et al. 2019, Khan & Vaheesan 2017). Despite the increased focus, much of the currently available analysis is either purely conceptual or is based on empirical work with aggregated macro data (Ennis et al. 2019, Zac et al. 2020, Dierx et al. 2017). There is much less evidence at the market level, which is hardly surprising; linking changes in market concentration to different demographic groups is not a trivial exercise given its intensive data requirements. We approach this problem differently, motivated by the question: do the poor pay more for goods and services, the leading thought behind Caplovitz (1963). By investigating some of the sources that would explain why some people pay a poverty premium, we ask whether the poor pay more if market concentration drives up prices. We view the main contribution of this paper is the rare, market-level evidence, on how local variation in the effect of market exit and entry can be linked to local variation in income. In an important empirical contribution, Allen et al. (2014) make the point (in an application to mortgage markets) that this average effect of mergers underestimates the increase in market power, and they show that competition benefits only consumers at the bottom and middle of the transaction price distribution. The evidence provided in our paper equivalently suggest that mergers and merger interventions have heterogeneous distributional impacts.

On the methodological side, we draw from to the literature on using causal forests to estimate heterogeneous treatment effects, as proposed by Athey et al. (2019). Whereas a large number of location, firm, and time characteristics in our data raise dimensionality issues, which would justify the use of a tree-based approach, at the same time, we have a relatively small cross-section of exits and entries in our sample. To handle this problem, we propose using an ensemble causal forest approach. We demonstrate through simulations, that this performs better than a single causal forest in cases with low number of observations and large number of estimable parameters.

Looking at petroleum retail margins for two products (unleaded petrol, and diesel) in Western Australia, between 2001 and 2019, we find that, in line with conventional industrial organisation theory, exit leads to an increase (although not significant on the average), and entry causes a drop, in the retail margin. When dissecting our estimates to explore the heterogeneity in these findings, we find that low-income households experience a larger (and significant) increase in the price margin with exit. At the same time, they do not benefit from a lower drop in the margin with entry. This suggests that low-income households either have a higher willingness to pay, or lower willingness to search, or both. We also find that concentrated markets witness a larger rise/fall in the price margin from exit/entry. Other factors, such as commuting distance, age, education also drive some of the heterogeneity but even after controlling for these factors, the difference between low and high-income households remains.

These findings offer evidence that a reconsideration of some of the conventional thinking around competition policies may be warranted. The lack of engagement of lower-income consumers with the market suggest that conventional antitrust policy tools may not be able to attain their objectives of improving consumer welfare. Instead, antitrust should not only focus on restoring the level of competition (for example through enforcement action, or by breaking up monopolies) but assign increased priority to improved demand-side remedies to enhance consumer engagement.

The paper is organised as follows. First, we provide a stylised economic framework, which pulls together some of the canonical theories from previous literature. This is followed by an introduction and description of our data, and a discussion of the methodology. We then present the results of our causal forest estimates before offering results from a linear regression, which also allows us to offer results that account for the potential endogeneity of exit and entry.

2 The economic framework

The theoretical motivation of this work links to the vast search literature, which highlights that differences in search and decision costs are likely to influence consumer engagement, and therefore even prices of homogeneous goods display some dispersion.[3] Some of these works assume sequential search, such as Pennerstorfer et al. (2020), who build on Varian (1980) and Stahl (1989) to model search in homogeneous goods, where consumers differ in their degree of informedness, and extend this model where consumers differ also in their willingness to pay for the product. This allows distinction between consumers based on how informed they are about the price of petrol. For some, obtaining an additional price quote is costly; others are aware of all prices charged in the relevant market as they have access to the "clearinghouse". This setup leads to a mixed equilibrium due to the tension between charging a high price to exploit uninformed consumers and charging a low price to attract informed consumers. As a result, the authors find an inverse-U shape relationship between price dispersion and the proportion of informed consumers.

Whilst sequential search models could be relevant to the study of petrol price dispersion in certain settings, our view is that it does not fit our purpose for three reasons. Firstly, in our case consumers are unlikely to search sequentially. Instead it is probably more fitting to assume that consumers do not drive around searching for petrol prices, but instead obtain price quotes in a more passive way, for example through their daily commutes. Secondly, in our setting consumers also have access to the FuelWatch, which allows an easy online comparison of prices. Thirdly, as our focus is on consumer heterogeneity, we do not want to assume that uninformed consumers within a market face the same search cost. It is plausible that regional income differences are not only related to the share of informed consumers and differences in willingness to pay, but also to search cost differences (we indeed proxy for this heterogeneity in search costs through commuting distance and access to internet). Finally, unlike Pennerstorfer et al. (2020), our focus is on the change in price (markup) of each individual firm, when the structure of the market in its local proximity changes, rather than the change in price dispersion.

Instead, our theoretical inspiration is closer to that of Lach & Moraga-González (2017), who proposed using a generalisation of Varian (1980) and Armstrong et al. (2009) in a way that allows for richer heterogeneity in consumer price information. To do this, they rely on a probability generating function for the number of prices observed by consumers. In their interpretation consumers differ in driving and commuting patterns, as well as in attentiveness to posted prices, but the formulation of their model is general enough to capture other sources of information. Because of the conflicting forces of the desire to steal business from its competitors by offering better deals, and the willingness to extracting surplus from consumers who do not compare prices, the market is characterised by a mixed strategy equilibrium. In this setting, if the amount of price information consumers have is heterogeneous, prices are typically dispersed in equilibrium. For this reason, a change in the number of suppliers does not simply affect the average price, but the distribution of prices across this heterogeneous set of consumers. Our paper offers an empirical test of this important contribution of Lach & Moraga-González (2017).

We do not observe individual level data on fuel purchases and on consumer informedness. Instead, we observe local variation, both in prices and in our proxies of how informed consumers are (such as differences in commuting habits,

[3]It may seem obvious to link our work to works on the demand elasticity of petrol, such as Wadud et al. (2010), who provide estimates of motor fuel elasticities for different income levels. They look at the heterogeneity in petroleum demand elasticity, and find, among others an inverse relationship between income and demand elasticity. Whilst this bears some relevance to our study, it is tangential to our research question, as we are interested in how consumers choose between different suppliers, i.e. the brand elasticity of petrol demand, rather than product elasticity.

internet access, or income). This variation would imply that some of our consumers (area average) are more informed than others.[4] Therefore in our study design the unit of observation is not the individual, but the area and we look at price dispersion and variation in consumer heterogeneity across these areas. Market concentration changes over time in these areas (through exits and entries) and we test the price impact of a change in market concentration, conditional on a given level of consumer informedness.

Although Lach & Moraga-González (2017) do not explicitly incorporate income, we assume that both willingness to pay and the level of informedness (i.e. the heterogeneity in search costs) are related to income for the following reasons. Motor fuel is a non-discretionary part of household expenditure. Such products (similar to rent or food) typically display significant distributional differences in that it constitutes a larger fraction of poorer households' expenditure.[5] Moreover, low-income households may be less likely to be able to switch to more expensive substitutes, which may require replacing the car or changing working habits if the price of petroleum goes up.

Regarding the relationship between income and search, Byrne & Martin (2021) provide a carefully constructed review of the relevant literature and concludes that most evidence points in the direction that low-income households engage less with the market. On the other hand, De los Santos (2018) finds that search duration decreases with income and is greater for retirement-age individuals. Nishida & Remer (2018) also find a positive relationship between search costs and income. Our results contradict this for two possible reasons: (1) in our specific case of Western Australia, the FuelWatch petrol retail price comparison website allows online price comparison - engaging with search this way imposes little extra search costs as long as the household has access to the internet, (2) people with higher income may drive more to work and therefore search becomes part of their commuting (without having to engage in search specifically).

3 Petrol retail markets and the data

The Australian petrol retail market is characterised by a small number of very large, and many fringe players. These can be divided into three distinct types. Refiner-wholesalers are vertically integrated retailers such as BP, Caltex, Mobil, and Viva Energy/Shell. This includes refiner–wholesaler controlled sites and independently operated but refiner–wholesaler branded sites. Large independent retail chains are independent retailers such as 7-Eleven, United, Puma Energy, and On The Run. Some supermarkets also sell fuel, such as Coles Express and Woolworths. At the country level, the combined retail market share (based on sales volume) of the large vertically integrated firms dropped significantly from over 80% in 2002, to under 40% in 2017. At the same time, the market share of supermarkets and independents increased substantially. Regarding the individual brands, Shell/Viva Energy (trading under Coles Express) and Woolworths (a supermarket) had a respective market share of 20-25% over our study period, followed by BP and Caltex, just under the

[4] Of course, in the absence of individual level data, we do not know which consumer drives past exactly which petrol station. Moreover, we also do not observe the heterogeneity in consumer attentiveness of the prices they drive past.

[5] For the UK, Mattioli et al. (2018) finds that the poorest households often spend around 20% of their income on motor fuel, and frequently more than this.

20% mark. The remaining sales volume is supplied by independent retailers.[6] Regarding the number of retail units in our sample, BP (260 stores) Caltex (254 stores), and Shell (216 stores including Coles) are the largest.

The main component of our data is daily prices at petroleum retail outlets in Western Australia, for the period 2001-2019, which was downloaded from FuelWatch.[7] FuelWatch is a price comparison service to motorists in Western Australia. At the time of introduction, the website was a response to policy concerns about the levels of price dispersion in the country, implying that some consumers would have been paying largely over the odds. Byrne et al. (2018) offers a detailed description of the FuelWatch data, here we focus on the most important features that are relevant for this paper. Since its launch in 2001, the scope of FuelWatch was largely extended in 2003, and today it covers approximately 80% of regional and 100% metropolitan retail outlets in Western Australia. This includes information about the geographical location of the retail outlet (precise address), the brand of the operator, and prices for unleaded petrol (ULP), premium unleaded petrol (PULP), 95 RON (octane) petrol, diesel, branded diesel, and liquefied petroleum gas (LPG). Not all outlets sell the whole range of products. The way consumers can access FuelWatch has also significantly improved since its launch and has been used to plug into various smartphone apps going back to 2010. Through FuelWatch, consumers have free access to the next day's petrol prices at the petrol station of their choice, reducing switching costs for consumers who use the internet to search for the best petrol deals.

We had 15,638,524 observations of daily petrol station level data for all products at 1299 (1053) petrol stations.[8] Most of these are ULP (3,964,180) and diesel (3,811,105) prices. As we are not directly interested in the daily variation of prices, and also to eliminate issues from rogue missing observations, we averaged the price data at the weekly level and limited our focus to the two most popular products, ULP, and diesel.

We also collected weekly average wholesale prices for the Western Australian region. We acknowledge that not all retail units pay the same wholesale price. Vertically integrated companies have better distribution systems and lower costs than independents. However, this brand-level wholesale price information is not publicly available. Instead, in our estimates, we will control for the brand to account for this cost variation. The wholesale price data was only available from 01 January 2004 onwards, which further reduced our sample size, leaving us with 489,721 observations of weekly petrol station level prices. Figure B.9 in the Appendix shows the over-time variation of ULP and diesel prices. There is significant seasonality in the data. As our interest is in the immediate shocks as a result of exit/entry, we de-seasonalised (removed weekly and yearly seasonality) the price data.

For each petrol station, we acquired the longitude and latitude coordinates (using its address) and applied the Haversine formula[9] to identify which petrol stations are located within 1, 2, and 5 miles from each other. To give an example, Figure 1 shows the number of competitors for a selected independent petrol station had within a 1, 2, and 5-mile radius

[6] `https://www.accc.gov.au/system/files/Petrol-market-shares-report.pdf`

[7] `https://www.fuelwatch.wa.gov.au`.

[8] The 1299 stations include 1053 distinct forecourts. For some of these, there were ownership changes in our sample period, which is why we have 1299 distinct petrol stations.

[9] The distance over the earth's surface. We could have used more sophisticated distance measures (e.g. the driving distance between two places) but our objective was not to precisely estimate the relationship between travelling distance and shopping behaviour, rather look at how a change in the number of petrol stations in proximity of a petrol station affects prices.

in our observed study period. Not all of these petrol stations were always available and competing in the entire sample period. Figure **??** illustrates this for the same independent petrol station. It shows that on 1 Jan 2004 it had 4 active rivals (Mobil, BP, Ampol, Puma). Then in early 2004 Ampol exited the market. In 2008 Puma also left the market, and in 2010 BP also left. Later in 2010 BP, and soon after that Ampol re-opened.

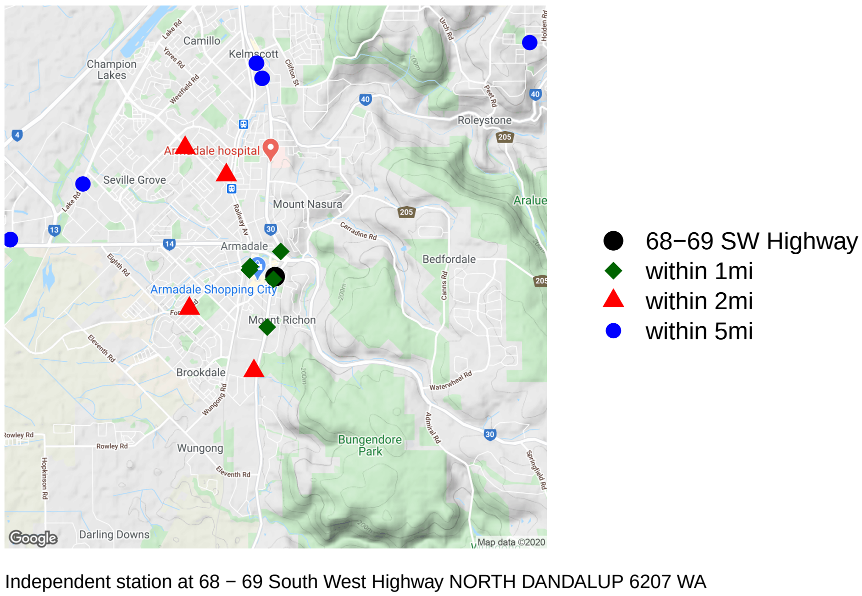

Figure 1: Example petrol station and surrounding competition

We do not observe individual purchases as in Allen et al. (2014) for example. Moreover, we also do not know where each consumer is located. Previous works on price dispersion looked at the distribution of prices within geographical areas around petrol stations. The idea is, that engaged consumers will find the cheapest prices, therefore what matters for them is not the average, but the lowest price in their proximity. Admittedly, one weakness of this approach, is that because the location of consumers is unobserved (with the exception of Pennerstorfer et al. (2020) who observe consumers commuting routes), these papers look at a radius around petrol stations and assume that the consumer is located exactly where the petrol station is, therefore what falls within a given radius of the petrol station also falls within a similar radius for the consumer and can be thought of as the local market (put differently, it assumes that that the same geographical market applies to all consumers within the boundaries of this radius). This is unlikely to be true in our case (especially in spread-out rural areas). To avoid this issue, and also because of differences in our research design, we do not look at price distributions around each firm. Instead we take a different approach, in which we look at each petrol station individually, and assume that their prices are affected (directly) by the characteristics of consumers around them, and (indirectly) by the number of rivals in their vicinity. More precisely, our focus is on how the price of each firm that remain in the market (in the case of exit), or had already been in the market (in the case of entry) changes in response to exit/entry, given this heterogeneity in nearby consumers.

