Public Transportation Pricing and Car Use

Kimmo Palanne[1,2]

[1]Aalto University
[2]VATT Institute for Economic Research

Abstract

This paper provides novel evidence on the effects of public transportation pricing on car use and CO_2 emissions. I analyze a pricing reform in the Finnish capital Helsinki that lowered public transit fares by 45 percent for individuals who ended up living in a specific, newly introduced travel zone. Using a difference-in-differences approach, I compare individuals who received the price reduction to those who lived just outside the travel zone and experienced almost no change in prices. This comparison is made possible by detailed individual-level data on vehicle mileage and ownership, as well as residential locations. I estimate the cross-price elasticity of driving to range between 0.06 and 0.27. However, I find no clear response in car ownership, either at the extensive or intensive margin. Based on a back-of-the-envelope calculation, the cost of reducing emissions with this reform landed in the range of 1000–3000 euros per tonne of CO_2.

1 Introduction

Transportation is one of the largest contributors to global CO_2 emissions, producing about 20% of all emissions. Moreover, nearly a half of transportation-related emissions come from passenger road transportation.[1] Insofar as rising global incomes increase the demand for cars and driving, we may even see the transportation sector increase its emissions share in the future. Successful climate change prevention thus requires implementing policies that are effective at promoting greener modes of transportation or reducing total kilometers driven.

The textbook solution to correcting the climate externality is a tax on CO_2, which is equivalent to a fuel tax in the case of transportation. A carbon tax incentivizes reducing fuel consumption at all relevant margins and is in many cases the most cost effective way to achieve emissions reductions (Goulder and Parry 2008; Anderson and Sallee 2016). However, current fuel tax levels might be below ideal levels if we compare them to estimates of the damages caused by CO_2 emissions.[2] Moreover, even if policy-makers wanted fuel taxes to play a more prominent role in fighting climate change, dramatic tax increases could provoke a backlash from voters. Fuel tax increases are often strongly opposed because of their perceived unfairness (Carattini, Carvalho, and Fankhauser 2018; Maestre-Andrés, Drews, and Bergh 2019), as exemplified by the yellow vests movement in France (Carattini, Kallbekken, and Orlov 2019). These complications raise the question whether implementing complementary policies that create additional incentives to lower fuel consumption could help in decarbonizing the transportation sector.

In this paper, I study how effective public transportation pricing is in reducing car use and ultimately CO_2 emissions from passenger road transportation. Public transportation as a substitute for passenger vehicles has the potential to create sizeable reductions in car use especially in urban areas. Compared to building new public transit infrastructure, changing the fare system is a more readily available way to make public transportation more attractive. Pricing decisions are therefore relevant in places where vast public transit networks are already in place. To provide evidence of the effectiveness of public transit pricing, I estimate the elasticity of vehicle kilometers traveled and car ownership with respect to public transit fares in the Helsinki region in Finland. Specifically, I exploit a travel zone reform that induced plausibly exogenous variation in public transit fares based on individuals' home locations. The reform created a natural experiment in which individuals who happened to be included in a specific travel zone received an annual fare

1. Source: https://ourworldindata.org/co2-emissions-from-transport (Accessed October 27, 2023)

2. In 2018, the average taxes on gasoline and diesel in OECD countries corresponded to effective tax rates of about €86 and €74 per tonne of CO_2, respectively (OECD 2019). In contrast, estimates of the social cost of carbon calculated by Pindyck (2019) based on survey responses from over 500 expert economists and climate scientists range from about \$80 to \$300 per tonne of CO_2. Bilal and Känzig (2024) propose an even higher value of more than \$1000 per tonne of CO_2.

reduction of €526, or 45 percent. In contrast, those whose homes were excluded from the travel zone were not eligible for the large fare reduction. In addition to estimating the elasticity of car use, I evaluate the climate impacts of this substantial reduction in public transportation prices.

The fare reduction was a part of a larger travel zone reform implemented by the public transit provider Helsinki Region Transport in April 2019. The reform replaced municipality border-based pricing with a fare system built around travel zones that crucially do not follow municipality borders. Prior to the reform, any two individuals traveling between the same municipalities always faced an identical fare. In the new system, however, individuals face different ticket prices when traveling between the same municipalities if their trips start or end within different travel zones. In particular, some individuals traveling to the capital Helsinki from the neighboring municipality of Vantaa can purchase tickets at substantially lower prices in the new system, while others experienced little to no change in fares. Access to the lower fare depends on which side of the new travel zone borders people's homes happened to be located. Crucially, the new borders were not revealed to the public until a few months before the reform. Furthermore, the choice of where to draw them did not directly depend on any population characteristics.

I estimate the effect of the transit pass fare reduction on car use by employing a difference-in-differences approach in which I compare individuals whose homes were barely included in the travel zone with the lower prices to those were barely excluded. I zoom within 600–2000 meters of the travel zone border and assume that car use among people on both sides of the border would have followed parallel trends in the absence of the reform. I measure car use with annual vehicle kilometers traveled (VKT) as well as car ownership. This comparison is made possible by rich vehicle-level data on odometer readings recorded during mandatory vehicle inspections along with individual-level data on car ownership and individuals' residential locations.

I find that being eligible for the 45 percent fare reduction resulted in a clear decrease in annual VKT. I estimate that the elasticity of VKT with respect to the price is around 0.06–0.27 depending on the specification. These effects are fairly large, given that the highest elasticity estimates are comparable in size to many estimates of the short-run price elasticity of fuel demand (e.g. Bento et al. 2009; Gillingham 2014; Knittel and Sandler 2018; Gillingham and Munk-Nielsen 2019). However, I find no change in car ownership either at the extensive or intensive margin. I also show that the reductions in VKT translate into a clear decrease in emissions. Providing an estimate of the cost of emissions reductions is challenging without data on ticket purchases. Nonetheless, back-of-the-envelope calculations suggest that the cost of emissions reductions could land in the range of €1000–€3000 per tonne of CO_2. This makes public transit pricing a relatively expensive tool to achieve emissions reductions if the cost is compared to estimates of the social cost of carbon (Pindyck 2019) or the cost of other policies that create emissions reductions (Gillingham and Stock 2018).

This paper contributes to the broader literature on the effects of public transportation pricing on car use. This study is to the best of my knowledge the first to provide explicit estimates of the elasticity of car use with respect to public transit prices using individual-level data on VKT and car ownership. Some papers do provide elasticity estimates but use different methods and focus on travel mode choices instead of VKT or car ownership directly. A meta-analysis by Wardman et al. (2018) suggests that the cross elasticity of choosing a car as a travel mode with respect to public transit fares is around 0.06–0.08. However, almost no papers in the literature use data on actual VKT, for instance from odometer readings, as this has been a relatively recent development in the literature.[3] Furthermore, these studies usually use survey data to evaluate impacts on car use, e.g. Cats, Susilo, and Reimal (2017) who analyze the effects of free public transit in Tallinn, Estonia. The working paper by Andor et al. (2023) provides evidence on the effects of pricing on VKT but also relies on survey data.

The analysis in this paper is also closely related to previous literature on the effects of public transportation accessibility and availability on car use. A meta-analysis by Ewing and Cervero (2010) suggests that if availability is measured by distance to transit stops, the cross-elasticity is around 0.05. Wardman et al. (2018) also report mean elasticities of car use with respect to public transit journey time between 0.04 and 0.14. Even though changing the pricing of public transit could be regarded as a less drastic measure than improving service and access, the existing elasticity estimates related to both policies are quite similar. Estimating the causal effect of availability is, however, complicated by endogeneity arising from residential sorting. An attempt to address this concern is offered by Gillingham and Munk-Nielsen (2019) who use population information from a hundred years earlier as an instrument for public transportation accessibility in Denmark. They do not directly evaluate the elasticity of VKT with respect to accessibility but estimate that VKT is more responsive to fuel price changes among commuters living far away from urban centers and argue that this is explained by better availability of public transit.

This paper is also among the first to explicitly quantify the climate impacts and evaluate the cost effectiveness of lower public transit fares via their effect on VKT and car ownership. Andor et al. (2023) evaluate the climate impacts of a €9 ticket in Germany and estimate that the policy had a cost of emissions reductions of around €2800 per tonne of CO_2. This cost estimate is similar in size to the highest cost estimates in my study. However, the study by Andor et al. (2023) differs from mine in two important ways.

3. Odometer data on VKT has mainly been used to estimate the rebound effect of fuel efficiency improvements or the elasticity of VKT with respect to fuel prices (e.g. Gillingham 2014; Gillingham, Jenn, and Azevedo 2015; De Borger, Mulalic, and Rouwendal 2016a, 2016b; West et al. 2017; Knittel and Sandler 2018)

First, the data on VKT in Andor et al. (2023) comes from a survey instead of odometer readings. Second, the €9 ticket policy evaluated in the paper, while substantial in size, only lasted for three months. In contrast, I evaluate the effects of a permanent reduction in public transit prices. A temporary price reduction might not create as large of a behavioral response.

The results presented in this paper do have some limitations. First, since I cannot observe whether the individuals I am analyzing actually wanted to purchase the transit passes with the lower prices, my treatment effect estimates represent intention-to-treat effects. Even though I do not observe how many drivers in my sample use public transit, survey evidence provided by Helsinki Region Transport suggests that most of those that do would normally purchase the types of tickets my analysis pertains to. Second, my preferred estimates of the elasticity of VKT only pertain to cars that are 10 years or older. Estimating annual VKT for cars under 10 years old is not as reliable due to these cars having less frequent inspections and thus fewer odometer readings. Cars under 10 years old constitute about half of all cars in my sample. However, I provide evidence that the effects are qualitatively similar if all cars regardless of age are included in the sample.

Finally, my treatment effect estimates only apply to a specific group of individuals living close to the travel zone border and represent short to medium-run individual-level estimates from a few years after the reform. Even if drivers around the border reduced their car use, the effects on emissions on aggregate could be very different. On reason for this is that the longer-run macro-level elasticities could be smaller and very close to zero because of the "fundamental law of road congestion" discussed by Duranton and Turner (2011). They argue that providing more public transportation ultimately has no effect on driving because the road capacity freed up by drivers switching from cars to public transit is met with a proportional increase in driving by other people, who now view the less congested roads as more desirable.

The remainder of this paper is organized as follows. Section 2 provides details of the Helsinki Region Transport pricing reform. Section 3 presents the administrative vehicle and individual-level data used in the analysis. Section 4 discusses both the estimation strategy and the identification strategy, after which descriptive statistics are presented in Section 5. The results are presented in Section 6, and a discussion of the limitations of the results is included in Section 7. Section 8 evaluates the climate impacts of the pricing reform and assesses its cost effectiveness. Finally, Section 9 concludes the study.

2 Helsinki Region Transport Pricing Reform

Helsinki Region Transport (HSL) provides public transportation for the over 1.3 million people living in the Finnish capital of Helsinki and eight surrounding municipalities.

Passengers can purchase single tickets as well as transit passes for longer periods of time. The tickets give passengers unlimited access to board and transfer between buses, trams, the metro, local trains and a ferry for the duration of the ticket. Traveling to Helsinki from the other municipalities in the region is very common, for example to work, to study or for leisure purposes. In 2021, about 33 percent of people working in Helsinki lived in the surrounding municipalities.[4]

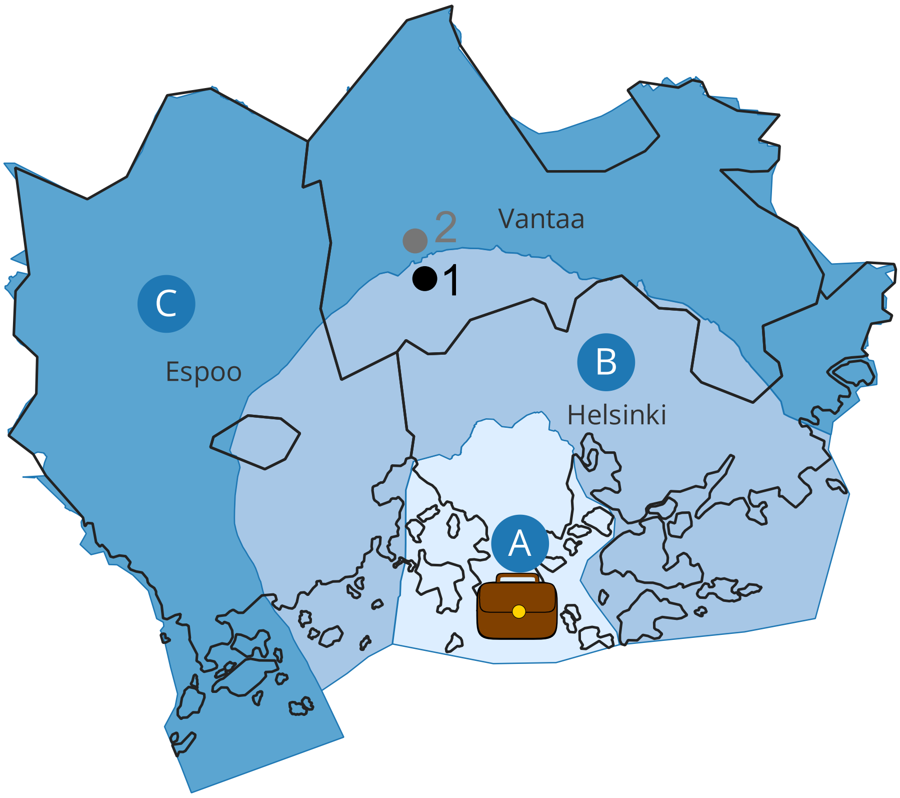

Figure 1: Map of the Helsinki capital region overlaid with HSL travel zones A–C

The pricing of tickets was drastically changed in 2019. Prior to the pricing reform, the prices of both single tickets and transit passes were determined by whether passengers wanted to cross municipality borders during their trips. The cheapest cross-municipality tickets cost nearly twice as much as single-municipality tickets. On April 27, 2019, HSL replaced this municipality border-based pricing scheme with a fare system built around four new travel zones, A, B, C and D. While the basic idea remained the same in that ticket prices depend on whether passengers want to travel across different zones, the crucial difference is that the new travel zones do not follow municipality borders. Figure 1 illustrates how the travel zones A, B and C instead resemble rings around downtown

4. Source: https://kaupunkitieto.hel.fi/fi/helsingissa-tyoskentelevat-ovat-yha-harvemmin-helsinkilaisia-etenkin-seudun-ulkopuolelta (Accessed September 2, 2024)

Helsinki, located inside travel zone A. Notably, the travel zones span multiple municipalities and also divide Espoo and Vantaa, the two most populous municipalities neighboring Helsinki, each into two different travel zones.

One of the primary purposes of the pricing reform stated by HSL was to eliminate the steep price increases at municipality borders and make shorter inter-municipality trips cheaper. The introduction of the travel zones created substantial ticket price reductions for a large number of inter-municipal travelers. Before the reform, any passengers traveling between the same municipalities always faced an identical fare. In the new system, however, the same passengers face different ticket prices if their trips start or end within different travel zones in the same municipalities. Frequent public transit users, such as commuters, benefited the most from these price reductions, as transit passes saw the largest price decreases of up to 45 percent.[5] The changes in the prices of single tickets were similar but smaller in magnitude.

