How does economic activity adapt to pollution pricing?

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Abstract

Policymakers around the world are exploring ways to tackle greenhouse gas emissions, but when evaluation focuses on narrow margins, policies can have unintended consequences. We study the phased introduction of London's Ultra-Low Emission Zone (ULEZ), a tax on highly-polluting vehicles. A simple model of location and commuting behaviour highlights four important margins of adjustment: vehicle investment, commuting mode, firm location and residential location. We study those four margins using event study and regression discontinuity methods, and exploit the randomness of the precise borders and the differential exposure of affected individuals based on their pre-existing commuting choices. We show the initial announcement of the ULEZ had large, significant positive effects on adoption of ultra-low emission vehicles. The policy had a positive effect on house prices within the zone. Individuals more exposed to the ULEZ used public transport more after the policy is introduced. Finally, we find that firms relocated outside the zone after the policy was announced. Within Greater London, response magnitudes on each margin vary substantially across space.

JEL: H23; R40; R48; Q58

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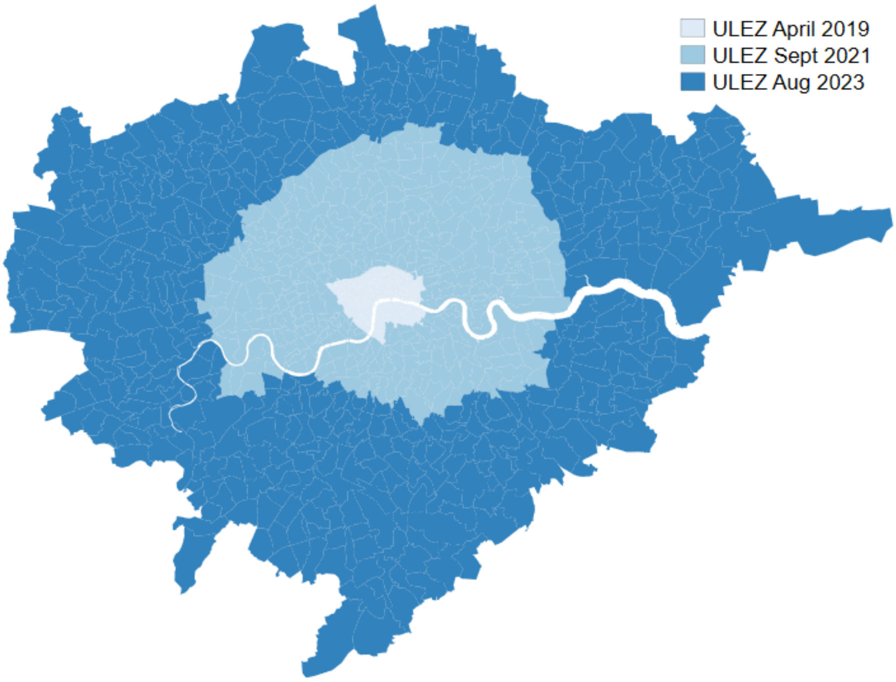
1 Introduction

There are high social costs to air pollution.[1] The intensity of pollution exposure and the number of people exposed mean that these costs are greatest in urban areas. Governments have therefore implemented a variety of policies in the transportation sector, a key source of urban air pollution. In Europe, "low emissions zones" now prohibit or heavily tax the use of highly-polluting vehicles in many city centers. There are several ways households and firms may adapt to such corrective taxes: by driving less, switching to other transit modes, changing their home or work location, investing in less-polluting vehicles or by keeping behaviour constant and paying the pollution price. These choices not only have consequences for pollution itself, but also for the wider pattern of economic activity across affected cities. In this paper, we therefore estimate the economic responses of affected citizens to the introduction and subsequent expansions of London's Ultra Low Emission Zone (ULEZ).

To fix ideas, we nest a model of commuting choice within a canonical model of spatial economics. The model highlights possible adaptations along four possible margins: affected individuals can react by purchasing ULEZ-compliant vehicles, switching to public transport, changing the location of their home or employer, or can simply choose to absorb the cost of driving into the ULEZ. We exploit the phased introduction of London's ULEZ using a shift-share event-study design (Borusyak, Hull, and Jaravel 2022) to study the adaptation of economic activity along all of the highlighted margins. In addition to estimating responses to the policy in retrospect, we provide a framework for monitoring the effectiveness of this policy throughout future phases in near-real time. The phased introduction of the ULEZ changes over time which vehicles can drive into particular areas of London without paying a fee, affecting otherwise similar commuter-belt postcodes differently based on their location and pre-existing economic choices. By estimating responses across all margins, we uncover otherwise neglected trade-offs and potentially unintended consequences of the policy.

[1] Chay and Greenstone 2005; Currie and Neidell 2005; Currie and Walker 2011; Deschenes, Greenstone, and Shapiro 2017; Alexander and Schwandt 2019; Deryugina, Heutel, Miller, Molitor, and Reif 2019.

Figure 1: The evolution of London's ULEZ expansion

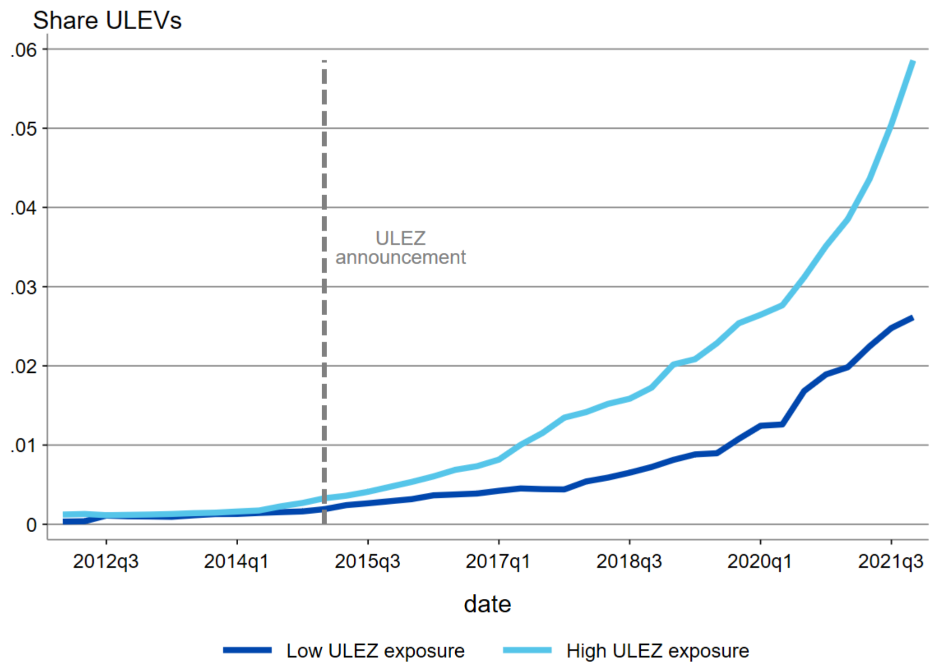


The ULEZ, described by the BBC as "the most radical plan you've never heard of", was first announced in March 2015 and introduced on 8 April 2019 as a £12.50 fee to drive a highly-polluting vehicle into central London.[2] However, the ULEZ at first exempted residents from taxation and thus applied only to commuters. An expansion of the ULEZ to cover a wider area of London was confirmed in June 2018, and this expansion began in September 2021. In the process, the ULEZ expanded in size about tenfold and no longer exempted residents. A so far final expansion was announced in November 2022. Under the new rules, which came into force on 29 August 2023, all London boroughs, and most of Greater London, were included in the ULEZ. Figure 1 shows the spatial expansion of the policy.

The ULEZ "treatment" varies strikingly across space, as those commuting into the ULEZ face the strongest incentives to substitute towards less-polluting vehicles, public transport, or towards working patterns that require less physical presence in Central London. We use this policy variation, together with pre-existing variation in the share of commuters across adjacent postcodes, to analyse how individuals adjust their

[2] BBC, 2019.

Figure 2: Adoption of ultra-low emissions vehicles in high and low ULEZ exposure postcode districts



economic activity in response to the policy.

Even a cursory look at public-use Driver and Vehicle Licensing Authority (DVLA) data on vehicle registrations by postcode district shows a large response in compliant vehicle purchases upon introduction of the ULEZ. These data report counts of the number of all registered vehicles and all tax-exempt vehicles (ULEVs) by postcode and quarter; it is illegal to drive an unregistered vehicle. To identify treated postcodes, we use data from the 2011 UK Census on commuting flows by origin and destination and calculate the share of commuters in each postcode who commute by car to destinations in the ULEZ. Figure 2 is suggestive of a sharper rise in ULEVs in regions where individuals are more exposed to the ULEZ due to their commuting behaviour.

We repeat this exercise for the ULEZ expansion, first announced in May 2016. This expanded zone went up to the North and South Circular roads, and was implemented in September 2021. The expansion increased the share of commuters driving into the ULEZ across many postcodes. Once again, Figure D2 in the appendix highlights a sharp rise in adoption of ULEVs for those more affected by the expansion, at the time the expansion was announced.

We use two empirical research designs to estimate the causal impact pollution pricing has on economic activity. For outcomes related to commuting behaviour (such as ULEV registration and public transport use), we employ a shift-share differences-in-differences (DiD) design (Bartik 1991; Borusyak, Hull, and Jaravel 2022; De Chaisemartin and d'Haultfoeuille 2022; Roth, Pedro HC Sant'Anna, Bilinski, and Poe 2023). Individuals are differentially exposed to the policy via pre-determined economic decisions. Thus we interact pre-existing commuter patterns at the postcode district level with time-varying coverage of the ULEZ with a shift-share design. For outcomes related to location choice (firm creation and house transactions and prices), we instead employ a regression discontinuity design (RDD) (Frölich and Huber 2019; Cattaneo and Titiunik 2022) and compare outcomes just inside and outside the ULEZ as the boundaries change. This captures the intuition that incentives on these margins change discontinuously for individuals living on either side of the ULEZ boundary.

We show that there is a large, positive and significant effect of ULEZ exposure on ULEV adoption. Our results suggest an average 1.3% rise in the share of electric vehicles by the end of 2019, for each 1% increase in the share of affected commuters in a postcode district. The median postcode district has 1% of commuters affected by the ULEZ, and 0.3% of vehicles are ULEVs. Thus the impact is also significant in economic terms. We also find evidence that the introduction of the ULEZ has differentially affected the value of sold residential properties, with a boost for house prices within the zone. We find that houses within the zone experienced a 12% increase in sold prices on average after the policy was announced in early 2015. The effect on public transport substitution behaviour suggests some switching towards the London underground network. Finally, there is evidence that firms differentially choose to locate outside the zone after the policy is announced.

This paper contributes to two active strands of the literature. First, a series of recent papers has investigated policies aimed at changing driving behaviour, especially taxing certain vehicles or taxing driving in specific zones. Closest to this paper are perhaps Barahona, Gallego, and Montero (2020) and Herzog (2023). Barahona, Gallego, and Montero (2020) investigate the effect of a policy introduced in 1992 in Santiago, Chile. This policy restricted the use of certain vintages of vehicles which were deemed more-polluting. They find the policy was effective at encouraging switching towards cleaner vehicles, and that this switch was welfare-improving. Likewise, the ULEZ places limits on older vintages of vehicles. Herzog (2023) focuses on the same geographic setting as we do, by investigating the introduction of the earlier Congestion Charge (CC) in London in 2003. The paper finds evidence the policy reallocated commuters between driving and public transport, with differential impacts across skill groups, leading the benefits to accrue progressively. Road traffic was reduced by approximately 1%, taking into account endogenous sorting and substitution towards untaxed driving routes.[3]

Second, recent research has also analysed the impact of emission-curbing policies on housing (Tang 2021; Gruhl, Volhausen, Pestel, and Moore 2022; Aydin and Kurschner Rauck 2023), with evidence that house prices respond positively. Driving taxes and low-emission zones are often motivated by a desire to reduce air pollution, and the evidence suggests mixed results on this front (Simeonova, Currie, Nilsson, and Walker 2019; Wolff and Zhai 2021; Gu, Deffner, Kuchenhoff, Pickford, Breitner, Schneider, Kowalski, Peters, Lutz, Kerschbaumer, Slama, Morelli, Wichmann, and Cyrys 2022; Bernardo, Fageda, and Flores-Fillol 2021).

This paper makes three contributions. First it provides causal evidence of how the economic geography of one of the world's largest cities changes in response to pollution pricing. It thus bridges the emerging literatures on the spatial impacts of climate change (Castro-Vincenzi 2022; Desmet and Rossi-Hansberg 2023; Ponticelli, Q. Xu, and Zeume 2023) and climate mitigation policies (Arkolakis and Walsh 2023; Colas and Saulnier 2023; Gilbert, Gagarin, and Hoen 2023). Second, it provides a rich set of policy-relevant elasticities that can inform the large literature on the optimal design of pollution pricing (Peltzman and Tideman 1972; Van Der Ploeg and Withagen 2014; Clausing and Wolfram 2023), and does so across all major margins of adjustment. By

[3] For more information on the background and impact of London's CC, we refer the interested reader to Leape (2006).

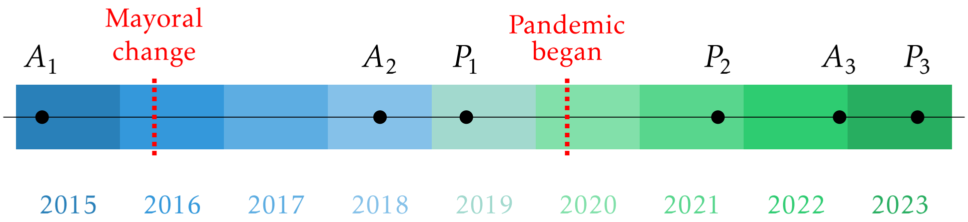
measuring relevant margins in the same setting, it allows policymakers to understand the full spectrum of adjustment behaviours. Third, alongside a few like-minded papers (Clemens and Lewis 2022; Fetzer, Gazze, and Bishop 2023; Fetzer 2023; Fetzer, Palmou, and Schneebacher 2023) this paper provides a framework for how to analyse policy responses to quasi-experiments in near-real time using a combination of high-frequency, granular data sources and transparent, pre-registered research design.

The rest of this paper is organised as follows. Section 2 provides an overview of the ULEZ and its introduction. Section 3 presents our theoretical model. Section 4 describes the data we use. Section 5 discusses our empirical approach. Section 6 presents our empirical results. Section 7 discusses the implications of our findings. A final Section 8 concludes and outlines next steps.

2 London's Ultra Low Emission Zone (ULEZ)

The Ultra Low Emission Zone (ULEZ) is an area of London for which an emission-standard based daily levy of £12.50 applies to non-compliant vehicles. The zone operates 24 hours a day, seven days a week. The daily charge currently applies to residents of the ULEZ as well as commuters. This was not always the case. The criteria for charging the levy is based on European emission standards. A penalty charge of £180 is applied for non-compliance. This charge is in addition to the Congestion Charge (CC) and applies to cars, motorcycles, vans, specialist vehicles (up to and including 3.5 tonnes) and minibuses. Vans and minibuses are also be subject to Low Emission Zone (LEZ) charges. Boris Johnson, then Mayor of London, announced the zone (covering the same central area as the Congestion Zone in 2015) would come into effect in September 2020. Sadiq Khan, Johnson's successor, introduced the Toxicity charge or "T-charge", a £10 emissions surcharge for older, more polluting vehicles in October 2017, which covered the same area as the Congestion Zone. The T-charge was replaced by the ULEZ when it came into effect in April 2019, ahead of schedule. The ULEZ was expanded out to the North and South Circular roads in 2021. In November 2022,

Figure 3: Timeline of ULEZ announcements and implementation. A = announcement, P = policy introduction. Subscripts indicate the first, second and third expansions of the policy.



Sadiq Khan announced the expansion of the zone to cover all 32 London boroughs from August 2023. This matches the existing Low Emissions Zone (LEZ) boundary. Figure 3 plots the timeline of the ULEZ expansion.

The expanded ULEZ is part of the effort to help improve air quality in and around London and reduce the impact on the health of residents and visitors to the city. The ULEZ is principally aimed at reducing levels of two key air pollutants from vehicle exhausts: nitrogen dioxide (NOx) and fine particle matter (PM). These pollutants have been linked to premature deaths and stunted growth of children's lungs. Appendix A provides a more detailed account of the ULEZ, and Appendix B a full timeline of policy announcements and implemented changes.

3 Theoretical model and main hypotheses

Given widespread beliefs in the public debate that the primary impact of the ULEZ is on commuting behaviour,[4] we focus our analysis on the economic geography of work across Greater London. Individuals currently driving into London for work in non-ULEZ compliant vehicles have three options when the policy applies to them: they can adapt on the commuting **mode** margin; they can adapt on the commuting **distance** margin; or they can **do nothing** and pay the ULEZ charge. If they choose to adapt on the commuting mode margin, they again have three options: they can purchase a ULEZ-compliant vehicle; they can switch to public transport; or they can work from home more often. If they choose to adapt on the commuting distance margin,

[4]The Guardian, 2023.

they can either change employer location (for instance, by switching jobs) or move home. The next subsection formalises these choices in the context of a simple model of commuting. The analysis of the hybrid-work margin is still work in progress and therefore omitted from this draft.

3.1 Model

We sketch out a simple model of commuting, following some of the ideas in Ommeren and Dargay (2004) and Monte, Redding, and Rossi-Hansberg (2018). This model includes the core components of spatial economics models, with a multinomial logit structure on commuting behaviour. Firms are spatially dispersed and produce with geographically-specific productivity and locally determined wages. Workers purchase goods and housing services to maximise utility, and choose their home and work locations taking commuting costs into account. Commuting behaviour is driven by direct costs (for instance, taxes, vehicles, tickets) and indirect costs (that is, congestion).

3.1.1 Environment

Utility:

$$U_{ij\omega} = \frac{b_{ij\omega}}{\kappa_{ij}} \left(\frac{C_{i\omega}}{\alpha} \right)^{\alpha} \left(\frac{H_{i\omega}}{1-\alpha} \right)^{1-\alpha}$$

for individual ω commuting from location i to location j. They receive idiosyncratic amenity b, pay commuting cost κ, purchase C consumption and H housing services. The budget constraint satisfies $C_{i\omega} + Q_i H_{i\omega} = w_j - \kappa_{ij}$, where Q_i is the housing price.

Solving the utility maximisation problem yields the indirect utility function:

$$V_{ij\omega} = \frac{b_{ij\omega}}{\kappa_{ij}} (w_j - \kappa_{ij}) Q_i^{\alpha-1}$$

Firm: firms produce output with a labour input and constant returns to scale $Y_j = Z_j L_j$. They choose input L_j to maximise profits. The wage is the outcome of Nash bargaining $\max_{w_j} (w_j - \kappa_{ij})^{\beta} (Z_j - w_j)^{1-\beta}$ which yields a wage which is the weighted average of commuting costs and local productivity $w_j = \beta\kappa_{ij} + (1-\beta) Z_j$.

Firm location: prior to making input and output choices, firm choose their location j based on expected profits $\mathbb{E}\pi_j = (Z_j - w_j)L_j$. The introduction of the tax on polluting vehicles affects expected profits differently for regions inside (j) and outside (k) the taxable zone.

Housing market: the demand for housing services is determined from utility maximisation.

$$H^d_{i\omega} = (1-\alpha)\frac{w_j - \kappa_{ij}}{Q_i}$$

Aggregating over all individuals living in i:

$$H^d_i = \sum_j \sum_{\omega \in (i,j)} (1-\alpha)\frac{w_j - \kappa_{ij}}{Q_i} = \frac{1-\alpha}{Q_i}\sum_j N_{ij}(w_j - \kappa_{ij})$$

where N_{ij} are the number of people living in i and working in j. The supply side is simply $H^s_i = A_i Q^\epsilon_i$. Therefore the equilibrium housing price is:

$$Q_i = \left[\frac{1-\alpha}{A_i}\sum_j N_{ij}(w_j - \kappa_{ij})\right]^{\frac{1}{1+\epsilon}}$$

Commuting: individuals have three commuting options with different costs from i to j:

1. Pay tax and drive old car: $\kappa_{1|ij} = \frac{\gamma \bar{k} d_{ij}}{s(N_d)^\eta}$
2. Invest in new car and drive (pay no tax): $\kappa_{2|ij} = \frac{\bar{k} d_{ij}}{s(N_d)^\eta} + \phi$
3. Take public transport: $\kappa_{3|ij} = \bar{k} d_{ij} \bar{s}$

where $\bar{k}$ is a fixed commuting cost, d_{ij} is the distance between i and j, s represents speed of commuting which is a function of congestion N_d with speed elasticity of cost $\eta > 0$. For drivers who pay the tax $\gamma > 1$ we scale up costs, whereas those who invest in a new non-taxable vehicle pay a separable cost $\phi > 0$.