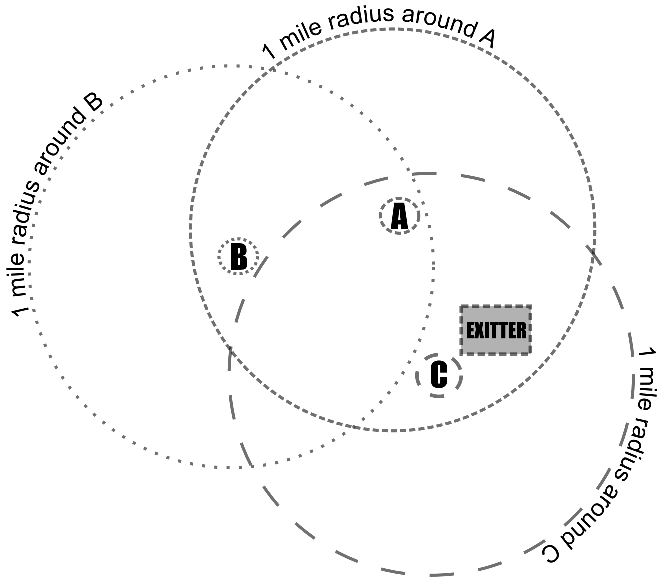

Figure 2: An illustrative example of our local market definition

Figure 2 helps understand our approach of preparing the data for our study design. For each firm in Western Australia we look at whether there had been an exit/entry within their vicinity (1, 2, and 5 mile radii). For example, on Figure 2 petrol station A witnessed an exit within 1 miles. We record this as a treatment for A, and collect local level information for the area where A is located (which will include for example, average household income, or that the number of rivals A had within 1 mile, changed from 3 to 2). Similarly, we record that C also witnessed an exit, and they now only have 1 rival within 1 mile, therefore C is also exposed to the same treatment.[10] For B however there was no exit within 1 mile (note that an analysis similar to Lach & Moraga-González (2017) would have needed us to look at the price at B when looking at price dispersion around A).

Two important features of the data need to be introduced here. First, we sometimes have observations of short spells of ”market exit” (i.e. a rival has no price observation for a short period). The data does not reveal whether there is a temporary gap in reporting the data or a genuine temporary closure of one of the stations (for example for restoration or development work). We drop these periods from our sample. Second, looking only at the number of competitors masks information about the identity of the competitor (in this case the independent had 5 BP stations competing within a 5-mile radius, therefore the finding that the BP station within 1 mile from the independent station decided to exit may have to be interpreted in this context.) We deal with this by introducing variables such as the number of same brand competitors in the area, or a vertical chain dummy.

As our next step we link up the data generated following the above procedure to local characteristics which is available at local area level(Statistical Area Level 2 code).[11] We do this by connecting the petrol station postcodes to their corresponding SA2 areas.

[10] In our empirical work we cluster for those exits and entries that affect multiple other firms.
[11] `https://www.abs.gov.au/websitedbs/d3310114.nsf/home/australian+statistical+geography+standard+(asgs)`

There are 137 distinct SA2 areas in our sample, which include 195 distinct postcode areas. SA2s generally have a population range of 3,000 to 25,000 persons, with an average population of about 12,000 persons in our sample. SA2s in remote and regional areas generally have smaller populations than those in urban areas. Using the SA2 code, we then link each petrol station to local characteristics, using data from the 2016/2011/2006 Australian censuses, and the Personal Income in Australia report of the Australian Bureau of Statistics (ABS). We match this data with the corresponding petrol stations and the prices reported for each corresponding census data (prices before 2008 were matched to the 2006 census, prices between 2009 and 2013 were linked to the 2011 census, and prices from 2014 onwards were linked to the 2016 data.[12] The list of all our variables, and their summary statistics are given in Tables B.1 and B.2 in the Appendix.

Income: We record the annual taxable income at the postcode level, for the whole study period 2004-2019. Unlike some of the other area characteristics, this data is time-variant (annual). The data is collected from the taxation statistics of the Australian Taxation Office[13]. For individual income, the coverage is complete for 2004-2018. For business income data we have net business income for 2004-2018, and net rent for the same period. For the other business income variables, data is available between 2011-2018. For these, for the period 2004-2010, we assumed the same value as in 2011.

Search: We measure search through two main variables, the median commuting distance in an area, and the level of home internet penetration in the area. These two measures account for two dimensions of search: (1) people who drive more to work have a lower opportunity cost of search, as they already survey the prices as they drive past them; (2) people with home internet access are more likely to engage with the FuelWatch price comparison tool. Both can be thought of as different dimensions of search costs. Internet access allows consumers to search online. In households without internet access, search has to be physical, which is associated with higher costs. Several previous works have looked at Internet use as a potential proxy for consumer informedness (Brown & Goolsbee 2002, Tang et al. 2010, De los Santos et al. 2012, Sengupta & Wiggins 2014). To also incorporate the cost of physical search, we measure commuting, which reduces the cost of search: the longer someone commutes to work, the more petrol stations they sample without incurring extra search costs. Previous empirical works that draw on information on commuting patterns to study heterogeneity in petrol prices include Cooper & Jones (2007), Houde (2012), Pennerstorfer et al. (2020).

ABS indices: The Australian Bureau of Statistics introduced several indices to measure the economic and social conditions in an area. The Index of Relative Socio-economic Disadvantage is a general socio-economic index. A low score indicates a relatively greater disadvantage in general. For example, an area could have a low score if there are many households with low income, or many people with no qualifications, or many people in low-skill occupations. The Index of Education (the level of qualification achieved or whether further education is being undertaken) and Occupation (classifies the workforce into the major groups and skill levels) reflect the educational and occupational

[12] The reason we did not use an exact matching is that demographic features are unlikely to change right at the time of taking the census. For example the local demographic characteristics in 2015) are likely to be better represented by the 2016 census rather than the 2011 one.

[13] `https://data.gov.au/data/dataset/taxation-statistics-postcode-data`

level of communities. This index does not include any income variables. The Index of Economic Resources is a proxy for the financial aspects of relative socio-economic advantage and disadvantage (it summarises variables related to income and wealth). This index excludes education and occupation variables.

Other area characteristics: We have data on the age structure in each SA2 area (age, and % of people in various age brackets), the level of education, the average, and the mean commuting distance, and the means of commuting.

Year and quarter dummies: In an event study design, treatments in different calendar times are compared. Although we have removed annual and weekly seasonalities, the treatment may affect the retail price margin differently in different periods. For example, FuelWatch was designed as a price comparison tool in 2001, but consumers only gradually learned to use it over the years.

Brand: As we have a homogeneous product, there should not be much quality variation in the actual product, but there might be in the services linked to the product. The same good sold in two different stores could also be differentiated by the retail environment in which it is sold, with 'high-quality' stores charging more. Controlling for brands allows one to control for quality variation noise in our price variation data. Implicitly this assumes that heterogeneity in quality might exist across, but not within brands.

Note that we have much more diverse data on demand-side factors than supply-side ones. However, even in the absence of firm-level data, we can allow for supply-side heterogeneity by controlling for area-level supply-related factors, such as the average (by business) business expense in an area, the average business tax paid, the average business income, and the average rent paid by businesses.

4 Descriptive analysis and study design

4.1 Descriptive analysis

As shown in previous works, there can be substantial dispersion in the price of petrol (Pennerstorfer et al. 2020, Byrne & de Roos 2017). This is no different in our sample. To get a better understanding of the source of dispersion in our data, this section presents some descriptive information on the retail margin. First, Table 1 shows how the retail margin varies with the level of competition. The table confirms conventional IO theoretical and empirical evidence that higher market concentration is associated with higher margins.

Table 1: Margin by competition

number of rivals within 1mile	0	1	2	3	4	5	6	7	8	9	10
ulp margin	1.134	1.121	1.112	1.124	1.118	1.118	1.115	1.105	1.094	1.085	1.092
diesel margin	1.134	1.129	1.123	1.132	1.123	1.125	1.121	1.115	1.126	1.113	1.102

Table 2 provides more insight into how the margins differ around our main variables of interest: competition, income, and our two measures of search costs, internet (based on the % of households with home internet access) and commuting distance (based on the distance commuted to work). In Table 2 we compare the margins for areas with low and high levels of competition, income, commuting distance, and internet access (to define low and how we split our sample around the median values of these four variables). The numbers confirm, that lower competition is associated with higher margins. Our theoretical introduction posited that some of the price dispersion may be due to the heterogeneity in the level of engagement with the market. In our measurement of search, low commuting distance paired with low internet access in an area is assumed to be associated with the lowest engagement (highest search costs), and vice versa. Table 2 reveals that areas with the highest proportion of informed consumers face the lowest margins. Regarding income, there is a mixed picture, in low competition areas low-income areas are associated with lower margins, but the same is not true for high competition areas. Table B.3 in the Appendix shows that averaging across the total sample, low-income areas experience slightly higher margins in general. Margins are also higher in low education areas, areas with less home internet penetration, and places with a higher proportion of people over 65.

Table 2: Mean margin by levels of competition, income, and search

		ULP			
		low internet		**high internet**	
		low commute	**high commute**	**low commute**	**high commute**
low competition	**low income**	1.188 (0.091)	1.124 (0.057)	1.154 (0.065)	1.122 (0.061)
	high income	1.208 (0.096)	1.146 (0.084)	1.197 (0.097)	1.094 (0.047)
high competition	**low income**	1.13 (0.067)	1.077 (0.027)	1.1 (0.039)	1.09 (0.033)
	high income	1.084 (0.037)	1.078 (0.03)	1.086 (0.031)	1.084 (0.032)
		Diesel			
low competition	**low income**	1.17 (0.075)	1.123 (0.043)	1.142 (0.056)	1.125 (0.056)
	high income	1.184 (0.076)	1.136 (0.055)	1.169 (0.078)	1.109 (0.044)
high competition	**low income**	1.135 (0.051)	1.102 (0.024)	1.111 (0.037)	1.109 (0.039)
	high income	1.11 (0.031)	1.105 (0.029)	1.12 (0.035)	1.112 (0.039)

Standard deviation in parentheses.

Moreover, as Table B.6 in the Appendix shows, there is also significant variation in the price margin across the brands. The small independent stores operate with the highest margins, but the large vertically integrated companies (BP, Caltex, Shell/Coles) are also in the top third. The bottom half of the distribution (lowest margins) constitutes mainly independent chains. This would suggest that cost-efficiency is likely to be dominated by other factors when it comes to setting the margin, as vertically integrated companies are likely to have lower retail costs, but still choose to have a high margin. Some of these differences may be explained by local cost conditions. Stores in urban areas may face higher rental prices and labour costs, which would lead them to charge more, though they might also face more intense local competition, leading them to charge less. Independent 'corner' shops are unable to exploit economies of scale in wholesale purchasing or other costs and so charge higher prices than large, national retailers for the same product. If poor households are concentrated in areas with high retail costs, then this may explain any finding that the poor pay more.

It is also possible that firms behave strategically when choosing whether to open stations in rich or poor areas. Table B.8 in the Appendix shows that this does not seem to be the case in our data. Looking at the largest brands (BP, Caltex, Shell), we can see that low-income areas often have more competitors, but at the same time also higher margins. To further confirm this, Table 3 shows that when the market is defined as a 1, or a 2-mile radius, the number of rivals is similar in low and in high-income areas (there is a difference when one looks at the significantly wider 5-mile radius geographical market). Moreover, there seems to be a difference in consumer informedness, with high-income areas displaying signs of more informed consumers.

Table 3: Main data features by income groups

	N within 1mi	N within 1mi per 10000 people	N within 2mi	N within 2mi per 10000 people	N within 5mi	N within 5mi per 10000 people	internet	commute
high income	2.831	3.656	5.539	5.973	16.288	14.651	0.239	6.457
	(1.751)	(2.864)	(3.771)	(3.574)	(17.811)	(12.85)	(0.06)	(4.152)
low income	2.911	2.761	6.817	5.863	29.351	22.786	0.264	7.666
	(1.748)	(2.138)	(4.425)	(4.006)	(22.43)	(16.253)	(0.062)	(4.664)

Standard deviation in parentheses.

Although these descriptive tables are useful for understanding the data, to test our hypotheses, we need an approach that brings together all possible effects into the same model. This is what we set up with our study design.

4.2 Study design

We estimate the causal impact of the exit and entry of petrol retail forecourts on the retail margin and investigate the heterogeneity of these estimates across different area (consumer) characteristics. Because we do not observe the same markets with and without exit/entry at the same time, we rely on observational data and employ an event study design to line up all relevant events (exits and entries). The use of event study design in quasi-experiments is increasingly common, Schmidheiny & Siegloch (2019) point out that around 5% of the papers published in QJE, AER, and JPE in the most recent years used an event study design. There are also numerous recent methodological contributions, such as Freyaldenhoven et al. (2019), Sun & Abraham (2020), Roth (2018), or Borusyak & Jaravel (2017). The research design implies that for our study, absolute time is normalised such that the observation period is measured concerning treatment time (observations are lined up around the treatments).

We created an event window (15 weeks before and 15 weeks after the treatment), which we applied to every petrol station that experienced exit/entry. We defined exit as the reduction of the number of rivals a petrol station has within a 1-mile radius (in the Appendix we provide our main results for exit/entry within 2 and 5-mile radii). We removed from our sample all instances of exit and entry, where there was another exit or entry within the same area, in our pre, and post-treatment periods (26 weeks before and after the treatment). This ensured that there was no confounding effect from another change in local market structure well before and after the treatment. The reduced sample included 392 instances of exit and 354 instances of local market entry. Figure 3 shows the annual distribution of these exit and entry events. There is a reduction in the number of exits, partly because with fewer petrol stations in the market, there are fewer potential stations to exit. Entries happen at a roughly even rate over time. The figure also displays the quarterly breakdown of exit and entry. This shows an increased number of exits and entry in the second and third quarters of the year, which is likely to do with the end of the tax year (end of June).