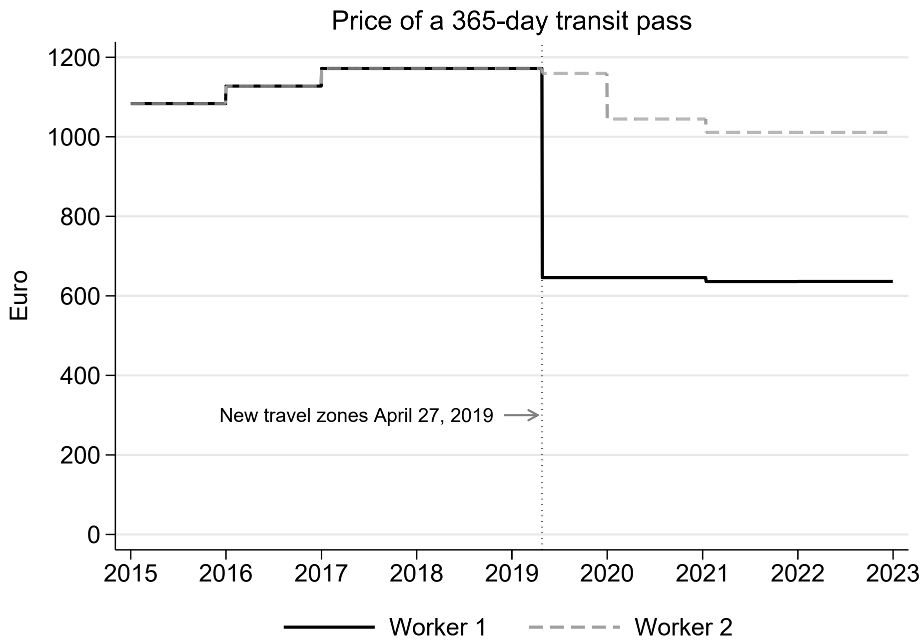

Figure 2: Annual transit pass prices for the two workers in Figure 1 commuting between Vantaa and Helsinki

As an example of the inter-municipality ticket price changes, consider workers 1 and 2 in Figure 1. Both workers commute between their homes in Vantaa and their jobs, indicated by the briefcase, in downtown Helsinki around 15 kilometers away. The cheapest transit pass enabling daily travel between Vantaa and Helsinki cost €1172 annually for

5. While inter-municipality travel became cheaper for many, ticket prices did, however, increase by about 7.5 percent for individuals only traveling within a single municipality.

both workers prior to the reform . After the reform, worker 1, who lives in zone B and commutes to zone A, only needs a transit pass allowing travel in zones A and B. On the other hand, worker 2 has to purchase a transit pass covering all three zones A, B and C to get from zone C to A. As shown in Figure 2, the annual price for worker 1 was cut nearly in half as it fell by €526, from €1172 to €646. Worker 2 only experienced an initial price reduction of €13, from €1172 to €1159. HSL further reduced the prices of some tickets on January 1, 2020 and on January 14, 2021. By the end of 2022, worker 1 ultimately paid €636 for the transit pass whereas worker 2 received a slightly larger price reduction and paid a price of €1011. The price for worker 2 still remained €375 higher than the price for worker 1.

Prior to the reform, HSL anticipated that the use of public transportation would increase especially in travel zone B, where ticket prices would be falling the most. In addition, HSL predicted that trips to Helsinki on public transit would increase, while fewer trips to Helsinki would be made by car. HSL studied the potential effects of the reform on public transit usage by measuring changes in public transit ridership from September through November 2018, before the reform, to September through November 2019, after the reform. Results from zones B and C in Espoo and Vantaa are presented in Table 1. Based on the ridership measures by HSL, the usage of both trains and buses increased substantially more in zone B relative to zone C in both municipalities. This suggests that the large price decreases in zone B relative to zone C could have increased public transit usage.

Table 1: Changes in public transit ridership from September through November 2018 to September through November 2019 according to HSL

	Change in train ridership	Change in bus ridership
Vantaa Zone B	+13%	+12%
Vantaa Zone C	+8%	+1%
Espoo Zone B	+15%	+4%
Espoo Zone C	+0%	+1%

HSL also conducted a survey in which Helsinki region residents were asked whether and how they had changed their car and public transit use after the reform. The results indicate that self-reported public transit use increased in zone B relative to zone C while self-reported car use decreased in zone B relative to zone C.[6] Expecting to find effects on

6. Source: https://hslfi.azureedge.net/globalassets/hsl/tutkimukset/muut-tutkimukset/vyohykeuudistuksen_vaikutukset_liikkumiseen_yhteenveto_05_2020.pdf (Accessed October 27, 2023)

car use among people living in zone B is thus consistent with both the goals of the reform and the suggestive evidence provided by HSL on increases in public transit ridership and decreases in self-reported car use.

3 Data

3.1 Vehicle Data

To estimate how car use might have been affected by the pricing reform, I use detailed vehicle-level data on all vehicles registered in Finland. The data are provided by Statistics Finland and comes from the official vehicle registry maintained by Traficom, the Finnish Transport and Communications Agency. The data consist of two parts. One has odometer readings recorded during mandatory vehicle inspections, covering readings from all inspections done to any vehicle since 2013. The other includes quarter-yearly cross-sections of the entire stock of vehicles in Finland on the last day of March, June, September and December each year starting from 2013. Each cross-section includes detailed technical information on each individual vehicle, such as fuel type[7], fuel economy, and make and model, as well as information on the owner[8] of the vehicle. All the vehicles and vehicle owners have a unique pseudonymized identifier and can be followed in time across the cross-sections and in the odometer readings data.

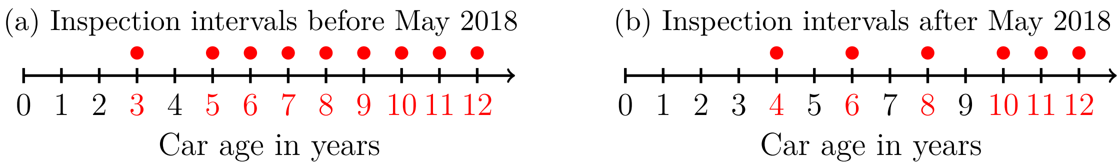

Figure 3: Mandatory inspection intervals by car age

The odometer readings make it possible to calculate vehicle kilometers traveled during the time between any two inspections for any vehicle. Here I use data only on passenger cars[9], which are the most commonly used type of vehicle in Finnish households. All cars

7. Gasoline, diesel, electricity etc.

8. Each vehicle has up to two different owners: the legal owner and the registered owner. From the legal perspective, the legal owner is the person who legally owns the vehicle, whereas the registered owner is another person who is the primary user of the vehicle. The registered owner is e.g. responsible for paying the annual vehicle tax. In line with the legal purpose, I assign the registered owner as the primary user of a vehicle if one has been reported. Otherwise I treat the owner as the primary user.

9. According to Finnish law, passenger cars are motor vehicles that are primarily used for transporting people and have at least four tires, a maximum speed exceeding 25 kilometers per hour and no more than eight seats in addition to the driver's seat.

are mandated by law to be inspected at regular intervals, the lengths of which depend on the age of the car, as shown in Figure 3. Prior to May 2018, cars had to be inspected for the first time after being in use for three years. The next inspection had to be done at five years old, after which inspections were required every year. Starting May 2018, mandatory inspections are more infrequent during the first ten years of a car's lifespan, while yearly inspections are still required after that. The first inspection is not required until after four years and subsequent inspections must be done every two years until the vehicle has been in use for ten years.

Ideally, I would like to observe VKT for each driver during every 12-month period before and after the pricing reform on April 27, 2019. However, this is not possible because 1) not everyone has their car inspected on April 27 each year, 2) an individual might drive multiple different cars during any period and 3) newer cars are inspected less frequently than older cars. In the absence of this perfect data on VKT, I estimate VKT at the individual level for every 12-month period before and after the pricing reform. To produce a VKT estimate for a specific period, I use odometer readings from all cars that an individual used during the period. Specifically, I assign VKT from each car to all of its drivers and all periods based on how many days each driver owned the car and by how much the inspection intervals overlap with the periods.[10] The 12-month periods in my sample extend from April 27, 2015 to April 27, 2023.

To detect changes in annual VKT in the most accurate way possible, my preferred VKT estimates only include kilometers from cars that are at least 10 years old. This guarantees that the cars were inspected approximately every 12 months. Including newer cars in the sample introduces a lot of measurement error due to the odometer readings being up to four years apart. In addition, estimating annual VKT for the newest cars is not possible during the last years in the sample, i.e. during post-reform years, because the cars have not yet been inspected. While ignoring newer cars might seem drastic, it is worth pointing out that the average car registered in Finland is around 12 years old. In my main sample, almost 50 percent of drivers have non-missing VKT observations for cars 10 years or older. Nearly 40 percent of drivers do not own any cars under 10 years old. The obvious problem with omitting newer cars is that VKT responses among older cars might be very different to those among newer cars. To address this concern, I also provide some results for a sample that consists of all cars regardless of age.

10. I first calculate for each inspection interval of a given car the average VKT per day. I then allocate the inspection interval-specific VKT among all individuals who owned the car during the interval by multiplying the daily average VKT by the number of ownership days. Finally, I assign the resulting kilometers into the 12-month periods before and after the reform based on the number of overlapping days between the inspection intervals and the periods. After repeating this for all cars, I sum up all the kilometers to obtain individual-level annual VKT estimates.

3.2 CO_2 Emissions

Combining the VKT estimates with car-level data on CO_2 emissions intensity, in grams per kilometer driven, I also estimate CO_2 tonnes emitted by each individual for all 12-month periods before and after the reform. However, the vehicle data unfortunately do not contain information on CO_2 emissions intensity for all cars. About 25 percent of all cars in Finland around the time of the pricing reform had a missing CO_2 value, with nearly all of them being cars registered before the year 2000.

To produce CO_2 emissions estimates for all drivers, I impute the missing CO_2 values based on the non-missing CO_2 values. I regress CO_2 emissions intensity on dummies for fuel type, make and model, and for cars registered after the year 2007, after which the European Union started regulating the fuel economy of new cars. I also add linear controls for car weight, cylinder volume and the registration year of the car, as well as various interactions between the variables. The model produces estimates that are highly accurate for the sample of cars with non-missing values, as the correlation between the OLS estimates and the real values is 0.94. This high accuracy in the estimating sample makes it plausible that the out-of-sample predictions are reasonably reliable as well.

The CO_2 data also have another source of unreliability outside of the problem related to missing values. The data originate from car manufacturers, who have been shown to dramatically understate the emissions and fuel consumption of cars (e.g. Tietge et al. 2019; Reynaert and Sallee 2021). To get more realistic CO_2 emissions estimates, I inflate the car-level CO_2 emissions intensity values in the data using estimates of the difference between real-world and official values produced by ICCT, the International Council on Clean Transportation (Dornoff, Morales, and Tietge 2019). The ICCT estimates, plotted in Figure 10 in the Appendix, represent the average gap between manufacturer-reported and real-world emissions by vehicle model year[11] based on measurements done for vehicles in Germany.

3.3 Data on Individuals Living in Finland

Using the pseudonymized identifier for the owner of each car, I merge the vehicle data with administrative individual-level population data provided by Statistics Finland. The population data contain information on e.g. age, gender, annual income, employment, residential location and household members for all individuals living in Finland on the last day of each calendar year. Crucially, the residential locations are given at the level of a 250 meters by 250 meters grid covering the entire country. By determining which

11. In the data, I only observe the year each car was first registered, but this should be very close to the year the car was built.

grid cells are located inside each HSL travel zone[12], I assign each individual a home HSL travel zone. I also calculate for each individual the distance to the border between travel zones B and C based on the center point of the home grid cell.

4 Empirical Strategy

4.1 Basic Approach

I estimate the effect of public transportation fares on car use by employing a difference-in-differences approach combined with a border discontinuity created by the pricing reform. Specifically, I compare changes in VKT and car ownership between people who lived in the same municipality but ended up on different sides of the B-C travel zone border. As demonstrated in Figure 1, these people paid the same ticket prices before the pricing reform but then faced different fare decreases after the zones were introduced on April 27, 2019. I assign to the treatment group individuals who lived in travel zone B close to the B-C travel zone border. The comparison group, on the other hand, consists of people who lived in travel zone C close to the border.

Determining which individuals were actually treated, however, is complicated by the fact that I do not observe the types of tickets different people bought either before or after the reform. When I assign individuals into the treatment and comparison group strictly based on the travel zone border, the implicit assumption is that all individuals in my sample would have always bought a ticket warranting travel between their home and downtown Helsinki in travel zone A. Under this assumption, all individuals living in zone B would have bought an AB transit pass and received an initial fare reduction of 45 percent. In contrast, individuals living in zone C would have had to buy an ABC transit pass and seen almost no change in the price of the transit pass in April 2019.

Unfortunately, this uncertainty over which tickets the individuals in my sample were buying might introduce some bias in my estimates. In particular, having individuals assigned into the incorrect group would bias my estimates towards zero. As an example, some individuals living in zone C could prefer to travel to Helsinki but only to regions in zone B instead of zone A. This would have allowed them to purchase a BC transit pass instead of an ABC transit pass. Because the BC pass has the same price as the AB pass, these individuals assigned into the comparison group would have in reality also received the treatment. Similarly, some individuals in zone B might have needed an ABC transit pass after the reform, meaning that they saw no decreased in ticket prices and should have been assigned to the comparison group instead. Individuals in the comparison group in zone C could have also benefited from the reduced fares for example by first walking

12. Data on the exact travel zone boundaries is provided by HSL at https://public-transport-hslhrt.opendata.arcgis.com/datasets/hsln-maksuvy%C3%B6hykkeet

over the border and boarding public transit in zone B. To reduce the probability of having these individuals in my sample, I drop all individuals who live in grid cells directly on top of the travel zone border.

To alleviate these problems related to the uncertainty of ticket types, I zoom in as close to the border as possible. I assume that the shorter the distance to the border is, the more similar individuals on different sides of the border are in terms of their ticket preferences. Despite focusing on individuals close to the border, some degree of uncertainty is inevitable. For this reason, the treatment effect estimates should be interpreted as intention-to-treat effects, i.e. only the effect of being eligible for the transit pass fare reduction.

Even though I do not have data on ticket purchases, a survey conducted by HSL in January 2020 sheds light on the types of tickets normally bought in different municipalities and travel zones. In Vantaa, one of the two municipalities divided into zones B and C, 80 percent of public transit users in zone B primarily bought an AB ticket, while 60 percent of public transit users living in zone C normally opted for an ABC transit pass.[13]. The second most popular ticket type in both travel zones was the BC ticket, with a share of 15 percent in zone B and 31 percent in zone C. Individuals purchasing a BC ticket in either zone could have benefited from the 45 percent fare reduction with prices falling from €1172 to €646 per year. Alternatively, if they only needed to travel within Vantaa, their annual transit pass fare would have increased slightly from €601 to €646 instead. Even though the BC tickets make treatment assignment a bit complicated, most people in my sample are likely to be assigned into the treatment or comparison group in the correct way.

4.2 Analysis in the Municipality of Vantaa

The pricing reform divided people into zones B and C in two municipalities, namely Espoo and Vantaa, but in this paper I only focus on Vantaa. The main reason for this is that Espoo saw major changes in public transit routes when a new metro line was opened 1.5 years before the pricing reform. The new metro line extended very close to the border between zones B and C but did not reach zone C. For individuals living very close to the metro line, public transit became a more attractive alternative. However, the metro line caused many bus lines to be rerouted, which made public transit a slower option for many people living a bit further away. Overall, disentangling the effects of the metro line and the pricing reform would be challenging. The only major change in the public transit network in Vantaa took place four years before the reform in 2015, when a new train line was built. The line had no clear asymmetrical effects on public transit

13. Source: https://hslfi.azureedge.net/globalassets/hsl/tutkimukset/muut-tutkimukset/vyohykeuudistuksen_vaikutukset_liikkumiseen_yhteenveto_05_2020.pdf (Accessed October 27, 2023)

access between zones B and C.

Figure 4 shows the location of all inhabited 250 meters by 250 meters grid cells in Vantaa and how they are connected by bus and train routes. The cells in red are all within two kilometers of the border between travel zones B and C, indicated by the blue line. The cells with the lightest red color are within 600 meters of the border. This threshold of 600 meters is the closest I can zoom in to the border while still having a sufficient number of individuals with non-missing VKT observations. It is evident from the map that all inhabited grid cells have a bus line, in black, in their vicinity. Bus lines crossing the southern border of Vantaa have their destination in zone A in Helsinki, and most cells within two kilometers of the border are served by these routes. Some cells are also close to one of the two Helsinki-bound train lines, in yellow on the map. The section of the train line going from west to east and connecting the two Helsinki-bound lines is the section that was built in 2015.