The model follows a nested structure for commuting choices. Given a home location i and work location j, the probability of choosing commuting option m is given by $\mathbb{P}_{m|ij} = \frac{\exp(\mu_m V_{m|ij})}{\sum_n \exp(\mu_n V_{n|ij})}$, where $V_{m|ij}$ is the previously derived indirect utility, when method m is chosen, while μ_m is a scaling parameter. Denote the inclusive value $G_{ij} = \frac{1}{\mu_m} \ln \sum_m \exp(\mu_m V_{m|ij})$. Given a home location i, the probability of commuting to j is $\mathbb{P}_{j|i} = \frac{\exp(G_{ij})}{\sum_k \exp(G_{ik})}$. Note that probability of choosing work location j and commuting method m is $\mathbb{P}_{jm|i} = \mathbb{P}_{j|i} \times \mathbb{P}_{m|ij}$.

Congestion: we specify a congestion function such that the speed of commuting depends negatively on the number of drivers N_d:

$$s(N_d) = \bar{s} \exp(-\delta N_d)$$

Market clearing: the number of drivers N_d must be consistent with the probabilities and total number of individuals N:

$$N_d = N \times \sum_i \sum_j \mathbb{P}_{j|i} (\mathbb{P}_{1|ij} + \mathbb{P}_{2|ij})$$

In addition, the total labour employed in region j will be equal to the sum of all the commuters from other regions i to j: $L_j = \sum_i N_{ij}$. Finally, the number of commuters from i to j will be equal to the number of commuters from i multiplied by the probability of making that commute: $N_{ij} = N_i \times \mathbb{P}_{j|i}$.

3.1.2 Comparative statics

Effect of tax on commuting costs: the tax on old vehicles γ affects commuting costs of driving directly and indirectly via congestion.

$$\frac{d\kappa_{1|ij}}{d\gamma} \frac{\gamma}{\kappa_{1|ij}} = 1 + \eta \delta N_d \epsilon_{N_d,\gamma}$$

$$\frac{d\kappa_{2|ij}}{d\gamma} \frac{\gamma}{\kappa_{2|ij}} = \frac{\kappa_{2|ij} - \phi}{\kappa_{2|ij}} \eta \delta N_d \epsilon_{N_d,\gamma}$$

where $\epsilon_{N_d,\gamma}$ is the elasticity of congestion to the tax, which we show below will be negative and small. Thus the tax makes commuting by car *more* costly in old vehicles and *less* costly in new vehicles (by reducing congestion).

Commuting costs from i to j will be weighted by the probabilities that individuals choose each method, so $\kappa_{ij} = \mathbb{P}_{1|ij}\kappa_{1|ij} + \mathbb{P}_{2|ij}\kappa_{2|ij} + \mathbb{P}_{3|ij}\kappa_{3|ij}$. And this responds to the tax:

$$\frac{d\kappa_{ij}}{d\gamma}\frac{\gamma}{\kappa_{ij}} = \underbrace{\eta\delta N_d \epsilon_{N_d,\gamma}}_{\substack{\text{Indirect} \\ \text{congestion cost}}} \underbrace{\left(\frac{\mathbb{P}_{1|ij}\kappa_{1|ij} + \mathbb{P}_{2|ij}(\kappa_{2|ij} - \phi)}{\sum_m \mathbb{P}_{m|ij}\kappa_{m|ij}}\right)}_{\substack{\text{Weighted avg. relative} \\ \text{commuting cost of drivers}}} + \underbrace{\gamma \frac{\mathbb{P}_{1|ij}\kappa_{1|ij}}{\sum_m \mathbb{P}_{m|ij}\kappa_{m|ij}}}_{\substack{\text{Direct tax effect} \\ \text{on old vehicles}}} + \underbrace{\frac{\gamma}{\kappa_{ij}}\sum_m \kappa_{m|ij}\frac{d\mathbb{P}_{m|ij}}{d\gamma}}_{\substack{\text{Switching between} \\ \text{transport modes}}}$$

This shows that the response of commuting costs to the tax depends on (1) the indirect effect via congestion, weighted by the average commuting cost of driving over all commuting costs, (2) the direct tax effect on drivers of old vehicles, (3) the tax-induced change in commuting costs relative to the baseline. This highlights that the response of commuting costs to the driving tax depends positively on the share of taxable vehicles being used for commuting in each region $\frac{\mathbb{P}_{1|ij}\kappa_{1|ij}}{\sum_m \mathbb{P}_{m|ij}\kappa_{m|ij}}$.

Effect of tax on public transport use: the tax on old vehicles will affect the optimal commuting choices of workers, by affecting commuting costs directly and indirectly via congestion. The share of commuters using public transport will be affected by the total number of drivers. The change in probability of using public transport with respect to the tax is:

$$\frac{dP_3}{d\gamma} = -\left(\frac{dP_1}{d\gamma} + \frac{dP_2}{d\gamma}\right)$$

because the probability of using any of the three methods of commuting must sum to one. The elasticity of using public transport with respect to the tax:

$$\frac{dP_3}{d\gamma}\frac{\gamma}{P_3} = \gamma \underbrace{\left(P_2\frac{d\kappa_2}{d\gamma} + P_1\frac{d\kappa_1}{d\gamma}\right)}_{\substack{\text{Weighted change in driving} \\ \text{commuting costs to the tax}}} = \underbrace{\eta\delta N_d \epsilon_{N_d,\gamma}}_{\substack{\text{Indirect} \\ \text{congestion cost}}} \underbrace{\left(P_1\kappa_1 + P_2(\kappa_2 - \phi)\right)}_{\substack{\text{Weighted avg. commuting} \\ \text{cost of driving}}} + \underbrace{P_1\kappa_1}_{\substack{\text{Commuting cost} \\ \text{of old vehicles}}}$$

The intuition of this elasticity is straightforward. The first term is the indirect congestion cost, which falls in response to the tax. This is multiplied by a weighted average of the commuting cost of driving. In sum, this negative term describes the reduced incentive to switch to public transport due to reduced congestion on the roads. However, the second term is the direct increased commuting cost of driving old vehicles from the tax. The sum of these terms produces an ambiguous sign.

Effect of tax on investment in new vehicles: the relative probability of using an old versus a new vehicle:

$$\frac{P_1}{P_2} = \exp\left(\phi - (\gamma - 1)\kappa_1\right)$$

The elasticity with respect to the driving tax γ yields:

$$\frac{dP_1/P_2}{d\gamma}\frac{\gamma}{P_1/P_2} = -\left[\kappa_1 + (\gamma - 1)\kappa_1 \delta\eta \frac{dN_d}{d\gamma}\right]$$

The elasticity of P_1/P_2 could be positive or negative, given $\frac{dN_d}{d\gamma}$ is negative. In other words, the relative probability of paying the driving tax rather than buying a new car can *rise*, which at first glance seems unintuitive. This result is due to the tax reducing congestion, which encourages driving (in old and new cars) at the margin. The net effect is the relative benefit of buying a new car decreases, even though the absolute number of those investing in a new vehicle rises.

Effect of tax on congestion: we can differentiate the equilibrium condition:

$$\frac{\partial N_d}{\partial \gamma} = N \times \left(\frac{\partial P_1}{\partial \gamma} + \frac{\partial P_2}{\partial \gamma}\right)$$

where $\frac{\partial P_i}{\partial \gamma} = -P_j \frac{d\kappa_j}{d\gamma}$. Combining with the response of commuting costs to the tax, we obtain:

$$\frac{dN_d}{d\gamma} = \frac{P_1\kappa_1}{\frac{1}{N} - \eta\delta(P_1\kappa_1 + P_2(\kappa_2 - \phi))}$$

For large N, the number of commuters in the economy, this derivative is negative.

Hence the elasticity of congestion N_d with respect to the driving tax on old vehicles γ is negative.

Effect on house prices: there are three channels we highlight for the effect of the driving tax γ on house prices: (1) wages, (2) commuting costs, (3) number of commuters.

$$\begin{aligned}
\frac{dQ_i}{d\gamma} &= \frac{1-\alpha}{1+\epsilon}\frac{1}{A_i Q_i^{\epsilon}}\sum_j\left(\frac{dN_{ij}}{d\gamma}(w_j-\kappa_{ij})+N_{ij}\left(\frac{dw_j}{d\gamma}-\frac{d\kappa_{ij}}{d\gamma}\right)\right)\\
&= \underbrace{\frac{1-\alpha}{1+\epsilon}}_{\substack{\text{Balance of S \& D}\\ \text{for housing}}} \quad \underbrace{\frac{L_i}{H_i^s}}_{\substack{\text{Inverse housing}\\ \text{supply per worker}}} \quad \sum_j \quad \underbrace{\left(\frac{d\kappa_{ij}}{d\gamma}\left(\frac{dN_{ij}}{d\kappa_{ij}}\frac{w_j-\kappa_{ij}}{N_{ij}}-(1-\beta)\right)\right)}_{\substack{\text{Tax response of commuters via commuting cost}\\ \text{weighted by net wages per commuter}}}
\end{aligned}$$

This produces three terms which multiply to describe the response of house prices to the driving tax. The first term is the ratio of the budget spent on housing services to housing supply elasticity, which is a proxy for the balance of supply and demand for housing. When housing supply is more inelastic (lower ϵ), the house price responds more to the driving tax because quantity cannot adjust as easily. The second term is the inverse of housing supply per worker. The response of house prices to the driving tax is greater when the house supply is constrained relative to the number of workers. The final term combines the three channels mentioned above: the response of commuting costs $\frac{d\kappa_{ij}}{d\gamma}$ is multiplied by the change in the number of commuters $\frac{dN_{ij}}{d\kappa_{ij}}$, weighted by the net wage per commuter $\frac{w_j-\kappa_{ij}}{N_{ij}}$, minus the share of the wage unaffected by commuting costs $1-\beta$. This whole term summarises the change in house prices due to the effect of the driving tax on commuting. It accounts for both the direct cost increase and the resulting shift in commuting patterns, adjusted for the net benefit of commuting and the share of the wage affected by the tax.

We will assume that higher commuting costs from i to j reduce the number of commuters on that route, so $\frac{dN_{ij}}{d\kappa_{ij}} < 0$. Thus the sign of $\frac{d\kappa_{ij}}{d\gamma}$ determines the response of house prices to the driving tax, and our previous analysis showed it is positive. This leads house prices to respond negatively to the driving tax, where the driving tax is present.[5]

Effect on firm location: we consider a taxed region j and untaxed region k. It is straightforward to show that expected profits respond to the tax through *up to* two channels; labour and the wage:

$$\frac{d\mathbb{E}\pi_j}{d\gamma} = (Z_j - w_j)\frac{dL_j}{d\gamma} - \frac{dw_j}{d\gamma}L_j$$

Using $L_j = \sum_i N_{ij} = \sum_i \mathbb{P}_{ij} N_i$ and $\mathbb{P}_{j|i}$ is the multinomial logit determining the probability of commuting from i to j.

$$\frac{d\mathbb{E}\pi_j}{d\gamma} = \underbrace{(Z_j - w_j)}_{\text{Profit}} \sum_i N_{ij} \underbrace{\left[\frac{\partial V_{ij}}{\partial \gamma} - \sum_k \mathbb{P}_{k|i}\frac{\partial V_{ik}}{\partial \gamma}\right]}_{\substack{\text{Difference in utility response} \\ \text{to tax in } j \text{ compared to} \\ \text{average in all other regions}}} - \underbrace{\frac{dw_j}{d\gamma}L_j}_{\substack{\text{Wage} \\ \text{response}}}$$

This result is quite intuitive. The response of expected profits to the tax is equal to firm rents ($Z_j - w_j > 0$) multiplied by the response of labour in j to the tax, minus the change in the wage with respect to the tax. Labour's adjustment to the tax is the sum of commuters to j from all other regions i, multiplied by the difference between the response of indirect utility for these commuters to the tax, compared to the probability-weighted average of changes to indirect utility in *all other regions*. Put simply, does the polluting tax change utility for workers in j more or less relative to the average of workers in all other regions?

Clearly the term in square brackets will be negative in taxed region j, and positive in untaxed region k. In an untaxed region, workers will be *less negatively* affected by the polluting tax compared to the average worker elsewhere (in taxed and untaxed regions), even if they are negatively affected overall.

The wage response is positive in taxed region j, as commuting costs rise. This ensures expected profits decline in the taxed region $\frac{d\mathbb{E}\pi_j}{d\gamma} < 0$. The wage response is

[5] In our context, it is resident locations where the tax is levied, not workplace locations. Individuals within the ULEZ are exempt from paying the tax; those residing outside are subject to it.

negative in the untaxed region k, due to $\frac{dN_d}{d\gamma} < 0$. The pollution tax reduces total commuting, which lowers commuting costs, reducing wages which are (in part) compensation for commuting costs. This ensures expected profits rise in the untaxed region $\frac{d\mathbb{E}\pi_k}{d\gamma} > 0$. Overall, we expect firms to relocate outside the taxable regions when a tax is introduced.

Table 1: Model summary

Margin	Comparative statics	Direction
ULEV investment	$\frac{d\kappa_{ij}}{d\gamma}\frac{\gamma}{\kappa_{ij}} = \underbrace{\eta\delta N_d \epsilon_{N_d,\gamma}}_{\text{Indirect congestion cost}} \underbrace{\left(\frac{\mathbb{P}_{1\|ij}\kappa_{1\|ij} + \mathbb{P}_{2\|ij}(\kappa_{2\|ij} - \phi)}{\sum_m \mathbb{P}_{m\|ij}\kappa_{m\|ij}}\right)}_{\text{Weighted avg. relative commuting cost of drivers}} + \gamma \underbrace{\frac{\mathbb{P}_{1\|ij}\kappa_{1\|ij}}{\kappa_{ij}}}_{\text{Direct tax effect on old vehicles}} + \underbrace{\sum_m \epsilon_{\mathbb{P}_m,\gamma}}_{\text{Switching between transport modes}}$	Likely ≥ 0
House prices	$\frac{dQ_i}{d\gamma} = \underbrace{\frac{1-\alpha}{1+\epsilon}}_{\text{Balance of S \& D for housing}} \underbrace{\frac{L_i}{H_i^s}}_{\text{Inverse housing supply per worker}} \sum_j \underbrace{\left(\frac{d\kappa_{ij}}{d\gamma}\left(\frac{dN_{ij}}{d\kappa_{ij}}\frac{w_j - \kappa_{ij}}{N_{ij}} - (1-\beta)\right)\right)}_{\text{Tax response of commuters via commuting cost weighted by net wages per commuter}}$	Likely ≤ 0
Public transport	$\frac{d\mathbb{P}_{3\|ij}}{d\gamma}\frac{\gamma}{\mathbb{P}_{3\|ij}} = \underbrace{\eta\delta N_d \epsilon_{N_d,\gamma}}_{\text{Indirect congestion cost}} \underbrace{\left(\mathbb{P}_{1\|ij}\kappa_{1\|ij} + \mathbb{P}_{2\|ij}(\kappa_{2\|ij} - \phi)\right)}_{\text{Weighted avg. commuting cost of driving}} + \underbrace{\mathbb{P}_{1\|ij}\kappa_{1\|ij}}_{\text{Commuting cost of old vehicles}}$	Ambiguous
Firm location	$\frac{d\mathbb{E}\pi_j}{d\gamma} = \underbrace{(Z_j - w_j)}_{\text{Profit}} \sum_i N_{ij} \underbrace{\left[\frac{\partial V_{ij}}{\partial\gamma} - \sum_k \mathbb{P}_{k\|i}\frac{\partial V_{ik}}{\partial\gamma}\right]}_{\text{Difference in utility response to tax in } j \text{ compared to average in all other regions}} - \underbrace{\frac{dw_j}{d\gamma}L_j}_{\text{Wage response}}$	≤ 0

3.2 Main hypotheses

Table 1 summarises the margins of interest and the model parameters they relate to. Our main null hypotheses state that the introduction of the ULEZ does not affect economic activity on any of the three commuting **mode** margins and any of the two commuting **distance** margins. Our set of secondary null hypothesis is that economic activity does not react to announcements (**strong version**) or reacts equally across all margins (**weak version**). Our final set of secondary hypotheses states that postcodes do not react differentially to policy announcements based on policy-relevant characteristics (e.g., the share of existing vehicle types eligible for different scrappage payment schemes).

1. $H_{0,1}$: There is no differential change in economic behaviour (in terms of purchasing electric vehicles, using public transport, working from home, work location or home location) for those that are "treated" by the introduction of the ULEZ compared to those that are not.

2. $H_{0,2}$: Outcome variables of interest do not react (or do not react differentially) to news announcements about upcoming policy changes.

3. $H_{0,3}$: Outcome variables of interest do not react differentially based on policy-relevant characteristics of the postcode.

To maintain transparency, we logged these hypotheses in our pre-analysis plan (PAP) before conducting our analysis (Open Science Foundation, 2023).

4 Data

This section describes the data sources we use. Interested readers can compare these with our pre-analysis plan, and will note that on some margins, statistical power or disclosure control considerations have forced us to deviate slightly from our original plan. Table 2 captures an overview of our data and approaches for four margins of economic adjustment to the policy. We document the level of geographic and time aggregation, the resulting number of observations and our identification approach.

Table 2: Cleaned data summary

Margin	Parameter	Geography	Time period	N	$N \times T$	Identification
ULEV investment	$\frac{d\kappa_{ij}}{d\gamma}\frac{\gamma}{\kappa_{ij}}$	Postcode district	2012 - 2021 (quarterly)	302	9,326	DiD
House prices	$\frac{dQ_i}{d\gamma}$	Postcode	2012 - 2022 (quarterly)	90,172	629,217	RDD
Public transport	$\frac{dP_3}{d\gamma}\frac{\gamma}{P_3}$	Postcode	2019 (daily)	375	135,683	DiD
Firm creation	$\frac{d\mathbb{E}\pi_j}{d\gamma}$	Postcode district	2012 - 2018 (quarterly)	307	8,098	RDD

Across all specifications, we also use postcode district crosswalks to output areas (OAs), lower- (LSOAs) and middle-super layers (MSOAs). These areas respectively have 310; 1,500; and 7,500 average residents. To construct ULEZ exposure and weights, we use population at OA level. To compute commuting shares, we use 2011 Census commuting behaviour.

4.1 Computing ULEZ exposure

The list of postcodes in each expansion of the ULEZ is released via freedom of information requests and we manually check for consistency with other sources.

There is geographic variation in how "exposed" different areas are to "shock" of the ULEZ, based on commuting behaviour into the ULEZ. Commuters and individuals living just outside the ULEZ face the greatest incentive to substitute towards less polluting vehicles or public transport. There are two sources of randomness with regards to the policy announcement: (1) randomness of the ULEZ boundary, (2) randomness of the share of people who drive into the ULEZ.

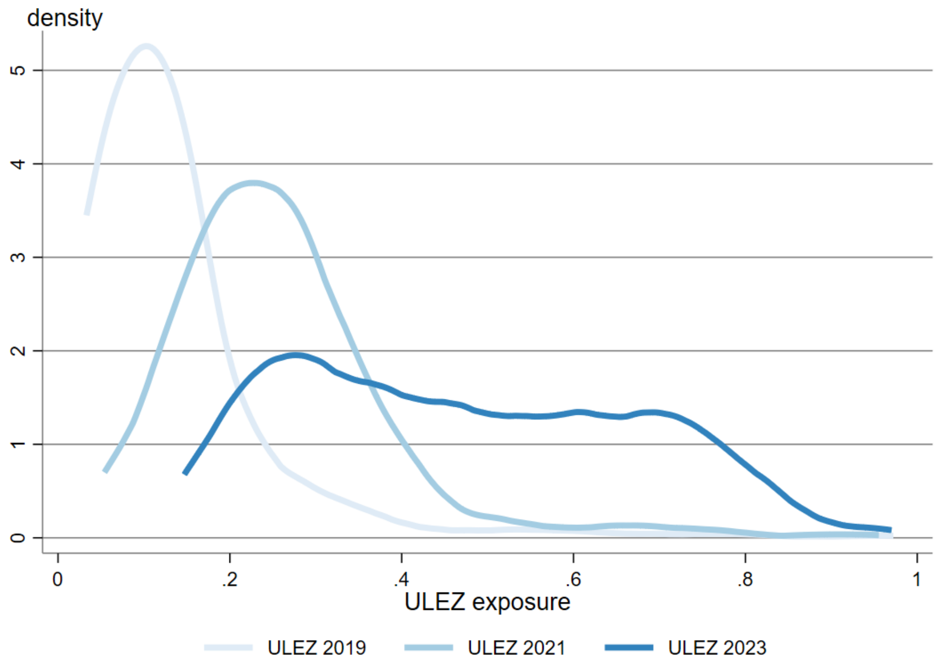
In order to compute the ULEZ exposure variable, we follow two steps:

1. **Allocate ULEZ by postcode district** - we have ULEZ assignment at the postcode level, but vehicle registrations are at the postcode district level which is more aggregated. We compute a population-based allocation at the postcode district level, which represents the share of residents who live within the ULEZ. For example, W1 4GE is in the ULEZ with 1,000 people, but W1 7PU with 500 people is not. Therefore W1 has a population-adjusted ULEZ score of 0.66.

