How do commuters adapt to local 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 choice and commuting behaviour highlights four important margins of adjustment: car purchases, commuting mode, firm location and residential location. We study these four margins using event-study and regression discontinuity methods, exploiting the randomness of the exact borders and differential exposure due to pre-existing commuting choices. We show that the introduction of the ULEZ had large, positive effects on the adoption of ultra-low emissions vehicles, raised house prices within the zone, shifted commuters towards public transport, and increased firm exits inside the zone. Responses to a later expansion are similar but smaller in magnitude. Adjustment also differs strongly by income. Beyond pollution pricing, we offer a blueprint for how commonly-available high-frequency data enable more careful and comprehensive policy design in real time.

JEL: H23; R40; R48; Q58

Keywords: low emission zones; pollution pricing; spatial economics

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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. Transport is the highest contributor to greenhouse gas emissions in the UK,[2] and therefore key to a successful transition towards Net Zero. Governments worldwide have implemented a range of policies in the transportation sector. In Europe, “low emissions zones” now prohibit or heavily tax the use of highly-polluting vehicles in many city centres. New York City recently became the first US city to do likewise.[3] 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 (see, for instance, Margaryan 2021), but also for the wider pattern of economic activity across affected cities. In this paper, we estimate the economic responses of affected commuters to the introduction and subsequent expansions of London’s Ultra Low Emission Zone (ULEZ).

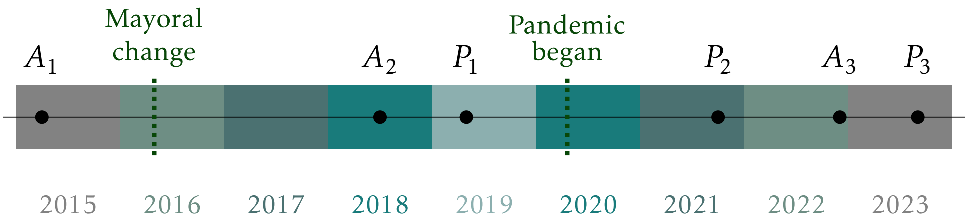
To guide our empirical analysis, we develop a spatial equilibrium model where the ULEZ charge acts as a tax on specific commuting pairs. In our framework, commuters maximise utility to balance place-specific amenities with commuting costs, given potential employers with differing productivities. The introduction of the ULEZ generates a wedge in the budget constraint for owners of non-compliant vehicles. Agents respond by paying the tax, investing in a cleaner vehicle to avoid paying it, switching transport modes, or relocating (either their residence or place of work). The model highlights that these are not independent choices; a tax on driving has general equilibrium effects through reduced congestion and spatial adjustment of housing prices, which affects commuters everywhere.

[1] See, for instance, Chay and Greenstone 2005; Currie and Neidell 2005; Currie and Walker 2011; Deschenes, Greenstone, and Shapiro 2017; Alexander and Schwandt 2022; Deryugina, Heutel, Miller, Molitor, and Reif 2019.

[2] Our World in Data, 2025.

[3] BBC, 2025.

Figure 1: Timeline of ULEZ announcements and implementation. A = announcement, P = policy introduction. Subscripts indicate the first, second and third expansions of the policy.



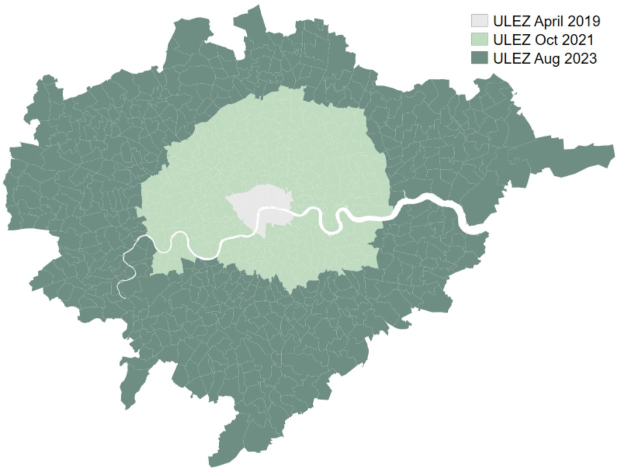
Empirically, we exploit the phased introduction of London's ULEZ and the randomness of its precise borders to study the adaptation of economic activity along all of the highlighted margins. In addition to estimating responses to the policy in retrospect, we provide a framework for monitoring the effectiveness 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 can uncover otherwise neglected trade-offs and potentially unintended consequences of the policy.

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.[4] 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 grew in size about tenfold and no longer exempted residents. The 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 plots the timeline of the ULEZ introduction and expansion.

The ULEZ "treatment" varies strikingly across space and over time (see Figure 2), as those commuting into the ULEZ face the strongest incentives to substitute towards

[4] BBC, 2019.

Figure 2: The evolution of London's ULEZ expansion



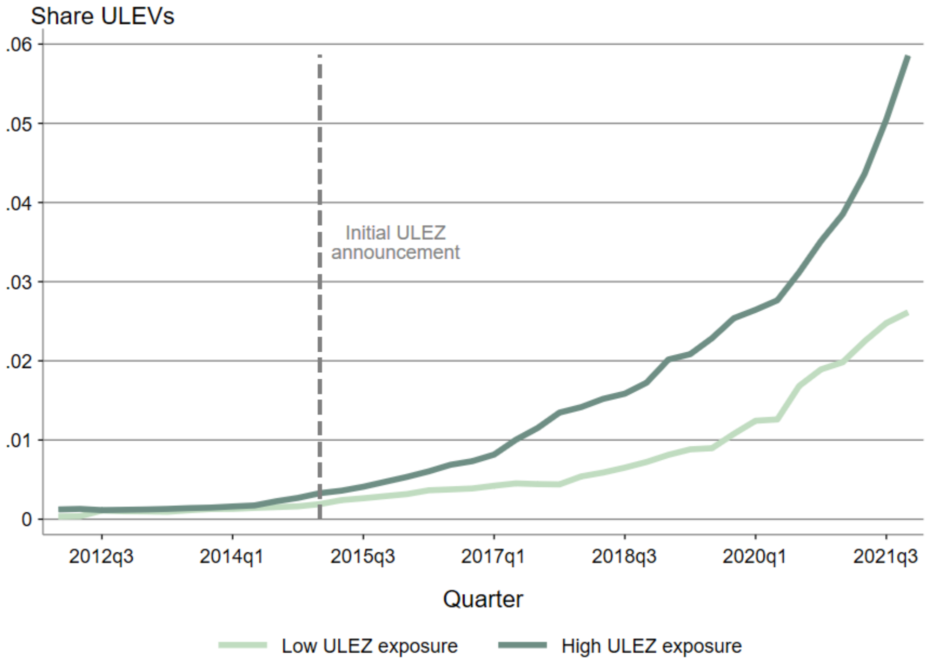
less-polluting vehicles, public transport, or change their work or home location. We use this policy variation, together with pre-existing variation in the share of commuters across adjacent postcodes, to analyse how individuals adjust their behaviour in response to the policy.

Even a cursory look at public-use Driver and Vehicle Licensing Authority (DVLA) data on vehicle registrations shows a large response in compliant Ultra-Low Emissions Vehicle (ULEV) purchases upon introduction of the ULEZ. Figure 3, is suggestive of a sharper rise in ULEVs in areas where individuals, due to their commuting behaviour, are more exposed to the ULEZ.[5]

We use two empirical research designs to estimate the causal impact pollution pricing has on economic activity, motivated by our model. For outcomes related to commuting mode (such as ULEV registration and public transport use), we employ a shift-share difference-in-differences (DiD) design (Bartik 1991; Borusyak, Hull, and Jaravel 2022; De Chaisemartin and d'Haultfoeuille 2022; J. Roth, 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 in a shift-share design. For outcomes related to location choice (firm creation and house transactions and prices), we

[5]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 3: Adoption of ultra-low emissions vehicles in high and low ULEZ exposure postcode districts



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 for forward-looking property owners change discontinuously at the ULEZ boundary.

We find a large, positive and significant effect of ULEZ exposure on ULEV adoption. Our results suggest an average 0.5% 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. Furthermore, there is evidence of modest substitution towards the London underground network, with an increase in tube use of roughly 2.3% at the average postcode. The spatial sorting effects are equally large. The resident exemption from the initial policy contributed to a 12% relative rise in the value of sold residential properties within the zone, relative to just outside. We find evidence of a significant effect on firm dynamism; exit rates rise approximately 3.5% inside the zone.

This paper contributes to two active strands of the literature. First, a series of recent papers have 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), Herzog (2024) and Isaksen and Johansen (2021). Barahona, Gallego, and Montero (2020) investigate the effect of a policy introduced in 1992 in Santiago, Chile, that, like the ULEZ, restricted the use of certain polluting vintages of vehicles. They find the policy was effective at encouraging switching towards cleaner vehicles, and that this switch was welfare-improving. Herzog (2024) focuses on the same geographic setting as we do, by investigating the introduction of the earlier Congestion Charge (CC) in London in 2003. He finds evidence that the policy reallocated commuters between driving and public transport, differentially across worker skill groups. Road traffic was reduced by approximately 1%, taking into account endogenous sorting and substitution towards untaxed driving routes.[6] Finally, Isaksen and Johansen (2021) leverage quasi-exogenous variation of congestion pricing introduced in 2016 in Bergen, Norway, which varied across vehicles and across time. They find that households exposed to the tax were 4.2% more likely to adopt an electric vehicle, almost exclusively driven by households in the top quartile of the income distribution.

Second, recent research has also analysed the impact of emission-curbing policies on downstream outcomes. Housing prices generally rise inside the low emission zones in response to these policies (Tang 2021; Gruhl, Volhausen, Pestel, and Moore 2022; Aydin and Rauck 2023). Driving taxes and low-emission zones are often motivated by a desire to reduce air pollution, but the evidence is mixed on this front (Simeonova, Currie, Nilsson, and Walker 2019; Margaryan 2021; 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; Ali Beshir and Fichera 2022; Chamberlain, Fecht, Davies, and Laverty 2023). London's earlier Low Emission Zone, introduced in 2008, may have increased test scores for teenage children, possibly via reduced air pollution (Avila-

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

Uribe, S. Roth, and Shields 2024).

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 2024; Ponticelli, Q. Xu, and Zeume 2023) and on 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 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 2024; Fetzer 2023; Fetzer, Palmou, and Schneebacher 2024) 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 explains our theoretical model, Section 3 the data we use, and Section 4 our empirical approach. Section 5 presents our results, and Section 6 puts them in context. A final Section 7 concludes.

2 Theoretical model and main hypotheses

Given widespread beliefs in the public debate that the primary impact of the ULEZ is on commuting behaviour,[7] we focus our analysis on the economic geography of work across Greater London. Individuals previously driving into London for work in non-ULEZ compliant vehicles have three options once 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

[7] The Guardian, 2023.

ULEZ-compliant vehicle; they can switch to public transport; or they can work from home more often.[8] If they choose to adapt on the commuting distance margin, they can either change employer location (for instance, by switching jobs) or move home. We now formalise these choices in the context of a simple model of commuting choice.

2.1 Model

Following ideas in Ommeren and Dargay (2006) and Monte, Redding, and Rossi-Hansberg (2018), we embed commuting mode choices in a standard spatial economics model via a multinomial logit structure. Firms are spatially dispersed and produce with geographically-specific productivity and locally determined wages. Workers purchase goods and housing services to maximise utility, and take commuting costs into account when choosing work and home locations. Commuting behaviour is driven by direct costs (for instance, taxes, vehicles or tickets) and indirect costs (congestion).

2.1.1 Environment

Utility. Utility is given by the function

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

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

Solving the utility maximisation problem (and dropping the individual-specific subscript ω for notational clarity) yields the indirect utility function:

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

[8] We largely abstract from the working-from-home margin in this paper due to data limitations for the earlier expansions, but show in the appendix that it likely accounts for some additional adjustment.

Firms. Firms produce output with a labour input and constant returns to scale technology $Y_j = Z_j L_j$. They choose labour L_j to maximise profits. The wage is the outcome of Nash bargaining between firms and matched workers $\max_{w_j} \left[L_i j(w_j - \bar{\kappa_j})\right]^{\beta} \left[L_j(Z_j - w_j)\right]^{1-\beta}$.[9] This yields a wage which is the weighted average of commuting costs and local productivity $w_j = \beta\bar{\kappa_j} + (1-\beta)Z_j$. The average commuting cost is given by $\bar{\kappa_j} = \sum_i \left(\frac{N_{ij}}{L_j}\right)\kappa_{ij}$, with $\kappa_{ij} = \sum_m \mathbb{P}_{m|ij}\kappa_{m|ij}$ the expected commuting cost from i to j.

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

Housing market. Utility maximisation determines housing demand.

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

Aggregating over all individuals living in i:

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

where N_{ij} are the number of people living in i and working in j and $\kappa_{ij} = \sum_m \mathbb{P}_{m|ij}\kappa_{m|ij}$. The supply side is simply $H_i^s = A_i Q_i^{\epsilon}$. 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 can commute from i to j in three ways:

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

[9] The surplus earned by firms in j is $Y_j - L_j w_j = L_j(Z_j - w_j)$. The surplus earned by all matched workers in j is $\sum_{\omega \in L_j}(w_j - \kappa_{\omega j}) = L_j w_j - \sum_{\omega \in L_j}\kappa_{\omega j} = L_j w_j - L_j\bar{\kappa_j}$. The last step arises from $L_j\bar{\kappa_j} = L_j\sum_i \frac{N_{ij}}{L_j}\kappa_{ij} = \sum_i N_{ij}\kappa_{ij} = \sum_i \left(\sum_{\omega \in N_{ij}}\kappa_{\omega j}\right) = \sum_{\omega \in L_j}\kappa_{\omega j}$.

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 $\phi > 0$.

Commuting choices are nested in the model. Given a home location i and work location j, the probability of choosing commuting option m is given by $\mathbb{P}_{m|ij} = \frac{\exp(\mu V_{m|ij})}{\sum_n \exp(\mu V_{n|ij})}$, where $V_{m|ij}$ is the previously derived indirect utility for method m, while μ is a scaling parameter. Denote the inclusive value $G_{ij} = \frac{1}{\mu} \ln \sum_m \exp(\mu V_{m|ij})$. Given a home location i, the probability of commuting to j is $\mathbb{P}_{j|i} = \frac{\exp(G_{ij})}{\sum_k \exp(G_{ik})}$. The 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 assume 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: $N_{ij} = N_i \times \mathbb{P}_{j|i}$.

2.1.2 Comparative statics

Effect of the tax on commuting costs. The tax on old vehicles γ affects the commuting cost 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 are weighted by the probabilities that individuals choose each method, so $\kappa_{ij} = \mathbb{P}_{1|ij}\kappa_{1|ij} + \mathbb{P}_{2|ij}\kappa_{2|ij} + \mathbb{P}_{3|ij}\kappa_{3|ij}$. This responds to the tax:

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

The response of commuting costs to the tax thus 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 expression 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 investment in new vehicles. The probability of choosing to drive a new vehicle $\mathbb{P}_{2|ij}$ responds to the pollution tax through indirect utility $V_{m|ij}$ and the impact on commuting costs $\kappa_{m|ij}$. The derivative of the logit probability to the tax is:

$$\frac{d\mathbb{P}_{2|ij}}{d\gamma} = \mathbb{P}_{2|ij}\left(\mu\frac{dV_{2|ij}}{d\gamma} - \sum_{m=1}^{3}\mathbb{P}_{m|ij}\mu\frac{dV_{m|ij}}{d\gamma}\right)$$

Indirect utility is affected by the tax:

$$
\begin{aligned}
\frac{dV_{m|ij}}{d\gamma} &= -b_{ij}Q_i^{\alpha-1}\Big(\frac{w_j}{\kappa_{m|ij}^2}\frac{d\kappa_{m|ij}}{d\gamma}\Big) \\
&= -\frac{w_j}{\kappa_{m|ij}^2}\frac{V_{m|ij}\kappa_{m|ij}}{w_j-\kappa_{m|ij}}\frac{d\kappa_{m|ij}}{d\gamma} \\
&= -V_{m|ij}\Big(\frac{w_j}{\kappa_{m|ij}(w_j-\kappa_{m|ij})}\Big)\frac{d\kappa_{m|ij}}{d\gamma}
\end{aligned}
$$

where the second line substitutes $b_i j Q_i^{\alpha-1} = \frac{V_{m|ij}\kappa_{m|ij}}{w_j-\kappa_{m|ij}}$.

The direct cost of taking public transport $\kappa_{3|ij}$ is unaffected by the tax, so $\frac{d\kappa_{3|ij}}{d\gamma} = 0$. Therefore the elasticity of the share of new-car drivers to the tax is:

$$
\begin{aligned}
\frac{d\mathbb{P}_{2|ij}}{d\gamma}\frac{\gamma}{\mathbb{P}_{2|ij}} &= \gamma\Big(\mu(1-\mathbb{P}_{2|ij})\frac{dV_2}{d\gamma} - \mu\mathbb{P}_{1|ij}\frac{dV_1}{d\gamma}\Big) \\
&= \underbrace{\mu(1-\mathbb{P}_{2|ij})V_2\epsilon_{V_2,\kappa_2}\epsilon_{\kappa_2,\gamma}}_{\text{Indirect congestion effect}} - \underbrace{\mu\mathbb{P}_{1|ij}V_1\epsilon_{V_1,\kappa_1}\epsilon_{\kappa_1,\gamma}}_{\text{Direct substitution effect}}
\end{aligned}
$$

where $\epsilon_{V_m,\kappa_m} < 0$ always, because the indirect utility of commuting by mode m decreases in response to a rise in the direct cost of that mode of commuting. As shown above, the elasticities of the commuting costs κ to the tax γ are negative for new vehicles and positive for old vehicles.

Therefore, the indirect congestion effect is positive (the tax discourages driving, reducing congestion, making new vehicles more appealing) and the direct substitution effect is negative (the tax directly makes old vehicles more costly, encouraging switching towards new vehicles). The overall effect is unsurprisingly positive - the tax encourages switching to new vehicles.

Effect of the tax on public transport use. The tax on old vehicles affects the commuting costs and thus optimal commuting choices of workers, directly and indirectly via congestion (stemming from the total number of drivers). The change in probability of using public transport with respect to the tax is:

$$\frac{d\mathbb{P}_{3|ij}}{d\gamma} = \mathbb{P}_{3|ij}\left(\mu\frac{dV_3}{d\gamma} - \sum_{m=1}^{3}\mathbb{P}_{m|ij}\mu\frac{dV_m}{d\gamma}\right)$$

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:

$$\begin{aligned}
\frac{d\mathbb{P}_{3|ij}}{d\gamma} &= -\mathbb{P}_{3|ij}\left(\mathbb{P}_{1|ij}\mu\frac{dV_1}{d\gamma} + \mathbb{P}_{2|ij}\mu\frac{dV_2}{d\gamma}\right) \\
\frac{d\mathbb{P}_{3|ij}}{d\gamma}\frac{\gamma}{\mathbb{P}_{3|ij}} &= -\gamma\left(\mu\mathbb{P}_{1|ij}V_1\epsilon_{V_1,\kappa_1}\epsilon_{\kappa_1,\gamma} + \mu\mathbb{P}_{2|ij}V_2\epsilon_{V_2,\kappa_2}\epsilon_{\kappa_2,\gamma}\right) \\
&= \underbrace{\mathbb{P}_{1|ij}\mu V_1\left(\frac{w}{w-\kappa_{1|ij}}\right)}_{\text{Direct substitution away from taxed cars}} \\
&\quad + \underbrace{(\eta\delta N_d\epsilon_{N_d,\gamma})}_{\text{Indirect congestion cost}}\left[\underbrace{\mathbb{P}_{1|ij}\mu V_1\left(\frac{w}{w-\kappa_{1|ij}}\right)}_{\text{Indirect substitution towards taxed cars}} + \underbrace{\mathbb{P}_{2|ij}\mu V_2\left(\frac{w}{w-\kappa_{2|ij}}\right)\left(\frac{\kappa_{2|ij}-\phi}{\kappa_{2|ij}}\right)}_{\text{Indirect substitution towards non-taxed cars}}\right]
\end{aligned}$$

The intuition of this elasticity is straightforward. The first term is the direct substitution away from taxed vehicles, which is unambiguously positive. The second term is the indirect congestion cost multiplied by the response of driving to reduced congestion, which is negative because the congestion cost falls in response to the tax. The sum of these terms can be positive or negative.