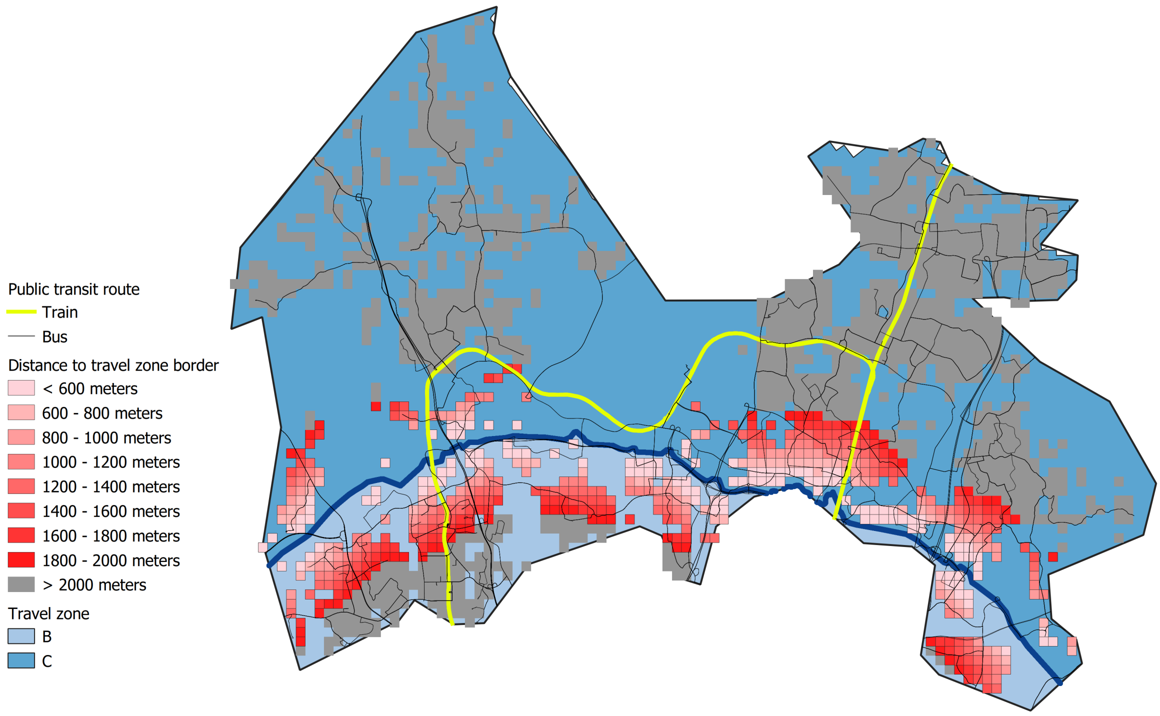

Figure 4: Inhabited grid cells and public transit routes in Vantaa

The border between travel zones B and C mainly follows a ring road, Ring III, around Helsinki. Ring III intersects all highways coming from the north towards Helsinki and enables fast travel across different neighborhoods in the Helsinki region. The road itself does not follow any administrative boundaries and is surrounded by various types of areas, ranging from suburbs to commercial zones and green areas, on both sides. Crossing the ring road by car is possible in multiple locations, as seen in Figure 4 where the bus lines intersect the travel zone border. In addition to these roads, Ring III and the travel zone border can also be crossed on foot or by bike using various pedestrian underpasses and overpasses.

4.3 Event Study Model

I first estimate the effect of the transit pass fare reduction on VKT with the following event study model:

$$VKT_{ist} = \alpha_s + \lambda_t + \sum_{\tau \neq -1} \beta_\tau D_{i\tau} + \varepsilon_{ist}, \tag{1}$$

where VKT_{ist} is vehicle kilometers traveled by individual i living in zone s during a 12-month period t before or after April 27, 2019, $D_{i\tau}$ is a treatment indicator for individuals living in zone B, and α_s and λ_t are zone fixed effects and time fixed effects respectively. Finally, β_τ is the effect of being eligible to buy the cheaper transit pass on VKT in year τ relative to treatment. Home travel zone s for individual i in period t is determined by the residential location of individual i on December 31 during period t. When estimating the model, I cluster the errors, ε_{ist}, at the level of the 250 meters by 250 meters grid cells.[14]

To interpret β_τ as the causal effect of the transit pass fare reduction I assume that VKT in the treatment and comparison groups, meaning zones B and C, would have followed parallel trends in the absence of the pricing reform. This assumption is plausible due to two factors. First, from the point of view of the individuals, the pricing reform was largely exogenous. The travel zone borders and new prices were not made public until a few months before the reform. Second, I restrict my sample of individuals only to those living very close to the B-C travel zone border, which makes it more likely that the individuals in the treatment and control groups are similar in terms of their car and public transit use.

I also estimate the effect of the transit pass fare reduction on car ownership using a model very similar to equation 1. I measure changes in car ownership in two ways. One is a dummy for whether an individual i owned a car at any point during a 12-month period t before or after April 27, 2019. This is the extensive margin of car ownership. The other is a dummy for whether an car owner i owned multiple cars instead of only one on December 31 during period t. This is a measure of the intensive margin of car ownership. As in equation 1, to interpret β_τ as the causal effect of the transit pass fare reduction, I assume that car ownership in the treatment and comparison groups would have followed parallel trends in the absence of the pricing reform.

14. The estimated standard errors are very similar when clustering at the individual or postal code level. The more granular individual-level clustering produces slightly smaller standard errors, whereas clustering at the larger postal code area-level somewhat increases standard errors. The results, however, remain qualitatively the same.

In addition to the uncertainty related to ticket types discussed in Section 4.1, assigning individuals into treatment and comparison groups is complicated by moving patterns. For this reason, I use two different ways of defining the treatment and comparison groups: 1) I follow the the same grid cells around the border over time regardless of which individuals live in the cells. In other words, individuals are assigned into the treatment group in period t if they lived in zone B on December 31 during period t even if they did not live there in any other year. 2) I only follow individuals who lived in zone B or zone C four months before the pricing reform on December 31, 2018. I include these individuals in the sample in all other periods t regardless of where they lived.[15] The first approach provides an overview of the trends in car use in zone B compared to zone C. However, it cannot discern changes in car use at the individual level from changes in car use stemming from compositional changes due to people moving. The second method, on the other hand, only focuses on individual-level responses.

4.4 Difference-in-Differences Model

To estimate the average effect of the fare reduction in the post-reform years, I use the following difference-in-differences model:

$$VKT_{ist} = \gamma + \varphi ZoneB_s + \psi Post_t + \delta ZoneB_s \times Post_t + \eta_{ist}, \tag{2}$$

where *ZoneB* is an indicator which takes on a value of 1 for individuals living in zone B and a value of 0 for individuals living in zone C, and *Post* is a dummy for 12-month periods after April 27, 2019. Assuming parallel trends in vehicle kilometers traveled between individuals living in zone B and zone C, δ gives the causal effect of the fare reduction. Substituting VKT_{ist} with the car ownership dummies enables me to estimate similar models for car ownership. In line with the event study model, I cluster the errors, η_{ist}, at the level of the 250 meters by 250 meters grid cells.

In addition to estimating the average effect of the fare decrease, I also analyze effect heterogeneity with respect to the individuals' characteristics, such as income, employment status, gender, the availability of public transit, and the number of cars owned.

4.5 Estimating Elasticities

The interpretation of the estimated treatment effects is complicated by the concurrent but smaller fare decreases in the comparison group. A similar problem of concurrent changes in the comparison group in a difference-in-differences setting is discussed in Harju et

15. Around 85 percent of these individuals lived in the same travel zone in Vantaa about 3.5 years before the reform on December 31, 2015. The same also holds about 3.5 years after the reform on December 31, 2022. However, many lived further away from the border in other years than they did in 2018.

al. (2022). Similarly to Harju et al. (2022), to obtain an unbiased estimate of the elasticity of car use with respect to the price of public transit in the treatment group, I have to make an assumption on the elasticity of car use in the comparison group.

The elasticity of VKT with respect to public transit fares in the treatment group, ϵ_T, is defined as

$$\epsilon_T := \frac{\dfrac{TE_T}{VKT_{T,0}}}{\dfrac{\Delta P_T}{P_0}} \tag{3}$$

Here the numerator is the percentage change in VKT caused by the price change in the treatment group, denoted by T, where TE_T is the treatment effect and $VKT_{T,0}$ is the level of VKT before the reform. The denominator gives the percentage change in the price of public transit in the treatment group, with ΔP_T being the change in the price and P_0 the price before the reform.

With no price change in the comparison group, the treatment effect in the treatment group could be directly estimated using the difference-in-differences approach in equation 2. When the parallel trends assumption holds, an estimate of TE_T would be given by $\hat{\delta}$ from estimating equation 2. However, even if the parallel trends assumption holds, a concurrent price change in the comparison group, denoted by C, introduces a bias into $\hat{\delta}$ such that

$$\begin{aligned} \hat{\delta} &= \Delta VKT_T - \Delta VKT_C \\ &= (TE_T + \Delta VKT_{trend}) - (TE_C + \Delta VKT_{trend}) \\ &= TE_T - TE_C \end{aligned} \tag{4}$$

If $\Delta VKT_{T,trend}$ and $\Delta VKT_{C,trend}$ are the changes in VKT in the treatment and comparison group, respectively, that would have taken place even in the absence of the pricing reform, the parallel trends assumption then translates into $\Delta VKT_{trend} := \Delta VKT_{T,trend} = \Delta VKT_{C,trend}$. Thus, the difference-in-differences coefficient only captures the difference between treatment effects in the treatment group and the comparison group.

Because the difference-in-differences estimate only captures the difference between treatment effects in the two groups, I estimate the elasticity by comparing the difference in treatment effects to the difference in price changes:

$$\hat{\epsilon}_T := \frac{\dfrac{\hat{\delta}}{VKT_{T,0}}}{\dfrac{\Delta P_T - \Delta P_C}{P_0}} = \frac{\dfrac{TE_T - TE_C}{VKT_{T,0}}}{\dfrac{\Delta P_T - \Delta P_C}{P_0}} \tag{5}$$

This estimator essentially captures how large the additional change in VKT was relative to the additional change in the price in the treatment group relative to the comparison group.

The value of $\hat{\epsilon}_T$ depends on the unobserved true elasticities in both the treatment and the comparison group. This can be seen by applying the elasticity definition in equation 3 to both groups to get $TE_T = \epsilon_T(\Delta P_T/P_0)\, VKT_{T,0}$ and $TE_C = \epsilon_C(\Delta P_C/P_0)\, VKT_{C,0}$. Plugging these expressions into equation 5 and rearranging, $\hat{\epsilon}_T$ can be written as a function of the true elasticities:

$$\hat{\epsilon}_T = \epsilon_T \frac{\Delta P_T}{\Delta P_T - \Delta P_C} - \epsilon_C \frac{\Delta P_C}{\Delta P_T - \Delta P_C} \frac{VKT_{C,0}}{VKT_{T,0}} \tag{6}$$

The weight on the elasticity in the comparison group, ϵ_C, depends both on the relative size of the price change in the comparison group and the pre-reform level of VKT in the comparison group relative to the treatment group.

By rearranging equation 6 it can be seen that there has to be a unique relationship between the elasticities in the treatment and comparison group for $\hat{\epsilon}_T$ to be equal to the true elasticity ϵ_T. In particular,

$$\hat{\epsilon}_T = \epsilon_T \iff \epsilon_C = \epsilon_T \frac{VKT_{T,0}}{VKT_{C,0}} \tag{7}$$

If the pre-reform levels of VKT in the treatment and comparison group are similar, equation 7 essentially boils down to the to the intuitive condition that the elasticities have to be same in both groups to obtain an unbiased elasticity estimate in the treatment group when using the estimator in equation 5.

5 Descriptive Statistics

One way to evaluate the plausibility of the parallel trends assumption is to analyze the degree of similarity between drivers living on either side of the B-C travel zone border. Table 2 provides descriptive statistics on drivers living within 600 meters of the border on December 31, 2018. Overall, drivers appear relatively similar to each other in zones B and C. They are comparable in terms of gender, age and household size. In addition, based on the somewhat limited information on job location provided by Statistics Finland, a nearly identical share of employed drivers work in zone A. Having to commute to zone A should be a good predictor of buying a transit pass covering zones AB or ABC when living in zones B or C.

Despite the similarities, the drivers in zone B and C do differ in two aspects. First, disposable income is about nine percent higher among zone B drivers compared to zone C drivers. This is most likely explained by the fact that almost 75 percent of drivers in zone B are employed but only 66 percent are employed in zone C. Second, based on a travel time matrix created by researchers at the University of Helsinki (Tenkanen and Toivonen 2020)[16], travel time by public transit to downtown Helsinki is almost eight minutes, or

16. The data can be downloaded at https://github.com/AccessibilityRG/HelsinkiRegionTravelTimeMatrix2018 (Accessed June 30, 2024)

18 percent, longer in zone B relative to zone C. Considering that zone B is closer to downtown Helsinki, the difference is perhaps a bit surprising. Upon closer inspection, the difference is explained by a slightly larger share of people living close to a train line in zone C. If higher income and slower public transit heavily affect car use, it is possible that car use could have evolved somewhat differently among drivers in zone B compared to drivers in zone C within 600 meters of the border.

Table 2: Descriptive statistics for drivers living within 600 meters of the border in 2018

	Zone B	Zone C	Difference	Standard error
Women (%)	32.4	35.1	2.6	(1.9)
Age	44.9	46.9	2.0**	(0.7)
Household size	2.5	2.4	-0.0	(0.1)
Employed (%)	74.7	65.6	-9.1***	(1.9)
Unemployed (%)	5.5	6.7	1.1	(1.0)
Working in zone A (%)	23.1	21.5	-1.6	(2.1)
Disposable income	30,751.0	28,127.9	-2623.1***	(606.6)
Disposable household income	53,231.3	48,400.4	-4831.0***	(1255.9)
Public transit to Helsinki (min)	51.1	43.3	-7.7***	(0.3)
Car to Helsinki (min)	42.5	43.8	1.3***	(0.1)
Number of drivers	866	2213		

The table includes all drivers with non-missing VKT for cars 10 years or older

T-test for the difference between zones B and C. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Zooming out further from the border to a distance of 2000 meters, the differences in income, employment and public transit journey times between zone B and zone C drivers are reduced to around five percent, as seen in Table 3. Tables 5 and 6 in the Appendix demonstrate how the differences diminish already at distances of 1000 and 1400 meters. Although increasing the distance to the border is conducive to leveling out these differences between drivers on the two sides of the border, the main drawback is a potentially increased disparity in the probability of purchasing a ticket covering travel zone A. At a distance of 2000 meters, some drivers in zone B are four kilometers closer to zone A compared to drivers in zone C. As the distance to downtown Helsinki from the border of travel zones B and C is around 15 kilometers, a four kilometer distance difference might already affect the probability of regularly traveling to downtown Helsinki.

Table 3: Descriptive statistics for drivers living within 2000 meters of the border in 2018

	Zone B	Zone C	Difference	Standard error
Women (%)	34.0	34.6	0.6	(0.7)
Age	46.9	46.3	-0.6*	(0.3)
Household size	2.5	2.4	-0.1***	(0.0)
Employed (%)	69.4	67.9	-1.5*	(0.7)
Unemployed (%)	5.4	6.2	0.9*	(0.4)
Working in zone A (%)	24.6	22.6	-2.0*	(0.9)
Disposable income	29,245.2	27,731.2	-1514.0***	(225.0)
Disposable household income	51,281.0	47,459.9	-3821.0***	(813.1)
Public transit to Helsinki (min)	47.4	44.9	-2.5***	(0.1)
Car to Helsinki (min)	41.7	45.5	3.8***	(0.0)
Number of drivers	8953	7914		

The table includes all drivers with non-missing VKT for cars 10 years or older

T-test for the difference between zones B and C. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Because of the observed differences in income, employment and public transit journey times very close to the border, I estimate the difference-in-differences models for numerous bandwidths around the border ranging from 600 to 2000 meters. I also estimate an alternative specification in which the distance to the border is fixed at 600 meters in the comparison group, while varying the distance from 600 to 2000 meters only in the treatment group. The latter strategy might be beneficial due to drivers in zone C being fairly similar to each other regardless of the distance to the border, as seen in Tables 2 and 3. Not extending the bandwidth around the border further away from zone A might alleviate the concern that drivers in zone B and C differ in terms of willingness to purchase a ticket to zone A.