2. **Compute ULEZ exposure by postcode district** - we calculate the vehicle-weighted shares of (ULEZ-taxable) commuting multiplied by the ULEZ score. For example, we compute the share of commuting that involves driving into the ULEZ from 2011 Census data.

Figure D1 in the appendix maps the ULEZ exposure by postcode district for London in the second quarter of 2019. The widening of the ULEZ leads to (weakly) increasing exposure to the ULEZ for a given postcode district, and more postcode districts becoming exposed to the ULEZ via commuting behaviour. Figure 4 highlights the impact on both the intensive and extensive margin across postcode districts in London.

Figure 4: Kernel density of computed ULEZ exposures for London's postcode districts.



4.2 Outcome variable construction

Electric vehicle adoption. We use data from the UK Driver and Vehicle Licensing Agency (DVLA) to estimate the substitution towards electric vehicles in response to the ULEZ. Large investment responses to the ULEZ are immediately visible in public-use tabulations of DVLA data by postcode district on vehicle registrations. These data are counts of the number of all registered vehicles and all tax-exempt vehicles (ULEVs) by postcode and quarter. It is illegal in the UK to drive an unregistered vehicle. To identify treated postcodes, we use data from the 2011 UK Census on commuting flows by origin and destination and calculate the share of commuters in each postcode who commute by car to destinations in the ULEZ.

Figure D6 shows the sharp increase in registered ultra-low emission vehicles (ULEVs) in the late 2010s in London. This is not simply a function of more vehicles registered in the capital; ULEVs are taking up a greater share of all new registrations, rising to over 1% in 2019 and over 2% in 2021, as seen in Figure D7. The geographic distribution of ULEV adoption in Greater London is contained in the appendix.[6]

House prices. To establish if affected individuals move residence in order to avoid paying the tax, we use the Price Paid Data (PPD) from HM Land Registry. The PPD includes information on property sales in England and Wales submitted to HM Land Registry for registration and excludes all commercial transactions and not for value sales. We use the 'standard' price paid entries from 2012 to 2022 to compute quarterly postcode district-level average price paid and counts of sales. We then regress prices on property characteristics (e.g., dwelling type, tenure type) before averaging in order to mitigate composition effects.

Public transport substitution. Commuters may also substitute towards public transport in response to the tax on highly-polluting vehicles. We use Transport for London (TfL) underground station-level average entry data to track the response of commuters who face the strongest incentives to substitute. The data includes station-level daily entry and exit counts for 2019, the year in which the ULEZ was first introduced.[7] We map London stations to postcodes and aggregate up to the postcode level.

Firm location. The Longitudinal Business Database (LBD) is a new, quarterly firm-level set of data spines by the UK Office for National Statistics (ONS) based on the UK's business register, the Inter-Departmental Business Register (IDBR).[8] It inherits firm and establishment postcodes from the IDBR and will be accessible through the ONS Secure Research Service (SRS). Recent analysis by the ONS uses establishment postcodes to identify labour reallocation dynamics (ONS, 2023).

We obtain postcode sector firm entry rates by quarter. For disclosure we remove any postcode sector × quarter where fewer than 10 firms are born. Figure D16 plots

[6]Our data sources for vehicle registrations by postcode district and quarter, 2012-2022 and ULEV registrations by postcode district and quarter, 2012-2022 are VEH0122 and VEH0134 respectively.

[7]TfL, 2019.

[8]For more information about the LBD, see Lemma, Lui, Romaniuk, Schneebacher, and Wolf (2023).

the distribution of detrended[9] firm entry rates over time in London. Although firm entry has stayed steady on average, there is evidence of a rise in spatial variation.

5 Empirical Approach

We use two different empirical approaches, depending on whether we expect the effect at the ULEZ boundary to be sharply discontinuous or more continuous.

5.1 Shift-share event study design

Our primary empirical approach is a shift-share event-study design of the following form:

$$\text{Outcome}_{it} = \alpha_i + \alpha_t + \beta \text{ULEZ}_i + \gamma_t \text{ShareDriveULEZ}_i + \varepsilon_{it} \tag{1}$$

where Outcome_{it} is one of the outcomes of interest in postcode district i, ULEZ_i is an indicator that i is in the 2019/2021/2023 ULEZ, and ShareDriveULEZ_i is the share of commuters in i who drive into the 2019/2021/2023 ULEZ. γ_t is the coefficient of interest. This event-study design works for the following outcomes: adoption of ULEVs and public transport commuting.

At its core, the DiD approach concerns identifying the causal impact of some (potentially non-random) treatment across units, over time. The key identifying assumption is that the relevant outcome of treated and non-treated units would have evolved *in parallel* in the absence of the treatment. It must also be that there is no causal effect of the treatment *prior* to its implementation. The 'parallel trends' assumption alongside the 'no anticipation' assumption permit identification of the average treatment effect on the treated (ATT).

Typically this would be estimated with a two-way fixed effects (TWFE) estimator. Equation (1) presents a time-varying TWFE estimator. However there are potential

[9] We regress the firm entry rate (new firms divided by all firms) at the postcode sector × quarter level on year and quarter fixed effects. The resulting residuals are used.

threats to identification: staggered rollout of treatment; heterogeneous treatment effects; non-parallel trends; multiple treatments; continuous treatment. The DiD literature has exploded in recent years (Callaway, Goodman-Bacon, and Sant'Anna 2024; Borusyak, Jaravel, and Spiess 2024; Chaisemartin and D'Haultfoeuille 2020; Sun and Abraham 2021; Callaway and Pedro H.C. Sant'Anna 2021; Goodman-Bacon 2021), with the aim of highlighting these issues and carefully describing the relevant assumptions in different contexts. Two key issues with a simple TWFE estimator is that researchers may be using 'bad controls' or averaging treatment effects with negative weights.

Our context features a continuous treatment which describes the exposure of postcode districts to ULEZ via the quasi-fixed pre-committed economic decisions of residents - their home and work locations, and commuting choice. The treatment has 'no anticipation' because prior to the initial announcement date in the first quarter of 2015, there had only been a consultation on the ULEZ (just one quarter prior). The policy had no public presence prior to this. We provide plots of Google Trends Web Search results for 'Ultra Low Emission Zone London' and 'ULEZ London' as supporting evidence.

We do not have staggered rollout, as all units are treated at the time of the policy announcement. However we do have multiple treatments, due to the ULEZ expansions which lead postcode districts to become more heavily treated over time. Put differently, the share of commuters who are affected by the ULEZ changes as the taxable area expands.

Our baseline event study plots γ_t from the time-varying TWFE in equation (1). We also compute the average TWFE coefficient, where γ doesn't vary over time. Given the focus of the recent literature on binary treatments, we also consider splitting ULEZ exposure at the median to convert treatment to binary. This allows us to follow the methodologies of Chaisemartin and D'Haultfoeuille (2020), Gardner (2022), and Clarke, Pailanir, Athey, and Imbens (2023).

It is important to check the validity of parallel trends. The standard approach is to compare the outcomes of treated and untreated groups prior to the treatment date. Placebo tests are another approach, where researchers run DiD on synthetic or fake treatment units. A placebo test can be run with treatment units that are not actually treated, or with an outcome variable that the researcher thinks should be unaffected by the treatment.

5.2 Regression discontinuity design

For house prices and establishment locations we will use a regression discontinuity design (RDD) at the postcode level around the boundary of the ULEZ. This approach is suitable given the spatial variation of the policy, which creates a clear boundary separating postcodes that are affected from those that are unaffected.

The RDD exploits the quasi-random assignment of treatment status to postcodes near the ULEZ boundary. It seems reasonable that postcodes on either side of the boundary are likely similar across both observables and unobservables that affect our outcomes of interest: house prices and firm locations.

$$\begin{aligned}\text{Outcome}_{it} = {} & \alpha_i + \alpha_t + \beta\text{ULEZ}_i + \delta\text{DistanceToULEZ}_i \\ & + \eta\text{ULEZintroduced}_t + \theta\text{ULEZ}_i \times \text{ULEZintroduced}_t + \varepsilon_{it}\end{aligned} \tag{2}$$

where ULEZintroduced_t is a binary indicator for dates before/after the introduction of the ULEZ, and DistanceToULEZ_i is the number of miles from a postcode to the ULEZ boundary.

The coefficient of interest is θ, which targets the average treatment effect of the policy announcement. This provides a local elasticity around the boundary. We include postcode fixed effects to control for time-invariant characteristics (such as attractiveness of a neighbourhood to live or work), while time fixed effects account for common trends. The running variable is distance to the boundary.

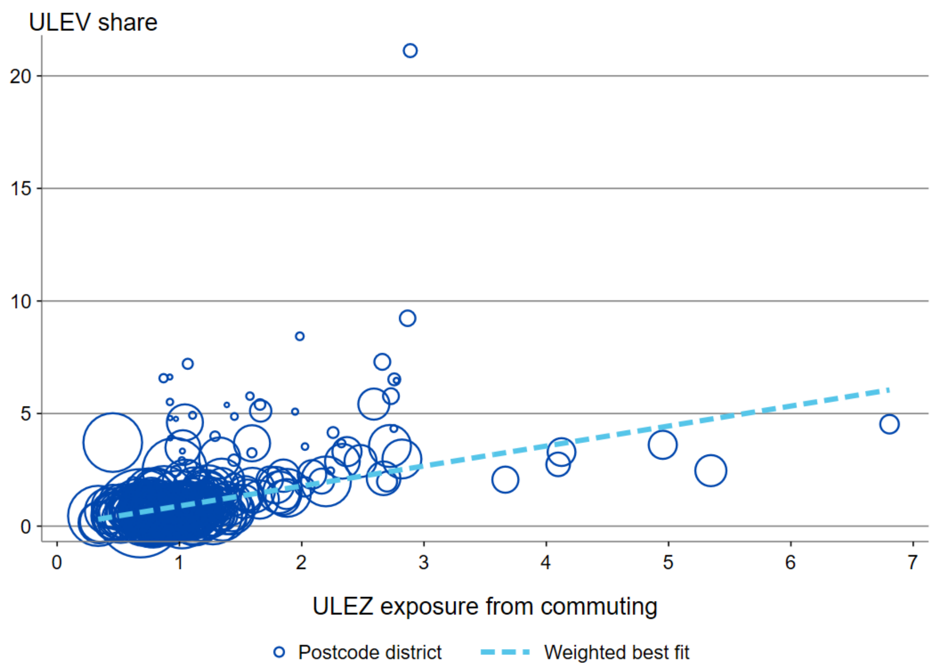
6 Results

This section discusses our main results. We cover, in order, the following margins: ULEV adoption, housing prices, public transport use and firm creation. We then examine spatial heterogeneity across these margins, and present robustness checks.

6.1 ULEV adoption

Our initial results focus only on the announcement of the first ULEZ expansion. We cut off the data at the end of 2019 to side-step the impact of the pandemic. This data is included for analysis of further expansions. Figure 5 presents a scatter of the share of ULEVs in a postcode district and the exposure to the initial ULEZ expansion. The relationship is positive and statistically significant; areas more-exposed to the zone based on pre-existing commuting choices in 2011 have a higher share of low-emission vehicle registrations in the second quarter of 2019.

Figure 5: Relationship between ULEV adoption and ULEZ exposure, 2019 Q2.



A difference-in-differences regression of the ultra-low vehicle adoption share on the interaction of the 'treatment' (exposure to the ULEZ) and a post-policy indicator is the simplest way to see if the policy introduction differentially affected adoption of electric vehicles. Table 3 shows the results from this regression, with three different weighting specifications. In all regressions, we find a large and statistically significant relationship on the interaction term. This provides evidence that postcode districts more exposed to the zone through driving commuting behaviour responded more to the policy introduction by adopting ULEVs at a higher rate.

Table 3: Difference-in-differences regression of ULEV share on ULEZ exposure interacted with a post-policy dummy

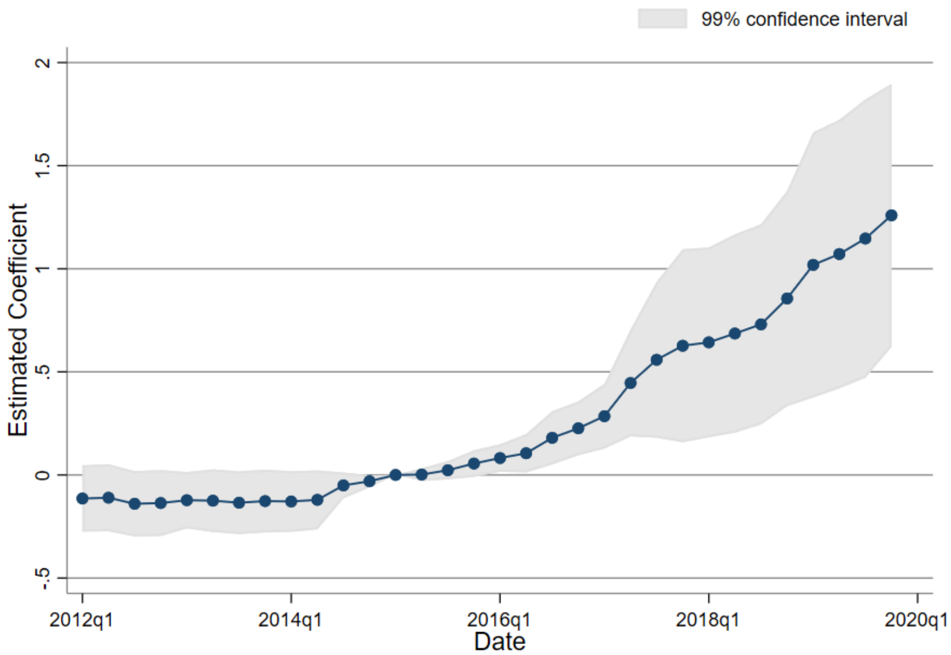
	(1)	(2)	(3)
	Dependent variable: ULEV share		
ULEZ exposure	0.107***	0.096***	0.094***
	(0.028)	(0.017)	(0.019)
ULEZ exposure × Post-indicator	0.608***	0.396***	0.383***
	(0.136)	(0.059)	(0.049)
Weight	None	Vehicles	Population
N	9,326	9,326	9,326
R^2	0.245	0.424	0.554

Standard errors in parentheses, clustered at the postcode district level.
* $p < 0.1$, * $p < 0.05$, *** $p < 0.01$. Control for population density, size of postcode district, number of commuters, population-adjusted ULEZ measure and year-quarter fixed effects.

Figure 6 shows time-varying coefficients from a differences-in-differences event study of ULEV adoption on ULEZ exposure, for the first ULEZ expansion announcement in Q1 2015. This specification controls for postcode district and quarter fixed effects, and whether or not an observation falls within the ULEZ itself. We cluster standard errors at the postcode district level.

These results suggest an average 1.3% rise in the share of electric vehicles by the end of 2019, for each 1% increase in the share of affected commuters in a postcode district. To put this into context, less than 2% of vehicle registrations in London at the end of 2019 were electric vehicles, and the average ULEZ exposure in London postcode districts is 1.2%. Alternative estimators from the recent literature Chaisemartin and D'Haultfoeuille (2020), Athey, Bayati, Doudchenko, Imbens, and Khosravi (2021), and Clarke, Pailanir, Athey, and Imbens (2023) are included in the Appendix, and show quantitatively and qualitatively similar results.

Figure 6: Baseline regression coefficients on ULEV adoption around first ULEZ announcement (Q1 2015)



6.2 House prices

Figure 7 shows a positive relationship between the average house price and exposure to the ULEZ across postcode district in London. However, we hypothesise that any effect on house prices is best identified by comparing prices on either side of the zone boundary, before and after the policy is announced. House prices are a channel through which the effects of the tax may be soaked up; residents inside the zone benefit from (1) not having to pay the tax and (2) reduced congestion due to the tax.

A simple RDD plot indicates a discontinuity in house prices *only after the policy is announced*. Figure 8 plots the average log house price at 0.005 mile intervals within 1 mile of the ULEZ boundary, both before and after the ULEZ is announced. We fit quadratic regression lines on either side of the boundary both before and after the policy announcement. The same plot with linear best fit is in Figure D13 in the appendix. While there is some evidence of a discontinuity in house prices around the border beforehand, there is a much larger gap in the regression lines either side of the boundary after the policy announcement.

Figure 7: Positive relationship between log house price and ULEZ exposure by postcode districts in London

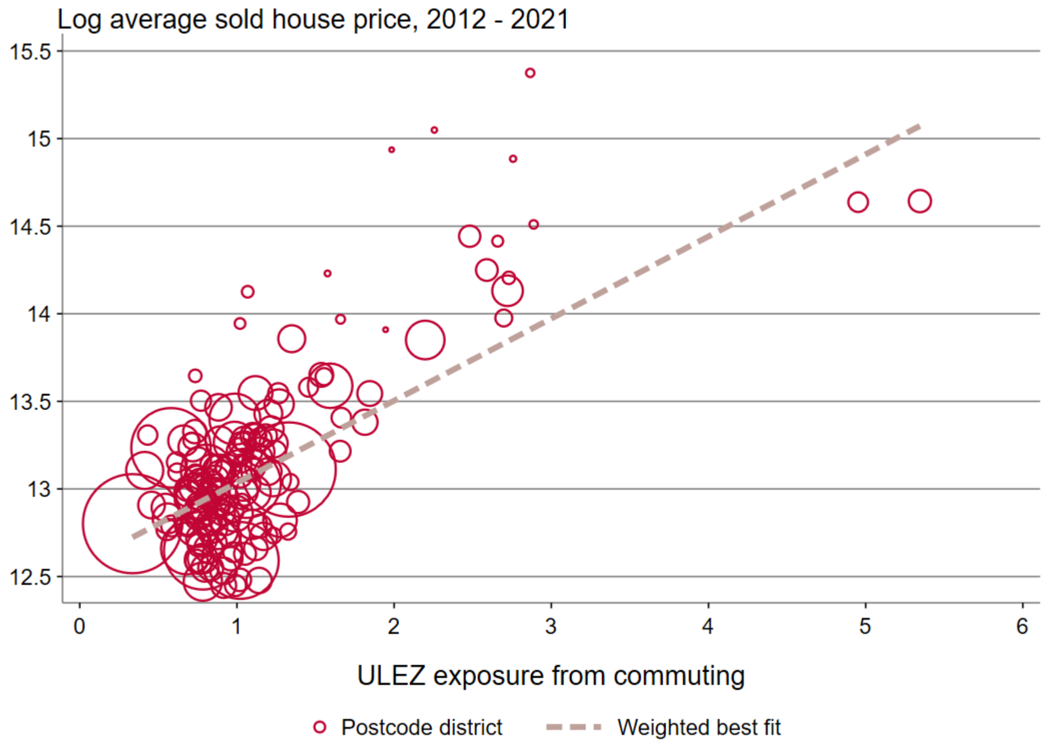
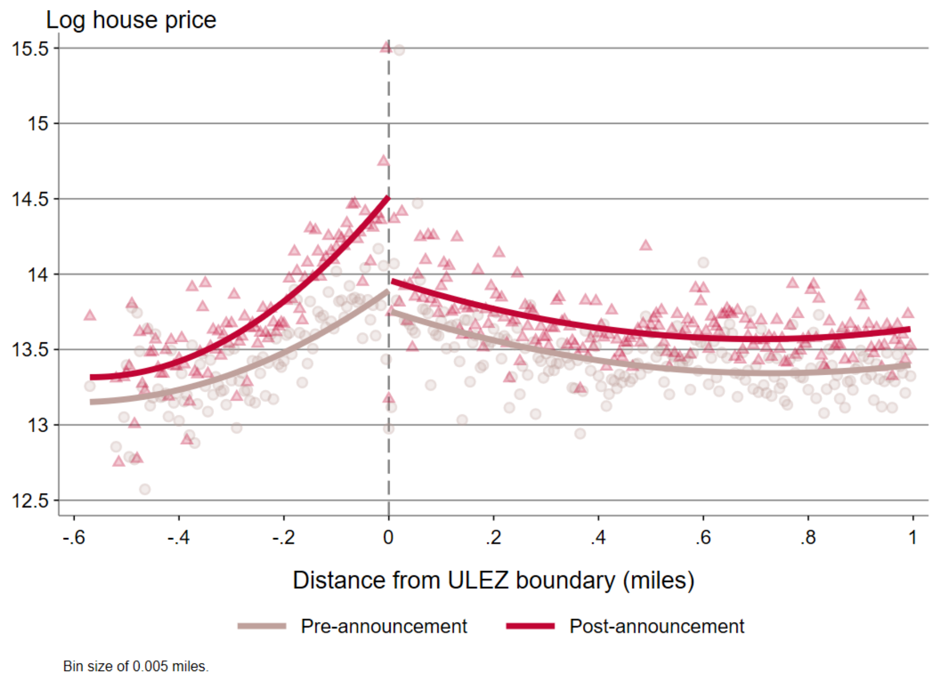


Figure 8: Binned average log house prices within 1 mile of the 2019 ULEZ boundary, before and after the policy announcement in March 2015.