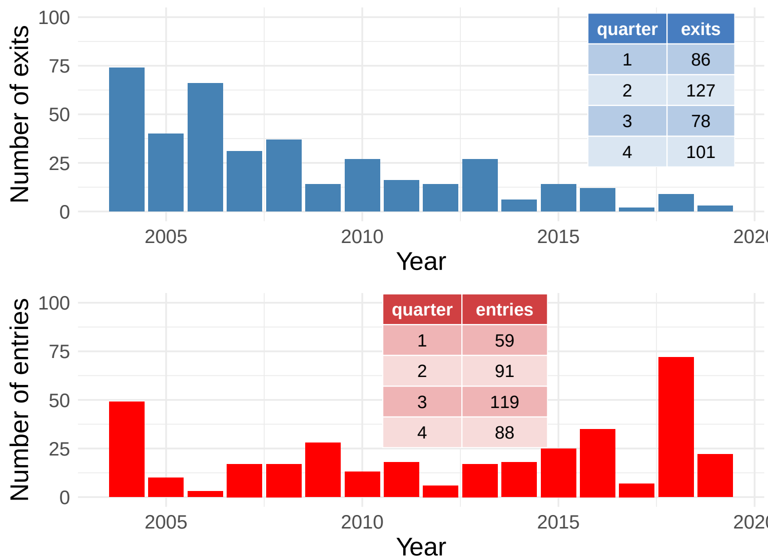

Figure 3: Number of entries and exits by year and quarter in our working sample

The frequency of entry and exit also varies across the different areas. Table 4 shows the ratio of exits and entries to the total number of petrol stations, broken down by our four main variables of interest (competition, income, and search costs as measured by internet access and commuting distance). With low competition, high-income areas witness proportionately more exits. High-population areas are more likely to see more changes in market structure. In general, it appears that areas with more competitors also see more shifts in market structure.

Table 4: The ratio of exits and entries to the total number of petrol stations by income, competition and population size

		low competition		high competition	
		low population	high population	low population	high population
Exits	low income	0.295	0.344	0.398	0.75
	high income	0.404	0.508	0.451	0.312
Entries	low income	0.299	0.296	0.341	0.38
	high income	0.34	0.516	0.28	0.359

In our event-study design, we align these instances of local market exit and entry for each of our ULP and diesel data. This creates unbalanced panel datasets of treated and units for the years 2004-2019 with varying dates of treatment application. Each instance of treatment (exit and entry) is therefore defined as petrol stations that had another station exit or enter the market within a 1-mile radius. For identification, we draw from the sample of non-treated units (petrol stations that did not experience a change in the number of rivals ± 26 weeks from the time of the treatment), to design a control group that can be used as a stand-in for the outcome that would have happened in the absence of exit/entry. Studies with a similar research design, that are only interested in the average treatment effect often average over all these potential control units. In our case, we are interested in the heterogeneous treatment effect. Averaging the control units would mean averaging their characteristics, making them ill-suited for our purpose. Instead, we decided to take the most similar petrol stations (based on the observable features in the 25 weeks period before exit/entry). For this we employed nearest neighbour matching, using the propensity score difference to specify distances from the treatment petrol station, and selecting the petrol station with the lowest distance. In a set of experiments, we looked at the nearest 1, 5, 10 neighbour(s) (each treatment petrol station is matched with 1, 5, 10 control petrol stations) but in our main discussion, we focus on the nearest 5 neighbours. We offer a sensitivity analysis of this choice in the Appendix. This gives us, for each instance of exit and entry, a set of 6 petrol stations (1 treatment and 5 control). Table B.4 in the Appendix compares the average and the standard deviation of the treatment and control groups to demonstrate similarity on observables.

In observational studies, a cursory look at the raw data can often provide a useful indication of whether the testable hypotheses hold. Put differently, caution is warranted if the answer to the main research question is not apparent from simple descriptive figures. Figure 4 shows how the average retail margin varies for the treatment and control groups for ULP for the lowest and highest income terciles. The vertical lines represent the time of exit. There seems to be an increase in the margin, but only in the low-income areas.

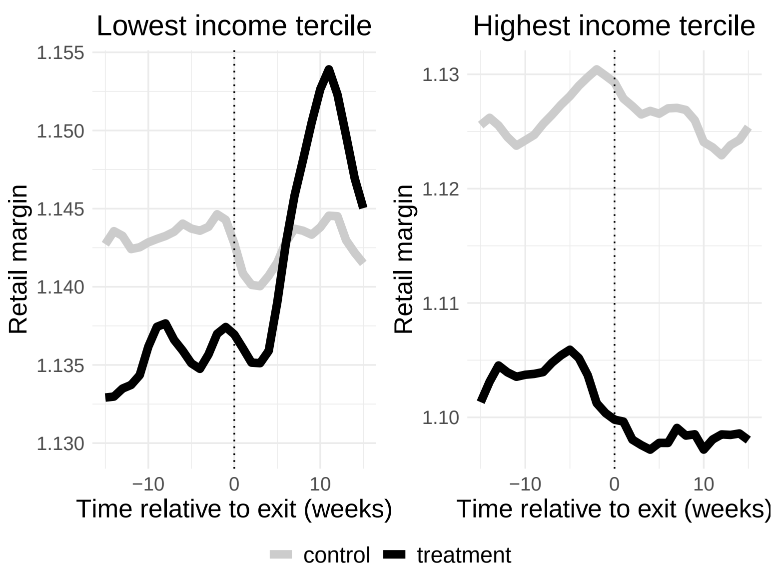

Figure 4: Retail price margin before and after exit for ULP at different income levels

Figure 5 shows that following an entry, the treatment group experienced a clear drop in margins for both low and high-income areas. Both of these figures confirm conventional industrial organisation theory and previous empirical findings, i.e. more competition leads to lower prices, although there seems to be a difference in the level and the change in the level of margins between low and high-income areas. In the following section, we formally test this difference.

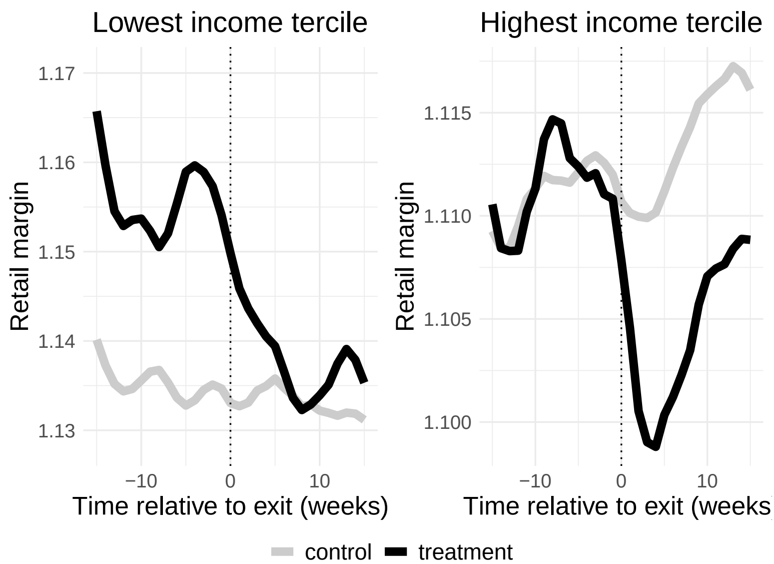

Figure 5: Retail price margin before and after entry for ULP at different income levels

4.3 Exogenous treament

Central to our identification is the assumption that market exit/entry are exogeneous. If the decision to exit/enter is done at the same level as the outcome decision (pricing), this assumption would not hold. One form of endogeneity, reverse causality, would mean that changes in the price/margin trigger the treatment, not the other way around. It is also possible that some unobserved factor is behind the variation in both the treatment (exit/entry) and the outcome variable (price/margin). Previous works have handled this potential problem differently, depending on their study design. In a study on the impact of an acquisition of a petrol retail brand, Hastings (2004) assume that the disappearance from the market of a rival brand is exogenous in local competitor stations' pricing decisions, conditioned on station-specific fixed effects and city-time effects. Hosken et al. (2011) also assume that mergers are exogenous to the local pricing decisions of rival petrol stations. These studies treat mergers as a natural experiment, which would justify the exogeneity assumption. In our case, there is no similar single natural experiment that drives exit/entry in our geographically and temporaneously disperse set of markets. However, below, we offer two reasons to support our assumption of exogeneous exit and entry.

4.3.1 Timing of the exit/entry

Our study design means that we look at the relationship between the short terms variation in margin/price, and the incidence of exit/entry. In this setup endogeneity should only concern us if the margin in the vicinity of exit/entry is what drives the decision to exit/enter. Given the cost of exit/entry, it is highly unlikely that these decisions are made and implemented on the whim of relatively short-term fluctuations in the margin. To support this argument, Figure 6 shows the weekly distribution of exit and entry. It displays a large spike in the number of exits and entries at week 25, which is the end of the tax year in Australia. For administrative purposes, it makes sense for businesses to close down at the end of the tax year, or open up right at the beginning of the new tax year. This implies that at least a large number of exit and entry instances in our data were not made in response to a short-term change in the retail margin, and provides support for our exogenous treatment assumption.[14]

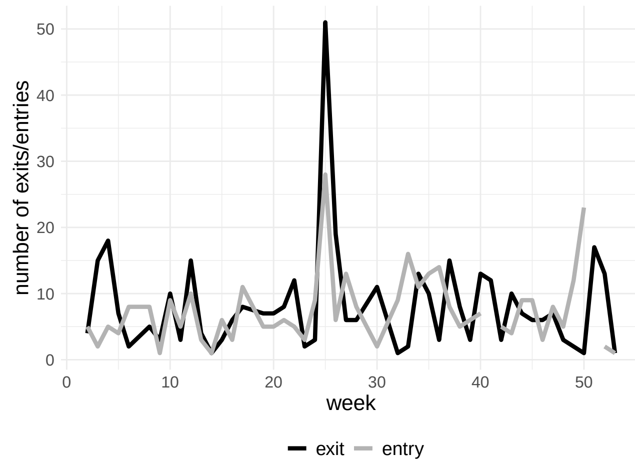

Figure 6: The number of exits and entries by calendar week

It may also seem theoretically plausible that the variation in local characteristics are causing exit/entry. This is not likely to be the case in our study, because, as we explained above, our localised measures of consumer characteristics are observed annually (at best) and are therefore remain constant in our specific intervals of ±25 weeks around the incidence of exit or entry.

4.3.2 Which firms exit/enter?

To decide whether exit/entry are exogenous, it would be useful to understand the reason for each instance of exit and entry in our sample. This was not possible for most cases.[15] Nevertheless, our data allows us to verify a few things. One sign of endogeneity would be if the exiting firm was systematically the least or the most expensive option within a given local geographical market (which fact could have triggered the exit). The first two columns of Table 5 show the % of weeks a petrol station spent as the least/most expensive option within a 1mi radius. The first two rows compare the 25 weeks before the exit with the weeks preceding this period. There does not seem to be any difference: i.e. the relative pricing of the exitting firms (on average) do not differ in the weeks immediately preceding the exit from the other weeks the same firm was in the market. Rows 3 and 4 compare the % of weeks a petrol station spent as the least/most expensive option within a 1mi radius for the exitting firm and the other (remaining) firms. This shows some difference. Exitting firms were more likely to be the most expensive ones than remaining ones. However, as rows 1-2 have shown, this is not only true for the period before exit, but for all periods when these firms were in the market. Moreover, when we regress the propensity to exit on characteristics such as the % of weeks a petrol station spent as the

[14] In Section **??** we present estimates for the sample that only includes exits/entries around the end of the tax year.

[15] For a small number of exits we found evidence from local newspapers or from historical archives of Google Maps StreetView, that they shut down for a longer period (>1 year) for refurbishing or complete renovation works.

least/most expensive option, this relationship becomes non-significant when using a two-way fixed effect model (i.e. other, unobserved, station-level differences explain variation in propensity of exit).

Table 5: Characteristics of exitting and entering firms

	% of weeks spent as cheapest station within 1mi	% of weeks spent as most expensive station within 1mi	number of same brand stations within 1mi	number of same brand stations within 2mi	number of same brand stations within 5mi
exitting firm <25 weeks pre-exit	0.293	0.433	0.341	0.787	2.500
exitting firm >25 weeks pre-exit	0.270	0.466	0.339	0.841	2.841
remaining firm <25weeks pre-exit	0.340	0.253	0.401	0.924	3.620
exitting firm <25 weeks pre-exit	0.272	0.465	0.338	0.842	2.850
entering firm <25 weeks post-entry	0.423	0.430	0.241	0.623	2.766
entering firm >25 weeks post-entry	0.476	0.314	0.203	0.532	2.386
incumbent firm <25weeks post-entry	0.297	0.367	0.383	0.817	3.492
entering firm <25weeks post-entry	0.477	0.313	0.201	0.528	2.390

Table 5 also provides evidence that our data is consistent with Lach & Moraga-González (2017), who suggest that the prices observed should be consistent with mixed-strategies. The fact that no single firm is the the most/least expensive option for the entire study period, implies that firms do indeed engage in mixed strategies, which sometimes make them more and other times less expensive than their rivals.

5 Econometric method and main results

5.1 Estimating heterogeneous effects

We need to estimate the treatment effect of exit and entry on the retail margin. The conceptual problem is similar to that formulated in Rubin (1974). Denote a vector of covariates for petrol station i by X_i. We let the treatment (exit and entry from and to the market) indicator W_i take on the values 0 (the control group, i.e. no exit/entry) and 1 (the treatment group, i.e. exit/entry). For petrol station i, $i = \{1, ..., N\}$, let Y_i denote the observed outcome and the outcome of interest (the retail margin) in the case of receiving the treatment as $Y_i(1)$ and when not receiving the treatment as $Y_i(0)$. The causal effect of exit/entry for petrol station i is therefore $Y_i(1) - Y_i(0)$. The Conditional Average Treatment Effect (CATE) is given by:

$$\tau(x) = E[Y_i(1) - Y_i(0)|X_i = x]. \tag{1}$$

The problem of causal inference is that we do not observe both $Y_i(1)$ and $Y_i(0)$ at the same time. Instead we estimate CATE (conditional on observable variables X) by a difference in means $\overline{Y}_t - \overline{Y}_c$, where $\overline{Y}_t$ and $\overline{Y}_c$ are the means of the outcome variable for the treated (t) and control (c) groups, respectively. For identification, we assume unconfoundedness, $\{Y_i(1), Y_i(0)\} \perp W_i|X_i$, i.e. the markets that experience exit/entry are selected randomly conditional on the observable covariates.

Our objective is to conduct estimation and inference on the function $\tau(x)$ to gain insight into the heterogeneity of the treatment response, across our observable local area characteristics. One way to do this would be through introducing interaction terms in the estimation of $\tau(x)$. Alternatively, we could estimate $\tau(x)$ for different sub-samples of the data, $\tau_b(x),\ b = 1, 2, \dots, B$. The problem with these multiple interaction terms is that they run the risk of using a misspecified model, and even if the correct model was estimated, it can quickly run into dimensionality problems.[16]

In this paper we use generalised random (causal) forests as proposed by Wager & Athey (2018) and Athey et al. (2019). The method fits our problem for multiple reasons. As a non-parametric tree-based method, it does not require us to specify a (potentially complex) linear relationship between our covariates and the treatment effect. It also allows the efficient handling of large covariate spaces. In our case the number of possible sources of treatment heterogeneity (accounting for all interactions) is much larger than the sample size, therefore methods such as OLS cannot be considered without the research filtering which features to use first. Moreover, Athey et al. (2019) showed that the estimates achieve asymptotic normality and as such, it is suitable for hypothesis testing on the treatment effects. Finally, independent variables are time-invariant in our event-study setup (they change every 1 or 5 years). Estimating a 2-way fixed effects linear model would mean simply losing important covariates as they would be subsumed by the fixed effects dummies.