Effect of tax on congestion. The level of congestion N_d depends on the driving tax via the probabilities of commuting through different methods. These probabilities themselves depend on the level of congestion. Using the implicit function theorem, we can differentiate the equilibrium condition: $T(\gamma, N_d) = N_d - N\sum_i\sum_j \mathbb{P}_{j|i}(\gamma, N_d)\Big(\mathbb{P}_{1|ij}(\gamma, N_d) + \mathbb{P}_{2|ij}(\gamma, N_d)\Big) = 0$.

$$\frac{dN_d}{d\gamma} = -\frac{\partial T/\partial\gamma}{\partial T/\partial N_d}$$

Consider the denominator first:

$$\frac{\partial T}{\partial N_d} = 1 - N \sum_i \sum_j \underbrace{\frac{\partial}{\partial N_d}(\mathbb{P}_{j|i}[\mathbb{P}_{1|ij} + \mathbb{P}_{2|ij}])}_{\text{Congestion feedback loop}}$$

The congestion feedback loop is negative. More congestion N_d (holding the tax γ constant) increases the commuting costs of driving, reducing the probabilities of commuting by car, weakly reducing the probability of worker living in i choosing to work in j ($\mathbb{P}_{j|i}$).

Consider the numerator:

$$\frac{\partial T}{\partial \gamma} = 0 - N \sum_i \sum_j \underbrace{\frac{\partial}{\partial \gamma}(\mathbb{P}_{j|i}[\mathbb{P}_{1|ij} + \mathbb{P}_{2|ij}])}_{\text{Direct tax effect}}$$

The direct tax effect is negative. A higher tax γ (holding congestion N_d constant) only increases the cost of commuting with old vehicles κ_1. This reduces commuting by this mode, so $\mathbb{P}_{1|ij}$ falls. While $\mathbb{P}_{2|ij}$ may rise, it will only fully offset the fall if no one switches to public transport. Thus the direct tax effect is weakly negative (zero in this edge case). In sum, the elasticity of congestion to the driving tax $\frac{dN_d}{d\gamma}$ is negative.

Effect on house prices. There are three channels through which the tax γ affects house prices: (1) wages, (2) commuting costs, (3) number of commuters.

$$
\begin{aligned}
\frac{dQ_i}{d\gamma} &= \frac{1-\alpha}{1+\epsilon}\frac{1}{A_i Q_i^{\epsilon}}\sum_j\left(\frac{dN_{ij}}{d\gamma}(w_j-\kappa_{ij})+N_{ij}\left(\frac{dw_j}{d\gamma}-\frac{d\kappa_{ij}}{d\gamma}\right)\right)\\
&= \underbrace{\frac{1-\alpha}{1+\epsilon}}_{\substack{\text{Balance of}\\ \text{S \& D}\\ \text{for housing}}}\ \underbrace{\frac{L_i}{H_i^s}}_{\substack{\text{Inverse}\\ \text{housing}\\ \text{supply}\\ \text{per worker}}}\ \sum_j\Bigg(\underbrace{N_{ij}\Bigg[\underbrace{(\beta-1)\frac{d\kappa_{ij}}{d\gamma}}_{\text{Bargaining effect}}+\underbrace{\beta\left(\sum_k\left(\frac{N_{kj}}{L_j}\frac{d\kappa_{kj}}{d\gamma}-\frac{d\kappa_{ij}}{d\gamma}\right)\right)}_{\substack{\text{Difference in change in commuting cost}\\ \text{between location } i \text{ and weighted average}}}\Bigg]}_{\text{Effect of tax on wages of non-moving workers}}\\
&\quad+\underbrace{\Bigg[\underbrace{\frac{dN_{ij}}{d\gamma}(w_j-\kappa_{ij})}_{\text{Direct sorting effect}}+\underbrace{N_{ij}\left(\beta\sum_k\frac{d(N_{kj}/L_j)}{d\gamma}\kappa_{kj}\right)}_{\text{Indirect sorting effect}}\Bigg]}_{\text{Changes in housing demand from workers that move}}\Bigg)
\end{aligned}
$$

This derivative consists of three terms multiplied together. 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 the supply is more inelastic (lower ϵ), the house price responds more to the 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 above (wages, commuting costs, number of commuters) into two specific mechanisms: (1) the effect of the tax on wages of non-moving workers, and (2) the changes in housing demand from workers that move. The first channel represents a fall in disposable income for workers who continue to live in region i. There is a direct bargaining effect, whereby the rise in the commuting cost (from the tax) does not get fully reflected in wages. This is negative because $\beta - 1 < 0$. There is also a term that describes the difference in how changes in commuting costs affect workers in i compared to a weighted average across the economy. It represents the exposure of wages to the tax, for workers in i relative to all workers, with an ambiguous sign.

The second channel relates to how workers move and thus influence the demand for housing. There is a direct sorting effect, as the tax discourages workers from demanding housing in region i. This effect is negative. There is also an indirect sorting effect, as workers that leave location i reduce local labour supply, affecting local wages through wage bargaining via $\bar{\kappa}_i$. The sign of this effect is ambiguous.

Overall, the effect of the tax on wages of non-moving workers is negative and so is the change in housing demand from workers that move. House prices in commuter area i thus respond negatively to the tax, where it is present.[10]

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.

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

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?

[10] 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.

Table 1: Model summary

Margin	Comparative statics	Direction
ULEV investment	$\frac{d\mathbb{P}_{2\mid ij}}{d\gamma}\frac{\gamma}{\mathbb{P}_{2\mid ij}} = \underbrace{\mu(1-\mathbb{P}_{2\mid ij})V_2\epsilon_{V_2,\kappa_2}\epsilon_{\kappa_2,\gamma}}_{\text{Indirect congestion effect}} - \underbrace{\mu\mathbb{P}_{1\mid ij}V_1\epsilon_{V_1,\kappa_1}\epsilon_{\kappa_1,\gamma}}_{\text{Direct substitution effect}}$	Likely ≥ 0
House prices	$\frac{dQ_i}{d\gamma} = \underbrace{\frac{1-\alpha}{1+\epsilon}}_{\substack{\text{Balance of}\\ \text{S \& D}\\ \text{for housing}}} \underbrace{\frac{L_i}{H_i^s}}_{\substack{\text{Inverse}\\ \text{housing}\\ \text{supply}\\ \text{per worker}}} \sum_j \Bigg(\underbrace{N_{ij}\Bigg[\underbrace{(\beta-1)\frac{d\kappa_{ij}}{d\gamma}}_{\text{Bargaining effect}} + \underbrace{\beta\Bigg(\sum_k\Bigg(\frac{N_{kj}}{L_j}\frac{d\kappa_{kj}}{d\gamma} - \frac{d\kappa_{ij}}{d\gamma}\Bigg)\Bigg)}_{\substack{\text{Difference in change in commuting cost}\\ \text{between location } i \text{ and weighted average}}} \Bigg]}_{\text{Effect of tax on wages of non-moving workers}} + \underbrace{\Bigg[\underbrace{\frac{dN_{ij}}{d\gamma}(w_j-\kappa_{ij})}_{\text{Direct sorting effect}} + \underbrace{N_{ij}\Bigg(\beta\sum_k\frac{d(N_{kj}/L_j)}{d\gamma}\kappa_{kj}\Bigg)}_{\text{Indirect sorting effect}} \Bigg]}_{\text{Changes in housing demand from workers that move}} \Bigg)$	Likely ≤ 0
Public transport	$\frac{d\mathbb{P}_{3\mid ij}}{d\gamma}\frac{\gamma}{\mathbb{P}_{3\mid ij}} = \underbrace{\mathbb{P}_{1\mid ij}\mu V_1\left(\frac{w}{w-\kappa_{1\mid ij}}\right)}_{\substack{\text{Direct substitution}\\ \text{away from taxed cars}}} + \underbrace{(\eta\delta N_d\epsilon_{N_d,\gamma})}_{\substack{\text{Indirect}\\ \text{congestion cost}}} \left[\underbrace{\mathbb{P}_{1\mid ij}\mu V_1\left(\frac{w}{w-\kappa_{1\mid ij}}\right)}_{\substack{\text{Indirect substitution}\\ \text{towards taxed cars}}} + \underbrace{\mathbb{P}_{2\mid ij}\mu V_2\left(\frac{w}{w-\kappa_{2\mid ij}}\right)\left(\frac{\kappa_{2\mid ij}-\phi}{\kappa_{2\mid ij}}\right)}_{\substack{\text{Indirect substitution}\\ \text{towards non-taxed cars}}} \right]$	Ambiguous
Firm location	$\frac{d\mathbb{E}\pi_j}{d\gamma} = \underbrace{(Z_j-w_j)}_{\text{Profit}}\sum_i N_{ij} \underbrace{\left[\frac{\partial G_{ij}}{\partial\gamma} - \sum_k \mathbb{P}_{k\mid i}\frac{\partial G_{ik}}{\partial\gamma}\right]}_{\substack{\text{Difference in utility response}\\ \text{to tax in } j \text{ compared to}\\ \text{average in all other regions}}} - \underbrace{\frac{dw_j}{d\gamma}L_j}_{\substack{\text{Wage}\\ \text{response}}}$	≤ 0

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

2.2 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 either of the two commuting **mode** margins or the two commuting **distance** margins. Our secondary null hypotheses state that economic activity does not react to announcements (**strong version**) or reacts equally across all margins (**weak version**) and that postcodes do not react differentially to policy announcements based on policy-relevant characteristics (for instance, their income level).

1. $H_{0,1}$: There is no differential change in economic behaviour (in terms of purchasing electric vehicles, using public transport, 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).

3 Data

This section describes the data sources we use.[11] Table 2 summarises the level of geographic and time aggregation, the resulting number of observations, and our identification approach for our four margins of adjustment to the policy.

Table 2: Cleaned data summary

Margin	Parameter	Geography	Time period	N	$N \times T$	Identification
ULEV investment	$\frac{d\mathbb{P}_{2\|ij}}{d\gamma}\frac{\gamma}{\mathbb{P}_{2\|ij}}$	Postal 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	$\frac{d\mathbb{P}_{3\|ij}}{d\gamma}\frac{\gamma}{d\mathbb{P}_{3\|ij}}$	Postcode	2019 (daily)	375	135,683	DiD
Firm location	$\frac{d\mathbb{E}\pi_j}{d\gamma}$	Postcode	2010 - 2023 (quarterly)	13,471	556,584	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 residents on average. To construct ULEZ exposure and weights, we use population at OA level. To compute commuting shares, we use 2011 Census commuting behaviour.

3.1 Computing ULEZ exposure

The list of postcodes in each expansion of the ULEZ has been released via freedom of information requests and we manually check for consistency with other sources. Different areas vary in how "exposed" they are to the "shock" of the ULEZ expansions, based on commuting behaviour into the ULEZ. There are two sources of randomness with regards to the policy announcement: (1) randomness of the precise ULEZ boundary, and (2) randomness in the share of people who drive into the ULEZ.

[11] 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.

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 at the more aggregate postcode district level. We compute a population-based allocation at the postcode district level, which represents the share of residents who live within the ULEZ. For example, if W1 4GE is in the ULEZ with a population of 1,000 residents, but W1 7PU with 500 residents is not, the hypothetical population-adjusted ULEZ score of their postcode district W1 would be 0.66.

2. **Compute ULEZ exposure by postcode district.** We calculate the vehicle-weighted shares of (ULEZ-taxable) commuting multiplied by the ULEZ score. We take the share of ULEZ-taxable commuting from 2011 Census data.

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

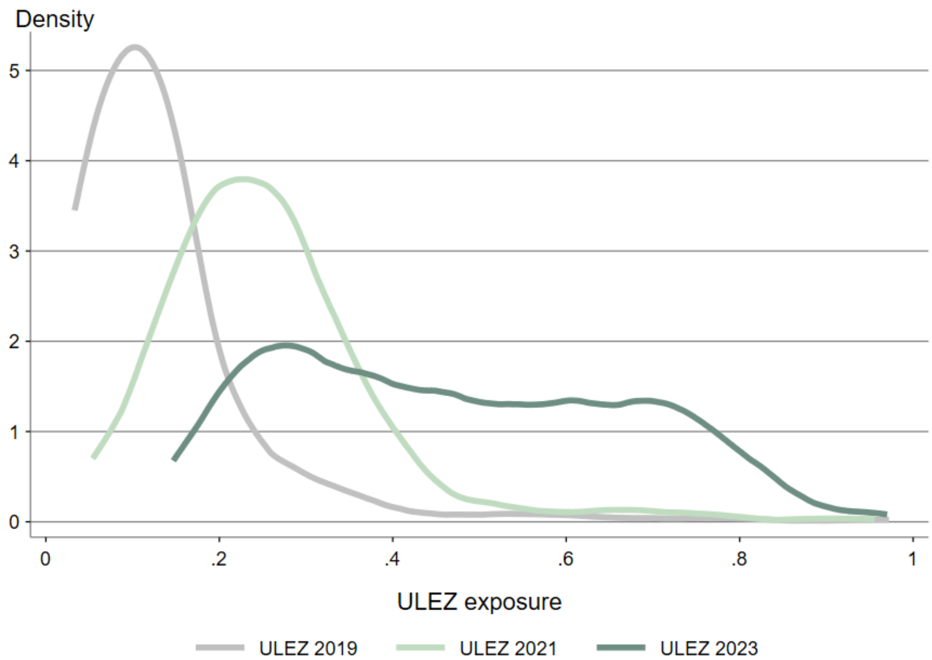


Figure B1 in the appendix maps the ULEZ exposure by postcode district for London in the second quarter of 2019. The widening of the ULEZ leads to (weakly) increasing exposure to the ULEZ for a given postcode district, and more postcode districts becoming exposed to the ULEZ via commuting behaviour. Figure 4 shows that the change on both the intensive and extensive margins has been substantial.

3.2 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.[12] 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.[13]

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 (for instance, 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.[14] We

[12] Figure B2 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 account for a greater *share* of all new registrations, rising to over 1% in 2019 and over 2% in 2021, as seen in Figure B3. The appendix also contains further information on the geographic distribution of ULEV adoption in Greater London.

[13] 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.

[14] TfL, 2019.

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).[15] It inherits firm and establishment postcodes from the IDBR and is accessible through the ONS Secure Research Service (SRS). Recent analysis by the ONS uses establishment postcodes to identify labour reallocation dynamics (ONS, 2023). We compute postcode-level firm exit and entry rates by quarter.[16] Our analysis focuses on postcodes inside the ULEZ boundary and within 1 mile on the outside, and within a three-year window either side of the policy announcement or introduction. The resulting universe of affected firms comprises roughly a quarter of a million observations. Averaged over 2017 - 2021, both entry and exit rates are slightly higher outside the 2019 ULEZ, at 6.6% and 6.9% respectively, relative to 5.9% and 6.6% inside the 2019 ULEZ. Tables A4 and A5 contain summary statistics on entry, exit, and distance from the boundary for our 2019 and 2021 regressions.

4 Empirical approach

Motivated by the comparative statics of our model, we use two different empirical approaches across the four margins of interest. For ULEV investment and public transport use, we use a shift-share approach that assumes only current commuters are affected by the changes. For housing prices and firm location, we use a regression discontinuity design that assumes these margins reflect the net present value of current and future commuting choices, including of potential future residents or workers.

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

[16] Firm exit and entry rates in quarter q are computed as smoothed averages over quarters $q-1, q, q+1$. In beginning and end periods (i.e. where there is no postcode observation in the previous or subsequent period), we compute averages over $q, q+1$ and $q, q-1$ respectively.

4.1 Shift-share event study design

Our first empirical strategy is a shift-share event-study design of the following form:

$$\text{Outcome}_{it} = \alpha_i + \beta_t + \gamma \text{ULEZ}_{it} + \delta_t \text{ShareDriveULEZ}_{i,2011} + \varepsilon_{it} \tag{1}$$

where Outcome_{it} is one of the outcomes of interest in postcode district i, ULEZ_{it} is an indicator that i is within the applicable ULEZ boundary in year t, and $\text{ShareDriveULEZ}_{i,2011}$ is the product of the ULEZ status of i and the share of commuters in i who drive into the applicable ULEZ before the policy is introduced. δ_t is the coefficient of interest.

We use this event-study design for adoption of ULEVs and public transport use. At its core, this approach estimates difference in outcomes (potentially conditional on some covariates) 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, or other spillovers from treated to non-treated units. 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 threats to identification: staggered rollout of treatment; heterogeneous treatment effects; non-parallel trends; multiple treatments; continuous treatment. The difference-in-differences (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 Sant'Anna 2021; Goodman-Bacon 2021), leading to a better understanding of the relevant assumptions in different contexts and the issue that may arise, including the use of "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 satisfies "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 in the appendix.

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 does not vary over time. Given the focus of the recent literature on binary treatments, we also split ULEZ exposure at the median to convert treatment to binary. This allows us to follow the methodologies of Chaisemartin and D'Haultfoeuille (2020), Callaway and Sant'Anna (2021), Gardner (2022), and Clarke, Pailanir, Athey, and Imbens (2023).

To check the validity of parallel trends, we compare the outcomes of treated and untreated groups prior to the treatment date. We also run placebo tests (where we estimate our DiD regressions for synthetic or fake treatment units).

4.2 Regression discontinuity design

For house prices and establishment locations we instead 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 and because for these two margins we expect individuals to take long-term, forward looking decisions, rendering the commuter-based DiD approach unsuitable.

The RDD exploits the quasi-random assignment of treatment status to postcodes near the ULEZ boundary and assumes that postcodes on either side of the boundary are similar across unobservables that affect our outcomes of interest: house prices and firm locations. We estimate:

$$\begin{aligned}\text{Outcome}_{it} = \alpha_i + \beta_t + \gamma \text{ULEZ}_i + \delta \text{DistanceToULEZ}_i \\ + \zeta \text{ULEZintroduced}_t + \eta \text{ULEZ}_i \times \text{ULEZintroduced}_t + \varepsilon_{it}\end{aligned} \tag{2}$$

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 η, 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.

5 Results

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

5.1 ULEV adoption

For brevity, we focus here on the first ULEZ expansion.[17] We cut off the data at the end of 2019 to side-step the impact of the pandemic. Figure 5a presents a scatterplot of the share of ULEVs in a postcode district against the exposure to the initial ULEZ expansion. The relationship is positive and statistically significant; areas more-

[17] Full results for the 2021 expansion are in the appendix. Evaluation of the 2023 expansion is pending due to data availability.

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 5b shows time-varying coefficients from a difference-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.

Table 3 presents the results from our baseline difference-in-differences 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 adopt ULEVs at a higher rate after the policy is introduced.

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

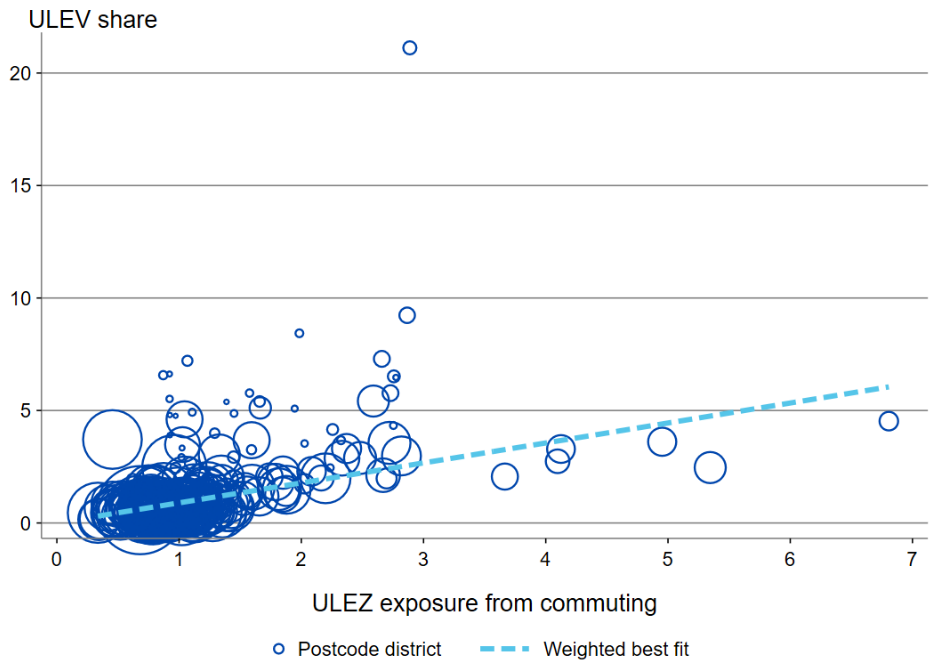
	(1)	(2)	(3)
	Dependent variable: ULEV share		
ULEZ exposure	0.107***	0.096***	0.094***
	(0.028)	(0.017)	(0.019)
ULEZ exposure × Post-indicator	0.608***	0.396***	0.383***
	(0.136)	(0.059)	(0.049)
Weight	None	Vehicles	Population
N	9,326	9,326	9,326
R^2	0.245	0.424	0.554

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.