To capture the effect of the introduction of the ULEZ on postcode-level house prices, we follow the RDD approach. We present three preliminary sets of results here. Firstly, we regress log house prices on the running variable – distance from the ULEZ boundary – alongside a binary ULEZ indicator, a binary post-policy indicator and their interaction, alongside year, quarter and 4-digit postcode fixed effects. Standard errors are clustered at the postcode level. We weight with a triangular kernel weight with a 2-mile bandwidth. We also consider focusing on just 1-mile from the boundary. The results are presented in Table 4.

Focusing on postcodes on either side of the 2019 ULEZ boundary (in columns (2) - (4)), house prices within the ULEZ are significantly lower on average, but around 12% higher after the policy was introduced.

Table 4: Baseline house price RDD for 2019 ULEZ boundary.

	(1)	(2)	(3)	(4)
	Dependent variable: Log house price			
ULEZ	0.104***	-0.110***	-0.170***	-0.161***
	(0.023)	(0.032)	(0.039)	(0.040)
ULEZ × Post-indicator	0.054***	0.118***	0.121***	0.123***
	(0.013)	(0.015)	(0.015)	(0.016)
Distance	-0.042***	-0.140***	-0.258***	-0.242***
	(0.005)	(0.032)	(0.053)	(0.054)
Fixed Effects	Yes	Yes	Yes	Yes
Triangular kernel weight (2 mile)	No	Yes	No	Yes
Within 1 mile	No	No	Yes	Yes
N	629,217	130,607	60,797	60,797
R^2	0.384	0.356	0.354	0.344

Standard errors in parentheses, clustered at the postcode level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Fixed effects are year-quarter and 4-digit postcode.

Secondly, we run the baseline regression (log house price on distance from the boundary and an indicator for being inside the zone) in the pre- and post-announcement periods separately. This allows for more flexibility; for example, it allows the coefficient on 'distance' to vary over time. However, it makes statistical inference slightly more complicated, as we require bootstrapping to obtain standard errors. For comparison with Table 4, weighting is a triangular kernel weight and the fixed effects are the same (year, quarter, 4-digit postcode). The coefficient on the ULEZ indicator is negative in both periods, but it is 11.8% higher after the policy introduction. This result is consistent with our findings in Table 4. The distribution of 2,500 bootstrapped estimates is plotted in Figure D19 in the appendix, with a bootstrapped standard error of 0.046 implying a statistically significant positive effect of the policy announcement on house prices inside the zone.

Overall, our results suggest that house prices within the ULEZ rose significantly in response to the policy introduction. Two possible explanations for this include the tax exemption for residents within the boundary and the expectation of reduced congestion/pollution due to the policy.

6.3 Public transport

Figure 9 plots the change in average station entry after the initial ULEZ introduction in 2019 for postcodes with above and below median exposure to the ULEZ. On average, there is a small rise in the use of public transport after the policy is introduced, and this rise is greater in areas more exposed to the policy. This suggests that areas more exposed to the policy experienced greater substitution towards public transport. However, the differences are not statistically significant.

This finding is replicated in a more robust differences-in-differences regression in Table 5. We regress the number of station entries at each postcode × day on ULEZ exposure based on the share of commuters who drive into the ULEZ, a post-policy indicator and their interaction. The interaction is statistically significant even when we include a dummy for being inside the ULEZ and the distance to the ULEZ. This suggests that postcodes more exposed to the policy saw increased use of public transport after the policy was introduced.

Another way to see this result is to visualise the predicted interaction effects in Figure D15. This graph shows the predicted station entry from the model in column (1) of Table 5. We can see the slightly higher impact of ULEZ exposure on station entry after the policy is introduced. The difference looks small, but for highly-exposed post-

Figure 9: Average change in number of London underground entry after the April 2019 ULEZ introduction, split by postcodes with above and below median ULEZ exposure.

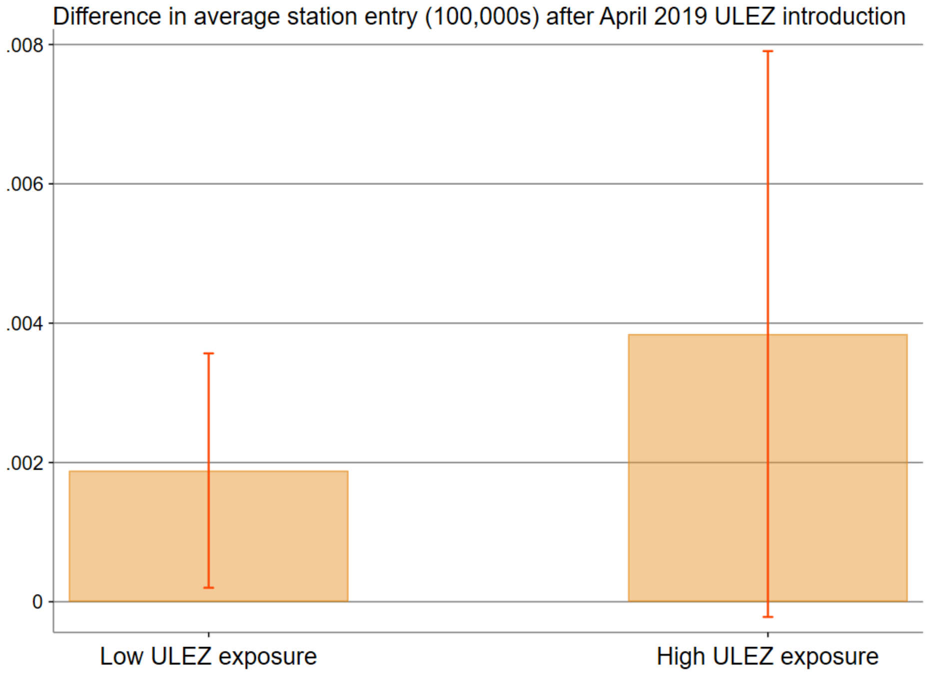


Table 5: Difference-in-differences regression of station entry/exit on ULEZ exposure interacted with a post-policy dummy

	(1)	(2)	(3)	(4)
	Dependent variable: Station entry/exit (100,000s)			
ULEZ exposure × Post-indicator	0.242***	0.213*	0.220***	0.208***
	(0.069)	(0.069)	(0.078)	(0.078)
ULEZ exposure	3.834***	1.869*	3.982***	1.893
	(1.216)	(1.121)	(1.269)	(1.158)
Post-indicator	0.000	0.001	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
ULEZ	0.205***	0.058***	0.208***	0.158***
	(0.056)	(0.056)	(0.055)	(0.056)
Distance to ULEZ		−0.012***		−0.012***
		(0.002)		(0.002)
Dependent variable	Entry	Entry	Exit	Exit
N	135,683	134,955	135,683	134,955
R^2	0.149	0.193	0.142	0.188

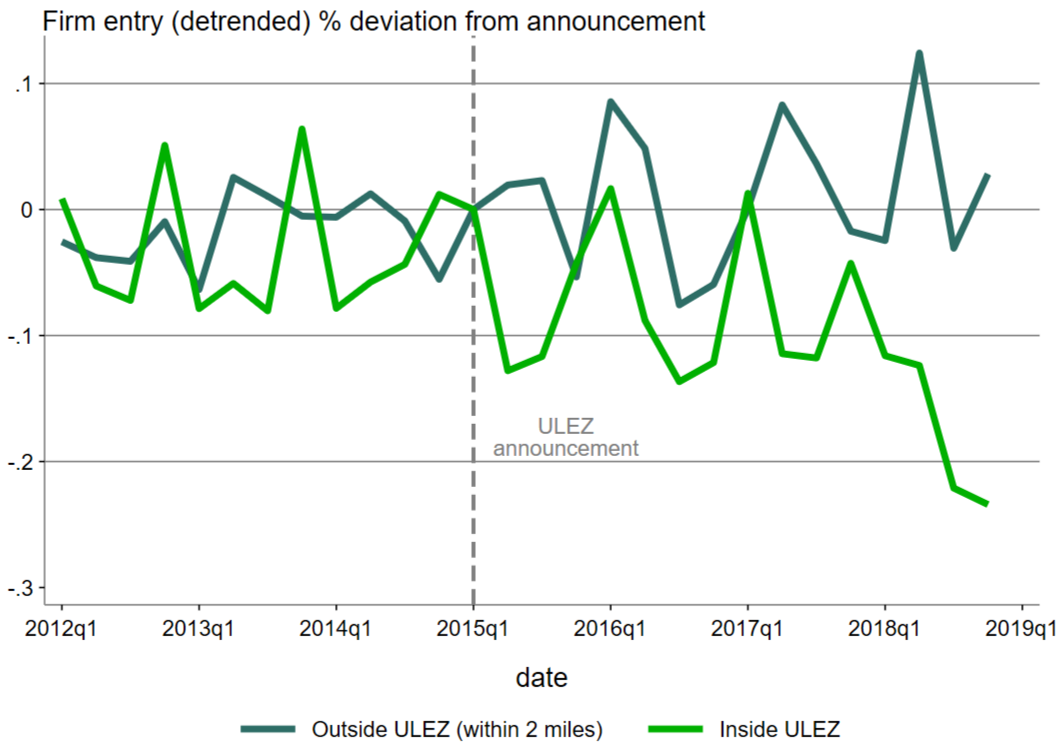
Standard errors in parentheses, clustered at the postcode level. * $p < 0.1$, * $p < 0.05$, *** $p < 0.01$.
Fixed effects are day, week, month, day of week, week of month.

codes, this amounts to over 1,500 additional station entries per day, which is around a 0.05% rise in London underground use.

6.4 Firm entry

Figure 10 plots detrended[10] firm entry rates averaged over postcode districts (weighted by number of firms), over time, for those within the ULEZ and those outside but within 2 miles. We normalise to the start of 2015 to measure deviations from the announcement date. This highlights different trends in firm entry inside and outside the ULEZ, with divergence beginning after the policy announcement, but accelerating a few years later.

Figure 10: Diverging firm entry rates inside vs outside the ULEZ

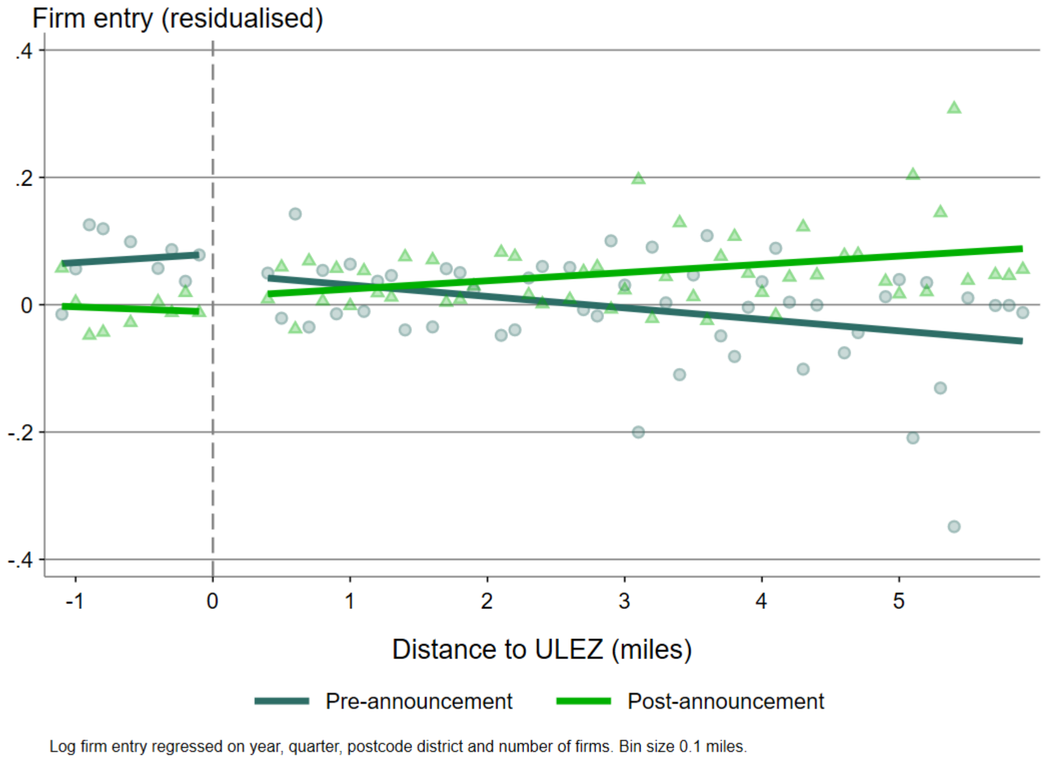


Focusing around the boundary, we conduct a simple RDD before and after the policy announcement. We see a drop in firm entry within the ULEZ (controlling for time, postcode district and number of firms as a proxy for economic activity) and a

[10]Postcode district firm entry rates are regressed on year and quarter fixed effects, and residuals are used for subsequent analysis.

rise in firm entry outside the zone.

Figure 11: Regression discontinuity plot around ULEZ boundary, before and after policy announcement



A more formal RDD treatment is presented in Table 6. We regress log firm entry on an indicator for the ULEZ, an indicator pre/post announcement and their interaction, along with the running variable which is the distance from the boundary. We include year, quarter and postcode letter[11] fixed effects. The six columns of Table 6 vary the specification in two ways: the weighting and the exclusion of certain regions. However, they consistently find that firm entry fell for postcode districts inside the zone *after* the policy was announced. Figure 11 in the appendix shows the regression discontinuity visually.

6.5 Spatial heterogeneity

ULEV adoption. We investigate heterogeneity by region, separating London into four sections based on the first letter of their postcode: "E", "N", "S" and "W". Ta-

[11] This is either 1 or 2 letters at the start of a full postcode.

Table 6: Baseline firm entry RDD for 2019 ULEZ boundary.

	(1)	(2)	(3)	(4)	(5)	(6)
			Dependent variable: Log firm entry			
ULEZ	0.390**	0.465**	0.209	0.280	0.166	0.239
	(0.195)	(0.195)	(0.173)	(0.180)	(0.187)	(0.197)
ULEZ × Post-indicator	-0.023	-0.056	-0.152***	-0.142***	-0.142**	-0.127*
	(0.053)	(0.054)	(0.048)	(0.050)	(0.066)	(0.071)
Weight	# firms	# firms	Triangular	Triangular	Combined	Combined
Distance to ULEZ	All London	<5 mi.	All London	<5 mi.	All London	<5 mi.
N	8,098	3,833	6,058	3,833	6,058	3,833
R^2	0.472	0.407	0.370	0.364	0.332	0.324

Standard errors in parentheses, clustered at the postcode district level.
* p<0.1, ** p<0.05, *** p<0.01. Fixed effects are year, quarter, postcode letter.

ble E9 in the appendix presents this heterogeneity analysis separately for each region. ULEV adoption is higher in the North, South and West when ULEZ exposure is greater, and substantially more so after the policy is announced. The effect in North London is the largest in magnitude.

We explore greater regional heterogeneity, separating *Greater London* into six districts: North central, South central, North-West Greater, North-East Greater, South-West Greater, South-East Greater. The map of these regions is in Figure D8 in the Appendix. We repeat our baseline differences-in-differences regression, interacting the treatment (ULEZ exposure) with a post-announcement indicator and a region dummy. Figure D24 plots the estimated coefficients from this triple interaction for each area, relative to the omitted region North-East Greater London.

The results in Figure D24 suggest the overall positive effect on ULEV adoption is driven by the regions we label as North and South London, but is typically known as the 'London postal area',[12] covering 620 square kilometres. The policy-induced rise in ULEV adoption is driven more by North London and there is a slight offsetting effect from South-East Greater London.

House prices. We present heterogeneity analysis in Table E10 in the appendix. We re-run the baseline RDD separately for postcodes beginning with "E", "N", "S" and

[12] Postcode areas: N, NW, SE, SW, W, WC, E, EC. For more information: Wikipedia.

"W", representing the four cardinal directions in central London. The narrower geographical range for this analysis is due to the methodological approach which focuses on outcomes either side of the policy boundary.

There are large and statistically significant increases in house prices inside the zone (relative to outside) after the policy announcement for all regions *except for North London*. Interestingly, this is contrast to the response in ULEV adoption, which was largest for postcodes in North London.

Public transport. Table E11 in the appendix documents regional heterogeneity in public transport entry in response to the initial introduction of the ULEZ. Once again we separate London into four sections based on the first letter of their postcode: "E", "N", "S" and "W". There is a statistically significant and positive effect on use of the tube in the West and South of London. However, the effect is negative in North London.

Firm entry. Results for regional heterogeneity in firm entry in response to the initial ULEZ announcement is presented in Table E12. The overall discontinuous decrease in firm entry is driven by East London. The effect for South London is of a similar magnitude, but not statistically significant. North and West London do not show any response on this margin.

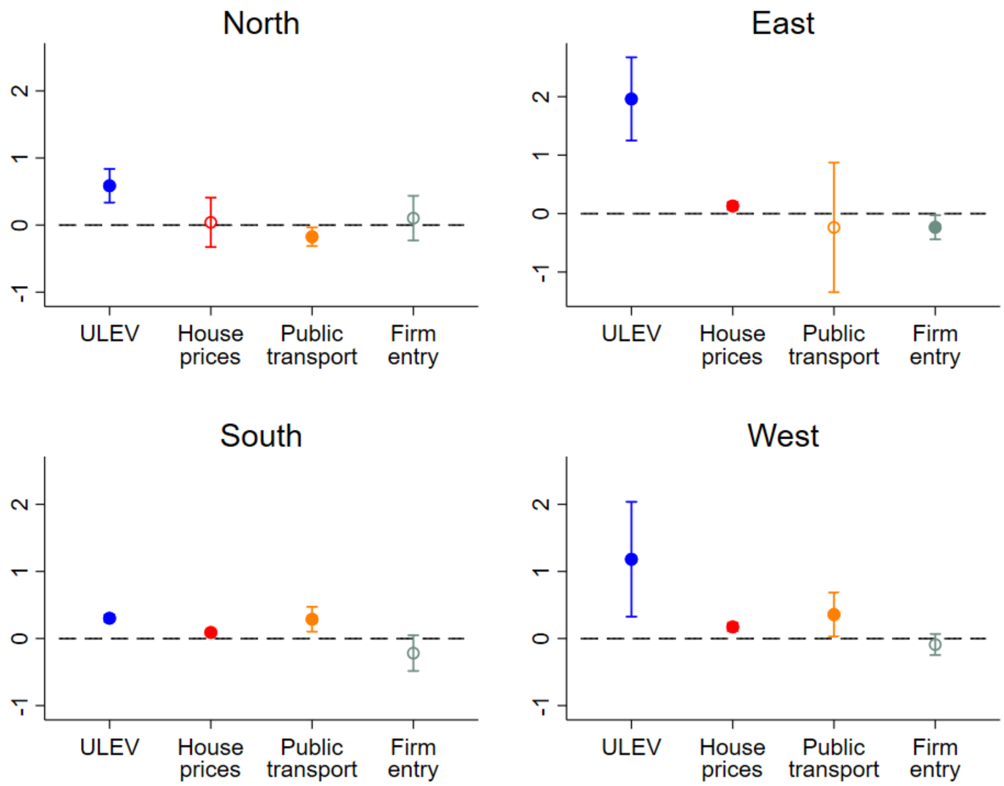
Figure 12 shows estimated elasticities across all four margins, on average and for each of the four quadrants of London. Each coefficient comes from our baseline DiD or RDD specification on a given margin from our main analysis.[13]

6.6 Robustness checks

ULEV adoption. For robustness, we run several parallel trend and placebo tests. The first runs the classic TWFE time-varying estimator on the pre-announcement data,

[13] This means that (1) the ULEV coefficient comes from an unweighted DiD regression with covariates; (2) house prices are logged, unweighted and from an RDD within one mile of the boundary; (3) public transport reports entry, unweighted with all date-related fixed effects; and (4) firm entry is weighted with a triangular distance to the boundary.