6 Results

6.1 Effects on Vehicle Kilometers Traveled

Figure 5 presents results for VKT from estimating the event study model in equation 1. The figure includes results for three different distances to the border: 600, 1000 and 1400 meters. The upper panel compares all individuals who lived around the border each year within the specified distance. The lower panel only includes individuals who lived around the border in 2018, four months before the reform. For these individuals, the distance is measured in 2018. In any other year, the individuals may have lived anywhere in Finland.

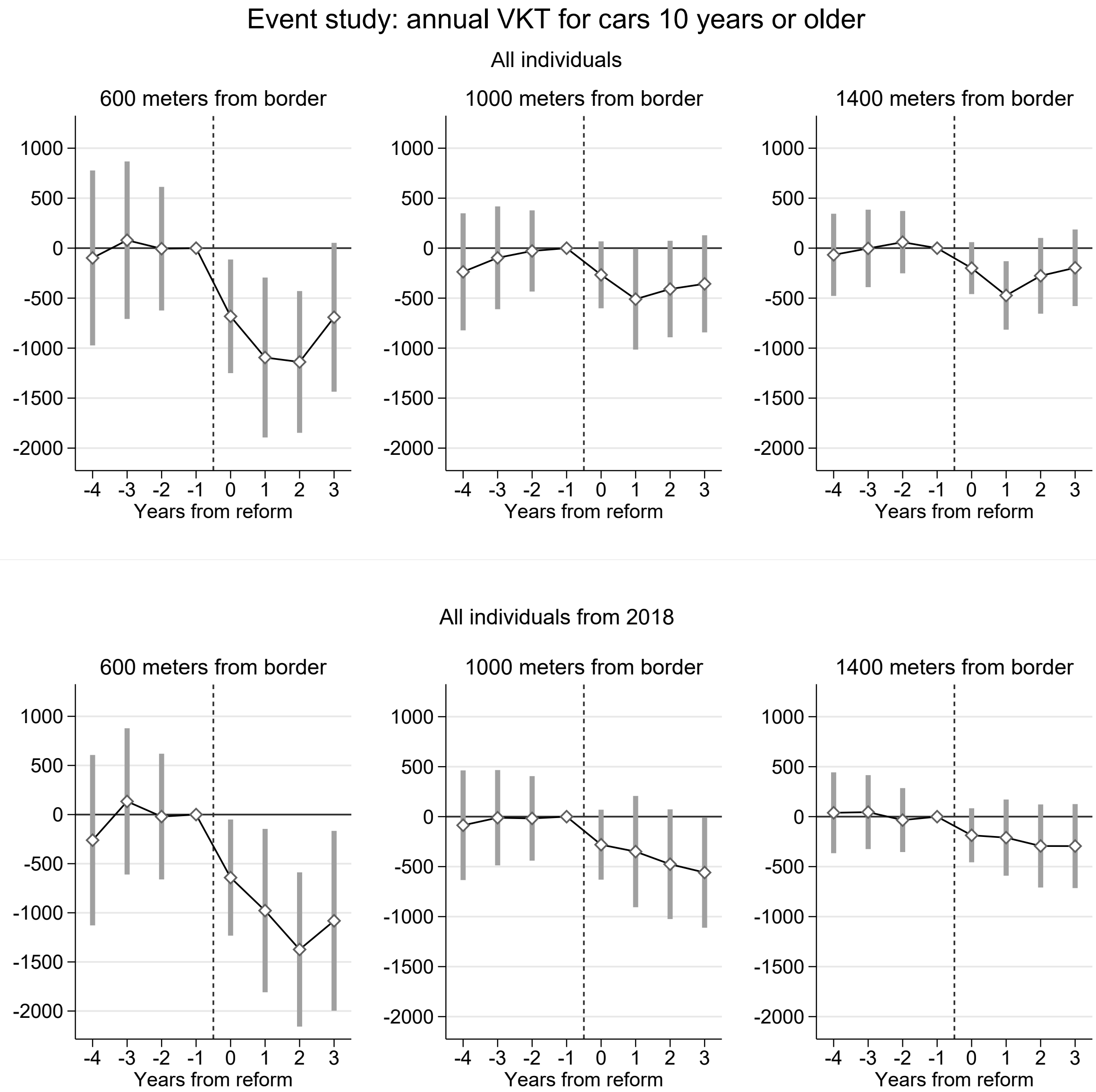

Figure 5: VKT event study for cars 10 years or older

During the four years before the pricing reform, the trends in VKT were statistically indistinguishable from each other between the treatment and comparison groups in all six subplots of Figure 5. These nearly identical pre-trends bolster the credibility of the parallel trends assumption. However, immediately after the year of the reform, average VKT in the treatment group decreased relative to the comparison group. The size of the effect decreases in absolute value with distance to the border. When zooming in within 600 meters of the border, driving decreased by between 700 and 1400 kilometers more per year in the treatment group. However, zooming out just 400 meters further to a distance of 1000 meters, the effect size decreases to between 300 and 500 kilometers per year. Moving from 1000 to 1400 meters makes the point estimates slightly smaller but not as dramatically as moving from 600 to 1000 meters. While all of the individual point estimates are not statistically significant, together they differ statistically significantly from the pre-reform level.

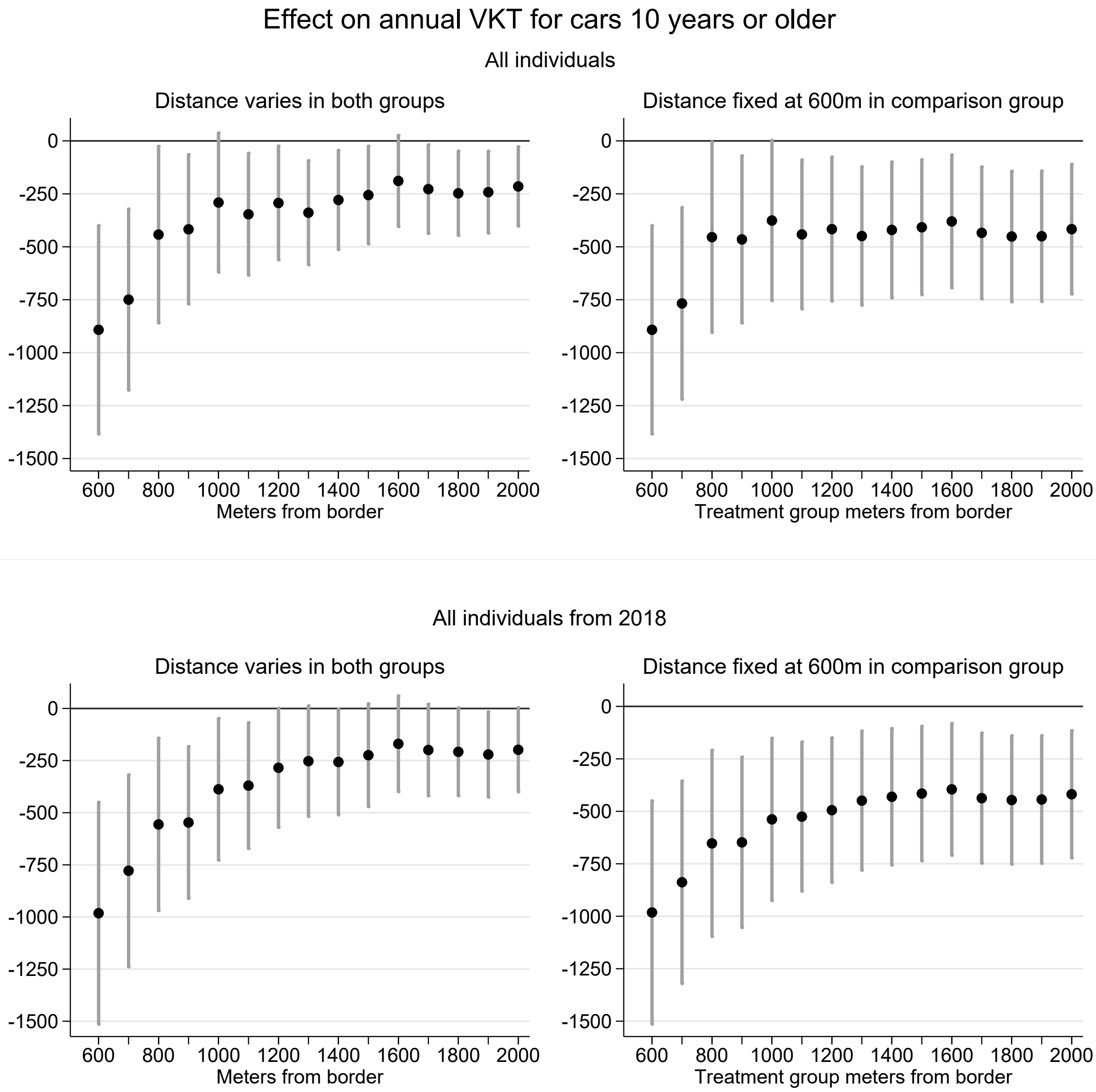

Figure 6: VKT difference-in-differences results for cars 10 years or older

The results are very similar across both samples, one including all individuals around the border each year and the other consisting of individuals around the border in 2018. This means that people moving from other regions to the border region after the reform does not explain the observed decrease in VKT. In contrast, the effect mostly comes from individuals living around the border right before the reform reducing their driving after the reform. Figure 11 in the Appendix presents the estimated changes in VKT in levels and confirms that it was in fact a reduction in VKT in the treatment group that caused the patterns visible in the event study plots. VKT fell sharply in the treatment group immediately after the reform, while at the same time there was a smoother downward trend in the comparison group.

Figure 6 presents estimates of the effect on VKT for the entire post-reform period based on the difference-in-differences model in equation 2. The figure shows how the effect size changes when increasing the bandwidth around the border in 100 meter increments from 600 to 2000 meters. In the left panel of the figure, the distance to border is varied in both the treatment and comparison group. The right panel provides results from the alternative specification in which the distance is fixed at 600 meters in the comparison group.

Like the event study plots above, the difference-in-differences results[17] suggest that the effect of the price reform on VKT decreases with distance to the border. VKT fell on average by 900–1000 kilometers per year more in the treatment group relative to the comparison group within 600 meters of the border. The effect size then diminishes and stabilizes to about 200–250 kilometers per year after going further than 1000 meters from the border. The pattern is very similar regardless of whether the sample consists of all individuals around the border each year or only individuals around the border in 2018. However, some of the point estimates in the latter sample are not statistically significant at distances larger than 1200 meters. When the distance to border is fixed at 600 meters in the comparison group, the size of the reduction in VKT does not diminish as steeply and instead stays around 400–450 kilometers per year for distances greater than 1000 meters in the treatment group. Furthermore, the point estimates are all statistically significant at least at the 5 percent level.

Table 4 presents estimates of the elasticity of VKT with respect to the price of public transit for the sample of individuals around the border in 2018. The elasticities are based on the difference-in-differences results and were calculated using equation 5. In the elasticity calculations, the difference in price changes between zones B and C over all the four post-reform periods is about €400, or 34 percent of the pre-reform price level. When the distance to the border varies in both groups, the reductions in VKT translate to elasticities ranging from about 0.27 within 600 meters of the border to 0.06 for the longest distances. Fixing the distance in the comparison group at 600 meters increases the elasticity estimates at longer distances in the treatment group such that they stabilize at around 0.12. These responses are relatively large in size, given that the highest elasticity estimates fall into a similar range with many estimates of the short-

17. Detailed difference-in-differences regression results are found in Tables 7–10 in the Appendix.

run price elasticity of fuel demand (e.g. Bento et al. 2009; Gillingham 2014; Knittel and Sandler 2018; Gillingham and Munk-Nielsen 2019). Furthermore, because the estimates represent intention-to-treat effects, the average treatment effect on the treated might be even higher if some drivers in the sample were not affected by the fare reduction.

Table 4: VKT elasticity estimates for the sample of individuals around the border in 2018

Distance to the border	Distance varies in both groups	Distance fixed at 600 meters in the comparison group
600 meters	0.269***	0.269***
700 meters	0.212***	0.228***
800 meters	0.156**	0.183**
900 meters	0.153**	0.181**
1000 meters	0.109*	0.151**
1100 meters	0.104*	0.148**
1200 meters	0.080	0.139**
1300 meters	0.072	0.128**
1400 meters	0.074*	0.124**
1500 meters	0.065	0.120*
1600 meters	0.049	0.115*
1700 meters	0.058	0.127**
1800 meters	0.060	0.130**
1900 meters	0.064*	0.130**
2000 meters	0.058	0.122**

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The steep gradient in the effect size with respect to distance to the border could be explained in at least two ways. On the one hand, individuals living very close to the border on either side might be the most comparable in terms of the probability to purchase a transit pass covering zone A. This would mean that the large elasticity estimates obtained by using individuals as close to the border as possible are the most reliable. On the other hand, individuals near the border in zone B might for some reason have had an abnormally large response to the price change. Based on the descriptive statistics in Section 5, drivers in zone B differ from drivers in zone C close to the border in terms of income, employment and public transit journey times to downtown Helsinki. Furthermore, the difference-in-differences results in Tables 7–10 in the Appendix indicate that pre-reform levels of VKT were around 12 percent higher in zone B relative to zone C within 600 meters of the border, while the difference was only five percent or less at longer distances.

Because of these problems related to the choice of bandwidth around the border, one of the most reliable comparisons might be that in which the distance in the treatment group is extended closer to 2000 meters but the distance in the comparison group is fixed at 600 meters. Having a wider bandwidth in the treatment group mitigates the problems related to the drivers in the treatment group differing from those in the comparison group with respect to observable characteristics. At the same, limiting the distance on the other side of the border keeps the drivers in the comparison group geographically closer to those in the treatment group, which could make it more likely that the two groups are more similar in their ticket preferences.

Regardless of these complications, distance to the border is the main source of heterogeneity in the effects of the pricing reform on VKT. I find no clear differences in VKT responses with respect to any background characteristics for the sample of individuals around the border in 2018. These characteristics include gender, age, income, employment status, job location inside zone A, and public transit availability, measured as minutes to downtown Helsinki by public transit both in absolute terms and relative to the travel time by car. However, detecting heterogeneity is complicated by the fact that standard errors grow very large when performing any subsample analysis on sample sizes that are already small.

6.2 Effects on Car Ownership

In contrast to the negative effects on VKT, I find no clear effects on car ownership. This holds true for both the extensive margin, meaning whether an individual owns a car at all, and the intensive margin, meaning how many cars an individual owns. Figure 7 presents event study results for the extensive margin of car ownership. The dependent variable in the event study regression is a dummy for whether an individual owned a car at any point during the 12-month period. As a result, the event study plots demonstrate how the share of car owners changed in the treatment group relative to the comparison group.

When it comes to car ownership, a simple comparison between the treatment and comparison groups appears to produce misleading results. Results from this comparison are presented in the subplots in the upper panel of Figure 7. Based on the results, car ownership actually increased in the treatment areas relative to the comparison areas both before and after the reform. Inspecting the changes in car ownership rates in levels in Figure 12 reveals that the relative increase in the treatment group stems from a decrease in car ownership in the comparison group, while the car ownership rate remained constant in the treatment group. This disparity in the trends suggests that the effects of the reform cannot be reliably detected without separating individual-level responses from moving patterns.