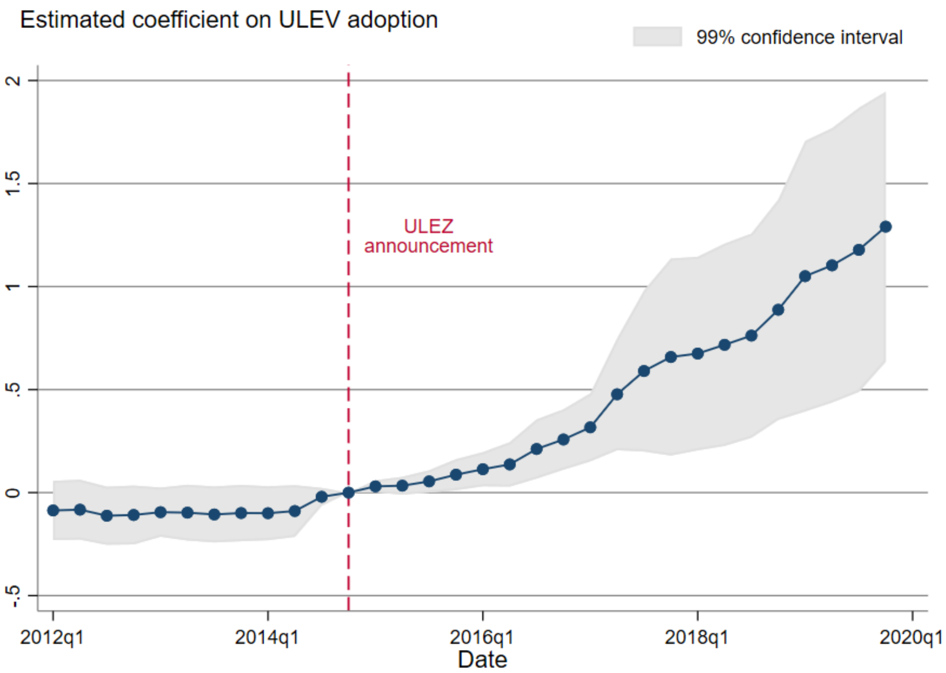
These results suggest an average 0.4 - 0.6% rise in the share of electric vehicles by the end of 2019, for each 1% increase in the share of affected commuters in a postcode district. To put this into context, less than 2% of vehicle registrations in London at the end of 2019 were electric vehicles, and the average ULEZ exposure in London postcode districts is 1.2%. Alternative estimators from the recent literature Chaisemartin and D'Haultfoeuille (2020), Callaway and Sant'Anna (2021), Athey, Bayati, Doudchenko, Imbens, and Khosravi (2021), and Clarke, Pailanir, Athey, and Imbens (2023) in the

Figure 5: ULEV adoption and the 2019 ULEZ

(a) Relationship between ULEV adoption and ULEZ exposure, 2019 Q2.



(b) Event study coefficients on ULEV adoption around 2019 policy announcement.



appendix show qualitatively and quantitatively similar results.

5.2 House prices

Figure 6a shows that, in general, houses inside the ULEZ are more expensive. However, this could be for many reasons, for instance accessibility to amenities. We test a more precise hypothesis: upon introduction, house prices rise more inside than outside the ULEZ boundary within narrow, otherwise similar areas. This hypothesis stems from our model, where 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 6b 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 B13 in the appendix, and shows very similar results.

Table 4 shows these results for a wider range of specifications. Firstly, we regress log house prices on the running variable (that is, distance from the ULEZ boundary) alongside a binary ULEZ indicator, a binary post-policy indicator and their interaction, and 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.[18] 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.[19]

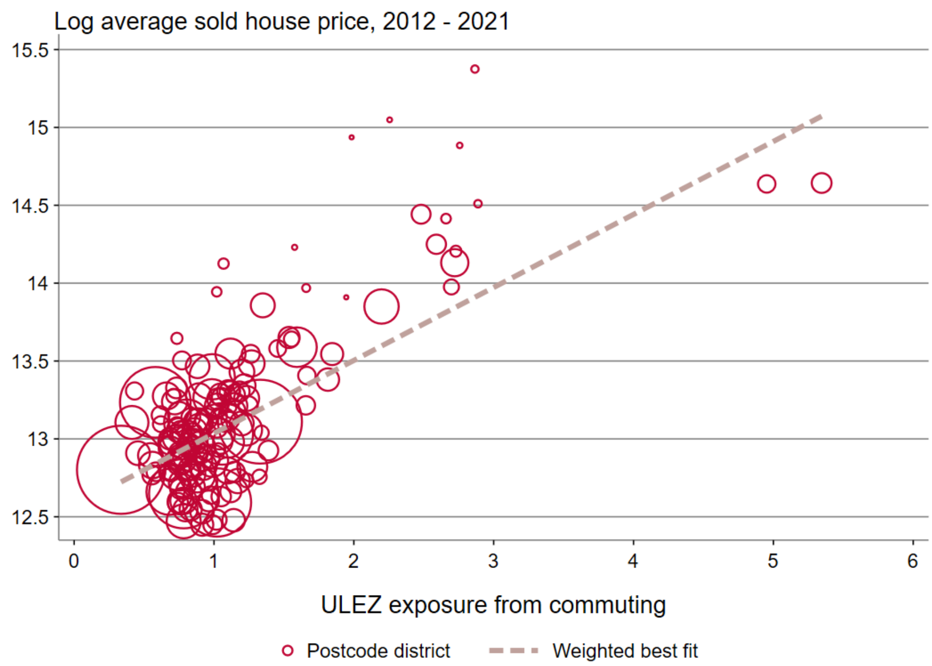
We then run the baseline regression (logged house price on distance from the boundary and an indicator for being inside the zone) in the pre- and post-announcement periods separately. This allows for more flexibility; for example, it allows the coeffi-

[18] An alternative 1-mile bandwidth does not affect the results.

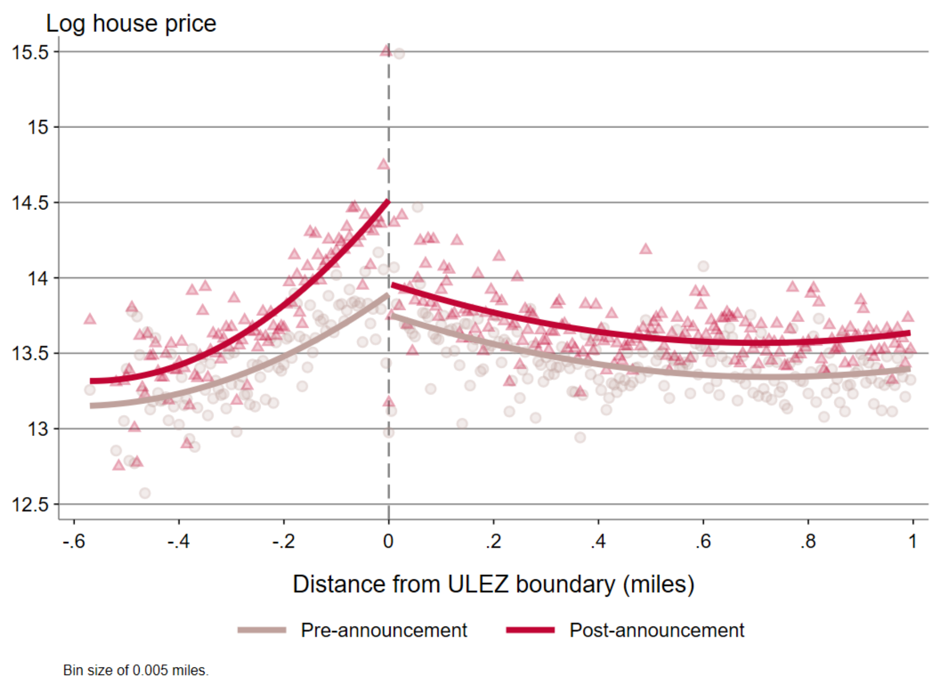
[19] Although a 12% rise in house prices is towards the higher end of estimates in the literature from similar policy changes (Glen and Nellis 2011; Percoco 2014; Tang 2021; Aydin and Rauck 2023; Li and Yang 2023), a back-of-the-envelope calculation suggests it is broadly in the right range for this particular tax. For a commuter paying £12.50 to travel into the ULEZ five times per week for 45 weeks of the year, at a 3% discount rate, the present discounted value of the tax is £93,750. The median price for houses in the ULEZ in 2014 (prior to the policy announcement) was £717,000. The implied value of the resident's exemption would be $93,750/717,000 = 13.1\%$.

Figure 6: House prices and the 2019 ULEZ

(a) Positive relationship between log house price and ULEZ exposure by postcode districts in London.



(b) Binned average log house prices within 1 mile of the 2019 ULEZ boundary, before and after the 2019 policy announcement.



cient 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

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.

the same (year, quarter, 4-digit postcode). The coefficient on the ULEZ indicator is negative in both periods, but significantly less negative after the policy introduction, corresponding to roughly 11.8% higher house prices. This result is consistent with our findings in Table 4. The distribution of 2,500 bootstrapped estimates is plotted in Figure B20 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.

5.3 Public transport

Figure 7a 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, given the simple binary exposure measure and large day-to-day fluctuations in public transport use, the differences are not statistically significant. Figure

7b by contrast shows the coefficients of our usual event-study design, at a monthly frequency, using continuous exposure and taking out common time and location factors. Due to TfL only having released station entry data for a single year (2019), we are limited in the length of the pre-trends we can show. After the introduction of the ULEZ, station entry increased, but with a lag of several months.

Table 5 looks at the public transport margin in our baseline DiD design. We regress the number of station entries at each postcode-by-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 ULEZ dummy and the distance to the ULEZ. Therefore, more exposed postcodes saw increased use of public transport after the policy was introduced.

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

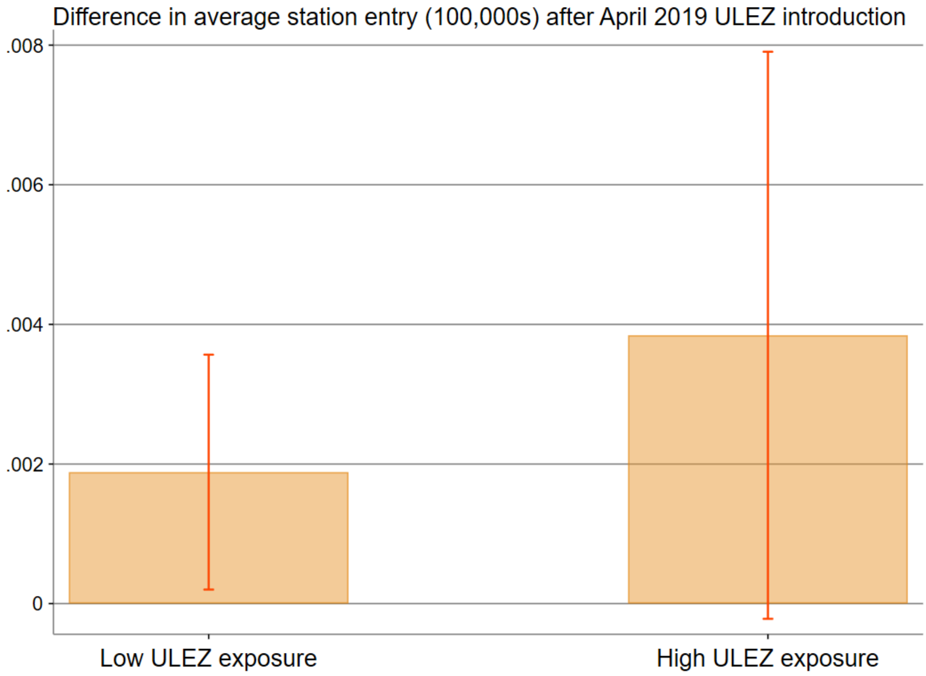
	(1)	(2)	(3)	(4)
	Dependent variable: Station entry/exit (100,000s)			
ULEZ exposure × Post-indicator	0.242***	0.213***	0.220***	0.208***
	(0.069)	(0.069)	(0.078)	(0.078)
ULEZ exposure	3.834***	1.869*	3.982***	1.893
	(1.216)	(1.121)	(1.269)	(1.158)
Post-indicator	0.000	0.001	0.000	0.001
	(0.001)	(0.001)	(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.

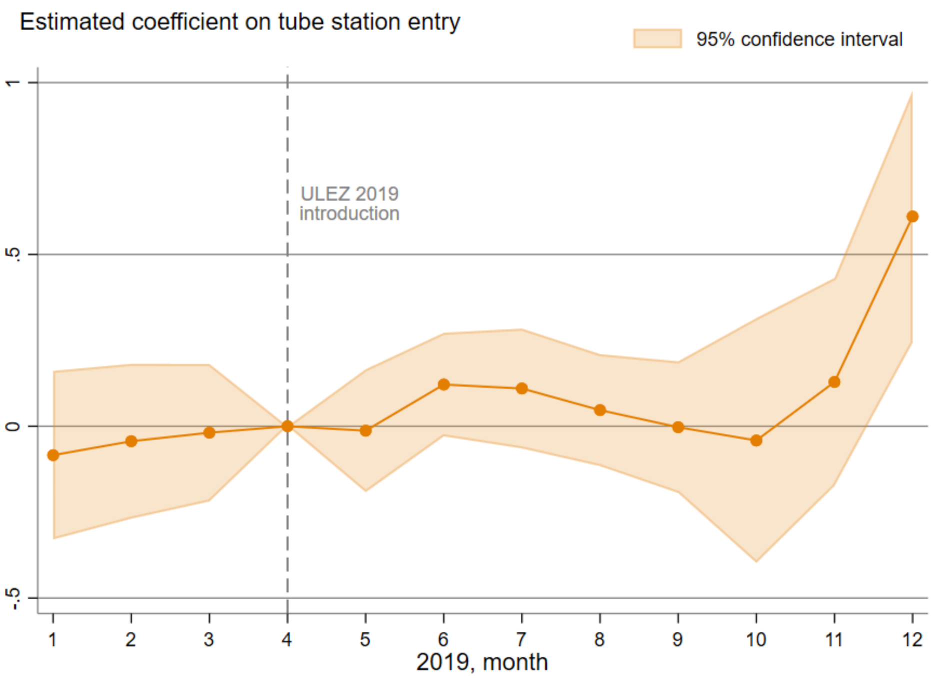
Another way to see this result is via the predicted interaction effects in Figure B14. This graph shows the predicted station entry from the model in column (1) of Table 5. While the coefficient looks small, the difference amounts to over 250 additional

Figure 7: Tube entry and the 2019 ULEZ

(a) Average change in London underground station entry after the 2019 ULEZ introduction, split by postcodes with above and below median ULEZ exposure.



(b) Event study coefficients on tube station entry around 2019 ULEZ introduction.



station entries per day at an average postcode, or a rise of around 2.3% in London underground use.[20]

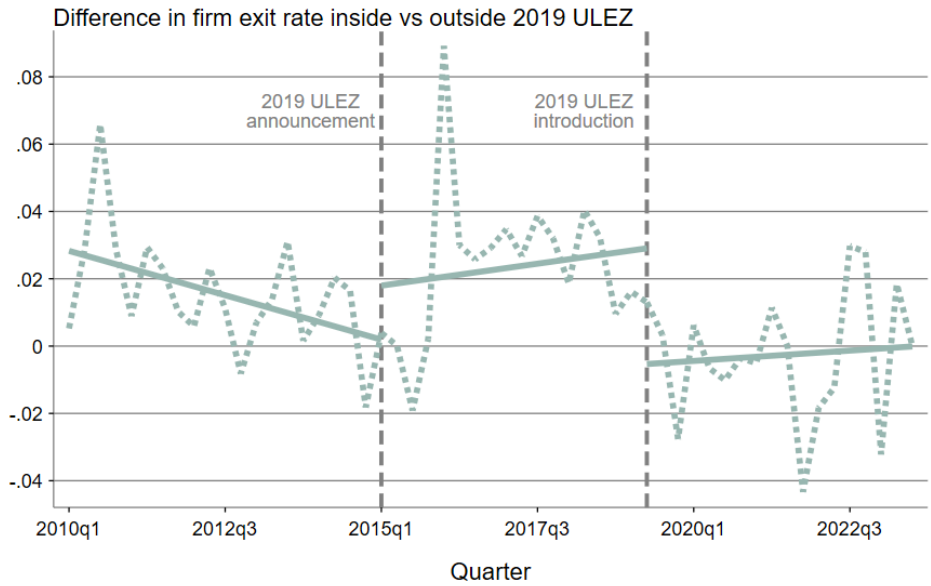
[20]Station entry and exit is recorded in 100,000s. A coefficient of 0.21 multiplied by the average exposure to the ULEZ (0.0121) equals 0.00254, or 254 daily passengers. The average daily station

5.4 Firm location

Figure 8 shows the difference in firm exit rates over time for postcodes inside and outside the 2019 ULEZ boundary. We regressed postcode-level firm exit rates on year and quarter fixed effects, and used the resulting residuals, weighted by the number of firms in each postcode. We fit linear trends before and after the announcement and before the policy introduction.

Firm exit rose *inside* the ULEZ, relative to just outside, after the policy was announced. This is suggestive evidence that firms left areas within the ULEZ at higher rates, after learning of the policy. Once the policy was introduced, firm exit rates fell inside the boundary, such that there was little difference inside compared to just outside.

Figure 8: Firm exit rates rose inside the ULEZ boundary (relative to outside) after the announcement



Data from Longitudinal Business Database (LBD). Firm exit rates computed at postcode level, residualised to account for year and quarter fixed effects. Dashed line represents firm exit inside vs outside the zone. Solid line is a linear trend within each time period. Difference computed for postcodes within 1 mile of 2019 ULEZ.

Figure 9 shows the regression discontinuity visually. Focusing on one mile either side of the boundary, we conduct a simple RDD before and after the policy introduction. Firm exit is residualised to control for quarter and year fixed effects, then

entry/exit at each postcode is approximately 11,200 (see Table A2). This implies an average effect of $254/11,200 \approx 2.28\%$.

aggregated across 0.1 mile bins, weighted by the pre-announcement number of firms. Firm exit rates rise inside the ULEZ boundary, relative to outside, after the policy is introduced. This is in contrast to the interpretations from the simple trendlines above.