Figure 12: Estimated elasticities to the initial ULEZ announcement, by London region. Unfilled points indicate the estimate is not statistically significant at the 95% level.



with a fake treatment date in 2013 Q2. It shows no evidence of pre trends.

We implement two other placebo tests. The first randomly assigns treatment data over all units, and then re-runs the baseline event study. The second uses an outcome variable we suggest should be unrelated to the treatment; the number of total vehicles per capita. Both placebo tests show no effect, so they provide supportive evidence that there is a real effect of the ULEZ announcement on ULEV adoption.

We repeat this analysis for the ULEZ expansion announcement (Q2 2016) and find broadly similar results. Figure D18 in the appendix contains the baseline event study.

Following the recent consensus in the literature, we test for pre-trends by running an event study regression on pre-treatment periods only (Callaway and Pedro H.C. Sant'Anna 2021; Roth, Pedro HC Sant'Anna, Bilinski, and Poe 2023). The result is plotted in Figure D22, providing supporting evidence for the validity of our empirical approach. We cannot reject the null hypothesis of parallel pre-trends.

Chaisemartin and D'Haultfoeuille (2020) show that the TWFE estimator is a weighted sum of treatment effects, and the weights may be negative when heterogeneous treatment effects exist. We implement their robust estimator, and make ULEZ exposure binary with a cut-off at the median value. The results are shown in Figure D9. Given the binary treatment, the estimated coefficients are smaller in size, but the interpretation of the magnitude of the effects is very close.

We also implement the Clarke, Pailanir, Athey, and Imbens (2023) synthetic DiD estimator, which combines the synthetic control and DiD approaches. It leverages the insights of synthetic control to ensure trends are parallel pre-treatment, by re-weighting control units accordingly. Once again we are restricted to binary treatment only, but the ATT is estimated at 0.005 with a standard error of 0.001 off 50 bootstrap replications. This is approximately in line with a weighted average that might be expected from Figure D9.

We implement the matrix completion approach of Athey, Bayati, Doudchenko, Imbens, and Khosravi (2021), which is a method to impute the missing counterfactuals due to treatment assignment. We implement this method with six 'placebo' pre-treatment periods in Figure D10. There's no evidence of pre-trends and the estimated time-varying ATTs are very much in line with our estimates from other methods.

House prices. We repeat our baseline RDD for a range of distances to the ULEZ boundary. Figure D23 plots the estimated coefficients and 95% confidence intervals as we widen the distance from the ULEZ boundary. The effect of the policy is approximately a 12% higher house price for postcodes within the ULEZ, and this falls as we compare to house prices further from the boundary.

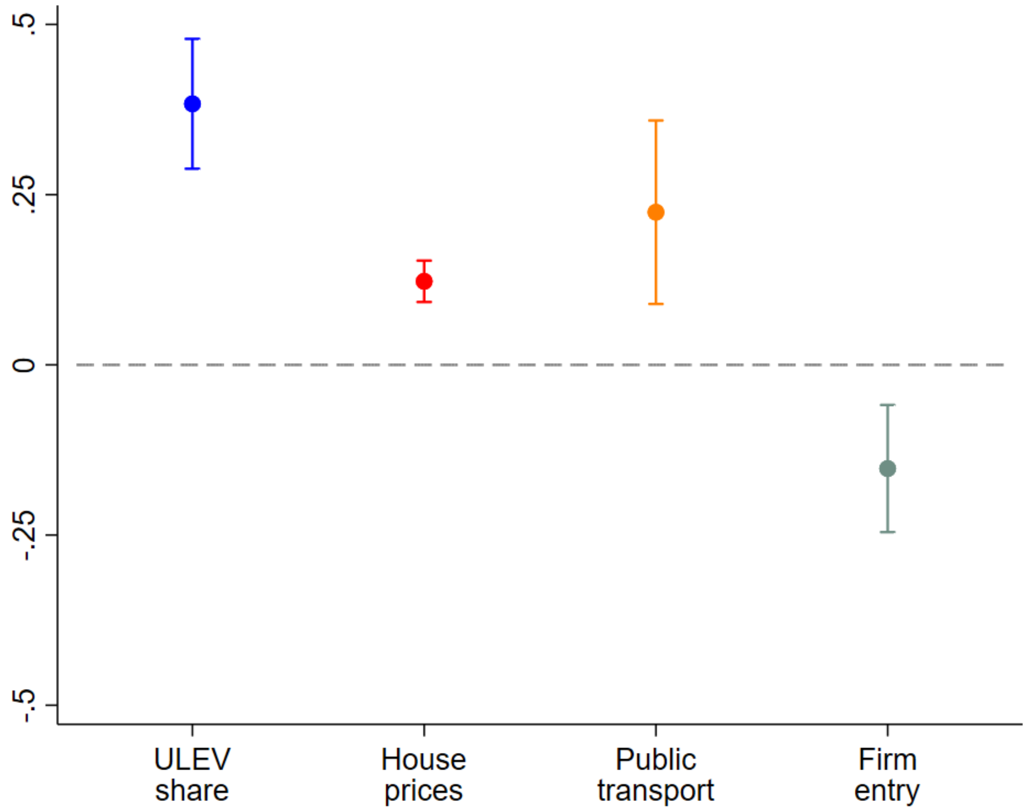
Further robustness checks are included in the appendix. Tables E1 to E6 adjust our baseline RDD to allow for: asymmetry of distance inside vs outside the zone; different triangular weightings; allowing distance to interact with the post-announcement indicator; a 'triple-interaction' between the treatment, the post-announcement indicator and distance; adjusting the functional form on distance; a 'donut' RDD (excluding the 0.05 miles on either side of the boundary). Each specification still returns a large positive and statistically significant estimate on the relationship between house prices and the interaction between being inside the zone after the policy announcement. Finally, Table E7 presents the null results from a placebo test. We chose a fake zone in London randomly, computed the distances to this boundary, and re-ran the baseline analysis.

7 Discussion

This section discusses relative magnitudes across margins, quadrants of London and in relation to other studies. Our baseline estimates are plotted in Figure 13. On average, the elasticities are largest for ULEV investment, but they are large and statistically significant across the other margins too. Affected commuters unsurprisingly find it easier to purchase a ULEZ-compliant vehicle than to move house or employer. Although the policy works as intended – there is evidence of switching to low-emissions vehicles and public transport – there are broader consequences which are important to understand.

We investigate regional heterogeneity too. There is little geographical heterogene-

Figure 13: Estimated elasticities to the initial ULEZ announcement.



ity in ULEV investments. This stands in contrast to the other three margins. House prices are not a significant margin at all in the North, and three times as important in the West than the South. Firms relocate significantly only in the East, where public transport is not a significant margin. These geographical differences in responses suggest demographic and economic differences across the four quadrants. Understanding them remains work in progress.

Comparing restricted and unrestricted car vintages of a vintage-specific pollution zone in Santiago de Chile, Barahona, Gallego, and Montero (2020) find the ratio of cars just below the vintage threshold to those just above the threshold is three times higher in affected municipalities than in unaffected ones. Herzog (2023) develops a general-equilibrium model of commuting behaviour to examine the impact of London's Congestion charge (CC), the ULEZ predecessor. Reduced form DiD estimates suggest that the introduction of the CC decreases traffic in affected areas by about 4%. In the model road traffic was reduced by about 1%, after taking into account sorting and substitution behaviour.

Aydin and Kurschner Rauck (2023) investigate the impact of the tightening of Berlin's Low Emission Zone (LEZ). They find a sizeable increase in price premia of around 5% for houses close to train stations within a 30-minute commute of Berlin's main station. This is similar to the 2% price premium associated with proximity to public transport in Beijing after driving restrictions were imposed (Y. Xu, Zhang, and Zheng 2015). However, Aydin and Kurschner Rauck (2023) also found house price penalties for regions further from the city centre, which they attribute to the negative externalities of increased public transport usage – noise and congestion. Gruhl, Volhausen, Pestel, and Moore (2022) analyse the impact across 58 LEZs in Germany, finding a 2% increase in rents for properties within a LEZ, and a smaller impact on house prices. It is important to note that Germany, specifically Berlin, has strong restrictions on rent price growth, which likely interact with other policies such as LEZs (Thomschke 2016).

8 Conclusion

Air pollution carries high social costs, especially in urban areas. As a result, governments now increasingly experiment with policies that alter incentives to pollute. One high-profile example is London's Ultra Low Emissions Zone (ULEZ). Economists generally understand that people adapt their behaviour to incentives on many margins, often in unexpected ways (Dharmasena and Capps Jr 2012; Smith 2022; Malovaná, Bajzík, Ehrenbergerová, and Janku 2023).

In this paper, we evaluate how economic activity adapts as commuting incentives change substantially, heterogeneously and dynamically for many Londoners. We bring together timely and granular data from many sources to estimate elasticities for ultra-low emissions vehicle adoption, public transport use, house prices and the location of workplaces. To estimate the impact of the policy on behaviour, we use the time series of announcements and implementation dates alongside variation in the geographical reach of the ULEZ over time and pre-existing commuting patterns.

We show that the announcement of the first ULEZ expansion in 2015 led to a large, positive and significant increase in the adoption of ultra-low emissions vehicles. The introduction of the ULEZ in 2019 led to an increase in public transport use, for areas which were more exposed to the tax. We also find a large positive increase on house prices within the ULEZ, compared to those on the other side of the boundary. Finally we show a fall in establishment creation inside the ULEZ and a rise outside the ULEZ in response to the policy announcement. In ongoing work we explore the impact of further spatial expansions of the policy in greater detail. Beyond the backwards-looking estimation of these elasticities, the near-real time nature of most of our data sources allows us to evaluate future changes to London's ULEZ almost concurrently. In order to structure this analysis, we have publicly posted a pre-analysis plan (PAP). This provides a blueprint for monitoring policy chances in near-real time with existing data sources, as pioneered by Fetzer, Palmou, and Schneebacher (2023).

The introduction of London's ULEZ features many interesting and time-varying design choices: in geographic coverage, in the treatment of residents versus commuters and in the incentives offered for the disposal of polluting vehicles. London's size and economic importance for the UK also make it a unique laboratory for pollution pricing, and the frequent changes to policy details suggest policymakers understand the importance of getting the incentives right. We hope that the estimates obtained in this project can inform better and more timely policy design choices, in the UK and abroad.

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A Additional policy details

A.1 Scrappage and retrofit schemes

A scrappage scheme was introduced to help those on income support or disability benefit to comply with ULEZ standards. The original 2019 scheme offered up to £7,000 compensation for a car or van plus up to £2,500 if it was replaced by an electric vehicle. However, when ULEZ was expanded in 2021, the £61m scrappage scheme reduced the compensation to £2,000 for cars (and for a limited number of vans and £15,000 for heavy vehicles).

The ULEZ schemes also allows some vehicles to retrofit emissions reduction technology to meet Euro VI-equivalent levels of emissions. A vehicle with retrofitted emissions technology needs to be certified by the Government's Clean Vehicle Retrofit Accreditation Scheme (CVRAS). The CVRAS register contains approved and Clean Air Zones (CAZ) compliant companies and emission reduction systems, based on make, model and engine type. The CVRAS certifies emission technologies for black taxis, vans, minibuses, motorhomes, buses, coaches, HGVs and refuse vehicles.

Financial assistance to scrap or retrofit non-compliant vehicles in preparation for the latest expansion of the ULEZ (August 2023) was announced at the end of July 2023, and offers £2,000 for scrapping a car and £1,000 for motorcycles, and £5,000 for wheelchair accessible vehicles. Parts of the scrappage payment is converted to an annual bus and tram pass. Between £5,000 and £9,500 grant is available for scrappage or retrofit of vans and minibuses used by small businesses, sole traders, and charities. It was initially open to people on low incomes, disability benefits and child benefit as well as some businesses but was extended to all Londoners and small businesses on the 21 August 2023.

A.2 Policy details

The vehicle emissions standards are taken from the vehicle logbook data held by the Driver and Vehicle Licensing Agency (DVLA).

Cars. As petrol and diesel engines produce different types of emissions, they require different standards. The ULEZ requires cars to meet minimum 'Euro' emissions standards; the ULEZ standard for passenger cars is Euro 4 (NOx) for petrol cars and Euro 6 (NOx and PM) for diesel cars. Cars featuring older technology are less likely to meet the Euro 4 standards. Petrol cars that meet ULEZ standards are generally those that were first registered with the DVLA from January 2006 (although some cars registered as early as 2001 may also meet the standards). Diesel cars registered with he DVLA after September 2015 generally meet the ULEZ standards.

Large vans and minibuses. Euro 6 for diesel engines and Euro 4 for petrol engines. Non-compliant vehicles would be required to pay a daily charge of £12.50.

Motorcycles. Motorcycles, mopeds, motorised tricycles and quadricycles (L-category) need to meet minimum Euro 3 emissions standards for NOx. Euro 3 engines are those registered with the DVLA after July 2007.

Lorries, coaches and larger vehicles over 3.5 tonnes Gross Vehicle Weight (GVW). Heavy goods vehicles (HGVs), lorries, vans, motor caravans, motorised horseboxes, and other specialist vehicles below 3.5 tonnes.

A.3 Exemptions

- 'Historic vehicles' aged 40 years or older (if registered as historic vehicle tax class).
- Hybrid electric vehicles(HEVs), Plug-in hybrids electric vehicles (PHEVs) and fully battery-powered electric vehicles (EVs or BEVs).
- LPG (Liquefied Petroleum Gas) conversions, depending on the individual model and engine.
- London-licensed taxis.

- Specialist agricultural vehicles or other specialist vehicles (Motorised horseboxes, breakdown and recovery vehicles, snow ploughs, gritters, refuse collection vehicles, road sweepers, concrete mixers, fire engines, tippers, removal lorries, cranes).
- Military vehicles.
- Some showman's vehicles are eligible for 100% discount.
- Residents parked in the zone that do not drive.
- Buses, coaches and minibuses over 5 tonnes GVW.
- NHS patient that are clinically assessed as too ill to travel to an appointment on public transport are eligible to claim back any ULEZ charge.

ULEZ exemptions will be in place until 2025 for community transport vehicles and until 2027 for people receiving certain disability benefits and vehicles for people with disabilities.

A.4 Grace periods

Grace periods covering vehicles for disabled people are in place until 25 October 2027. Some businesses and charities also have a short grace period. Small business (50 employees), micro businesses (up to 10 employees), charities and sole traders with a registered address in London boroughs and city of London fall in this category if they ordered a new minibus or light van or retrofitted their light van or minibus and the delivery is due after 29 August 2023. There will be no exemption from the charges beyond 29 May 2024.

B ULEZ full timeline

- 27 October 2014: Mayor and TfL announce consultation on ULEZ.
- 30 December 2014: Reminder of ULEZ consultation ending soon.
- 26 March 2015: Mayor confirms ULEZ.
- 26 October 2015: Mayor and TfL finalise ULEZ requirements for taxi and minicabs.
- 17 February 2017: Mayor confirms £10 T-charge from October 23rd.
- 4 April 2017: Mayor launches consultation for replacing T-charge with ULEZ from 2019.
- 23 October 2017: T-Charge comes into effect.
- 3 November 2017: Mayor announces ULEZ will start in 2019.
- 30 November 2017: Mayor launches ULEZ expansion consultation.
- 8 June 2018: Mayor announces ULEZ to expand up to North and South Circular.
- 29 November 2018: First ULEZ signs go up in London.
- 8 March 2019: TfL reminds of ULEZ one-month countdown London ULEZ.
- 8 April 2019: ULEZ comes into force.
- 16 May 2019: TfL announces that 74 per cent of vehicles comply in first month.
- 15 May 2020: The Congestion Charge, Ultra Low Emission Zone and Low Emission Zone are reinstated.
- 6 August 2020: TfL announces installation of new infrastructure.
- 18 October 2021: TfL urges drivers to check their vehicle ahead of Ultra Low Emission Zone expansion on 25 October.

- 20 May 2022: TfL seeks views on expanding ULEZ.
- 25 November 2022: Mayor announces that ULEZ will be expanded London-wide.
- 30 January 2023: Mayor announces the scrappage scheme.
- 23 March 2023: Tfl data shows over 90% of cars driving in outer London already meet ULEZ standards.
- 21 April 2023: Tfl announces £18m allocated from scrappage scheme ahead of ULEZ expansion.
- 28 July 2023: High Court rules in favour of ULEZ expansion.
- 4 August 2023: Mayor announces expansion of scrappage scheme to all Londoners.
- 23 August 2023: Scrappage scheme becomes open to all Londoners.
- 29 August 2023: ULEZ expands London-wide.

C Data appendix

Table C1: Summary statistics - vehicles (London postcode districts only)

	Mean	N	StDev	Min	p(25)	p(50)	p(75)	Max
All Vehicles	12,587	11,645	8,259	49	7,325	12,007	17,648	66,267
ULE Vehicles	80.77	11,645	179.41	0	6	27	94	7531
Population	29,317	11,645	21,407	0.065	15,284	27,734	40,653	140,711
ULEZ	0.081	11,645	0.22	0	0	0	0	0.91
Taxable ULEZ Share	0.012	11,645	0.0075	0.0034	0.0078	0.0098	0.013	0.068
Share ULEVs	0.010	11,645	0.022	0	0.00056	0.0030	0.010	0.734
Vehicles per capita	48.25	11,645	731.53	0.039	0.34	0.50	0.68	13,649.70
ULEVs per capita	2.37	11,645	46.70	0	0.00023	0.0014	0.0052	2,013.64

Vehicle data from VEH0122 and VEH0134 from the DVLA. Commuting and population data from 2011 Census. Constructed variables computed by authors.

Table C2: Summary statistics - house prices (within 1 mile of ULEZ boundary only)

	Mean	N	StDev	Min	p(25)	p(50)	p(75)	Max
Log house price	13.67	60808	0.96	7.81	13.07	13.46	14.05	20.20
Distance to boundary	0.33	60808	0.41	-0.82	-0.06	0.35	0.70	1.00

For postcodes within 1 mile of ULEZ boundary. House price data from Price Paid Data (PPD). Distance to boundary computed by authors.