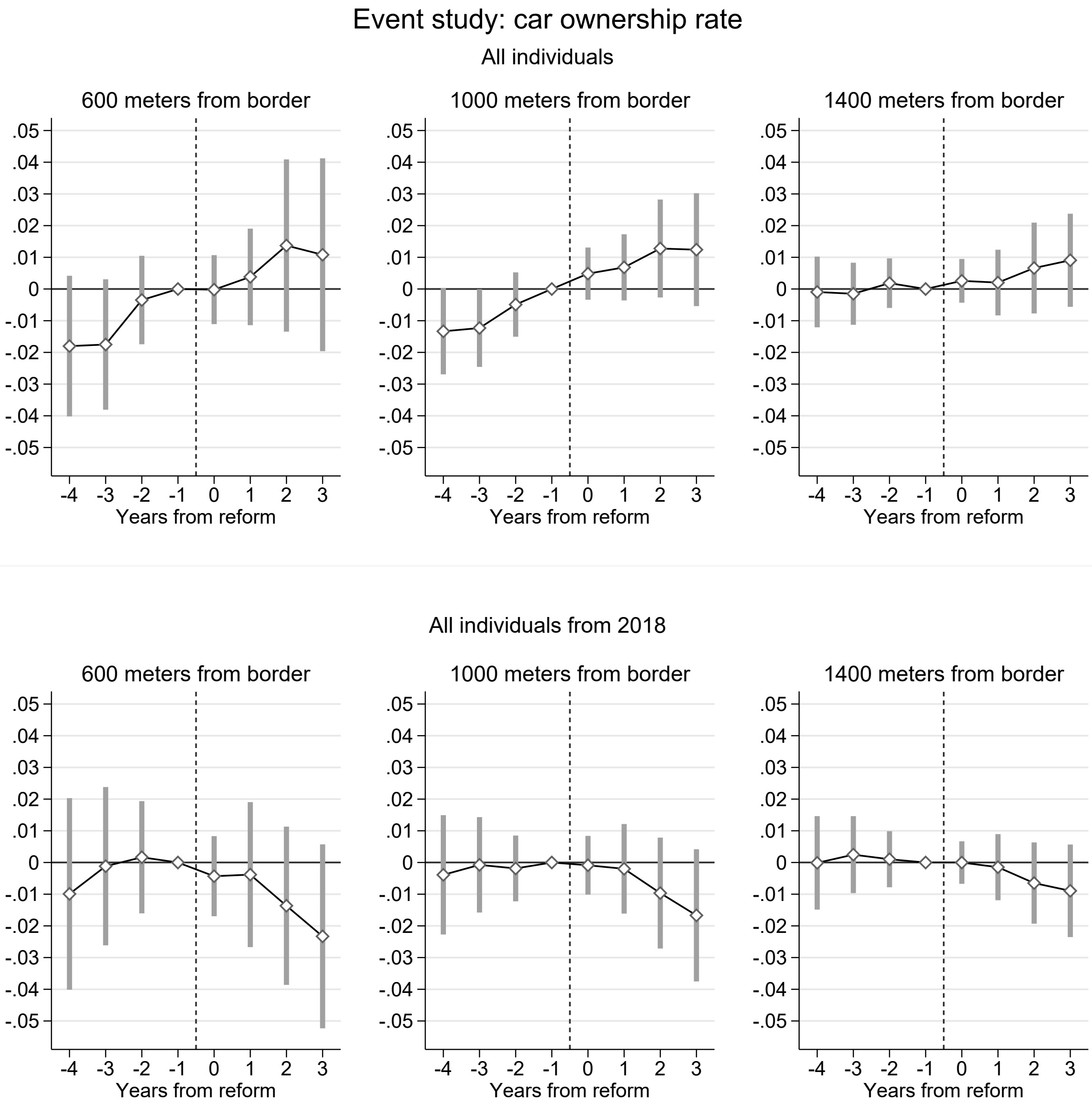

Figure 7: Car ownership event study

Because of the diverging overall trends in car ownership, the preferred sample only consists of individuals who lived around the border in 2018. Event study results with this sample are presented in the lower panel of Figure 7. In these plots, there are no statistically significant differences in the car ownership pre-trends at any distance to the border. The results suggest that car ownership in the treatment group relative to the comparison group might have decreased ever so slightly after the reform. However, the point estimates are not statistically significant. The plots in levels in Figure 12 also point to an increase in car ownership in the comparison group rather than a decrease in the treatment group as an explanation for the results. Overall, the results do not clearly point to car ownership decreasing among individuals who were eligible for the cheaper transit passes.

Results from the difference-in-differences regressions in Figure 8 further support the conclusion that the pricing reform most likely had no effect on car ownership. The point estimates are all very close to zero and statistically insignificant apart from a few exceptions in the specifications using the sample of all individuals around the border. Due to the fact that no effects are detected among individuals who lived around the border in 2018, the small effects found in the sample including all individuals are likely explained by moving patterns not related to the reform.

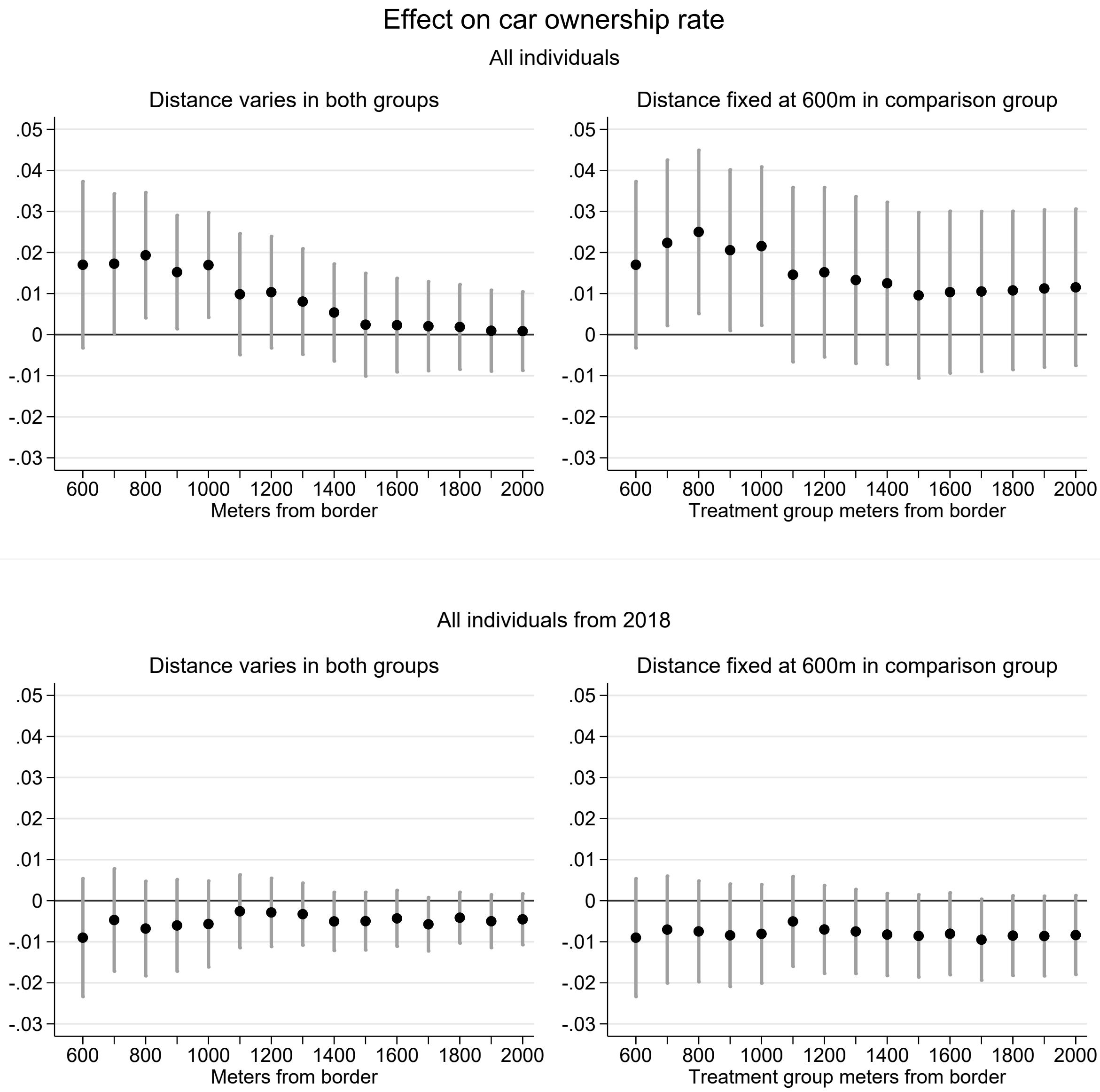

Figure 8: Car ownership difference-in-differences results

In addition to the null effects at the extensive margin of car ownership, I find no effects at the intensive margin either. Figure 13 in the Appendix presents results from event study regressions in which the dependent variable is a dummy for car owners with multiple cars at the end of each year. Regardless of whether the sample includes all individuals each year or just the individuals from 2018, the trends in the share of car owners with multiple cars exhibit no differences between the treatment and the comparison group.

A plausible explanation for why car ownership did not react to the price change is that public transit in the Helsinki region is not a perfect substitute for all trips made by car. Drivers seemed to respond by switching from a car to public transit only on a smaller subset of their trips rather than giving up driving altogether. This is perfectly reasonable given that people are likely to travel outside of the Helsinki region as well, and many of these trips might require a car.

7 Robustness and Limitations

7.1 Parallel Trends Assumption

The validity of my results depends most crucially on the validity of the parallel trends assumption. Observing no differences in the pre-trends does make the assumption more credible, but it still faces a few main threats. First, there might have been self-selection into treatment. One way this selection could have occurred is via residential sorting: people who were already public transit users and had lower car use could have moved to zone B right before the pricing reform to take advantage of the large price decrease. Although I control for moving patterns by including a specification in which I only focus on individuals living around the border on December 31, 2018, these individuals might still be a self-selected group. However, while the reform had been planned since the year 2012, the new prices and travel zone boundaries were not made public until a few months prior to the reform. Specifically, the prices and zone boundaries were first publicized on October 30 and December 11 in 2018, respectively, but the final details of the reform, including the date of its implementation, were announced only six weeks before the reform on March 13, 2019. Thus, pre-reform residential sorting is probably not a major concern, as individuals had at most a month or two to react to the upcoming changes. Moreover, around 85 percent of the individuals living in zone B in 2018 were already living there in 2015.

Second, another problem would arise if the travel zone boundaries were drawn based on regional factors correlated with the residents' driving behavior. Specifically, if the people who ended up in travel zone B were already more likely to reduce their car use than those who ended up in zone C, my estimates would overstate the actual size of the effects. Even though the travel zone border follows a main road, Ring III, the road itself does not follow any administrative borders. Nevertheless, the road might still divide the municipality of Vantaa according to some unobserved factors correlated with car use. I address this problem by comparing individuals close to the border at various distances, so that the individuals should in principle be very similar in terms of their travel preferences and infrastructure. The descriptive statistics in Section 5 confirm that individuals around the border are very similar with respect to observable characteristics within 2000 meters of the border. While the estimated effects on VKT change with distance to the border, as is evident in Figure 6, they remain statistically significantly different from zero for all distances up until 2000 meters.

Third, the COVID-19 pandemic could have affected the treatment and comparison groups differently, which would complicate the analysis in two ways. On the one hand, different effects in these two groups in the absence of the reform would constitute a violation of the parallel trends assumption. For instance, there could have been an asymmetric reduction in commuting to zone A from zones B and C starting 2020 in response to COVID-19 even without the pricing reform if the likelihood of commuting depends heavily on the distance to the workplace. However, because I zoom into areas very close to the travel zone border, the individuals living on either side of the border should have a fairly similar commuting distance. On the other hand, one could argue that the negative effects on VKT could just reflect a larger response in zone B to COVID-19 instead of to the pricing reform. Luckily, the estimated effect for year 0 in the event study plots in Figure 5 represents the year just before COVID-19 hit Finland. Because the estimated size of the effect in year 0 is very similar to those in years 1–3, it seems likely that the effects are not driven by responses to the pandemic.

7.2 Price Change in the Comparison Group

As explained in Section 4.5, the elasticity estimates obtained from the difference-in-differences model potentially suffer from a bias stemming from the concurrent price change in the comparison group. Equation 7 demonstrates how the elasticity estimates are unbiased only if the parallel trends assumption holds and the elasticity in the comparison group is the same as in the treatment group scaled by the pre-reform levels of VKT. Based on the difference-in-differences results in Tables 7–10 in the Appendix, the pre-reform levels of VKT are no more than 12 percent higher in the treatment group. This means that the elasticity in the comparison group would have to be essentially at the same level as in the treatment group.

If the assumption of nearly equal elasticities in the treatment and comparison group does not hold, equation 6 can be used to assess the degree to which the estimated elasticity diverges from the true elasticity in the treatment group. This can be done by rearranging the equation and writing the true elasticity in the treatment group, ϵ_T, as a function of of the estimated elasticity in the treatment group, $\hat{\epsilon}_T$, and the true elasticity in the comparison group, ϵ_C:

$$\epsilon_T = \hat{\epsilon}_T \frac{\Delta P_T - \Delta P_C}{\Delta P_T} + \epsilon_C \frac{\Delta P_C}{\Delta P_T} \frac{VKT_{C,0}}{VKT_{T,0}} \tag{8}$$

Plugging in the estimated elasticity in the treatment group along with the price changes and pre-reform levels of VKT, equation 8 gives a value for the true elasticity in the treatment group that is consistent with the estimated elasticity in the treatment group for any given elasticity in the comparison group.

In the sample with individuals living around the border in 2018, the elasticity estimates range from 0.269 within 600 meters of the border to 0.058 within 2000 meters of the border. If the true, unobserved elasticity in the comparison group is somewhere between 0 and 0.5, the true elasticity in the treatment group would fall between 0.204 and 0.312 at the 600-meter bandwidth and between 0.044 and 0.162 at the 2000-meter bandwidth. Thus, the results remain qualitatively the same even with very low and high elasticities in the comparison group.

7.3 External Validity

The analyses conducted in this paper are limited in their external validity for two main reasons. The first reason is that I only analyze changes in VKT for cars 10 years or older. While these cars represent approximately half of all cars, the results for the remaining half might be different. For instance, people with newer cars might not be as responsive to the price of public transit. Figure 15 in the Appendix presents VKT event study results for cars under 10 years old. The annual VKT estimates for these newer cars are based on vehicle inspections with multiple years between them. Not only does this introduce clear measurement error, it also means that many cars do not appear in the data during the last years of the sample because they have not had their first inspection. Discerning between real driving responses and the noise caused by the data generation process in Figure 15 is very challenging. Nevertheless, the results do lend support to the idea that VKT decreased relatively more in the treatment group despite the noisy pre-trends.

However, if the newer cars are included in the same sample together with the older cars, the results are similar to the main results with the sample of only older cars. Event study results using all cars regardless of age are presented in Figure 16 in the Appendix. Here, the pre-trends, while not perfect, are statistically indistinguishable from zero. Furthermore, VKT in the treatment group relative to the comparison group starts declining right after the pricing reform. Even though the effect does not set in immediately, it grows in time and reaches the same magnitude as in the main analysis in Figure 5 two or three years after the reform. This is to be expected due to the longer time it takes to observe changes in VKT among newer cars because of their less frequent odometer readings. Overall, the results suggest that the decrease in VKT is not limited to older cars.

The second reason for potentially low external validity is that the effects I estimate are very local in nature because I zoom very close to border between travel zones B and C. Drivers in other parts of the travel zones might not have responded in the same way. Addressing these concerns would require a more detailed analysis of the effects of the pricing reform in other municipalities and around other travel zone borders. Travel zones B and C in Vantaa do, however, provide the clearest setting to analyze the effects of the reform.

8 Climate Impacts of the Reform

The estimated reductions in VKT reported in Section 6.1 can be translated into reductions in CO_2 tonnes emitted by using information on CO_2 emissions intensity at the car level provided in the data. Figure 9 shows that CO_2 emissions fell by between 0.05 and 0.2 tonnes more in zone B relative to zone C. Because car ownership did not respond to the price decrease, these reductions in CO_2 emissions resulting from decreased VKT represent the full effect on emissions around the B-C travel zone border in Vantaa.