Below we provide a brief introduction to tree-based methods and causal forests. This should be sufficient for those who are unfamiliar with these methods to understand its intuition, but for details, we refer the reader to Athey & Imbens (2015) and Athey et al. (2019). Regression trees are a non-parametric machine learning approach, which are frequently used for prediction problems in data science.[17] Assume we have k covariates and N observations, and we want to partition the covariate space $\mathcal{X}$ into M mutually exclusive regions $R_1, ..., R_M$, where the outcome for an individual i with covariate vector X_i in region R_m is estimated as the mean of the outcomes for training observations in R_m. Denote the subset of covariates observations corresponding to R_m as $\mathbb{X}_m$. Let X_j be a splitting variable and s be a split point. For the initial stage, with $M = 2$, define the observations of covariates associated with observations of X_j that exceed the point s as $R_1(j,s) = \mathbb{X}_1 = \{X \mid X_j \leq s\}$ and similarly, $R_2(j,s) = \mathbb{X}_2 = \{X \mid X_j > s\}$. The algorithm selects the pair (j,s) that solves:[18]

$$\min_{j,s}\left[\sum_{i:X_i\in\mathbb{X}_1}(Y_i - \overline{Y}_1(j,s))^2 + \sum_{i:X_i\in\mathbb{X}_2}(Y_i - \overline{Y}_2(j,s))^2\right] \tag{2}$$

where $\overline{Y}_1(j,s)$, and $\overline{Y}_2(j,s)$ are the mean outcomes in $R_1(j,s)$ and $R_2(j,s)$. Eq.2 splits the data into two regions, then the process is repeated on each of the two resulting regions. Regression forests are ensemble methods, whereby the forest predictions are constructed as the average of the tree-based predictors. (Eq.2) can also be thought of as the 'growing' or 'splitting' part of constructing regression trees.

Causal trees build on the same concept, but for each node, instead of minimising the mean squared error (MSE) for the difference between the average outcomes for each node, it minimises the MSE for the difference in the estimated treatment effects.

[16]For k potential sources of heterogeneity, this would mean adding $2^k - k - 1$ interaction terms to our model.
[17]For details see: Breiman et al. (1984)
[18]The number of splits is chosen through cross-validation in order the balance the tradeoff between low bias and high variance of regression trees.

Athey et al. (2019) proposes using an honest approach to estimating these causal trees, i.e. they grow the tree on a sample of the data, and they estimate it using a different sample. In the context of causal trees, the idea is that the (regions) are small enough that the (Y_i, W_i) pairs for each leaf had come from a randomised experiment. In this case, the treatment effect in the small space of each leaf with the corresponding set $\mathbb{X}_m$ is given by:

$$\hat{\tau}_{\mathbb{X}_m} = \frac{1}{|\{i : W_i = 1, X_i \in \mathbb{X}_m\}|} \sum_{\{i:W_i=1,X_i\in\mathbb{X}_m\}} Y_i - \frac{1}{|\{i : W_i = 0, X_i \in \mathbb{X}_m\}|} \sum_{\{i:W_i=0,X_i\in\mathbb{X}_m\}} Y_i \tag{3}$$

Finally, to construct a causal forest, we draw repeated bootstrap samples of size B from the training data to recursively estimate a number of causal trees. The prediction for an individual with a vector of covariates X_i is then $\hat{\tau} = \frac{1}{B}\sum_{b=1}^{B}\hat{\tau}_b$, where $\hat{\tau}_b$ is the estimate produced by tree b. Athey et al. (2019) show that the estimated treatment effect is asymptotically normal.

Causal forests are useful for finding heterogeneity in the treatment effect in a cross-section setup. Our event study design, however, means that we have longitudinal data, so does this mean that we lose important information by our choice of method? The variables in X_i are area petrol station and area characteristics and can be considered constant within the 30-week event window of our analysis. The outcome variable Y_i on the other hand is time-variant. Therefore choosing causal forests as our method does mean that we forego the possibility of estimating time-dependent treatment effects (for example in the sense of traditional event study designs). We believe this trade-off is justified as we are primarily interested in the heterogeneity in the treatment effect, rather than its dynamics.

For our causal forest therefore the outcome variable of interest Y_i is the change in the retail margin for petrol station i before and after the exit/entry:

$$Y_i = \frac{1}{|\{T_1\}|}\sum_{t\in\{T_1\}} margin_{it} - \frac{1}{|\{T_0\}|}\sum_{t\in\{T_0\}} margin_{it} \tag{4}$$

T_0 and T_1 represent the pre- and post-treatment periods, respectively.

In X_i we include the features listed in Table B.1, which reveals some overlap. For example, we have many different ways of measuring education, or wealth, and we have no a priori knowledge, which one of these is important in driving the treatment effect heterogeneity. Athey & Wager (2019) proposes removing the least important features from the estimation of causal trees to improve estimates. This is a feasible option, but we are specifically interested in the effect of some variables on the treatment effect, and this solution may eliminate some of our variables of interest. Instead, we add an extra layer to causal forests with a bagging ensemble learning method. The idea is, to randomly draw several features, add our features of interest, and re-estimate the forest in each draw, on this reduced sample of features. This way the estimated ensemble treatment effects are $\tau^G = \frac{1}{G}\sum_{g=1}^{G}\hat{\tau}_g$, where G is the number of causal forests we run to get our ensemble ~~individual~~ treatment effects. The standard errors are derived from the bootstrapped standard errors of the individual causal forest~~s~~ and the squared deviation of the treatment effects:

$$\sigma_{\hat{\tau}^G} = \sqrt{\frac{\sum_{g=1}^{G}[\hat{\sigma}_g^2(\hat{\tau}_g - \bar{\tau})^2]}{G}} \tag{5}$$

We argue that this ensemble method is more fitting in cases where there is a relatively small sample size, and a large number of parameters, and we have specific (theory-driven) interest in a selected set of these features. In Section A of the Appendix, we provide details and simulations to justify our approach.

5.1.1 Unconfoundedness assumption

Up to this point, we have assumed that selection to treatment was random (unconfoundedness assumption), conditional on our observable variables. Non-random selection means that unless all relevant variables are observed, our estimates will be biased. A frequent violation of the random assignment assumption is when unobserved factors are correlated with the treatment and the outcome variable in question, leading to biased estimates (omitted variable bias). Conventionally, researchers try to remedy this problem by employing fixed effects, or instrumental variables in their models. The problem with this approach is that it relies on strong assumptions that may not hold, and it is hugely limited by dimensionality issues in a conventional linear regression setup. For example, in our study, some convoluted (non-linear) interactions between the independent variables likely affect the treatment, and not controlling for this would lead to biased estimates. But the use of a linear model constrains researchers in how many of these interactions they can include in their models. Our choice of method handles this problem and allows a much richer set of observable factors to control for. Although it is never possible to observe and account for all relevant factors, under our model the conditional independence assumption relies on a much wider range of attributes than would be possible in linear models. We can include a large number of observed variables and their interactions with the way the treatment affects the outcome, reducing the risk of omitted variable bias.

5.2 Causal forest results

Table 6 shows the conditional average treatment effects (CATE) and the conditional average treatment effects on the treated (CATT). The average effects are as anticipated in our descriptive part: exit, on average triggered a small (statistically not significant) increase in the margin, entry, on average lead to a larger drop in prices. Our interpretation of the asymmetry between exit and entry is to do with the level of market concentration in markets where we observed exit and markets where we were sampling instances of entry. Table C.1 in the Appendix shows that in our estimation sample entry was more likely to happen in more concentrated markets. In these markets, the effect of a change in market concentration on the retail margin is more pronounced.

However, the standard deviation of these average treatment effect estimates suggests that there can be substantial variation in the individual treatment effects across the petrol stations in our sample. To start exploring this heterogeneity, Figure 7 offers visual verification of the relationship between the income and the treatment effect for ULP and diesel. The red horizontal line shows zero treatment effect, and the black vertical line indicates the median value of the feature

Table 6: Conditional average treatment effects

	Exit		Entry	
	CATE	**CATT**	**CATE**	**CATT**
ULP	0.08 (0.065)	0.079 (0.058)	-0.329 (0.081)	-0.335 (0.072)
Diesel	0.082 (0.064)	0.086 (0.061)	-0.067 (0.077)	-0.059 (0.073)

Bootstrapped standard errors in parentheses.

on the horizontal axis (income). The figure reveals that in areas below the median level of income, the treatment effect is very dominantly positive (and large). Above the median income, $\hat{\tau}_i$ is closer to and around zero. The income-related heterogeneity seems more pronounced for ULP than for diesel. In Section C of the Appendix, we provide more figures to show the relationship between several of our variables and the treatment effect.

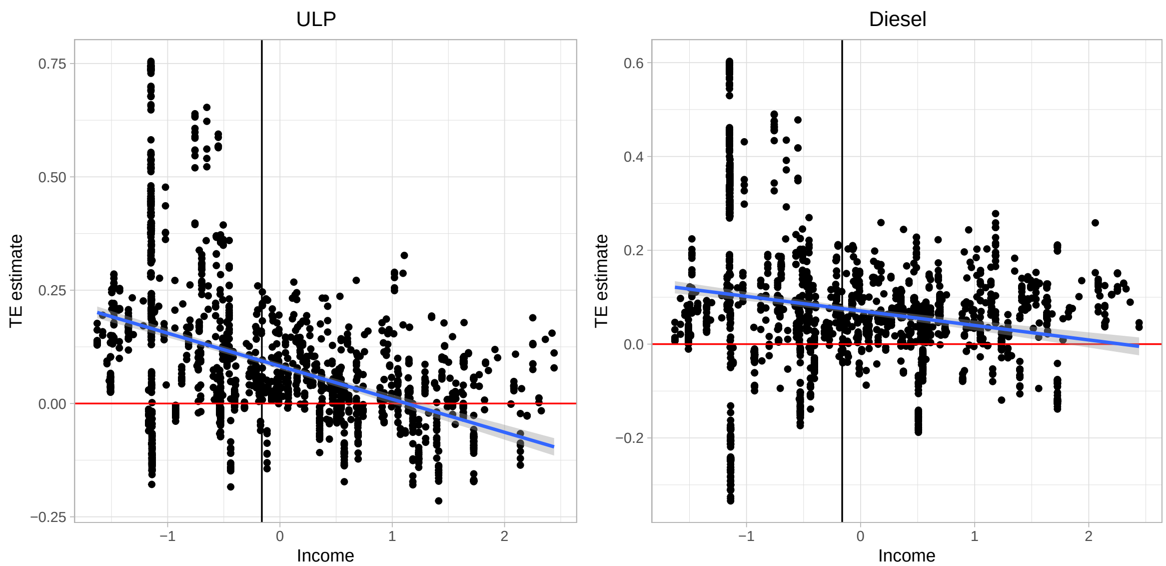

Figure 7: Treatment effects by income

To test our three hypotheses, Table 7 presents a more detailed breakdown of the treatment effects related to exit in ULP, breaking it down to our main variables of interest. We defined low and high for these four variables, by taking the values corresponding to their 10th and 90th percentiles respectively. We then used our estimated causal forests to predict the treatment effect, assuming mean values for all other covariates.

Several stylised findings can be deduced from this exercise. Most importantly for our investigation, with exit, lower-income households see a larger price increase. This difference is more pronounced in markets where competition is lower. The competition rows indicate that prices increase more in markets that were less competitive before the exit. This suggests that increasing market concentration increases price dispersion (the extent to which businesses choose to price discriminate) with low-income areas seeing a larger price increase. Moving on to our measures of the informedness of consumers, in general, estimates in the bottom right corner (which imply more informed consumers) are lower than estimates in the upper left corner for both low and high competition levels. It appears that commuting more, i.e. having a higher chance of physically browsing prices, reduces the price inflating impact of exit more than having more households with access to home internet. Areas, where people commute longer to work, experience lower price increases as a result of exit, as though more commuting was proportionate with search intensity. This is consistent with several previous works such as Pennerstorfer et al. (2020).

Table 7: Predicted treatment effects of **exit** in **ULP** by different levels of competition, income, and search

		low internet		**high internet**	
		low commute	**high commute**	**low commute**	**high commute**
low competition	**low income**	0.271 (0.118)	0.243 (0.109)	0.272 (0.126)	0.237 (0.118)
	high income	0.125 (0.086)	0.097 (0.076)	0.116 (0.09)	0.079 (0.082)
high competition	**low income**	0.127 (0.074)	0.113 (0.065)	0.13 (0.08)	0.109 (0.07)
	high income	0.035 (0.056)	0.02 (0.049)	0.03 (0.059)	0.009 (0.053)

Predicted treatment effects, ”low” implies fixing the given variable at its 10th, and ”high” refers the 90th percentile. Bootstrapped standard errors in parentheses.

Entry on the other hand results in a fall in prices, which is more pronounced in areas with less competition. This is intuitive, on the margin, areas with low levels of competition can gain more from a new rival. We get a more peculiar result regarding the heterogeneity due to differences in income levels. Areas with different income levels experience similar price drops as a result of falling local market concentration (marginally larger in high-income areas). Both of our measures of consumer informedness suggest that more informed consumers are associated with a larger fall in the price margin following entry.

Tables C.4 and C.5 in the Appendix report the results for diesel. The tables tell a similar story (somewhat smaller magnitude) for the diesel margin following exit. The effect of entry is negative, although much smaller (in magnitude and significance) than in ULP. The role of income and search is similar to ULP - areas with more high-income households (or more informed households) experience a smaller diesel margin increase with exit. With entry, there does not seem to be a pronounced difference between the price drop experienced by low and high-income households.

Table 8: Predicted treatment effects of **entry** in **ULP** by different levels of competition, income, and search

		low internet		**high internet**	
		low commute	**high commute**	**low commute**	**high commute**
low competition	**low income**	-0.264 (0.045)	-0.251 (0.039)	-0.366 (0.053)	-0.346 (0.048)
	high income	-0.306 (0.041)	-0.294 (0.037)	-0.381 (0.044)	-0.364 (0.04)
high competition	**low income**	-0.223 (0.036)	-0.212 (0.031)	-0.329 (0.047)	-0.31 (0.042)
	high income	-0.268 (0.032)	-0.258 (0.029)	-0.346 (0.039)	-0.329 (0.035)

Predicted treatment effects, ”low” implies fixing the given variable at its 10th, and ”high” refers the 90th percentile. Standard deviation in parentheses.

Altogether, these results suggest two main effects that hold for each product and both exit and entry. The number of rivals in the market is important, concentrated markets witness a larger rise/fall in the price margin from exit/entry. The

Table 9: Comparing the samples around the end of tax year (ETY)

		income	internet	commuting	comp1mi	comp2mi	comp5mi
exit	month before ETY	46164.32	0.229	6.286	2.751	6.393	22.028
		(9219.653)	(0.062)	(4.399)	(2.135)	(4.610)	(23.066)
	rest of the sample	47455.648	0.226	5.997	2.914	6.538	22.653
		(10087.512)	(0.072)	(4.353)	(2.312)	(4.916)	(22.292)
entry	month after ETY	47101.253	0.225	6.059	3.158	7.103	23.544
		(9842.299)	(0.083)	(4.198)	(2.238)	(4.971)	(22.805)
	rest of the sample	47299.158	0.227	6.033	2.873	6.477	22.500
		(9997.499)	(0.070)	(4.371)	(2.293)	(4.867)	(22.365)

results on income are also consistent, low-income households always experience a larger increase in the price margin with exit. The impact of the informedness of consumers remains similar across the two products. More informed consumer in an area is associated with a lower price increase with exit and a larger price drop with entry (with the exception of entry in diesel).