Figure 9: Regression discontinuity plot for firm exit around ULEZ boundary, before and after policy introduction

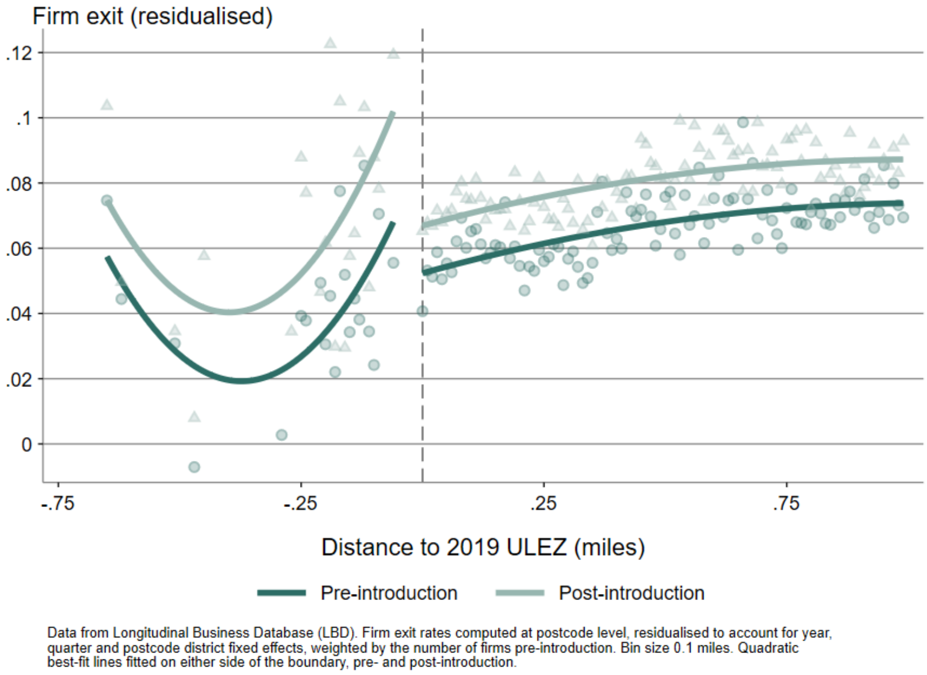


Table 6 shows similar results across several specifications. We regress firm exit rates on an indicator for the ULEZ, a post-introduction indicator and their interaction, along with the running variable which is the distance from the boundary. We include year, quarter and postcode district fixed effects and focus on within 1 mile of the boundary. The three columns of Table 6 vary the specification with different weights. Triangular weights simply place a decreasing weight as we move further from the boundary. Once we weight by the number of firms in each postcode (with or without triangular weights for distance), firm exit rates rise by around 3.5 percentage points, relative to outside, after the policy was introduced in 2019. Results for firm entry are in Table C8 in the appendix, indicating a 1.3 percentage point rise in firm entry, relative to outside, after the policy was introduced in 2019 (suggesting a general rise in business dynamism).

Table 6: Baseline firm exit rate RDD for 2019 ULEZ boundary.

	(1)	(2)	(3)
		Dependent variable: Firm exit rate	
ULEZ	-0.0050	-0.0096	-0.0028
	(0.0049)	(0.0091)	(0.0079)
ULEZ × Post-indicator	0.0085	0.0350**	0.0345**
	(0.0054)	(0.0164)	(0.0159)
Distance	0.0105**	0.0195	0.0440
	(0.0040)	(0.0136)	(0.0309)
Weight	Triangular	# firms	Combined
N	259,590	245,189	245,189

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

5.5 ULEZ expansions

The ULEZ has been further expanded twice, in 2021 and 2023, as depicted in Figure 2. This section presents core results for the 2021 expansion, and compares them to the 2019 introduction. We intend to update with results for the 2023 expansion shortly, when data becomes available.

In 2021, we find a positive impact on ULEV adoption for postcode districts more exposed to the ULEZ expansion, a negative impact on house prices for properties just inside the ULEZ expansion compared to those outside, and a positive impact on public transport use in more exposed postcodes. Table C2 shows the baseline DiD for ULEV adoption. There is a 3-5% increase in the adoption of ULEVs for each additional percentage of ULEZ exposure due to commuting, after the announcement of the 2021 ULEZ expansion. Figure B19 in the appendix contains the baseline event study.

Table C5 shows the equivalent results for house prices. We find a statistically significant and stable negative coefficient for house prices within the 2021 ULEZ boundary after the expansion, with a magnitude between 3.8-5.8% depending on weighting and the distance from the zone. This estimate may superficially seem to contradict the estimates from the 2019 ULEZ introduction (a roughly 12% increase in house prices inside the ULEZ). However, the 2021 expansion removed the exemption for ULEZ residents, and thus the 2021 estimates here are entirely consistent with both the earlier

2019 results and the comparative statics from our model. These results are robust to varying the RDD bandwidth (Table C7) or using a "donut" RDD (Table C6). Table C4 contains the baseline DiD regression for public transport use. Both station entry and exit in more exposed postcodes experience a statistically significant increase after the 2021 ULEZ expansion. Table C9 presents the baseline RDD for firm exit and entry rates after the 2021 ULEZ expansion, finding negative coefficients across all specifications that are not statistically significant.

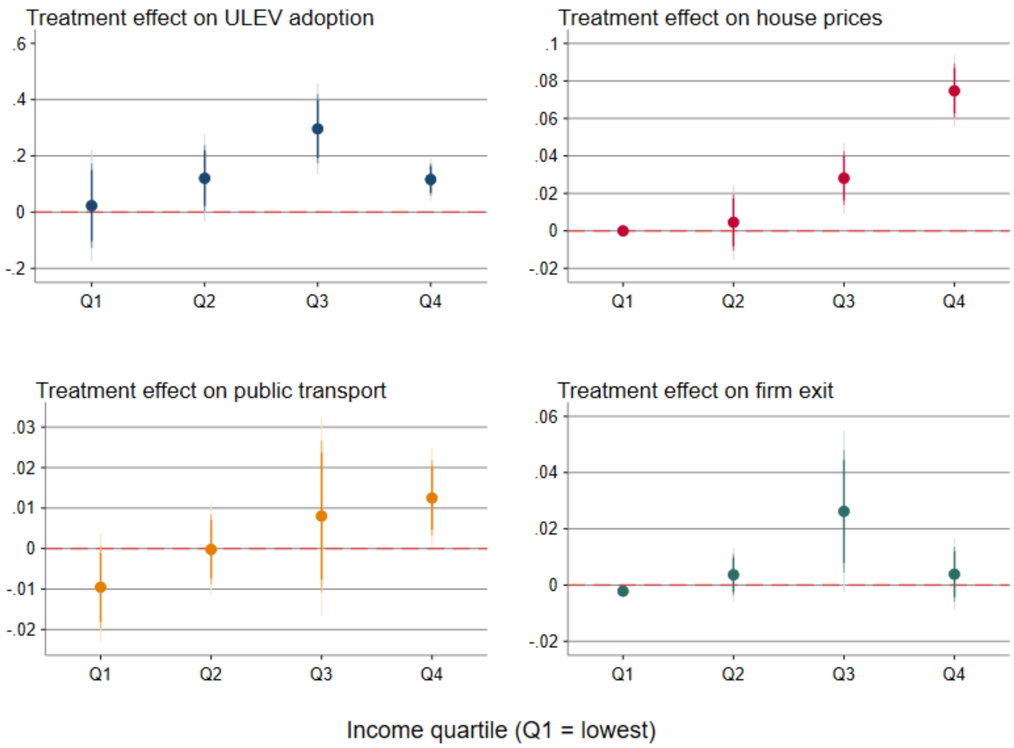
5.6 Income heterogeneity

Average income in London postcodes and postcode districts exhibits substantial heterogeneity. We obtain regional income data from before the first ULEZ announcement.[21] We compute the mean 2014 net income (before housing costs) at the postcode and postcode district level, using a MSOA crosswalk. Postcodes and postcode districts are assigned to quartiles, and we repeat our analysis on all four margins across the income distribution.

Figure 10 shows our main standardised coefficients of interest, estimated separately by quartile of the Greater London income distribution. We find that ULEV adoption, house prices and public transport use are more responsive to the ULEZ introduction at higher levels of income. This suggests that low-income commuters are more likely to simply pay the tax and not alter their commuting behaviour. Firm exit also varies by local income, although the effect is only statistically significant for the lowest and third income quartiles. In the context of our model, this might be expected as location *productivity*, not income matters for firm location decisions. Figure B33 in the appendix repeats this exercise for the 2021 expansion. For the most part, the gradient across income quartiles is similar, if attenuated. This suggests some degree of anticipatory adjustment behaviour.

[21] See ONS, 2023.

Figure 10: Estimated elasticities from the initial 2019 ULEZ introduction, across income quartiles



Standardised coefficients for 2019 ULEZ introduction across four margins, with heterogeneity based on 2014 regional net income quartiles.

5.7 Robustness checks

We implement several robustness checks. Where we use a difference-in-differences event-study design (ULEV adoption and public transport use), we implement the following tests: (1) placebo tests; (2) tests for pre-treatment trends; (3) robust alternative estimators following Chaisemartin and D'Haultfoeuille (2020) and Callaway and Sant'Anna (2021); (4) synthetic difference-in-differences following Clarke, Pailanir, Athey, and Imbens (2023); and (5) a matrix completion approach following Athey, Bayati, Doudchenko, Imbens, and Khosravi (2021). Where we use a regression discontinuity design (house prices and firm entry), we instead implement the following tests: (1) variation of interval size; (2) flexible treatment of the running variable; (3) different triangular weightings; (4) triple interactions between treatment, time and distance; (5) alternative functional forms; and (6) a "donut" design.

Our results are robust to these alternative methodological choices. We also show suggestive, descriptive evidence on ULEZ impacts on working-from-home behaviour, CO2 emissions and traffic flows.

ULEV adoption. We implement three 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 think should be unrelated to the treatment; the number of total vehicles per capita. Both placebo tests in Figure B22 show no effect, so they provide supportive evidence that there is a real effect of the ULEZ announcement on ULEV adoption. Finally, we run classic TWFE time-varying estimator on the pre-announcement data, with a fake treatment date in 2013 Q2. Figure B21 shows no evidence of an effect.

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 Sant'Anna 2021; J. Roth, Sant'Anna, Bilinski, and Poe 2023). The result is plotted in Figure B23, 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, which requires making the ULEZ exposure binary with a cut-off at the median value. The results are shown in Figure B8. Given the binary treatment, the estimated coefficients are smaller in size, but the interpretation of the magnitude of the effects is very close. Similar results are shown with the Callaway and Sant'Anna (2021) approach in Figure B9. 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 from 50 bootstrap replications. This is approximately in line with a weighted average that might be expected from Figure B8. Finally, 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 B10. There's no evidence of pre-trends and the estimated time-varying ATTs are very much in line with our estimates from other methods.

Public transport. We randomly assign postcodes to the ULEZ and re-run the baseline regression. Table C3 shows no effect of the fake ULEZ on station entry and exit. In addition, we test for pre-trends by running an event study regression on pre-treatment periods only (Callaway and Sant'Anna 2021; J. Roth, Sant'Anna, Bilinski, and Poe 2023). Figure B24 shows this result, with evidence supporting our empirical approach. We cannot reject the null hypothesis of parallel pretrends.

We consider alternative event study estimators, such as Callaway and Sant'Anna (2021) or Chaisemartin and D'Haultfoeuille (2020), using a binary treatment variable by splitting at the median exposure to the ULEZ. This is necessary because such estimators require binary treatment categorisation, but naturally we lose variation in the treatment variable for identifying the causal effect. The results show limited effects on a monthly basis. These results are plotted in Figures B25 and B26. The synthetic DiD ATT is estimated at 0.0013 with a standard error of 0.0013 from 50 bootstrap replications. Finally, the matrix completion method of Athey, Bayati, Doudchenko, Imbens, and Khosravi (2021) also shows no effect on station entry over time, shown in Figure B27.

House prices. We repeat our baseline RDD for a range of distances to the ULEZ boundary. Figure B28 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. Additionally, Table C1 contains results that 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 C1 also 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.

Firm location. We adjust the bandwidth in the baseline RDD around the 2019 boundary in Table C8. The RDD coefficient is statistically significant at the 5% level for both firm entry and firm exit across all bandwidths considered. The coefficient falls slightly in magnitude as the bandwidth is expanded from 1 mile to 5 miles, from around 1.3% to 1.0% for firm entry and 3.5% to 2.8% for firm exit. Our results are also robust to a "donut" RDD (as we did for house prices above); Table C8 shows that coefficients for both firm entry and firm exit rise in magnitude to 1.4% and 3.7% respectively and remain statistically significant. A "triple-interaction" specification in Table C8 is only statistically significant at the 10% for firm exit rates, but the magnitude is similar at 4.0%. Finally, we run a placebo test on a fake ULEZ boundary in Table C8, with negative coefficients on firm entry and firm exit and no statistical significance for the weighted specifications. We also present results on how firm entry rates respond to the 2021 ULEZ expansion and heterogeneity across income quartiles in Figure B17.

Working from home. An alternative margin of adjustment to the introduction of the ULEZ is to avoid commuting entirely, by choosing to work from home (WFH). Unfortunately, this margin is difficult to evaluate due to a lack of data pre-2018, a high level of regional aggregation for the data that is available, and the impact of the pandemic on WFH patterns.

We obtain data on WFH shares from Fraja, Matheson, Mizen, Rockey, Taneja, and Thwaites 2021, which is at the NUTS3 level from 2018 to 2020. The limited panel and high level of regional aggregation restricts our analysis. Figure B31 plots the WFH over time for NUTS3 regions that have any overlap with the 2019 ULEZ. There is a steady increase in WFH share both inside and outside the zone. The increase from 2018 to 2019 is larger outside the ULEZ (both in absolute and relative terms), providing suggestive evidence of a greater rise in WFH for regions outside the zone. Furthermore, Figure B32 shows a weak positive correlation between WFH shares in 2018 and commuting exposure to the 2019 ULEZ, with the latter explaining around 17% of the variation in WFH patterns.

CO2 emissions and traffic. We focus on the impact the introduction and subsequent expansions have had on the economic geography of commuting in Greater London. We therefore relegate descriptive evidence on the impact of the ULEZ on CO2 emissions and traffic levels to the online appendix.

6 Discussion

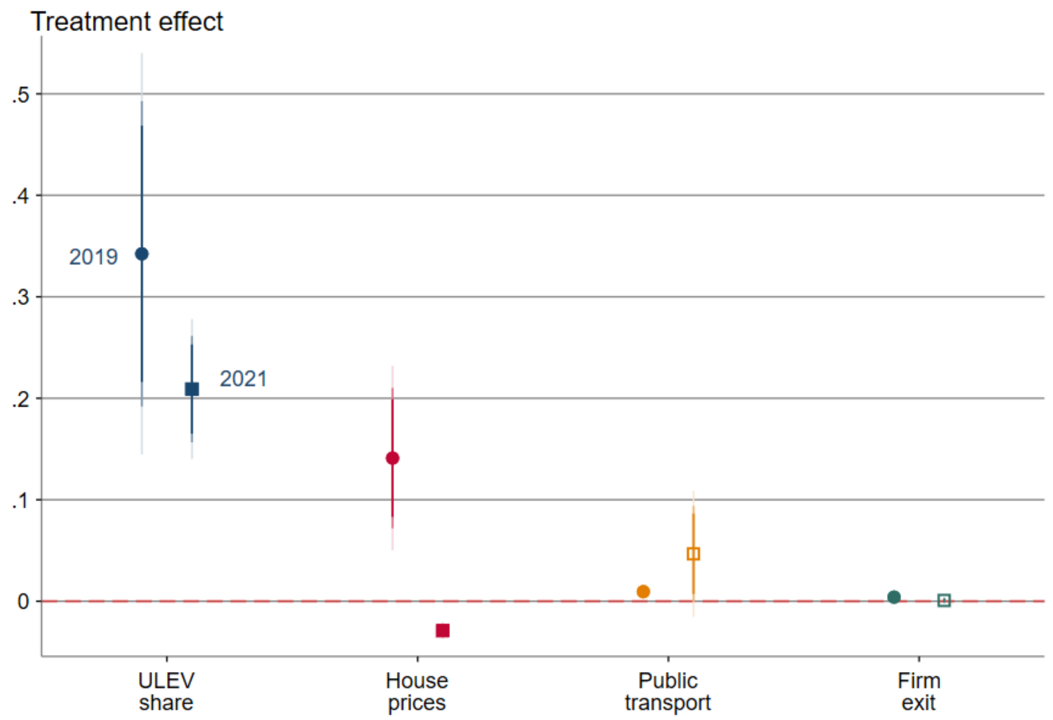
This section discusses the relative magnitudes across margins, expansions, income quartiles and in relation to other studies. Our baseline estimates are plotted in Figure 11. On average, the elasticities are largest for ULEV investment, but they are large and statistically significant across other margins too. Although the policy works as intended (there is evidence of switching to low-emissions vehicles and public transport) other substitution behaviour also occurs: house prices rise more for some Londoners than for others, and economic activity relocates outside of the ULEZ.

Figure 11 plots our baseline coefficients across the four margins of interest for the 2019 ULEZ introduction and the 2021 ULEZ expansion, respectively. Over time, responses fall on all margins.[22] This suggests that once a policy is understood, people will adjust their behaviour in anticipation. Separate regressions across income quartiles suggest that initially high-income individuals were able to adjust most effectively

[22]For house prices, the *sign* also changes in 2021 compared to 2019. This is consistent with our model, as individuals living inside the ULEZ lose their exemption in 2021.

while low-income individuals mostly paid the commuter tax. Over time, this difference too seems to have attenuated.

Figure 11: A comparison of 2019 and 2021 ULEZ expansion elasticities



Standardised coefficients for 2019 ULEZ and 2021 expansion across four margins. Filled symbols are statistically significant at the 95% confidence level.

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 (2024) 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 Rauck (2023) investigate the impact of the tightening of Berlin's Low Emission Zone (LEZ). They find a sizeable increase of around 5% in prices 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). Aydin and 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.[23]

Finally, Davis (2008) studies the introduction of a one-weekday travel ban in Mexico City, *No Hoy Circula*. While the focus of the study is predominantly on air pollution, the author finds no effect on car use over the course of the week, no effect on public transport use or taxi use, and if anything a substitution towards more polluting vintages. Across these studies, the evidence suggests that policy design details matter greatly for the exact impact on substitution behaviour.

7 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. London's Ultra Low Emissions Zone (ULEZ) is a high-profile case in point. But 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.

[23] Germany, specifically Berlin, has strong restrictions on rent price growth, which likely interact with other policies such as LEZs (Thomschke 2016).

We show that the the introduction of the ULEZ led to a large, positive and significant increase in the adoption of ultra-low emissions vehicles, as well as an increase in public transport use in more exposed areas. We also find a large positive increase in house prices within the ULEZ, compared to those on the other side of the boundary. Finally we show a rise in firm exits inside the ULEZ, relative to outside the ULEZ, in response to the policy introduction. The policy affected Londoners at different income levels differently. The expansion of the ULEZ in 2021 resulted in qualitatively similar but quantitatively much smaller effects, suggesting anticipatory adaptive behaviour. 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 (2024).

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 setting 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 Data appendix

Please see the Online Appendix for additional results.

Table A1: 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

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

Table A2: Summary statistics - station entry and exit

	Mean	N	StDev	Min	p(25)	p(50)	p(75)	Max
Station entry	0.111	134,955	0.182	0	0.025	0.053	0.115	1.914
Station exit	0.113	134,955	0.189	0	0.023	0.051	0.114	1.868
Distance to boundary	4.51	134,955	3.63	0.099	1.82	3.50	6.48	18.24
ULEZ	0.081	134,955	0.272	0	0	0	0	1
Population	40,701	134,955	24,932	0.065	24,325	40,653	53,032	140,711
Taxable Share ULEZ	0.012	134,955	0.0075	0.0046	0.0079	0.0097	0.013	0.068

2019 daily station entry and exit from Transport for London. Commuting and population data from 2011 Census. Constructed variables computed by authors.

Table A3: 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.

Table A4: Summary statistics - firm dynamism for 2019 ULEZ

	Mean	*Median*	*StDev*	*N*
Panel I: outside 2019 ULEZ				
Entry rate	0.069	0.000	0.191	3,764,776
Exit rate	0.068	0.000	0.188	3,764,776
Distance	9.655	8.313	7.034	3,764,776
Panel II: inside 2019 ULEZ				
Entry rate	0.057	0.000	0.151	1,809
Exit rate	0.063	0.000	0.145	1,809
Distance	-0.177	-0.141	0.118	1,809
Panel III: all postcodes				
Entry rate	0.069	0.000	0.191	3,766,585
Exit rate	0.068	0.000	0.188	3,766,585
Distance	9.650	8.310	7.036	3,766,585

Average quarterly postcode-level firm entry and exit rates from 2017 - 2021, for postcodes within and outside the 2019 ULEZ boundary. Distance to boundary in miles, computed by authors.