D Additional figures

Figure D1: ULEZ exposure by postcode district

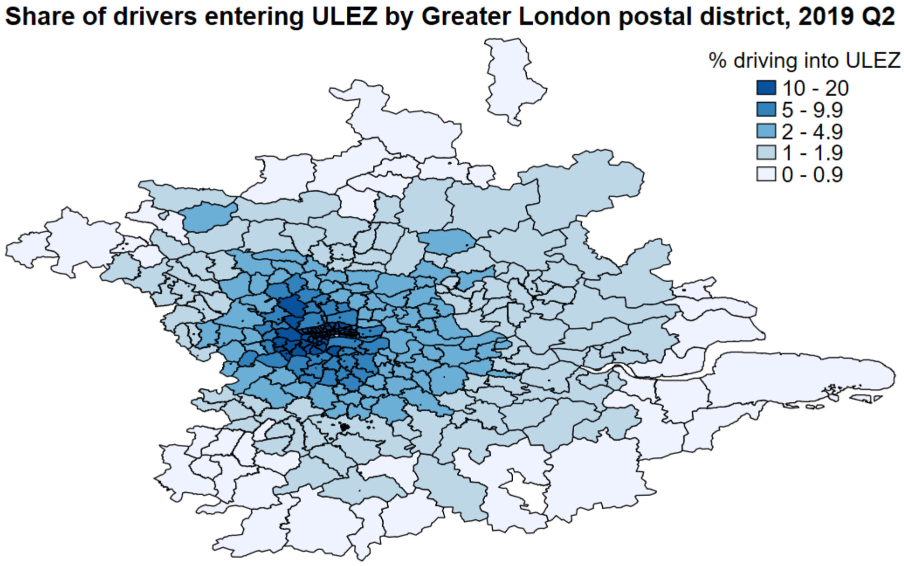


Figure D2: Adoption of ultra-low emissions vehicles in high and low ULEZ exposure postcode districts for the first expansion

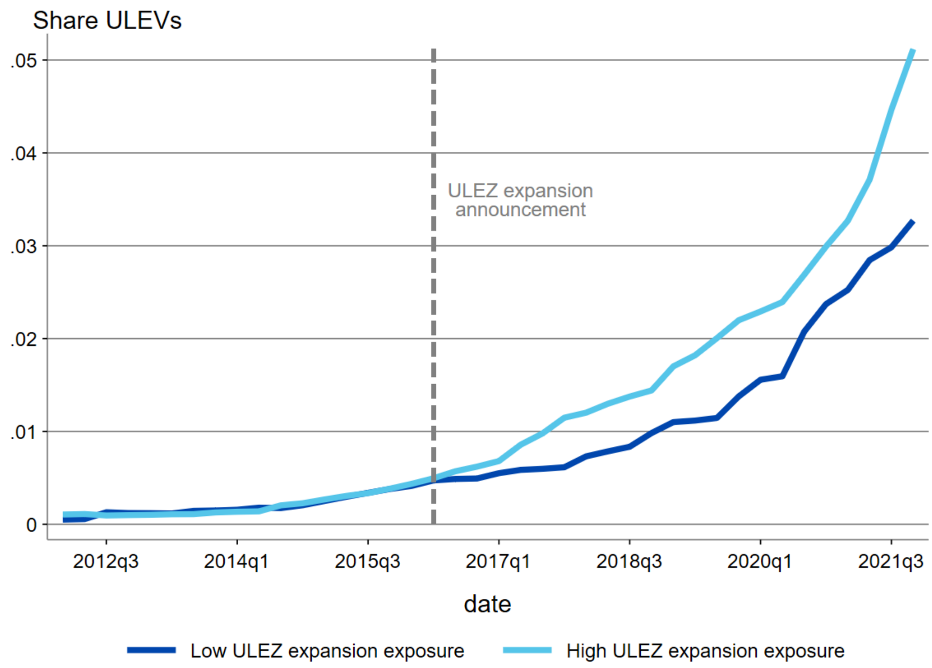


Figure D3: Histogram of computed ULEZ exposures for London's postcode districts

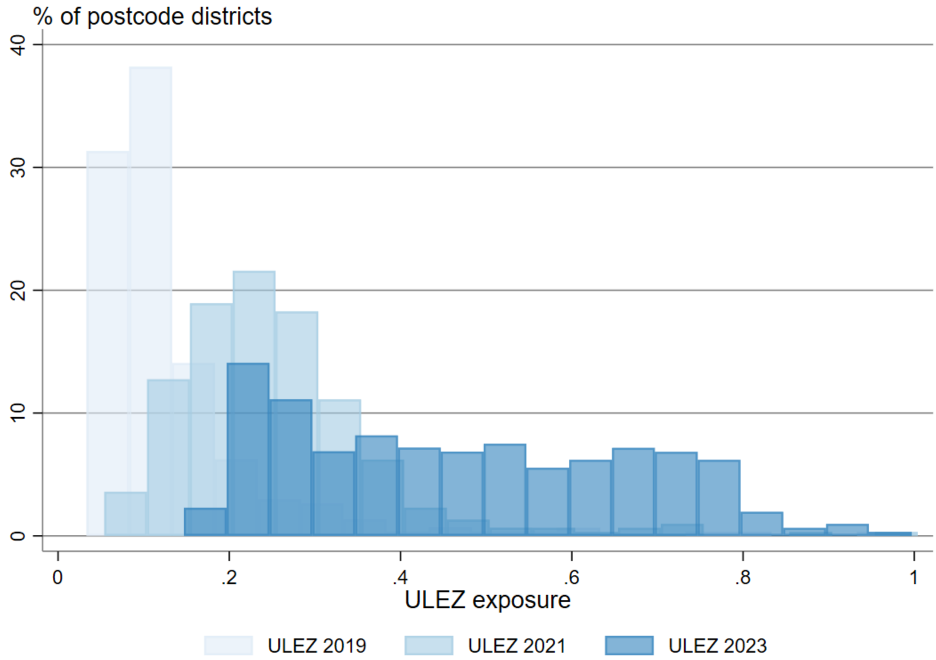


Figure D4: ULEV adoption by postcode district

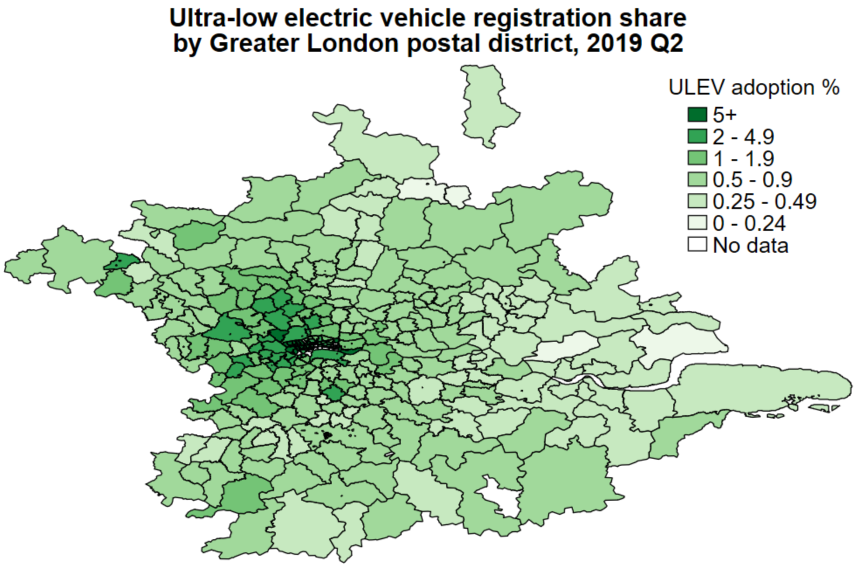


Figure D5: Google Trends web search for ULEZ London

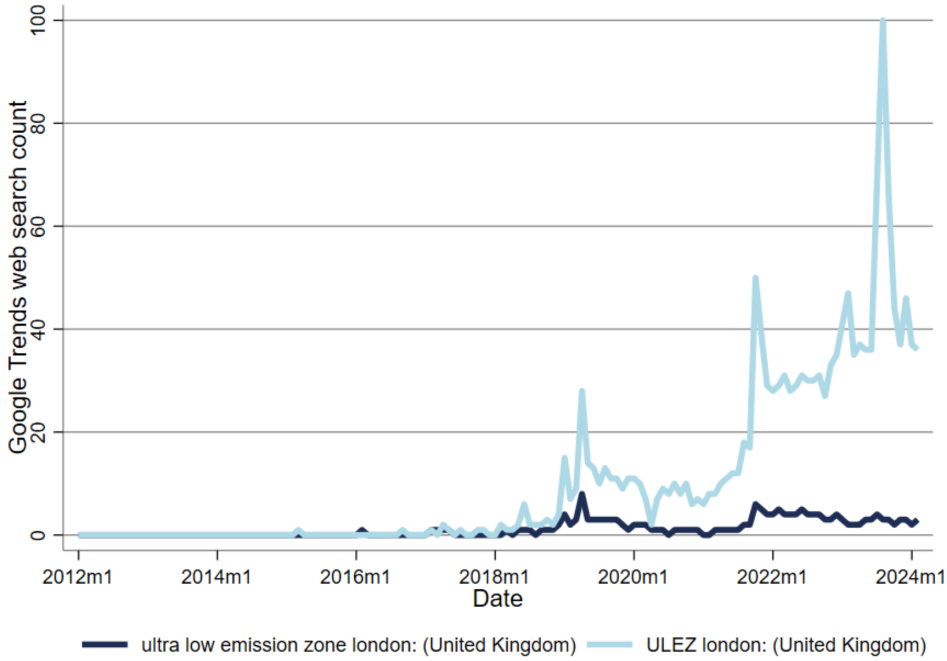


Figure D6: Adoption of ultra-low emissions vehicles in London

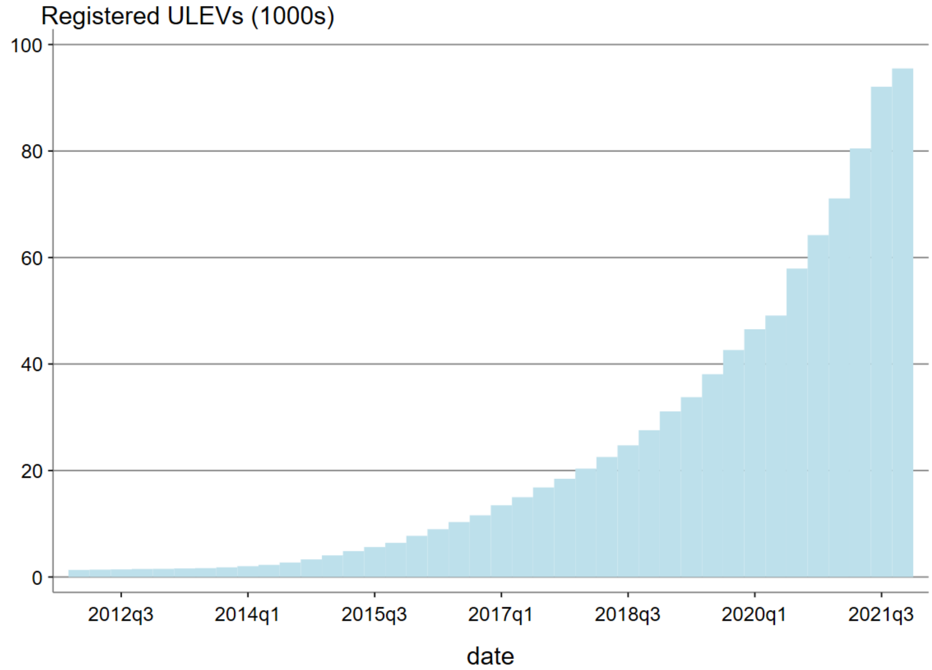


Figure D7: Adoption of ultra-low emissions vehicles in London

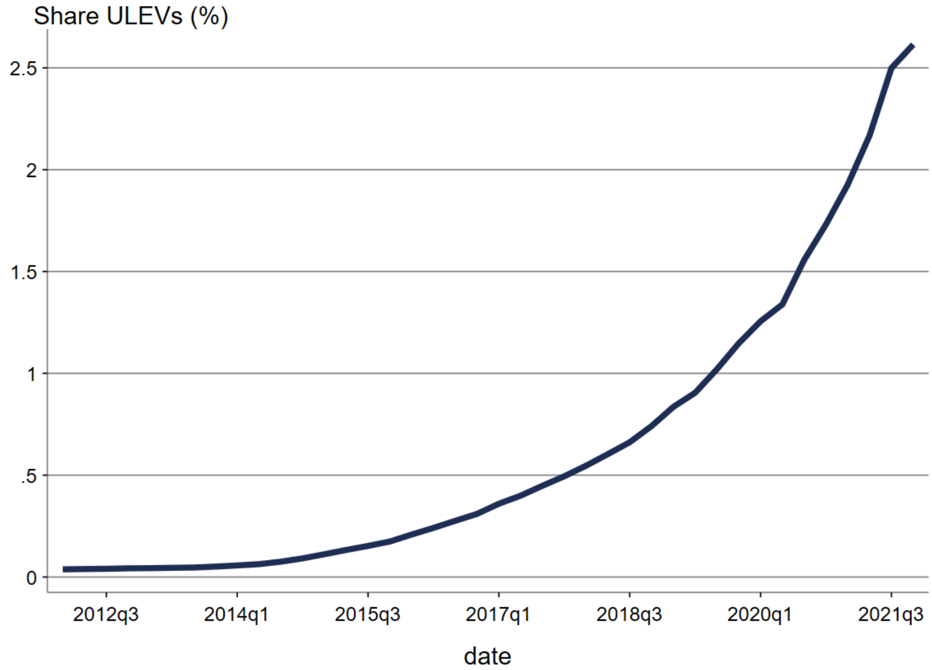


Figure D8: Six areas of London for heterogeneity analysis.

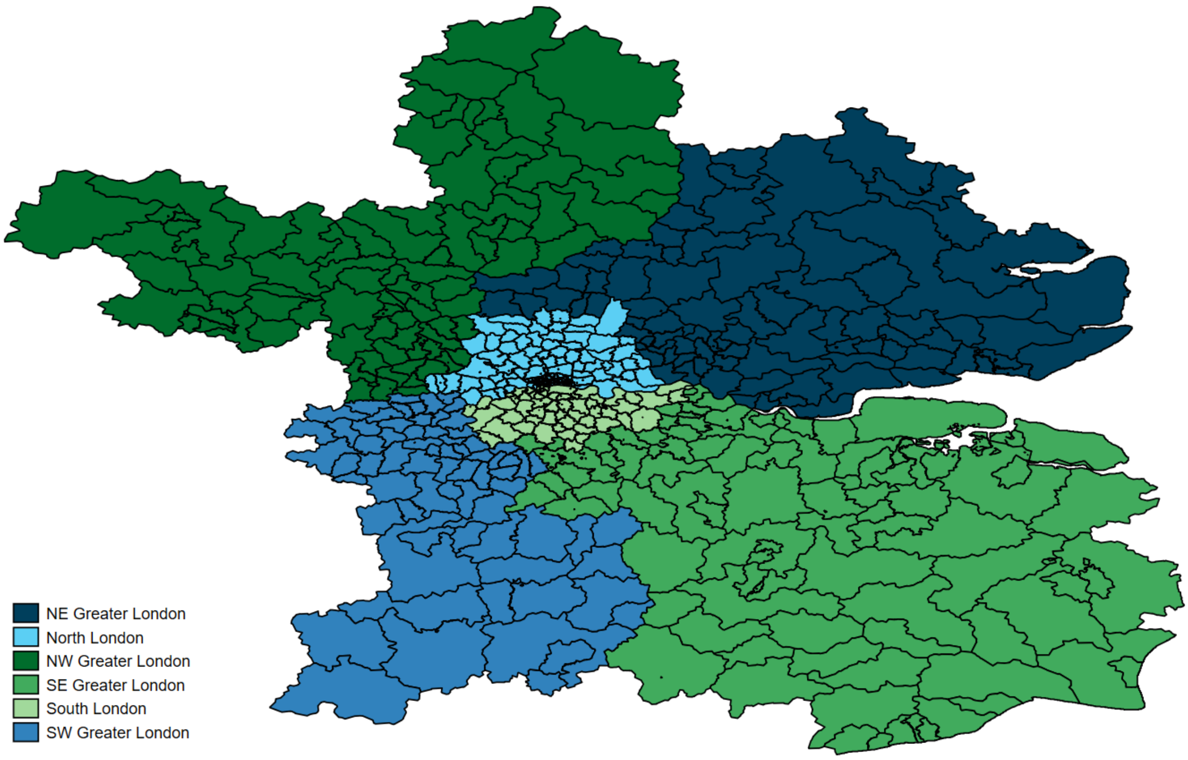


Figure D9: Chaisemartin and D'Haultfoeuille 2020 event study for ULEV adoption around first ULEZ announcement (Q1 2015)

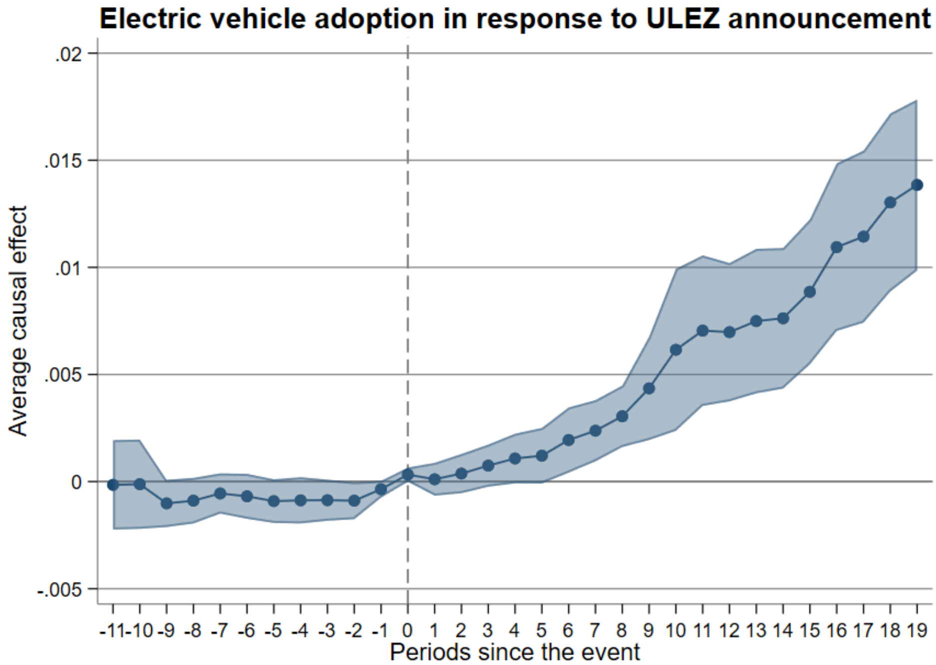


Figure D10: Athey, Bayati, Doudchenko, Imbens, and Khosravi 2021 matrix completion method to estimate event study for ULEV adoption around first ULEZ announcement (Q1 2015)

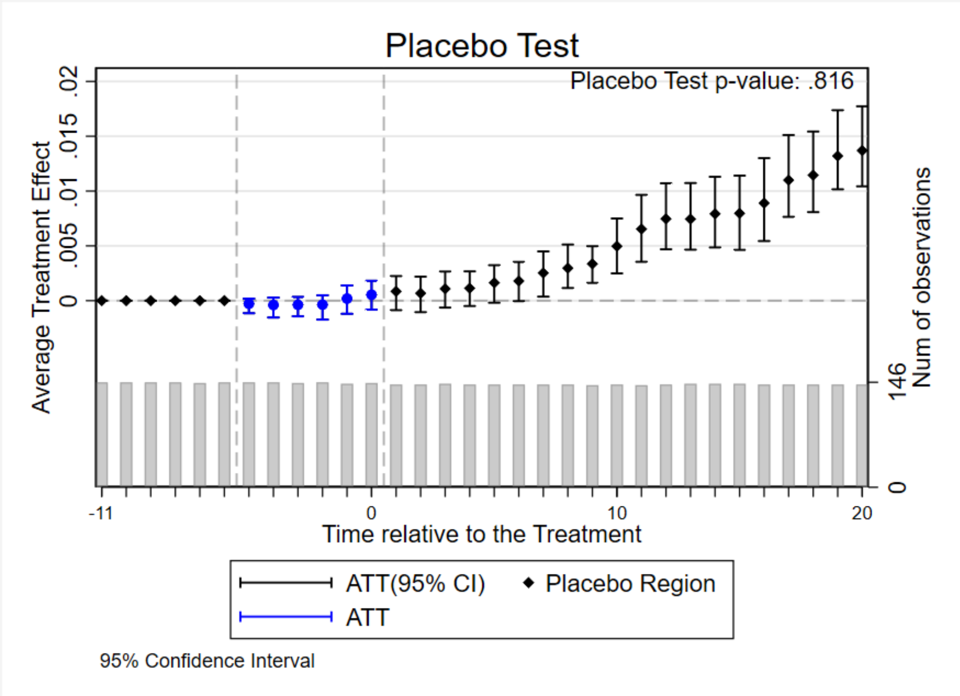


Figure D11: Price of sold houses and number of transactions in London

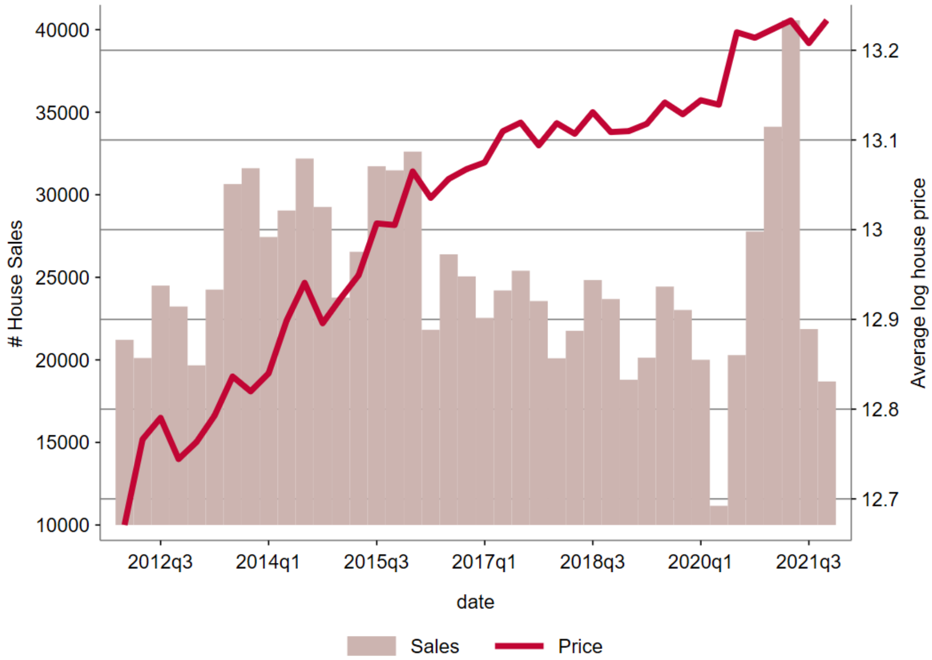


Figure D12: Average log house price in high and low ULEZ exposure postcode districts in London