Because HSL is a public entity that is financed with taxpayer money, comparing the achieved emissions reductions with ticket revenue losses is a reasonable way to calculate the cost effectiveness of the reform from a climate standpoint. However, assessing cost effectiveness is difficult without data on ticket purchases by the people in my sample. A proper calculation would require information both on how existing ticket buyers changed their ticket purchases and how many non-ticket buyers started spending money on tickets and how much. Nonetheless, a simple back-of-the-envelope calculation can be made to obtain a plausible range for the cost of emissions reductions.

Over the entire four-year post-reform period in my analysis, the price of the annual transit pass permitting travel to travel zone A fell by €400 more in zone B compared to zone C. This is the revenue loss related to an individual who already had a travel pass before the reform. According to a travel survey conducted by HSL in 2018, 35 percent of people in the capital region had a transit pass, and 39 percent of people in Vantaa used public transit at least a few times a week.[18] If 40 percent of the drivers in my sample already had a transit pass and the pricing reform created no new public transit users, the cost of the reform would be €160 per driver in zone B relative to zone C. With an estimated reduction in CO_2 emissions of 0.05–0.2 tonnes, this would translate into a cost of €800–€3200 per tonne of CO_2. However, if some people who never purchased a transit

18. Source: https://hslfi.azureedge.net/globalassets/hsl/tutkimukset/liikkumistutkimus/liikkumistutkimukset-helsingin-seudulla-2018-paaraportti.pdf (Accessed June 30, 2024)

pass started regularly buying one after the reform, the cost of emissions reductions would be lower. If the share of drivers with an annual transit pass increased from 40 percent to, say, 50 percent, 10 percent of drivers would now pay HSL €646 more per person, the average price of the annual AB transit pass after the reform. This would result in a cost of around €500–€1900 per tonne of CO_2.

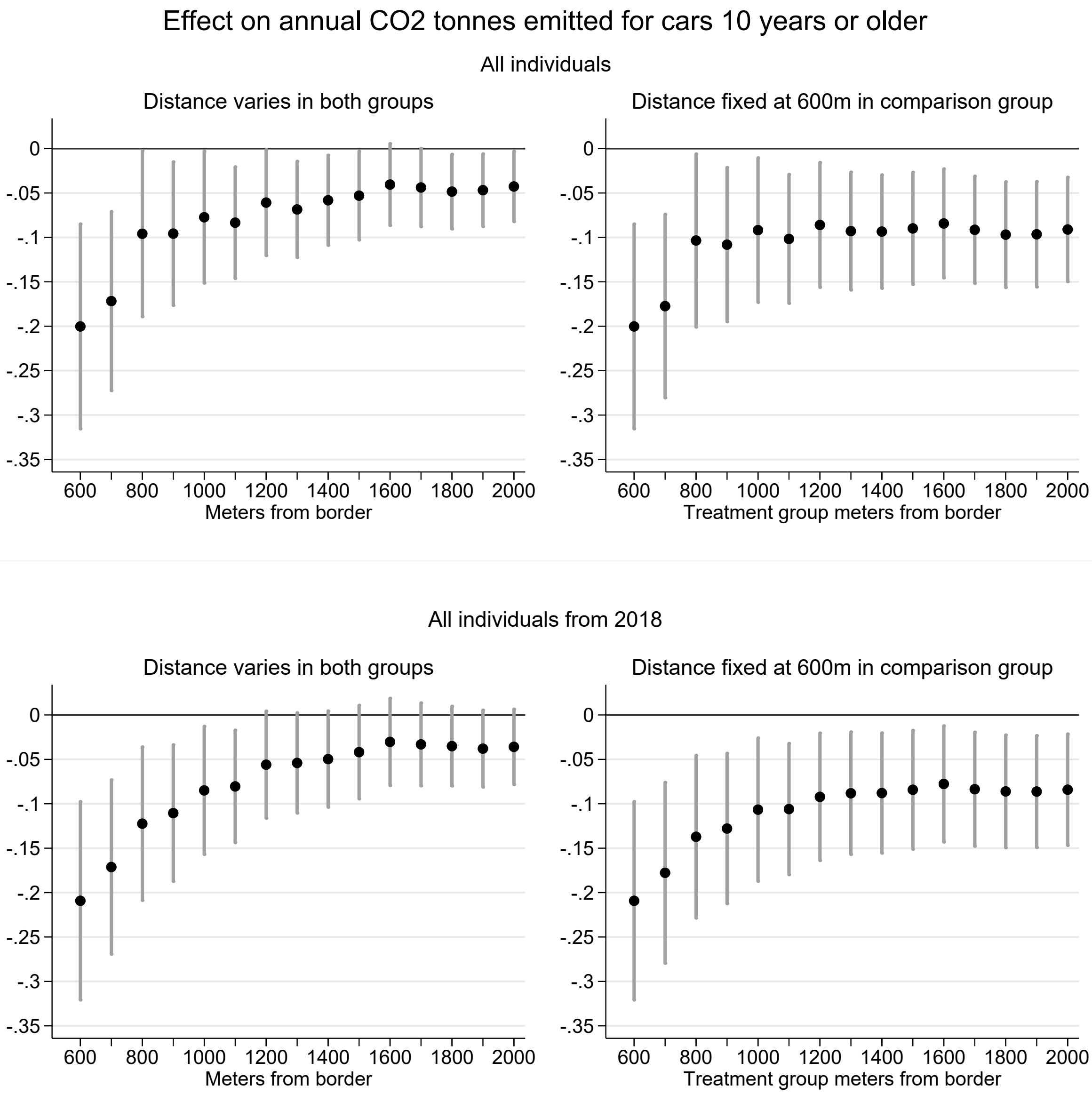

Figure 9: CO_2 difference-in-differences results for cars 10 years or older

One way to evaluate the size of these cost estimates is to compare them to the damages caused by increased CO_2 emissions. A cost closer to €1000–€3000 per tonne of CO_2 is undoubtedly very high compared to most existing estimates of the social cost of carbon. Pindyck (2019), for instance, report values ranging from \$80 to \$300. However, higher estimates do also exist, such as the value of \$1056 provided by Bilal and Känzig (2024).

Another way to assess how cost effective public transit pricing is in reducing emissions is to compare it to other measures that create emissions reductions. Even though not all cost estimates are directly comparable due to differences in how they were obtained, the review by Gillingham and Stock (2018) suggests that a cost in the thousands of euros per tonne of CO_2 makes public transit pricing a considerably more expensive climate policy than most other alternatives.

9 Conclusion

In this paper, I analyze how effectively car use and ultimately CO_2 emissions from transportation can be reduced by lowering the price of public transit. I use detailed individual-level data on vehicle kilometers traveled, car ownership and residential locations, together with a natural experiment arising from a travel zone reform, to estimate the effects of a 45 percent transit pass fare reduction in the Helsinki region in Finland. I estimate the cross-price elasticity of vehicle kilometers traveled to range between 0.06 and 0.27. However, I find no clear response in car ownership either at the extensive or intensive margin.

Assessing the cost of emissions reductions is challenging due to a lack of data on public transit ticket purchases. Nevertheless, back-of-the-envelope estimates of the cost land close to €1000–€3000 per tonne of CO_2. This makes public transit pricing a relatively expensive climate policy tool when comparing the cost to estimates of the social cost of carbon or the cost of other measures. However, if other climate policies, such as fuel taxation, are not stringent enough to meet the ambitious climate targets set by countries around the world, the results in this paper suggest that public transit pricing might be a viable tool to cut emissions especially in urban areas.

References

Anderson, S. T., and J. M. Sallee. 2016. "Designing Policies to Make Cars Greener." *Annual Review of Resource Economics* 8 (Volume 8, 2016): 157–180. https://doi.org/10.1146/annurev-resource-100815-095220.

Andor, M. A., F. Dehos, K. Gillingham, S. Hansteen, and L. Tomberg. 2023. *Public transport pricing: An evaluation of the 9-Euro Ticket and an alternative policy proposal.* Ruhr Economic Papers 1045. RWI - Leibniz-Institut für Wirtschaftsforschung, Ruhr-University Bochum, TU Dortmund University, University of Duisburg-Essen. https://ideas.repec.org/p/zbw/rwirep/279544.html.

Bento, A. M., L. H. Goulder, M. R. Jacobsen, and R. H. von Haefen. 2009. "Distributional and Efficiency Impacts of Increased US Gasoline Taxes." *American Economic Review* 99 (3): 667–699. https://doi.org/10.1257/aer.99.3.667.

Bilal, A., and D. R. Känzig. 2024. *The Macroeconomic Impact of Climate Change: Global vs. Local Temperature.* Working Paper, Working Paper Series 32450. National Bureau of Economic Research. https://doi.org/10.3386/w32450.

Carattini, S., M. Carvalho, and S. Fankhauser. 2018. "Overcoming public resistance to carbon taxes." *WIREs Climate Change* 9 (5): e531. https://doi.org/10.1002/wcc.531.

Carattini, S., S. Kallbekken, and A. Orlov. 2019. "How to win public support for a global carbon tax." *Nature* 565:289–291. https://doi.org/10.1038/d41586-019-00124-x.

Cats, O., Y. O. Susilo, and T. Reimal. 2017. "The prospects of fare-free public transport: evidence from Tallinn." *Transportation* 44:1083–1104. https://doi.org/10.1007/s11116-016-9695-5.

De Borger, B., I. Mulalic, and J. Rouwendal. 2016a. "Measuring the rebound effect with micro data: A first difference approach." *Journal of Environmental Economics and Management* 79:1–17. https://doi.org/10.1016/j.jeem.2016.04.002.

———. 2016b. "Substitution between cars within the household." *Transportation Research Part A: Policy and Practice* 85:135–156. https://doi.org/10.1016/j.tra.2016.01.007.

Dornoff, J., V. V. Morales, and U. Tietge. 2019. *On the Way to 'Real-World' CO2 Values? The European Passenger Car Market After 5 Years of WLTP.* White paper. ICCT.

Duranton, G., and M. A. Turner. 2011. "The Fundamental Law of Road Congestion: Evidence from US Cities." *American Economic Review* 101 (6): 2616–52. https://doi.org/10.1257/aer.101.6.2616.

Ewing, R., and R. Cervero. 2010. "Travel and the Built Environment." *Journal of the American Planning Association* 76 (3): 265–294. https://doi.org/10.1080/01944361003766766.

Gillingham, K. 2014. "Identifying the elasticity of driving: Evidence from a gasoline price shock in California." SI: Tribute to John Quigley, *Regional Science and Urban Economics* 47:13–24. https://doi.org/10.1016/j.regsciurbeco.2013.08.004.

Gillingham, K., A. Jenn, and I. M. Azevedo. 2015. "Heterogeneity in the response to gasoline prices: Evidence from Pennsylvania and implications for the rebound effect." Frontiers in the Economics of Energy Efficiency, *Energy Economics* 52:S41–S52. https://doi.org/10.1016/j.eneco.2015.08.011.

Gillingham, K., and A. Munk-Nielsen. 2019. "A tale of two tails: Commuting and the fuel price response in driving." *Journal of Urban Economics* 109:27–40. https://doi.org/10.1016/j.jue.2018.09.007.

Gillingham, K., and J. H. Stock. 2018. "The Cost of Reducing Greenhouse Gas Emissions." *Journal of Economic Perspectives* 32 (4): 53–72. https://doi.org/10.1257/jep.32.4.53.

Goulder, L. H., and I. W. H. Parry. 2008. "Instrument Choice in Environmental Policy." *Review of Environmental Economics and Policy* 2 (2): 152–174. https://doi.org/10.1093/reep/ren005.

Harju, J., T. Kosonen, M. Laukkanen, and K. Palanne. 2022. "The heterogeneous incidence of fuel carbon taxes: Evidence from station-level data." *Journal of Environmental Economics and Management* 112:102607. https://doi.org/10.1016/j.jeem.2021.102607.

Knittel, C. R., and R. Sandler. 2018. "The Welfare Impact of Second-Best Uniform-Pigouvian Taxation: Evidence from Transportation." *American Economic Journal: Economic Policy* 10 (4): 211–42. https://doi.org/10.1257/pol.20160508.

Maestre-Andrés, S., S. Drews, and J. van den Bergh. 2019. "Perceived fairness and public acceptability of carbon pricing: a review of the literature." *Climate Policy* 19 (9): 1186–1204. https://doi.org/10.1080/14693062.2019.1639490.

OECD. 2019. *Taxing Energy Use 2019: Using Taxes for Climate Action.* Paris: OECD Publishing. https://doi.org/10.1787/058ca239-en.

Pindyck, R. S. 2019. "The social cost of carbon revisited." *Journal of Environmental Economics and Management* 94:140–160. https://doi.org/10.1016/j.jeem.2019.02.003.

Reynaert, M., and J. M. Sallee. 2021. "Who Benefits When Firms Game Corrective Policies?" *American Economic Journal: Economic Policy* 13 (1): 372–412. https://doi.org/10.1257/pol.20190019.

Tenkanen, H., and T. Toivonen. 2020. "Longitudinal spatial dataset on travel times and distances by different travel modes in Helsinki Region." *Scientific Data* 7:1–15. https://doi.org/10.1038/s41597-020-0413-y.

Tietge, U., S. Díaz, P. Mock, A. Bandivadekar, J. Dornoff, and N. Ligterink. 2019. *From Laboratory to Road: A 2018 Update on Official and "Real-World" Fuel Consumption and CO_2 Values for Passenger Cars in Europe.* White paper. ICCT.

Wardman, M., J. Toner, N. Fearnley, S. Flügel, and M. Killi. 2018. "Review and meta-analysis of inter-modal cross-elasticity evidence." *Transportation Research Part A: Policy and Practice* 118:662–681. https://doi.org/10.1016/j.tra.2018.10.002.

West, J., M. Hoekstra, J. Meer, and S. L. Puller. 2017. "Vehicle miles (not) traveled: Fuel economy requirements, vehicle characteristics, and household driving." *Journal of Public Economics* 145:65–81. https://doi.org/10.1016/j.jpubeco.2016.09.009.