Table A5: Summary statistics - firm dynamism for 2021 ULEZ

	Mean	*Median*	*StDev*	*N*
Panel I: outside 2021 ULEZ				
Entry rate	0.061	0.000	0.180	2,995,124
Exit rate	0.064	0.000	0.182	2,995,124
Distance	5.528	3.406	6.037	2,995,124
Panel II: inside 2021 ULEZ				
Entry rate	0.059	0.000	0.163	6,403
Exit rate	0.066	0.000	0.172	6,403
Distance	-0.390	-0.400	0.175	6,403
Panel III: all postcodes				
Entry rate	0.061	0.000	0.180	3,001,527
Exit rate	0.064	0.000	0.182	3,001,527
Distance	5.515	3.391	6.037	3,001,527

Average quarterly postcode-level firm entry and exit rates from 2019 - 2023, for postcodes within and outside the 2021 ULEZ boundary. Distance to boundary in miles, computed by authors.

B Additional figures

Figure B1: ULEZ exposure by postcode district

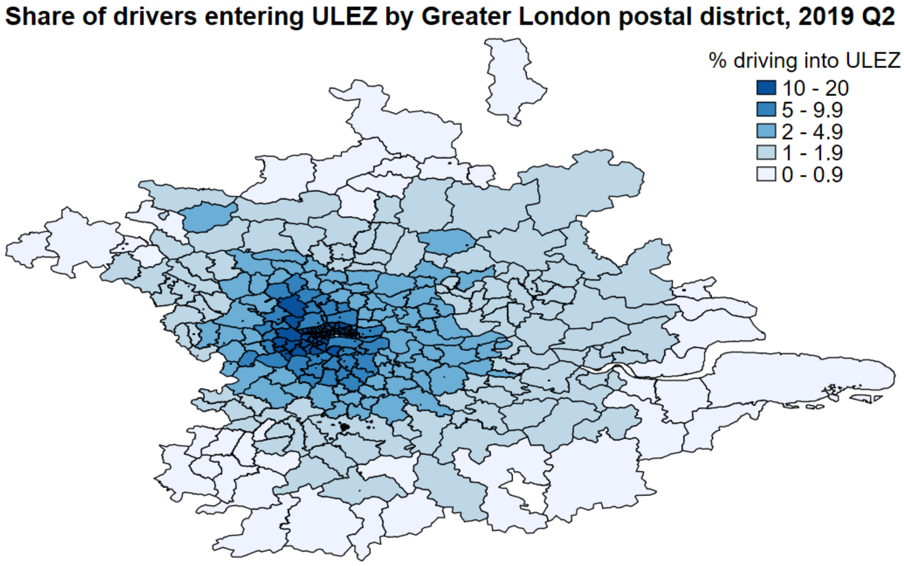


Figure B2: Adoption of ultra-low emissions vehicles in London

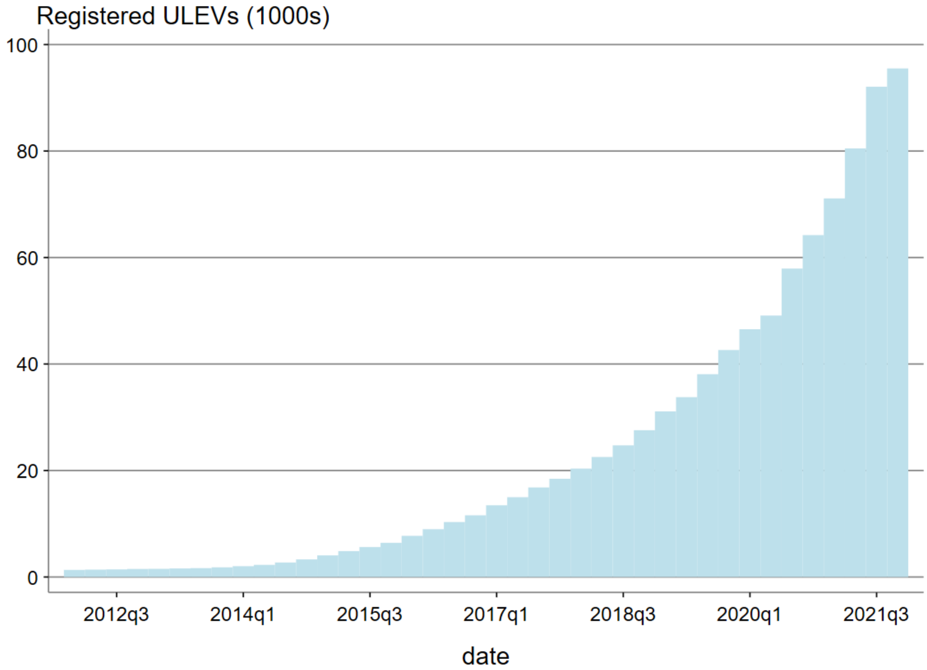


Figure B3: Adoption of ultra-low emissions vehicles in London

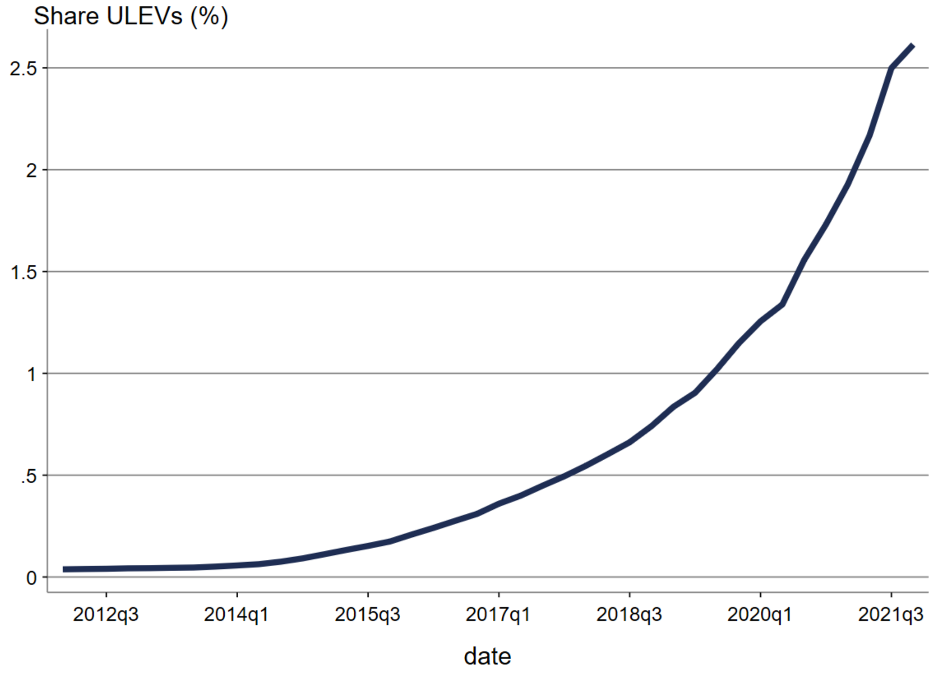


Figure B4: Adoption of ultra-low emissions vehicles in high and low ULEZ exposure postcode districts for the 2021 expansion

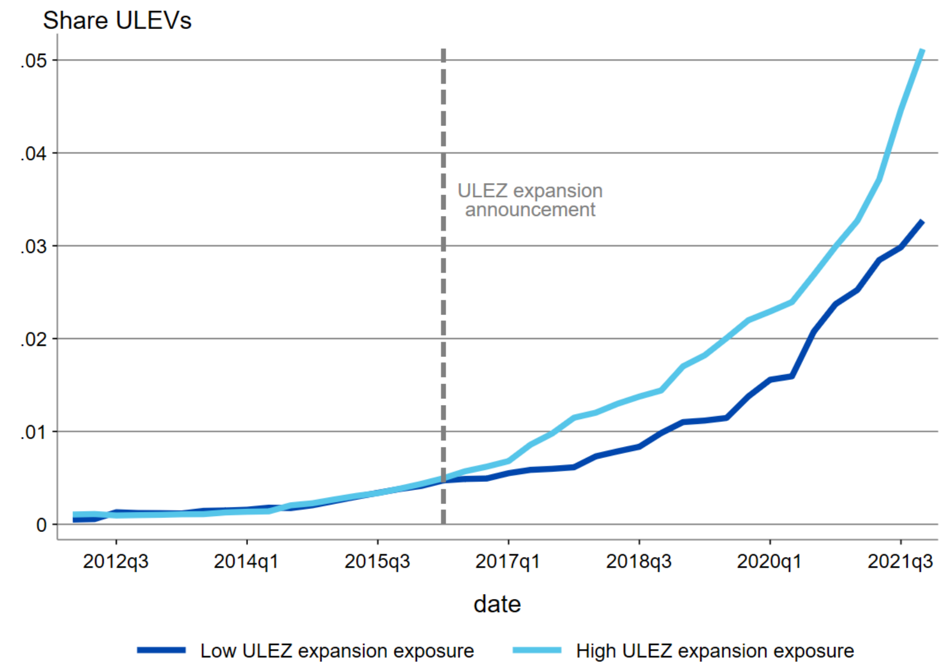


Figure B5: Histogram of computed ULEZ exposures for London's postcode districts

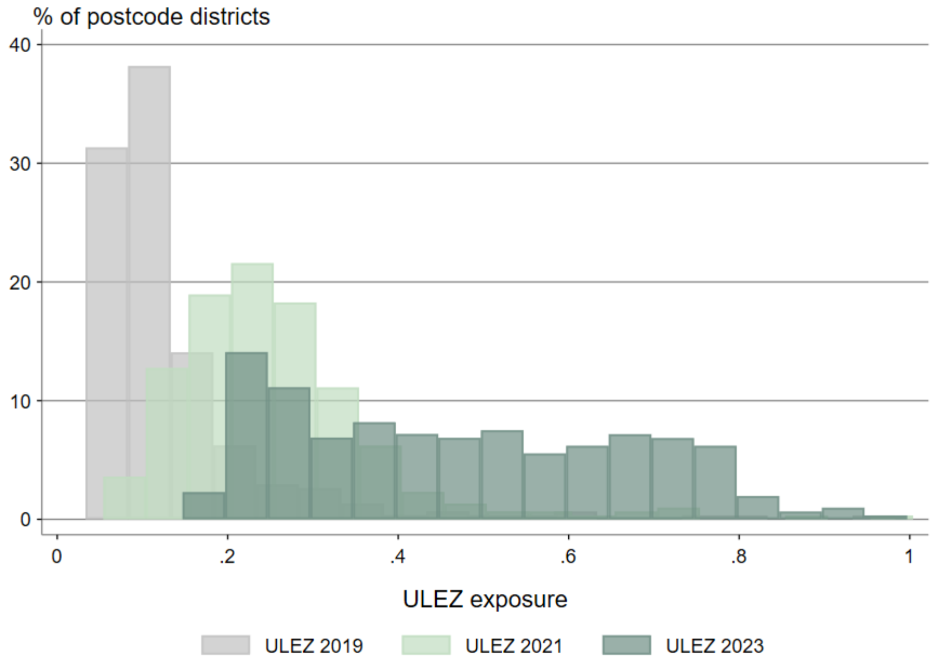


Figure B6: ULEV adoption by postcode district

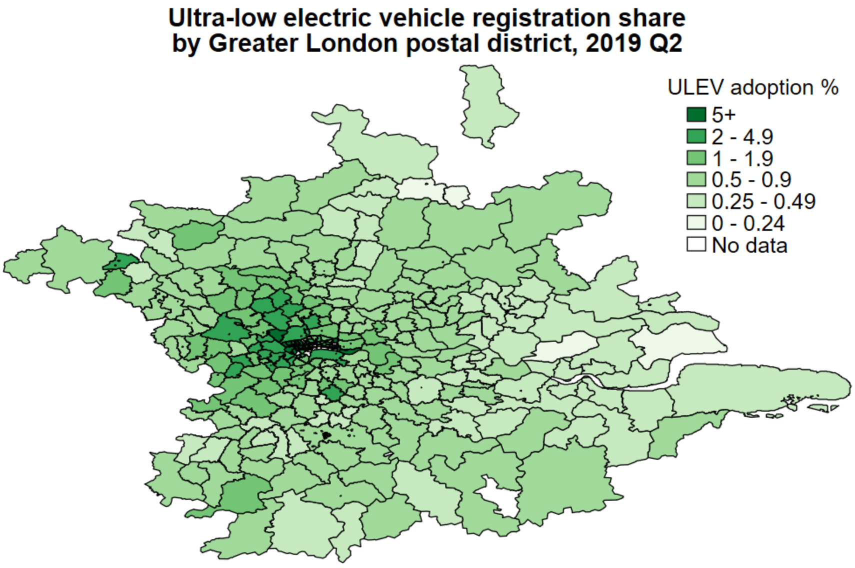


Figure B7: Google Trends web search for ULEZ London

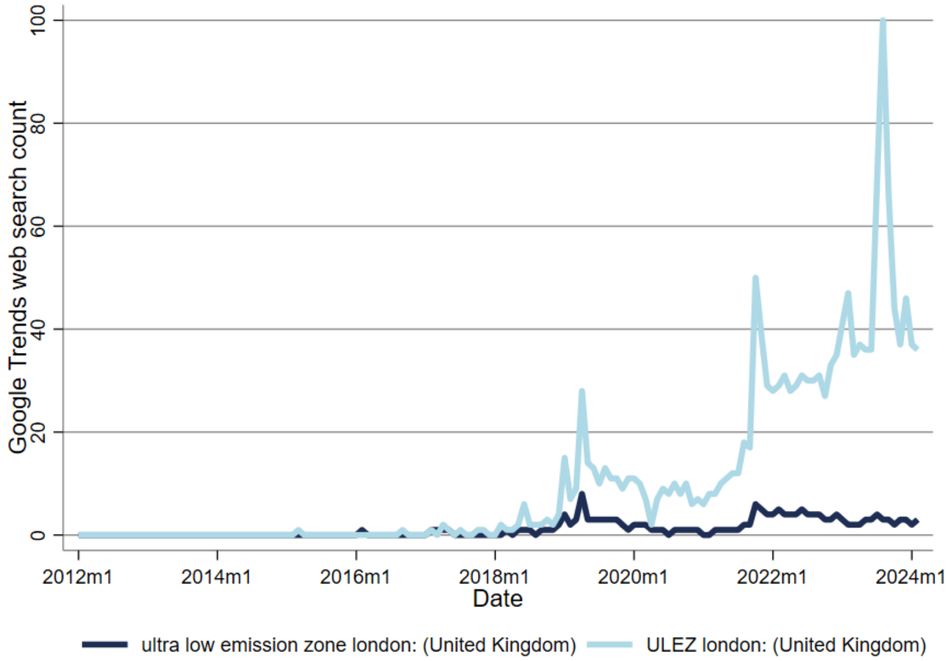


Figure B8: Chaisemartin and D'Haultfoeuille 2020 event study for ULEV adoption around first ULEZ announcement (Q1 2015)

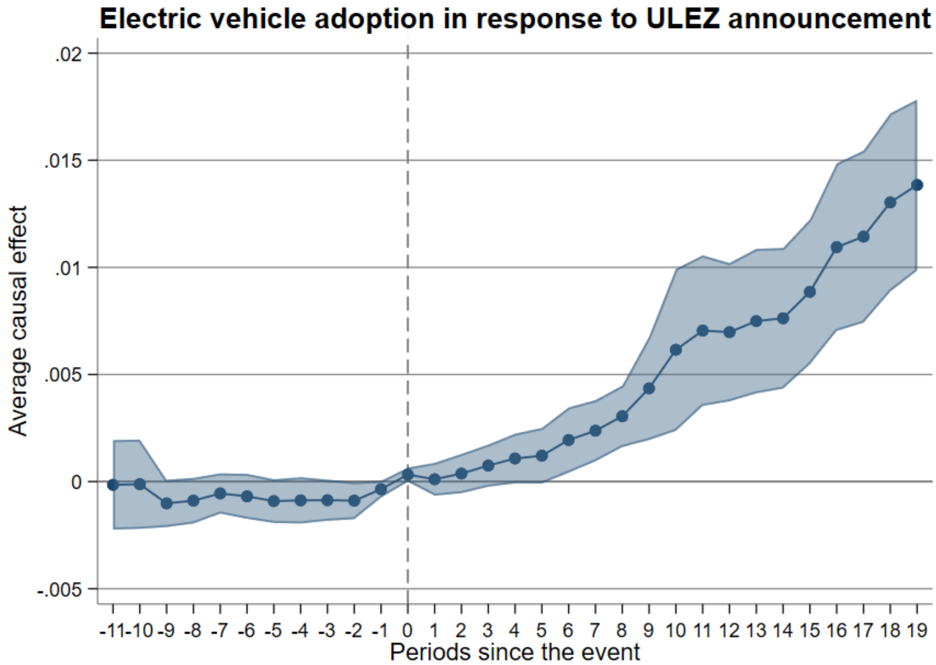


Figure B9: Callaway and Sant'Anna 2021 event study for ULEV adoption around first ULEZ announcement (Q1 2015)

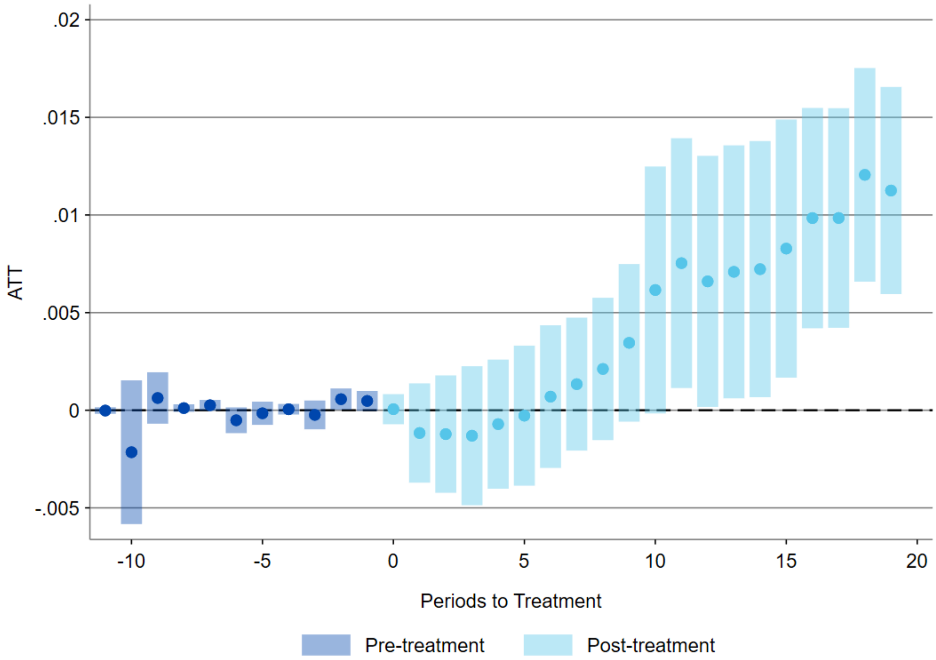


Figure B10: Athey, Bayati, Doudchenko, Imbens, and Khosravi 2021 matrix completion method to estimate event study for ULEV adoption around first ULEZ announcement (Q1 2015)