5.3 Robustness checks

5.3.1 Further tests on the exogeneity assumption

To benefit from the strong possibility that exits and entries around the end/start of the tax year are exogenous, we re-estimated our causal forests for the subgroups of exits happening within 4 weeks before the end of the tax year, and subgroups of entries happening within 4 weeks after the start of the tax year. These respective sub-samples contain 73 instances of market exit and 35 instances of entry. Firstly, Table 9 compares the mean and standard deviation of our main variables of interest for these sub-samples with the rest of the sample. There appears to be no systematic difference between these exits/entries and the rest of the sample.

Table 10 shows the conditional average treatment effects for the sub-samples of exits/entries around the end of tax year. We only replicate estimates of the conditional average treatment effects and not the estimates on the heterogeneity in the treatment effects, because here we have a limited sample size with more limited variation in the features that we expect to drive this heterogeneity (competition, search, income). The CATEs reported in Table 10 are similar to those estimated for the total sample (Table 6). Standard errors are higher, due to the limited sample size. If one accepts the claim that these exits/entries around the end of the tax year are exogenous, this finding suggests that our main results are not biased by potential endogeneity (reverse causality).

Table 10: CATEs for the samples around the end of tax year for ULP

exit		entry	
CATE	CATT	CATE	CATT
0.056	0.082	-0.247	-0.368
(0.148)	(0.134)	(0.252)	(0.217)

Bootstrapped standard errors in parentheses.

To provide further reassurance that reverse causality is not affecting our findings, we provide a set of experiments, where we narrow the pre-event side of our study window. The idea is to move our study window closer to the treatment date, to ensure that changes in the margin were not affecting the decision to exit/enter (for example a change in the margin 5 weeks before the exit is unlikely to be the reason for exit). Tables C.9 and C.10 in the Appendix show that our qualitative results remain, although the magnitude of the results changes.

It is also possible that not the treatment, but an event before the treatment consistently confounds our estimates. To test this, we look at pre-treatment parallel trends by focusing on the 6 months preceding the treatment. If pre-treatment the parallel trend assumption is not violated, we would expect to see zero treatment effect. For this exercise we assumed a placebo treatment to happen halfway through our test period (3-months before the real treatment). Table C.8 shows our results for the impact of exit on the ULP margin and finds that the effect is not significantly different from zero in any of the tested instances.

5.3.2 Other sensitivity checks

In the results highlighted above, we looked at how the exit or entry of a rival within a 1-mile radius impacts the ULP and diesel margins of a petrol station. In our experiments, we also looked at what happens to the same margins if a rival from a 2-mile and a 5-miles radius exits or enters the market. The results for ULP exit (within 2 miles and within 5 miles) are presented in Tables C.11, and C.12, respectively. Both tables follow the same logic as 7 above. We use the estimated causal forests to predict the treatment effect for various levels of competition, income, internet, and commuting. High levels refer to the value of the respective variable at the 90th percentile, and low refers to the value at the 10th percentile. The tables show that the treatment effect falls as we are looking at the impact of a petrol station exiting/entering at a further distance, with the largest effect size from an exit/entry within 1-mile (Table 7), lower average treatment effect if the exit/entry happened within 2-miles away, and even lower if it happened 5-miles away. This is expected. The exit/entry of a close rival is likely to have a larger absolute impact on the retail margin.

As with other tree-based methods, causal forests allow the clustering of estimands (see Athey & Wager 2019). We use this as a robustness check to cluster the petrol stations that experience the same exit/entry as one of their competitors. Results with causal forests clustered by postcode are reported in TablesC.13 and C.14 in the Appendix.

Finally, we also looked at how sensitive our results are to choosing a different nearest neighbour matching to select our control group petrol stations. Tables C.15, C.16 in the Appendix show the results for choosing the 2 and the 10 nearest neighbours. Our story remains qualitatively unchanged.

6 Discussion of the results

Firstly, we presented evidence supportive of exit leading to a small increase, and entry triggering a larger drop in the price margin. We argue that the asymmetry is because in our sample entry tends to happen in more concentrated markets. Although the effect of exit is not significant on average, by looking at treatment effect heterogeneity, we identify the cases where it leads to a significant increase in the margin. First of all, the margin increasing effect of exit (and the margin reducing the effect of entry) is larger in absolute value in less competitive markets.

We also offered detailed evidence that areas with more low-income households experience a larger increase in the retail margin of petroleum products when market concentration increases. At the same time, we do not find that the same low-income households enjoy a larger drop in margins when competition intensifies (in fact high-income households seem to enjoy a somewhat larger drop in the margin). Starting from the theoretical and empirical results of search literature, which suggests that heterogeneity in the level of engagement with the market can lead to price dispersion even in homogeneous goods, our results imply that changing market concentration can have distributional effects through the heterogeneity in the level of consumer engagement (low-income households engage less).

Looking at the role of search we found that in areas with a larger proportion of informed consumers there was a lower increase in prices as a result of exit and a higher fall in margins as a result of entry. Interestingly though, there was still a difference between low and high-income households even after accounting for these measures of search and informedness. In our models, we control for a large number of observable features and income remains an important factor in how much margins increase/fall as a result of increasing/falling market concentration. For example, age and education are often-cited sources of search heterogeneity. But Tables C.6 and C.7 show that even after fixing the level of age and education, the difference between low-income and high-income households remains. This would suggest that unobservable factors may play an important role in how low and high-income households engage with the market. Byrne & Martin (2021) for example argues that differences on a cognitive level, differences in biases, and in how people process information may also be behind low-income people engaging less with the market. These above findings offer strong support to the Lach & Moraga-González (2017) model.

It is also likely that low-income households have a higher willingness to pay. This statement may seem counter-intuitive at first glance, but it makes sense if one considers that higher motor fuel prices eventually encourage the average consumer to cut back on driving or switch to more fuel-efficient vehicles. However in the short-run low-income households may have few options but to continue buying motor fuel and cut back on other expenditures (or get further into debt). Our findings suggest that their higher willingness to pay may also play a role.

Of our search variables, commuting seems to play a more important role in improving consumer informedness than internet access. Several previous works argue that the Internet and price comparison websites do not contribute to better-informed consumers (Ellison & Fisher Ellison 2005, Ellison & Ellison 2009), and our findings may be interpreted in support of these arguments. But it is also important to add that our measure of the Internet is through the level of local penetration of home Internet, which therefore does not capture access to mobile Internet, which has likely dominated Internet use in the second half of our sample period.

There seems to be a difference between the ULP and the diesel results, which is not unexpected. Diesel demand is likely to be dominated by commercial users (heavy goods vehicles, and large goods vehicles). This would explain that there is less of an impact of changing local market concentration, as these users cover longer distances, and are better positioned to shop around to reduce the impact of local price fluctuations.

Probably the main important implication of our findings for policy is that competition alone cannot reduce prices. Unless consumers engage with the market, the benefits of competition are less likely to be transferred to them. On the other hand, if they do engage with the market, then increasing market concentration is less likely to leave them facing increased margins even in concentrated markets. Moreover, engagement with the market is also important when concentration falls (as a result of entry). Although margins are likely to drop, they drop more in areas with more informed consumers.

These translate to two main messages for policymakers. First of all, the harm avoided by blocking a harmful increase in concentration includes some regressive distributional effects (acknowledging of course that not all exits are harmful on average, and some may reflect improved efficiency in the firms that remain in the market). Second, getting the market structure right may only offer a partial solution to a competition problem. Demand-side remedies may also be needed to ensure that consumers engage with the market. Moreover, our findings also give support to arguments that even where blocking or breaking up concentration is not possible, demand-side remedies may help mitigate harmful effects, provided that some choice still exists for consumers.

Finally, these findings should also offer useful lessons to merger retrospectives.[19] Most previous studies focus on the average price effect of exit through mergers, but in mergers with geographically distinct local markets, it should be possible to look at distributional effects, using an approach similar to ours.

7 Conclusion

Motor fuel is a non-trivial part of poorer households' expenditure, which means that poorer households already pay a larger share of their income on transport-related fuel. If they pay a higher price for increased market concentration, the impact is much more pronounced in relative terms. This is important because it implies that antitrust needs to revisit some of its conventional wisdom and account for the possibility that some people benefit more from the elimination of conduct that reduces competition, and this should be reflected in the design of remedies, i.e. remedies should not be designed with the average consumer in mind, but accounting for the heterogeneity of the impact of remedies across different income groups.

An important implication of our findings is that they offer support for the argument that antitrust could help address inequality while staying true to its mission of promoting competition.[20] We do not argue that income or wealth equality should be incorporated directly into competition policies. But we emphasise that ill-designed and executed competition policy and enforcement can contribute to increased inequality. Moreover, the success of the competition policy should not be evaluated for the average consumer. Instead, competition policy, when possible, should consider the possibility of a differential impact and impose remedies accordingly.

[19] Kwoka (2014), and Mariuzzo & Ormosi (2019) provide an overview of these retrospectives

[20] See for example Baker & Salop (2015), or Shapiro (2018).

Motor fuel is similar to food in the sense that it is a non-discretionary part of household expenditure, which also displays significant distributional differences. Mattioli et al. (2018) identify a distinct group of households, around 10% of the UK's population who are in car-related economics stress: on low income, experience high motoring costs, and a low response to fuel price changes. This thinking is seemingly also gaining some consideration in the regulatory review of mergers. The UK Competition and Markets Authority specifically emphasised the difference in local areas regarding food and petrol expenditure (lower-income areas spending a relatively larger proportion of their income on food/petrol) in the Sainsbury's/Asda merger.[21] Whilst we think this is an important and welcome development, we also believe that more micro-level evidence is needed on this topic. To build up the evidentiary toolkit of competition authorities, we hope that this paper will help foster the drive to deliver more merger retrospectives that estimate not only the average but the distributional effects of mergers as well.

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[21] Para. 8.283

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Appendix

A Simulations to demonstrate our ensemble bagging model of causal forests

We conducted some experiments to justify using an ensemble causal forest method in cases where the data has a relatively small number of individuals and many potential sources of treatment heterogeneity. We simulate a dataset with $n = 1000$ individuals, where the number of factors increases from $k = 20$ to $k = 120$. In each loop, the treatment effect is a linear function of $J = k/5$ of these factors.

Our data generating process (DGP) is as follows:

$$Y_i = \alpha X_i + \beta_W W_i + \beta W_i X_i + U_i \tag{6}$$

Where W_i is the treatment variable, following a binomial distribution $W_i \sim B(n, 0.5)$, and $U_i \sim N(0, 1)$. The variable X_i represents a vector of J covariates, generated from a multivariate normal distribution. β is a vector of J parameters with $\beta_j = 1$ (for $j = 1, \ldots, J$) (i.e. as we increase J we add add covariates. The starting DGP is defined as $J = 2$; $k = 10$, $N = 1000$.

Assume that we are interested in the effect of a set of factors X_1, X_2, X_3, where theory supports some relationship between the factors and the treatment effect. For this reason we then record estimates of $\beta_{1,2,3}$, as we systematically vary the k (20,30,40,50,60,70,80,90,100), and correspondingly the J (the number of variables causing heterogeneity in treatment) parameter (4,6,8,10,12,14,16,18,20), while holding everything else constant.

We compare the following two processes:

- **Single causal forest:** We follow Athey & Wager (2019) and start by training two random forests for Y and W and use its parameters as parameter choices to run our causal forest. Similarly to Athey & Wager (2019) we first train a pilot causal forest including all features, and then train a second forest only on those features that had most splits in the first forest (features that had at least the average share of splits). This helps in focusing efforts on the most important features. Our change in comparison to this formula is that we force our feature of interest ($X_{1,2,3}$) to be in the second, smaller pool of features as we are specifically interested in their role in treatment heterogeneity.

- **Ensemble causal forests:** We estimate C = 1000 causal forests. In each iteration, we repeatedly draw a random sample of $J/10$ features, plus we add our features of interest and estimate the causal forest on this small sample of features. The idea is that through our iterations, each feature has interacted with $X_{1,2,3}$. We then average over the estimates to give our ensemble estimate. For example for X_1 we get $\hat{\beta}_1 = 1/C \sum_{c=1}^{C} \hat{\beta_{c,1}}$.

Figure 8 shows the estimates (and standard errors) for $\hat{\beta}_1$. The horizontal red line marks the true effect β_1. Using the single forest method, the estimates drop as we have an increasing number of features and a small sample size. Using the ensemble method, the estimates are not affected by the increase in the number of features.