Appendix A: Figures

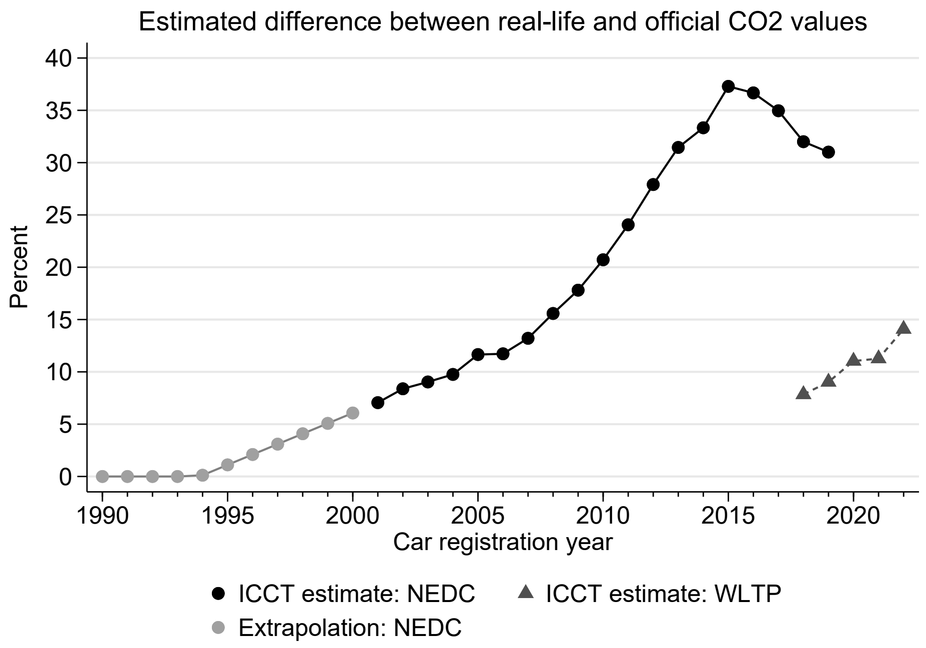

Figure 10: Estimates of the gap between real-world and manufacturer-reported CO_2 emissions