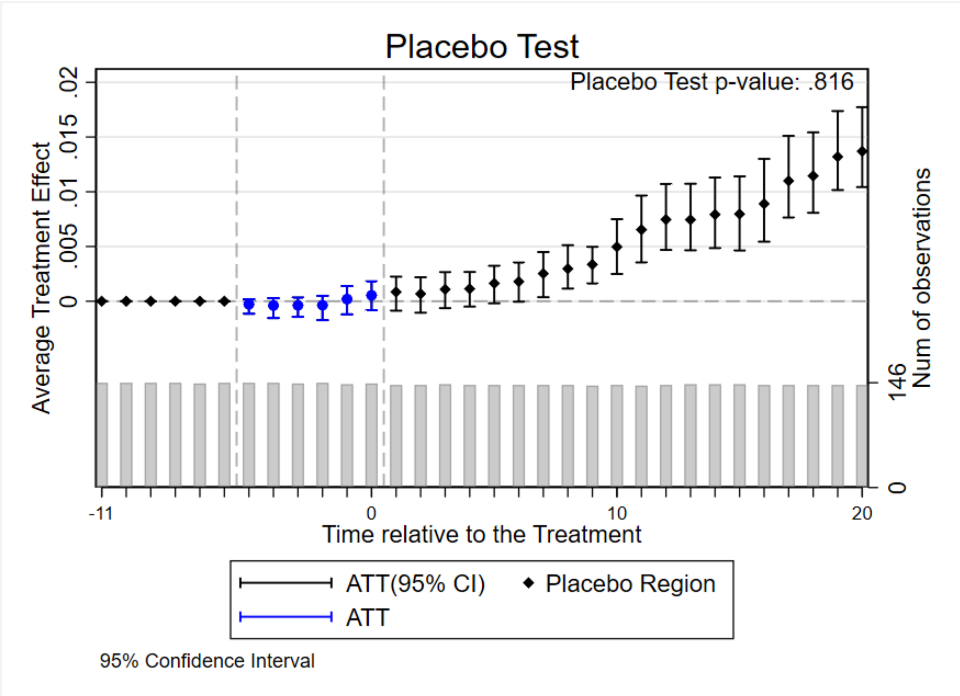


Figure B11: Price of sold houses and number of transactions in London

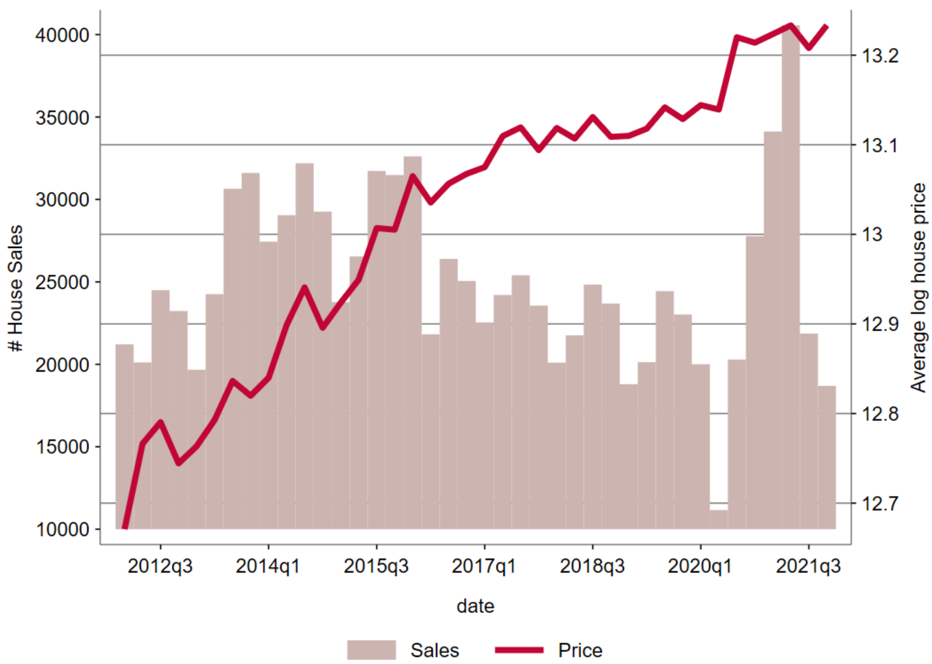


Figure B12: Average log house price in high and low ULEZ exposure postcode districts in London

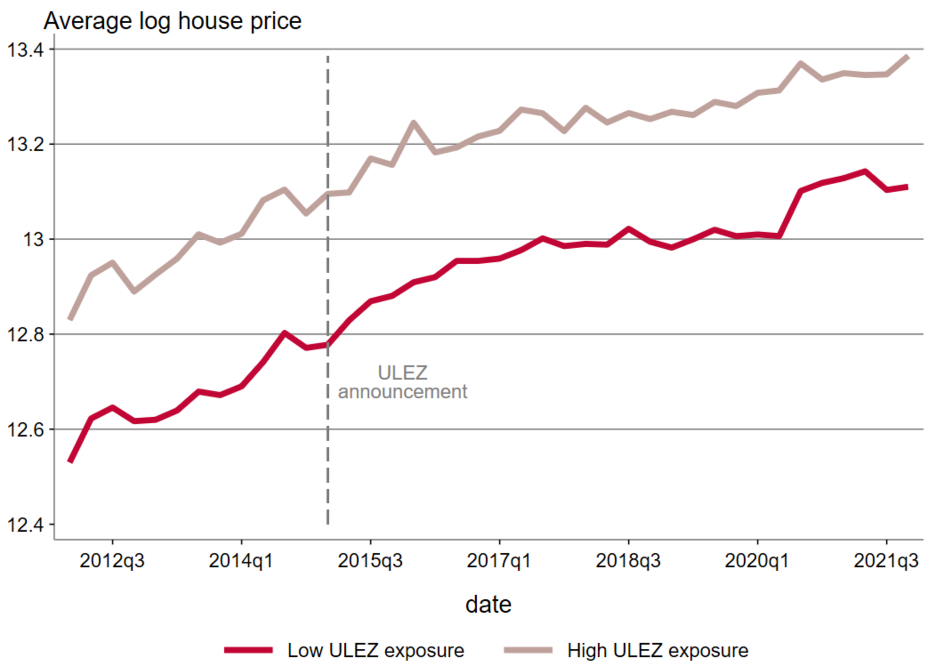


Figure B13: Binned average log house prices within 1 mile of the 2019 ULEZ boundary, before and after the policy announcement in March 2015.

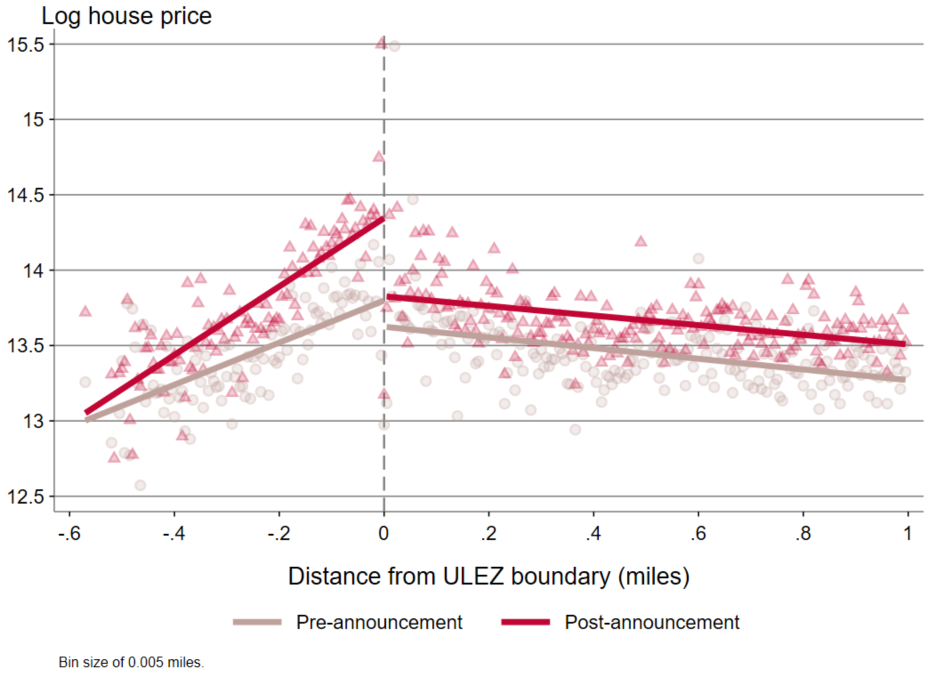


Figure B14: Marginal predicted effect of ULEZ exposure on station entry, pre- and post-policy introduction.

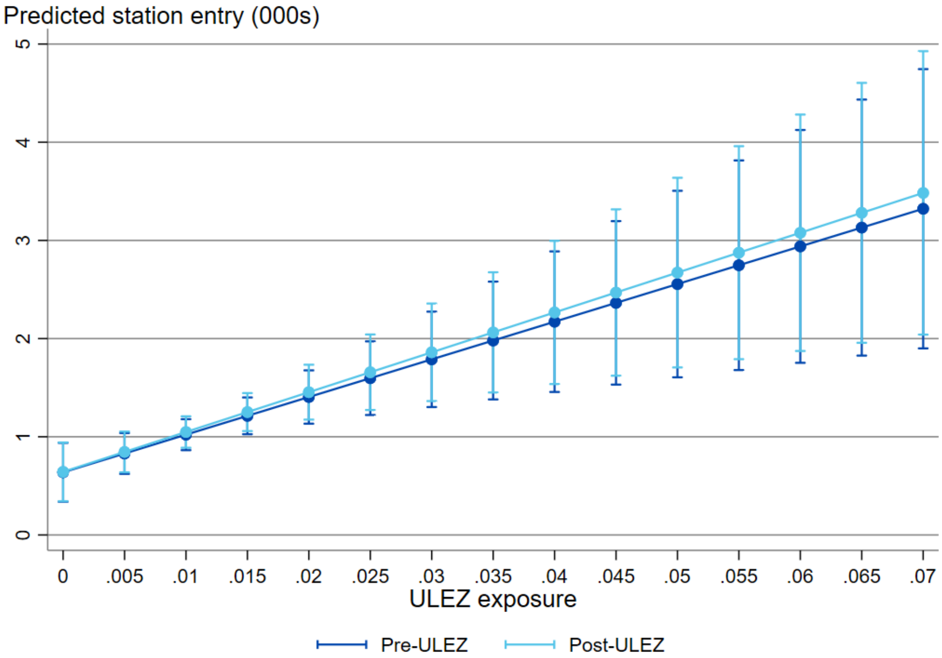


Figure B15: Firm entry rates fell inside the ULEZ boundary (relative to outside) after the announcement

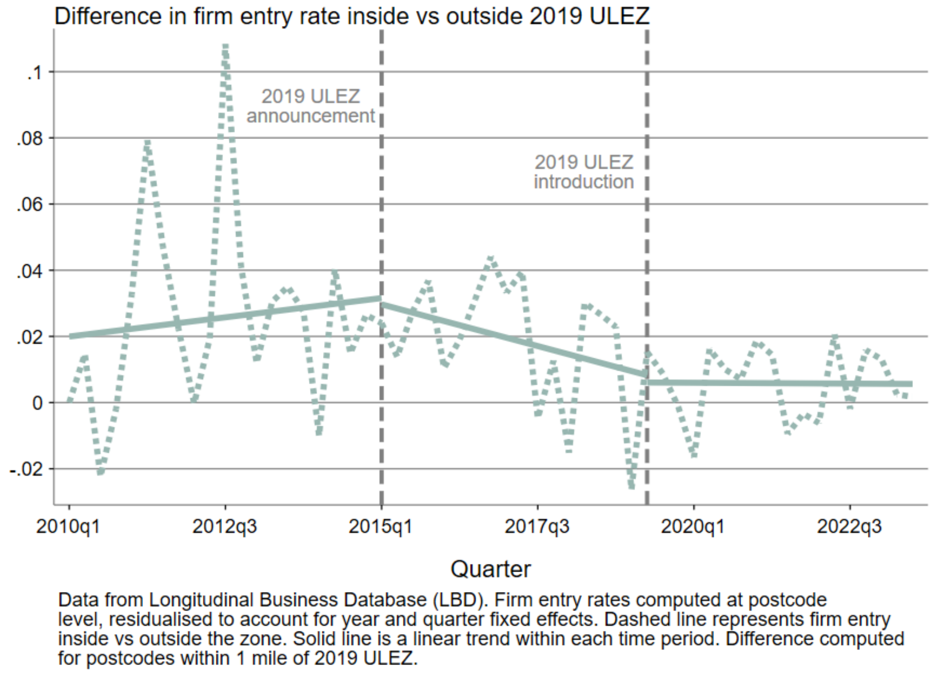
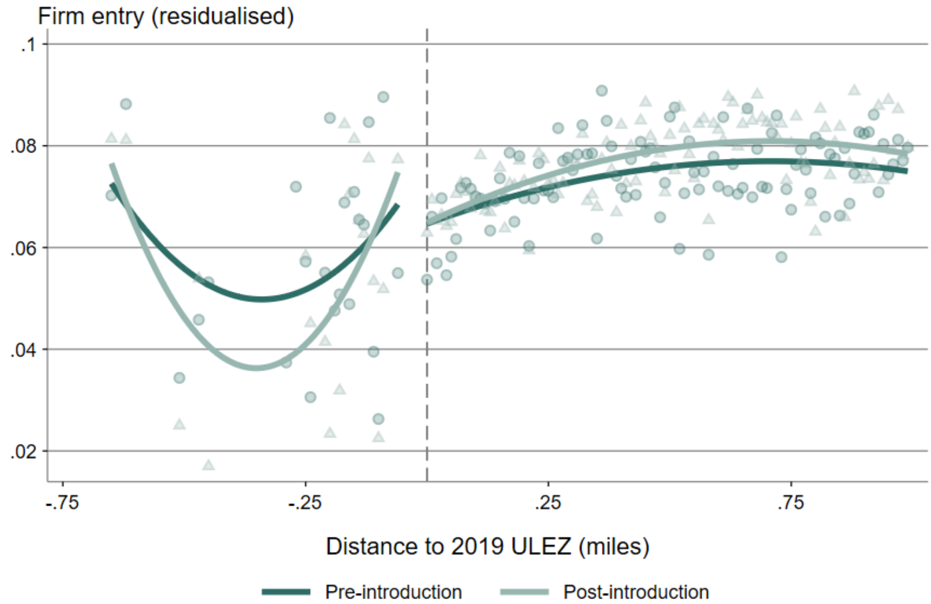


Figure B16: Regression discontinuity plot for firm entry around ULEZ boundary, before and after policy introduction



Data from Longitudinal Business Database (LBD). Firm entry rates computed at postcode level, residualised to account for year, quarter and postcode district fixed effects, weighted by the number of firms pre-introduction. Bin size 0.1 miles. Quadratic best-fit lines fitted on either side of the boundary, pre- and post-introduction.

Figure B17: Heterogeneous treatment effects on the firm entry rate for 2019 and 2021 ULEZ boundaries, across pre-policy postcode-level income quartiles.

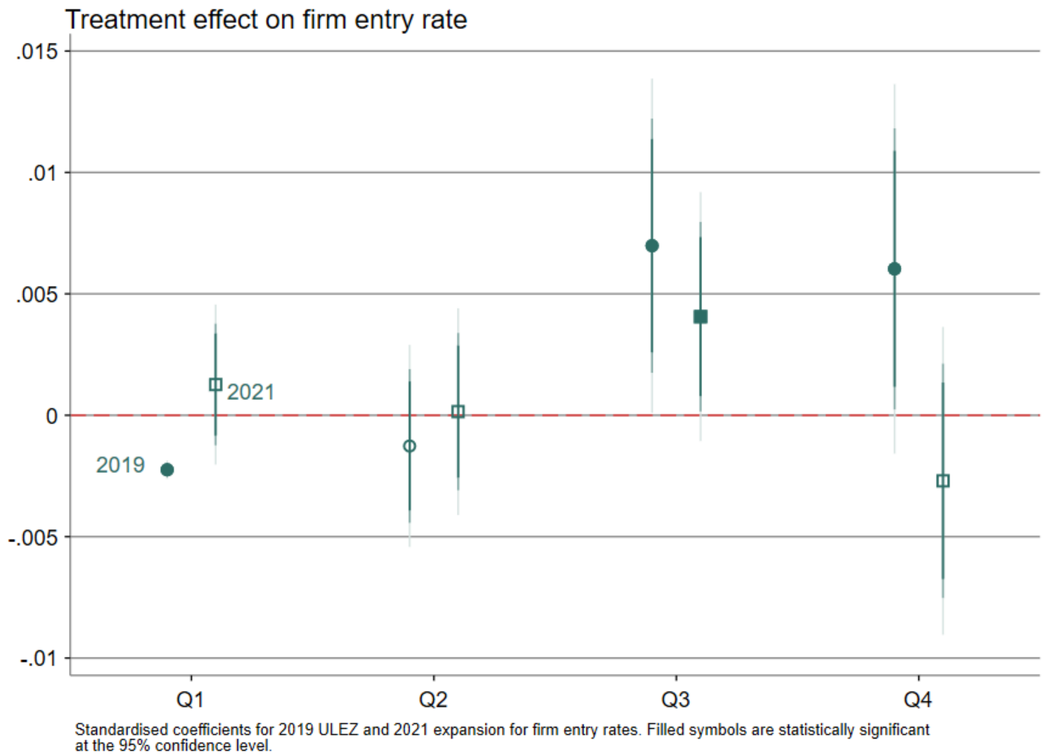


Figure B18: Distribution of true and simulated ULEZ exposure

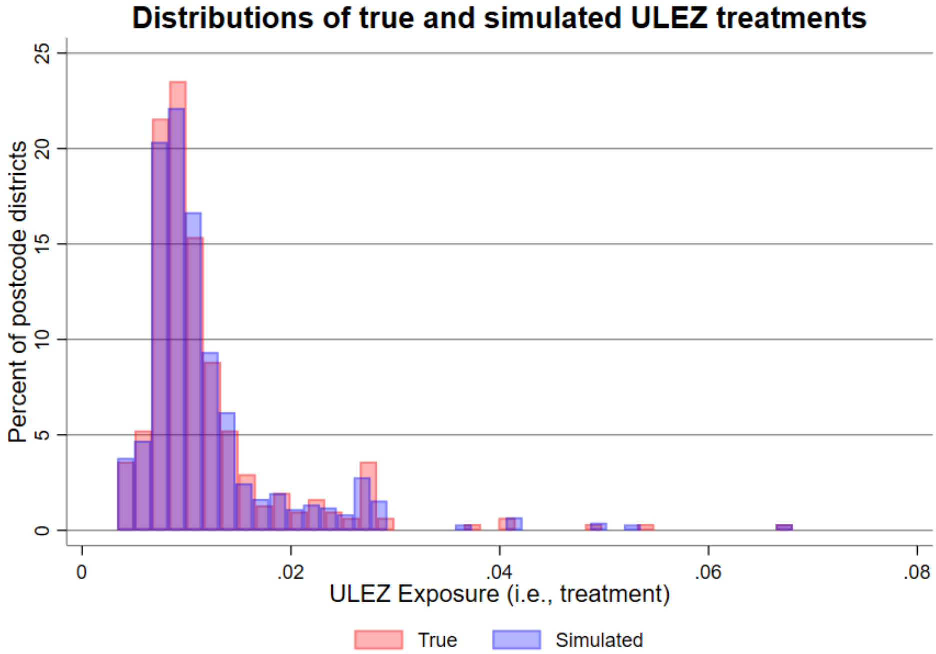


Figure B19: Baseline regression coefficients on ULEV adoption around ULEZ expansion announcement (Q2 2016)

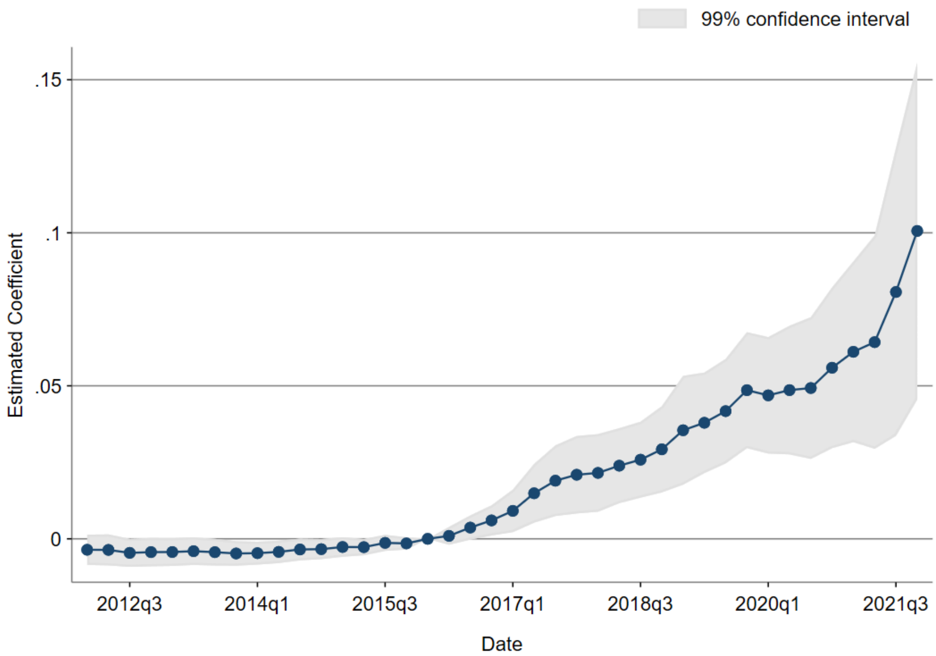


Figure B20: Bootstrapped estimates on the difference in the estimated coefficient on ULEZ indicator for house price RDD regression.