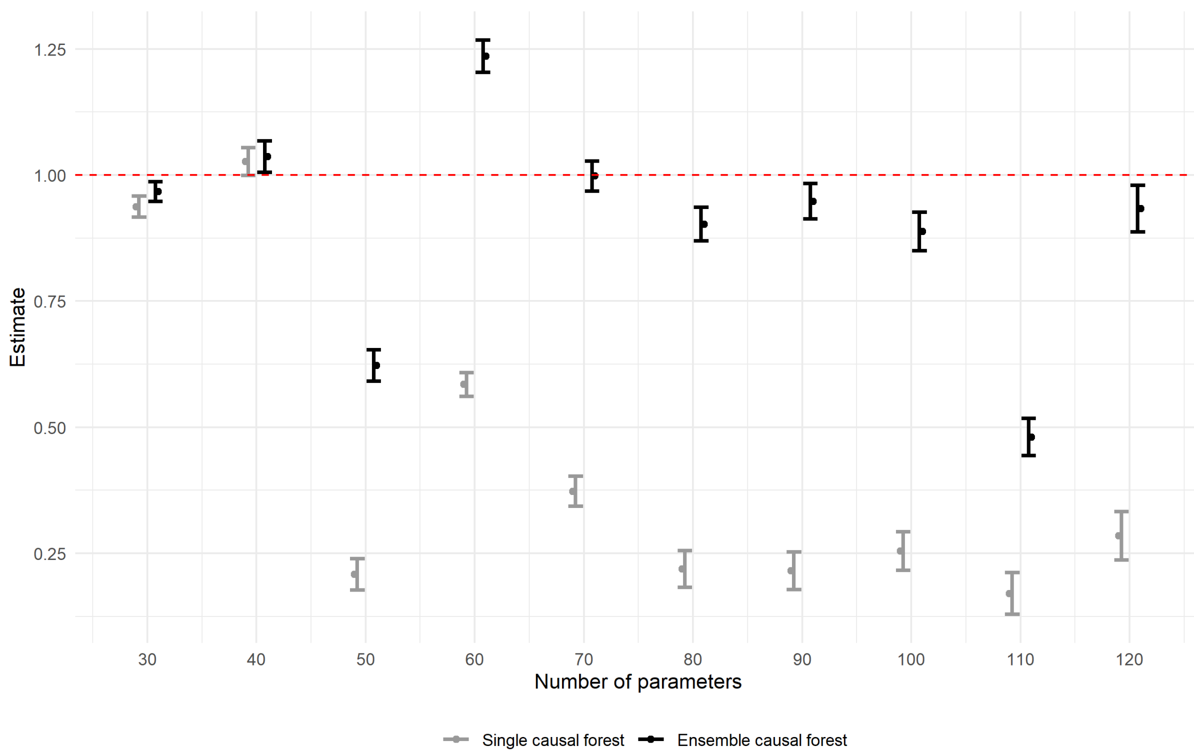

Figure 8: Ensemble v single causal forest - simulation results

B Figures and tables for descriptive part

B.1 Tables

Table B.1: Main features of the data

topic	variable	topic	variable
time	year	**housing**	average rent
	quarter		weekly rent $1-74
wealth	gini coefficient		weekly rent $75-99
	income share of top 1%		weekly rent $100-124
	income share of top 5%		weekly rent $125-149
	income share of top 10%		weekly rent $150-174
	% of people in lowest quartile (relative to AUS)		weekly rent $175-199
	% of people in second quartile (relative to AUS)		weekly rent $200-224
	% of people in third quartile (relative to AUS)		weekly rent $225-249
	% of people in highest quartile (relative to AUS)		weekly rent $250-274
employment	number of employed people living in region no		weekly rent $275-299
	number of earners		weekly rent $300-324
	age of earners		weekly rent $325-349
	sum income		weekly rent $350-374
	median income		weekly rent $375-399
	mean income		weekly rent $400-424
business income/cost	mean ind income before tax		weekly rent $425-449
	mean total business income		weekly rent $450-549
	mean total business expense		weekly rent $550-649
	mean net business income		weekly rent $650-749
	mean estimated business tax		weekly rent $750-849
	mean gross rent		weekly rent $850-949
	mean net rent		weekly rent $950 and over
age	age 0-4		nil rent payments
	age 4-10	**internet access**	internet accessed from dwelling (%)
	age 10-15		internet not accessed from dwelling (%)
	age 15-20		internet access from home / population
	age 20-25	**number of cars**	no cars
	age 25-30		one motor vehicle
	age 30-35		two motor vehicles
	age 35-40		three motor vehicles
	age 40-45		four or more
	age 45-50		average no cars
	age 50-55	**commuting**	average commuting distance (mi)
	age 55-60		median commuting distance (mi)
	age 60-65		interquartile range of commuting (mi)
	age 65-70		standard deviation of commuting (mi)
	age 70-75		train
	age 75-80		bus
	age 80-85		ferry
	age 85-99		tram
	age 65+		taxi
	age 35-65		car as driver
	age 15-35		car as passenger
	age 0-15		truck
	population		motorbike scooter
education	index of education and occupation		bicycle
	advanced diploma and diploma level		walked only
	bachelor degree level		worked at home
	certificate I II level		did not go to work
	certificate III IV level	**competition**	number of rivals within 1 mile
	certificate level		number of rivals within 2 mile
	certificate level nfd		number of rivals within 5 mile
	graduate diploma and graduate certificate level	**brand**	brand size
	level of education not stated		top brand (bp, shell, caltex)
	postgraduate degree level		number of same brand stations within 1 mile
	index of economic resources		number of same brand stations within 2 mile
	index of relative socio-economic advantage and disadvantage		number of same brand stations within 5 mile
	index of relative socio-economic disadvantage		

Table B.2: Summary statistics of the main variables

	mean	sd	10%	25%	50%	75%	90%
ulp retail price	138.28	10.30	127.29	131.92	137.16	143.72	151.05
diesel retail price	146.19	8.80	136.49	140.77	145.46	150.56	156.83
ulp wholesale price	123.54	6.85	115.94	119.65	123.61	127.78	131.78
diesel wholesale price	129.62	6.77	122.01	125.62	129.46	133.69	137.08
cushing price	25.81	2.75	22.85	24.55	25.84	27.07	28.53
ulp margin	1.12	0.07	1.06	1.08	1.10	1.15	1.21
diesel margin	1.13	0.06	1.07	1.09	1.12	1.15	1.19
number of rivals (within 1mi)	1.85	1.70	0	1	2	3	4
number of rivals (within 2mi)	5.19	4.24	0	2	5	8	10
number of rivals (within 5mi)	21.82	20.94	1	4	14	37	56
median income	50607.22	9083.98	41979	45225	50049	54605	60254
mean income	64962.14	18375.49	51568	55371	61552	68390	79770
usual resident population	12249.66	7150.07	4297	5870	11790	16517	23065
people aged 0-14 years	18.90	4.61	14.3	16.7	19.2	21.7	24.4
people aged 15-64 years	66.35	9.08	60.7	63.2	67	70.4	73.6
people aged 65 years and over	14.75	8.85	6.2	10.1	14.1	18.4	21.1
median age	38.46	6.57	32.2	33.5	37.6	41.5	44.7
sex ratio	109.50	41.00	92	96.9	99.7	105.1	120
earners age	43.07	4.63	37	39	44	47	48
number of earners	7123.57	4754.28	2040	3303	6589	9942	13621
no educational attainment	53.46	72.83	3	10	26	67	136
average commuting distance (mi)	11.27	8.34	4.33	6.18	9.36	13.95	18.33
median commuting distance (mi)	6.74	4.65	1.79	2.79	6.55	8.96	12.99
car as driver	0.28	0.08	0.22	0.25	0.29	0.32	0.34
one motor vehicle	0.25	0.07	0.17	0.20	0.26	0.30	0.32
index of relative socio-economic disadvantage	993.10	80.00	917	975	997	1040	1071
index of relative socio-economic advantage and disadvantage	991.43	74.62	901	956	988	1041	1084
index of economic resources	1003.85	79.78	925	973	1016	1050	1089
index of education and occupation	980.20	73.46	886	928	977	1016	1101

The price statistics are reported for 489,721 weekly observations, the area characteristics are reported for the 1053 distinct petrol station locations.

Table B.3: Difference in ULP and diesel margins by the main variables of interest

	petrol		diesel	
commuting distance	**low**	**high**	**low**	**high**
	1.14	1.104	1.144	1.116
	(0.082)	(0.056)	(0.063)	(0.045)
competition 1mi	low	high	low	high
	1.121	1.118	1.129	1.127
	(0.074)	(0.065)	(0.057)	(0.052)
competition 2mi	low	high	low	high
	1.136	1.099	1.136	1.119
	(0.08)	(0.051)	(0.063)	(0.042)
competition 5mi	low	high	low	high
	1.149	1.088	1.142	1.113
	(0.082)	(0.037)	(0.065)	(0.037)
income	low	high	low	high
	1.133	1.11	1.134	1.126
	(0.072)	(0.07)	(0.058)	(0.054)
education	low	high	low	high
	1.135	1.109	1.135	1.126
	(0.078)	(0.064)	(0.062)	(0.05)
% of people +65 age	low	high	low	high
	1.129	1.114	1.137	1.123
	(0.086)	(0.055)	(0.065)	(0.046)
% people internet home	low	high	low	high
	1.133	1.111	1.135	1.124
	(0.082)	(0.061)	(0.061)	(0.051)

Standard deviation in parentheses.

Table B.4: Comparison of treatment and control

	control	treatment
income	49298.52	50032.73
	(9165.062)	(8772.878)
earners age	43.392	43.392
	(4.662)	(4.519)
number of earners	7327.915	7465.363
	(4944.795)	(4452.06)
Median commuting distance (kms)	10.806	9.57
	(7.356)	(6.946)
gini coefficient	0.471	0.473
	(0.053)	(0.057)
Car as driver	0.276	0.291
	(0.048)	(0.042)
One motor vehicle	0.259	0.265
	(0.059)	(0.055)
Usual Resident Population	12251.86	12336.72
	(7327.96)	(6777.509)
Index of Education and Occupation	976.245	983.53
	(88.463)	(80.113)
competitors (1mi)	2.405	2.558
	(2.249)	(1.805)
competitors (2mi)	5.392	7.128
	(4.617)	(4.343)
competitors (5mi)	20.772	23.543
	(22.499)	(20.502)

Standard deviation in parentheses.

Table B.5: Number of exits and entries by brand

brand	exitter	exitter_market	entrants	entrant_market	total number of petrol stations in the sample
BP	92	42	90	40	260
Caltex	119	49	84	38	254
Shell	111	39	47	23	163
Puma	10	4	46	20	89
Independent	38	15	30	15	75
Gull	68	25	10	3	61
Caltex Woolworths	2	1	38	15	57
Coles Express	4	1	24	11	53
7-Eleven	1	1	37	16	46
Ampol	54	17	7	3	36
Mobil	40	11	14	5	35
Liberty	39	17	11	8	31
Peak	21	9	9	6	30
United	6	1	7	5	26
Vibe	0	0	18	6	20
Kleenheat	29	9	0	0	13
Wesco	20	7	0	0	12
Better Choice	0	0	2	1	8
Amgas	8	6	1	1	6
Eagle	4	2	7	2	6
BOC	4	3	4	1	5
Kwikfuel	4	2	3	1	5
Oasis	0	0	1	1	2
Swan Taxis	4	2	0	0	2
Black and White	0	0	0	0	1
FastFuel 24/7	3	1	3	1	1
Metro Petroleum	0	0	2	1	1
United Fuels West	0	0	0	0	1
TOTAL	681	264	495	223	1299

Table B.6: Margin by brand

brand	frequency	petrol	petrol_margin	diesel	diesel_margin
Eagle	6	139.642	1.164	142.211	1.156
Independent	75	133.078	1.147	137.958	1.147
United Fuels West	1	129.847	1.123	132.161	1.119
BP	260	129.290	1.114	133.051	1.131
Shell	163	127.676	1.108	132.955	1.129
Coles Express	53	136.030	1.103	143.913	1.144
Ampol	36	121.979	1.097	127.092	1.118
Caltex	254	127.026	1.095	131.814	1.121
7-Eleven	46	137.068	1.090	126.891	1.116
Caltex Woolworths	57	131.436	1.089	134.804	1.114
Gull	61	122.681	1.086	127.297	1.105
Wesco	12	115.058	1.086	119.853	1.104
United	26	129.351	1.081	133.313	1.098
Liberty	31	122.115	1.079	126.795	1.092
Vibe	20	130.425	1.078	134.635	1.091
Puma	89	126.971	1.072	132.927	1.106
Kwikfuel	5	121.896	1.058	127.323	1.089
Metro Petroleum	1	135.732	1.056	146.029	1.085
Peak	30	121.682	1.056	127.388	1.085
Better Choice	8	126.182	1.050	131.054	1.065
Amgas	6	114.070	1.048	120.848	1.088
Mobil	35	120.515	1.045	127.508	1.080
FastFuel 24/7	1	124.164	1.042	128.275	1.064
Oasis	2	118.343	1.037	124.152	1.070

Table B.7: Margin by competition

level of competition within				
1 mile	**2 miles**	**5 miles**	**ulp margin**	**diesel margin**
low	low	low	1.150	1.141
high	low	low	1.168	1.153
low	high	low	1.130	1.129
high	high	low	1.147	1.137
low	low	high	1.087	1.111
high	low	high	1.086	1.105
low	high	high	1.085	1.113
high	high	high	1.091	1.115

We split the number of competitors within each radius around their median values. For within 1 mile: 0-2 (low) versus 3 or more (high) competitors, for within 2 miles: 0-5 (low) versus 6 or more (high) competitors, and for within 5 miles: 0-15 (low) versus 16 or more (high) competitors.

Table B.8: Margin by brand by competition by income

station	low income	med income	high income	station	low income	med income	high income
7-Eleven	1.088	1.089	1.084	**Liberty**	1.111	1.101	1.124
n	6	20	9	n	9	11	8
comp 1mi	1.761	1.899	2.764	comp 1mi	1.022	2.389	2.261
comp 5mi	46.821	33.118	31.138	comp 5mi	13.911	22.951	46.469
Ampol	1.158	1.076	1.091	**Mobil**	1.081	1.072	1.068
n	15	6	13	n	5	15	7
comp 1mi	1.683	1.866	2.515	comp 1mi	2.158	2.584	3.149
comp 5mi	7.092	53.11	42.823	comp 5mi	47.131	29.382	47.531
BP	1.169	1.099	1.136	**Peak**	1.118	1.079	1.072
n	66	61	70	n	4	11	13
comp 1mi	2.476	1.548	2.215	comp 1mi	1.976	1.069	1.252
comp 5mi	13.206	28.724	28.633	comp 5mi	30.032	18.292	24.896
Caltex	1.14	1.099	1.12	**Puma**	1.11	1.078	1.091
n	71	59	64	n	15	26	24
comp 1mi	2.254	1.646	2	comp 1mi	1.269	1.854	2.687
comp 5mi	12.741	30.465	26.431	comp 5mi	19.933	28.445	29.227
Caltex Woolworths	1.102	1.081	1.123	**Shell**	1.169	1.1	1.133
n	12	13	13	n	51	33	45
comp 1mi	2.491	0.92	2.195	comp 1mi	2.129	1.434	1.659
comp 5mi	21.877	27.859	18.925	comp 5mi	11.983	19.552	27.305
Coles Express	1.108	1.09	1.112	**United**	1.118	1.064	1.094
n	15	15	18	n	7	7	6
comp 1mi	2.356	2.203	2.431	comp 1mi	4.01	0.805	1.663
comp 5mi	31.78	43.328	32.121	comp 5mi	11.671	28.136	18.224
Gull	1.13	1.108	1.106	**Vibe**	1.109	1.084	1.054
n	23	14	14	n	6	8	5
comp 1mi	2.653	0.946	1.882	comp 1mi	1.756	1.012	3.53
comp 5mi	11.36	17.385	41.297	comp 5mi	8.382	8.85	43.091
Independent	1.184	1.144	1.206				
n	31	17	12				
comp 1mi	1.199	0.752	2.365				
comp 5mi	3.491	3.343	18.521				

Table B.9: Mean margin by income, competition, and population size

		low competition		high competition	
		low population	high population	low population	high population
ulp	low income	1.149 (0.081)	1.102 (0.05)	1.151 (0.074)	1.121 (0.051)
	high income	1.135 (0.085)	1.099 (0.061)	1.096 (0.05)	1.111 (0.069)
diesel	low income	1.144 (0.066)	1.115 (0.038)	1.142 (0.065)	1.122 (0.04)
	high income	1.133 (0.062)	1.123 (0.05)	1.117 (0.043)	1.13 (0.052)

Standard deviation in parentheses.