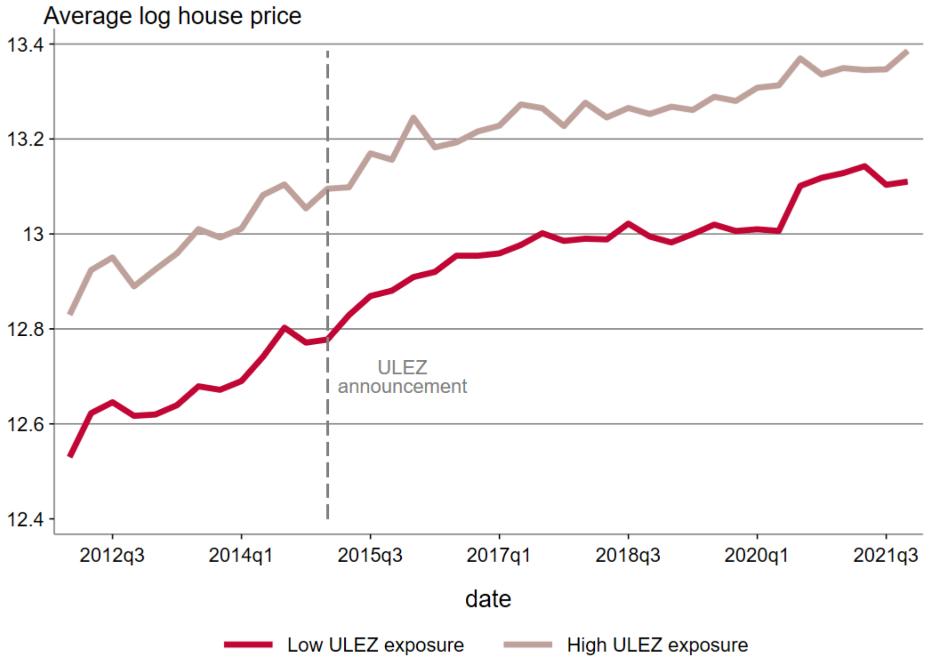


Figure D13: Binned average log house prices within 1 mile of the 2019 ULEZ boundary, before and after the policy announcement in March 2015.

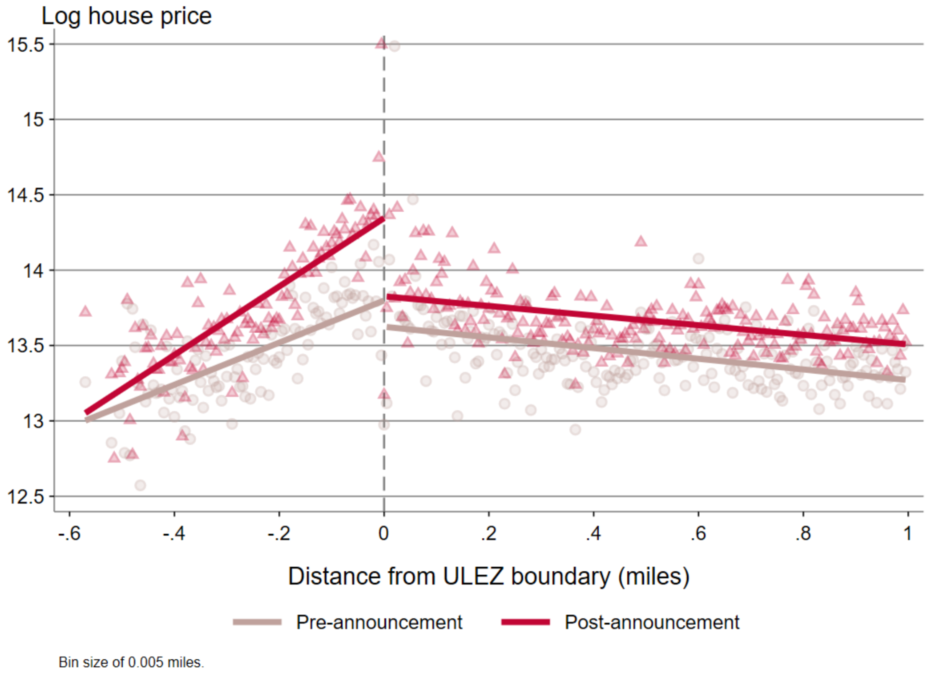


Figure D14: Weekly station entry (detrended) by quartiles of ULEZ exposure

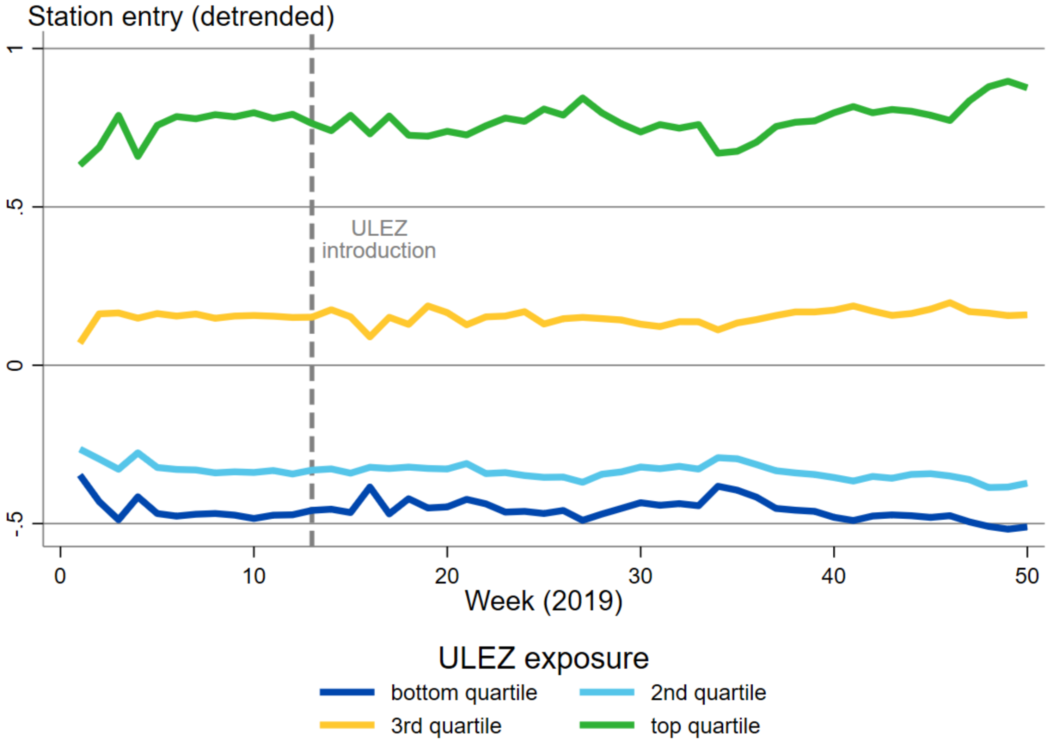


Figure D15: Marginal predicted effect of ULEZ exposure on station entry, pre- and post-policy introduction.

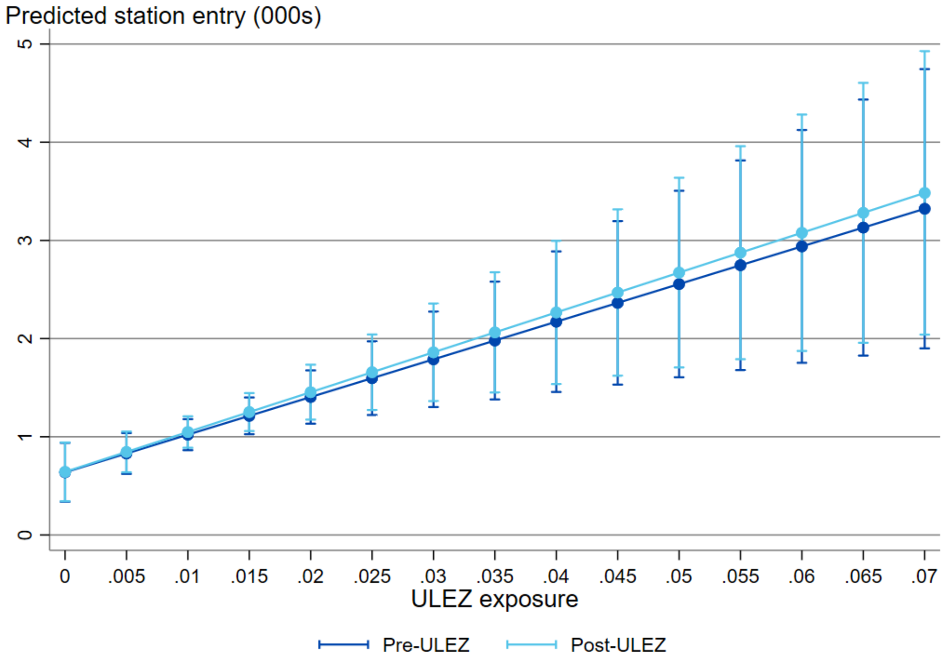


Figure D16: Evolution of distribution of seasonally adjusted firm entry rates by post-code sector

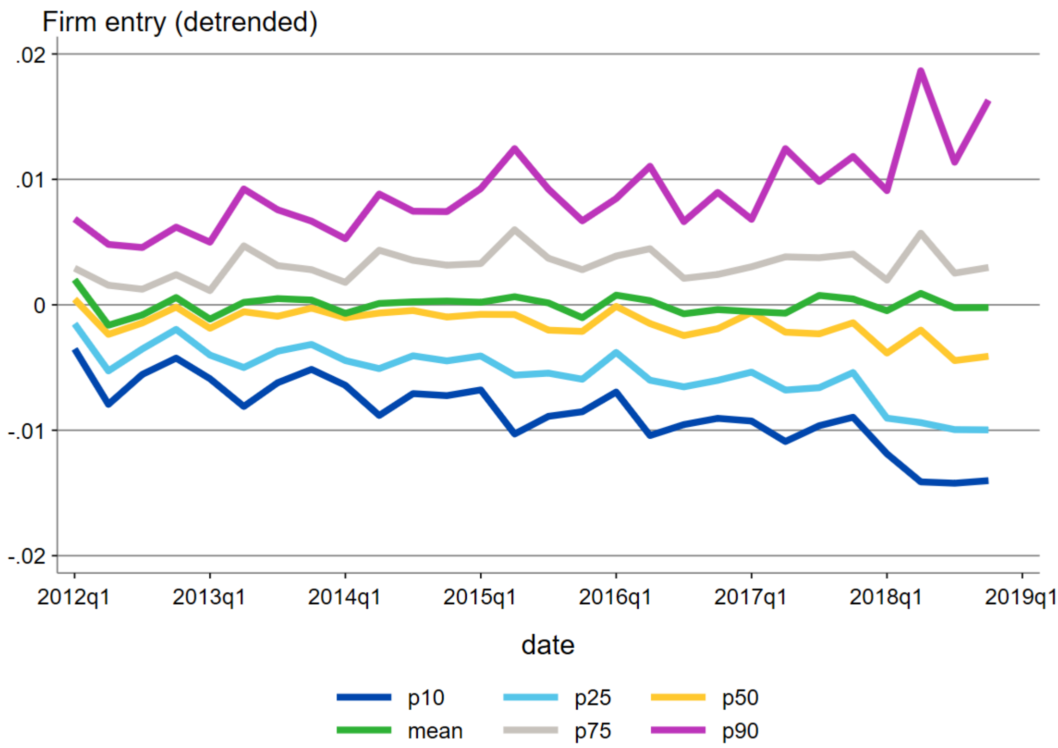


Figure D17: Distribution of true and simulated ULEZ exposure

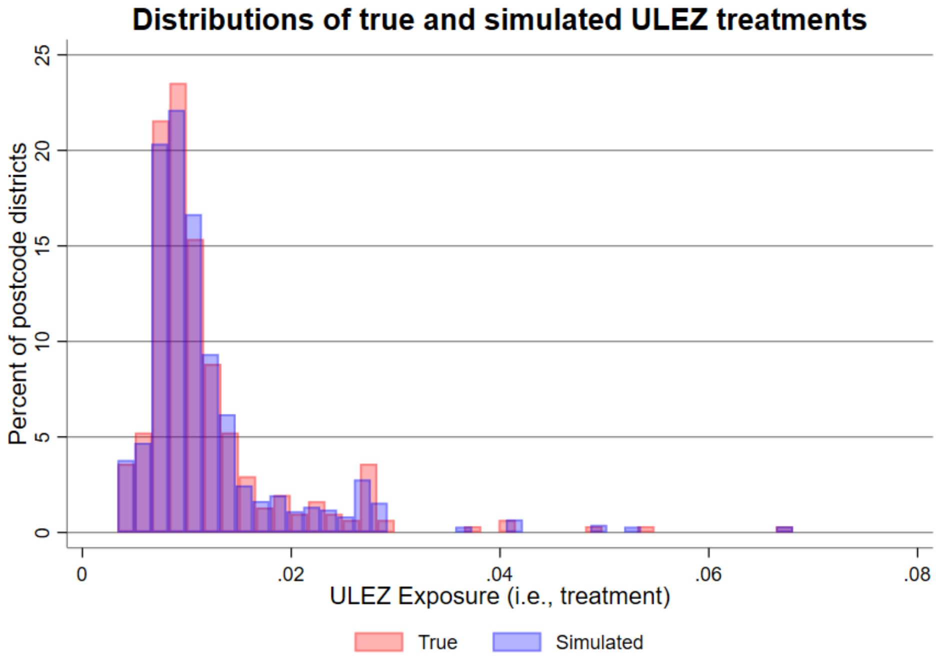


Figure D18: Baseline regression coefficients on ULEV adoption around ULEZ expansion announcement (Q2 2016)

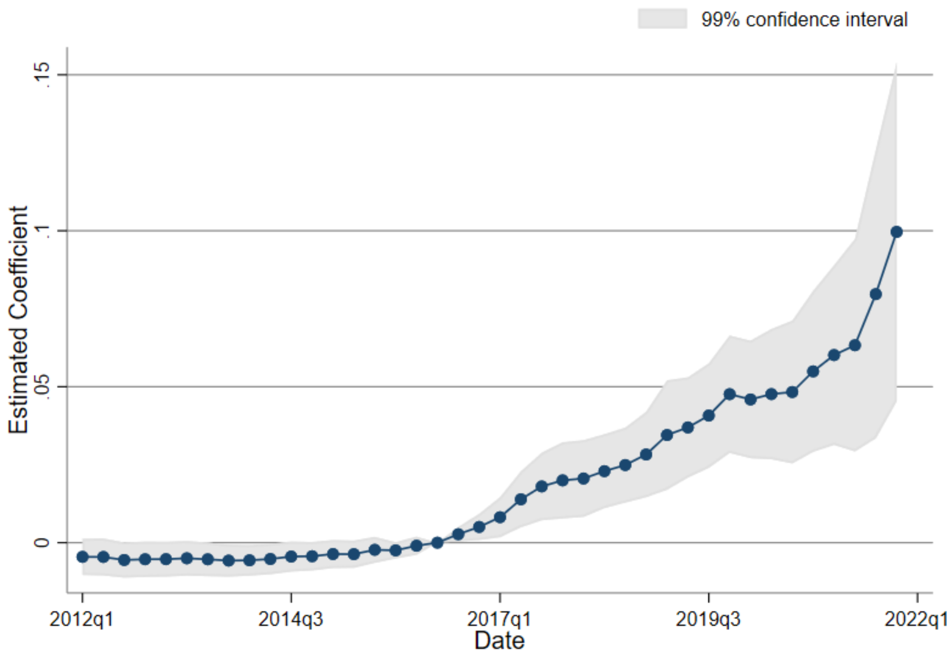


Figure D19: Bootstrapped estimates on the difference in the estimated coefficient on ULEZ indicator for house price RDD regression.

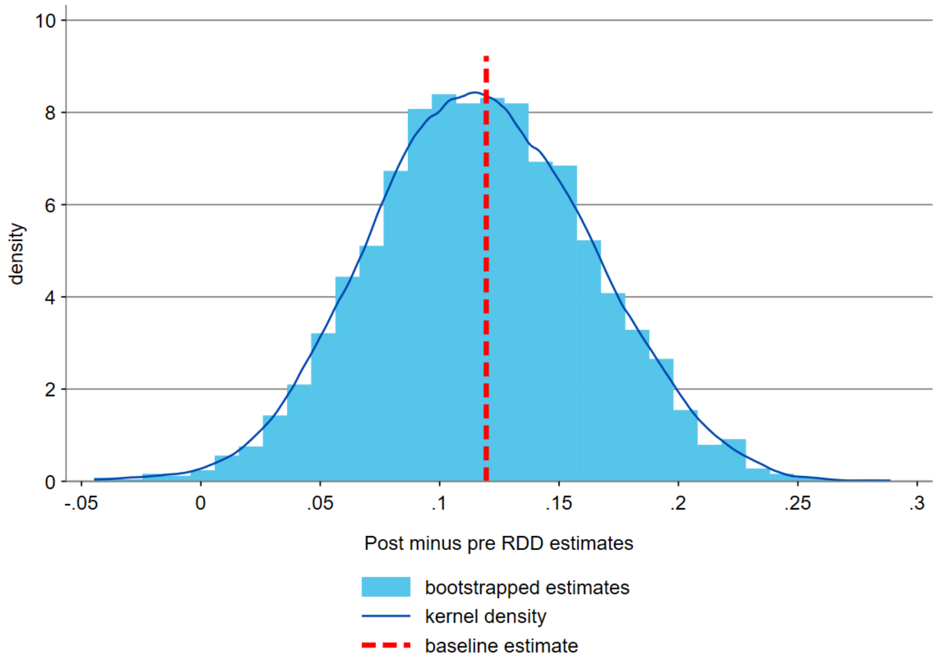


Figure D20: Placebo test on pre-announcement data with fake announcement date.

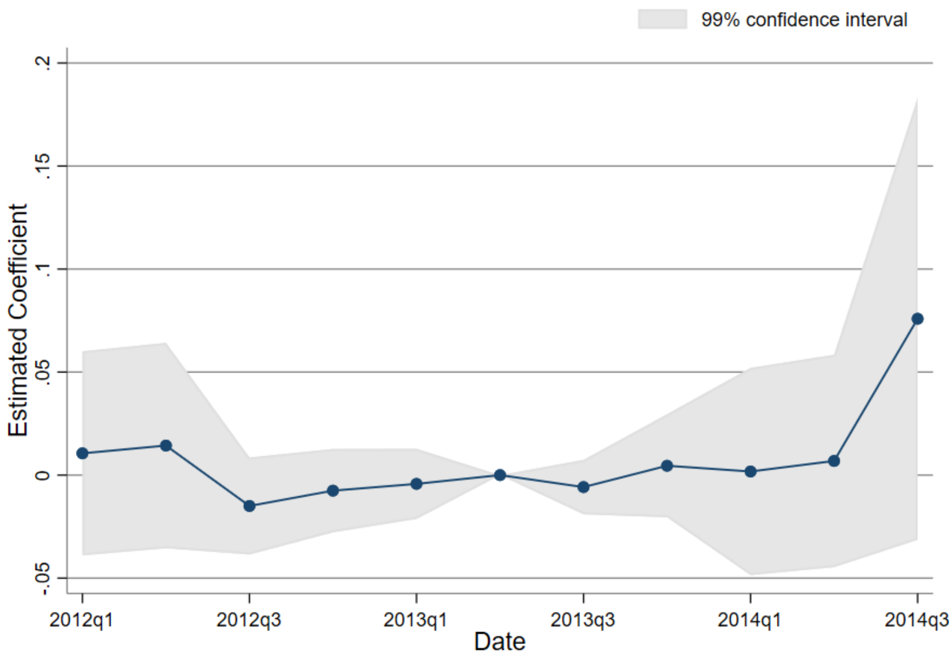
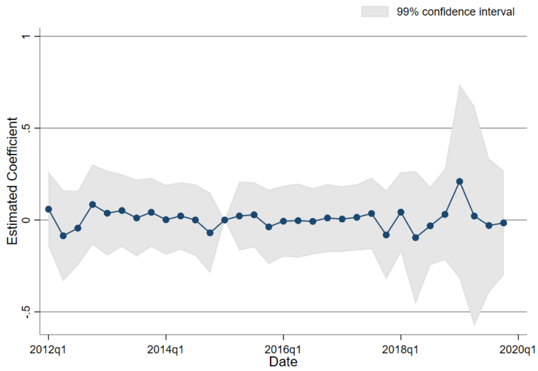


Figure D21: Placebo tests

(a) Placebo test with randomly assigned treatment data.