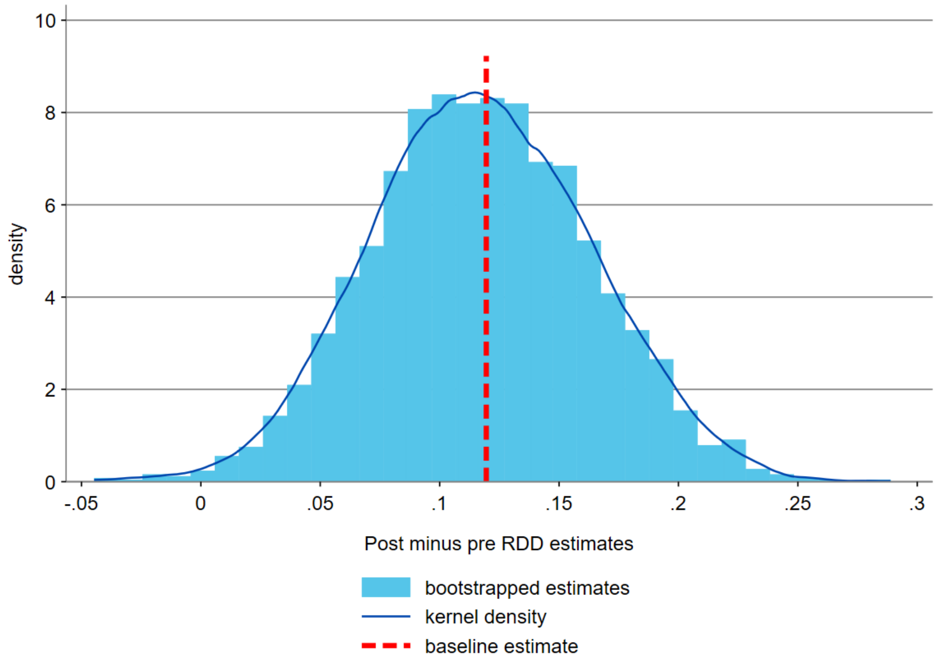


Figure B21: Placebo test on pre-announcement data with fake announcement date.

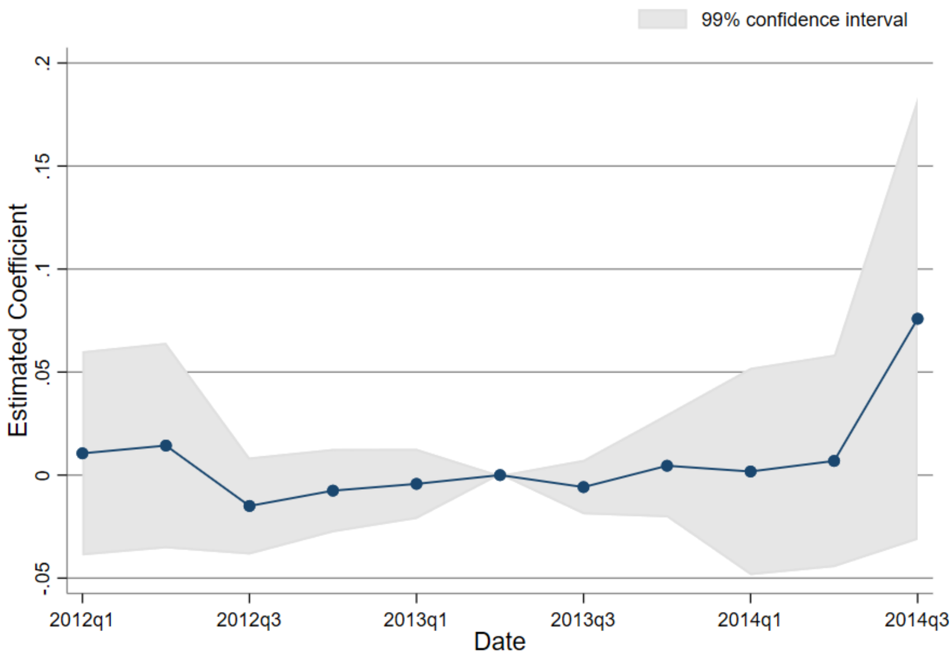
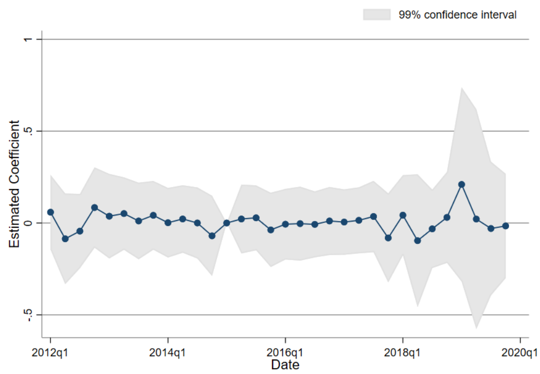


Figure B22: Placebo tests

(a) Placebo test with randomly assigned treatment data.



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

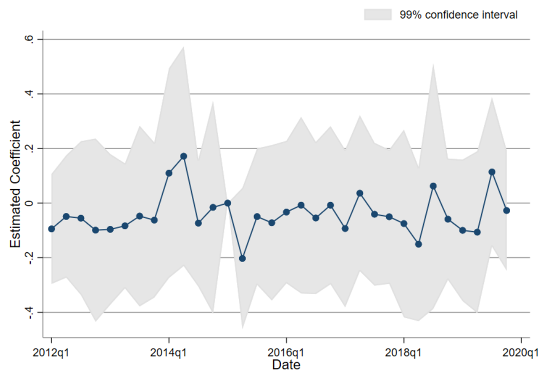


Figure B23: Testing for pre-trends in ULEV adoption prior to initial ULEZ announcement.

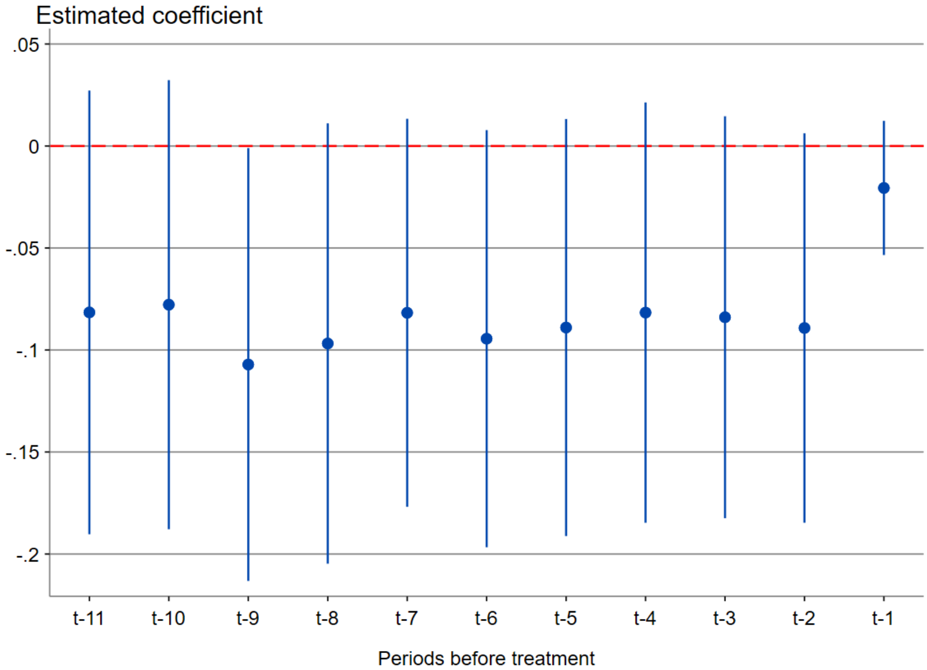


Figure B24: Testing for pre-trends in tube station entry prior to 2019 ULEZ introduction.

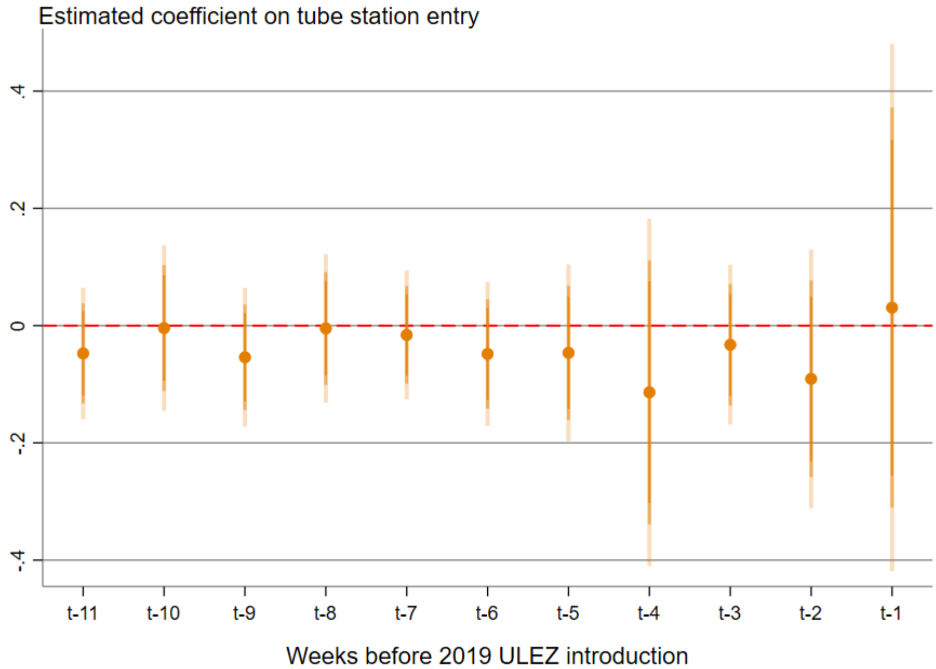


Figure B25: Chaisemartin and D'Haultfoeuille 2020 event study for tube station entry around 2019 ULEZ introduction

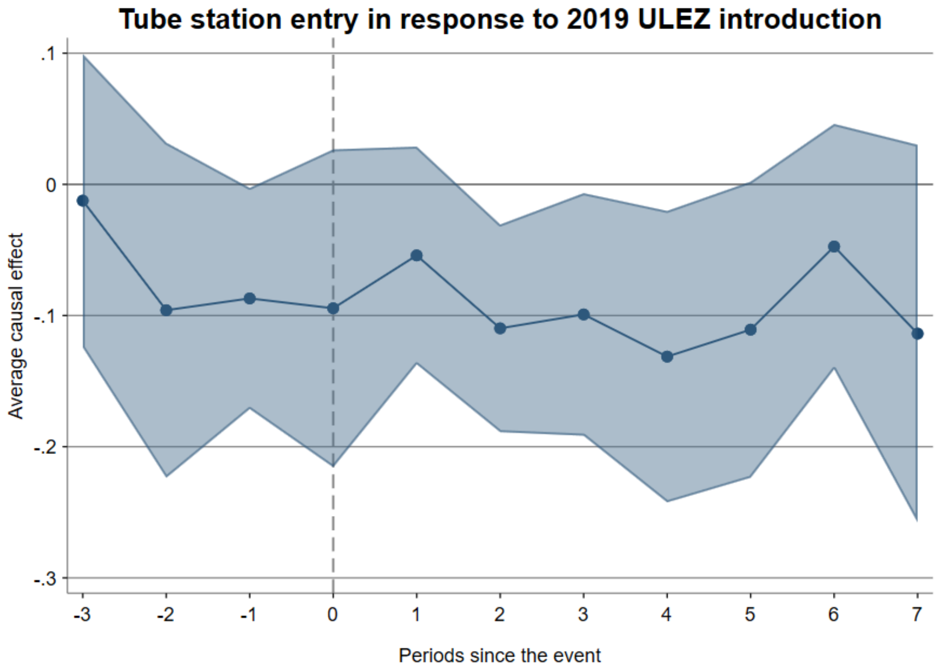


Figure B26: Callaway and Sant'Anna 2021 event study for tube station entry around 2019 ULEZ introduction

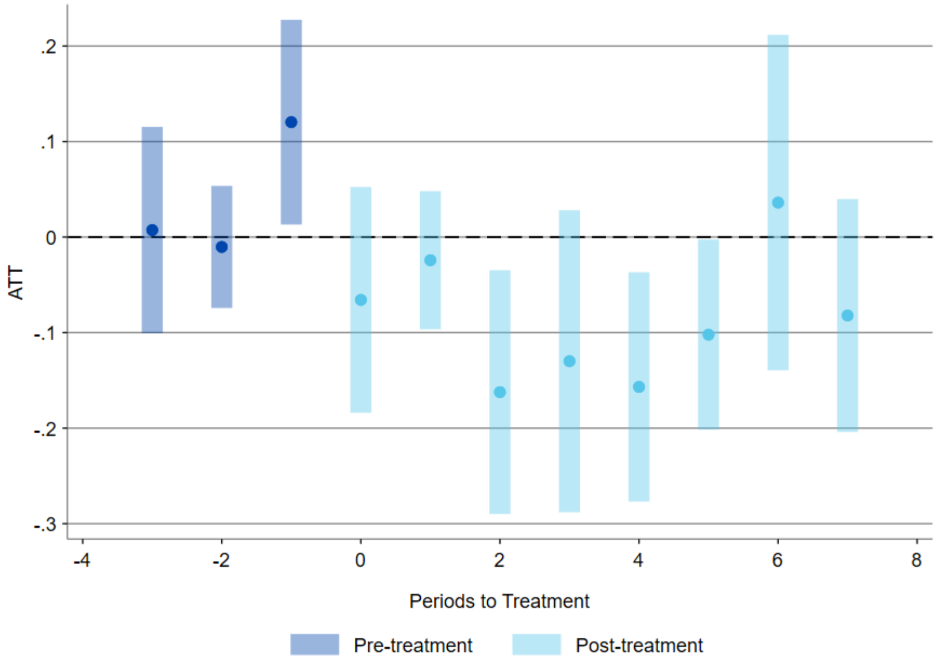


Figure B27: Athey, Bayati, Doudchenko, Imbens, and Khosravi 2021 event study for tube station entry around 2019 ULEZ introduction

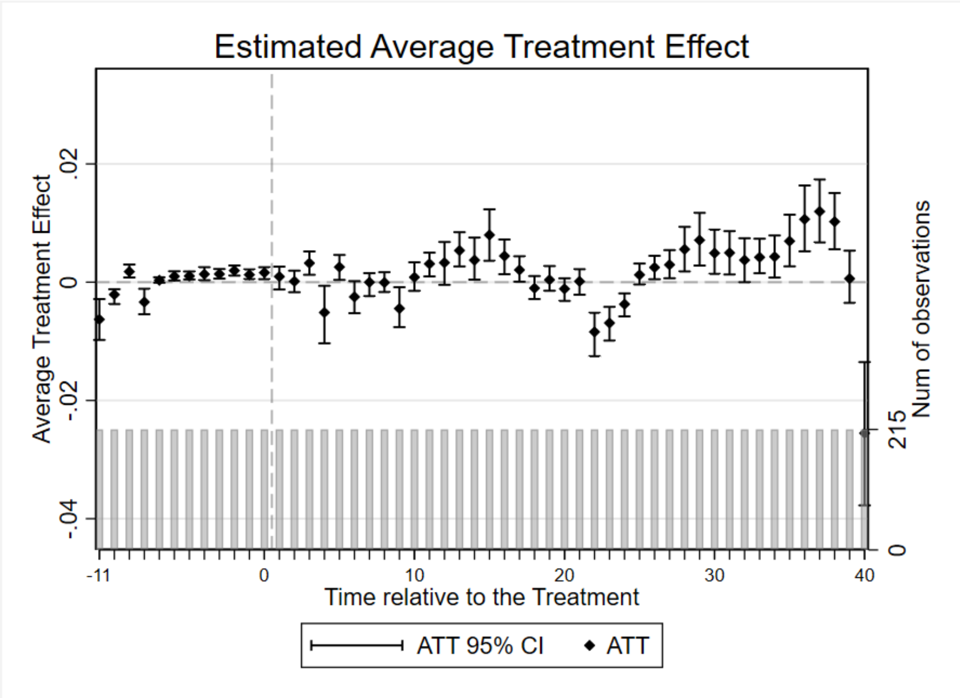


Figure B28: Baseline house price RDD for various distances from 2019 ULEZ boundary.

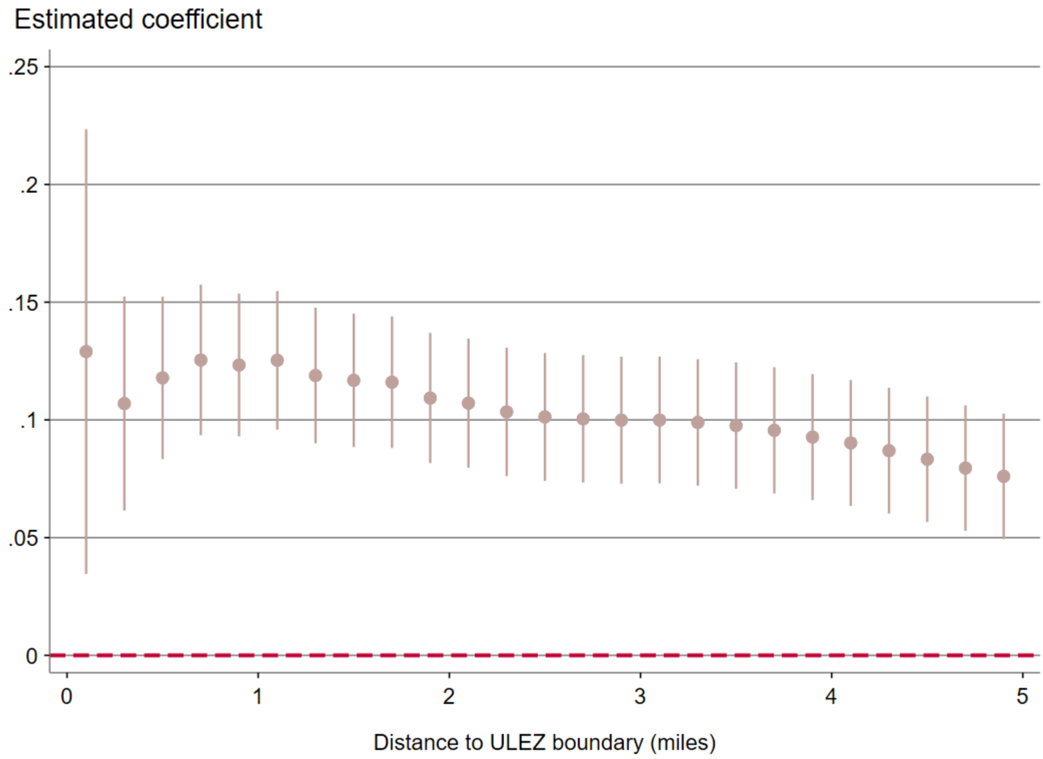


Figure B29: Binned average log house prices within 1 mile of the 2021 ULEZ boundary, before and after the policy announcement in June 2018.