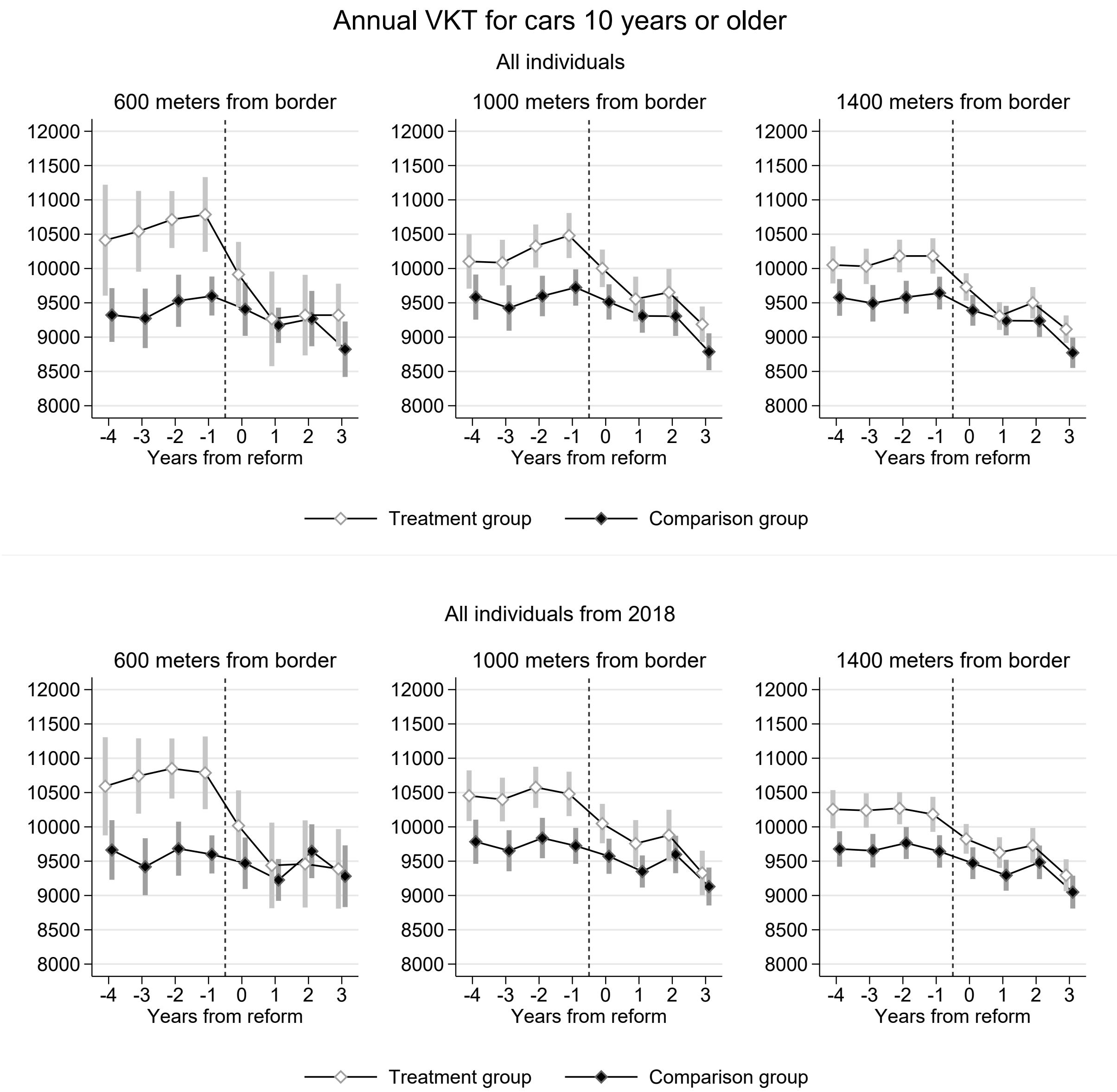

Figure 11: VKT in levels for cars 10 years or older

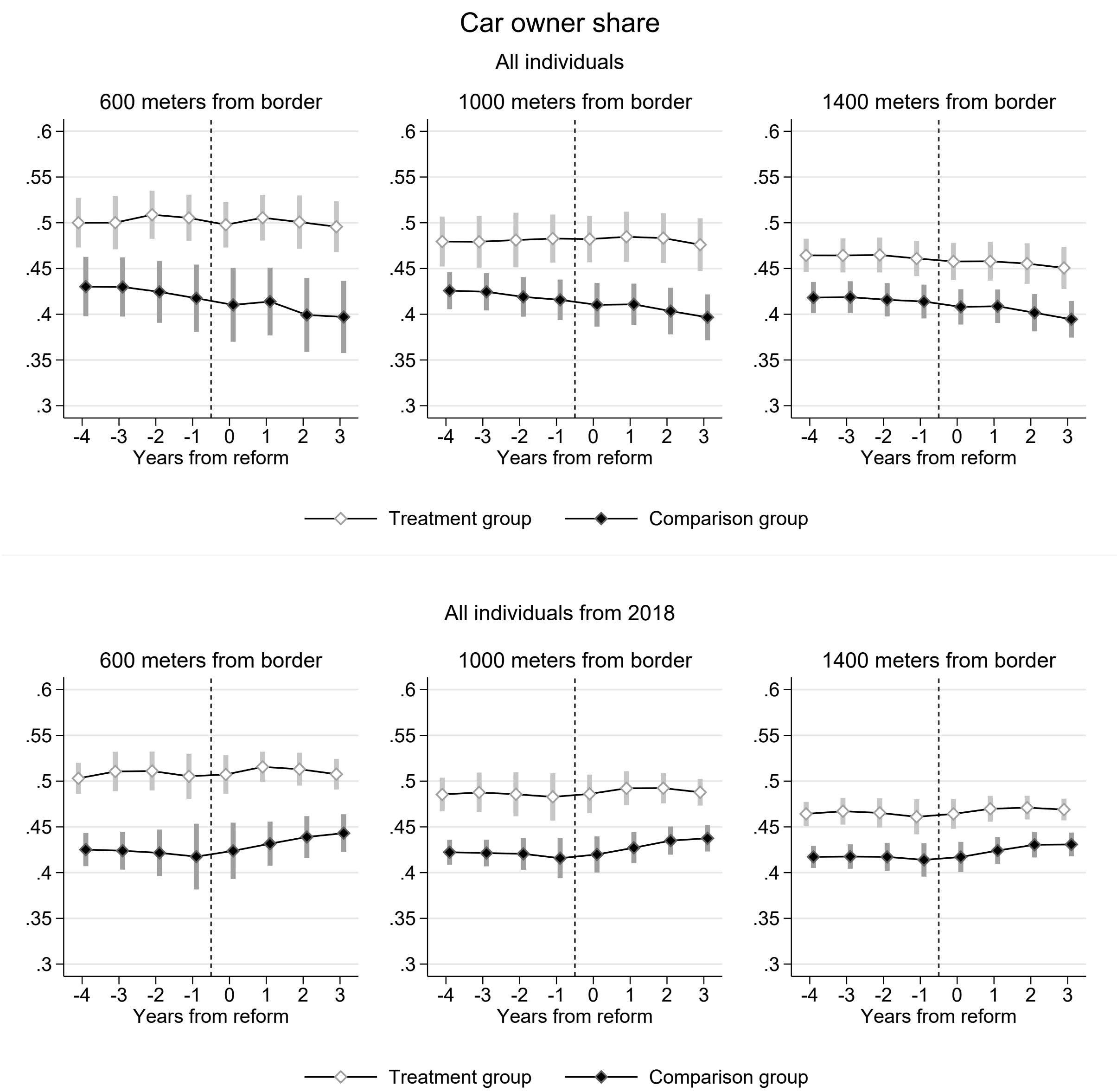

Figure 12: Car ownership rate in levels

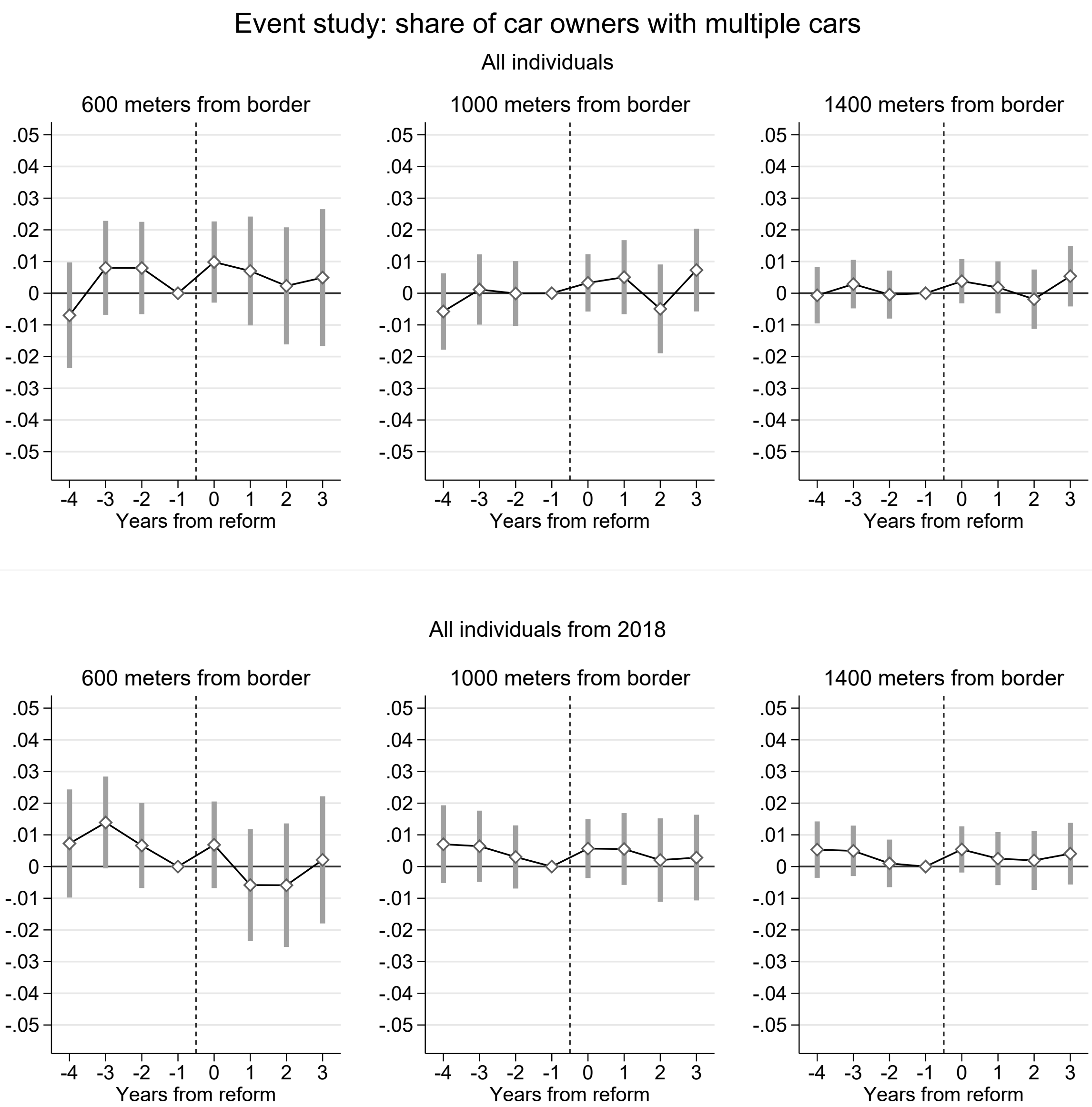

Figure 13: Multiple car ownership event study

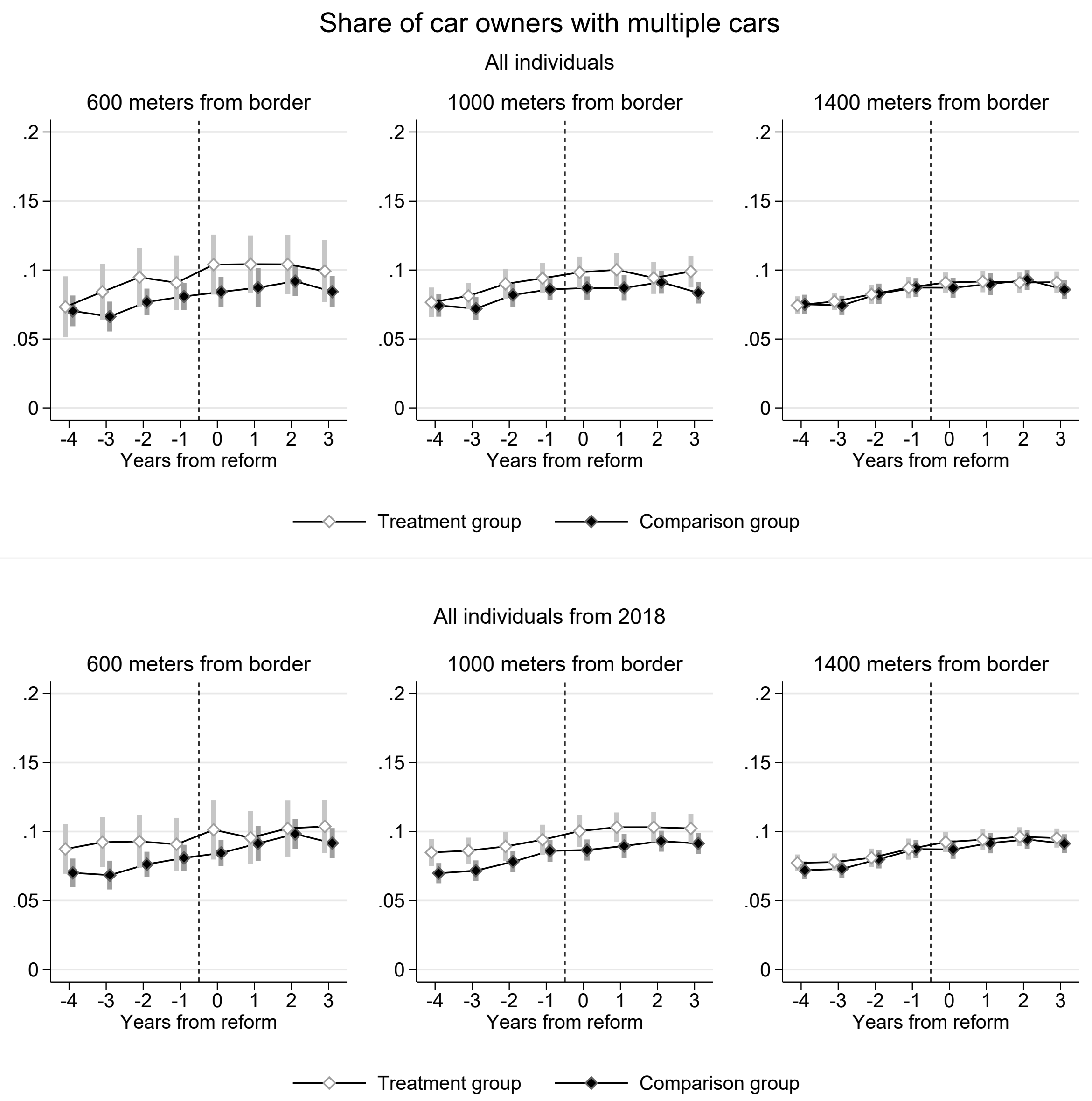

Figure 14: Multiple car ownership rate in levels

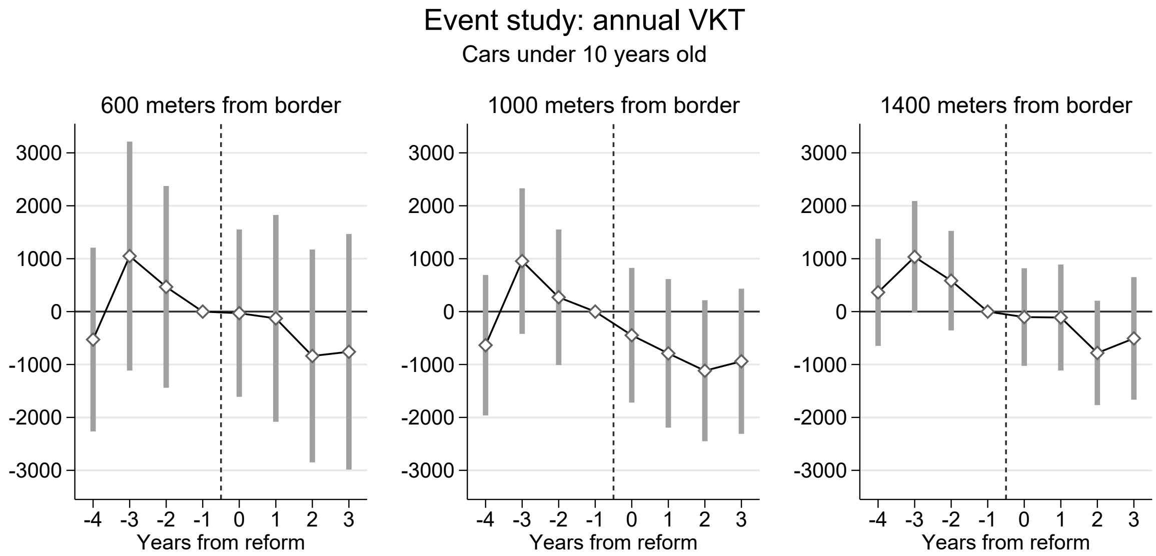

Figure 15: VKT event study for cars under 10 years old

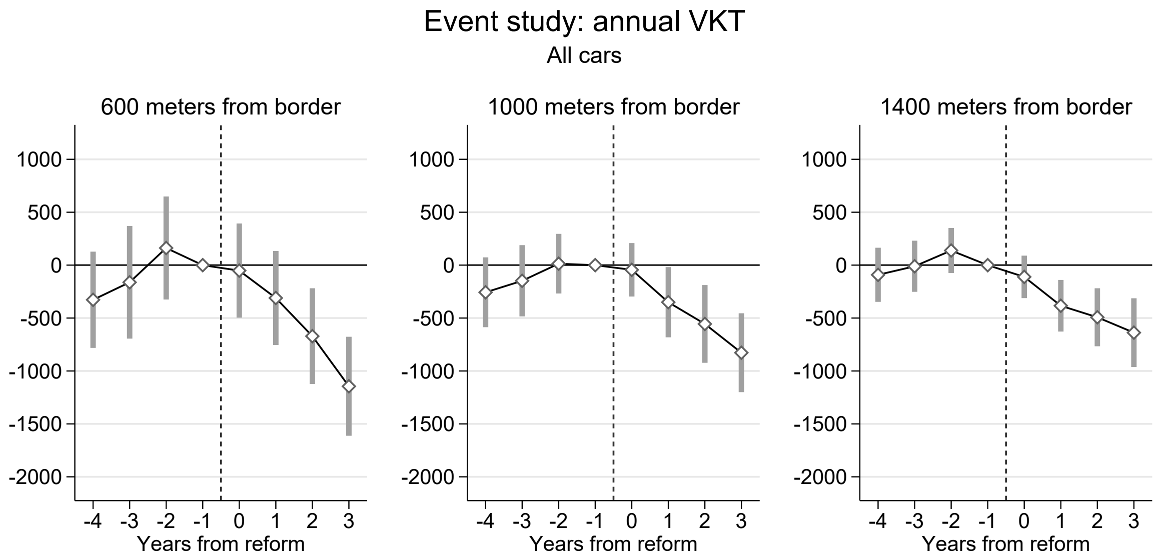

Figure 16: VKT event study for all cars regardless of age

Appendix B: Tables

Table 5: Descriptive statistics for drivers living within 1000 meters of the border in 2018

	Zone B	Zone C	Difference	Standard error
Women (%)	32.7	34.9	2.2	(1.2)
Age	45.2	46.3	1.1**	(0.4)
Household size	2.6	2.4	-0.2***	(0.0)
Employed (%)	74.1	67.7	-6.4***	(1.2)
Unemployed (%)	5.5	6.4	0.9	(0.6)
Working in zone A (%)	23.1	21.8	-1.3	(1.4)
Disposable income	30,405.6	28,082.4	-2323.2***	(373.4)
Disposable household income	55,252.4	47,751.2	-7501.2***	(1914.2)
Public transit to Helsinki (min)	50.2	44.4	-5.8***	(0.2)
Car to Helsinki (min)	42.3	44.5	2.2***	(0.1)
Number of drivers	2407	4332		

The table includes all drivers with non-missing VKT for cars 10 years or older

T-test for the difference between zones B and C. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Descriptive statistics for drivers living within 1400 meters of the border in 2018

	Zone B	Zone C	Difference	Standard error
Women (%)	33.8	34.3	0.5	(0.9)
Age	46.4	46.4	-0.0	(0.3)
Household size	2.5	2.4	-0.2***	(0.0)
Employed (%)	70.9	67.4	-3.5***	(0.9)
Unemployed (%)	5.6	6.6	1.0*	(0.5)
Working in zone A (%)	23.4	21.8	-1.6	(1.0)
Disposable income	29,519.6	27,802.4	-1717.3***	(275.5)
Disposable household income	52,564.2	47,448.3	-5115.8***	(1171.1)
Public transit to Helsinki (min)	48.3	44.4	-3.9***	(0.1)
Car to Helsinki (min)	42.2	44.9	2.7***	(0.0)
Number of drivers	5172	5860		

The table includes all drivers with non-missing VKT for cars 10 years or older

T-test for the difference between zones B and C. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: VKT difference-in-differences results: all individuals, distance varies in both groups

	(1) 600m	(2) 700m	(3) 800m	(4) 900m	(5) 1000m	(6) 1100m	(7) 1200m	(8) 1300m	(9) 1400m	(10) 1500m	(11) 1600m	(12) 1700m	(13) 1800m	(14) 1900m	(15) 2000m
Zone B × Post	-892.0***	-749.6***	-442.4*	-417.4*	-291.0	-346.4*	-293.0*	-338.9**	-278.7*	-255.7*	-189.0	-227.7*	-247.4*	-242.0*	-214.8*
	(251.0)	(218.1)	(212.7)	(179.9)	(167.7)	(146.9)	(137.1)	(125.4)	(119.5)	(117.7)	(109.8)	(106.8)	(101.5)	(98.6)	(95.7)
Zone B	1186.0***	1134.6***	794.4***	801.7***	667.5***	750.7***	709.6***	698.8***	538.3***	504.8***	366.4**	367.8**	334.9**	312.2**	295.9**
	(265.0)	(236.1)	(223.7)	(190.4)	(179.9)	(166.4)	(155.2)	(147.3)	(145.7)	(140.1)	(134.1)	(128.2)	(122.1)	(116.8)	(113.2)
Post	-278.5*	-296.2*	-290.4**	-326.1**	-363.4***	-373.7***	-401.9***	-388.8***	-420.1***	-430.6***	-469.7***	-485.0***	-482.4***	-486.6***	-480.3***
	(140.6)	(118.5)	(111.3)	(107.8)	(103.3)	(95.5)	(91.5)	(86.8)	(84.5)	(84.4)	(78.4)	(76.3)	(72.1)	(68.3)	(66.6)
Constant	9438.5***	9472.2***	9518.5***	9549.7***	9586.6***	9548.8***	9555.4***	9544.9***	9575.5***	9584.5***	9633.1***	9658.7***	9698.3***	9696.6***	9704.6***
	(152.1)	(140.9)	(143.9)	(132.3)	(126.9)	(118.2)	(111.7)	(107.0)	(101.8)	(99.4)	(96.0)	(94.3)	(91.6)	(87.2)	(85.2)
Observations	24,524	31,723	39,736	47,817	53,332	62,939	69,279	77,661	86,969	91,075	102,016	109,125	118,118	126,270	132,345
Elasticity	0.247	0.208	0.126	0.119	0.084	0.099	0.084	0.097	0.081	0.075	0.056	0.067	0.073	0.071	0.063

The dependent variable is annual VKT for all cars 10 years or older. The change in the price of public transit used to calculate the elasticity of VKT is -34 percent.
Standard errors, in parentheses, are clustered at the level of 250 meters by 250 meters grid cells. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: VKT difference-in-differences results: all individuals, distance in comparison group fixed at 600 meters

	(1) 600m	(2) 700m	(3) 800m	(4) 900m	(5) 1000m	(6) 1100m	(7) 1200m	(8) 1300m	(9) 1400m	(10) 1500m	(11) 1600m	(12) 1700m	(13) 1800m	(14) 1900m	(15) 2000m
Zone B × Post	-892.0***	-767.3**	-454.3*	-465.1*	-375.9	-441.5*	-416.5*	-449.2**	-420.3*	-407.8*	-380.2*	-434.2**	-451.3**	-450.1**	-416.7**
	(251.0)	(230.9)	(229.4)	(201.3)	(192.9)	(179.4)	(173.6)	(167.0)	(163.8)	(162.6)	(160.0)	(159.0)	(157.5)	(157.3)	(156.2)
Zone B	1186.0***	1168.2***	874.5***	912.9***	815.6***	861.1***	826.5***	805.3***	675.3***	650.8***	560.9**	588.0***	594.7***	570.3***	562.0***
	(265.0)	(243.0)	(229.0)	(204.6)	(198.4)	(191.9)	(186.3)	(182.6)	(184.2)	(181.2)	(178.4)	(174.9)	(171.9)	(170.5)	(169.1)
Post	-278.5*	-278.5*	-278.5*	-278.5*	-278.5*	-278.5*	-278.5*	-278.5*	-278.5*	-278.5*	-278.5*	-278.5*	-278.5*	-278.5*	-278.5*
	(140.6)	(140.6)	(140.5)	(140.5)	(140.4)	(140.4)	(140.4)	(140.4)	(140.3)	(140.3)	(140.3)	(140.3)	(140.3)	(140.3)	(140.3)
Constant	9438.5***	9438.5***	9438.5***	9438.5***	9438.5***	9438.5***	9438.5***	9438.5***	9438.5***	9438.5***	9438.5***	9438.5***	9438.5***	9438.5***	9438.5***
	(152.1)	(152.1)	(152.0)	(152.0)	(151.9)	(151.9)	(151.9)	(151.9)	(151.8)	(151.8)	(151.8)	(151.8)	(151.8)	(151.8)	(151.7)
Observations	24,524	26,555	29,565	33,842	36,483	42,591	46,077	51,666	58,089	60,952	67,337	72,644	78,394	82,983	87,428
Elasticity	0.247	0.213	0.130	0.132	0.108	0.126	0.119	0.129	0.122	0.119	0.112	0.127	0.132	0.132	0.123

The dependent variable is annual VKT for all cars 10 years or older. The change in the price of public transit used to calculate the elasticity of VKT is -34 percent.
Standard errors, in parentheses, are clustered at the level of 250 meters by 250 meters grid cells. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: VKT difference-in-differences results: individuals around the border in 2018, distance varies in both groups

	(1) 600m	(2) 700m	(3) 800m	(4) 900m	(5) 1000m	(6) 1100m	(7) 1200m	(8) 1300m	(9) 1400m	(10) 1500m	(11) 1600m	(12) 1700m	(13) 1800m	(14) 1900m	(15) 2000m
Zone B × Post	-981.8***	-778.3***	-556.0**	-547.0**	-387.7*	-369.7*	-284.5	-252.6	-256.7*	-224.2	-169.5	-199.3	-207.8	-220.7*	-198.0
	(271.4)	(234.8)	(210.9)	(185.8)	(173.3)	(154.0)	(145.7)	(135.3)	(129.3)	(125.8)	(117.2)	(112.2)	(107.4)	(104.4)	(102.8)
Zone B	1156.9***	1187.0***	832.7***	875.8***	729.8***	771.3***	739.2***	684.5***	552.0***	508.4***	362.6**	350.2**	298.7*	276.0*	262.9*
	(258.8)	(229.7)	(210.8)	(185.1)	(173.6)	(158.7)	(148.8)	(143.0)	(143.6)	(138.4)	(134.3)	(128.3)	(121.2)	(116.3)	(113.5)
Post	-185.6	-245.1*	-282.3*	-286.4**	-336.1***	-341.1***	-395.5***	-382.0***	-359.5***	-376.8***	-411.4***	-423.5***	-423.9***	-408.6***	-406.2***
	(135.3)	(117.2)	(112.1)	(104.1)	(100.0)	(96.7)	(96.5)	(92.9)	(89.4)	(88.2)	(82.8)	(80.5)	(77.1)	(74.5)	(73.6)
Constant	9590.8***	9616.2***	9671.9***	9675.0***	9748.8***	9703.1***	9694.0***	9679.2***	9684.5***	9699.3***	9738.2***	9765.6***	9811.6***	9803.0***	9817.0***
	(156.0)	(137.4)	(138.6)	(126.8)	(122.9)	(114.7)	(108.1)	(104.3)	(99.1)	(97.2)	(94.9)	(93.6)	(90.9)	(87.1)	(86.0)
Observations	23,421	30,223	38,070	46,079	51,551	60,700	66,892	75,152	84,508	88,538	99,149	105,938	115,137	123,088	129,082
Elasticity	0.269	0.212	0.156	0.153	0.109	0.104	0.080	0.072	0.074	0.065	0.049	0.058	0.060	0.064	0.058

The dependent variable is annual VKT for all cars 10 years or older. The change in the price of public transit used to calculate the elasticity of VKT is -34 percent.
Standard errors, in parentheses, are clustered at the level of 250 meters by 250 meters grid cells. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 10: VKT difference-in-differences results: individuals around the border in 2018, distance in comparison group fixed at 600 meters

	(1) 600m	(2) 700m	(3) 800m	(4) 900m	(5) 1000m	(6) 1100m	(7) 1200m	(8) 1300m	(9) 1400m	(10) 1500m	(11) 1600m	(12) 1700m	(13) 1800m	(14) 1900m	(15) 2000m
Zone B × Post	-981.8***	-837.8***	-652.6**	-647.7**	-538.2**	-525.2**	-494.4**	-449.0**	-430.6**	-415.3*	-395.3*	-437.2**	-446.1**	-443.7**	-418.7**
	(271.4)	(246.0)	(226.2)	(207.2)	(197.2)	(181.5)	(175.7)	(169.0)	(165.9)	(163.6)	(160.2)	(158.2)	(156.1)	(155.3)	(154.8)
Zone B	1156.9***	1212.4***	913.9***	960.0***	887.9***	883.6***	842.4***	772.9***	645.7***	617.0***	510.1**	525.1**	519.5**	488.2**	489.1**
	(258.8)	(241.6)	(223.2)	(206.7)	(199.2)	(191.2)	(187.0)	(184.1)	(187.8)	(184.7)	(182.7)	(179.3)	(175.7)	(174.6)	(173.3)
Post	-185.6	-185.6	-185.6	-185.6	-185.6	-185.6	-185.6	-185.6	-185.6	-185.6	-185.6	-185.6	-185.6	-185.6	-185.6
	(135.3)	(135.3)	(135.3)	(135.2)	(135.2)	(135.2)	(135.2)	(135.2)	(135.2)	(135.2)	(135.2)	(135.2)	(135.2)	(135.2)	(135.2)
Constant	9590.8***	9590.8***	9590.8***	9590.8***	9590.8***	9590.8***	9590.8***	9590.8***	9590.8***	9590.8***	9590.8***	9590.8***	9590.8***	9590.8***	9590.8***
	(156.0)	(156.0)	(156.0)	(156.0)	(156.0)	(156.0)	(156.0)	(156.0)	(156.0)	(156.0)	(156.0)	(156.0)	(156.0)	(156.0)	(156.0)
Observations	23,421	25,394	28,339	32,604	35,201	40,895	44,361	50,009	56,386	59,180	65,232	70,224	76,254	80,833	85,198
Elasticity	0.269	0.228	0.183	0.181	0.151	0.148	0.139	0.128	0.124	0.120	0.115	0.127	0.130	0.130	0.122

The dependent variable is annual VKT for all cars 10 years or older. The change in the price of public transit used to calculate the elasticity of VKT is -34 percent.
Standard errors, in parentheses, are clustered at the level of 250 meters by 250 meters grid cells. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$