(b) Placebo test with vehicles per capita as outcome variable.

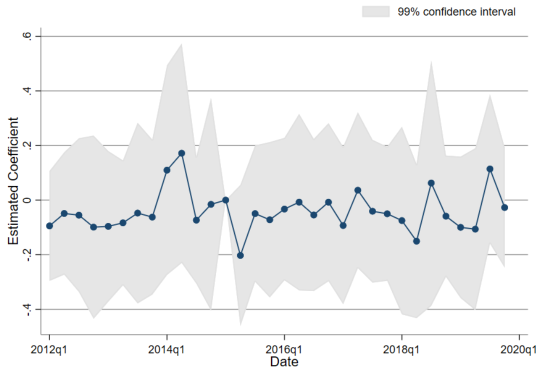


Figure D22: Testing for pre-trends in ULEV adoption prior to initial ULEZ announcement.

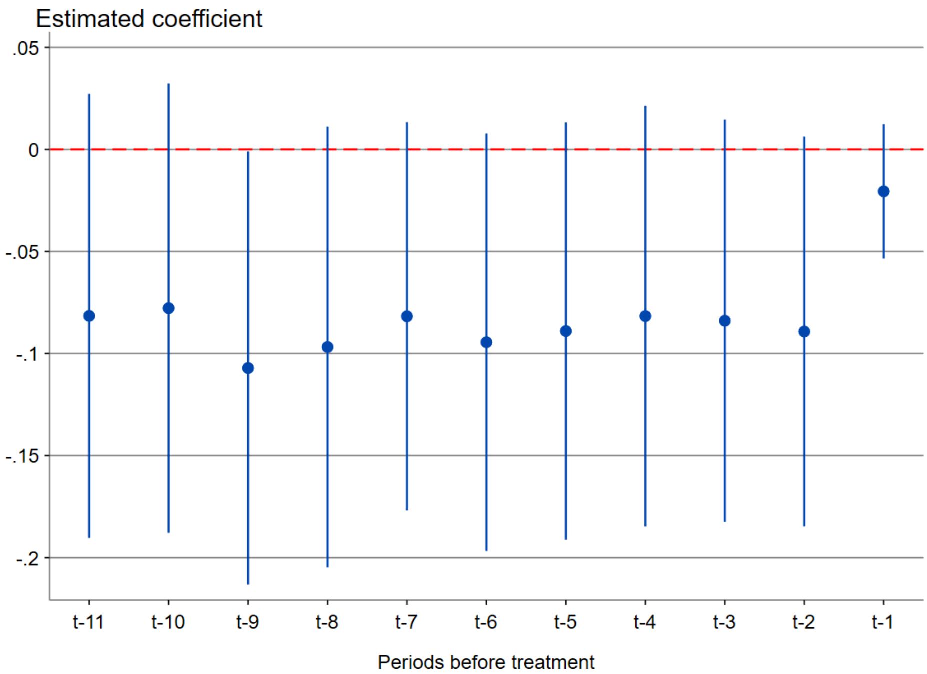


Figure D23: Baseline house price RDD for various distances from 2019 ULEZ boundary.

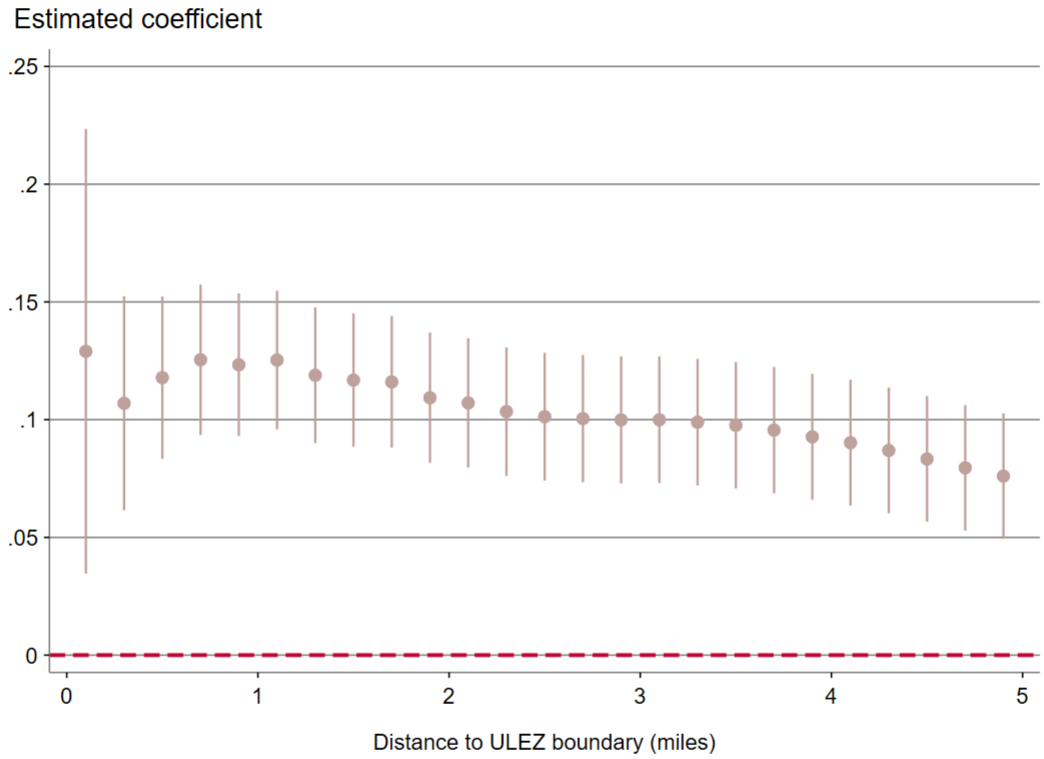
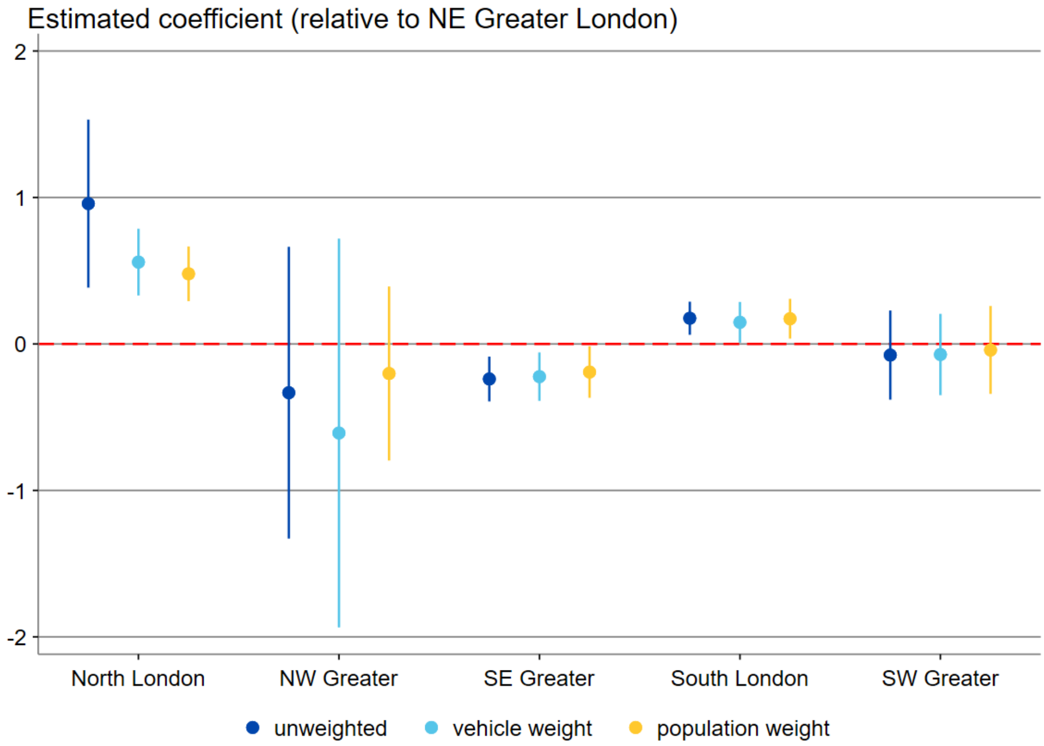


Figure D24: Estimated average treatment effect from initial ULEZ announcement, by London region.



E Additional tables

Table E1: House price RDD for 2019 ULEZ boundary with distance heterogeneity

	(1)	(2)	(3)	(4)
	Dependent variable: Log house price			
Positive distance	-0.042***	-0.189***	-0.435***	-0.451***
	(0.003)	(0.026)	(0.056)	(0.057)
Negative distance	-0.402***	-0.439***	-0.507***	-0.505***
	(0.123)	(0.125)	(0.124)	(0.125)
ULEZ	0.192***	-0.018	-0.079*	-0.088**
	(0.035)	(0.039)	(0.042)	(0.042)
ULEZ × Post-indicator	0.053***	0.117***	0.120***	0.122***
	(0.013)	(0.015)	(0.015)	(0.015)
Fixed Effects	Yes	Yes	Yes	Yes
Triangular kernel weight (2 mile)	No	Yes	No	Yes
Within 1 mile	No	No	Yes	Yes
N	629,217	130,607	60,797	60,797
R^2	0.384	0.357	0.355	0.345

Standard errors in parentheses, clustered at the postcode level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Fixed effects are year-quarter and 4-digit postcode.

Table E2: House price RDD for 2019 ULEZ boundary varying triangular bandwidths

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Log house price					
ULEZ	-0.056	-0.139***	-0.133***	-0.110***	-0.071**	-0.029
	(0.055)	(0.043)	(0.036)	(0.032)	(0.030)	(0.028)
ULEZ × Post-indicator	0.116***	0.124***	0.123***	0.118***	0.113***	0.109***
	(0.021)	(0.017)	(0.015)	(0.015)	(0.014)	(0.014)
Distance	0.058	-0.196***	-0.179***	-0.140***	-0.092***	-0.041***
	(0.103)	(0.061)	(0.039)	(0.026)	(0.018)	(0.014)
Bandwidth (miles)	0.5	1	1.5	2	2.5	3
N	37,054	60,797	89,868	130,607	170,657	210,474
R^2	0.303	0.326	0.343	0.356	0.360	0.361

Standard errors in parentheses, clustered at the postcode level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Fixed effects are year-quarter and 4-digit postcode.

Table E3: House price RDD for 2019 ULEZ boundary with distance interaction

	(1)	(2)	(3)	(4)
	Dependent variable: Log house price			
ULEZ	0.080***	-0.122***	-0.179***	-0.167***
	(0.024)	(0.033)	(0.041)	(0.042)
ULEZ × Post-indicator	0.087***	0.136***	0.133***	0.131***
	(0.014)	(0.020)	(0.026)	(0.027)
Distance	-0.046***	-0.153***	-0.270***	-0.251***
	(0.003)	(0.026)	(0.052)	(0.053)
Post-indicator × Distance	0.006***	0.019*	0.017	0.013
	(0.000)	(0.011)	(0.026)	(0.028)
Fixed Effects	Yes	Yes	Yes	Yes
Triangular kernel weight (2 mile)	No	No	Yes	Yes
Within 1 mile	No	Yes	No	Yes
N	629,217	130,607	60,797	60,797
R^2	0.385	0.356	0.354	0.344

Standard errors in parentheses, clustered at the postcode level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Fixed effects are year-quarter and 4-digit postcode.

Table E4: House price RDD for 2019 ULEZ boundary with triple interaction

	(1)	(2)	(3)	(4)
	Dependent variable: Log house price			
ULEZ	0.129***	-0.067	-0.118***	-0.121***
	(0.037)	(0.041)	(0.044)	(0.044)
ULEZ × Post-indicator	0.143***	0.187***	0.175***	0.170***
	(0.024)	(0.027)	(0.030)	(0.030)
Distance	-0.04***	-0.197***	-0.427***	-0.435***
	(0.003)	(0.027)	(0.058)	(0.060)
ULEZ × Post-indicator × Distance	0.356***	0.382***	0.401***	0.420***
	(0.103)	(0.105)	(0.106)	(0.109)
Fixed Effects	Yes	Yes	Yes	Yes
Triangular kernel weight (2 mile)	No	No	Yes	Yes
Within 1 mile	No	Yes	No	Yes
N	629,217	130,607	60,797	60,797
R^2	0.385	0.357	0.355	0.345

Standard errors in parentheses, clustered at the postcode level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Fixed effects are year-quarter and 4-digit postcode.

Table E5: House price RDD for 2019 ULEZ boundary with polynomial distance

	(1)	(2)	(3)
		Dependent variable: Log house price	
ULEZ	-0.110***	-0.101**	-0.055
	(0.032)	(0.044)	(0.046)
ULEZ × Post-indicator	0.118***	0.118***	0.118***
	(0.015)	(0.015)	(0.015)
Distance	-0.140***	-0.122*	0.020
	(0.026)	(0.067)	(0.074)
Distance^2		-0.011	-0.479***
		(0.032)	(0.084)
Distance^3			0.227***
			(0.033)
Fixed Effects	Yes	Yes	Yes
Triangular kernel weight (2 mile)	Yes	Yes	Yes
N	130,607	130,607	130,607
R^2	0.356	0.356	0.357

Standard errors in parentheses, clustered at the postcode level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Fixed effects are year-quarter and 4-digit postcode.

Table E6: House price 'donut' RDD for 2019 ULEZ boundary

	(1)	(2)	(3)	(4)
	Dependent variable: Log house price			
ULEZ	0.131***	-0.093***	-0.169***	-0.160***
	(0.025)	(0.034)	(0.042)	(0.043)
ULEZ × Post-indicator	0.050***	0.114***	0.117***	0.119***
	(0.014)	(0.015)	(0.016)	(0.016)
Distance	-0.041***	-0.153***	-0.282***	-0.270***
	(0.003)	(0.026)	(0.051)	(0.053)
Fixed Effects	Yes	Yes	Yes	Yes
Triangular kernel weight (2 mile)	No	Yes	No	Yes
Within 1 mile	No	No	Yes	Yes
N	625,673	127,063	57,253	57,253
R^2	0.382	0.356	0.354	0.343

Standard errors in parentheses, clustered at the postcode level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Fixed effects are year-quarter and 4-digit postcode.

Table E7: House price placebo test 2019 ULEZ with fake boundary

	(1)	(2)	(3)	(4)
	Dependent variable: Log house price			
ULEZ	-0.156***	-0.131***	-0.079*	-0.075*
	(0.025)	(0.040)	(0.044)	(0.045)
ULEZ × Post-indicator	-0.023***	0.004	-0.000	0.005
	(0.007)	(0.009)	(0.012)	(0.012)
Distance	-0.055***	-0.022	0.064*	0.042
	(0.003)	(0.026)	(0.037)	(0.038)
Fixed Effects	Yes	Yes	Yes	Yes
Triangular kernel weight (2 mile)	No	Yes	No	Yes
Within 1 mile	No	No	Yes	Yes
N	629,217	76,887	43,074	43,074
R^2	0.384	0.292	0.303	0.298

Standard errors in parentheses, clustered at the postcode level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Fixed effects are year-quarter and 4-digit postcode.

Table E8: Relative magnitudes across margins

Margin	Parameter	Standardised average effect	North	South	East	West
ULEV investment	$\frac{d\kappa_{ij}}{d\gamma}\frac{\gamma}{\kappa_{ij}}$	0.342***	0.430***	0.506***	0.361	0.368***
House prices	$\frac{dQ_i}{d\gamma}$	0.032***	0.002	0.021***	0.042***	0.064***
Public transport	$\frac{dP_3}{d\gamma}\frac{\gamma}{P_3}$	0.009***	0.007**	0.015***	-0.002	0.014**
Firm creation	$\frac{d\mathbb{E}\pi_j}{d\gamma}$	−0.089***	0.041	−0.125	−0.151**	−0.058

All coefficients standardised so equal to $\beta \times \sigma_x/\sigma_y$ where β is the estimated coefficient of interest in the differences-in-differences/regression discontinuity framework, σ_x is the standard deviation of treatment variable and σ_y is the standard deviation of the outcome variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E9: Difference-in-differences regression of ULEV share on ULEZ exposure by London region.

	Dependent variable: ULEV share			
Postcode area:	East	North	South	West
ULEZ exposure	0.014	0.196***	0.060***	0.069***
	(0.020)	(0.039)	(0.009)	(0.031)
ULEZ exposure × Post-indicator	0.523	0.58***	0.267***	0.363***
	(0.386)	(0.123)	(0.030)	(0.099)
N	1,357	1,078	2,129	1,367
R^2	0.568	0.720	0.649	0.541

Standard errors in parentheses, clustered at the postcode district level.
* $p < 0.1$, * $p < 0.05$, *** $p < 0.01$. Control for population density, size of postcode district, number of commuters, population-adjusted ULEZ measure and year-quarter fixed effects.

Table E10: House price RDD for 2019 ULEZ boundary by London region.

	Dependent variable: Log house price			
Postcode area:	East	North	South	West
ULEZ	-0.033	0.707*	-0.206***	-0.200***
	(0.091)	(0.389)	(0.050)	(0.098)
ULEZ × Post-indicator	0.131***	0.041	0.090***	0.173***
	(0.034)	(0.188)	(0.022)	(0.032)
Distance	-0.011	-0.197***	-0.358***	0.417***
	(0.136)	(0.167)	(0.063)	(0.211)
Fixed Effects	Yes	Yes	Yes	Yes
Triangular kernel weight (2 mile)	Yes	Yes	Yes	Yes
Within 1 mile	Yes	Yes	Yes	Yes
N	12,381	11,401	23,450	13,565
R^2	0.289	0.229	0.385	0.289

Standard errors in parentheses, clustered at the postcode level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Fixed effects are year-quarter and 4-digit postcode.

Table E11: Differences-in-differences regression of station entry on ULEZ exposure by London region.

	Dependent variable: Station entry (100,000s)			
Postcode area:	East	North	South	West
ULEZ exposure	-5.626	0.029	2.521	7.220**
	(9.615)	(1.126)	(1.717)	(2.819)
ULEZ exposure × Post-indicator	-0.236	-0.173**	0.287***	0.357**
	(0.566)	(0.071)	(0.095)	(0.167)
ULEZ	0.254**	0.136***	0.268*	0.073
	(0.103)	(0.017)	(0.142)	(0.059)
N	36,369	28,220	24,337	23,219
R^2	0.190	0.027	0.181	0.136

Standard errors in parentheses, clustered at the postcode level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Fixed effects are day, week, month, week of month, day of week.

Table E12: Firm entry RDD for initial ULEZ announcement by London region.

	Dependent variable: Log firm entry			
Postcode area:	East	North	South	West
ULEZ	-0.423	1.513***	-0.159	-0.059
	(0.336)	(0.388)	(0.296)	(0.246)
ULEZ × Post-indicator	-0.234**	0.103	-0.218	-0.090
	(0.105)	(0.170)	(0.136)	(0.080)
Distance	-0.015	0.088	-0.041	0.091
	(0.093)	(0.095)	(0.056)	(0.112)
N	1,085	601	1,187	960
R2	0.300	0.540	0.228	0.221

Standard errors in parentheses, clustered at the postcode district level.

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

Fixed effects are year and quarter. Weighted by triangular kernel, bandwidth 10 miles.