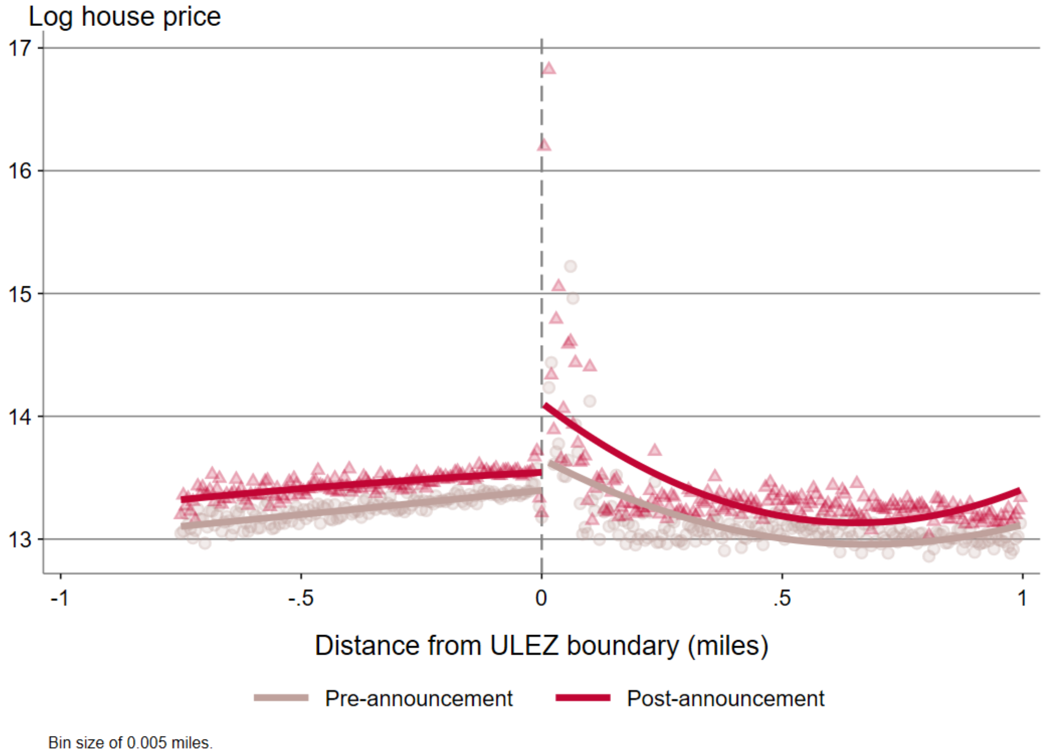


Figure B30: Average change in London underground station entry after September 2021 ULEZ expansion, split by postcodes with above and below median ULEZ exposure.

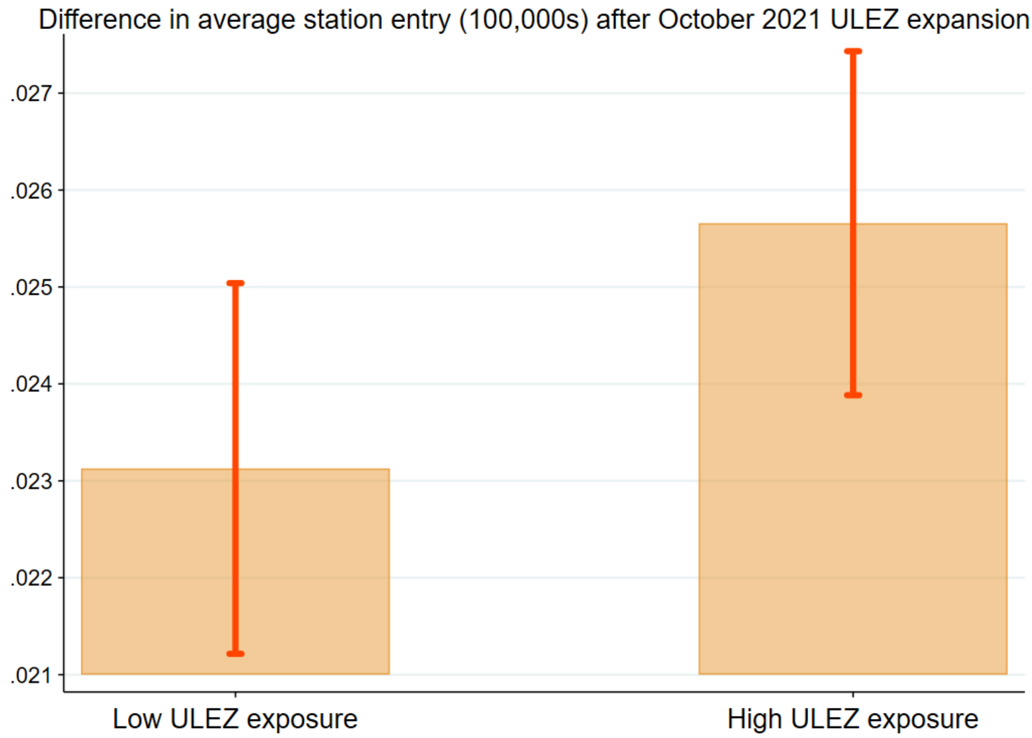


Figure B31: Average working from home share over NUTS3 regions inside and outside the 2019 ULEZ, 2018 - 2020.

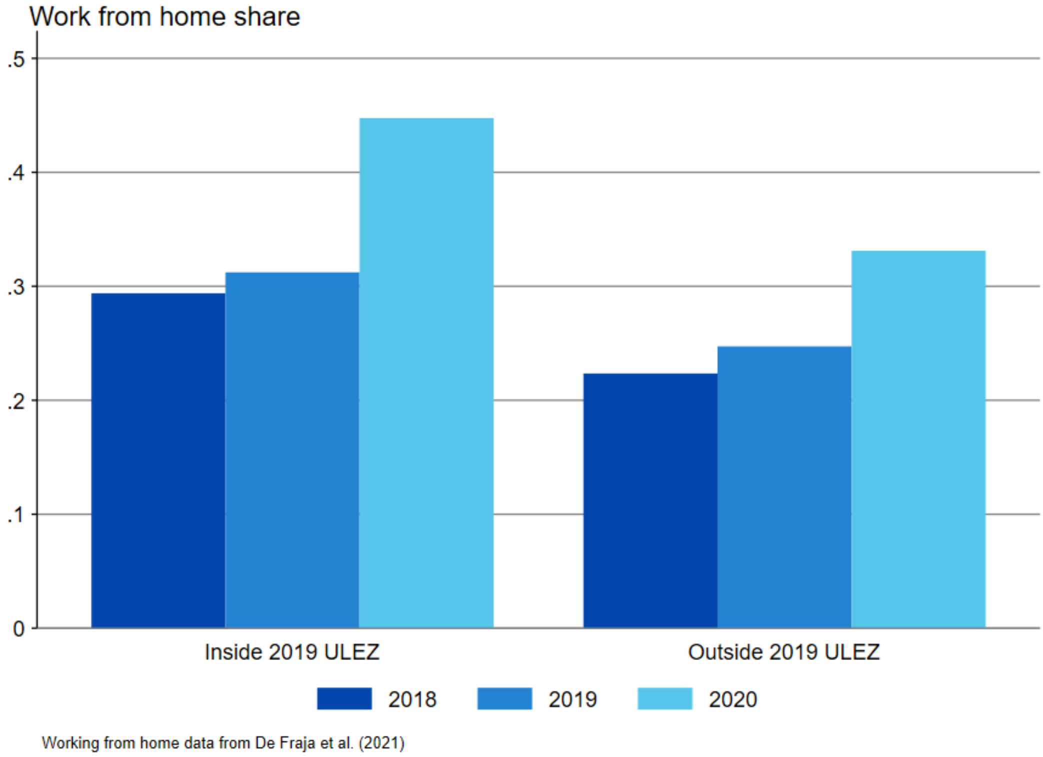


Figure B32: Working from home share (2018) against 2019 ULEZ exposure over NUTS3 regions.

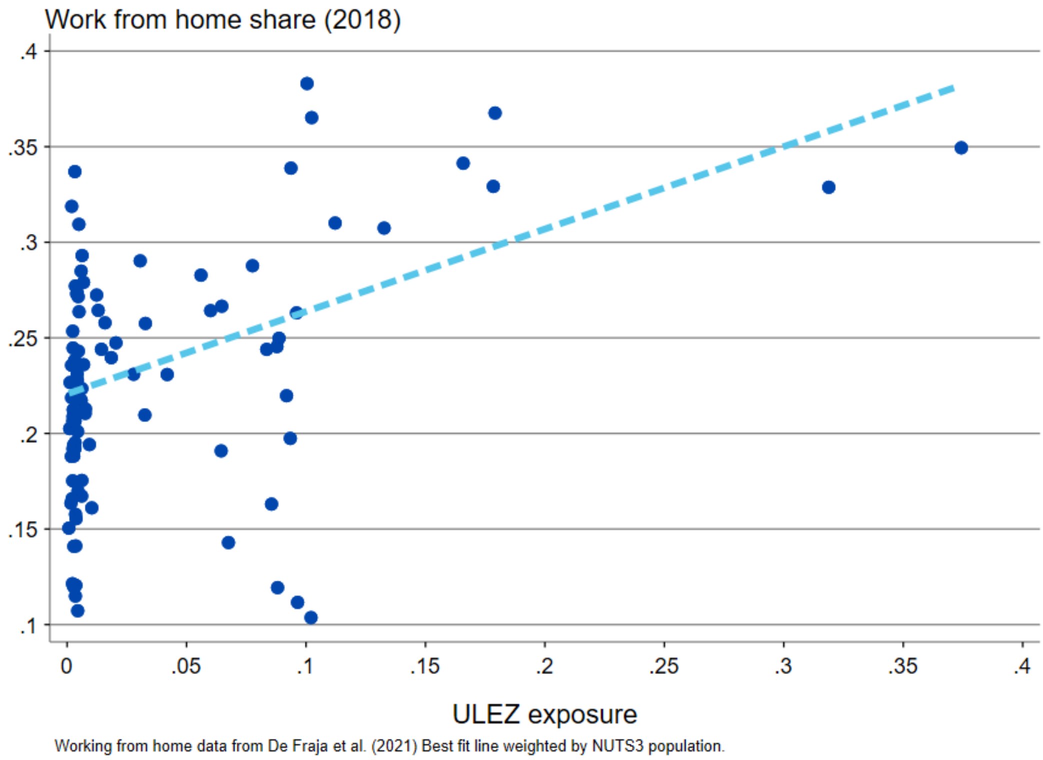
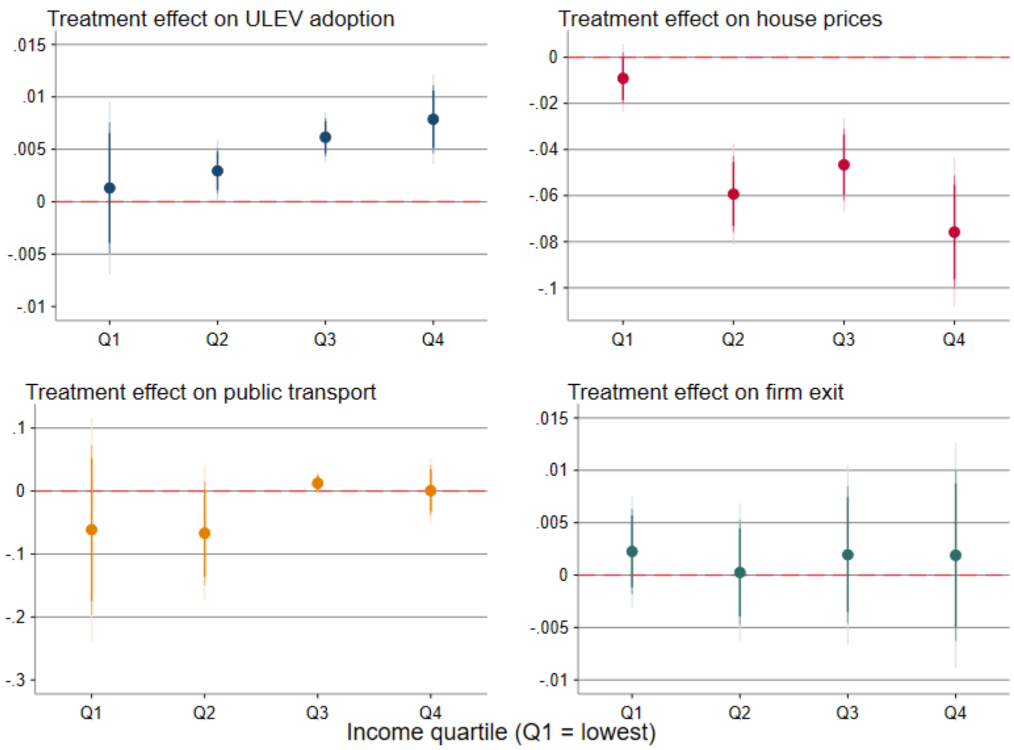


Figure B33: Estimated elasticities to 2021 ULEZ expansion, across income quartiles.



Standardised coefficients for 2021 ULEZ expansion across four margins, with heterogeneity based on 2014 regional net income quartiles.

C Additional tables

Table C1: Summary of robustness checks for house price RDD

Robustness check	Coefficient	Standard error	N
	Dependent variable: Log house price		
A. Functional form			
1. Asymmetric slopes	0.122***	(0.015)	60,797
2. Cubic polynomial distance	0.118***	(0.015)	130,607
3. Donut RDD	0.119***	(0.016)	57,253
B. Bandwidth sensitivity			
4. Narrow bandwidth (0.5 miles)	0.116***	(0.021)	37,054
5. Wide bandwidth (2.0 miles)	0.118***	(0.015)	130,607
C. Heterogeneity			
6. Distance interaction	0.131***	(0.027)	60,797
7. Triple interaction	0.170***	(0.030)	60,797
D. Falsification test			
8. Placebo boundary	0.005	(0.012)	43,074

The table reports the coefficient on the *ULEZ* × *Post-indicator* term for various specifications. Unless otherwise noted, specifications use triangular kernel weights and limit the sample to within 1 mile of the boundary. Standard errors in parentheses, clustered at the postcode level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C2: Difference-in-differences regression of ULEV share on 2021 ULEZ expansion exposure interacted with a post-policy dummy

	(1)	(2)	(3)
	Dependent variable: ULEV share		
2021 ULEZ exposure	-0.009**	−0.002	0.003
	(0.003)	(0.003)	(0.002)
2021 ULEZ exposure × Post-indicator	0.048***	0.032***	0.038***
	(0.008)	(0.008)	(0.005)
Weight	None	Vehicles	Population
N	11,645	11,645	11,645
R^2	0.330	0.413	0.559

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 C3: Tube station entry placebo test 2019 ULEZ with fake boundary

	(1)	(2)	(3)	(4)
	Dependent variable: Log entry/exit (10000s)			
ULEZ	-0.183	-0.249	-0.123	-0.170
	(0.199)	(0.215)	(0.251)	(0.262)
ULEZ × Post-indicator	0.012	0.014	0.014	0.017
	(0.010)	(0.011)	(0.010)	(0.011)
Post-indicator	-0.009	-0.013	-0.013	-0.017
	(0.018)	(0.022)	(0.018)	(0.022)
Dependent variable	Entry	Entry	Exit	Exit
Postcode district FE	No	No	Yes	Yes
N	104,557	104,703	104,557	104,703
R^2	0.068	0.066	0.455	0.451

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 and week of month.

Table C4: Difference-in-differences regression of station entry/exit on 2021 ULEZ exposure interacted with a post-policy dummy

	(1)	(2)	(3)	(4)
	Dependent variable: Station entry/exit (100,000s)			
2021 ULEZ exposure × Post-indicator	0.033*	0.033***	0.034**	0.035**
	(0.017)	(0.017)	(0.016)	(0.016)
2021 ULEZ exposure	0.011	-0.017	0.011	-0.019
	(0.023)	(0.027)	(0.025)	(0.029)
Post-indicator	-0.004	-0.004	-0.005	-0.005
	(0.005)	(0.005)	(0.005)	(0.005)
ULEZ	0.010	-0.011	0.010	-0.013
	(0.008)	(0.012)	(0.009)	(0.012)
Distance to ULEZ		-0.008***		-0.009***
		(0.002)		(0.002)
Dependent variable	Entry	Entry	Exit	Exit
N	135,631	134,903	135,631	134,903
R^2	0.072	0.098	0.063	0.090

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.

Table C5: Baseline house price RDD for 2021 ULEZ boundary.

	(1)	(2)	(3)	(4)
	Dependent variable: Log house price			
2021 ULEZ	0.031**	-0.051***	-0.043**	-0.075***
	(0.013)	(0.017)	(0.018)	(0.018)
2021 ULEZ × Post-indicator	-0.058***	-0.043***	-0.038***	-0.042***
	(0.002)	(0.004)	(0.004)	(0.005)
Distance	-0.008***	-0.059***	-0.031***	-0.058***
	(0.002)	(0.011)	(0.014)	(0.014)
Fixed Effects	Yes	Yes	Yes	Yes
Triangular kernel weight (2 mile)	No	Yes	No	Yes
Within 1 mile	No	No	Yes	Yes
N	1,028,500	524,444	430,081	430,078
R^2	0.392	0.375	0.371	0.375

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 C6: House price "donut" RDD for 2021 ULEZ boundary

	(1)	(2)	(3)	(4)
	Dependent variable: Log house price			
2021 ULEZ	0.039***	-0.026	-0.017	-0.045**
	(0.013)	(0.017)	(0.018)	(0.019)
2021 ULEZ × Post-indicator	-0.055***	-0.038***	-0.034***	-0.036***
	(0.002)	(0.004)	(0.004)	(0.005)
Distance	-0.008***	-0.042***	-0.010	-0.034***
	(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	990,472	486,416	392,053	392,050
R^2	0.387	0.373	0.370	0.374

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 C7: House price RDD for 2021 ULEZ boundary varying triangular bandwidths

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Log house price					
2021 ULEZ	-0.219***	-0.139***	-0.077***	-0.051***	-0.033**	-0.023
	(0.028)	(0.020)	(0.018)	(0.017)	(0.016)	(0.015)
2021 ULEZ × Post-indicator	-0.060***	-0.046***	-0.043***	-0.043***	-0.044***	-0.045***
	(0.012)	(0.006)	(0.004)	(0.004)	(0.003)	(0.003)
Distance	-0.256***	-0.115***	-0.067***	-0.059***	-0.049***	-0.043***
	(0.029)	(0.017)	(0.013)	(0.011)	(0.010)	(0.009)
Bandwidth (miles)	0.5	1	1.5	2	2.5	3
N	335,307	428,523	483,064	524,444	561,572	597,231
R^2	0.393	0.380	0.376	0.375	0.374	0.375

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 C8: Summary of robustness checks for firm entry and exit RDD

Dependent variable:	Firm entry rate		Firm exit rate		
Robustness check	Coeff	SE	Coeff	SE	N
A. Functional form					
1. Baseline	0.013**	(0.005)	0.035**	(0.016)	245,189
2. Donut RDD	0.014**	(0.005)	0.037**	(0.017)	225,852
3. Triple difference	0.009	(0.006)	0.040*	(0.023)	245,189
B. Bandwidth sensitivity					
4. Narrow bandwidth (1 mile)	0.013**	(0.005)	0.035**	(0.016)	245,189
5. Wide bandwidth (5 miles)	0.010**	(0.004)	0.028**	(0.012)	1,079,163
C. Falsification test					
6. Placebo boundary	-0.067	(0.051)	-0.044	(0.029)	245,189

Notes: The table reports the coefficient on the *ULEZ* × *Post-indicator* term (or *Fake ULEZ* equivalent) for various specifications. All specifications use "combined" weights (triangular kernel + firm count weighting) unless otherwise noted. Standard errors in parentheses, clustered at the postcode level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C9: **Table C9:** Baseline firm entry and exit RDD for 2021 ULEZ boundary.

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Firm entry rate			*Dependent variable*: Firm exit rate		
ULEZ	-0.0095**	-0.0031	-0.0032	-0.0072**	-0.0000	0.0021
	(0.0029)	(0.0032)	(0.0035)	(0.0030)	(0.0059)	(0.0073)
ULEZ × Post-indicator	0.0023	-0.0001	-0.0004	0.0011	0.0031	0.0009
	(0.0033)	(0.0028)	(0.0030)	(0.0043)	(0.0048)	(0.0055)
Distance	-0.0038*	-0.0012	-0.0029	-0.0051**	0.0019	0.0011
	(0.0022)	(0.0030)	(0.0036)	(0.0022)	(0.0023)	(0.0026)
Weight	Triangular	# firms	Combined	Triangular	# firms	Combined
N	1,024,202	961,039	961,039	1,024,202	961,039	961,039

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