B.2 Figures

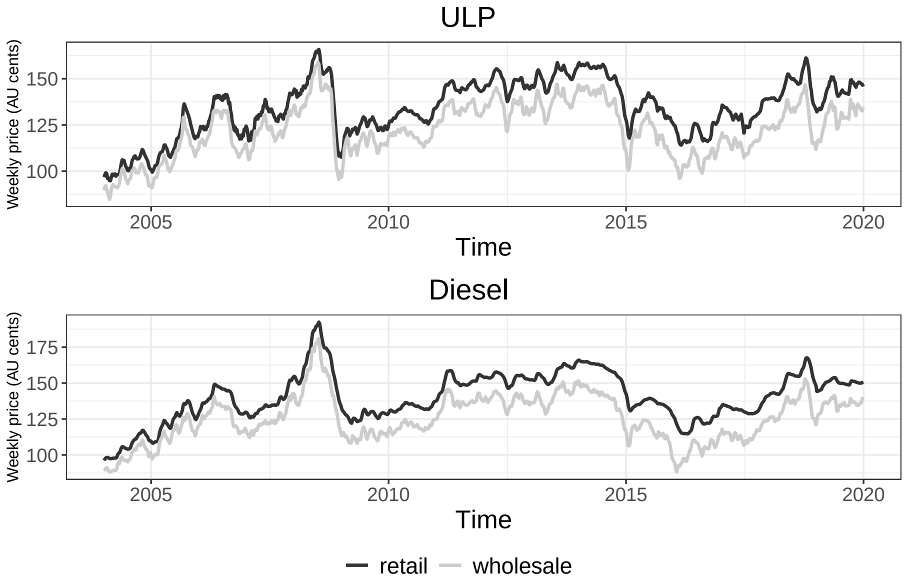

Figure B.9: Weekly retail ULP and diesel retail and wholesale prices over time

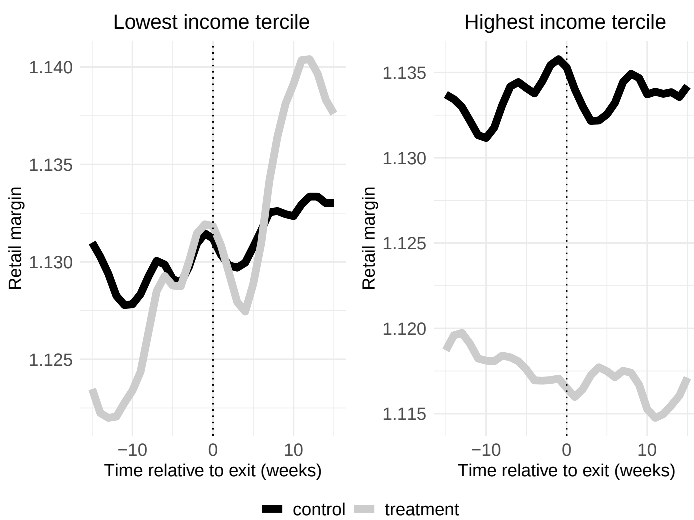

Figure B.10: Retail price margin before and after exit for diesel at different income levels

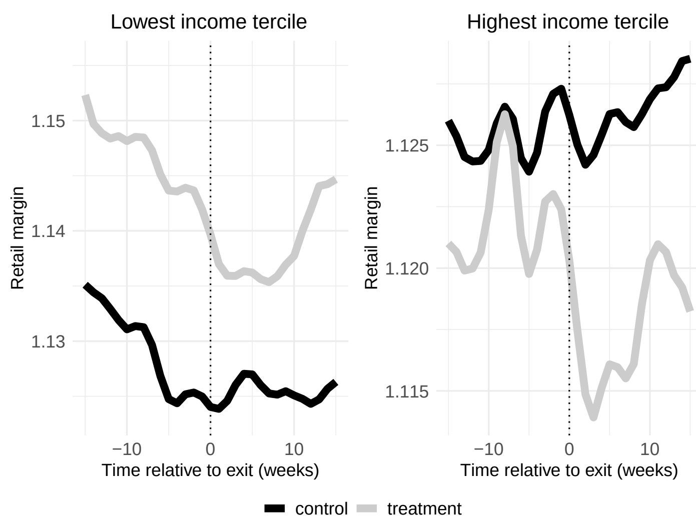

Figure B.11: Retail price margin before and after entry for diesel at different income levels

C Tables and figures for the Results section

C.1 Tables

Table C.1: Number of rivals in the exit and entry samples used for the causal forest estimation

	exit	entry
number of rivals within 1 mile	2.862	2.579
	(2.216)	(1.789)
number of rivals within 2 mile	6.605	5.690
	(4.066)	(5.517)
number of rivals within 5 miles	23.268	15.241
	(19.893)	(21.687)

Table C.2: Top 30 most important features for ULP and Diesel exit

variable	ulp exit	variable	diesel exit
Ferry	0 146	three motor vehicles	0 157
Certificate I II Level	0 13	four or more	0 15
mean estimated business tax	0 097	Ferry	0 144
average no cars	0 089	two motor vehicles	0 121
three motor vehicles	0 081	age 59	0 087
four or more	0 081	weekly residential rent 325-349	0 084
age 35	0 076	Certificate III IV Level	0 08
Motorbike scooter	0 074	one motor vehicle	0 078
age 65	0 065	Motorbike scooter	0 076
Did not go to work	0 06	Internet not accessed from dwelling	0 073
Standard deviation kms	0 059	age 70	0 071
median income	0 059	Certificate I II Level	0 07
age 60	0 058	Did not go to work	0 07
age 40	0 057	average no cars	0 069
age 70	0 057	Certificate Level	0 068
Bus	0 057	Car as driver	0 065
two motor vehicles	0 057	age 35-65	0 061
mean net business income	0 056	median income	0 06
age 75	0 055	age 40	0 058
age 80	0 05	mean net business income	0 057
age 85-99	0 047	age 45	0 057
age 20	0 047	weekly residential rent 125-149	0 057
Certificate III IV Level	0 047	age 85-99	0 056
IEO	0 047	Walked only	0 056
age 15	0 043	weekly residential rent 100-124	0 053
Car as driver	0 043	weekly residential rent 350-374	0 053
age 65 PLUS	0 041	mean estimated business tax	0 052
Certificate Level	0 041	comp5mi	0 052
weekly residential rent 125-149	0 041	age 04	0 051
Median commuting distance kms	0 04	age 35	0 051

The figures refer to importance score of each variable. It is calculated as the decrease in node impurity weighted by the probability of reaching that node.

Table C.3: Top 30 most important features for ULP and Diesel entry

variable	ulp exit	variable	diesel exit
Median commuting distance kms	0.151	age 70	0 136
age 65+	0.144	one motor vehicle	0 115
Interquartile range kms	0.143	three motor vehicles	0 113
Advanced Diploma and Diploma Level	0.143	four or more	0 11
age 75	0.141	age 75	0 103
age verage commuting distance kms	0.138	age 65+	0 103
Car as driver	0.133	mean net rent	0 097
Walked only	0.112	Bus	0 097
age 80	0.109	age 59	0 091
mean-net-rent	0.103	two motor vehicles	0 089
Index of Economic Resources	0.097	age 04	0 086
age 40	0.09	age 80	0 081
Car as passenger	0.09	Certificate I II Level	0 08
Motorbike scooter	0.083	Advanced Diploma and Diploma Level	0 079
Index of Relative Socio-econ Adv and Disadv	0.08	Index of Economic Resources	0 076
age 85-99	0.079	Nil payments	0 068
Ferry	0.079	age 65	0 067
age 65	0.078	age 10	0 066
average-no-cars	0.078	age 45	0 066
age 70	0.077	age 25	0 064
age 35	0.075	None	0 063
age 60	0.072	Interquartile range kms	0 062
Certificate I II Level	0.071	age 60	0 062
four or more	0.071	Truck	0 062
Index of Relative Socio-econ Disadv	0.066	age verage commuting distance kms	0 061
Bus	0.066	age 40	0 061
age 45	0.057	age 0-15	0 06
age 15	0.056	weekly residential rent 250-274	0 058
age 50	0.056	weekly residential rent 325-349	0 058
age 0-15	0.055	weekly residential rent 100-124	0 056
age 59	0.053	weekly residential rent 225-249	0 056

The figures refer to importance score of each variable. It is calculated as the decrease in node impurity weighted by the probability of reaching that node.

Table C.4: Predicted treatment effects of exit in Diesel, by different levels of competition, income, and search

		low internet		high internet	
		low commute	**high commute**	**low commute**	**high commute**
low competition	**low income**	0.222 (0.114)	0.173 (0.098)	0.194 (0.102)	0.143 (0.085)
	high income	0.161 (0.089)	0.11 (0.075)	0.152 (0.081)	0.1 (0.066)
high competition	**low income**	0.093 (0.079)	0.068 (0.066)	0.091 (0.074)	0.06 (0.061)
	high income	0.056 (0.067)	0.026 (0.056)	0.068 (0.063)	0.033 (0.052)

Bootstrapped standard errors in parentheses.

Table C.5: Predicted treatment effects of entry in Diesel, by different levels of competition, income, and search

		low internet		high internet	
		low commute	**high commute**	**low commute**	**high commute**
low competition	**low income**	-0.098 (0.044)	-0.089 (0.038)	-0.102 (0.049)	-0.09 (0.044)
	high income	-0.095 (0.043)	-0.091 (0.037)	-0.083 (0.049)	-0.075 (0.045)
high competition	**low income**	-0.08 (0.038)	-0.073 (0.034)	-0.076 (0.044)	-0.065 (0.04)
	high income	-0.079 (0.038)	-0.076 (0.033)	-0.058 (0.045)	-0.052 (0.042)

Bootstrapped standard errors in parentheses.

Table C.6: Predicted treatment effects of exit in ULP, by different levels of competition, income, commute and % of people age 65+

		low % of age 65+		high % of age 65+	
		low commute	**high commute**	**low commute**	**high commute**
low competition	**low income**	0.246 (0.124)	0.205 (0.114)	0.275 (0.131)	0.249 (0.125)
	high income	0.108 (0.096)	0.068 (0.084)	0.148 (0.093)	0.122 (0.087)
high competition	**low income**	0.116 (0.079)	0.09 (0.069)	0.121 (0.082)	0.108 (0.076)
	high income	0.028 (0.065)	0.003 (0.057)	0.047 (0.058)	0.034 (0.052)

Bootstrapped standard errors in parentheses.

Table C.7: Predicted treatment effects of exit in ULP, by different levels of competition, income, commute and education

		low education		high education	
		low commute	**high commute**	**low commute**	**high commute**
low competition	**low income**	0.318 (0.143)	0.285 (0.136)	0.215 (0.098)	0.191 (0.093)
	high income	0.171 (0.117)	0.138 (0.11)	0.093 (0.081)	0.07 (0.074)
high competition	**low income**	0.175 (0.1)	0.157 (0.093)	0.09 (0.064)	0.08 (0.059)
	high income	0.08 (0.083)	0.062 (0.075)	0.015 (0.054)	0.006 (0.05)

Bootstrapped standard errors in parentheses.

Table C.8: Predicted treatment effects of exit in ULP, by different levels of competition, income, and search - placebo treatment

		low internet		high internet	
		low commute	**high commute**	**low commute**	**high commute**
low competition	**low income**	0.222 (0.114)	0.173 (0.098)	0.194 (0.102)	0.143 (0.085)
	high income	0.161 (0.089)	0.11 (0.075)	0.152 (0.081)	0.1 (0.066)
high competition	**low income**	0.093 (0.079)	0.068 (0.066)	0.091 (0.074)	0.06 (0.061)
	high income	0.056 (0.067)	0.026 (0.056)	0.068 (0.063)	0.033 (0.052)

Bootstrapped standard errors in parentheses.

Table C.9: Predicted treatment effects of **exit** in **ULP**, by different levels of competition, income, and search - [-5,+15] event window

		low internet		high internet	
		low commute	**high commute**	**low commute**	**high commute**
low competition	**low income**	0.291 (0.099)	0.255 (0.091)	0.262 (0.099)	0.239 (0.089)
	high income	0.121 (0.089)	0.078 (0.085)	0.094 (0.09)	0.069 (0.085)
high competition	**low income**	0.161 (0.072)	0.152 (0.065)	0.149 (0.075)	0.15 (0.065)
	high income	0.061 (0.061)	0.044 (0.058)	0.048 (0.064)	0.045 (0.06)

Bootstrapped standard errors in parentheses.

Table C.10: Predicted treatment effects of **entry** in **ULP**, by different levels of competition, income, and search - [-5,+15] event window

		low internet		high internet	
		low commute	**high commute**	**low commute**	**high commute**
low competition	**low income**	-0.287 (0.068)	-0.313 (0.059)	-0.348 (0.066)	-0.366 (0.058)
	high income	-0.282 (0.062)	-0.314 (0.056)	-0.33 (0.059)	-0.355 (0.054)
high competition	**low income**	-0.211 (0.042)	-0.241 (0.037)	-0.272 (0.045)	-0.292 (0.04)
	high income	-0.208 (0.036)	-0.245 (0.035)	-0.256 (0.04)	-0.284 (0.037)

Bootstrapped standard errors in parentheses.

Table C.11: Predicted treatment effects of exit (within 2 miles) in ULP, by different levels of competition, income, and search

		low internet		high internet	
		low commute	**high commute**	**low commute**	**high commute**
low competition	**low income**	0.194 (0.131)	0.162 (0.111)	0.17 (0.12)	0.15 (0.097)
	high income	0.101 (0.099)	0.082 (0.077)	0.077 (0.1)	0.068 (0.074)
high competition	**low income**	0.145 (0.091)	0.107 (0.078)	0.139 (0.081)	0.112 (0.067)
	high income	0.078 (0.055)	0.047 (0.046)	0.069 (0.055)	0.046 (0.044)

Bootstrapped standard errors in parentheses.

Table C.12: Predicted treatment effects of exit (within 5 miles) in ULP, by different levels of competition, income, and search

		low internet		high internet	
		low commute	**high commute**	**low commute**	**high commute**
low competition	**low income**	0.123 (0.088)	0.085 (0.064)	0.053 (0.086)	0.025 (0.06)
	high income	0.068 (0.08)	0.05 (0.059)	0.018 (0.086)	0.005 (0.06)
high competition	**low income**	0.067 (0.085)	0.062 (0.064)	0.011 (0.079)	0.011 (0.058)
	high income	0.009 (0.079)	0.029 (0.06)	-0.029 (0.081)	-0.009 (0.059)

Bootstrapped standard errors in parentheses.

Table C.13: Predicted treatment effects of **exit** in **ULP**, by different levels of competition, income, and search - clustered by postcode

		low internet		high internet	
		low commute	**high commute**	**low commute**	**high commute**
low competition	**low income**	0.168 (0.076)	0.154 (0.066)	0.162 (0.086)	0.159 (0.076)
	high income	0.105 (0.072)	0.086 (0.063)	0.082 (0.083)	0.074 (0.072)
high competition	**low income**	0.105 (0.08)	0.078 (0.053)	0.087 (0.073)	0.073 (0.054)
	high income	0.055 (0.076)	0.025 (0.049)	0.021 (0.071)	0.004 (0.051)

Table C.14: Predicted treatment effects of **entry** in **ULP**, by different levels of competition, income, and search - clustered by postcode

		low internet		high internet	
		low commute	high commute	low commute	high commute
low competition	low income	-0.332 (0.05)	-0.325 (0.045)	-0.37 (0.056)	-0.36 (0.051)
	high income	-0.333 (0.04)	-0.329 (0.036)	-0.366 (0.042)	-0.358 (0.038)
high competition	low income	-0.286 (0.044)	-0.279 (0.04)	-0.326 (0.048)	-0.317 (0.044)
	high income	-0.292 (0.033)	-0.288 (0.03)	-0.327 (0.036)	-0.319 (0.032)

Table C.15: Predicted treatment effects of **exit** in **ULP**, by different levels of competition, income, and search - nearest 2 control firms

		low internet		high internet	
		low commute	high commute	low commute	high commute
low competition	low income	0.004 (0.061)	-0.022 (0.033)	0.024 (0.057)	0.007 (0.034)
	high income	0.151 (0.091)	0.125 (0.081)	0.168 (0.079)	0.15 (0.068)
high competition	low income	0.134 (0.082)	0.08 (0.06)	0.171 (0.076)	0.128 (0.056)
	high income	0.337 (0.136)	0.282 (0.123)	0.367 (0.12)	0.322 (0.106)

Table C.16: Predicted treatment effects of **entry** in **ULP**, by different levels of competition, income, and search - nearest 2 control firms

		low internet		high internet	
		low commute	high commute	low commute	high commute
low competition	low income	-0.302 (0.06)	-0.332 (0.059)	-0.374 (0.058)	-0.388 (0.056)
	high income	-0.281 (0.053)	-0.319 (0.055)	-0.346 (0.053)	-0.367 (0.052)
high competition	low income	-0.237 (0.041)	-0.271 (0.043)	-0.313 (0.045)	-0.33 (0.044)
	high income	-0.22 (0.037)	-0.262 (0.041)	-0.288 (0.042)	-0.312 (0.041)

C.2 Figures

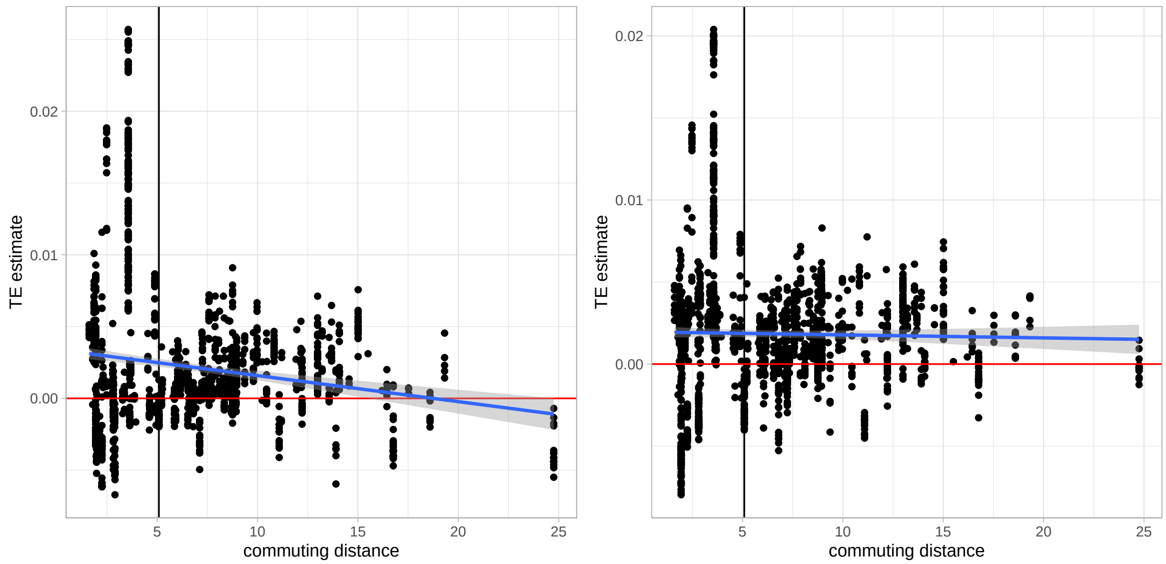

Figure C.12: Treatment effects by commuting distance

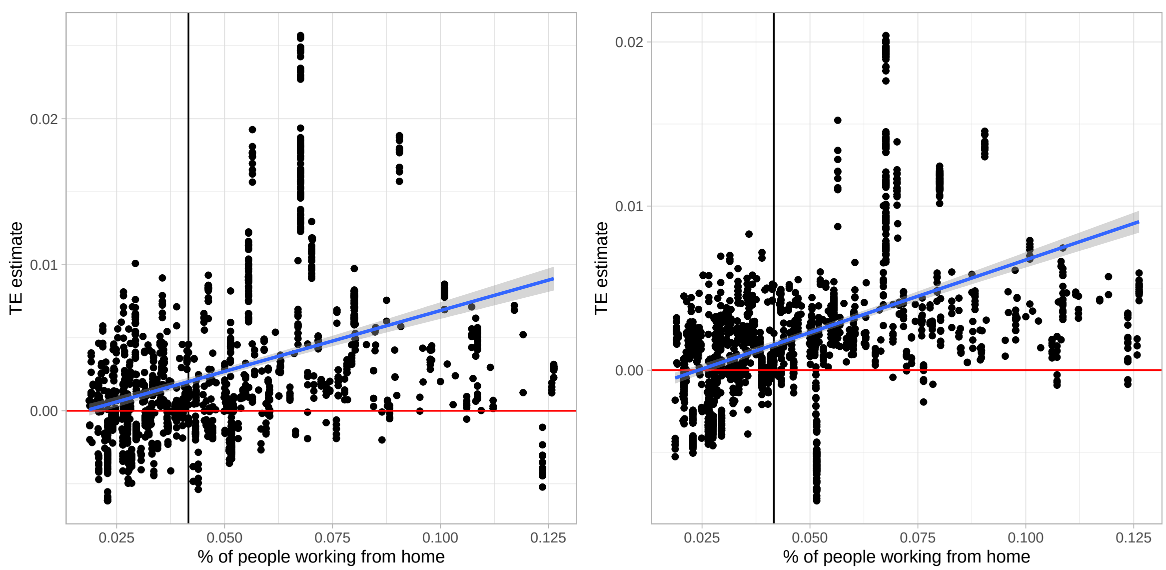

Figure C.13: Treatment effects by home working prevalence

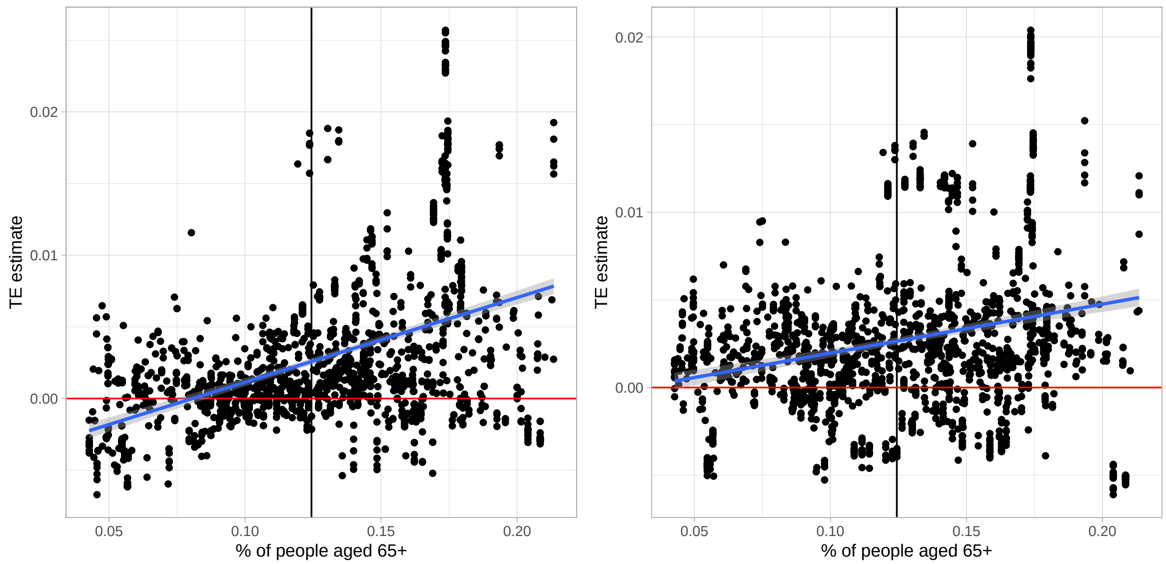

Figure C.14: Treatment effects by % of people aged 65+

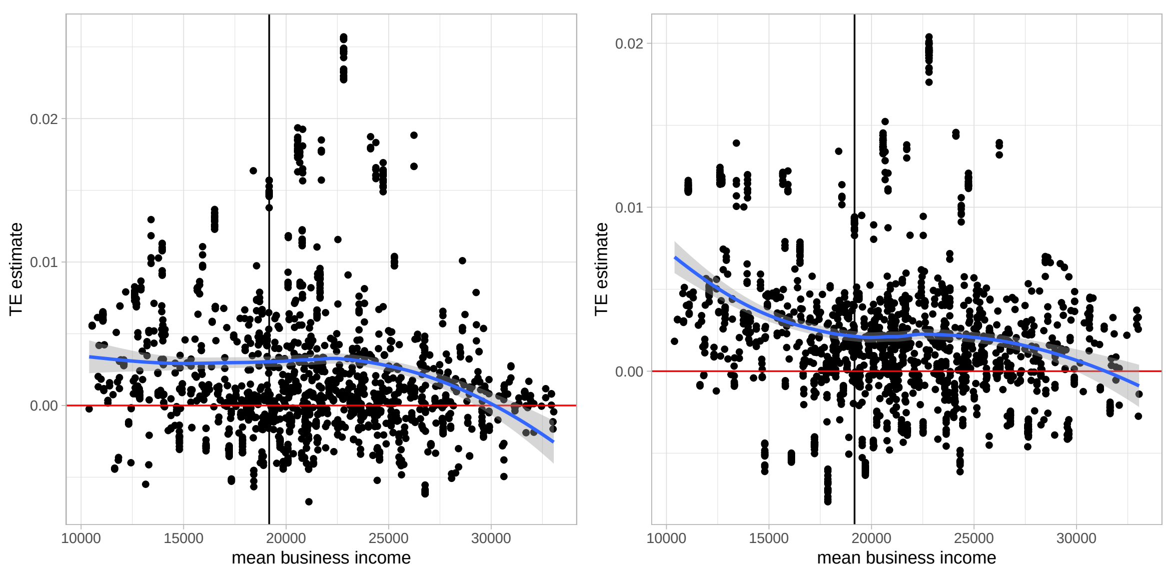

Figure C.15: Treatment effects by business income

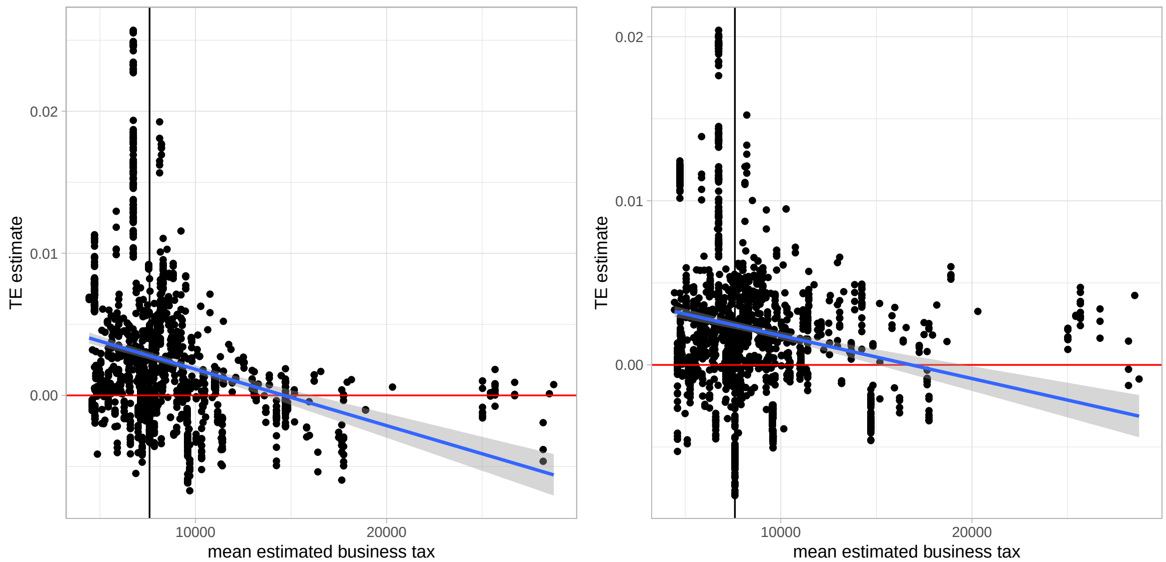

Figure C.16: Treatment effects by business tax

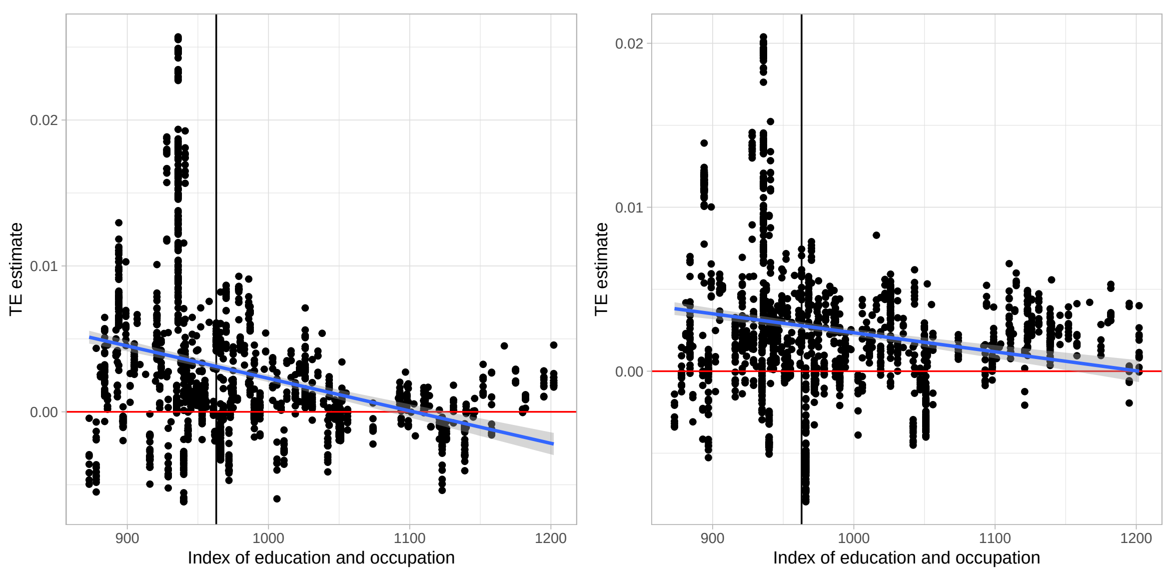

Figure C.17: Treatment effects by educational level